Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106145	0	0	0	0	The world is experiencing one of the most extremes of global pandemic in the form of COVID-19 this year. Can we elaborate the connections of this pandemic with the issues of changing climate? I think AR6, in general (if not specifically), need to address this big FAQ and provide knowledge-commentary on this nexus. One of the chapters could include this critical most FAQ/issue of this time that the world is facing. Leaving this for IPCC AR6 to provide some useful information in this line (if possible) [Atiq Kainan Ahmed, Thailand]	Rejected. This question is not addressed in Chapter 11 but in Chapter 7 (Rejected.). Effects on climate were limited as highlighted in the Rejected..
99257	0	0	0	0	overall the length of the chapters make it very hard to access the information. I find the level of detail overwhelming and ask the authors to cut back and make the report accessible. Some text, for example discussion of Precip assesses such a large range of indicators, that I loose the overview what the pattern now is. Maybe a summative table might help to shorten the section and provide clarity. For me the mixture of highly detailed information of countries does not mesh with the higher level information for example in the flow section. At the end it is hard to decipher what the main message is given the length and detail [Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The chapter has been shortened and the chapter team has made efforts to make the text overall more concise.
99261	0	0	0	0	The figures as currently drafted are extremely dense and hard to read even for someone who is used to looking at such information. I think it is important to consider the audience again and reduce some figures in content and move some into an appendix [Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The chapter team has reworked the figures to make them more accessible.
38363	0	0	0	0	The abbreviations are inconsistently used in the report. For example, in the report, TXx is mostly used to refer to the annual maximum daily maximum temperature, annual hottest daytime temperature (Box11.4 Table 1 on page 145), but average annual TXx (line 2, page 44) or warm/temperature extremes (Table 11.7) is sometimes also used. It is suggested to harmonize the normative use of abbreviations in order to avoid misleading readers. [Yaming LIU, China]	Rejected. The acronyms can be understood from the context.
66015	0	0	0	0	Suggest additional references: - Barbero, R., Westra, S., Lenderink, G., & Fowler, H. J. (2018). Temperature-extreme precipitation scaling: a two-way causality?. International Journal of Climatology, 38, e1274-e1279. - Berg, P., Moseley, C., & Haerter, J. O. (2013). Strong increase in convective precipitation in response to higher temperatures. Nature Geoscience, 6(3), 181-185. - Busuioc, A., Birsan, M. V., Carbunaru, D., Baciu, M., & Orzan, A. (2016). Changes in the large-scale thermodynamic instability and connection with rain shower frequency over Romania: verification of the Clausius–Clapeyron scaling. International Journal of Climatology, 36(4), 2015-2034. - Hardwick Jones, R., Westra, S., & Sharma, A. (2010). Observed relationships between extreme sub-daily precipitation, surface temperature, and relative humidity. Geophysical Research Letters, 37(22). - Lenderink, G., & Van Meijgaard, E. (2008). Increase in hourly precipitation extremes beyond expectations from temperature changes. Nature Geoscience, 1(8), 511-514. - Lenderink, G., & Van Meijgaard, E. (2010). Linking increases in hourly precipitation extremes to atmospheric temperature and moisture changes. Environmental Research Letters, 5(2), 025208. - Lenderink, G., Mok, H. Y., Lee, T. C., & Van Oldenborgh, G. J. (2011). Scaling and trends of hourly precipitation extremes in two different climate zones–Hong Kong and the Netherlands. Hydrology and Earth System Sciences, 15(9), 3033-3041. - Loriaux, J. M., Lenderink, G., De Roode, S. R., & Siebesma, A. P. (2013). Understanding convective extreme precipitation scaling using observations and an entraining plume model. Journal of the atmospheric sciences, 70(11), 3641-3655. - Mishra, V., Wallace, J. M., & Lettenmaier, D. P. (2012). Relationship between hourly extreme precipitation and local air temperature in the United States. Geophysical Research Letters, 39(16). - Park, I. H., & Min, S. K. (2017). Role of convective precipitation in the relationship between sub daily extreme precipitation and temperature. Journal of Climate, 30(23), 9527-9537. [Kushla Munro, Australia]	Considered. The most relevant and post-AR5 references are assessed in 11.4
57453	0	0	0	#REF!	In general, the discussion of extremes in the Caribbean region is very limited in the text of Chapter 11, in comparison with other regions, except in Table 11.4. The references referring to extreme indices in the Caribbean, cited in Tabe 11.4 have not been discussed in the text. So, most of the assertments about Central America in the text, do not include the Caribbean. I suggest to balance the discussions including in the text some of the relevant references used in the Table.We have some brief discussion in the Atlas regional section including the three CAM regions (Atlas, pages:68:lines 44-47; 70:lines 37-53) . Citing this paragraph may be a choice. [Daniel Martinez Castro, Cuba]	Noted. Small islands are now addressed in a cc-box in the Atlas. Some relevant material is included in the revised regional tables provided in Section 11.9.
19323	0	0			The definition of the compound event listed on page 11-12 should be moved to page 11-6 line 6. [Mansoureh Kouhi, Iran]	Accepted. The definition has been included in the first mention of compound events.
35075	0				Trends in extremes described in multiple figures (e.g. 11.1, 11.7) are based on different data periods, and in turn these are different periods than are used for the trends described in the Atlas chapter. It would be highly desirable to try to standardize the analysis period for trends across this chapter, and across multiple chapters in WG1. [David Gutzler, United States of America]	Noted. The ES indicates that "Assessments of past changes and their drivers are from 1950 onward, unless indicated otherwise". Hence this default time period is considered for most trends. However, this needed to be adapted in some assessments and figures depending on the available datasets. We made sure to always indicate the time frames over which the trends are computed when they depart from this default time frame.
66329	0				Being this a chapter on extreme and belonging to the regional chapters and given that often in the chapter there is the assessment on model validation and projection non only on continental scale but rather on a more regional scale, it would be appropriate to use not only and always GCM. Given the literature partially cited in the chapter but definitely available in other chapters of the report that show the added values when dynamical and statistical downscaling is used, especially for precipitation extremes in many regions, it would be good to have some figures showing example of this as well as example on extreme precipitation projection of multiple ensemble to highlight the differences in the regions where data are available. [Erika Coppola, Italy]	Accepted. More literature based on RCM studies has been integrated in the chapter, in particular in the revised regional tables in Section 11.9.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
81441	0				I will only give overarching comments on the chapter, without going into specific details. In general, the chapter is well organized and relates important information. The main drawback I see, and that I wanted to bringto the author's attention, is that essentially by far most of the information provided is based only on CMIP5 and CMIP6 data. This is quite disappointing, as there is by now a substantial literature based on RCM simulations at resolutions from 25-50 km to convection permitting which addresses issues of extreme events. This literature is only superficially and partially mentioned throughout the chapter, and for example there are no figures based on RCM and other downscaling studies. In particular, there are papers based on multi-modeland/or ensemble based CORDEX data (e.g. Giorgi et al., 2014 on precipitation extremes;; Spinoni et al., JC, 2020 on global drought hotspots, or Coppola et al., CD, 2020 on hazards) which could provide suitable figures. In fact, one could argue that current GCMs are not the most suitable tools to look, for example, at precipitaiton extremes, since a number of studies have demonstrated that they fail in reproducing the tail of the precipitaiton distribution. The downscaling community has mad a huge effort in improving its coordination exactly for the purpose of providig more solid information for use in programs such as the IPCC, and I think this should be better recognised than done in this chapter. Incidentally, a better treatment of downscaled information would also improve the consistency of this chapter with others of the report and with the Atlas. [Filippo Giorgi, Italy]	Accepted. More literature based on RCM studies has been integrated in the chapter, in particular in the revised regional tables in Section 11.9.
106531	0				WGII ch2 "Terrestial and freshwater ecosystems" has a section on exrreme events and a CCB EXTREMES highlighting extreme events and their impacts on biological systems - these overlap ch11. Care should be taken to ensure consistency between WGs in messages and uncertainty assessments of those messages. [camille parmesan, France]	Noted. Because of the timing of the WGII report, the present chapter cannot refer to this upcoming report.
23085	0				Chapter as far as I can tell lacks a schematic visual abstract presently yet these are present for almost all other chapters. [Peter Thorne, Ireland]	Accepted. A visual abstract has been added.
67895	0				Especially for Sub Chapter 11.2, there should be a brief and clear description of the Palaeoclimate Extreme Notes based on the existing instruments [Ruandha Agung Sugardiman, Indonesia]	Noted. It is unclear what “the paleoclimate extreme notes” is. Box 11.3 provides comparison of extremes in paleoclimate archives with instrument records
67897	0				In this chapter there is also no information about potential effects of 'extreme weather' for the case of an island state which is of marine nature, for instance coral bleaching. Coral bleaching has reached extreme levels, for example in the Great Barrier Reef, Australia. In Indonesian waters, cases of coral bleaching have been reported to increase. This issue may be considered as one of the effect of 'extreme weather' for island or archipelagic countries. Coral bleaching may affect not only biodiversity, but it will also have socio-economic impacts on the community. [Ruandha Agung Sugardiman, Indonesia]	Rejected. Marine heatwaves do not belong to the scope of chapter 11 and are addressed in chapter 9. This scope is indicated in the ES and introduction to chapter 11.
114747	0				I do see the advantages of a focus on warming levels. These can also be very uswful in the commuication of findings. But I also see a need for adressing teh time dimension a bit more. Early in the process of writing WGI AR6 it was decided to use a core set of scenarios across the chapters. These are SSP1-1.9, SSP1-2.6, SSP2-4.5, SSP3-7.0, SSP5-8.5 (with additional scenarios where appropriate). It would strengthen the report if these (to the extent possible) are also considered in ch11. That will also support a better integration across chapters in the TS, SPM and also finally in SyR [Jan Fuglestvedt, Norway]	Accepted. We have included a new Rejected. (11.1) addressing the issues highlighted by the reviewer.
114771	0				The use of GMST vs GSAT has beed discussed at BOGs during pre-LAM. It will be important to implement the conclusions from this process and ensure clarity and transparency on this issue. [Jan Fuglestvedt, Norway]	Noted. This issue is addressed in chapter 2, in a Rejected. to which chapter 11 contributed. In model-based analyses from chapter 11, GSAT is used as reference.
23135	0				Tendency to bold summary statements of sections is at odds with all other chapters reviewed thus far. [Peter Thorne, Ireland]	Rejected. We consider that this format is useful and would encourage the other chapters to use a similar format. This was also applied in the IPCC SREX report. The summary paragraphs were also considered useful by other reviewers (e.g. review comment #96105)
96105	0				Altogether, a well structured chapter, especially the reoccurring sub-chapters (mechanisms/drivers, observed trends, model evaluation, event attribution, projections), the summary at the end of each sub-chapter, the blue boxes with additional information on case studies, etc. and the extensive literature list. The FAQ section is a nice idea but it could be extended, in my opinion. [Nicole Wilke, Germany]	Noted. Thank you for the positive feedback. The FAQs have been revised.
23163	0				When discussing pertinent trends to the regional case studies considered in chapter 10 thought should be given to cross-referencing and where the assessments overlap arguably it would be better to reconcile in chapter 10 in their case studies to avoid overt repetition. [Peter Thorne, Ireland]	Noted. More references to chapter 10 case studies have been included.
74367	0				There is a situation where different anomalies in precipitation and temperature occurs simultaneously at the same time and place. This situation is called as unusual events (see Yulizar and Bardossy, 2020, Study of changes in the multivariate precipitation series) [Yulizar Yulizar, Indonesia]	Rejected. Too detailed for the assessment and "unusual event" is not very specific.
23167	0				There is a comparably very high rate of self-citation compared to many other chapters. This may be because the authors are drwan from a small field and thus this is inevitable or it may indicate a need to assess a broader range of literature than was done in this draft in going to the FGD. [Peter Thorne, Ireland]	Noted. The rate of self-citation has been carefully checked and reduced where possible. In some areas, the chapter authors have contributed to a large fraction of the available literature, which makes some level of self-citation unavoidable. A broader literature basis has been considered wherever possible, including regional literature.
74369	0				In the context of identifying the precipitation extremes, the zero values of precipitation should also be considered [Yulizar Yulizar, Indonesia]	Noted. This is the case for instance when using "consecutive dry days", i.e. the number of consecutive days with zero precipitation.
23177	0				Chapter uses GCM and ESM interchangably. The balance of the report tends to use ESM so suggest replacing GCM with ESM unless GCM is essential term to use. [Peter Thorne, Ireland]	Rejected. Other chapters, in particular regional chapters (Chapter 10, Atlas), also often use the term GCM. Both terms are used across the AR6.
109965	0				There is a significant mismatch in how much weight chapters 8 and 11 place on PDSI versus SPEI. Chapter 8 almost exclusively considers PDSI while chapter 11 downweights considerably this metric. Capters 8 and 11 need to agree who takes primary definitional aspects of drought (my feeling is it should be 8) and both then need to follow this. [Peter Thorne, Ireland]	Taken into account. The drought assessment has been strongly coordinated for this round of revision between chapters 11, 8, 12 and the Atlas. Definitional aspects (e.g. diagram on processes leading to droughts) is now moved to chapter 8 in a simplified version. The present assessment also considers PDSI, including caveats from the literature.
107409	0				In general, the howle chapeter does not introduce enough climate changes in North Africa, especially in Mediterranean region. There are many studies that highlight causes and effects on climate changes. [Rachda Berrached, Algeria]	Noted. More literature on climate change in North Africa has been included in the assessment (see in particular revised regional tables in Section 11.9)
109977	0				Atmospheric Rivers are also assessed by chapter 8. They do not need to be assessed twice and chapter 8 as scoped is the logical place to assess these. I suggest that the text in 11.7 gets given to chapter 8 and integrated there. [Peter Thorne, Ireland]	Taken into account. The assessment on atmospheric rivers has been coordinated between Chapter 11 and Chapter 8.
23195	0				You quite often use a construct "studied N continents / basins including ..." and then list all N cases. If you are going to list all N such cases then you don't need this complicated construct and you would be better just to list the continents / basins / whatever which would be an easier read and also save space. [Peter Thorne, Ireland]	Noted. We have simplified this type of wording where possible.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9123	0				Excellent chapter. Well structured with a comprehensive assessment of published material. All statements in the Executive Summary are qualitative. Is it possible to quantify the likelihood and severity of observed and projected changes (since WG2 has also been asked to do this)? Needs spell-checking. [Kevin Hennessy, Australia]	Noted. Thank you for the positive feedback. The ES has been revised to include more quantitative information.
10925	0				The two different definitions of "attribution" should be made clear in this chapter. e.g.,1) the attribution of changes of frequency/magnitude of extremes over time, by testing hypothesis of an anthropogenic influence, versus 2) event attribution where the frequency/magnitude of a particular event due to anthropogenic influence is estimated by assuming anthropogenic influences have caused the climate to change/warm. In several places in this chapter there are phrases that imply it is possible to attribute the drivers of a specific individual event, when that is not currently possible. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken partially into account. These two types of attribution are more clearly distinguished in the text. However, even for single events, the (probabilistic) role of individual drivers can be assessed in some cases.
109999	0				Cross-referencing is uneven. There are some great examples where cross-references are made to the specific sections. Then there are some so-so efforts where a whole chapter is referenced and then there are places where no effort has been made at cross-referencing. All sections should strive to add cross-references to other chapters with section-level specificity in the FGD. [Peter Thorne, Ireland]	Noted. In the FGD revisions, the chapter team has made substantial efforts to improve the cross-referencing to other chapters as well as recent IPCC reports (e.g. IPCC SRCCL).
110005	0				Section 11.7.2 needs to be refocussed upon an assessment of extreme ETCs rather than all ETCs. The present assessment overlaps very substantively with chapters 2,3,4, and 8 and chapter 11 should really only be concerned with the extreme storms rather than trying to cover all storms which is beyond its given remit. Material that pertains to general ETCs should be shared back to and integrated within these prior chapters and instead cross-references made. The revised section should concentrate upon extreme ETCs. [Peter Thorne, Ireland]	Accepted. The text in this section has been updated to cover only changes in extreme ETCs and refer to other chapters, including chapters 2, 3 and 8, whenever needed.
23231	0				The chapter has a huge number of brief introductions to sections many of which are not adding any real value and several of which arguably obfuscate rather than clarify. The propensity for use of these segments is also somewhat at odds with most other chapters. I would suggest removing or significantly reducing the vast majority of such segments in the FGD as it is the substantive assessment that matters not these link pieces and teh chapter would be more in keeping with others were it to do so. [Peter Thorne, Ireland]	Noted. The chapter has been shortened and the chapter team has made efforts to make the text overall more concise.
110041	0				This chapter is disproportionately long compared to all chapters that have preceded it. For balance of the report as a whole it should be reduced in length. One of the easiest ways to do this would be to reduce redundancy with earlier chapters. Quite often chapter 11 is over-reaching to perform assessment redundantly of mean state changes of hydrological cycle changes with resulting redundancies with chapters 2,3,4 and 8. It should instead start from these chapters findings making appropriate cross-reference and then focus upon changes in extremes. [Peter Thorne, Ireland]	Noted. The chapter has been shortened and the chapter team has made efforts to make the text overall more concise. Overlaps with other chapters have been reduced in coordination with these (e.g. as part of the cross-chapter drought team)
110057	0				Figures would benefit from a number of tidies with a view to making them self-describing standalone items suitable for use in outreach and education. Self-describing figure titles, clearer and larger fonts and more use of labelling to increase accessability would pay substantial dividends for realtively minor effort. [Peter Thorne, Ireland]	Noted. The author team has improved the readability of the figures.
105973	0				The extensive discussion of "compound events" in this Chapter is very welcome, as it highlights a thus-far underexplored area of extreme event research. While brief references are made to the potential for "successive" events, it would strengethn the Chapter to further expand on the challenges such "successive" events can pose. Otto, et al. (2020) notes one illuminating lens through which to do so: the notion of "loss amplification," which contends that back-to-back blows from extreme weather/climate events can degrade the resilience of both natural and human/infrastructural systems — making it more and more difficult for them to endure, abosorb, and recover from each successive event. Otto, Christian, Franziska Piontek, Matthias Kalkuhl, and Katja Frieler. “Event-Based Models to Understand the Scale of the Impact of Extremes.” Nature Energy 5, no. 2 (February 2020): 111–14. https://doi.org/10.1038/s41560-020-0562-4. [Sohum Pawar, United States of America]	Noted. This could not be added because of space limitations.
28921	0				Excellent SOD with good improvements on extreme precipitation. As usual there are a number of inconsistencies and overlaps with Chapter 8 which can be agreed and addresed e.g. 11.1.5 with 8.2.2.3 11.4.2 with 8.3.1.3 11.4 with 8.4.1.3.3 11.5.2 with 8.3.1.5 and Box 8.2 11.5.5 with 8.4.1.5 11.7.1 with 8.4.2.5 11.7.1.2 with 8.3.2.5 and 8.2.3.2 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Inconsistencies and overlaps with chapter 8 have been addressed. The most relevant and post-AR5 references are assessed in 11.4.
130555	0				Please consider to list all the Acronym for extreme indices,. [Panmao Zhai, China]	Considered. While the literature assessed here used different indices, indices are not the foci of Chapter 11’s assessment and as such do not need to be highlighted in the chapter text. Additionally, a list of all acronym with their definition would make a long chapter even longer. Hence, no acronym list was added.
125797	1	1	1	1	For more standard word order, authors could consider: Extreme weather and climate events in a changing climate. Or simply Extreme events in a changing climate. [Trigg Talley, United States of America]	Rejected. The title is prescribed by the IPCC outline.
62641	1	1	230	20	In general there is a high tendency of self citation throughout the text. Even there are many places where the authors of this chapter included own paper which is less relevant. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The rate of self-citation has been carefully checked and reduced where possible. Also single citations have been carefully checked. In some areas, the chapter authors have contributed to a large fraction of the available literature, which makes some level of self-citation unavoidable. A broader literature basis has been considered wherever possible, including regional literature.
80689	1	1	271	1	In many places in the chapter there are issues with space between words and before or after a parenthesis. It will need to be carefully checked in the final version [Helene Jacot Des Combes, Marshall Islands]	Accepted. The text has been carefully checked for the FGD version.
80691	1	1	271	1	There are several typos in the text that will need to be corrected in the final version. I did not made a comment for each because the text may change until the final version but a careful proofreading is necessary. [Helene Jacot Des Combes, Marshall Islands]	Accepted. The text has been carefully checked for the FGD version. Further proofreading will be provided by the TSU for the typeset version.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83383	1	1	271	7	There is no dicsussion/coverage of weather and extreme ecents in a changing climate around Antarctica. This is a major deficiency and a missed opportunity. Please rectify this. See, for example: (1) Massom, R.A., Stammerjohn, S.E., Smith, R.C., Pook, M.J., Iannuzzi, R.A., Adams, N., Martinson, D.G., Vernet, M., Fraser, W.R., Quetin, L.B., Ross, R.M., Massom, Y. and Krouse, H.R. 2006. Extreme anomalous atmospheric circulation in the West Antarctic Peninsula region in austral spring and summer 2001/2, and its profound impact on sea ice and biota. Journal of Climate, 19, 3544-3571. and (2) Bergstrom, D.M., Woehler, E.J., Klekociuk, A.R., Pook, M.J. and Massom, R.A. 2018. Extreme events as ecosystems drivers: Ecological consequences of anomalous Southern Hemisphere weather patterns during the 2001/02 austral spring-summer. Advances in Polar Science, 29(3), 190-204. doi/10.13679/j.advps.2018.3.00190. and (3) Robinson, S. A., Klekociuk, A. R., King, D. H., Pizarro Rojas, M., Zuniga, G. E. & Bergstrom, D. M. (2020). The 2019/2020 summer of Antarctic heatwaves. Global Change Biology, https://doi.org/10.1111/gcb.15083. [Robert Massom, Australia]	Noted. This chapter is focusing on inhabited regions and is thus not covering extreme events in Antarctica. This scope is now clarified in the ES.
41015	1	1	271	9	The term 'human influence' is used throughout the chapter, but it's rather vague. What is meant by this term needs to be explained upfront (perhaps with a callout to another chapter), while more specificity should be given on the particular combination of human influence factors involved (e.g., aerosols) in the attributed changes presented, where appropriate. [TSU WGI, France]	Accepted. We have made the text more explicit where possible, e.g. distinguishing between greenhouse gas forcing, land use forcing and aerosol forcing where possible in the ES.
41069	1	1	271	9	There's a need for more specificity in some chapter text (e.g., instead of saying 'some regions' state the regions). [TSU WGI, France]	Accepted. We have included much more details on the assessed regional changes (e.g. list of affected AR6 regions in ES).
32777	1	1	300	50	The definition of the compound event listed on page 11-12 should be moved to page 11-6 line 6. [sadegh zeyaeyan, Iran]	Accepted. This is fixed in the final version of the chapter.
33107	1	1	300	50	The definition of the compound event listed on page 11-12 should be moved to page 11-6 line 6. [Sahar Tajbakhsh Mosalman, Iran]	Accepted. This is fixed in the final version of the chapter.
71471	1	1			In Section 10.3.3.5-7 we are discussing the representation of large-scale circulation (10.3.3.4), regional phenomena (10.3.3.5), regional feedbacks (10.3.3.6) and regional drivers such as aerosoles and land-use changes (10.3.3.7) in different types of models. Much of this is relevant for Chapter 11 and should be referred to appropriately. [Douglas Maraun, Austria]	Noted. Some references to chapter 10 have been added where appropriate.
66935	1	1			My congratulations and appreciation to the authors for a well-written chapter that was very interesting to read. [Mathew Barlow, United States of America]	Noted. Thank you for the positive feedback!
100777	1	1			The literature on Air Pollution extremes is available and should have its own Section 11.7bis (however short) because it is a big item for WGII and the governments. I highlight the work that I have been involved with below because it takes a unique "climate" approach instead of the regulatory ones that many studies have done. To ignore this because the Chapter 11 title is weather and climate is not appropriate because weather and climate are driving these extremes. The chapter is missing the work done on air quality / pollution extremes by J Schnell. The data analysis provided regularly gridded daily AQ data over North America and Europe where observations are available and dense. It identified climatic extremes on regional/continental scales that are reproduced by global chemistry-climate models (CMIP5). The extremes are characterized by scale and duration (100s km and days). Further the projection of the occurrence of such extremes in a 2090s RCP8.5 world is analyzed, as well as connections between AQ and ENSO. This should be included in Chapter 11. J. L. Schnell, M. J. Prather, B. Josse, V. Naik, L. W. Horowitz, G. Zeng, D. T. Shindell (2016), Effect of climate change on surface ozone over North America, Europe, and East Asia, Geophys. Res. Lett., 43, 3509–3518, doi:10.1002/2016GL068060. J. L. Schnell, M. J. Prather, B. Josse, V. Naik, L. W. Horowitz, P. Cameron-Smith, D. Bergmann, G. Zeng, D. A. Plummer, K. Sudo, T. Nagashima, D. T. Shindell, G. Faluvegi, and S. A. Strode (2015), Use of North American and European air quality networks to evaluate global chemistry-climate modeling of surface ozone, Atmos. Chem. Phys., 15, 10581-10596, doi:10.5194/acp-15-10581-2015. J. L. Schnell, C. D. Holmes, A. Jangam, M. J. Prather (2014) Skill in forecasting extreme ozone pollution episodes with a global atmospheric chemistry model, Atmos. Chem. Phys., 14, 7721–7739, doi:10.5194/acp-14-7721-2014. L. Xu, J.-Y. Yu, J. L. Schnell, and M. J. Prather (2017), The seasonality and geographic dependence of ENSO impacts on U.S. surface ozone variability, Geophys. Res. Lett., 44, 3420–3428, doi:10.1002/2017GL073044. [Michael PRATHER, United States of America]	Rejected. Air pollution aspects, including extremes, are addressed in Chapter 7.
69951	1	17	1	17	Please change country name from "South Korea" to "Republic of Korea" to keep consistency of naming in other chapter. [Young-Hwa BYUN, Republic of Korea]	Accepted. "South Korea" has been replaced with "Republic of Korea" in the text.
69953	1	19	1	19	Please change country name from "Korea" to "Republic of Korea" to keep consistency of naming in other chapter. [Young-Hwa BYUN, Republic of Korea]	Accepted. "South Korea" has been replaced with "Republic of Korea" in the text.
53543	3	8			Beyond floods and peak flows, global warming can lead to other policy-relevant changes in the seasonality of extreme events which may deserve a stronger emphasis in both CH11 and CH12 given their possible dramatic impacts (e.g., spring heat wave effect on major crops, extended dry and wildfire seasons, ...) [Hervé Douville, France]	Partly taken into consideration. For droughts, chapter 11 now also provides seasonal projections (Fig. 11.19). Much more details could not be included in the text because of the space limitations.
1425	4	1	9	38	As in the other chapters, the executive summary is a difficult read. It follows the particular IPCC "dialect" that noone else would use in their writing og talking. I think most readers almost forget what they read in the beginning of the key message by the time they finish the sentence. The key points can be expressed in a simpler and more elegant way, and thus have stronger impact beyond the IPCC community. [Rasmus Benestad, Norway]	Noted. The ES has been carefully revised to make it more readable for the general public.
11641	4	37	4	37	in table of contents but also in header for box 11.5: “small islands territories” is awkward and grammatically incorrect. Better to change to “small island territories”. [Amy East, United States of America]	Not applicable: This box is no longer in the chapter.
105147	6	1	6	1	The editorial style used in the executive summary is a bit tedious, in the sense that most sentences use the formulation “There is [calibrated term] that [statement]”. Generally, more direct language could be used by simply making the statement at attaching the calibrated assessment at the end of the statement in parentheses. [Francis Zwiers, Canada]	Accepted. The text was simplified as suggested by the reviewer where possible.
105149	6	1	6	1	It is also not always clear who the intended audience is for the executive summary. I assume the primary audience should be policy makers since the SPM is constructed by selecting headlines from the executive summaries of the individual chapters. Some statements in the executive summary therefore seem excessively detailed, and in several instances, it is not apparent how a policy maker would be able to use the information that is presented. [Francis Zwiers, Canada]	Noted. The text was revised to be better targeted at policymakers.
34951	6	1	9	26	Detailed Comments by SOD Chapter – Chapter 11: The SOD claims increasing so-called weather extremes. See rebuttal comment #12 above. [Jim O'Brien, Ireland]	Rejected. The comment is unclear. It is not clear what is the comment #12 that is referred to.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66689	6	1	9	50	There are too many Exec Summary points and they are too detailed. This is inconsistent with the level of detail exposed in the other chapters. If everyone did this, then the SPM would blow out in length. (Classic tragedy of the commons…) Because of the geographical patterns of studies, the amount of detail actually ends up being heavily skewed towards the global north. Perhaps make some stronger selections, and some more generalisations, with pointers towards the sections which contain the geographical details. [Dave Frame, New Zealand]	Accepted. The text was shortened and regional information was condensed. The text now uses AR6 regions acronyms to save space, while remaining comprehensive.
34703	6	1	9	50	I've read the Executive Summary for every chapter in the WG1 report. In my opinion, Chapter 11 has done the best job in this respect. My compliments to the CLA and LA team. [Russell Vose, United States of America]	Noted. Thank you for the positive feedback!
5593	6	1	9	50	There are no informations about the coastal floodings, although this thematic is in the chapters 9 and 12, why ? [Benoit Laignel, France]	Rejected. Coastal flooding is covered in Chapters 9 (and partly 12), which is why we are not providing summary assessments on this topic in the SPM. However, we have now pointed the reader to Chapter 9 for background information.
79633	6	1	9	52	This chapter places a focus on the assessment of past changes and their attribution to causes, and projected future changes in extremes, for few different types of extremes, such as temperature extremes, heavy precipitation, floods, droughts, and storms in separate sections. But, little effort is devoted to sector-relevant extremes such as humidity (important for both agricultural and health indices), wind speed and direction (important for health indices, building design, energy, transportation, etc.), Sea Surface Temperatures (SSTs; useful for marine applications and in relation to the onset and variability of the El Niño-Southern Oscillation (ENSO), Indian Ocean Dipole (IOD)), onset and cessation dates for monsoon, snow fall, snow depth, snow-water equivalent, days with snowfall and hydrological parameters (particularly important for mid-and high latitude applications). Perhaps, it would help readers of the report if the authors team provide couple of justifications in the Framing Section as to why extremes wind, humidity and others are not considered in this Chapter? The assessment of changes in such extremes would facilitate the Chapter 12 transition of changes in metrics that quantify impact-relevant hazards, supplemented by assessments of other hazards and supported by the Atlas, and foster key handshake with the WGII report. [Wilfran MOUFOUMA OKIA, Switzerland]	Rejected. Sector-relevant indices are addressed in Chapter 12, which focuses on climatic impact drivers.
62853	6	3	6	12	Consider adding a brief definition of what "extreme events* are to the executive summary [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Too much textbook like. But the definition is found in the chapter.
18679	6	3	6	12	A brief definition of compound and concurrent would be helpful to the readers. [Govindasamy Bala, India]	Accepted. A definition is now included "multivariate extremes, also termed compound events"
62371	6	4	6	7	Here we need to be more specific about the details of extreme climate and weather events (hailstorm, heatwaves, etc.) "This chapter assesses changes in weather and climate extremes with a regional focus, including observed and projected changes, as well as their attribution. The considered extremes include temperature extremes, rainfall extremes, floods, droughts, storms (including tropical cyclones), as well as compound events." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We provided a bit more detail on the material covered (e.g. "heavy precipitation and pluvial floods", "river floods") and where other topics are covered in the report (e.g. coastal floods).
79105	6	5	6	5	floods, droughts, storms, xxx, are parts of rainfall extremes, so can be omitted [Andong Shi, Sweden]	Rejected. Rainfall is only one factor contributing to floods or droughts. And storms also affect wind extremes.
125799	6	5	6	5	"rainfall" or "precipitation"? Is snow excluded from assessment? [Trigg Talley, United States of America]	Noted. The text now uses "precipitation" if snow is included.
125801	6	5	6	7	This passage leaves it unclear whether coastal flooding is treated in this chapter or in Chapter 9. [Trigg Talley, United States of America]	Accepted. Now clarified that Ch11 addresses pluvial floods and river floods, but that coastal floods are addressed in Ch9 (and partly 12).
62743	6	5			While listing the considered extremes, storms should be elaborated on slightly. The addition of (including tropical cyclones) is good, but doesn't tell what other storms are considered. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The main subcategory considered are tropical cyclones. The other categories are only partly addressed because of limited literature and evidence. Because of space limitations it does not seem justified to provide more details on storms at this level of the ES. Note that these other categories (e.g. severe convective storms) are addressed in the paragraphs on storms.
62769	6	6	6	6	Compound events should be defined because it could be misunderstood here. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. A definition is now included "multivariate extremes, also termed compound events"
9125	6	6	6	6	define "compound events". Note that page 6 line 17 refers to concurrent extremes, which is a subset of compound events [Kevin Hennessy, Australia]	Accepted. A definition is now included in parentheses "(multivariate and concurrent extremes)"
82743	6	7	6	7	May be better to say "quasi-global coverage", as there are substantial coverage gaps even today for many variables. [Blair Trewin, Australia]	Noted. Text no longer included.
107383	6	7	6	7	"observations with global coverage" - better change to "near/quasi-global" as our observational datasets mostly do not have full global coverage [Markus Donat, Spain]	Noted. Text no longer included.
49939	6	7	6	9	What kind of observations are being mentioned here? On the heels of the discussion of things that are instead in Chapter 9, this sentence is confusing. It would be helpful to be specific, something like, "Reliable observations of weather and climate extremes..." [Daniel Gilford, United States of America]	Noted. Text no longer included.
14617	6	9	6	11	Here it is stated that "future projections are provided as a function of global warming levels", but all regional projections given in Table 11.4-11.9 appear to be by scenario (going by the references)?. Or have the projections by scenario been mapped onto GWLs. If so this should be mentioned and an example of how this was done should be provided. [Roshanka Ranasinghe, Netherlands]	Noted. We have clarified in the text that while we focus on providing projections as function of global warming levels, information on the translation to scenarios is also provided (new Rejected. 11.1)
68703	6	10	6	11	It is not clear what is meant by 'provided as a function of global warming levels'. Do you mean 'provided as a function of global mean surface temperature'? [Bodeker Greg, New Zealand]	Rejected. "Global warming levels" or "level of global warming" are well established terms. More information can be found in the provided reference (new Rejected. 11.1)
125803	6	10	6	11	Maybe it should be obvious to the reader what non-quantitative phrase "global warming levels" means (i.e., increase of GMST), but it might be helpful to define it on first usage. [Trigg Talley, United States of America]	Rejected. "Global warming levels" or "level of global warming" are well established terms. More information can be found in the provided reference (new Rejected. 11.1)
68705	6	11	6	11	It is not clear what this means without knowing what metric is being referred to. Do you mean that the 'timing at which global mean surface temperature reaches different thresholds above preindustrial levels under different emission scenarios is assessed'? [Bodeker Greg, New Zealand]	Rejected. "Global warming levels" or "level of global warming" are well established terms. More information can be found in the provided reference (new Rejected. 11.1)
19321	6	11			The definition of the compound event listed on page 11-12 should be moved to page 11-6 line 6. [Mansoureh Kouhi, Iran]	Accepted. A definition is now included "multivariate extremes, also termed compound events"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33093	6	14	6	14	major has no magnitude; propose to remove "major" [Sahar Tajbakhsh Mosalman, Iran]	Accepted. Removed "major".
96107	6	14	6	14	Please be clear that observations underlying the text start in 1950 only. This is been mentioned in the SPM and needs to be added here. [Nicole Wilke, Germany]	Rejected. This is not necessary as the ES explicitly states "Assessments of past changes and their drivers are from 1950 onward, unless indicated otherwise.".
21119	6	14	6	14	major has no magnitude; propose to remove "major" [Iman BABAEIAN, Iran]	Accepted. Removed "major".
32763	6	14	6	14	major has no magnitude; propose to remove "major" [sadegh zeyaeyan, Iran]	Accepted. Removed "major".
130565	6	14	6	15	This highlighted sentence has not delivered key message since AR5. [Panmao Zhai, China]	Rejected. The following sentence describes what are the mentioned advances.
18677	6	16	6	16	Why are heat waves missing from the list of extremes where there is evidence in the recent period? [Govindasamy Bala, India]	Noted. The text highlights areas in which substantial progress and more robust evidence is available. In the case of hot extremes, including heatwaves, the evidence was already very robust at the time of the AR5.
24079	6	17	6	17	Is this increase in land area relative to AR5 or relative to 1950? Or what? [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The increase is since 1950. This is specified at the beginning of the ES for all the assessed observed changes: "Assessments of past changes and their drivers are from 1950 onward, unless indicated otherwise".
51559	6	17	6	17	Is this increase in land area relative to AR5 or relative to 1950? Please clarify. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The increase is since 1950. This is specified at the beginning of the ES for all the assessed observed changes: "Assessments of past changes and their drivers are from 1950 onward, unless indicated otherwise".
28923	6	17			Can a confidence statement and time-scale be assigned to the increase in areas affected by concurrent extremes? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This assessment is at high confidence (see ES text).
68707	6	20	6	20	I would suggest 'in extremes are projected to continue'. [Bodeker Greg, New Zealand]	Not applicable. This sentence is no longer included.
51567	6	20	6	20	It would be useful to state explicitly that we have already seen changes in extremes (for example over 0.5°C of warming) as shown nicely by Table 11.1 (starting on page 20), as well as noting that observed changes will continue into the future. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. A new synthesis paragraph on observed changes has been included in the ES.
17123	6	20	6	20	I suggest these changes: Many of the observed changes in extremes are predicted to will continue in the future. [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This sentence is no longer included.
17125	6	20	6	22	This chapter would focus on a regional scale, but why the author mention a claim of "An additional half degree ... at the global scale." I should underlined that this statement is true, but please adjust it to be more specific to a regional scale claim. Thanks [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence has been revised to also mention regional changes.
107379	6	20	6	27	The summary of future changes reads fully certain at the moment ("will continue") and should better reflect the conditional nature of these statements, e.g. "will if GHG concentrations continue to rise", or "are expected to"? [Markus Donat, Spain]	Not applicable. This sentence is no longer included.
39239	6	20			can you consider having a confidence level in this statement or finding? [Lourdes Tibig, Philippines]	Not applicable. This sentence is no longer included.
62845	6	22	6	25	Consider replacing the word "limited" by "reduced/smaller" in this context as it may refer to the fact that 1.5 degree changes will be limited and I believe the authors want to refer to the comparison of 1.5 vs 2 degree related changes in extremes [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This sentence is no longer included.
24081	6	23	6	23	"Continues to strengthen" [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This sentence is no longer included.
68709	6	23	6	23	Replace 'continues strengthening' with 'continues to strengthen'. [Bodeker Greg, New Zealand]	Not applicable. This sentence is no longer included.
51561	6	23	6	23	Suggested edit: "Continues to strengthen" instead of "continues strengthening" [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This sentence is no longer included.
33095	6	24	6	24	Replace the word "stabilized" with "limited" [Sahar Tajbakhsh Mosalman, Iran]	Not applicable. This sentence is no longer included.
125805	6	24	6	24	What does "limited" mean here? Does it mean "not so bad" (a common meaning of that word).or does it mean "smaller than if not stabilized at 1.5°C"? [Trigg Talley, United States of America]	Not applicable. This sentence is no longer included. The text has been substantially revised to clarify the implications of increases in global warming for extremes.
21121	6	24	6	24	Replace the word "stabilized" with "limited" [Iman BABAEIAN, Iran]	Not applicable. This sentence is no longer included.
32765	6	24	6	24	Replace the word "stabilized" with "limited" [sadegh zeyaeyan, Iran]	Not applicable. This sentence is no longer included.
21129	6	25	6	25	replace term "higher levels of global warming" with "or more" [Iman BABAEIAN, Iran]	Not applicable. This sentence is no longer included.
79103	6	25	6	25	omit 'levels of global warming' [Andong Shi, Sweden]	Not applicable. This sentence is no longer included.
68711	6	25	6	26	I would rather suggest 'Climate models are generally suitable for projecting changes in extremes'. [Bodeker Greg, New Zealand]	Not applicable. The sentence has been replaced by a more extensive assessment of the performance of climate models for extremes (FGD, page 6, lines 41-48).
24083	6	25	6	27	"suitable overall" would read better but more substantively what does suitable mean exactly given that the accuracy depends on the extreme (and therefore presumably the accuracy for some extremes is not great) [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. More background has been provided on the performance of climate models for extremes (FGD, page 6, lines 41-48)
51563	6	25	6	27	"Climate models are overall suitable for projections of changes in extremes, but their accuracy depends on the considered extreme": suggest that 'suitable overall' would read better here, but more substantively what does suitable mean exactly given that the accuracy depends on the extreme (and therefore presumably the accuracy for some extremes is not great)? It would be helpful to provide some context here around which extremes are more accurately represented by climate models, and those that we have less confidence in. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. More background has been provided on the performance of climate models for extremes (FGD, page 6, lines 41-48)
23065	6	26	6	26	Accuracy pre-supposes that the truth is known / knowable which cannot be true (in some limit) so I would suggest using suitability instead of accuracy as it is, anyway, suitability which strictly speaking is quantifiable using the available data and tools. [Peter Thorne, Ireland]	Not applicable. The sentence has been replaced by a more extensive assessment of the performance of climate models for extremes (FGD, page 6, lines 41-48).
18681	6	26	6	26	"their accuracy" is probably not the right choice of words here. A better choice would be "the level of uncertainty in the projections" [Govindasamy Bala, India]	Not applicable. The sentence has been replaced by a more extensive assessment of the performance of climate models for extremes (FGD, page 6, lines 41-48).
33097	6	31	6	31	chnge "hot" to "warm" [Sahar Tajbakhsh Mosalman, Iran]	Rejected. The term "hot extremes" is well established.
21123	6	31	6	31	chnge "hot" to "warm" [Iman BABAEIAN, Iran]	Rejected. The term "hot extremes" is well established.
32767	6	31	6	31	chnge "hot" to "warm" [sadegh zeyaeyan, Iran]	Rejected. The term "hot extremes" is well established.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23067	6	32	6	32	globally rather than on global scale? Also applies to similar cases elsewhere in ES [Peter Thorne, Ireland]	Noted. "Global scale" is meant (i.e. when averaged at global scale).
62847	6	32	6	36	Does "on all continents" refer to hot days only or is it valid for "increase in the number of warm days and nights, an increase in the intensity and duration of heatwaves, and a decrease in the number of cold days and nights" in the previous lines? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This sentence is no longer included. The revised text includes more regional aspects.
40553	6	33	6	33	No mention in the SPM that there has already been "an increase in the intensity and duration of heatwaves" (Ch11 ES) [TSU WGI, France]	Noted. This is a comment on the SPM rather than the chapter 11 ES.
62849	6	33	6	36	Clarify "warm" (line 33) vs. "hot" (line 36) days [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The term "hot extremes" is well established in the literature, and includes "warm days" which are moderate hot extremes. The FGD text only mentions "hot extremes".
5683	6	34	6	35	Please clarify: does "increasing temperature" mean that the cold extremes get warmer (increase in temperature) or do they get colder (increase in temperature that has a negative sign)? [Joachim Rock, Germany]	Not applicable. This sentence is no longer included.
35149	6	35	6	25	replace term "higher levels of global warming" with "or more" [Sahar Tajbakhsh Mosalman, Iran]	Not applicable. This sentence is no longer included.
68713	6	35	6	35	So far not mention has been made of 'trends' so you need to be explicit here in what trends you are talking about. Are you talking about trends in the mean of some distribution, trends in threshold exceedances, or trends in some other statistic of the distribution? [Bodeker Greg, New Zealand]	Not applicable. This sentence is no longer included.
109337	6	37	6	37	Add "the magnitude of" before "temperature" for clarity. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This sentence is no longer included.
108843	6	37	6	37	Clarify that this applies to trends in the severity of extremes or in other words the temperature during these events. [Erich Fischer, Switzerland]	Not applicable. This sentence is no longer included.
40549	6	37	6	38	No mention here that there is "...medium confidence that deforestation has contributed about 1/3 of the total warming of hot extremes in some mid-latitude regions since pre-industrial times" [TSU WGI, France]	Rejected. This point is implicitly accounted for with "forcing from land use and land-cover change" in the following sentence: "The effect of enhanced greenhouse gas concentrations on extreme temperatures is moderated or amplified at the regional scale by regional processes such as soil moisture or snow/ice-albedo feedbacks, by regional forcing from land use and land-cover changes, or aerosol concentration, ..."
76659	6	37			I think "by 50% to 200%" should be erased or the whole sencence rephrased, "Trends in values of warm temperature extremes..." because 1) Expressing a change using "%" has not clear meaning for indicator such as frequency of hot day or warm nights 2) probably the sentence was written thinking about warm extremes. Its meanining for cold extremes is not clear to me (in general indicators of cold increase decreasewith global warming) [Piero Lionello, Italy]	Not applicable. This sentence is no longer included.
108841	6	39	6	41	In order not to be accussed for cherry-picking periods it should be acknowledged that in some regions trends are less clear when starting earlier, e.g. in the dust-bowl period over the contiguous US. [Erich Fischer, Switzerland]	Not applicable. This sentence is no longer included. As mentioned at the beginning of the ES, trends since 1950 are considered by default.
107381	6	40	6	40	"trends" - better explicitly specify here that this refers to "observed changes" or "observed trends" [Markus Donat, Spain]	Not applicable. This sentence is no longer included.
70935	6	41	6	41	Do you mean "South America"? I have no idea what "Southern America" means; it could be Alabama [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This sentence is no longer included.
108839	6	43	6	43	The quantitative interpretation of the term "main contributor" is somewhat ambiguous. Is it more than half or more than the contribution of other factors, which may imply that it is not that large if there would be a small contribution from many factors. [Erich Fischer, Switzerland]	Accepted. The text was changed to "Human-induced greenhouse gas forcing is the main driver..."
62373	6	43	6	46	Some word need to be replaced with proper technical words like rise in temperature/human induced change "It is extremely likely that human influence is the main contributor to the observed increase in the likelihood and severity of hot extremes and the observed decrease in the likelihood and severity of cold extremes on global scales." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Sentence has been substantially revised. Also not clear what word was suggested to be replaced.
39241	6	43	6	47	There seem to be ambiguity here in the confidence levels attached to the findings. The first statement carres "extremely likely", while in the second (pertains to continental scale), it is "very likely". Then in the third, it is "medium confidence for that of the attribution of some recent extreme events. [Lourdes Tibig, Philippines]	Rejected. The confidence and likelihood levels can be different on global vs regional scale because the considered scale affects the signal-to-noise ratio.
29455	6	43		53	the release of CO2 (on land) in the last decade is having significant effect on temperature extremes. [Babatunde Oyekan, Nigeria]	Noted. More details have been included in the text on how human influence impacts climate extremes, in particular related to greenhouse gas emissions.
24107	6	45	6	47	To have medium confidence in the evidence suggesting something seems an extremely weak statement to me, implying there is considerable doubt even whether the evidence is suggesting it. I suspect the evidence is suggesting it with quite high confidence but I would prefer a likelihood statement here if supported? In any event the statement as it is written looks un-supportedly weak to me. [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The text has been substantially revised. The mentioned sentence is no longer included, but a sentence on this topic has been included at the beginning of the ES with a likelihood statement: "Some recent hot extreme events would have been extremely unlikely to occur without human influence on the climate system"
51565	6	45	6	47	"The available evidence suggests that some recent extreme events could not have occurred without human influence (medium confidence)." To have medium confidence in the evidence suggests something comes across as an extremely weak statement, implying there is considerable doubt even if the evidence is suggesting it. Suggest revisiting this to see if this statement can be supported with a likelihood statement here or high confidence, if supported? In any event the statement as it is written looks un-supportedly weak to me. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The text has been substantially revised. The mentioned sentence is no longer included, but a sentence on this topic has been included at the beginning of the ES with a likelihood statement: "Some recent hot extreme events would have been extremely unlikely to occur without human influence on the climate system"
125807	6	46	6	47	That seems like a very strong statement. One wonders if there is some physical limit that would absolutely have prevented these events from happening in the absence of human influence. [Trigg Talley, United States of America]	Noted. The text has been substantially revised. The mentioned sentence is no longer included, but a sentence on this topic has been included at the beginning of the ES with a likelihood statement: "Some recent hot extreme events would have been extremely unlikely to occur without human influence on the climate system"
108845	6	49	6	49	Consider being more specific at what variables are affected by urbanization. I assume it is only robust for warm nighttime temperatures [Erich Fischer, Switzerland]	Accepted. This is revised in the FGD version.
79957	6	49	6	49	",or aerosols." .. Maybe add more explanation or the aspects of aerosols here? Such as ",or aerosols emissions"" ,or aerosols burden? " ",or aerosols optical depth?" Because right before the "aerosols", "land use and land cover changes" specifiied the aspects of Land. " [Fei Luo, Netherlands]	Accepted. Text was changed to "aerosol concentrations".
62851	6	50	6	51	Consider adding a confidence level for "Changes in aerosol concentrations have affected trends in hot extremes in some regions, with the presence of aerosols leading to attenuated warming, in particular from 1950-1980" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The revised text in the FGD version is "Changes in anthropogenic aerosol concentrations have likely affected trends in hot extremes in some regions"
105151	6	51	6	52	This statement about the impact of irrigation and land use change seems rather strong, even if “only” assessed with medium confidence. In this case, it would probably be wise to use language that is slightly less assertive, for example, by replacing “have attenuated” with “may have attenuated”. [Francis Zwiers, Canada]	Rejected. This statement is based on robust literature evidence. See Sections 11.1.6 and 11.3.2 in FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
76661	6	51			concerning effect of aerosol: this is certainly true for average temperatures. Also for extremes? [Piero Lionello, Italy]	Noted. The text has been revised. An assessment on the role of aerosols for trends in extremes is provided in the main text (see in particular Section 11.1.6)
62745	6	52			I do not recommend the word "attenuated", especially next to the word "increased". This can lead to misunderstanding of the given information. Simpler language should be considered. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Wording appears understandable and was not strongly criticized by other reviewers. Not clear what other wording could be used instead.
117049	6		6		be explicit on "human influence' (through emissions of GHG?). [Valerie Masson-Delmotte, France]	Accepted. Where possible, it was clarified if the attribution is done to greenhouse gas forcing more specifically or human influence in general. The literature does not always distinguish the effects of greenhouse gas forcing vs other forcings for climate extremes attribution. For general statements and temperature, the attribution can be done to greenhouse gas forcing specifically (see revised text)
108847	7	1	7	9	I think it would be necessary to specify that this statement applies to the forced response or long-term changes. Internal variability is large e.g. for TNn and could easily mask the forced response for many decades over some regions. [Erich Fischer, Switzerland]	Rejected. Too detailed for ES.
105153	7	2	7	2	Delete “throughout the 21st century”. Saying that increases will occur throughout the 21st century implies that stabilization, such as might occur under RCP 2.6, is considered to be impossible by this chapter. [Francis Zwiers, Canada]	Accepted. The text now states that increases will happen "with increasing global warming levels".
32769	7	4	7	4	remove "+" before 1.5oC [sadegh zeyaeyan, Iran]	Accepted.
33099	7	4	7	4	remove "+" before 1.5oC [Sahar Tajbakhsh Mosalman, Iran]	Accepted.
21125	7	4	7	4	remove "+" before 1.5oC [Iman BABAEIAN, Iran]	Accepted.
30679	7	6	7	6	In some places the entire report refers to 'heat waves' (as here), and in others to 'heatwaves' (e.g., Ch 11, p. 9, line 30). We should be uniform in our terminology. [Ian Simmonds, Australia]	Rejected. Both spellings are common in the literature.
68715	7	7	7	8	This statement that 'changes in the magnitude of temperature extremes are proportional to global warming levels' seems to contradict the statement on the previous page that says 'Trends in temperature extremes are generally larger (by ca. 50% to 200%) than those in global mean temperature'. A clear distinction needs to be made in all cases whether youare talking about the magnitude or frequency/likelihood of extreme events. [Bodeker Greg, New Zealand]	Rejected. The two statements are not contradictory. The changes in intensity are proportional but larger (the ratio is more than 1). The word "magnitude" was replaced with "intensity" to clarify the text. The overall text was substantially revised to make it clearer.
113475	7	7	7	9	In general, but especially when both are being referred to in the same sections, add 'high/positive/hot' to 'temperaure extremes' to clarify you do not mean cold extremes. [Diego Miralles, Belgium]	Noted. The text was revised to make it more explicit. The word "magnitude" was replaced with "intensity".
76663	7	7		9	this is true for warm temperature extremes.I suggest to add "warm" [Piero Lionello, Italy]	Noted. The intensity of both hot and cold extremes are changing proportionally to global warming. This was clarified in the revised text. The last sentence was revised and only mention hot extremes to make it clearer.
23069	7	8	7	9	As written this is a little ambiguous. The likelihood of hot extremes increases exponentially but presumably that of cold extremes decreases (perhaps also exponentially)? [Peter Thorne, Ireland]	Noted. The text was revised. It now only refers to hot extremes and mentions the non-linear characteristics of the changes in frequency.
105155	7	8	7	9	This statement about the exponential increase in likelihood needs to be better nuanced. First, this is evidently about warm extremes rather than cold extremes, since the likelihood of cold extremes does not increase. Second, the change in likelihood as a function of temperature will take the form of a sigmoid function, with likelihood saturating at 1 for high enough warming, and therefore “exponential increase” would only roughly describe the lower part of the sigmoid function, where the curvature is positive (i.e., positive 2nd derivative). For some types of extremes (e.g., changes in the frequency of extreme warm regional seasonal mean temperatures as defined for the current climate), projected warming under, for example RCP8.5, leads to saturated states with likelihoods very close to the asymptotic limit of 1. For example, see Sun et al., 2014 (doi:10.1038/NCLIMATE2410), Fig. 4. [Francis Zwiers, Canada]	Accepted. The text was revised taking into account the comments from the reviewer.
112711	7	9			exponential? Are you sure? [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The text was revised taking into account the comment from the reviewer (replaced "exponential" with "non-linear"; see also answer to comment #105155).
62375	7	11	7	11	I have suggestion to to add erratic "Heavy and erratic precipitation" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. "Erratic" precipitation is not a commonly used term in the literature.
113481	7	11	7	40	It would be good in this summary to clarify the (past/future) relative increase (likelihood and confidence) in P extremes compared to the increase in just average P. Some effort is already done for T. [Diego Miralles, Belgium]	Noted. The question of changes in mean vs extremes is addressed in FAQ 11.1 (including a comparison of changes in mean precipitation vs extreme precipitation). A more in-depth discussion would lie beyond the scope of this chapter, as it does not assess changes in mean temperature or mean precipitation (changes in mean precipitation are assessed in chapter 8). The discussion about changes in mean temperature vs extreme temperature is limited to the scaling to global mean warming.
109591	7	13	7	13	The word "some" should be inserted before "land regions" as many land regions have not experienced any significant change. [Reynold Stone, Trinidad and Tobago]	Noted. The changes are observed in a majority of land regions with good observational coverage. This is explicitly stated in the FGD text.
86533	7	13	7	23	It is recommened that quantitative information (probably as ranges per region) is added in order to give an impression of the significance of the trends. Are we talking about 0,5%, 5% or 50% increases which have been detected? [Jochen Harnisch, Germany]	Rejected. Not enough space for this level of detail.
29457	7	13		32	enhanced and induced rainfall using cloud nuclei is also leads to heavy rainfall in some developed countires where cyclones are expected to be stormy with heavy precipitation. Some prefers to have heavy precipitation to heavy storms. If this continues for a long time, there is no way precipitation won't increase more and more. [Babatunde Oyekan, Nigeria]	Rejected. Not enough space for this level of detail.
76665	7	13			has intensified over "MOST" or "MANY" land regions.It is not valid for all land regions [Piero Lionello, Italy]	Noted. Changed to "a majority of land regions with good observational coverage".
28925	7	13			suggest "on global scale over land regions" -->"over the global land"; possibly also: "It is likely that the land area experiencing increases in annual maximum daily or 5-day precipitation amount is greater than the area experiencing decreases since 1950 for regions with sufficient observation coverage for assessment." [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text was changed to clarify that the changes apply on global scale as well as to a majority of land regions with good observational coverage.
105157	7	15	7	15	Replace the somewhat awkward “more regions than it has increased over” with “the majority of land regions”. Majority means more than half, and by inference, if there is increase in more than half, then there is decrease in less than half. [Francis Zwiers, Canada]	Noted. The text was simplified and mentions now that the observed changes are found in a majority of land regions with good observational coverage.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
82745	7	16	7	16	"including" is redundant here. [Blair Trewin, Australia]	Accepted.
109593	7	16	7	17	It is not accurate to state with high confidence that heavy precipitation has increased over North America. No trend has been observed in the southern and western parts of the United States (Hoerling et al. 2016. Characterising recent trrends in U.S. heavy precipitation. Journal of Climate, vol. 29, no. 7, pp. 2313-2332). Also, no trend has been detected in Canada (see page 56 of Chapter 11, line 2). [Reynold Stone, Trinidad and Tobago]	Rejected. The assessment does not imply that increases are observed everywhere within the continent but that they are identified for continental-scale analyses.
76667	7	16			"at the continental scale" suggests thta this has happened everywhere in the named continents. "This is also true over most areas of three continents ..." [Piero Lionello, Italy]	Rejected, "At continental scale" only implies that this applies to continentally-aggregated statistics.
125809	7	17	7	17	"larger" than what? [Trigg Talley, United States of America]	Not applicable. This sentence is no longer included.
29275	7	20	7	20	Some of the phrasing here describing regions is a bit awkward (e.g., "north South America", and "southeast South America"). Even adding suffixes like "northern South America" would help to improve the flow. [Andra Garner, United States of America]	Accepted. Regional information has now been included using the IPCC AR6 regions acronyms.
62603	7	20	7	20	please change “north South America” to “northern South America” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Regional information has now been included using the IPCC AR6 regions acronyms.
9127	7	22	7	23	"Ëlsewhere" refers to regions excluded in lines 16-22. However, it's incorrect to say there is generally low confidence in observed trends in heavy precipitation in these excluded regions due to data limitations. In some regions, there are insignificant trends in heavy precipitation without data limitations, e.g. southern and eastern Australia. Therefore, I suggest replacing this sentence with "Regional decreases in heavy precipitation have been observed in [list regions], insignificant changes have been observed in [list regions] and unquantifiable changes have occurred in other regions due to data limitations". [Kevin Hennessy, Australia]	Accepted. Sentence was removed.
76669	7	23			add "and large interannual variability" after "due to data limitations".The issues is not only lack of data. Non rarely, interannual variability is too large to identify the signal. In my experience I have considered 15 coastal stations along the whole Mediterranean coastline and did not find any statistically significant change in the number of intense precipitation events (Reale M, P Lionello P (2013) Synoptic climatology of winter intense precipitation events along the Mediterranean coasts. Nat Hazards Earth Syst Sci, 13:1707–1722. doi:10.5194/nhess-13-1707-2013) ...I think more emphasis should be given to this issue (e.g. Fischer, E. M., and Knutti, R. (2014), Detection of spatially aggregated changes in temperature and precipitation extremes, Geophys. Res. Lett., 41, 547– 554, doi:10.1002/2013GL058499. van den Besselaar, E.J.M., Klein Tank, A.M.G. and Buishand, T.A. (2013), Trends in European precipitation extremes over 1951–2010. Int. J. Climatol, 33: 2682-2689. doi:10.1002/joc.3619. I DO NOT MEAN TO DENY THE WIDESPREAD OBSERVED SIGNIFICANT INCREASE OF EXTREMES, BUT TO MAKE CLEAR THAT IT IS NOT OBSERVED EVERYWHERE AND THAT INDIVIDUAL EXCEPTIONS DO NOT DISPROVE THE OVERALL INCREASING TREND [Piero Lionello, Italy]	Not applicable. This sentence is no longer included.
13655	7	24	7	24	change thatextreme by that extreme [Maria Amparo Martinez Arroyo, Mexico]	Noted, seems to apply to line 34, not 24. The typo was corrected.
108849	7	25	7	25	Can you be more quantitative with what you mean by "main cause". More than half of the intensification? [Erich Fischer, Switzerland]	Noted. Changed wording to "main driver". This indeed implies more than half.
109595	7	25	7	26	This statement seems to be biased because the role of natural internal variability due to natural oscillations (e.g. AMO, PDO, NAO, ENSO etc.) has been virtually ignored in this report. This, despite the huge peer-reviewed literature available showing precipitation patterns worldwide are significantly influenced by these oscillations. (here are some examples: Macdonald, N. and H. Sangsteer. 2017. High-magnitude flooding across Britain since AD 1700. Hydrol. Earth Syst. Sci., vol. 21, pp. 1631-1650; Malik et al. 2017. Decadal to multi-decadal scale variability of Indian summer monsoon rainfall in the coupled ocean-atmosphere-chemistry climate model SOCOL-MPIOM. Climate Dynamics, vol. 49, pp. 2551-3572; Valdes-Pineda, R. et al. 2018. Multi-decadal 40- to 60-year cycles of precipitation variability in Chile (South America) and their relationship to the AMO and PDO signals. Journal of Hydrology, vol. 556, pp. 1153-1170; Riechelmann, S. et al. 2017. Sensitivity of Bunker Cave to climatic forcings highlighted through multi-annual monitoring of rain-, soil-, and dripwaters. Chemical Geology, vol. 449, pp. 194-205; Lapointe, F. et al. 2017. Influence of North Pacific decadal variability on the western Canadian Artic over the past 700 years. Clim. Past, vol. 13, pp. 411-420; Lim, J. et al. 2017. Holocene changes in flooding frequency in South Korea and their linkage to centennial- to millennial-scale El Nino-Southern Oscillation activity. Quaternary Research vol. 87, no. 1, pp. 1-12; Park, J. et al. 2017. The combined influence of Pacific decadal oscillation and Atlantic multi-decadal oscillation on central Mexico since the early 1600s. Earth and Planetary Science Letters, vol. 464, pp. 1-9). [Reynold Stone, Trinidad and Tobago]	Noted. The role of natural internal variability is mentioned in the FGD version of the ES: "The effect of enhanced greenhouse gas concentrations on extreme temperatures is moderated or amplified at the regional scale by regional processes such as soil moisture or snow/ice-albedo feedbacks, by regional forcing from land use and land-cover changes, or aerosol concentrations, and decadal and multidecadal natural variability."
39337	7	25	7	27	Can you possibly add the reason for "excepting North Atlantic"?This is the ES, so try to include all known findings. Policy makers usually just read the ES. [Lourdes Tibig, Philippines]	Noted. It is not clear what text the reviewer is referring to: "excepting North Atlantic" is not included on page 5, lines 25-27.
113477	7	26	7	26	Add space after point at '.T'. There are several minor errors like this, like in all chapters I have read. I am not going to comment on them because I am quite sure this will be proofread at some point and corrected. [Diego Miralles, Belgium]	Not applicable. This sentence is no longer included. The chapter has been proofread.
9129	7	26	7	26	replace "in land regions" with "över land regions", consistent with page 7 line 13 [Kevin Hennessy, Australia]	Accepted. This was revised as suggested.
108851	7	26	7	27	Is the first part of the statement also for land only. If so it is necessary to be more explicit. [Erich Fischer, Switzerland]	Not applicable. This sentence is no longer included.
125813	7	26	7	27	[PRECISION] What precisely is meant by a "global increase in annual max ... precipitation?" For example, "global warming" refers to an increase in global mean temperature, but doubt the phrase used in this passage means an increase in the global mean annual max T. Here and elsewhere, more precision is needed in language. [Trigg Talley, United States of America]	Noted. The text has been simplified and made clearer.
125811	7	26	7	28	Not clear what this sentence is saying. [Trigg Talley, United States of America]	Not applicable. The text has been revised to make it clearer. This sentence is no longer included.
17127	7	26	7	28	I suggest these changes: "One of the evidence includes attribution of the observed global increase in annual maximum one-day and five-day precipitation to human influence (high confidence). Furthermore, a large land fraction with intense human activity land showed more enhanced extreme precipitation, and larger probability in record-breaking one-day precipitation. [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. It is unclear what the reviewer means with "a large land fraction with intense activity land". The text has been substantially revised.
125815	7	27	7	27	What is a "large fraction" of land? [Trigg Talley, United States of America]	Not applicable. This text is no longer included.
105159	7	27	7	28	Replace “a large fraction of land showed enhanced …. one-day precipitation” with “a large fraction of land that shows enhanced extreme precipitation, and a larger than expected probability of record-breaking one-day precipitation”. [Francis Zwiers, Canada]	Not applicable. This text is no longer included.
125817	7	28	7	28	"Larger" than what? Is this supposed to mean "increased" (i.e., over time)? [Trigg Talley, United States of America]	Not applicable. This text is no longer included.
23071	7	28	7	30	Should this not have a confidence assigned? [Peter Thorne, Ireland]	Not applicable. This text has been substantially revised. It is still indicated as a statement of fact: "Evidence of a human influence on heavy precipitation has emerged in some regions."
108853	7	29	7	29	Is the trend not detectable or not attributable? [Erich Fischer, Switzerland]	Not applicable. This text is no longer included.
28927	7	29			can this statement "less detectable... but evidence is emerging." be usefully converted to a low/medium confidence statement? Perhaps only if medium confidence rather than low confidence. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This text is no longer included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
125819	7	30	7	30	The phrase "evidence is emerging" implies knowledge that the evidence will be greater in the near future than it is at present. How can such a thing be known? [Trigg Talley, United States of America]	Noted. The text has been revised. The revised sentence is "Evidence of a human influence on heavy precipitation has emerged in some regions."
105161	7	30	7	30	Insert “the” before “intensification”. [Francis Zwiers, Canada]	Not applicable. This sentence has been substantially revised.
23073	7	30	7	32	Again the lack of confidence is a possible issue here? [Peter Thorne, Ireland]	Not applicable. This text is no longer included. The revised text explicitly refers to Section 11.9 in which tables with assessments for the AR6 regions are provided.
39243	7	30	7	32	No uncertainty language in this statement? [Lourdes Tibig, Philippines]	Not applicable. This text is no longer included. The revised text explicitly refers to Section 11.9 in which tables with assessments for the AR6 regions are provided.
79107	7	34	7	34	NO space gap, same on line 38 [Andong Shi, Sweden]	Not applicable. The text has been substantially revised.
49941	7	34	7	34	Space missing between "thatextreme" [Daniel Gilford, United States of America]	Not applicable. The text has been substantially revised.
7447	7	34	7	34	Typo : « thatextreme » please add a space between « that » and « extreme » [Geremy PANTHOU, France]	Not applicable. The text has been substantially revised.
64837	7	34	7	34	missing space between that and extreme [Martin Ménégoz, France]	Not applicable. The text has been substantially revised.
11643	7	34	7	34	insert a space between “that” and “extreme” [Amy East, United States of America]	Not applicable. The text has been substantially revised.
42425	7	34	7	34	Typo: thatextreme -> that extreme [Joan Bech, Spain]	Not applicable. The text has been substantially revised.
6849	7	34	7	34	"thatextreme precipitation" to read as "that extreme precipitation" [Constantinos Cartalis, Greece]	Not applicable. This text has been substantially revised.
89281	7	34	7	34	‘thatextreme’ to ‘that extreme’ [Tinghai Ou, Sweden]	Not applicable. This text has been substantially revised.
109597	7	34	7	35	This assertion is based on the expectation that the hydrological cycle will intensify in response to global warming. Yet, despite recent warming, there is little unequivocal evidence of such an acceleration at the global scale (see Mirales, D.G. et al. 2013. El Nino-La Nina cycle and recent trends in continental evaporation. Nature Climate Change 4, 122-126). Also, the risk of having heavy precipitation under warming global temperature seems to have been overestimated (see Zhou, Y et al. 2016. On the detection of precipitation dependence on temperature. Geophysical Research Letters, vol. 43, issue 9, pp. 4555-4565). The assertion should therefore be less certain than indicated by the term "likely". [Reynold Stone, Trinidad and Tobago]	Rejected. This is not based on the expectation that the hydrological cycle will intensify in the response to global warming. It is a summary of the relevant assessment in Section 11.4
32773	7	34	7	36	that extreme [sadegh zeyaeyan, Iran]	Not applicable. The text has been substantially revised.
33103	7	34	7	36	that extreme [Sahar Tajbakhsh Mosalman, Iran]	Not applicable. The text has been substantially revised.
113479	7	34	7	38	Another example 'thatextreme' and 'likelyaccelerate' (I will stop now listing these…) [Diego Miralles, Belgium]	Not applicable. The text has been substantially revised.
108857	7	34	7	40	Again, I think it would be safer to point out that internal variability is large and may regionally amplify or offset these changes particularly in the near- to mid-term and for lower warming levels. [Erich Fischer, Switzerland]	Considered. The revised ES makes it clear that this is on global scale and changes on regional and local scales will vary, depending on regional warming and other factors such as atmospheric circulation.
71617	7	34	7	40	There are different typos in this paragraph (e.g. thatextreme and likelyaccelerate). [Sixto Herrera, Spain]	Not applicable. This text has been substantially revised.
43305	7	34			Read " it is very likely that extreme precipitation" rather than " it is very likely thatextreme precipitation" [Cyriaque Rufin Nguimalet, Central African Republic]	Not applicable. The text has been substantially revised.
19325	7	34			that extreme [Mansoureh Kouhi, Iran]	Not applicable. The text has been substantially revised.
76675	7	34			missing blank "that extremes" [Piero Lionello, Italy]	Not applicable. This text has been substantially revised.
108855	7	35	7	37	This seems to suggest that CC scaling would work with GSAT everywhere. However, often it is argued that it does not necessarily scale with GSAT but rather with the temperature change in the region of moisture convergence. Also note that CC scaling is 6-7% dependent on the temperature. The sentence seems to suggest that 50-yr return level are globally increasing less than other return levels? What other return levels? "High confidence" seems to be surprisingly high for some parts of this statement. [Erich Fischer, Switzerland]	Considered. The revised ES makes it clear that this is on global scale and changes on regional and local scales will vary, depending on regional warming and other factors such as atmospheric circulation.
39245	7	35	7	37	High confidence for the synthesis of studies on probability of increase in magnitude of extreme precipitation over most land regions. Will it be also in theregional scale? [Lourdes Tibig, Philippines]	Considered. The revised ES makes it clear that this is on global scale and changes on regional and local scales will vary, depending on regional warming and other factors such as atmospheric circulation.
68717	7	35	7	37	This sentence is grammatically highly convoluted to the extent that it is difficult to try and unravel what it is trying to say. [Bodeker Greg, New Zealand]	Considered. This is modified.
125821	7	35	7	37	Don't understand the distinction between the 7% "in general" vs. the "slightly smaller" rate for a very specific event. Also don't understand what pairs of things are being referred to by "respectively." Maybe break this into distinct sentences to make meaning clear. [Trigg Talley, United States of America]	Considered. The wording is modified.
69533	7	35	7	37	The 7%/K estimate is okay for the extratropics as a whole, but the rate in different regions may differ substantially from this. In particular, the rate of increase in the tropics is very uncertain in CMIP models. Currently this point reads as if 7%/K applies to all regions. See: O'Gorman, P. A. & Schneider, T. The physical basis for increases in precipitation extremes in simulations of 21st-century climate change Proc. Nat. Acad. Sci., 2009, 106, 14773-14777 Pfahl, S.; O'Gorman, P. & Fischer, E. Understanding the regional pattern of projected future changes in extreme precipitation Nature Climate Change, Nature Research, 2017, 7, 423-427 [Martin Singh, Australia]	Considered. The revised ES makes it clear that this is on global scale and changes on regional and local scales will vary, depending on regional warming and other factors such as atmospheric circulation.
79799	7	35	7	37	Not sure this sentence makes sense. The general phrase "extreme precipitation" goes up by 7%/degC warming but then more specific extreme precip measure increase slighly less, but without defining what the original "extreme precipitation" means it is not possible to know what other extremes are increasing at the higher rate. Is it more rare events increasing faster or less rare events? Or is something else implied? [Simon Brown, United Kingdom (of Great Britain and Northern Ireland)]	Considered. The wording is modified.
105163	7	35	7	37	This rather convoluted statement about the impact of warming on extreme precipitation. At the moment is seems to say that we generally expect Clausius-Clayeron scaling of extreme precipitation except for two specific kinds of 50-year events. [Francis Zwiers, Canada]	Considered. The wording is modified.
71645	7	35	7	37	How is the number "7%" and "50-yr" concluded from a number of literatures ? [Ryo Mizuta, Japan]	Considered. The wording is modified.
39823	7	36	7	36	"slightly smaller rate" -> Please quantify [TSU WGI, France]	Noted. The sentence is modified.
23075	7	36	7	37	I don't undertsand this qualifier about 50-year events. Can you clarify? [Peter Thorne, Ireland]	Considered. The wording is modified.
84887	7	36	7	37	Confusing what the 7% refers to, I suppose the annual average 1-day and 5-day precipitation whilst the 50-year RP 1-day and 5-day totals are slightly less than 7%? [Turner Jessica, United Kingdom (of Great Britain and Northern Ireland)]	Considered. The wording is modified.
69535	7	36	7	37	I don't understand what "50-yr event of annual maximum 1-day and 5-day precipitation" means. If is it annual, how can it be a 50-year event? This needs to be made clearer to a nonspecialist audience. [Martin Singh, Australia]	Considered. The wording is modified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9131	7	36	7	37	A clunky statement. If the rate is only slightly smaller than 7% for the 5-day event, just say "about 7% per 1C of global warming for the annual maximum 1-day and 5-day precipitation events with a 50-year return period" [Kevin Hennessy, Australia]	Considered. The wording is modified.
17129	7	36	7	37	I suggest these changes: "… with an increase of 7% in the 50-yr event of annual maximum 1-day precipitation rate and a slightly smaller rate in the 5-day precipitation rate respectively (high confidence).". Thanks [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Considered. The wording is modified.
76677	7	36		38	"likely accelerate" (line 38), is marginally contradicting the former statement of a regular linear 7% increase (line 36) per 1°C global warming. is false. I suggest to write "with accelerated global warming" [Piero Lionello, Italy]	Considered. The former statement is about change in magnitude while it is meant for changes in frequency here. The text is modified.
28929	7	36			a bit confusing, suggest: "an increase in the 50-yr event annual maximum 1-day precipitation of 7% per 1°C warming and a slightly smaller rate for 5-day precipitation events" or just state "about 7%" to apply to both [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Considered. The wording is modified.
76679	7	36			This would be Clausius-Clapeyron, which is not alwyas true at regional scale . This sentence is not valid in regions where the circulation does not support sufficient moisture flux (see also page 11, lines 34-35) and Drobinski, P., Silva, N.D., Panthou, G. et al. Scaling precipitation extremes with temperature in the Mediterranean: past climate assessment and projection in anthropogenic scenarios. Clim Dyn 51, 1237–1257 (2018). https://doi.org/10.1007/s00382-016-3083-x [Piero Lionello, Italy]	Considered. The revised ES makes it clear that this is on global scale and changes on regional and local scales will vary, depending on regional warming and other factors such as atmospheric circulation.
17131	7	37	7	39	I suggest these changes: "The increase in the likelihood of extreme precipitation will very likely accelerate with the increased global warming, with larger incremental increases at higher global warming levels, and especially for rarer extreme events." Thanks [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Considered. The wording is modified.
49943	7	38	7	38	Space missing between "likelyaccelerate" [Daniel Gilford, United States of America]	Not applicable. The text has been substantially revised.
13657	7	38	7	38	change likelyaccelerate by likely accelerate [Maria Amparo Martinez Arroyo, Mexico]	Not applicable. The text has been substantially revised.
11645	7	38	7	38	insert a space after "very likely" [Amy East, United States of America]	Not applicable. The text has been substantially revised.
89283	7	38	7	38	'likelyaccelerate' to 'likely accelerate' [Tinghai Ou, Sweden]	Not applicable. The text has been substantially revised.
105165	7	38	7	38	I would urge you to avoid statements about acceleration (which involve the 2nd derivate), if at all possible. As with temperature (see comment concerning page 7, lines 8-9), this refers to situations where the change in likelihood is apt to occur in the lower part likelihood function when expressed as a function of warming. One can imagine event definitions for extreme precipitation (e.g., perhaps extreme regional mean precipitation), where the likelihood of projected changes approaches saturation. Implicit in this statement is the notion that the statement applies locally, in which case I would probably agree, so perhaps an indication of scale should be provided. [Francis Zwiers, Canada]	Taken into account. "will accelerate" is replaced with "will increase non-linearly" to ensure consistency with similar wording for changes in the frequency of extreme temperature. The previous sentence was replaced as follows: ""The frequency of heavy precipitation events will increase non-linearly with further global warming"
74491	7	38	7	38	words likelyaccelerate to separe on likely accelerate [Moulay Driss HASNAOUI, Morocco]	Not applicable. The text has been substantially revised.
32775	7	38	7	39	likely accelerate [sadegh zeyaeyan, Iran]	Not applicable. The text has been substantially revised.
33105	7	38	7	39	likely accelerate [Sahar Tajbakhsh Mosalman, Iran]	Not applicable. The text has been substantially revised.
43307	7	38			Read " the likelihood of extreme precipitation will very likely accelerate " rather than " the likelihood of extreme precipitation will very likelyaccelerate " [Cyriaque Rufin Nguimalet, Central African Republic]	Not applicable. The text has been substantially revised.
19317	7	38			that extreme [Mansoureh Kouhi, Iran]	Not applicable. The text has been substantially revised.
19319	7	38			likely accelerate [Mansoureh Kouhi, Iran]	Not applicable. The text has been substantially revised.
11111	7	42	7	42	Here "Floods and water logging". In chapter 8 and 12, the phrase fluvial (River) and pluvial flood is used. Should the terminology be consistent? [Wen Wang, China]	Accepted. The term "water logging" has been removed.
11647	7	42	7	42	the term "water logging" is a strange choice, as it is a colloquial phrase basically meaning saturation. It is not commonly used in scientific literature. Do you mean "soil saturation" instead? Or is "logging" meant to refer to monitoring and data collection? Either way, this choice of phrasing is not communicating effectively. [Amy East, United States of America]	Accepted. The term "water logging" has been removed.
125823	7	42	7	42	"Waterlogging" is one word. (Are there any statements made about waterlogging?) [Trigg Talley, United States of America]	Noted. The term "water logging" has been removed.
31647	7	42	8	4	I suggest to add a key message on coastal flood here: in the majority of the cases, sea-level rise has affected already extreme water levels as shown in Ch9, but it is true that local processes (waves, currents, rainfall, river discharge) make attribution difficult. However, there is a clear projected aggravation of chronic flooding and flooding during storms due to the magnitude of projected sea-level rise. Examples of papers are given in other comments and below (not exhaustive list, references therein can also be useful). Nicholls, R.J. and Cazenave, A., 2010. Sea-level rise and its impact on coastal zones. science, 328(5985), pp.1517-1520. Purvis, M. J., Bates, P. D., & Hayes, C. M. (2008). A probabilistic methodology to estimate future coastal flood risk due to sea level rise. Coastal engineering, 55(12), 1062-1073 Vitousek, Sean, Patrick L. Barnard, Charles H. Fletcher, Neil Frazer, Li Erikson, and Curt D. Storlazzi. "Doubling of coastal flooding frequency within decades due to sea-level rise." Scientific reports 7, no. 1 (2017): 1-9 Sayol, J. M., & Marcos, M. (2018). Assessing flood risk under sea level rise and extreme sea levels scenarios: application to the ebro delta (Spain). Journal of Geophysical Research: Oceans, 123(2), 794-811 [Gonéri Le Cozannet, France]	Rejected. Coastal floods are addressed in chapter 9 and chapter 12. However, we have changed the subtitle to "River floods" to clarify the scope covered in ch11.
107685	7	44	7	44	seasonality of floodS (flood missing an S) [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The text has been revised.
105167	7	44	7	45	I'm wondering if this is about flooding per se, or about streamflow. Also, I'm wondering if a distinction is being made between results from hydrologic models, which often simulate the naturalized state of a basin with unchanging land use, and observed streamflow and flooding occurrence. Overall, I'm concerned that the assessment is not as clear or as carefully nuanced at it should be. The body of the paragraph (line 45-49) describes trends in peak streamflow, indicating, correctly, that there is low confidence in any attribution to "human influences" (I assume this means human influences on the climate, because human influence on drainage basins is enormous and clearly evident all around us). The headline, on the other hand, picks off a point where there is some evidence (the timing of annual peak streamflow) of the impact of warming, e.g., in midlatitude nival basins. While we might have medium, or perhaps, high confidence in evidence concerning timing, I suspect that we need to be much more retrospect about magnitude – since projections of streamflow change for nival basins generally suggest that while the timing of the spring melt advances, the magnitude of the spring peak flow diminishes since snow storage in basins declines. [Francis Zwiers, Canada]	Not applicable. The text has been revised.
55167	7	44	7	48	Recommend adding a sentence to this paragraph to support the bolded first sentence about changes in the seasonality of floods. The following sentences seem to introduce new results related to increases and decreases in trends in peak streamflow and presumably not (only) related to snow-melt. [Nancy Hamzawi, Canada]	Noted. The text has been substantially revised. The new bolded statement is a summary of the paragraph.
68719	7	45	7	45	Involved in what? [Bodeker Greg, New Zealand]	Rejected. The text implies that snowmelt is involved in the magnitude of river floods.
13659	7	47	7	47	Standardize the name of US because in other chapters it is written as United States [Maria Amparo Martinez Arroyo, Mexico]	Noted. Regional changes are now indicated using the AR6 standard regions.
17133	7	48	7	49	I suggest these changes: There is low confidence in directly attributing changes in the probability or magnitude of individual floods to human influences. [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The text has been revised.
125825	7	48			Should say "southwestern US." rather than "southeastern US". [Trigg Talley, United States of America]	Not applicable. The text has been revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
90841	7	48			The interplay of human and water systems leading to drought and flood risk has always been dynamic especially in Asia region, past studies have discussed on human-flood risk assessment and indicate medium confidence on human attribute to flood [Vivien How, Malaysia]	Noted. No action is suggested by the comment.
88543	7	49	7	49	You need to be more specific here, attribution of sea level rise is not consistent with this assessed level of uncertainty. [Baylor Fox-Kemper, United States of America]	Considered. The revised text does not contain anything related to sea level rise.
105169	7	51	8	4	An editorial suggestion is to rephrase this convoluted headline as “Projected increases in extreme precipitation, especially at high global warming levels, are expected to increase flooding potential in urban areas (high confidence)”. A more important concern about this headline, however, is whether urban flooding potential is within the scope of this chapter or even the WG1 report. Is this chapter in a position to assess how or whether a change in the intensity of extreme precipitation events falling on urban areas will affect urban flooding? Such an assessment presumably requires an in depth understanding of urban land surface characteristics and urban drainage systems – i.e., a rather complex combination of engineering, surface hydrology and climatology. A further concern is that the headline doesn’t seem to be connected with the rest of the body of the paragraph, which deals with riverine flooding (and consequently overlaps with the previous paragraph (page 7, lines 44-49)). [Francis Zwiers, Canada]	Considered. The text is revised.
88541	7	51	8	4	Coastal cities are potentially also affected by flooding related to sea level and tides, flooding along estuaries or near the mouths of rivers are common in many urban areas and result from a complex of factors. The drought & flood thematic team should work on sharpening this ES statement to reflect these issues in collaboration with chapters 9, 12. [Baylor Fox-Kemper, United States of America]	Considered. The coastal related flooding is covered in Ch12.
29459	7	51		52	as much there is high confidence of extreme precipitation leading to flooding, we can also note that areas with good drainage system will experience less flooding than area with bad or without drainage system. High precipitation does not guarantee high flooding when proper system is in place. areas such as countries in west africa tend to experience high flooding even with low precipitation due to bad drainage system. [Babatunde Oyekan, Nigeria]	Noted. This is based on everything else being equal for the current and future conditions. Assessment on drainage system is beyond the scope of Ch11.
105181	8	2	8	2	Delete “the potential for”. Data issues related to TCs are real, are they not? [Francis Zwiers, Canada]	Noted but the page/line number this comment points to does not contain this wording.
66333	8	3	8	4	“Regional changes in river floods are more uncertain” this statement is inconsistent with what is shown in CH12 where there are regions where we have high or medium confidence in river flood projection. See table 12.12 where the river flood column is all medium confidence except that for Australasia. [Erika Coppola, Italy]	Rejected. This is compared with that of pluvial floods change which has high confidence. This is not inconsistent with Chapter 12 assessment.
62369	8	3	8	4	Considering that in the development of the section corresponding to floods you speak of a high uncertainty in “At regional and local scales, projected changes in river floods are characterized by high uncertainty” (page 72, line 9) I would modify that sentence to emphasize that uncertainty. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. The text is revised.
17135	8	3	8	4	Weather change is a complex problem too, but scientists model it and keep accomodating its uncertainty. I suggested these change: Regional changes in river floods are more difficult to be quantified because as complex hydrological processes are involved, although some patterns are existed in the observations (low confidence). [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Considered. The intend here is not about "more difficulty to quantify" but about level of uncertainty in the projections between pluvial floods and river floods.
99157	8	6	8	6	I would suggest that the word "drought" generally means a reduction of water availability below some norm that lasts for some relatively limited period and there is an expectation there will be a return to the higher level. While I agree that global warming will intensify droughts, the expansion of the subtropics is causing the polar edges of those regions to experience increasing dryness that will continue and become the new norm--this is persisten change and not a temporary, even multi-decadeal, change--it is a really long lasting change (the Sahara is not experiencing a 6000-year drought from its formerly vegetated state). It thus seems to me that this summary needs to be differentiating between what one might call drought as a variation and the longer term aridification of regions that is taking place in sothwestern North America, southern Australia, etc. I think this is key to explain as the response sto drougth versu the responses to aridification are different--to deal with a drought, build bigger reservoirs, raise water price temporarily, etc.; for aridification, build desalination plants, change over prevailing residential landscaping, reuse waste water, etc. And then there is the complexity that both can be happending at once--an underlying trend toward aridification and then variability about this longer-changing trend. I think it really important to provide a good explanation of this--and not just of the types of drought that staerts off the first finding. [Michael MacCracken, United States of America]	Noted. The assessment focuses on changes in drought events conditions, some of which can be related to a process of aridification.
52133	8	6	8	22	Drought condition in South Asia and SouthEast Asia should be included. [Oo Kyaw Lwin, Myanmar]	Noted. More regional details have been included in the ES drought summary. There is low confidence in changes in agricultural and ecological droughts, respectively hydrological droughts, in South Asia and Southeast Asia (see regional tables in Section 11.9).
125827	8	6	8	46	[DROUGHT] This summary seems to underplay the existence of differing views on the relative roles of temperature, radiation, CO2 fertiization, plant growth and so on that are found throughout the contemporary drought/climate literature. These differing views have fundamentally different implications for drought impacts. [Trigg Talley, United States of America]	Noted. Uncertainties associated to the role of temperature, radiation and CO2 physiological effects are addressed in the chapter assessment. Two sentences have also been added in the ES on the CO2 physiological effects in the context of drought events: "There is low confidence that effects of enhanced atmospheric CO2 concentrations on plant water-use efficiency alleviate extreme agricultural and ecological droughts in conditions characterized by limited soil moisture and enhanced AED. There is also low confidence that these effects will substantially reduce global plant transpiration and the severity of hydrological droughts". The text also notes that soil moisture droughts are "sometimes amplified by increased atmospheric evaporative demand", making it clear that increased AED does not automatically lead to increased soil moisture deficits.
113483	8	6	8	54	I would reduce the amount of text in bold. This may contrast with other chapters. One sentece rather than half a paragraph. [Diego Miralles, Belgium]	Accepted. The amount of bolded text was reduced in the FGD version.
71433	8	6			If I haven't overlooked it, there is no statement about changes in the persistence of meteorological drought. Given that this is frequently discussed in the media, in particular following the 2018 drought in central and northern Europe, I suggest to add a statement here saying that there is no conclusive evidence yet because of high internal variability and low skill of climate models (I guess such a statement would be backed up by the literature). [Douglas Maraun, Austria]	Rejected. Too detailed. In addition, dynamical aspects associated to drought have low confidence (see Section 11.6: "There is low confidence in the effects of greenhouse gas forcing on changes in atmospheric dynamic (Chapter 2, Section 51 2.4; Chapter 4, Section 4.3.3), and, hence, on associated changes in drought occurrence.").
32771	8	8	8	8	insert "evapotranspiration deficit drought " before " soil mosturee deficit " [sadegh zeyaeyan, Iran]	Not applicable. The text has been substantially revised.
33101	8	8	8	8	insert "evapotranspiration deficit drought " before " soil mosturee deficit " [Sahar Tajbakhsh Mosalman, Iran]	Not applicable. The text has been substantially revised.
21127	8	8	8	8	insert "evapotranspiration deficit drought " before " soil mosturee deficit " [Iman BABAEIAN, Iran]	Not applicable. The text has been substantially revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6847	8	8	8	10	"Different drought types (related to precipitation deficits, soil moisture deficits, streamflow deficits or increased atmospheric evaporative demand) are associated with different impacts and respond differently to increased greenhouse gas forcing." The referral to "different impacts" and to "respond differently" is very general and needs to be quantified. [Constantinos Cartalis, Greece]	Rejected. There is not enough space to include more details on this in the ES. However Section 11.6 provides more background on these points.
109339	8	8	8	13	Suggest condensiing to 2 sentencese and removing the reference to impacts and maybe moving this up to the preamble as it is not really a headline statement. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The text has been substantially revised. The bolded text has been shortened.
23077	8	8	8	14	There is substantial overlap here with chapter 8 and it would be worthwhile reconciling this. I wonder whether the primary drought assessment should be in chapter 8. [Peter Thorne, Ireland]	Noted. The AR6 drought assessment has been coordinated as part of a cross-chapter drought team including chapter 11, chapter 8, chapter 12 and Atlas. Chapter 8 address climatological aspects while chapter 11 includes the main drought assessment.
105171	8	8	8	22	What is the message to policy makers in this paragraph? What I would take from this is that there isn't a clear message about drought at a global scale because of the heterogeneous nature of the land surfaces and drought processes – but that could be said in just one sentence. Nevertheless, the authors seem to suggest that some assessments can be made with medium or high confidence for some types of drought in some regions. Perhaps that is all that should be said in the executive summary – it would be sufficient to direct readers who have regional interests to section 11.6 for details. I suspect that you will receive the full spectrum of comments on the ability of models. Generally, these have to be considered to be poor or at least poorly constrained given the heavy reliance on scarce or non-existent land surface data for their parameterization and calibration. [Francis Zwiers, Canada]	Noted. The text has been substantially revised. The definitional aspects have been shortened. The regional assessments are presented in more details.
113485	8	11	8	12	Clarify whether 'Atmospheric evaporative demand displays a global drying tendency over continents' refers to past or future. [Diego Miralles, Belgium]	Noted. The text has been revised. It now refers to past AED trends.
39247	8	11	8	14	please indicate the confidence llevels in these findings as this is very important in drought occurrences/events, whatever the type. [Lourdes Tibig, Philippines]	Noted. The text has been substantially revised. The drought-related assessments include confidence levels.
109599	8	12	8	13	The assertion that there is an observered tendency towards increased drying in the dry season since the beginning of the 20th century on a global scale is contradicted by several studies. For example, it has been reported that robust dryness changes cannot be detected in over three-quarters of the global land area (see Greve, P. et al. 2014. Global assessment of trends in wetting and drying over land. Nature Geoscience, vol. 7, no. 10, pp. 716-721). These results were supported by a subsequent study (see Feng, H. and M. Zhang. 2015. Global land moisture trends: drier in dry and wetter in wet over land. Scientific Reports 5, Article number: 18018(2016)). [Reynold Stone, Trinidad and Tobago]	Rejected. Both Greve et al. 2014 and Feng and Zhang 2015 analyse annual changes in water availability, not dry-season changes. However, the text was revised to focus more on actual drought metrics: "Human-induced climate change has contributed to increases in agricultural and ecological droughts in some regions due to evapotranspiration increases (medium confidence)".
13661	8	13	8	13	standardize writing format 20th [Maria Amparo Martinez Arroyo, Mexico]	Not applicable. The text has been revised.
2411	8	13	8	13	"increased drying" in what sense? i.e., precipitation, runoff/streamflow, soil moisture, evaporative demand? This should be mentioned explicitly and carefully. In general, throughout the chapter (the summary here, and in the detailed section), I would recommend avoiding the term "drying" where possible, as i) this is ambiguous (i.e., drying of what hydrological variable? If anything, what recent research has shown is that different land surface hydrological variables show different trends under greenhouse warming - as is indeed stated here - and thus general statements about "drying" should be avoided) and ii) this tends to refer to climatological trends, i.e changes in background climate, rather than changes in droughts. Of course, both are linked, in non-trivial ways actually, but this chapter should focus on changes in droughts conditions only. I would thus recommend replacing "drying" with, e.g., "increased X drought conditions" (with X being precipitation / soil moisture / runoff). [Alexis Berg, United States of America]	Accepted. The term "drying" has been removed. The assessment is now provided by drought types, based on three drought types: 1) agricultural and ecological drought (soil moisture based, potentially amplified by increased AED), 2) hydrological drought (runoff based), 3) meteorological drought (precipitation based).
125829	8	13	8	14	How does "when aggregated on global scale" differ from the phrase "on global scale" used widely elsewhere in the document? [Trigg Talley, United States of America]	Noted. The text has been revised to indicate that this trend is happening over a predominant fraction of the land area.
10081	8	13			"increased drying" should be "increased soil drying" to be consistent with the body of Section 11.6. Section 11.6 is only making this conclusion about soil moisture drought, not about other types of drought (hydrological and precipitation drought.) [Jacob Scheff, United States of America]	Noted. The ES text on drought has been changed to focus on drought types. In this sentence it is now specified that the assessment is related to changes in agricultural and ecological droughts: "Human-induced climate change has contributed to increases in agricultural and ecological droughts in some regions due to evapotranspiration increases (medium confidence)".
39751	8	14	8	14	"high confidence (medium confidence)" - > What is meant here? [TSU WGI, France]	Noted. The syntax was confusing and the text has been revised.
96109	8	14	8	14	Mismatch of terms: Please delete the term 'high confidence' and add it to the preceding sentence. [Nicole Wilke, Germany]	Noted. The syntax was confusing and the text has been revised.
68721	8	14	8	14	I always find this use of parentheses to present to disparate sentences as a single sentence to be very confusing. I would suggest against using this method. [Bodeker Greg, New Zealand]	Accepted. This syntax is not used anymore.
17137	8	14	8	14	Choose one, high confidence or medium confidence. Be Just. Thanks [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The syntax was confusing and the text has been revised.
70937	8	14	8	15	Please avoid this syntax, with parentheses indicating an alternative reading. It is difficult to parse even for a native English speaker. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This syntax is not used anymore.
39471	8	14	8	15	This sentence has both the words 'high confidence' and '(medium confidence)' consecutively included, one of the two words need to be selected. [Tamara van 't Wout, Qatar]	Accepted. This syntax is not used anymore.
113487	8	14	8	15	Not sure the use of brackets makes the message clear. [Diego Miralles, Belgium]	Noted. The syntax was confusing and the text has been revised.
107411	8	14	8	15	North Africa is also subjected to precipitation deficit since the 20th centry. [Rachda Berrached, Algeria]	Noted. The revised assessment indicated regional trends based on the AR6 regions. Northern Africa is part of the Mediterranean AR6 region.
71619	8	14	8	15	It is not clear which is the meaning of the parenthesis used in: "There is high confidence (medium confidence) that precipitation deficits have increased since the mid 20th century in west Africa, central Africa, and southern Africa (Northeastern Brazil).". [Sixto Herrera, Spain]	Noted. The syntax was confusing and the text has been revised.
51571	8	14	8	18	These overall findings (and the related text) seems to be inconsistent with the findings in Chapter 8 on drought - and these have found their way into the SPM too. Chapter 8 [p.45 L38-42] states that "In summary, it is very likely that the frequency and the severity of droughts has increased over the last decades in the Mediterranean, western North America and Australia. These changes can be attributed to anthropogenic warming in the Mediterranean (high confidence), South Africa (medium confidence), and Australia (medium confidence) while for the other regions confidence in an anthropogenic contribution to the observed droughts is low" We have made similar comments in the SPM and Chapter 11. Would it be possible to work with Chapter 8 and SPM authors to ensure cross-referencing and consistency across chapter findings please? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The revised drought assessment for the FGD has been done as part of a cross-chapter team on drought including authors from chapters 11, 8, 12 and the Atlas.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9133	8	14	8	18	Please clarify why southern Australia is not included, given the drying trend since 1950 and the following entry in Table 11.6 "far southwest Western Australia [has] statistically significant increases in drought intensity and southeast Australia has shown a significant increase in the average length of droughts (Gallant et al., 2013)". This seems inconsistent with the statement on page 86 lines 23-25 "Several studies of Australian droughts of varying length demonstrate no significant change in meteorological droughts in the region related to anthropogenic climate change based on analysis of precipitation deficits". Should the statement on page 86 say "There is no anthropogenic attribution to trends in meteorological drought in Australia"? [Kevin Hennessy, Australia]	Taken into account. The relevant text is revised to reflect the final assessment.
18683	8	14	8	20	A generalized statement could be made here: The subtropical land regions have experienced dryness [Govindasamy Bala, India]	Considered. There are different ways to make generalized statements and it was decided to provide statements for continents and in some cases AR6 regions. For this reason, a generalized statement for the subtropical land region is not provided.
90843	8	14			high confidence (medium confidence)?? [Vivien How, Malaysia]	Noted. The syntax was confusing and the text has been revised.
62793	8	15	8	15	Specific comment: In the following list "(...) west Africa, central Africa, and southern Africa (Northeastern Brazil)." Consider remove the parentesis, as Northeastern Brazil is not a region from southern Africa, but a different one. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The syntax was confusing and the text has been revised.
96111	8	15	8	15	Unclear formulation: ..and southern Africa (North-eastern Brazil). ?? [Nicole Wilke, Germany]	Noted. The syntax was confusing and the text has been revised.
62605	8	15	8	15	the juxtaposiiton of " southern Africa" and "(Northeastern Brazil)" doesn't make sense here [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The syntax was confusing and the text has been revised.
74493	8	15	8	15	no need , because we have after and in the the sentence ... and Southern Africa ... [Moulay Driss HASNAOUI, Morocco]	Noted. This comment is difficult to interpret, no clear recommendation is available. The reviewer might have been referring to the confusing syntax of this sentence, the text has been substantially revised.
74495	8	15	8	15	to check if the there isn't any wrong matter in the the sentence "... and Southern Africa (North easthern Brazil)", i.e between Africa and Brazil? [Moulay Driss HASNAOUI, Morocco]	Noted. The syntax was confusing and the text has been revised.
105183	8	15	8	17	Spring is highlighted, but not fall. To increase confidence, it might be useful to say something about that asymmetry. [Francis Zwiers, Canada]	Rejected. It is not clear what text the reviewer is referring to since there is no mention of "spring" in the referred text.
45671	8	16	8	16	understanding ==> delineating, presenting, letting understand? [Christophe Deissenberg, Luxembourg]	Noted. It is not clear what text the reviewer is referring to since there is no mention of "understanding" in the referred text.
96113	8	16	8	16	Please clarify period (same as in the sentence bevor?) [Nicole Wilke, Germany]	Noted. The indication "since the 20th century" has been removed in the previous sentence. As indicated at the beginning of the FGD ES, all observed trends are indicated for conditions since 1950 unless indicated otherwise.
32781	8	18	8	18	Please add Middle East in the examples for Hydrological Drought (Barlow et al .2016) Barlow M, Zaitchik B, Paz S, Black E, Evans J, Hoell A (2016) A review of drought in Middle East and south west Asia, Journal of Climate, https://doi.org/10.1175/JCLI-D-13-00692.1 [sadegh zeyaeyan, Iran]	Rejected. The suggested study is not focusing on hydrological droughts in the Middle East. This is an useful study, which has been cited in others sections of Chapter 11 but it mostly focused on atmospheric mechanisms of meteorological droughts in the middle East but it does not provide information on the dynamic of hydrological droughts in the region. In the WCA region the assessment is of low confidence on hydrological droughts as there is limited evidence.
33111	8	18	8	18	Please add Middle East in the examples for Hydrological Drought (Barlow et al .2016) Barlow M, Zaitchik B, Paz S, Black E, Evans J, Hoell A (2016) A review of drought in Middle East and south west Asia, Journal of Climate, https://doi.org/10.1175/JCLI-D-13-00692.1 [Sahar Tajbakhsh Mosalman, Iran]	Rejected. The suggested study is not focusing on hydrological droughts in the Middle East. This is an useful study, which has been cited in others sections of Chapter 11 but it mostly focused on atmospheric mechanisms of meteorological droughts in the middle East but it does not provide information on the dynamic of hydrological droughts in the region. In the WCA region the assessment is of low confidence on hydrological droughts as there is limited evidence.
21087	8	18	8	18	Please add Middle East in the examples for Hydrological Drought (Barlow et al .2016) Barlow M, Zaitchik B, Paz S, Black E, Evans J, Hoell A (2016) A review of drought in Middle East and south west Asia, Journal of Climate, https://doi.org/10.1175/JCLI-D-13-00692.1 [Farnaz Pourasghar, Iran]	Rejected. The suggested study is not focusing on hydrological droughts in the Middle East. This is an useful study, which has been cited in others sections of Chapter 11 but it mostly focused on atmospheric mechanisms of meteorological droughts in the middle East but it does not provide information on the dynamic of hydrological droughts in the region. In the WCA region the assessment is of low confidence on hydrological droughts as there is limited evidence.
113489	8	18	8	20	This is ill-phrased: 'There is medium confidence that trends in potential evaporation have exceeded trends in precipitation in some regions and seasons.' Sure it has happerned 'somewhere', that is not just 'medium confidence'. So unless you add where you refer to, the sentence does not really mean much and is not really correct in terms of confidence attribution. — In fact, the previous sentence suffers from the same issue unless you take the regions out of brackets. [Diego Miralles, Belgium]	Not applicable. The text has been substantially revised.
71491	8	19	8	19	I would unify the therminology. If above it is used atmopsheric evaporative demand, I recomend to use again this term instead potential evaporation. In addition high confidence would be better in this statement [Sergio Vicente-Serrano, Spain]	Accepted. The ES only uses the term atmospheric evaporative demand.
51573	8	19	8	19	The phrase "potential evaporation", while scientifically useful, is quite hard to understand for non-experts and has made its way into the SPM. Could this be replaced with something more layperson friendly, for example, "there is medium confidence that evaporation has exceeded precipitation", if this would still be accurate. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The text has been substantially revised to make it more accessible. The word "potential evaporation" is no longer used in the ES.
2413	8	24	8	24	I am not sure what "potential for worsened drought conditions" means. This statement should be clarified. Is it that human influence has increased the number of droughts, or made droughts that do happen, worse? "Potential" introduces some ambiguity as to whether any attrutable change in drought actually happened. [Alexis Berg, United States of America]	Not applicable. The text has been substantially revised. This wording is no longer used.
6851	8	24	8	24	" There is high confidence that human influence has increased the potential for worsening..." to "There is high confidence that human influence have worsened drought conditions..." [Constantinos Cartalis, Greece]	Not applicable. The text has been substantially revised. This wording is no longer used.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
105173	8	24	8	24	What is the basis for this high confidence, and how is the “potential” quantified? This seems rather abstract, overall, suggesting that models and process understanding lead us to believe that there should be evidence of worsening drought in the historical record, if only observations were good enough to permit a search for expected signals. Is this a strong enough message to warrant inclusion in the executive summary? [Francis Zwiers, Canada]	Not applicable. The text has been substantially revised. This wording is no longer used.
10083	8	24	8	25	Same as comment on line 13 -- this sentence must be phrased to make clear that it is about *soil* drying and *soil* drought, not precipitation drought or hydrological (stream) drought! This is relatively clear in section 11.6, but is not clear at all here in the Executive Summary. [Jacob Scheff, United States of America]	Accepted. The revised ES clearly distinguishes the assessments for agricultural and ecological droughts (related to soil drying, potentially amplified by atmospheric evaporative demand), hydrological drought (streamflow deficits) and meteorological drought (precipitation deficits)
125831	8	24	8	25	[DROUGHT] "worsening of drought conditions" is vague, given the existence of different drivers and manifestations of drought. What specifically is meant here? [Trigg Talley, United States of America]	Not applicable. The text has been substantially revised. This wording is no longer used.
105185	8	24	8	26	I think this statement needs to be reworked. Resolution dependence is a source of uncertainty, so it seems odd to say that there is high confidence. This statement also begs the question of whether the authors are talking about super Clausius-Clapeyron scaling (it seems to point in that direction). The question of how to calculate the sensitivity of changes in extreme precipitation to warming at small scales remains unresolved, as is the question of whether the Clausius-Clapeyron relation provides useful guidance at small scales and in contexts where the system in question is not closed. [Francis Zwiers, Canada]	Noted. It seems that this comment is referring to another part of the text (CC scaling and heavy precipitation). Cannot be addressed.
18685	8	24	8	31	When one looks at the precipitation projections from models, it is clear that there would be a derease in the subtropical regions. This could be stated here for better understanding. [Govindasamy Bala, India]	Rejected. The assessment does not support such a broad statements.
71493	8	25	8	25	I would change drought conditions by drought events [Sergio Vicente-Serrano, Spain]	Not applicable. The text has been substantially revised. This wording is no longer used.
2415	8	25	8	25	Same comment as on line 13 - "towards drying”: drying in what sense? Precipitation, runoff/streamflow, soil moisture, evaporative demand? This should be mentioned explicitly and carefully. In general, throughout the chapter (the summary here, and in the detailed section), I would recommend avoiding the term “drying” where possible, as i) this is ambiguous (i.e., drying of what hydrological variable? If anything, what recent research has shown is that different land surface hydrological variables show different trends under greenhouse warming - as is indeed stated here - and thus general statements about "drying" should be avoided) and ii) this tends to refer to climatological trends, i.e changes in background climate, rather than changes in droughts. Of course, both are linked, in non-trivial ways actually, but this chapter should focus on changes in droughts conditions only. I would thus recommend replacing “drying" with, e.g., “increased X drought conditions” (with X being precipitation / soil moisture / runoff). [Alexis Berg, United States of America]	Considered. The word "drying" has been avoided both in the ES and Section 11.6.
71495	8	27	8	27	Net radiation better? On the contrary, the term radiation can be confuse (It may refer to direct solar radiation). [Sergio Vicente-Serrano, Spain]	Not applicable. The text has been substantially revised. This wording is no longer used.
68723	8	27	8	27	Is it the case that an increase in radiation-induced evaporative demand can only arise from an increase in radiation? I would have thought so. But where is this increase in radiation coming from? I assume it is the back radiation from the atmosphere. But has that really increased that much as to increase evaporation? [Bodeker Greg, New Zealand]	Not applicable. The text has been substantially revised. This wording is no longer used.
45673	8	29	8	29	treating ==> using, considering? [Christophe Deissenberg, Luxembourg]	Noted. Not clear what this comment is referring to since the word "treating" is not in the ch11 text.
113491	8	29	8	31	Make sure this agrees with Ch 8 because I recall higher confidence on e.g. human influences on Hadley cell expansion [Diego Miralles, Belgium]	Noted. The ES does not include material on circulation anymore. However, the chapter 11 assessment is in agreement with the assessment in Ch. 8, which states: "There is medium confidence that the recent observed expansion of the Hadley Circulation was caused by GHG forcing, especially in the Southern Hemisphere, but there is only low confidence in how it influences the drying of subtropical land areas.", "understanding of poleward expansion of the Hadley Cells has improved (Section 2.3.1.4.1) but its role in subtropical drying is limited to the zonal mean and dominated by ocean regions (Byrne and 35 O’Gorman, 2015; Grise and Polvani, 2016; He and Soden, 2017; Schmidt and Grise, 2017; Siler et al., 36 2018a; Chemke and Polvani, 2019; Grise and Davis, 2020)." ". Changes to the HC in the Northern Hemisphere may have contributed to subtropical drying and a poleward expansion of aridity during the boreal summer, but there is low confidence due to limited evidence
17139	8	29	8	31	In Chap. 2, It is stated that there is high confidence that greenhouse gas forcing to changes in atmospheric circulation processes. It is contradicting with the statement here. I suggest these changes: Therefore, it is still difficult to quantify There is low confidence in the direct contribution of greenhouse gas forcing to changes in atmospheric circulation processes affecting drought. [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Changes in circulation are no longer addressed in the ES. The chapter material refers to Chapter 2.
105187	8	30	8	31	Cherry picking one assessment to serve as a general headline for assessments of compound events doesn’t seem like a good idea to me. It seems to me that the second sentence, if well supported in the literature, should really be the headline. [Francis Zwiers, Canada]	Noted. This comment seems to be misplaced and should have been indicated for page 9, lines 30-31 (headline statement for compound events). The headline statement has been revised and is now an overarching statement for the overall paragraph on compound events.
113493	8	33	8	33	This 'high confidence' should be 'virtual certainty'; at leastfor the first half of the sentence. [Diego Miralles, Belgium]	Not applicable. The text has been substantially revised. This wording is no longer used.
105175	8	33	8	33	This also seems a rather academic way to describe drought projections and their limitations. How would you convey this to a policy maker? Maybe a way to cut through some of the detail is to use evidence/agreement language to describe the evidence from the models, and confidence language to temper the message. For example: “Models suggest a (continued?) shift in the growing season moisture balance towards drier conditions with continued warming (robust evidence, high agreement). Model and process understanding limitations however, indicate that there is only medium confidence in drying projections overall, with lower confidence in most regions. [Perhaps now highlight key exceptions].” [Francis Zwiers, Canada]	Considered. The whole text has been substantially revised to make it less academic and better understandable for policymakers.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18687	8	33	8	34	Climate models do project a decrease in precipitation in the subtropical land areas such as mediterranean, Australia, South Africa and SW North America. Increase in ET is not the main reason for dryness [Govindasamy Bala, India]	Rejected. In some regions, decreases in precipitation also play a relevant role, but increases in evapotranspiration is often a dominant driver, which is why projected changes in meteorological droughts and agricultural/ecological droughts differ substantial (see Section 11.6). This specific sentence was, however, removed and the text has been revised to focus on changes in different drought types.
66335	8	34	8	37	There is inconsistency for drought projection confidence statment with CH12 for South Africa, Mediterranean, Brazil, Central America. There is mediumconfidence in CH11 against high confidence in CH12. [Erika Coppola, Italy]	Noted. The AR6 FGD drought assessment has been coordinated as part of a cross-chapter drought team including chapter 11, chapter 8, chapter 12 and Atlas. Chapter 12 now refers to chapter 11 for the drought assessment.
66369	8	34	8	37	non conventional AR6 region names are used like for example southern North America [Erika Coppola, Italy]	Considered. The regional assessments are now all provided using the standard AR6 regions.
9135	8	37	8	37	Include the following sentence from page 91 lines 30-31: "There is medium confidence for an increase in hydrological droughts in the Mediterranean, southern Africa, southern Australia, eastern and northern New Zealand and southern South America''. [Kevin Hennessy, Australia]	Noted. The Section 11.6 assessment on projected changes in hydrological droughts for AR6 regions is now provided in the ES. The final text is: "Several regions are projected to be more strongly affected by hydrological droughts with increasing global warming (at 4°C of global warming: NEU, WCE, EEU, MED, SAU, WCA, SCA, NSA, SAM, SWS, SSA, WNA, WSAF, ESAF, MDG; medium confidence or higher)."
100073	8	37			Suggest using "strong agreement among climate models" (rather than the current "high agreement" [Ronadh Cox, United States of America]	Not applicable. The text has been substantially revised. This wording is no longer used.
125833	8	39	8	41	[PRECISION] This issue seems related to the problem with the absence of definitions seen throughout the drought section of the Executive Summary. Suggest clarification of terminology at the outset of this section. [Trigg Talley, United States of America]	Considered. The terminology used for different drought types is now clarified in at the outset of the drought section in the ES.
113495	8	39	8	46	Please mention VPD stress on stomata, not just CO2. Both are clearly going up, and have similar impacts in terms of transpiration (if we leave aside the atmospheric demand that VPD causes and the greening due to CO2). [Diego Miralles, Belgium]	Noted. This aspect is included within the definition of agricultural and ecological drought in the revised ES: "Lack of sufficient soil moisture, sometimes amplified by increased atmospheric evaporative demand (AED), results in agricultural and ecological drought". More details would lie beyond the scope of the ES.
39825	8	44	8	44	"some regions" - > Which regions? [TSU WGI, France]	Considered. The revised ES provides an exhaustive list of AR6 regions displaying projected increases in droughts, depending on the considered drought type.
105189	8	44	8	44	Replace “some remaining” with “many remaining”. There are many remaining uncertainties. [Francis Zwiers, Canada]	Not applicable. This comment seems to be misplaced and should have been indicated for page 9, line 44 (prior section on "limits to the assessment"). This section has been removed from the revised ES version because it was not informative enough and very general.
100075	8	44			Suggest "changes in global temperature as small as 0.5°C" (rather than "changes as small as 0.5°C in global warming") [Ronadh Cox, United States of America]	Not applicable. The text has been substantially revised. This wording is no longer used. Effects of changes of 0.5°C in global warming on droughts are now addressed in the 3rd paragraph of the ES with simpler wording.
105191	8	45	8	45	Replace “have become much more robust” with “has become more robust”. Evidence is singular (thus use has instead of have), and while evidence has developed, there is still many questions, so I would not suggest extra emphasis. [Francis Zwiers, Canada]	Not applicable. This comment seems to be misplaced and should have been indicated for page 9, line 45 (prior section on "limits to the assessment"). This section has been removed from the revised ES version because it was not informative enough and very general.
39249	8	45	8	46	Can these "some regions projected to become drier" be mentioned? [Lourdes Tibig, Philippines]	Considered. The revised ES provides an exhaustive list of AR6 regions displaying projected increases in droughts, depending on the considered drought type.
105193	8	45	8	48	Any statement in this chapter about tipping points should, presumably, be focused specifically on extremes, so it would appear that some clarification is required here. [Francis Zwiers, Canada]	Not applicable. This comment seems to be misplaced and should have been indicated for page 9, line 45 (prior section on "limits to the assessment"). This section has been removed from the revised ES version because it was not informative enough and very general.
113497	8	46	8	46	Perhaps clarify: 'this has to do wih the more direct and local effect of evaporation trends on soil moisture than on precipitation'. [Diego Miralles, Belgium]	Noted. The text has been substantially revised and this sentence is no longer included. But the role of evapotranspiration in increasing trends in agricultural and ecological droughts is highlighted in the revised text.
96115	8	48	8	48	Please rename the headline from 'Storms' in 'Tropical cyclones' (as in TS 2.7.4), since all 3 adjacent paragraphs are dealing with these tropical events only [Nicole Wilke, Germany]	Considered. The headline statement was renamed "Extreme storms, including tropical cyclones (TCs)".
62377	8	48	8	49	Under the "Storms" heading there are many extreme events are missing, we can mention hailstorm or heatstress events likewise both are very common in South Asian countries and agriculture is highly vulnerable. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The ES summarizes the assessment from chapter 11. Hail storms are addressed in more detail in chapter 12. Heat stress events do not belong under storms.
24085	8	50	8	50	"Stronger" - stronger than what? Suggest re-wording to be more precise as to meaning. [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The text has been revised to clarify that it is referring to Cat3-5 TC conditions: "It is likely that the global proportion of category 3-5 tropical cyclone instances (FOOTNOTE) has increased over the past four decades”. The FOOTNOTE is “6-hourly intensity estimates during the lifetime of each TC”.
51569	8	50	8	50	"...proportion of stronger tropical cyclones (TCs) has increased..." - stronger than what? Suggest a re-wording to be more precise as to meaning of this statement. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The text has been revised to clarify that it is referring to Cat3-5 TC conditions: "It is likely that the global proportion of category 3-5 tropical cyclone instances (FOOTNOTE) has increased over the past four decades”. The FOOTNOTE is “6-hourly intensity estimates during the lifetime of each TC”.

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39835	8	50	8	50	"stronger tropical cyclones" -> Which categories? [TSU WGI, France]	Noted. The text has been revised to clarify that it is referring to Cat3-5 TC conditions: "It is likely that the global proportion of category 3-5 tropical cyclone instances (FOOTNOTE) has increased over the past four decades". The FOOTNOTE is "6-hourly intensity estimates during the lifetime of each TC".
9137	8	50	8	50	There is no statement about observed changes in TC frequency, yet this is highly policy-relevant. Insert the following statement from page 93 lines 42-44 "There is low confidence in TC frequency-based or intensity-based trends due to changes in technology used to collect the best-track data". [Kevin Hennessy, Australia]	Considered. "Low confidence" statement is generally not used in ES. The revised ES has a medium confidence about "decrease or remain unchanged" about global TC warming with warming. This implicitly covers past changes in total TC frequency.
105177	8	50	8	50	Define "stronger" TCs. [Francis Zwiers, Canada]	Noted. The text has been revised to clarify that it is referring to Cat3-5 TC conditions: "It is likely that the global proportion of category 3-5 tropical cyclone instances (FOOTNOTE) has increased over the past four decades". The FOOTNOTE is "6-hourly intensity estimates during the lifetime of each TC".
125835	8	50	8	51	[CONFIDENCE] "There is medium confidence that the global proportion of stronger tropical cyclones (TCs) has increased detectably over the past 40 years." Confidence level for detectable (unusual compared to natural variability) increase in proportion of stronger TCs should be low confidence. If IPCC allows split confidence levels, the authors could consider between low confidence and low-to-medium confidence. One can say medium confidence it has increased, but researchers don't really know why, and researchers don't have medium confidence that the increase is unusual compared to natural variability. Rationale: The conclusion that there is medium confidence that an increase in proportion of stronger TCs globally has become detectable is based on the likely increase in the proportion of stronger TCs globally over the past 40 years, and the statement that this is consistent with theoretical understanding and numerical simulations (citing Knutson et al. 2015, 2019b, and Walsh et al. 2015, 2016, Bender et al 2010 and Kossin et al. 2013). Also cited is the new Kossin et al. (2020) manuscript reporting the observed trend. None of these studies provide convincing evidence that the change reported by Kossin (2020) is outside the range of behavior expected from natural variability -- which is what must be demonstrated for detection. This is different from finding that an observed change over some time period is similar to a modeled signal: One must still show that the change is highly unusual compared to natural variability, otherwise such agreement with a projection could be coincidental and not indicating detection. One way detection could be done is to compare the observed trend in the metric to a distribution of trends in climate model long control runs or large ensembles of natural forcing only runs. The modeled signals cited in the above papers are not comparable to the observed change in Cat 3-5 proportion in any case. Further, Bender et al. and Knutson et al. 2015 are based on future climate change scenarios, not historical simulations and so are not really comparable to observed changes over the historical period. The 10% per decade increase in Cat 4-5 numbers in Bender et al. (2010) was reduced somewhat in their expanded simulation study for the Atlantic (Knutson et al. 2013) and their global study, such that they no longer found a statistically significant increase in Cat 4-5 frequency in the Atlantic. Even with this diminishment of signal to noise in the updated study, the Bender et al. study still estimated it would take about six decades along an IPCC A1B scenario for a detectable signal to emerge in Atlantic Cat 4-5 frequency. Sobel et al. (2016, see Figures 3 and 4) show that aerosols may have offset much of the impact of historical GHG warming on TC intensity, with an expected signal only beginning to emerge near the end of the 20th century. Their Figure 4 looks at NH Power Dissipation (not proportion of Cat 3-5 storms) and find that multidecadal variability in the observed NH mean TC power dissipation index may dwarf the expected climate change signal in that metric due to anthropogenic forcing since 1950. As these metrics are related this could indicate how natural variability could confound detection over multiple decades. What Kossin (2020) shows is a strong rising trend in proportion of Cat 3-5 hurricanes for 1979-2017. No formal detection was claimed, and anthropogenic influence was not quantified. The signal in Kossin et al. (2020) is strongest in the N. Atl. and South Indian. The signal is weak in NW Pac and slighly negative in N. Ind., with moderate rise in the NE Pacific and South Pacific. In the N. Atlantic, the increase could be due to aerosol reduction or natural variability playing big roles (e.g., Murakami et al., Bhatia et al.), so its difficult to claim that it is a GHG forced rising signal (in the N. Atlantic). So the statistical significance of this change at present may come down to trying to understand why the South Indian Ocean has such a strong rising signal. This will require further study. Unfortunately, researchers don't yet know what the natural (internal) climate variability of this metric on multidecadal time scales looks like. [Trigg	Considered. The revised ES separates the detection of change from attribution. Thus the confidence about a change being detected is not related to evidence of attribution.
34953	8	50	8	54	The SOD asserts that the global proportion of stronger tropical cyclones has increased detectably over the last 40 years; yet the facts say otherwise, see http://www.policlimate.com/tropical/, which demonstrates that global peak cyclone energy peaked in the 1990s. See general comment #12 above. [Jim O'Brien, Ireland]	Noted. The assessment is about the proportion of stronger tropical cyclones in total tropical count rathe than the total count of stronger tropical cyclones.
45677	8	50	9	1	The paragraph is confusing [Christophe Deissenberg, Luxembourg]	Considered. The text has been substantially revised for increased clarity.
79871	8	50	9	26	Information on extra-tropical cyclones is too limited and hidden amongst tropical cyclone information making it difficult to get an overview of ETC results. There has obviously been a deliberate choice to try and present TCs and ETC together but I dont think it works. There will be many users that are only interested in one. Also I don't think there is any information on frequency or track location/speed/severity changes in the exec summary. [Simon Brown, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We understand the reviewer's point. However, the assessment of TCs and ETCs differs substantially in this report because different aspects of ETCs are assessed in different Chapters (2, 3, 4, 8 and 11) while TCs are essentially assessed in Ch. 11. The ES of Ch. 11 thus includes high confidence statements related with extreme ETCs and our assessment concluded that this are associated with associated precipitation rates. Other aspects of ETCs have been elevated to the ES of other chapters
45675	8	51	8	51	it ==> WMO [Christophe Deissenberg, Luxembourg]	Noted. This comment seems to be misplaced, "it" is not mentioned in the text.
105179	8	52	8	53	Delete "substantially". How big does a latitude shift have to be to be "substantial"? It is presumably large enough to be discernably larger than zero according to some objective criterion, so I would think that simply saying that there is an increase is sufficient. Is there documented evidence that exposure has increased at higher latitudes? If there is evidence, is this limited to the eastern seaboard of North America, or is the evidence more widespread? [Francis Zwiers, Canada]	Noted. The text has been revised, "substantially" is no longer used.
39879	8	53	8	53	"unlikely" -> Is this confidence language? [TSU WGI, France]	Not applicable. The text has been revised, and the respective sentence removed.
39251	8	53	9	5	What exactly are data artefacts? Is "unlikely" an uncertainty language? If it is, then it should be italicized.Isn't it that if it can not be explained by natural variability, this wukd be a n evidence for anthropogenic influence? [Lourdes Tibig, Philippines]	Not applicable. The text has been revised, and the respective sentence removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
125837	8	54			[CONFIDENCE] Confidence level that the increase in latitude of maximum intensity in NW Pacific since 1940s cannot be explained by natural variability should be low-to-medium confidence (if IPCC allows split confidence levels), otherwise low confidence. One can say with medium confidence the latitude has increased, but researcheers don't really know why the increase has occurred, and don't have medium confidence that the increase is unusual compared to natural variability. Rationale: The WMO TC/climate assessment (Knutson et al.. 2019a) assessed this finding. The author team for that report expressed the following opinion on confidence levels (Table 1): low to medium confidence, 8 authors; medium confidence, 1 author; medium to high confidence, 2 authors. IPCC does not report distribution of opinion, but a single confidence level. This case study was discussed in detail in Knutson et al. 92019a), and there are no new published findings on it since that assessment. The methodology of assessing how unusual the observed change is compared to natural variability consists of regressing out ENSO, PDO (or IPO), and the AMO and examining trend of the residuals. This assumes that natural multidecadal variability in the metric is linearly related and well described by some combination of the predictors with little influence of any other process (e.g., atmospheric internal variability, coupled variability unrelated to the predictors) not included in the predictor list. The ability of these predictor variables to statistically describe the variability of the TC metric could be more thoroughly explored using climate model control runs which would give more confidence, though not complete confidence in this methodology. [Trigg Talley, United States of America]	Considered. The revised ES separates the detection of change from attribution. Thus the confidence about a change being detected is not related to evidence of attribution.
125839	9	1	9	2	[CONFIDENCE] Confidence level should be low confidence for"forward motion (translation speed) has slowed detectably over the US since 1900." This would actually be a good candidate for a balance of evidence statement in the case of trying to avoid Type II errors as discussed by Knutson et al. (2019a), if IPCC were using that approach. In Knutson et al. (2019a), there was a balance of evidence/Type II error avoidance statement for detection of a global reduction in TC propagation speed, but that should now be dropped altogether for the global reduction, owing to the Comment and Reply in Nature on this topic (which was published too late for Knutson et al. to consider). Instead, recommend applying a similar balance of evidence/Type II error avoidance statement to the continental US TC slowdown result since 1900 (detectable vs. not detectable). However, since IPCC is not using this Type II error/ balance of evidence approach, the recommendation that fits with their handling of confidence levels is just low confidence for the continental US propagation speed decrease since 1901. A few related comments/questions on the continental U.S. slowdown finding follow, as this was not discussed in Knutson et al. (2019a). One of the difficulties with this type of analysis concerns how long the propagation speed of an individual tropical cyclone is tracked over U.S. land. At some point, the tropical cyclones may transition to extratropical cyclones. Is that when the propagation speed tracking is discontinued for that storm? In other words, what specific criterion was used to decide when to stop computing a propagation speed for a storm that would contribute to the annual mean value for a given year? Are we assured that the process of determining when a tropical cyclone is no longer a tropical cyclone is something that is homogeneous over time since 1900? It would be much harder to "make the call" on extratropical transition in 1900 than during the satellite era, even over land. Another issue is the small sample size (going from global to just U.S. land) and the presumably non-normal nature of the propagation speed data (zero bounded below) and with likely a skewed distribution at higher speeds, as storms pick up speed in the higher latitude westerlies, leading at times to large values. How robust is the trend analysis to this non-normality? Is the trend influenced by a small number of large values (tail of distribution) and are there enough independent samples in each year to estimate a robust mean value for that year? Should the annual median value be used instead to construct the time series? [Trigg Talley, United States of America]	Considered. The revised ES separates the detection of change from attribution. Thus the confidence about a change being detected is not related to evidence of attribution.
65081	9	1	9	5	I am concerned that the assessment of TC translation speed change is different in this chapter than that in Chapter 8. Here the assessment is "medium confidence" that TCs have slowed, with "low confidence" of a global signal due to data heterogenity , but in Chapter 8, the assessment is "low confidence" of TC slowdown, based in part based on one of the same global studies. [Laurie Agel, United States of America]	Noted. The statements in the FGD version are consistent now.
24105	9	1	9	7	The chapter seems to have ducked make any assessment of the attribution of TCs. The chapter should include such an assessment. What happens if this is asked about in the plenary approval session of the SPM and there is no chapter assessment to support a response? [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Considered. Attribution literature is now addressed in the revised ES (page 9, lines 23-24).
51577	9	1	9	7	The chapter seems to have omitted making any assessment of the attribution of TCs. The chapter should include such an assessment. What happens if this is asked about in the plenary approval session of the SPM and there is no chapter assessment to support a response? Please could you summarise in section 11.7.4.1 what your overarching assessment of TC attribution is, and how this compares/builds on to those made in AR5 and the SROCC. This could then be usefully included in the 'Storms' section of the chapter executive summary. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Considered. Attribution literature is now addressed in the revised chapter, as well as in the ES (page 9, lines 23-24): "Available event attribution studies of observed strong TCs provide medium confidence for a human contribution to extreme TC rainfall."
112713	9	1			Impressive advances on TCs worth highlighting in the SPM [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thank you for the positive feedback!
13663	9	2	9	2	standardize the US or U.S. format [Maria Amparo Martinez Arroyo, Mexico]	Considered. Now using "USA".
24087	9	3	9	5	Is this low confience correct given the slow down is consistent with theory and modelling studies of circulation changes. Low confidence implies we don't know whereas perhaps we do especially if we have reasons to suspect climate models aren't yet up to simulating TC speeds accurately - is medium confidence not justified? [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The new ES text does not contain a similar statement.
51575	9	3	9	5	The low confidence statement here seems to be inconsistent with theory and modelling studies of circulation changes and slowdown? Low confidence implies we don't know - it would be helpful to explain here the reason for the low confidence statement and if there is evidence to suggest that climate models aren't yet up to simulating TC speeds accurately. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The new ES text does not contain a similar statement.
18689	9	4	9	5	I believe that the theory tells us that the vertical motion (or horizontal convergence) would decrease under warming, not the translational speed. [Govindasamy Bala, India]	Not applicable. The new ES text does not contain a similar statement.
49945	9	4	9	17	It should also be noted here that it isn't just the lack of robust model agreement, but also a fundamental lack of the models to accurately physically model TCs for a long enough period of time at sufficient resolution. Because of models are phyiscally lacking in very basic ways to capture TC properties at sufficient scale, even robust agreement would not substantially change the fact that there is low confidence in future TC changes. I.e. some reference to the uncertainties noted on pg 11-98 is needed. [Daniel Gilford, United States of America]	Considered. Statements are recalibrated with evidence in the new text.
39253	9	9	9	10	Does this confirm a SROCC statement about more severe TCs annually-meaning typhoons becoming more severe even in the western North Pacific? [Lourdes Tibig, Philippines]	Noted. The statement is for a global assessment. Assessment for smaller scale would normally has lower confidence.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
125841	9	9	9	10	[CONFIDENCE] Confidence levels should be: medium-to-high confidence (as in the WMO TC/climate assessment, Knutson et al. 2019b) for the statement that average peak TC wind speeds and the proportion of Cat 4-5 TCs will increase globally with warming. If this is not available due to IPCC rules, then medium confidence for both. Rationale: There is good model agreement on an increase but evidence is still lacking for a clear detection of an observed increase (i.e., that an observed increase is highly unusual compared to expected changes realizable from natural variability only). This is an essential part of a case for high confidence in a projection (don't just rely on models and theory for confidence, but that actually see the change unambiguously in the data, and it's clearly distinguishable from natural variability). One of the tricky things about future changes of mean intensity, or of the related change in the shape of the pdf of intensity, is that it depends not just on SST but also on details of the atmospheric profile of temperature change in the tropics. Both models and theory indicate this. For Emanuel potential intensity, it's through outflow temperature changes, while in a high resolution hurricane prediction model it's the amount of upper tropospheric warming relative to surface warming as discussed, for example, in Tuleya et al. (2016: Impact of upper tropospheric temperature anomalies and vertical wind shear on tropical cyclone evolution using an idealized version of the operational GFDL hurricane model. Journal of the Atmospheric Sciences, 73(10), DOI:10.1175/JAS-D-16-0045.1). Unfortunately, there is also uncertainty in precisely how tropical upper tropospheric temperature is going to evolve in the future, which contributes to uncertainty in the TC intensity projections, along with changes in subsurface ocean stratification, salinity, etc. [Trigg Talley, United States of America]	Considered. The AR5 and AR6 assessments, a change is observed is NOT a necessarily condition for a high confidence for future projection.
88545	9	9	9	17	It would be nice if storm surge and wave extremes change could be mentioned here [Baylor Fox-Kemper, United States of America]	Noted. Storm surges are addressed under the section on compound events.
113499	9	9	9	26	I have not double-checked this, but I got the feeling in Ch8 the level of confidence in TC changes was reported as lowerr. Make sure it is consistent. [Diego Miralles, Belgium]	Noted. The statement in the FGD version of Chapters 8 and 11 are cross-checked and consistent.
14619	9	12	9	15	These TC projections are for which scenario/warming level? [Roshanka Ranasinghe, Netherlands]	Noted. The assessment is in relation to warming, the emphasis here is about "with warming".
68725	9	13	9	15	This sentence pretty much says nothing. It says that there is medium confidence that wind speeds associated with TCs will change with some (unspecified) regions experiencing increases and other (unspecified) regions experiencing decreases. What should the reader conclude from that? [Bodeker Greg, New Zealand]	Considered. This sentence is removed in FGD.
109341	9	15	9	17	Please clarify if this result holds everywhere or only where spring can be defined or if some other addition is appropriate. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Considered. The revised text clarifies that the assessment applies to the USA and that evidence is limited elsewhere.
82747	9	15	9	17	The result on severe convective storms is specific to the United States, not global as implied here. Also affects table on p25. [Blair Trewin, Australia]	Considered. The revised text clarifies that the assessment applies to the USA and that evidence is limited elsewhere.
39255	9	15	9	17	There is meium confidence confidence that the frequency of springtime severe convective torms (such as hail and severe thunderstorms) will increase, leding to a lrengthening of the severe convective storm seasonwheres there is low confidence in past trends of its characteristics (lines 5-7)? [Lourdes Tibig, Philippines]	Considered. The revised text clarifies that the assessment applies to the USA and that evidence is limited elsewhere.
96117	9	16	8	16	Is it possible to mention in the chapter title that this is one of the three "regional chapters"? Applies to chapter 12, too. Title of chapter 10 is already clear. [Nicole Wilke, Germany]	Rejected. The chapter title is provided in the IPCC scoping document and cannot be changed by the authors.
125843	9	19	9	20	[CONFIDENCE] "There is high confidence that average and maximum rain-rates associated with tropical cyclones" will increase as atmospheric water vapor increases with warming. At least for TCs, this should be medium-to-high confidence (as in the WMO TC/climate assessment, Knutson et al. 2019b). If this is not available due to IPCC rules, then medium confidence. This projection has high level of agreement among existing modeling studies (although not as many studies have examined this as TC frequency change), the mechanistic understanding is strong, as is support for anthropogenic increases in total precipitable water, a key ingredient. What remains missing is a clear detection of an observed increase (i.e., that an observed increase is highly unusual compared to expected changes realizable from natural variability only). This is an essential part of a case for high confidence in a projection (don't just rely on models and theory for confidence, but actually see the change unambiguously in the data, and it's clearly distinguishable from natural variability). Recent detection/attribution studies for the Harvey event by Risser and Wehner and van Oldenbourgh et al., while of high quality, analyze observed long-term changes in extreme precipitation in general, not tropical cyclone precipitation. Additionally, the Harvey event was mainly due to the multi-day stall-out of the hurricane in the region. [Trigg Talley, United States of America]	Considered. The AR5 and AR6 assessments, a change is observed is NOT a necessarily condition for a high confidence for future projection.
50045	9	19	9	26	It could be useful to add here some information regarding the excess floods that are expected when more intense TCs will be combined with higher sea levels (I see something like this is mentioned in the coumpound extremes session, but it could be useful here as well). [Eftychia (Efi) Rousi, Germany]	Noted. This is addressed in the compound events section.
125845	9	22	9	22	In this context, what temperature is being talked about? Ocean? Atmosphere, surface or aloft? And on what horizontal and temporal scale? Global? Or...? [Trigg Talley, United States of America]	Considered. This is now explicit about "local temperature".
68727	9	23	9	23	wind intensity' not 'windintensity' [Bodeker Greg, New Zealand]	Not applicable. The text has been substantially revised.
11649	9	23	9	23	insert space in “wind intensity” [Amy East, United States of America]	Not applicable. The text has been substantially revised.
42427	9	23	9	23	Typo: windintensity -> wind intensity [Joan Bech, Spain]	Not applicable. The text has been substantially revised.
89285	9	23	9	23	‘windintensity’ to ‘wind intensity’ [Tinghai Ou, Sweden]	Not applicable. The text has been substantially revised.
43309	9	23			Read "by regional increases in TC wind intensity " rather than "by regional increases in TC windintensity " [Cyriaque Rufin Nguimalet, Central African Republic]	Not applicable. The text has been substantially revised.
109343	9	24	9	24	Add "projected" before "increase". [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Considered. This sentence is no longer included.
20229	9	24	9	26	This sentence means to address the magnitude of precipitation increase as projected by numerical simulations, rather than what will happen in the real world. This should be made clear. [philippe waldteufel, France]	Considered. This sentence is no longer included.
96119	9	24	9	26	We do not find any verification of the "high confidence". Please revise. [Nicole Wilke, Germany]	Considered. This sentence is no longer included.
84889	9	24	9	26	The "magnitude of the increase in precipitation" itself doesn't depend on model resolution, the modelled representation of the magnitude of the increase in precipitation depends on the model resolution. [Turner Jessica, United Kingdom (of Great Britain and Northern Ireland)]	Considered. This sentence is no longer included.
7365	9	24	9	26	Unclear wording. At the least, insert 'simulated' before 'magnitude of the increase' [Hans-Martin Füssel, Denmark]	Considered. This sentence is no longer included.
112715	9	24		26	no what will happen doesn’t depend on the resolution ;) what the models simulate does [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Considered. This sentence is no longer included.
108859	9	30	9	31	Is there really high confidence that this holds globally? [Erich Fischer, Switzerland]	Considered. The assessment is provided in Section 11.8.3 and is based on an extensive literature basis.
23079	9	30	9	38	Finding ignores sequenced events such as e.g. lates season cold and snow followed by drought (2018 NW Europe) or hurricane followed by heatwave (Florida, 2018). These are not concurrent but rather consecutive. Should compound events be expanded here to include such a definition? Later text implies they should. [Peter Thorne, Ireland]	Noted. Literature on compound event is an emerging field and thus only a few topics covered with sufficient literature could be considered.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
113501	9	30	9	38	Again here, this is ill-phrased: you can always report with plenty of confidence that any change (and its opposite) has occurred 'somewhere'. Unless you list where, the statement mean very little. [Diego Miralles, Belgium]	Noted. The text has been revised to make it clearer. Where "some regions" are mentioned ("The probability of compound flooding (storm surge, extreme rainfall and/or river flow) has increased in some locations"), these regions are identified in Section 11.8.
44383	9	30	9	38	The use of the term "risk" in this paragraph is not according the IPCC guidance on risk document. Replace with the word "probability". WG1 assesses the climatic impact drivers (probabilites or trends) and only WG2 can make statements about risk taking aspects of exposure and vulnerability into account. [Jana Sillmann, Norway]	Considered. The word "risk" has been replaced with "probability", except in the cited definition.
84891	9	30	9	38	I would like to see a discussion of the important compound event of extratropical cyclone wind damage with flooding. [Turner Jessica, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Literature on compound event is an emerging field and thus only a few topics covered with sufficient literature could be considered.
17141	9	30	9	38	The key sentece of this paragraph is the last sentence. I suggested that the last sentence is placed at the beginning of the paragraph, with this change: "There is medium confidence that various risks of other compound events will increase under higher levels of global warming." The word other is unprecise, better deleted. The various risks wiil be explained by the rest following sentences. [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Considered. The new headline statement for this paragraph now addresses compound events in general and is adapted from the previous last sentence.
39473	9	31	9	32	As the sentence mentioned 'affecting similar sectors (e.g. breadbaskets)', consider changing 'breadbaskets' into 'agriculture' as it is a sector [Tamara van 't Wout, Qatar]	Considered. The word "breadbasket" was replaced with the term "critical crop-producing areas for global food supply".
62855	9	31	9	34	Consider rephrasing/simplifying the following sentence to make the meaning of locations, regions and sectors clearer in the context: "There is high confidence that concurrent extremes events at different locations, but possibly affecting similar sectors (e.g., breadbaskets) in different regions, will become more frequent at higher levels of warming, in particular above 2°C of global warming. " [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The sentence was revised to make it clearer.
11651	9	32	9	32	delete the “s”, just “extreme events” [Amy East, United States of America]	Accepted. This was corrected.
49947	9	34	9	35	For compound flooding, should changes in TC intensity and storm surge also be included alongside sea level and heavy precipitation factors? [Daniel Gilford, United States of America]	Noted. Since compound flooding also occurs in regions not affected by TCs, the text was kept more general only referring to increasing heavy precipitation.
62509	9	34	9	36	Executive summary: "There is medium confidence that the likelihood of compound flooding (storm surge, extreme rainfall and/or river flow) has increased in some locations, and will continue to increase due to both sea level rise and increases in heavy precipitation.". I would suggest that the compound event should be further considered by superimposing on the maximum spring high tide event. The confidence level should be attributed accordingly. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Literature on compound event is an emerging field and thus only a few topics covered with sufficient literature could be considered.
109345	9	36	9	36	Add "weather" after "wildfire". [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Done.
44429	9	36	9	38	replace "risk" with "probability". See IPCC guidance on risk for appropriate use of the term "risk" throughoput the IPCC report. [Jana Sillmann, Norway]	Accepted. The text was revised to remove the term "risk".
107385	9	37	9	37	"in some regions" - also for consistency with other sections, it would be good to name a few of these "some regions", otherwise there is not much information in this sentence [Markus Donat, Spain]	Noted. Chapter 12 is providing a more detailed regional assessment. This assessment is only relating wildfire probability to compound hot and dry conditions.
130557	9	37	9	37	"risk" shoud be for the future. As this part is for the past, perhaps we can consider to use "potential" to replace "risk". [Panmao Zhai, China]	Noted. The text was revised to remove the term "risk".
23081	9	40	9	50	Most other chapters do not include limits as an ES finding. Consistency may need to be strived for here? [Peter Thorne, Ireland]	Noted. This section was removed.
84895	9	42	9	49	The difficulties modelled extremes that result from insufficient spatial or temporal resolution is discussed several times in the body of the paper but is not mentioned in this section on limitations. Resolution seems like an important limitation. [Turner Jessica, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This section was removed.
100077	9	42			Change "assessments on changes" to "assessments of changes" [Ronadh Cox, United States of America]	Not applicable. This section was removed.
68729	9	43	9	43	Replace'in past and future' with 'in the past and future'. [Bodeker Greg, New Zealand]	Not applicable. This section was removed.
62719	9	43	9	43	such as hail should be replaced with such as hail and thunderstorm [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This section was removed.
100079	9	43			Change "extreme events in past and future" to "extreme events both past and future" [Ronadh Cox, United States of America]	Not applicable. This section was removed.
100081	9	43			Place "such as hail" in parentheses; OR change sentence to "Some topics, such as hail, are still insufficiently investigated" [Ronadh Cox, United States of America]	Not applicable. This section was removed.
100083	9	44			Delete "some"; let phrase read "there are remaining uncertainties" [Ronadh Cox, United States of America]	Not applicable. This section was removed.
68731	9	45	9	45	Replace 'evidence have' with 'evidence has'. [Bodeker Greg, New Zealand]	Not applicable. This section was removed.
39257	9	45	9	48	Indeed, It is necessary to alert policymakers of the fact that even low-probability, high-impact events related to extreme events should not be discounted, since the high warming level of a 3 °C warming is nearing. [Lourdes Tibig, Philippines]	Noted. This section was removed, but a new section on "low-likelihood high impact (LLHI) events" was added-
17143	9	45	9	49	I suggest these changes: Also, there is low confidence regarding the global warming levels at which possible changes associated with in global and regional tipping points (low-probability high-impact events) related to of extremes would occur. However, these changes cannot be excluded, especially at high global warming levels (>3°C). [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This section was removed.
100085	9	45			Change "evidence have become" to "evidence has become" [Ronadh Cox, United States of America]	Not applicable. This section was removed.
51579	9	47	9	47	Low-likelihood high impact probability events and tipping points are not the same thing; an extreme event is by definition low-probability but isn't necessarily a tipping point. Could you please amend? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This section was removed. A new section on "low-likelihood high impact (LLHI) events" was added.
42429	9	47	9	47	Typo: events)related ->events) related [Joan Bech, Spain]	Not applicable. This section was removed.
6853	9	47	9	47	introduce space after the parenthesis : (low-probability high-impact events)related [Constantinos Cartalis, Greece]	Not applicable. This section was removed.
89287	9	47	9	47	‘events)related’ to ‘events) related’ [Tinghai Ou, Sweden]	Not applicable. This section was removed.
43311	9	47			Read " tipping points (low-probability high-impact events) related to extremes " rather than " tipping points (low-probability high-impact events)related to extremes [Cyriaque Rufin Nguimalet, Central African Republic]	Not applicable. This section was removed.
100087	9	48	9	49	Change "there are still remaining important data and literature gaps..." to "important data and literature gaps remain..." [Ronadh Cox, United States of America]	Not applicable. This section was removed.
39259	9	48	9	50	Please do not discount data sparsity in some parts of Asia, which are teeming with populations living in the most vunerable delts and coastlines. [Lourdes Tibig, Philippines]	Not applicable. This section was removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
80693	9	49	9	49	There is limited information regarding extremes from the Pacific SIDS. This region should be added in the region with data and literature gaps. In addition, there is no reference about data and literature gaps in South America in the 'Limits of the Assessment' section on page 147 [Helene Jacot Des Combes, Marshall Islands]	Not applicable. This section was removed.
113503	9	49	9	49	South' [Diego Miralles, Belgium]	Not applicable. This section was removed.
89605	9	49			There are also still data and literature gaps over the small islands. [Faye Abigail Cruz, Philippines]	Not applicable. This section was removed.
81259	9		9		Please avoid using the teminology "Limits to the assessment", this means limits to the work/efforts done which is different from limits in the available literrature/knowledge [Fatima Driouech, Morocco]	Considered. This section was removed because it was not informative enough.
52075	10	5	5	48	It may be important to mention the impact of Climate extremes briefly which is discussed detail in Chapter 12 [Amarasinghage Tharindu Dasun Perera, Switzerland]	Noted but no action is taken. We have already mentioned that Chapter 12 assess hazard or climate impact drivers in which impacts of extremes will be briefly discussed. Assessment of impact is with the WGII report.
68733	10	5	10	5	Assessment of' is the more usual term than 'Assessment on'. [Bodeker Greg, New Zealand]	Accepted.
24091	10	7	10	7	It's interesting that the IPCC defines a extreme climate event as a "pattern" yet extreme event attribution as defined later in the chapter is in terms of exceedance of thresholds (as elucidated in page 11 lines 19 etc). I realise this is the focus of much of the literature but this presentation in the draft chapter highlights a current research concern - ie an aim to investigate events more as patterns and attributing those patterns, which also means understanding mechanisms better rather than just statistical distributions of extremes. Even if the impacts users allegedly don't care about the meteorology, this definition of extreme climate events as involving patterns provides some justification for arguing that very different patterns that are associated with similar thresholds being exceeded should be treated differently (eg models should be inspected to see if they are getting threshold exceedances for the right reasons, ie the patterns that are actually observed are being simulated). Arguably the overall confidence in IPCC conclusions from event attribution studies should take account if studies do not consider extreme climate events as defined by IPCC (as patterns) but rather merely as exceedance of thresholds regardless of the patterns involved in causing them (I think implicit in the problematic sentence at page 35 lines 3 to 4 - see later comment on that sentence - is a recognition of this point about events defined as exceedance of specific thresholds are not the same as events defined as patterns, ie multi-variable constructs). It would be worth a comment on whether the usual statistical tests often applied are sufficient versus a more comprehensive analysis of the prevalence of the relevant patterns (regimes, teleconnections etc). More fundamentally, it would be worth checking that the headline conclusions on attribution of extreme events in this report, where events are defined as patterns (not threshold exceedances), are supported based on the evidence from the literature if some of that literature is framed in terms of single thresholds, which are not patterns. It would be worth un-packing a bit this issue of event defintion. [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Considered. The definition of extreme weather event and extreme climate events in this chapter follows the AR5 glossary. While it is important and interesting to un-packing the implication of "pattern" vs threshold-crossing on changes in extremes and event attribution in particular, there is not a lot literature on this. It is tempting to un-packing more but that also seems to cross (at least blur) the line between assessment and new research.
51581	10	7	10	7	It's interesting that the IPCC defines a extreme climate event as a "pattern" yet extreme event attribution as defined later in the chapter is in terms of exceedance of thresholds (as elucidated in page 11 lines 19 etc). I realise this is the focus of much of the literature but this presentation in the draft chapter highlights a current research concern - ie an aim to investigate events more as patterns and attributing those patterns, which also means understanding mechanisms better rather than just statistical distributions of extremes. The overall confidence in IPCC conclusions from event attribution studies should take account if studies do not consider extreme climate events as defined by IPCC (as patterns) but rather merely as exceedance of thresholds regardless of the patterns involved in causing them (I think implicit in the problematic sentence at page 35 lines 3 to 4 - see later comment on that sentence - is a recognition of this point about events defined as exceedance of specific thresholds are not the same as events defined as patterns, ie multi-variable constructs). It would be worth a comment on whether the usual statistical tests often applied are sufficient versus a more comprehensive analysis of the prevalence of the relevant patterns (regimes, teleconnections etc). More fundamentally, it would be worth checking that the headline conclusions on attribution of extreme events in this report, where events are defined as patterns (not threshold exceedances), are supported based on the evidence from the literature if some of that literature is framed in terms of single thresholds, which are not patterns. It would be worth un-packing a bit this issue of event defintion. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Duplicate of #24091 (above). See response to comment #24091
1427	10	8	10	10	Difficult sentence. Why not use the WMO description of risk and hazard? https://public.wmo.int/en/resources/bulletin/quantifying-risk-disasters-occur-hazard-information-probabilistic-risk-assessment [Rasmus Benestad, Norway]	Noted. Sentence is reformulated.
51583	10	9	10	12	The wording of this sentence is a bit confusing. Are you trying to say that extreme events affect exposure and resilience to *future* extremes? There are also other factors that influence people and systems' exposure and resilience to extremes. Could you clarify this phrasing please? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Sentence is reformulated.
44385	10	10	10	10	please correct: … extremes (which are termed climatic impact drivers in the risk framework, see also Chapter 1 Cross-chapter box 1.2 and Chapter 12). [Jana Sillmann, Norway]	Noted. Sentence is reformulated.
109347	10	11	10	11	It is not clear how extremes influence "exposure and resilience" (other than more hazards reducing resilience) thus suggest removing this. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Sentence is reformulated.
100089	10	22			Change "to assess" to "to the assessment of" [Ronadh Cox, United States of America]	Noted. The sentence is removed.
109349	10	23	10	23	Change "three" to "four" and add Atlas in the brackets. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The sentence is removed.
62857	10	23	10	25	Consider shortening and being more to the point. E.g. something like..."We assess changes in extremes from a global and continental perspective to provide a large-scale context and we provide a regional focus on changes in extremes at regional scales." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. The introduction is shortened.
20231	10	35	10	35	understanding [philippe waldteufel, France]	Noted. Sentence is removed.
109351	10	35	10	35	Before "Chapter 12" add " the Atlas provides an assessment of the underlying mean climate changes," [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This does not apply anymore as 11.1.1 is shortened.
68735	10	35	10	35	spelling - understaning [Bodeker Greg, New Zealand]	Noted. The sentence is removed.
66371	10	35	10	36	I don't think this is the role of CH12 [Erika Coppola, Italy]	Noted. This does not apply anymore as 11.1.1 is shortened.
14621	10	35	10	37	This sentence re what Ch 12 does needs to be rephrased. Ch 12 now uses words like indices and CIDs. Perhaps "Chapter 12 synthesizes knowledge from previous chapters and from other climate impact assessment literature to present the spatio-temporal evolution of the main climatic impact drivers in each region and globally, and provides projections mostly by scenario." [Roshanka Ranasinghe, Netherlands]	Noted. This does not apply anymore as 11.1.1 is shortened.
44411	10	36	10	37	According to the terminology introduced in Ch1, and used in Ch12, the word "hazard" needs to be replaced with "climatic impact driver". [Jana Sillmann, Norway]	Noted. This does not apply anymore as 11.1.1 is shortened.
109353	10	37	10	37	Add "climatic impact drivers" after "and" and change "supported by" to "along with". [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This does not apply anymore as 11.1.1 is shortened.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
109355	10	42	10	45	It would help the reader to note that in the subsections deails are given on regional changes in the extremes addressed in each section. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
100091	10	46	10	47	Change "summarizes regional information on extremes by continents in tables" to "presents tables that summarize regional information on extremes, by contnent" [Ronadh Cox, United States of America]	Noted.
79635	10	51	31	18	The whole discussion on the type of extremes and methods used for the observations of extremes does not mention the WMO calculations/development/establishment of climate normals, in particular, for climatological standard normals exchanged among all WMO Members countries. Is there a scientific basis for such an omission? WMO climate normals serve as a benchmark against which recent or current observations can be compared, including providing a basis for many anomaly based climate datasets (for example, global mean temperatures). They are also widely used, implicitly or explicitly, as a prediction of the conditions most likely to be experienced in a given location. Practices regarding climate normals are described in the WMO WMO Guidelines on the Calculation of Climate Normals (2017): https://library.wmo.int/doc_num.php?explnum_id=4166#:~:text=Number%20of%20years%20required%20for%20the%20calculation%20of%20a%20normal,years%20in%20the%20averaging%20period. [Wilfran MOUFOUMA OKIA, Switzerland]	Noted, no action is taken. The WMO guidelines on the calculation of climate normals recommend methods and data requirement for the computation of mean values over a 30-year period for different variables including extreme values. The objective of this chapter is to assess long-term changes and as such, we do not consider guidelines for calculation of normals to be very relevant.
89607	10	51			Since there is a discussion on extreme indices in Technical Annex VII, I suggest to check this annex for consistency with Ch11 and to cross-reference in the chapter (e.g. Table AVII.1). [Faye Abigail Cruz, Philippines]	Noted.
44431	10	53	10	53	Why are you refering to the risk framework defined in SREX and not the updated version of the risk framework used in AR6 (i.e. with reference to Ch1 where it is introduced, incl. links to SREX and AR5). The ambition in AR6 was to follow a harmonized approach in terms of the risk framework across chapters in WG1 and across Working groups. Referring to SREX makes it more difficult to use the modified language (i.e. climatic impact drivers instead of hazards) in the subsequent text in Ch11. [Jana Sillmann, Norway]	Accepted. This is now calibrated with Ch1 definition.
68737	10	53	11	1	I think that a more accurate portrayal of the SREX framework is that exposure and vulnerability to hazards, such as extremes, modulate the impact of those hazards on risk. [Bodeker Greg, New Zealand]	Accepted.
39475	10	53	11	2	Consider to add the word 'disaster risk reduction' and 'which' to "that adaptation and disaster risk reduction reduces exposure and vulnerability, which will increase resilience' according to the disaster risk reduction definition in the SREX report (IPCC, 2012) on page 34 [Tamara van 't Wout, Qatar]	Noted. This does not apply anymore as 11.1.2 is shortened.
66373	10	53	11	3	This sentence is quite confusing “vulnerability of hazard such extreme” [Erika Coppola, Italy]	Noted. Text removed.
20713	10	53	11	17	This discussion stops short of developing the issue of size (meant here as the concerned area) along to magnitude and duration. In terms of impact, one wonders indeed whether a moderate drought over a large area might be more a disaster than a deep drought over a more limited surface [philippe waldteufel, France]	Noted, no action is taken. Spatial scale is implied by the time scale. There is not enough space to discuss difference such as a moderate drought over extended areas and deep drought over a limited space, and such discussion is also not within the scope of this chapter.
52637	10	53	11	53	Extreme events have explained as weather and climate events depending on duration and time. It is very important contribution in order to make the difference clear. But it needs to be explained little bit as short and long terrm extreme events. [Nazan AN, Turkey]	Noted, no action is taken. The length limitation does not permit to have textbook-like text
117061	10		10		A visual guide to the chapter and a clear introduction on links with other chapters is needed. Missing description of links with ch 2-3-4, 5, 8 and 6 (SLCF) and 7 (feedbacks). [Valerie Masson-Delmotte, France]	Considered. This is added.
109357	11	1	11	1	Suggest replacing "magnitude" with "risk" and inserting "adverse" before "impacts". [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is now calibrated with Ch1 definition.
44413	11	1	11	1	The word "hazard" needs to be replaced with "climatic impact driver". [Jana Sillmann, Norway]	Noted. This is now calibrated with definition in Ch1
109359	11	2	11	2	Suggest replacing "impacts" with "these risks". [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This is now calibrated with definition in CH1
125847	11	2	11	5	The dividing line between physical aspects and impacts is not clear. In particular, where do natural ecosystems fit in this framework? And what about managed ecosystems? [Trigg Talley, United States of America]	Noted, no action is taken. SREX report defines climate and flood and drought as physical aspects and every else as impacts.
105965	11	8	11	11	The way the distinction between “extreme weather event” and “extreme climate event” is drawn here could be clarified to note that while the two have differing timescales, both can be affected/influenced/driven/exacerbated by climatic trends. [Sohum Pawar, United States of America]	Noted, no action is taken. The whole chapter shows both can be affected by changes in the climate.
68739	11	9	11	9	Replace 'space and time' with 'spatial and temporal'. [Bodeker Greg, New Zealand]	Accepted.
6855	11	13	11	13	"Some studies consider an event as an…". References need to be provided. [Constantinos Cartalis, Greece]	Noted, no action is taken. This is a common knowledge and there are too many studies for both to cite.
90845	11	15			Using 2013 summer temperature might be overclaim as extreme temperautre (heat waves) has remarkable in northern Europe in year 2018 and so. [Vivien How, Malaysia]	Noted. The region, eastern China, is now added.
24089	11	16	11	16	Is this summer globally? Or just NH? Or? Please specify. [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The region, eastern China, is now added.
82749	11	16	11	16	Need to add "in eastern China" after "temperature" as the quoted result is specific to there. [Blair Trewin, Australia]	Accepted.
51585	11	16	11	16	Is this summer globally? Or just Northern Hemisphere? Please specify. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The region, eastern China, is now added
39851	11	16	11	16	"the 2013 summer temperature" -> What location? [TSU WGI, France]	Accepted. The region, eastern China, is now added
62731	11	16	11	17	just mention the hottest summer in the example but not mention the hottest summer of which location. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The region, eastern China, is now added.
23083	11	16	11	17	I assume this event occurred somewhere (Europe?) but this context needs to be made explicit here. [Peter Thorne, Ireland]	Noted. The region, eastern China, is now added.
107387	11	16	11	17	should specify the region for which this sentence on 2013 summer temperature is valid [Markus Donat, Spain]	Accepted. The region, eastern China, is now added
9139	11	16	11	17	Unclear. Globally-averaged, the 2013 summer was hottest on record? What is meant by "it has a recurrence interval of about 4 years in the climate of 2013"? [Kevin Hennessy, Australia]	Noted. The region, eastern China, is now added.
100093	11	16	11	17	The sentence "For example, the 2013 summer temperature was the hottest on record at the time, but it has a recourrence interval of about 4 years in the climate of 2013? is very unclear...I am not sure what it's trying to say. [Ronadh Cox, United States of America]	Noted. The region was missing which is now added.
74497	11	17	11	17	to correct the word recourrence by recurrence [Moulay Driss HASNAOUI, Morocco]	Accepted
74499	11	17	11	17	may be is not necessary to put "in the climate of 2013" after the recurrence periode of 4 years. [Moulay Driss HASNAOUI, Morocco]	Rejected. It is important to spell out "in the climate of 2013" to indicate the transit nature of climate.
109361	11	17	11	17	You cannot define the climate of a year (2013 in this case) so suggest rephrasing. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted, but we do not agree. This is to reflect the transit nature of climate in the warming world.
11653	11	17	11	17	fix spelling of “recurrence” [Amy East, United States of America]	Accepted.
42431	11	17	11	17	Possible typo: the term 'recourrence' is not found in common dictionaries - do you mean 'recurrence'? [Joan Bech, Spain]	Accepted.
89289	11	17	11	17	‘recourrence interval’ to ‘recurrence interval’ [Tinghai Ou, Sweden]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43313	11	17			What does mean "recourrence "? [Cyriaque Rufin Nguimalet, Central African Republic]	Noted, typo is corrected.
201	11	18	11	37	As for the attribution of extreme weather events: By using the classification of weather events (patterns), Ohba and Sugimoto (2019) suggest the unique attribution method to understand the impact of global warming on extreme precipitation events. Please consider my proposal to add the following reference. Ohba, M., and S. Sugimoto 2019: Differences in climate change impacts between weather patterns: possible effects on spatial heterogeneous changes of future extreme rainfall, Climate Dynamics, 52, 4177–4191. doi:10.1007/s00382-018-4374-1 [Masamichi Ohba, Japan]	Noted, no action is taken. The Ohba and Sugimoto (2019) is about future project while event attribution assessed here is in a different context (recently observed events).
74501	11	19	11	20	when defining extremes in the sentence "... exceeds (or lies below) a threshold is not general because we consider as extremes also the cases of simply the highest/ or lowest values measured for a certain time over a period of time, e.g. maximum daily rainfall in mm each year, maximum instantanous discharge in m3/s each year, etc. [Moulay Driss HASNAOUI, Morocco]	Noted, no action is taken. It is true annual maximum daily precipitation amounts are considered as extreme values in many analyses. The corresponding events when these values are observed are often extremes as well as they often exceed the thresholds used to define extreme precipitation.
32931	11	19	11	20	Exceeding of the threshold can be in plus or in minus (depending on the type of event and related threshold) so is enough to comment that "variable exceeds the threshold" [Tomasz Walczykiewicz, Poland]	Noted, no action is taken. The text did consider "minus" by saying "or lies below".
68079	11	19	11	24	Definition of the base period is also critical, and needs also to be considered as a parameter in the definition of extremes. For example, the 1915 Colorado River streamflow at Lees Ferry was considered a baseline for dividing up freshwater resources amongst US western states, but in the context of the 20th century, or the prior 4 centuries, was an extreme wet year. Similarly, 1951-1980 was a typical baseline for global mean temperature until in the past several decades, it was replaced by 1971-2000, thereby affecting estimates of extremes. In addition, there is the probability distribution of events. How well is that known? Here there is a (limited) role for paleoclimatology, as in other chapters, and this could be introduced here. But it is possible to to do, for example, at the level of Fig 11.1. [MIchael Evans, United States of America]	Noted, no action is taken. An extreme event defined here is in a general term and high level. While we agree that base period is important, we consider that to be details.
100095	11	19			Change "as extreme" to "extreme" [Ronadh Cox, United States of America]	Accepted
74361	11	28	11	31	It is a drawback of using a threshold value in order to identify the occurrence of extreme events. An extreme at one location defined by a certain threshold might not be extreme in another location. [Yulizar Yulizar, Indonesia]	Noted, no action is taken. The threshold is often defined locally (or regionally).
112831	11	28	11	38	Note that compound events are here restricted to compound extremes in the climate system (e.g. heat and drought). Especially in the context of covid-19 (but also more generally) there is also increasing attention for compound events referring to impacts (e.g. the compound impact of covid-19, the economic impact of the covid-19 response measures, plus floods and locusts in East Africa). Might be something to spell out in more detail in 11.8, but maybe you could make a quick reference to the restriction to compound hazards here in the ES. [Maarten van Aalst, Netherlands]	Noted. This is comment is not relevant to 11.1 but is considered in section 10.8 where we make it clear the compound event in the context of Ch11 is meant to be within climate/physical systems.
100097	11	29			change "as exceedence" to "because exceedence" [Ronadh Cox, United States of America]	Accepted.
62747	11	31			After outlining both indices for extreme events, some clarification can be offered as to how these indices were examined in this report. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted, action is taken. Later in the section, we state that "Framing, including how extremes are defined … is considered when forming our assessments".
62749	11	33	11	53	In the introduction to the types of changes in extremes, frequency is used, in the subsequent lines rarity is used seemingly interchangably. Consistency of language should be clarified here. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. "Frequency" and "rarity" are not exactly interchangeable in the context of this paragraph.
52639	11	33	11	53	It is mentioned the thresholds but also it needs to be explained some the thresholds can change locations' normals. [Nazan AN, Turkey]	Rejected. While changes in extremes affect normals (i.e. 30-yr mean values). The threshold (defined in some way) in itself does not change climate and thus does not affect normals. Also climatology is not assessed in this chapter.
20715	11	33	11	53	While Chapter 10 authors insisted again and again on the necessity to co-produce regional messages including the context and the users (although they exonerated themselves from this necessity...), this seems still more relevant here, and first when discussing definitions. Which kind of extreme event matters for the citizens and for the public authorities? Even recognizing that answers may be difficult to formulate, these are stimulating and fruitful questions. Please comment. [philippe waldteufel, France]	Considered but this is not within the scope of Chapter 11.
68741	11	35	11	35	Replace 'are dependent' with 'depend'. [Bodeker Greg, New Zealand]	Accepted
13829	11	36	11	36	It's recommended to explain briefly that it refers to “return period", since it's difficult to understand the concepts return period and return levels. [Maria Amparo Martinez Arroyo, Mexico]	Noted. This is a common terminology in engineering applications
62751	11	37			"Return levels" is used but not defined. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. This is a common terminology in engineering applications
29615	11	43	11	43	Some studies have found that the magnitude of temperature extremes do not scale linearly with background warming, and changes in variance and skewness must also be accounted for (Ballester et al., 2009, Kharin et al., 2013, Seneviratne et al., 2014, Fischer and Schär, 2010, Kürbis et al., 2009). [Rachel Taylor, Australia]	Noted, no action is taken. It is true changes in local mean and variance can both affect extreme temperature locally. What is discussed here is changes in extreme temperature in relation to changes in global mean temperature.
84047	11	45	11	46	The "threshold approach" is definetely an improvement and very useful for decision makers. However, the description of the reasons behind choosen the "threshold approach" leaves room for improvement. We reccomend Sharpe 2019 (DOI: 10.5194/gc-2-95-2019) [Marco Tulio Cabral, Brazil]	Considered. Sharpe 2019 is cited and some words are added to reflect his view.
112717	11	46			The change in percentiles also depends on the level of internal variability with stronger changes in low variability / low range regions worth saying (the old paper that said that was Morak et al in my group but I am sure Slava said that too) [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Considered. But the space is limited to expand on this discussion.
11655	11	47	11	47	fix spelling of “exceedance” [Amy East, United States of America]	Accepted
42433	11	47	11	47	Typo exendance -> excedance [Joan Bech, Spain]	Accepted
6857	11	47	11	47	"the exendance" to be written as "the exceedance" [Constantinos Cartalis, Greece]	Accepted
43315	11	47			Is it "the exendance " or "the exceedance"? [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
77703	11	47			"exendance" replace by "exceedance" [Emer Griffin, Ireland]	Accepted
38405	11	49	11	49	Ther term intensity is used here, which needs to introduce in the beginning of this paragraph (Page 11, Lines 33-36). Changes in frequency and magnitudes are mention but not intensity. At the beginning of this paragraph, rarity is mentioned then sevirity may also needed to be mentioned. [Mansour Almazroui, Saudi Arabia]	Considered
112719	11	49			it might be worth also stating that magnitude has been less used due to its bias in many models (and example in Cowtan et al just out nature climate change on dustbowl heatwaves sorry self serving) - doesn’t need to be cited there are probably lots of examples. and now its 5 minutes to deadline and i cant send any more comments sorry!!! i will finish reading the chapter with great interest, [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Noted, no action is taken. We consider this to be too technical to have the space for such discussion.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68081	11	50	11	50	Similarly: at a more basic level, the definition of extrema in either frequency or magnitude depends on the base period. Again, there's a role for paleoclimatic reconstructions and modeling, for instance, from past warm periods, as described in Chapters 01, 02, 03. [MIchael Evans, United States of America]	Noted, no action is taken. In a more general sense, it is not the base period but the climate that determines the frequency and magnitude of extremes. The discussion presented here is meant to be a simple illustration of the concepts rather than in-depth technical discussion. In a changing climate, return periods (or frequency) is a useful concept condition on the underlying climate.
70939	11	50	11	51	So another aspect of extremes is whether the shape of the distribution changes in time; everything up to this point in the paragraph seems to be based just on a shift in the mean, which is rather simplistic. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Here we simply discuss impacts of change in magnitude or frequency. But we do not discuss what causes changes in magnitude or frequency, which can be attributed to a shift in mean, a change in shape of the distribution, or combination of both.
52649	11	50	11	52	Burls et al. (2019) indicated that there is no change in the number of midlatitude cyclone (cold fronts) affecting Cape Town, but the duration of rainfall associated with these events has decreased. The total rainfall remained the same and this is counteracted by an increase in rainfall intensity. The decrease in the number of rainfall days has to do with the change in wind direction. https://www.nature.com/articles/s41612-019-0084-6 [Mary-Jane Bopape, South Africa]	Noted, no action is taken. This is not relevant to the discussion here.
66337	11		11		Table 11.2 Low confidence in increase extreme precipitation projection for Central and south America, CH12 has mostly high confidence in increase. Medium confidence for drought increase in South Africa, Mediterranean, Brazil, Central America South Africa, Mediterranean, Brazil, Central America, CH12 has high confidence. [Erika Coppola, Italy]	Noted. These are resolved through cross-chapter coordination.
42299	11		11		Here a reference to CH12 could be added explaining that extremes are also "climatic impact drivers" and the the link to impacts, through various aspects, are developed in CH12. [robert vautard, France]	Accepted, a sentence is added
72099	12	1	12	46	No mention of aridity which I think is a longterm drought. Is this considered in this chapter? [Mouhamadou Sylla, Rwanda]	Noted. Aridity is not considered in this chapter but it is assessed in Chapter 8 from large scale perspective and in Chapter 12 as an climate impact driver.
52641	12	3	12	46	Here is also available for explaning briefly difference between short and long term extreme events [Nazan AN, Turkey]	Noted.
42297	12	4	12	4	After the CH11-12 collaborative process CH12 could remove "floods" ; [robert vautard, France]	Noted. No action is taken here
84893	12	4	12	4	tropical cyclones should be tropical and extratropical cyclones since both are considered in the text [Turner Jessica, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
5595	12	4			not only river floods, but also coastal floods [Benoit Laignel, France]	Noted. No action is taken. Coastal floods removed from this chapter.
80695	12	7	12	8	There was also a section in chapter 6 of the SROCC report on compound events (section 6.8). It would be good to include this reference in this sentence [Helene Jacot Des Combes, Marshall Islands]	Accepted
40845	12	7	12	8	SROCC Ch6 also assessed compound events. [TSU WGI, France]	Accepted
14623	12	8	12	10	Marine related extremes are also assessed in Ch 12 [Roshanka Ranasinghe, Netherlands]	Noted.
5597	12	9			The extreme sea level is assessed only in the chapter 9, not in this chapter, why ? [Benoit Laignel, France]	Noted. Discussion between the two chapters decides to place such assessment in one place in the WGI report.
68743	12	12	12	12	are of different spatial and temporal scales' compared to what? Do you mean that different classes of extreme phenomena in the atmosphere have different temporal and spatial scales? [Bodeker Greg, New Zealand]	Noted. This is now reworded.
71497	12	14	12	14	I do not think drought may affect a whole continent (even the millenium drought in Australia did not affect the intere region). I suggest replace "entire continent" by "vast regions" or "large regions". [Sergio Vicente-Serrano, Spain]	Accepted.
68745	12	15	12	16	I would suggest replacing 'affecting our capability in detecting and attributing, and in projecting changes in' with 'affecting our capability to detect, attribute and project changes in'. [Bodeker Greg, New Zealand]	Accepted
39477	12	26	12	27	Consider to add the words 'risk reduction' to 'adaptive and risk reduction capacity of a system' [Tamara van 't Wout, Qatar]	Considered. The text modified.
44415	12	26	12	27	What is meant by "extreme hazard"? Is it that extreme events become more severe? I suggest to delete the whole first sentence (also to avoid the word hazard that should in principle be climatic impact driver). The overall message from the paragraph will not suffer by removing the first sentence, and similar content and wording is used on page 110 lines 7-8, as well as "For this reason" in the next sentence which it is repeated in the last sentence. [Jana Sillmann, Norway]	Considered. Rephrased
62771	12	26	12	31	First time that the term "stressor" is mentioned over the text, however, no explicit mention was made above about what is considered as stressor?. Please, reformulate. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. The text modified.
52643	12	26	12	31	It should be mentioned with two or three sentences about the effects of combined events on adaptive capacity. [Nazan AN, Turkey]	Noted. We'd like to but space is also limited
79959	12	27	12	27	"more quickly." --> "quicker." as the word "more" was already used once before in this sentence. [Fei Luo, Netherlands]	Noted. The first instance "more" is removed.
105977	12	27	12	31	The description of "compound events" here could more clearly lay out the various types of "compound events" (e.g. multiple stressors driving a single event, multiple concurrent events driven by different stressors, successive independent events) as is done in the second paragraph of 11.8. [Sohum Pawar, United States of America]	Noted, no action is taken. While it would be useful to do so, it also takes more space.
41147	12	28	12	28	We have a definition for 'Compound weather/climate events' from SROCC (see the WGI SOD glossary) [TSU WGI, France]	Considered. SROCC is cited
68747	12	33	12	33	Projected future changes in what? [Bodeker Greg, New Zealand]	Considered, "in extremes" is added
29617	12	33	12	41	This paragraph could benefit from mentioning the potential for tipping points to unexpectedly alter the model projections. [Rachel Taylor, Australia]	Considered but there is not specific literature to cite
74503	12	34	12	34	to correct SR15 by SR1.5 [Moulay Driss HASNAOUI, Morocco]	accepted
13665	12	34	12	34	change SR15 by SR1.5 [Maria Amparo Martinez Arroyo, Mexico]	accepted
20233	12	41	12	41	facilitate [philippe waldteufel, France]	accepted
42435	12	41	12	41	Typos (2): fascilitate easier comparsion -> facilitate easier comparison [Joan Bech, Spain]	accepted
68749	12	51	12	51	It is not clear to me what you mean here. Clearly, on paleoclimate timescales, human emissions are not the main drivers of changes in the climate but other (external) forcings e.g. solar insolation are. I guess I am finding it hard to think of what would not be an external forcing in this context - unless you mean internal variability. Is that what you mean? [Bodeker Greg, New Zealand]	Noted, but it is not relevant anymore. Discussion about drivers is removed to save space.
35077	12	51	13	4	This paragraph as written seems to underestimate the role of natural variability. Quantitative estimates of 'climate change', for example based on changes in 30-year means or trends calculated within 30-year or 50-year samples, include unforced climate variability that can exceed the magnitude of forced changes to the climate. [David Gutzler, United States of America]	Noted, but it is not relevant anymore. Discussion about drivers is removed to save space.
100099	12	52			Change "the changes in extrenes" to "changes in extremes" [Ronadh Cox, United States of America]	Noted, but the sentence is removed now.
13667	12	53	12	53	change SR15 by SR1.5 [Maria Amparo Martinez Arroyo, Mexico]	Accepted

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68751	12	53	12	53	What do you mean by 'extremes are an integral part of the climate system'. Clearly you don't mean an integral part of the climate system in the same way that e.g. oceans are. 'extremes' are not a component of the climate system, they are simply a manifestation of a shifting distribution of (approximately) Gaussian distributed states. It just wasn't clear to men what this sentence was trying to communicate. [Bodeker Greg, New Zealand]	Noted, the sentence is removed.
62773	13	6	13	7	I would reformulate this sentence as: " At the global but also at the regional scale...because regional scales also include continental scales. Other case, you should explicitly mention oceanic and / or continental... [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
113505	13	6	13	9	Need to report the associated confidence in this statement [Diego Miralles, Belgium]	Noted, no action is taken. This is stated as a fact and as a result, confidence level is not assessed.
100101	13	6			"much or the changes" should be either 'many of the changes" OR "much of the change" [Ronadh Cox, United States of America]	Accepted.
100103	13	7			Change "the enhanced radiative forcing" to "enhanced radiative forcing" [Ronadh Cox, United States of America]	Accepted
79109	13	8	13	8	double check the term 'water-holding capacity', same term used again on P15, L26, L31. [Andong Shi, Sweden]	Noted, no action is taken. Not sure what is being commented on
100105	13	8			Change "its resultant increase" to "resultant increase" [Ronadh Cox, United States of America]	Accepted
100107	13	8			Add comma after "of the atmosphere" [Ronadh Cox, United States of America]	Accepted
78151	13	9	0	0	"statbility" replace by "stability" [Emer Griffin, Ireland]	Accepted
113507	13	9	13	9	Correct 'statbility' [Diego Miralles, Belgium]	Accepted
68753	13	9	13	9	Spelling - statbility [Bodeker Greg, New Zealand]	Accepted
77705	13	9			"statbility" replace by "stability" [Emer Griffin, Ireland]	Accepted
71135	13	11	13	17	The quasi-linear relationship between global mean warming and warming of regional extremes only holds for transient warming. After stabilisation we would expect to see changes in the spatial pattern of temperature extremes with a relative reduction in heat extremes over land for a given global warming level. Relevant papers showing this difference between transient and equilibrium warmer climate states include Rugenstein et al. (2019) and King et al. (2020). Figure 4 of King et al. (2020) shows relative difference in seasonal-scale temperature extremes for a given global warming level. For daily-scale extremes, research on this problem is lacking. Refs: King., A. D., T. P. Lane, B. J. Henley, and J. R. Brown, 2020b: Global and regional impacts differ between transient and equilibrium warmer worlds. Nat. Clim. Chang., 10, 42–47, doi:10.1038/s41558-019-0658-7. Rugenstein, M., and Coauthors, 2019: LongRunMIP: Motivation and Design for a Large Collection of Millennial-Length AOGCM Simulations. Bull. Am. Meteorol. Soc., 100, 2551–2570, doi:10.1175/BAMS-D-19-0068.1. [Andrew King, Australia]	Considered. We added "at least under transit warming" to the sentence.
6859	13	12	13	12	replace "statbility" to "stability" [Constantinos Cartalis, Greece]	Noted. But this line does not have this typo.
68755	13	14	13	14	I would suggest 'emissions scenarios' rather than 'emission scenarios' since each scenario includes emissions of more than one gas/substance. [Bodeker Greg, New Zealand]	Accepted
82751	13	15	13	16	Figure 11.1 suggests that TXx and T on land track very closely at global scale - is this worthy of comment? [Blair Trewin, Australia]	Noted, no action is taken. It would be nice to comment on this but then space is also very limited.
68757	13	16	13	16	I would suggest 'global mean temperature' rather than 'mean global temperature' since there is no such thing as the global temperature. [Bodeker Greg, New Zealand]	Accepted
13669	13	17	13	17	change (Section 11.1.6) by (Section 11.1.6). [Maria Amparo Martinez Arroyo, Mexico]	Accepted
20717	13	17	13	19	Throughout this SOD, linearity is discussed as a relation between two continuous physical quantities. In a few cases, a threshold effect is mentioned as a reason for non-linearity (example: disappearance of the snow cover). But here the threshold effect results, rather than from physical mechanisms, from the way an indicator is defined. Conceivably, if a heatwave was defined in terms of the total energy excess with respect to climatology, there would not be any non-linearity [philippe waldteufel, France]	Noted, no action is taken. Heatwave assessed in WGI report has not been defined as suggested.
43317	13	17			Read "in some regions (Section 11.1.6). The number of heatwave days " rather than "in some regions (Section 11.1.6) The number of heatwave days " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
100109	13	21	13	22	Change "in the observations" to "both in observations [Ronadh Cox, United States of America]	Accepted
104549	13	22	13	24	Zhou and Wang (2017) comprehensively revealed the scaling of drizzle to heavy rains with mean air temperature. Fig. 1e from Zhou et al. (2018) quantitatively shows regional 7-day precipitation in China scaled with global mean air temperature. To supplement these two lines of quantitative evidence in China, this sentence would be better revised as: 'Extreme short-duration precipitation in North America and China also scales with global mean temperature (Li et al., 2018a; Prein et al., 2016b; Zhou and Wang, 2017; Zhou et al., 2018).' Reference: Zhou, C., and K. Wang, 2017: Quantifying the sensitivity of precipitation to the long-term warming trend and interannual-decadal variation of surface air temperature over China. J. Clim., 30, 3687-3703. Zhou, C., Wang, K., and Qi, D. (2018). Attribution of the July 2016 Extreme Precipitation Event Over China's Wuhang. Bull. Am. Meteorol. Soc. 99, 107–111. doi:10.1175/BAMS-D-17-0090.1. [Chunlüe Zhou, United States of America]	Accepted. Sentences modified.
71499	13	26	13	26	The term land use is not adequate here. Better to use land cover or land coverage. The same land coverage may have different uses. E.g. a forest may be used to provide wood, recreational services, ecosystem services, etc. But what may affect climate is not the use but the type of coverage. I would replace this term throughout the chapter. [Sergio Vicente-Serrano, Spain]	Considered. It is rephrased.
125849	13	26	13	26	Why "land use/albedo"? What sets albedo apart from other biophysical characteristics associated with land cover? [Trigg Talley, United States of America]	Considered. It is rephrased.
113509	13	29	13	29	Correct 'moisterning' [Diego Miralles, Belgium]	Accepted
68759	13	29	13	29	Replace 'confidence of those' with 'confidence in those'. [Bodeker Greg, New Zealand]	Accepted
20235	13	34	13	48	Figure 11.1: one of the temperature curves cannot be seen [philippe waldteufel, France]	Accepted - the figure has been recreated. All lines should now be visible. (Is now Figure 11.2)
42437	13	38	13	38	Typo: fromCMIP5 -> from CMIP5 [Joan Bech, Spain]	Editorial
39479	13	51	13	54	Consider to add the word 'and' to 'changes in the frequency and/or severity' [Tamara van 't Wout, Qatar]	Accepted
39261	13	51	14	13	Extreme event attribution to anthropogenic climate change has been increasingly being done as can be seen in the annual editions of Explaining Extreme Events from Climate Perspective by the BAMS, and there have been many.. These attribution studies are not limited to just extreme rainfall and heat events, although some conclusions have been just these events were unlikely to happen without warming. It is, however, true that many high-impact events have not been subjected to attribution studies (lines 2 to 5, page 14) as in the case of the strongest typhoons that had occurred in the Pacific basin. [Lourdes Tibig, Philippines]	Noted but it is unclear what specific comment/suggestion is.
71299	13	54	14	2	Not true. For example, the hurricane caterina, sandy, Haiyan, etc. [Kenji Taniguchi, Japan]	Accepted. Hurricane is added as another example.
125851	13	55	14	5	The following reference examines the role of anthropogenic climate change for a large flood events, including an attribution of the economic flood losses: Villarini, G., W. Zhang, F. Quintero, W.F. Krajewski, and G.A. Vecchi, Attribution of the impacts of the 2008 flooding in Cedar Rapids (Iowa) to anthropogenic forcing, submitted to Nature Communication, 2019. [Trigg Talley, United States of America]	Noted, but we do not have access to this paper and we also have found this paper

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66715	14	2	14	5	It would be good to have a comment after this sentence to draw attention to the lack of studies in the developing world, and that this is where many climate impacts are expected to be most strongly felt, and most socially damaging. How about: "These limitations disproportionately affect developing countries, especially the least developed, where vulnerabilities are highest, and where seasonal cycles (L. Harrington, D. Frame, A. King and F. Otto, 2018, How Uneven Are Changes to Impact-Relevant Climate Hazards in a 1.5°C World and Beyond?, Geophysical Research Letters, Volume 45, Issue 13, Pages 6672-6680, https://doi.org/10.1088/1748-9326/aa95ae) enhance the already uneven pattern of damaging climate impacts in emergent temperature-related climate events (E. Hawkins D. Frame L. Harrington M. Joshi A. King M. Rojas R. Sutton, 2020, Observed emergence of the climate change signal: from the familiar to the unknown, Geophysical Research Letters, https://doi.org/10.1029/2019GL086259, D. J. Frame, M. M. Joshi, E. Hawkins, L. J. Harrington and M. de Roiste, 2017, Population-based emergence of unfamiliar climates, Nature Climate Change 7, 407–411, doi:10.1038/nclimate3297)." That also bridges across to the emergence literature, indicating that we expect this pattern of enhanced emergence in the LDCs to continue into the future (and you can see it in the past, as shown in Ed's paper). [Dave Frame, New Zealand]	Considered. We added to the effect of lack of studies in developing world in general without getting into impact (as that is not within the scope of WGI report).
20237	14	4	14	5	"Lack of scientific capacity"? This reference is not available at the time of SOD. In view of its title which highlights "higher income countries", Chapter 11 authors are invited to be careful. [philippe waldteufel, France]	Considered. This is reworded.
79961	14	4	14	5	what does it mean by "scientific capacity" ? . It's a litte bit confusing here as it's not as self-explanatory as previous "lack of observational data" and "lack of reliable climate models". [Fei Luo, Netherlands]	Considered. This is reworded.
20239	14	7	14	7	properties [philippe waldteufel, France]	Accepted
105431	14	9	14	9	I have some concerns about this type of figure, which in essence summarizes evidence across a cohort of studies of opportunity where the common features are a focus on immediacy and the use of a particular methodological approach. It is not evident that the available studies are either globally or regionally representative. Studies are typically motivated by the occurrence of impacts as opposed to the occurrence of events posing a hazard, and are constrained by the availability of data, personnel with an interest, and capacity to undertake the studies. Their publication is also, surely, affected by publication bias wherein studies demonstrating a positive link with anthropogenic forcing are, I would guess, more likely to be published, even though there are examples of the opposite occurring. In addition, one might be concerned about the assignment of confidence levels following the schematic shown in Fig 11.4. I think it would be ok to attempt to classify studies according to their attributes (hopefully this can be done reliably and hopefully there is a paper trail indicating the classification for each study), but I would avoid using the IPCC calibrated language in this context, which was not conceived for application to individual studies – it's a bit over the top, in my view, to suggest that there is consensus amongst the entire Chapter 11 author team on the assessment of each study! [Francis Zwiers, Canada]	Considered. The figure is removed.
105195	14	9	14	23	I think a much more powerful synthesis figure, integrating observational, detection and attribution, and event attribution might be possible. The suggestion would be provide summary information for individual continents describing observed warming (either the mean, or for an index such as TXx), observed precipitation changes (again either the mean, or an index like RX1day or RX5day), an estimate of attributed change for both (trend detection), and to the extent that this is possible, a summary of event attribution results, perhaps in a 2x2 table (columns for heat and water related extremes; rows indicating fraction or number of studies finding an impact of anthropogenic climate change, and fraction where a discernable influence is not found). This would bring together two lines of evidence (observational and models) at two distinct time scales (multi-decadal and individual, short term instances of events). Where long term observations are lacking, it would still be possible to complete essentially 2/3rds of the presentation by showing model simulated long-term changes rather than attributed long-term changes and summarizing event attribution results. Confidence in an overall assessment of human influence at these two time scales would, however, be lower confidence in those regions than elsewhere. I think such figure would be a very power tool for conveying a highly integrated set of messages about human influence on the climate, and would make the synthesis less reliant on one class of studies of opportunity. [Francis Zwiers, Canada]	Considered. There is now a fig in SPM based on 11.9 summary tables.
42439	14	11	14	11	Typo: CO2(Lemordant -> CO2 (Lemordant [please note that 2 is a subindex] [Joan Bech, Spain]	Rejected. Not sure what this is meant for. The page/line does not have this.
39263	14	13	14	23	I look forward to seeing Fig.11.2. [Lourdes Tibig, Philippines]	Noted. No action is taken
71501	14	15	14	20	Figure 11.2 can be confuse in relation to droughts. Here there are more regions in which it is stated an attribution signal than in the 11.6 section. A clear example is the Mediterranean. [Sergio Vicente-Serrano, Spain]	Noted. Fig 11.2 is now redrawn.
40355	14	15	14	21	Hurricanes are called typhoons in the western north Pacific, so could label them just 'Tropical cyclones' [TSU WGI, France]	Noted. Fig 11.2 is now redrawn.
14819	14	17	28	21	The box does not include much over the past ocean. The vertical convection in the North Atlantic and the Atlantic meridional overturning circulation (AMOC), may both be weakened or even stopped ('shut down') by pulses of freshwater into the surface ocean at high northern latitudes. Such fluctuations have huge impacts on the continents. [Marie-France Loutre, Switzerland]	Noted. But this does not seem to be relevant.
14821	14	17	28	21	abrupt changes can also cascade through coupled climate, ecological, and social systems. There is not much said about that. An example could be the impact of a volcanic eruption. Volcani eruption can induce large climate changes leading to famine and societal reorganization. [Marie-France Loutre, Switzerland]	Noted. But this does not seem to be relevant.
45575	14	28	15	15	This discussion should include more diverse references, such as Merrieifield et al. 2019 (https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2019GL083945) or Suarez-Gutierrez et al 2020 (https://link.springer.com/article/10.1007/s00382-020-05233-2) [Laura Suarez-Gutierrez, Germany]	Accepted. This sentence has changed since the SOD but we have added one of the references in the new text.
113545	14	28	17	21	In this box, perhaps at the beginning, it needs to be very clear that this distinction between dynamic and termodynamic is just for convenience, because in reality the examples you give involve typically both thermodynamic changes and circulation changes. Needless to say that thermodynamics is a form of dynamics. [Diego Miralles, Belgium]	Rejected. The text already makes it clear that changes are a combined influence of both factors and further clarification may confuse the reader.
109601	14	30	14	36	Despite the recognition of the role of ocean motions in changes in weather and climate extremes, very little attention is given to natural oscillations (AMO, PDO, NAO, ENSO etc.) on which there is a huge peer-reviewed literature. Too much emphasis is placed on greenhouse gas forcing while virtually ignoring the role of natural oscillations. [Reynold Stone, Trinidad and Tobago]	Rejected. This is out of the scope of the Box. The influence of low-frequency variability is briefly discussed in 11.3.1 and 11.4.1 for temperature and precipitation extremes respectively.
42301	14	30	14	36	Splitting thermodynamics with dynamics makes sense in the extratropics where thermodynamics feedback on dynamics is weak, but this is not the case in tropics. This may be worth mentioning [robert vautard, France]	Rejected: the literature demonstrates a clear thermodynamic and dynamic influence on tropical precipitation.
29623	14	32	14	35	This sentence is too long and the reader gets lost. I would suggest something along the lines of "While thermodynamic and dynamic processes are necessarily interconnected, considering them separately may allow disentangling of their roles in changes to climate extremes". [Rachel Taylor, Australia]	Accepted. The sentence has been shortened.
28931	14	33			suggest "may allow disentangling" --> "elucidates the" or "can disentangle the" [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
62737	14	38			"Temperature extremes" section: mainly focus on feedbacks due to the temperature change, but not focus on extremes [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Most of changes in temperature extremes are explained by changes in long-term mean changes so it makes sense to assess changes in local mean temperatures. In addition, the last paragraph of the "Temperature extremes" section discusses the influence of changes in the dynamics.
20719	14	39	15	20	All this is true. However, what is concluded on page 14 lines 40-42 applies generally: various causes (direct thermodynamic, feedback indirect feedback, circulation) induce a shift of the temperature distribution, with incidences on the extremes. Beyond this, reasons why the shape of the temperature distribution is modified are not convincingly explained. Bye the way on line 39 the warming concerns tropospheric air rather than air in general. [philippe waldteufel, France]	Taken into account. The reference to the temperature distribution has been removed.
28933	14	45			may be more precise to say "global temperature" or "average temperature" since more water vapour reduces sunlight received at the surface in the daytime and it is plausible this effect dominates over the greenhouse effect in the tropical noon for example (sorry, pedantic!) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been revised.
125853	14	47	14	48	It might be more clear to speak of positive or negative temperature tendencies (or contributions to trends) rather than simply increases or decreases. [Trigg Talley, United States of America]	Accepted. Text has been revised.
1429	14	52	15	1	It should be noted that most of the warming in the Arctic has taken place during the polar nights when the albedo effect is absent. Chapter 4 in the Adaptive Actions in a Changing Arctic (AACA - Barents region) by the Arctic Council's Arctic Monitoring and Assessment Programme (AMAP) also highlights the temperature feedback - because the temperatures are lower in the Acrtic region, the heat loss is also much smaller than in the warmer parts (e.g. Stefan-Poltzmann law). The advection of heat and changed cloudiness due to less may also play a role as well as a lower boundary layer (e.g. Davy and Esau, 2016; DOI: 10.1038/ncomms11690). [Rasmus Benestad, Norway]	Rejected. We clearly state that there are a number of feedback processes playing a role in the Arctic and that these depend on seasonality. In the text, we refer the reader to other sections of the report where this subject is discussed in more detail.
117065	14		14		Refer to the CCBox on polar amplification in box 11.1 [Valerie Masson-Delmotte, France]	Accepted. Text revised.
62861	15	1	15	1	Is it 3 times higher than the global mean extreme T or as written now the global mean T? This statement in the current version seems confusing and should be clarified. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. The sentence has been removed.
28935	15	3			consider splitting long sentence in 2 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence has been shortened.
71503	15	4	15	4	Note that precipitation decrease is not clearly observed in the Mediterranean. In addition, a decrease is only stated by some studies for winter precipitation, which cannot be relevant to support the stated mechanisms that drive the described thermodynamic processes. Main driver of the mechanisms would probably more related to the increase in the atmospheric evaporative demand (clearly observed in the region), and related feedbacks. This paper can be useful for the context of Western Mediterranean: Long-term precipitation in Southwestern Europe reveals no clear trend attributable to anthropogenic forcing" by Peña-Angulo et al. Environmental Research Letters 2020. [Sergio Vicente-Serrano, Spain]	Accepted. The statement on "precipitation decreases" has been removed.
52645	15	4	15	4	Here, it is mentioned that the precipitation decreases can results in a drying soil. I think, also the issue of agricultural impact especially drought depending on it should be included here in one or two sentences, "just emphasis" [Nazan AN, Turkey]	Rejected. A reference to the impact of agricultural practices is out of the scope of the Box.
113531	15	7	15	7	Please add after 'extremes': 'These feedbacks affect temperature and precipitation not just locally, but also in remote downwind regions due to changes in moisture and heat advection (Miralles et al., 2019; Schumacher et al., 2019)'. Miralles, D. G., Gentine, P., Seneviratne, S. I. and Teuling, A. J.: Land-atmospheric feedbacks during droughts and heatwaves: state of the science and current challenges, Ann. N.Y. Acad. Sci., 2019. Schumacher, D. L., Keune, J., Heerwaarden, C. C., de Arellano, J. V. X. G., Teuling, A. J. and Miralles, D. G.: Amplification of mega-heatwaves through heat torrents fuelled by upwind drought, Nature Geosci, 1–8, doi:10.1038/s41561-019-0431-6, 2019. [Diego Miralles, Belgium]	Rejected. This is out of the scope of this Box.
71505	15	10	15	12	The CO2 effects are subject to uncertainties and vegetation responses may be strongly affected by soil water availability (see below in 11.6). I think necessary to be consisten among sections. Recent review by Brodribb et al., Science 368, 261–266 (2020) covers this issue in depth. [Sergio Vicente-Serrano, Spain]	Accepted. Text has been revised.
125855	15	10	15	12	This seems to say that the physiological response to CO2 causes direct and indirect radiative forcing. If that is the intended meaning, then it needs further explanation. If not, then it needs re-writing. [Trigg Talley, United States of America]	Accepted. Text has been revised.
13671	15	11	15	11	change CO2(Lemordant by CO2 (Lemordant [Maria Amparo Martinez Arroyo, Mexico]	Not applicable. The sentence has been removed.
11657	15	11	15	11	within blue box, line 11: add space after “CO2” [Amy East, United States of America]	Not applicable. The sentence has been removed.
62735	15	14	15	16	the example followed doesn't give a robust support for the conclusion, so the conclusion should be add uncertainty words like"likely" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text has been revised and we have tried to clarify the meaning.
70941	15	14	15	20	More robust (for the NH) are the changes in temperature variability associated with regional (thermodynamic) patterns of temperature changes, acted on by atmospheric dynamics which, to first order, appears to be little changed (Tamarin-Brodsky et al. 2020 doi: 10.1038/s41561-020-0576-3) [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text has been revised according to this comment.
71507	15	16	15	18	Recent studies suggest few influence of Polar amplification on circulation of the North hemisphere: https://doi.org/10.1038/s41558-020-0694-3 https://www.researchgate.net/publication/339382480_Insignificant_effect_of_Arctic_amplification_on_the_amplitude_of_midlatitude_atmospheric_waves. I suggest to rewrite/qualify this statement. [Sergio Vicente-Serrano, Spain]	Accepted. We note that there is limited evidence for a clear link between polar amplification and mid-latitude circulation response with reference to Cross Chapter Box 10.1.
42957	15	16	15	20	Should make a reference to the X-Arctic box in Chapter 10. [Rein Haarsma, Netherlands]	Accepted. Reference has been added.
62733	15	19	15	20	the example show "...could explain some of the summer temperature increases over the last four decades, although there is only low confidence in these changes in atmospheric circulation patterns and their persistence characteristics", I think when sth could be explain there would be proof to support it, "low confidence" proof may lead to a "low confidence" result. "low confidence" should be added into the example as well [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. We provide confidence levels only at the end of the Box but in the main text we give an idea on the confidence we have "much less robust".
1431	15	22	15	35	Another factor affecting the rainfall statistics is the ratio of the global area of evaporation, A_e, to the area of precipitation, A_p. If all evaporated water over A_e is returned as precipitation over A_p, then the rate of change in the mean precipitation intensity scales with the changes in mean evaporation times A_e/A_p (Benestad 2018; DOI: 10.1088/1748-9326/aab375). Such a "funnel effect" will have implications for extreme rainfall. There is no true estimate of the global area of precipitation on a daily basis, but satellite observations such as the TRMM can be used to provide estimates of 'semi-global' A_p between 50S and 50N (77% of Earth's surface). The TRMM gives daily A_p from 1998 and suggests that there was a decrease in the rainfall area by 7% befteen 1998 and 2016 (Benestad 2018) which corresponds to a rate −17 × 106 km 2/^C if it depends on the global warming. [Rasmus Benestad, Norway]	Rejected: while relevant, this is quite a short satellite record and so is not considered central to the assessment in this case.
125857	15	24	15	24	Byrne and O'Gorman (2015) do consider thermodynamic and dynamic contributions but they only consider mean precipitation minus evaporation. Therefore it is not clear that this paper should be cited for the sentence, unless the sentence was meant to apply to mean precipitation also: "The thermodynamic vs. dynamic decomposition framework has been used to understand the observed and projected future changes in precipitation extremes." [Trigg Talley, United States of America]	Not applicable. This sentence has been largely modified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
125859	15	25	15	27	Do authors really mean to include evaporation as a link between temperature and water vapor that is separate from C-C? Isn't C-C pretty much obeyed, regardless of the residence time of vapor in the atmosphere? And isn't evaporation pretty much controlled by energy availability (which of course is not the same as temperature) and water availability? [Trigg Talley, United States of America]	Noted. A more detailed description is not deemed necessary here since the purpose is to state that water vapour is constrained by atmospheric temperature and evaporation provides the source of additional moisture.
28937	15	28			The 7% per oC applies to water vapour at low altitudes or to the column values so either state "column integrated water vapour at the global scale" or "with an increase at low altitudes of approximately 7%..." (sorry if I wasn't clear last time!) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been revised.
28939	15	29		35	These statements do not make total sense to me since temperature can rise more than the global mean but if relative humidity remains constant then water vapour will increase in line with C-C regionally. Also, ocean moisture supply can increase but just not enough to maintain relative humidity. I suggest changing to "Nonetheless, increases at regional scales may differ from this C-C rate if there is a change in meteorological regime (e.g. convective to suppressed) that alters relative humidity. Additionally, declining relative humidity over land is driven by limitations in moisture transport from the ocean which warm more slowly than land (Byrne and O'Gorman, 2018) and this is further amplified by decreases in evapotranspiration rates due to land-atmosphere feedbacks and CO2 effects on photosynthesis (Berg et al. 2016)." A link to 8.2 would be useful I think (or refer to Allan et al. (2020) NYAS http://doi.org/10.1111/nyas.14337) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been revised.
113533	15	32	15	34	No need to refer to the future here. Already now, if a regon is water limited, you may not expect humidity changes to scale at C-C rates with temperature; that is why RH is declining in those regions. So water limitation is enough to support <7%. [Diego Miralles, Belgium]	Not applicable. This sentence has been largely modified.
71509	15	33	15	34	This statement seems to contradict Ch8 P5 12-23: Based on theoretical understanding of the coupling between the global energy and water budgets, global mean evaporation (E) and precipitation (P) are virtually certain to increase as the climate system continues to warm. Expected increases in global mean E and P of around 2-3% per °C of global annual mean surface warming. I suggest unify between the two chapters. [Sergio Vicente-Serrano, Spain]	Rejected. The statement in Chapter 8 refers to global evaporation and the statement in the text refers to a region.
37599	15	34	15	34	any potential decrease in evapotranspiration would be due to a CO2 effect on leaf diffusive porostiy (a stomatal effect), rather than the effect on photosynthesis [Timothy Brodribb, Australia]	Not applicable. This sentence has been largely modified.
71511	15	34	15	35	I think this statement is confuse. In Byrne and O'Gorman (2018) it is not stated a decrease in the water supply. Under enhanced vpd given increased T it is expected that atmopsheric demand over oceans will increase and given unlimited water availability, the supply to the atmopshere would increase. The issue is related to the moisture transport and the necessary water vapor to maintain RH constant given increased T over continental areas. I suggest to rewrite this statement. [Sergio Vicente-Serrano, Spain]	Accepted. Text has been revised.
125861	15	34	15	35	There are not "decreases in the moisture supply from the ocean" under climate warming. However, it is true that the moisture supply from over ocean does not keep up with the warming over land (leading to decreases in land relative humidity) as found in Byrne and O'Gorman (2018). [Trigg Talley, United States of America]	Accepted. Text has been revised.
102521	15	37	15	38	This statement needs a reference [Philippe Tulkens, Belgium]	Accepted: a reference has been added.
20721	15	40	15	42	In the precipitation case, the "parallelism" of extremes seems to deserve a discussion [philippe waldteufel, France]	Rejected: this is confusing and so not deemed necessary to discuss.
71647	15	44	15	47	It should be noted that dynamic contributions related to vertical motion are more enhanced for rarer events, and more for sub-daily precipitation events (Mizuta and Endo, 2020, GRL). Mizuta, R., and H. Endo, 2020: Projected changes in extreme precipitation in a 60-km AGCM large ensemble and their dependence on return periods, Geophys. Res. Lett., in press. [Ryo Mizuta, Japan]	Taken into account. The reference has been added although we do not mention how changes differ according to the rarity of temporal scale of events (see section 11.4).
1433	15	47	15	47	The frequency (probability) of extreme rainfall events (e.g. exceeding 50 mm/day) can also be explained statistically as a product between the number of rainy days and the probability distribution for wet days. Put simply, if it rains twice as often, then we expect to se twice as many exxtreme events, and if it rains more every time it rains, we also expect to see more extreme events. Benestad et al. (2019; DOI: 10.1088/1748-9326/ab2bb2) examined the trend in the probability of daily rainfall above 50 mm/day for 1875 rain gauges with more than 50 years of data worldwide between 1961 and 2018, and found increasing trends in 76% of the cases and decreasing trends in the remaining 24%. Most of the increase in the probability (95%) of heavy rainfall could be explained by increasing trend in the wet-day mean precipitation whereas 72% of the cases could be attributed to more rainy days. For the sites with reduced probability of heavy precipitation, 76% also indicated a decrease in the wet-day mean precipitation. This statistical perspective complements the discussion about the dynamic contribution (the circulation pattern does have an influence on whether it rains or not). [Rasmus Benestad, Norway]	Rejected. This is out of the scope of the Box.
113535	15	51	15	52	it is medium [...] this is because' does not work, because it is unclear whether you will pose next an argument for it to be higher or lower than expected. Say 'this is not higher/lower because...'. [Diego Miralles, Belgium]	Not applicable. This sentence has been largely modified.
117067	15		15		why sch a focus on CMIP3-CMIP5 and not CMIP6 and RCM here? What about highresMIP? [Valerie Masson-Delmotte, France]	Accepted. We now refer to "Climate models" to also include CMIP6 and HighresMIP.
125863	16	7	16	18	It is worth noting here that there are widespread negative dynamical contributions to projected changes in precipitation extremes over land in June-July-August over both the Southern and Northern Hemispheres. This is shown in Figure S8 of the cited paper by Pfahl et al. (2017) and Figure 1 of Tandon et al. (2018). Citations: Pfahl et al, Nature Climate Change, 2017 DOI: 10.1038/NCLIMATE3287 Tandon N. F., Nie, J., & Zhang, X. (2018). Geophysical Research Letters, 45, 10,665-10,672 [Trigg Talley, United States of America]	Taken into account. We clearly state in the text the dynamic contribution can be negative: "with large regions in the subtropics showing robust reductions and other areas (e.g., equatorial Pacific) showing robust amplifications (Box 11.1, Figure 1c). "
68083	16	7	16	36	For the dynamic factors, these are clearly tied to an ENSO-like pattern; the extent to which this depends on changes in frequency, amplitude of events, or a change in the mean state should be stated and supported. It's not clear this has occurred over the past century (Ch 02, Ch 03). Also, confidence in the dynamic changes should be added with IPCC terminology, if possible. (l. 35-36). This is discussed on pg 17 and summarized on pg 18, l 3-6. [MIchael Evans, United States of America]	Taken into account. The influence of ENSO is out of the scope on the BOX. Confidence statements about dynamic contributions are now provided at the end of the Box.
13673	16	12	16	12	change 4 by 4% [Maria Amparo Martinez Arroyo, Mexico]	Not applicable. This sentence has been modified.
109811	16	20	16	20	note that these dynamical feedbacks are more localised but potentially larger for sub-daily precipitation extremes - see review paper by fowler - undergoing minor revisions - Fowler, H.J., Lenderink, G., Prein, P., Westra, S., Allan, R.P., Ban, N., Barbero, R., Berg, P., Blenkinsop, S., Do, H.X., Guerreiro, S., Haerter, J., Kendon, E., Lewis, E., Schaer, C., Sharma, A., Villarini, G., Wasko, C., Zhang, X. Intensification of short-duration rainfall extremes with global warming and implications for flood hazard. Submitted to Nature Reviews Earth and Environment. [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This is out of the scope of the Box.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71435	16	20	16	32	I am wondering whether the ordering of this paragraph could be improved. E.g. the release of latent heat and its influence on updrafts could be considered a thermodynamic-dynamic feedback, so it would count as an example for the last sentence in the paragraph. A reference for this effect is Lenderink et al., J Climate 2017, DOI: 10.1175/JCLI-D-16-0808.1. In fact, in its current version, there is no literature cited backing up the last sentence. Also I am wondering why there is a discussion of CC-scaling in specific humidity, but not here for precipitation. The Lenderink et al. reference above is providing a plausible explanation why heavy convective precipitation could follow a 2-CC relationship. Also the text is jumping back and forth between processes and their representation in models. [Douglas Maraun, Austria]	Considered. Note that the 2 times CC-scaling of Lenderink earlier work (2008 Nature GeoScience) does not actually estimate precipitation response to warming. This is now clearly discussed/assessed in Section 11.4. We have added references supporting how changes in latent heat affect vertical motions.
69539	16	23	16	25	I think there are two effects being confalted here. Latent heating may be thought of as amplifying large-scale vertical motion in the atmosphere through the QG-omega equation, as in the study of Nie et al. (2018). On the other hand, the language of latent heating is less helpful when thinking about convective-scale updrafts, where the key is the buoyancy obtainable by clouds, which is strongly sensitive to the environmental lapse rate. One meaure of this obtainable buoyancy is the CAPE, which has been shown to increase with warming in models and based on theory, although whether this contributes to enhancement of precipiation extremes is less clear. [Martin Singh, Australia]	Taken into account. We have tried to separate the discussion of both effects in the new text.
68763	16	28	16	29	Or, presumably, better parameterizations of these sub-grid-scale processes? [Bodeker Greg, New Zealand]	Accepted. Text has been revised.
113537	16	30	16	32	Positive dynamic feedbacks […] exceed those expected'. This is somehow logic. You could also say 'Negative dynamic feedbacks […] reduce those expected'. You just said that the intertwine between dynamic and thermodynamic is complex. I would say 'Dynmic feedbacks would therefore affect...'. [Diego Miralles, Belgium]	Not applicable. This sentence has been modified.
113539	16	34	16	36	You mean 'wuld drive' not 'will drive'. The latter implies it will happen, which you contradict in the second half of the sentence. [Diego Miralles, Belgium]	Not applicable. This sentence has been modified.
105197	16	50	16	50	The paragraph on drought in Box 11.1 does a better job, in my view, than the paragraphs on this topic in the executive summary, being both more informative and more succinct. [Francis Zwiers, Canada]	Noted.
125865	16	50	17	11	The dynamic/thermodynamic dichotomy here entirely leaves out biophysical processes that regulate plant water use, transpiration, and total ET. That might be the intent of the authors, but, as a result, the reader may lose sight of the fact that other processes are operative, particularly as they are not mentioned much elsewhere. [Trigg Talley, United States of America]	Rejected. We briefly mention the influence of vegetation in transpiration and the effect in temperatures. A full assessment of biophysical processes is out of the scope of the Box.
125867	16	50	17	11	It's hard to understand this section. It is hard to know in a given phrase or sentence whether the subject is precipitation or "evaporative demand." [Trigg Talley, United States of America]	Taken into account. We have substantially changed this section and we hope it has improved.
74505	16	51	16	51	word alsoresult to separe on also result [Moulay Driss HASNAOUI, Morocco]	Accepted. Text has been revised.
42303	16	51	16	51	also result [robert vautard, France]	Accepted. Text has been revised.
13675	16	51	16	51	change alsoresult by also result [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Text has been revised.
69241	16	51	16	51	The words "alsoresult" need a space between "also" and "result". [Kaoru Magosaki, Japan]	Accepted. Text has been revised.
11659	16	51	16	51	add space in “also result”. There are a lot of other instances of needing spaces between words in this chapter; in this review format it is not easy or practical for a reviewer to point out all of them. [Amy East, United States of America]	Accepted. Text has been revised.
52647	16	51	16	51	"Droughts alsoresult from…" should be "Droughts also result from " [Nazan AN, Turkey]	Accepted. Text has been revised.
29619	16	51	16	51	also result [Rachel Taylor, Australia]	Accepted. Text has been revised.
42441	16	51	16	51	Typo: alsoresult -> also result [Joan Bech, Spain]	Accepted. Text has been revised.
6861	16	51	16	51	replace "Droughts alsoresult" with "Droughts also result" [Constantinos Cartalis, Greece]	Accepted. Text has been revised.
43319	16	51			Read "Droughts also result from" rather than "Droughts alsoresult from" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. Text has been revised.
90847	16	51			"alsoresult" --> also result [Vivien How, Malaysia]	Accepted. Text has been revised.
10085	16	53			Meaning of "atmospheric drying" is unclear - should use a specific physical quantity e.g. "decreased relative humidity" or "vapor pressure deficit", especially since readers usually think of climate change as moistening the atmosphere by increasing the specific humidity. [Jacob Scheff, United States of America]	Accepted. Text has been revised.
66691	17	1	17	4	It's not usual to write confidence statements around what we don't know, or around the inadequacy of simplifications. Can this be rewritten into a more positive summary? Perhaps just delete the clause befor the comma, and say something like: "Many dry or wet regions display uncertain changes, and some humid regions currently display drying trends and/or are projected to become drier (Greve et al., 2014; Byrne and O’Gorman, 2015) (high confidence). These trends, which include uncertain trends and drying in humid regions, show that the simple statement that “dry-gets-drier, wet-gets-wetter” is too simple to capture observed general trends." [Dave Frame, New Zealand]	Accepted. The confidence language has been removed.
125869	17	1	17	7	It seems this is not just about the incompleteness of C-C to explain thermodynamic responses, but also about the fact that there are dynamic responses (e.g., via change in Hadley circulation) in addition to thermodynamic responses. [Trigg Talley, United States of America]	Not applicable. We have largely re-formulated this paragraph.
70943	17	1	17	11	There is also an important distinction to be made between precipitation, and P-E. The latter is controlled by moisture flux, and thus sensitive to circulation change, whereas the former can often reflect moisture recycling. I find that in IPCC, statements of confidence are often crafted on the basis of which is more convenient, rather than which is more important. The reader may not always appreciate the distinction, which can be substantial (e.g. for Mediterranean climates see Zappa et al. 2020 doi: 10.1073/pnas.1911015117) [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This discussion is out of the scope of the Box. Mechanisms and drivers of droughts are discussed in more detail in Section 11.5.
10087	17	1			The word "fully" here should be deleted, because its presence seems to imply that "dry-gets-drier, wet-gets-wetter" still works reasonably well on land, but not perfectly. Actually, it does not work well on land at all (according to the references cited here) so "cannot be encompassed" is quite justified. [Jacob Scheff, United States of America]	Accepted. Text has been revised.
10089	17	3	17	4	Not only "some humid regions currently display drying trends and/or are projected to become drier", but also vice versa (some dry regions are projected to become wetter)! E.g. Sahara, East Africa, north interior China. I believe this is shown in Greve et al. (2014) or other Greve paper. So should be rephrased to make clear it is wrong in both directions. [Jacob Scheff, United States of America]	Accepted. Text has been revised.
113541	17	4	17	5	This highlights that thermodynamic processes cannot be understood using the C-C relationshipalone' (note typo). I disagree, this could just show that drivers of drought can also be dynamic, as you just mentioned. Revise the entire \logic in this paragraph. Also be more explicit when refering to dynamic changes; it is barely mentioned, and changes like the Hadley cells widening are ver relevant for trends in drought. [Diego Miralles, Belgium]	Accepted. Text has been revised.
70363	17	4	17	7	It is not the evaporative demand that is modified, but the supply of moisture to meet that demand. Further, it is not just limited moisture supply but also plants stomatal control over that moisture flux as stomata close under higher CO2 that modifies the eventual resulting ET flux. Thus we recommend the following wording for the middle section of the sentence: “over continents, limited moisture supply and plant stomatal responses to increasing CO2 can strongly modify the evaporative flux together with internal climate variability”. If a reference is needed for the plant stomatal responses statement Milly and Dunne 2016 is appropriate. [Abigail Swann, United States of America]	Accepted. We have tried to clarify this in the new text.
79111	17	5	17	5	No space gap [Andong Shi, Sweden]	Accepted. Text has been revised.
74507	17	5	17	5	words relationshipalone to separate on relationship alone [Moulay Driss HASNAOUI, Morocco]	Accepted. Text has been revised.
29621	17	5	17	5	relationship alone [Rachel Taylor, Australia]	Accepted. Text has been revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42443	17	5	17	5	Typo: relationshipalone -> relationship alone [Joan Bech, Spain]	Accepted. Text has been revised.
43321	17	5			Read " using the C-C relationship alone " rather than " using the C-C relationshipalone " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. Text has been revised.
13677	17	7	17	7	change .In by . In [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Text has been revised.
66693	17	7	17	7	Insert space between fullstop and "In additional, regional..." [Dave Frame, New Zealand]	Accepted. Text has been revised.
10091	17	9			Same as p16 li53 comment - what physical quantity is meant exactly by "atmospheric drying" ? [Jacob Scheff, United States of America]	Accepted. Text has been revised.
125871	17	10			Add in this modified phrase after "(Douville and Plazzotta, 2017)": "... nor are regional precipitation trends over many land regions over the 20th century (Knutson and Zeng, 2018) with possible consequences for drought, flooding, and heavy precipitation projections." [Trigg Talley, United States of America]	Not applicable. We have largely re-formulated this paragraph.
113543	17	11	17	11	A rather general comment. This chapter should refer to chapter 8 more frequently. This is an example. Note te overlap between both is quite (too?) high. [Diego Miralles, Belgium]	Accepted. We have now consistently referred to Chapter 8 across the Box.
20723	17	13	17	21	While the critical issue here is the "parallelism" mentioned in previous comments, this summary circumvents the issue [philippe waldteufel, France]	Rejected. This is confusing and so not deemed necessary to discuss.
113547	17	14	17	14	related to' for 'affected by' [Diego Miralles, Belgium]	Not applicable. We have largely re-formulated this paragraph.
100111	17	15			Change "more easily attributable" to "more reliably attributable" OR "more robustly attributable" [Ronadh Cox, United States of America]	Not applicable. We have largely re-formulated this paragraph.
28941	17	17			Since uncertainties in snow and vegetation feedbacks are not explicitly discussed in the box I think the examples can be removed from the summary paragraph "(e.g., snow-albedo temperature feedbacks or soil moisture-evapotranspiration temperature/precipitation feedbacks)." [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. We have largely re-formulated this paragraph.
42305	17	26	17	26	This section could expand more on changes in weather regimes and their persistence (eg Jézéquel et al 2018, Schaller et al 2016) as these are of a smaller scale than the planetary modes and strongly affectong the weather; Jézéquel, A., J. Cattiaux, P. Naveau, S. Radanovics, A. Ribes, R. Vautard, M. Vrac, and P. Yiou, 2018, Trends of atmospheric circulation during singular hot days in Europe. Environ. Res. Lett., vol. 13, no 5, p. 054007 [robert vautard, France]	Considered. Space limit does not allow much expansion.
125873	17	26	17	26	[ACCESSIBILITY] From the titles, one would expect Section 11.1.5 to be part of 11.1.4. Is 11.1.4 intended to be just about thermodynamic mechanisms? And where is a section for biophysical processes? The dividing line among 11.1.4, 11.1.5, and Box 11.1 is unclear, and presence of redundancies across these items makes it a bit hard to follow the train of thought. [Trigg Talley, United States of America]	Considered. The box is slightly re-focused.
62775	17	26	28	6	I would move this subsection before than the previous one because you are talking about large-scale processes in a general way. After this subsection, we can talk about GHGs. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted, no action is taken. Our rational is that we discuss GHG first, and then larger-scale and regional-scale processes in 11.1.5 and 11.1.6.
71463	17	26			As discussed in the pre-LAM BOG on regional assessment, one could add a brief discussion of model performance here, e.g., in the context of projections (line 48). This could include a reference to Chapter 10, where we discuss the relevance of representing the large-scale circulation adequately for regional projections, e.g., with the example of blocking. [Douglas Maraun, Austria]	Considered. Model evaluations are included in subject sections.
71437	17	30			I would add duration or persistence of extreme events (heatwaves, drought). This is not necessarily the same as severity. In this context, one should also refer to blocking events causing cold waves, heat waves and drought. [Douglas Maraun, Austria]	Considered. "Duration" is mentioned in the revised sentence.
62753	17	30			First use of the term "severity", previously, magnitude was used in this case. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered, reworded.
113549	17	36	17	36	strength' [Diego Miralles, Belgium]	accepted
29625	17	36	17	36	strength [Rachel Taylor, Australia]	Accepted
42445	17	36	17	36	Typo: strengh -> strength [Joan Bech, Spain]	Accepted
84049	17	36	17	46	Kundzewicz et al. 2019 (DOI: 10.3390/w11071399) condicted a literature review about the relationship between floods and large-scale circulation, including: El Niño-Southern Oscillations (ENSO), North Atlantic Oscillations (NAO), Atlantic Multi-decadal Oscillation (AMO), and Pacific Decadal Oscillations (PDO). [Marco Tulio Cabral, Brazil]	Considered. The paper is cited.
113551	17	37	17	37	precipitation and winds' for 'precipitation, drought and winds' [Diego Miralles, Belgium]	Accepted
23935	17	37	17	39	The statement here seems to be textbook rather than something that requires a citation from as recent as 2018. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, text reworded.
29627	17	43	17	44	I would consider extreme winds, precipitation and temperatures to be features of weather systems, rather than independent factors affected by weather systems. [Rachel Taylor, Australia]	Accepted.
68765	17	48	17	54	I am surprised that nothing has been said here about externally forced changes in the Southern Annular Mode and its effect on the position of the southern midlatitude jet. Surely a lot could also be said about the northern midlatitude jet and the effects of changes in the stability of the jet on extremes e.g. along the US east coast? On teh whole, Section 11.1.5 feels far too short. Perhaps this material is covered in other chapters in which case more cross-referencing is needed. [Bodeker Greg, New Zealand]	Noted. Part of relevant assessments by earlier chapters is mentioned but space is also limited.
28943	17	48		54	Is there more that can be said about robust circulation shifts and extremes? For example slowing of tropical circulation, reduced tropical cyclone translation speed and extreme rainfall totals (8.2.2.2 and elsewhere). Also narrowing and intensification of the ITCZ? Some coordination with 8.4.2 may be useful. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Part of relevant assessments by earlier chapters is mentioned but space is also limited.
88177	17	49	17	50	Note Ch 2 concluded Hadley cell very likely expanded since the 1980s with medium confidence in extent of changes. [Sharon Smith, Canada]	Considered. Now cite Annex
71111	17	49	17	50	Chapter 3 assesses that human influence has contributed to the observed Hadley cell expansion, rather than confidence on the observed expansion itself. [Yu Kosaka, Japan]	Accepted.
23087	17	50	17	50	Given that the observational assessment was performed by chapter 2 this cite should be to chapter 2 instead of / as well as chapter 3. [Peter Thorne, Ireland]	Accepted
39265	17	52	17	54	Please define 'uncertain". In terms of the projections of ENSO events.The SROCC indicated that there will be more extreme ENSO events in the 21st century (See Chapter 6, SROCC). [Lourdes Tibig, Philippines]	Accepted
117069	17		17		The chapter could build more on chapter 10 and the annex on Modes [Valerie Masson-Delmotte, France]	Noted. We refer to the Box in Ch. 10. Mostly due to lack of space, we discuss little about low-frequency modes of variability in the Box.
23089	18	3	18	6	Why are you only concerned about aspects of strength here when also shifts in location arguably matter at least as much? [Peter Thorne, Ireland]	Considered, text modified.
113553	18	4	18	4	"...especially on the interannual time scale." This applies here but everywhere else in the report. I found some instances in which 'In summary,...' is followed by a statement that contains new information. That should be consistently checked and avoided. Here, in the paragraphs above the interannual scale is not mentioned. [Diego Miralles, Belgium]	Accepted
13831	18	9	18	9	In this section it is important to mention the effect of land use and land cover change on the water cycle through biogeophysical processes. [Maria Amparo Martinez Arroyo, Mexico]	Considered, but there needs to be a more limited scope due to space.
62777	18	10	19	32	Based on this title, I would include a subsection title above as: "Effects of global-scale processes, forcings and feedbacks on changes in extremes. Inside this new subsection I would include the current 11.1.4 and 11.1.5 subsections but as sub-subsections. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered, but no action is taken regarding flow/structure of this section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
113555	18	15	18	15	Please add to non-local effects: Schumacher, D. L., Keune, J., Heerwaarden, C. C., de Arellano, J. V. X. G., Teuling, A. J. and Miralles, D. G.: Amplification of mega-heatwaves through heat torrents fuelled by upwind drought, Nature Geosci, 1–8, doi:10.1038/s41561-019-0431-6, 2019. [Diego Miralles, Belgium]	Noted, but we are not sure what is the comment/suggestion here.
100493	18	15	18	16	Two additional relevant references in this context are: de Vrese, P., Hagemann, S., & Claussen, M. (2016). Asian irrigation, African rain: Remote impacts of irrigation. Geophysical Research Letters, 43(8), 3737-3745.; Schumacher, D. L., Keune, J., Van Heerwaarden, C. C., de Arellano, J. V. G., Teuling, A. J., & Miralles, D. G. (2019). Amplification of mega-heatwaves through heat torrents fuelled by upwind drought. Nature Geoscience, 12(9), 712-717. [Wim Thiery, Belgium]	Considered. Papers cited.
93785	18	22	18	24	Thiery et al (2020) have brought more evidence supporting the fact that irrigation has limited the increasing frequency of hot extremes in many regions, or even reverted it in the case of the Indo-Gangetic plains. Thiery et al. (2020). Warming of hot extremes alleviated by expanding irrigation, Nature Communications [Quentin Lejeune, Germany]	Considered. Paper cited.
100489	18	24	18	24	As some CMIP6 models now do represent irrigation (I believe at least CESM, NASA-GISS, and IPSL), this phrase can better be reworded to: '...a process not represented in most state-of-the-art Earth System Model...' [Wim Thiery, Belgium]	Noted. The passage is shortened.
74509	18	25	18	25	project to projects by adding s [Moulay Driss HASNAOUI, Morocco]	Noted but this is not applicable anymore.
69243	18	27	18	27	It is a little difficult to understand the words "cool hot days". Explanation is required. [Kaoru Magosaki, Japan]	Noted but this is not applicable anymore.
38407	18	27	18	28	In the statement "reduced surface evaporation, may also asymmetrically cool hot days more than median days, with effects of ca. 1°C (Davin et al., 2014)" what is effects of ca. 1°C? The reference Davin et al., 2014 does not say anything about ca. 1°C and median days. Needs clarification. [Mansour Almazroui, Saudi Arabia]	Noted, but this does not seem to be a complete comment.
62755	18	27	18	28	Darvin et al., 2014, states that the hottest summer days would be cooled by "about 2 C" or more", while over the mean summer temperature "1 °C or less". The presented lines seem to present a less accurate combination of the two statements [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted but this is not applicable anymore.
74511	18	28	18	28	ca. Is to check if appropriate to define [Moulay Driss HASNAOUI, Morocco]	Noted but this is not applicable anymore.
113557	18	28	18	29	the decrease in soil evaporation may also mitigate the onset of drought'. Clearify what type of drought. If you mean meteorological, the onset will be enhanced by decreased evaporation. The paper cited in reference to this statement did not focus on soil moisture feedbacks nor drought, and all I have found in it is '...enhanced soil moisture-induced evaporative cooling during the warmest days relative to the control simulation [...] also implies a reduction of summer drought conditions'. This does not mean evaporation goes down... Clarify this statment please. [Diego Miralles, Belgium]	Noted but this is not applicable anymore.
68767	18	29	18	29	Replace 'deforestation has been shown' with 'deforestation haves been shown'. [Bodeker Greg, New Zealand]	Noted but this is not applicable anymore.
29629	18	30	18	34	Deforestation also effects local temperatures and weather patterns due to the loss of surface friction leading to enhanced winds. [Rachel Taylor, Australia]	Noted but this is not applicable anymore.
109363	18	31	18	31	Suggest adding a reference to support the assertion at the end of the sentence. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted but this is not applicable anymore.
11411	18	33	18	35	”several models simulate a warming of daytime temperatures for regions with forest vs non-forest cover (Lejeune et al., 2017).” This is section doesn’t play well with the findings in SRCCL. I suggest that the section is made more complete by including more references. See SRCCL ch. 2: “There is high agreement that temperate deforestation leads to summer warming and winter cooling (Bright et al. 2017; Zhao and Jackson 2014; Gálos et al. 2011, 2013; Wickham et al. 2013; Ahlswede and Thomas 2017; Anderson-Teixeira et al. 2012; Anderson et al. 2011; Chen et al. 2012; Strandberg and Kjellström 2018).” and “Lejeune et al. (2018) found systematic warming of the hottest summer days following historical deforestation in the northern mid-latitudes, and this echoes Strandberg and Kjellström (2018) who argue that the August 2003 and July 2010 heatwaves could have been largely mitigated if Europe had been largely forested.” [Strandberg Gustav, Sweden]	Noted but this is not applicable anymore.
20725	18	45	18	47	This is not entirely exact. King et al write in their abstract that "Aerosol-induced cooling delays the timing of a significant human contribution to record-breaking events": they say that what was actually delayed was not the identification, but the human contribution itself becoming significant. [philippe waldteufel, France]	Noted.
39827	18	47	18	47	"some regions" - > Which regions? [TSU WGI, France]	Noted, but there is not space to expand on this.
28945	18	47			A further feedback mechanism identified by Allen et al. (2019) Nature Clim. https://doi.org/10.1038/s41558-019-0401-4 Is that continental drying from increasing land-ocean temperature contrast can increase aerosol pollution (thereby a small negative feedback) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Considered. No action is taken. This becomes too detailed for which we don’t have the space.
39267	19	1	19	19	This paragraph is on how regional feedback mechanisms (e.g., soil moisture feedbacks, feedbacks between soil moisture content and precipitation occurrence, snow and ice-albedo feedback , etc.) can also substantially affect extremes (high confidence).Does the uncertainty language applies to all of these feedbacks or just the finding that these can affect extremes (both presnt and projected)? the discussion that followed the first statement is ike a iterature review. Can a synthesis be done and uncertainty language be indicated? [Lourdes Tibig, Philippines]	Noted. This applies to the findings only. Other aspects are provided in summary.
24509	19	2	19	4	Sato and Nakamura (2019) pointed out soil moisture anomaly could shift westerly jet, leading to recent reginal increase in hot extremes over Eurasia. It is suggested to mention the effect of such internal variability related with land-atmosphere interaction, as well as large-scale warming, amplifies the hot extreme. Sato, T. and T. Nakamura, 2019: Intensification of hot Eurasian summers by climate change and land-atmosphere interactions. Scientific Reports, 9, 10866(2019), DOI: 10.1038/s41598-019-47291-5 [Tomonori Sato, Japan]	Considered, the paper is cited.
23091	19	8	19	9	negative spatial feedbacks is unclear. What do you mean here? [Peter Thorne, Ireland]	Considered. Text is reworded
113559	19	9	19	9	local' for 'temporal' I believe. [Diego Miralles, Belgium]	Considered. Text is reworded
29631	19	9	19	9	"spatial feedbacks" is very vague. Please define the scale to which you are referring [Rachel Taylor, Australia]	Considered. Text is reworded
71513	19	14	19	14	Maybe to comment that this may also have implications downstream: https://science.sciencemag.org/content/367/6483/1252/tab-article-info [Sergio Vicente-Serrano, Spain]	Noted. No change is made to limit the scope of discussion here.
70945	19	15	19	15	The effect of the freezing line on the skewness of temperature variability has been examined by Gao et al. (2015 doi: 10.1088/1748-9326/10/4/044001) [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We try to keep the discussion in general.
109813	19	24	19	24	could add reference to this study on heatwave/droughts from china recently published: Kong, Q., Guerreiro, S., Blenkinsop, S., Li, X.-F., Fowler, H.J. 2020: Increases in summertime concurrent drought and heatwave in eastern China. Weather and Climate Extremes, 28, 100242, DOI: 10.1016/j.wace.2019.100242. [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Considered. Paper cited.
23093	19	29	19	31	This feels a very odd sentence in that it conflates warming and cooling in a way that may be misinterpreted. Can a cleaner way of stating this, perhaps in two sentnces, be given? [Peter Thorne, Ireland]	Noted. The emphasis is cooling in some regions.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
100113	19	30			Should "higher warming of extreme temperatures" be "predictions of higher temperature extremes"? Or "higher predictions of extreme temperatures"? "warning" seems the wrong word here. [Ronadh Cox, United States of America]	Noted. But this is not about discussion.
23095	19	35			This global synthesis feels very odd coming here as it by and large precedes the substantive assessment that it summarises. There is a bit of a feeling of proverbially the cart being put before the horse in doing so and I wonder whether this would be better coming much closer to the end of the chapter than here so the trace is prior to rather than after the assessment. [Peter Thorne, Ireland]	Considered. The decision of having a synthesis here is to highlight take home message as early as the chapter is long.
68769	19	37	19	37	Replace 'provide a synthesis for' with 'provide a synthesis of'. [Bodeker Greg, New Zealand]	Noted.
105199	19	51	19	51	The idea that there might be entries in this figure above the x=y line, perhaps in the upper left-hand corner is intriguing, implying high confidence in process understanding despite low confidence in attribution results based on historical data. Such bold statements would clearly require review so that the authors can understand whether they are consistent with scientific consensus. This suggests that it might now be too late to include such a figure, particularly if bold statements are to be made, since there is not a further opportunity for expert review before the report is finalized and accepted. [Francis Zwiers, Canada]	Considered. The assessment does not contain the scenario of dots in the upper left corner of the figure.
117075	19		19		Missing links to chapters 7, 8, 9, 10 in the first paragraph. [Valerie Masson-Delmotte, France]	Considered. But we are not sure why citation to these chapters is necessarily here.
102523	20	0	20	0	Some cells not filled with regional info [Philippe Tulkens, Belgium]	Considered. Cells are now filled.
42447	20	0	20	0	Table 11.1 Search typos (blank missing): contributoron -> contributor on; generalbecause -> general because [Joan Bech, Spain]	Noted. Typos fixed.
105201	20	6	20	6	Given the degree of certainty in the assessment on temperature extremes, is it still useful to continue to call out the individual types, as in previous reports? Perhaps it is sufficient to summarize by condensing the first 4 rows into one. [Francis Zwiers, Canada]	Considered, these four rows are now summarized into one.
105203	20	6	20	6	I'm concerned that the high confidence assessment concerning flood seasonality may be over confident. [Francis Zwiers, Canada]	Noted. Not applicable anymore as flood is not included in the table.
105205	20	6	20	6	Decreases in flooding and "water logging" (whatever that might be – the concept is not defined in the chapter!) might also be of concern in places where proper ecosystem function and health depends on seasonal flooding or water logging, and where this might be an important factor for aquifer recharge. [Francis Zwiers, Canada]	Noted. Not applicable anymore as the entry is removed.
105207	20	6	20	6	The assessments concerning the human contribution to TC related precipitation seem to be stretching (almost tortuously) to make inferences based on indirect evidence. The statement that there are attributable increases in water vapor does perhaps have some relevance, although it doesn't imply that such a change equally affects all precipitation producing phenomena. The suggestion that detected changes extreme precipitation must have some implication for TC related precipitation is, in my view, an "extreme" stretch. This kind of interference would, presumably, then also extend to every other process responsible for extreme precipitation. [Francis Zwiers, Canada]	Considered. Details are provided in Section 11.7.
105209	20	6	20	6	Is medium confidence too strong an assessment of the possible role of aerosol forcing on the intensity and frequency of North Atlantic TCs? [Francis Zwiers, Canada]	Considered. The rational is provided in Section 11.7.
105211	20	6	20	6	The assessment of evidence of a poleward migration of TCs seems much better nuanced in this table than in the Executive Summary. [Francis Zwiers, Canada]	Noted. No action taken.
105213	20	6	20	6	I'm concerned that an assessment of wild fire intensity oversteps the appropriate scope for this chapter. An authoritative assessment of changes in intensity would have to include factors such as changes in forest structure, fuel load, forest management, wildland urban interface development, etc., as well as changes in meteorological and climatological fire risk factors. [Francis Zwiers, Canada]	Considered. The chapter only assess fire weather in the context of compound event. Detailed assessment is provided in Chapter 12.
39269	20	6	22	1	Table 11.1 is very informative, but it should be improved for an easier read (for instance, be consistent in the order findings are presented, whether global or regional), and please be consistent also when italicizing uncertinty language (in some, entries are all italicized). [Lourdes Tibig, Philippines]	Considered. The table is improved.
20727	20	6	25	2	Nowhere is these tables 11.1 and 11.2 is account taken of the areas concerned by extreme events such as floods, droughts, etc. [philippe waldteufel, France]	Noted. These are summarized in regional assessment tables.
125875	20	6			In Table 11.1, row 'Floods and water logging: Increases in intensity and/or frequency', column 2: Change "Northwest US" to "Northeast US" to align with the evidence presented in Section 11.5.2, the section's summary language (page 69, line 19), and the Executive Summary (page 7, line 47). [Trigg Talley, United States of America]	Noted. Not applicable anymore as the entry is removed.
113561	20	8	20	8	It would have been great to have a table (or figure) like this comparing AR5 and AR6. Even the different reports across time… [Diego Miralles, Belgium]	Noted. But the chapter is already very long.
113563	20	8	20	8	Not sure grouping all droughts in one cathegory is useful or even suitable here. [Diego Miralles, Belgium]	Considered. Four drought types are assessed.
113565	20	8	20	8	It would have been great to have a table (or figure) like this comparing AR5 and AR6 for similar scenarios. Even the different reports across time… [Diego Miralles, Belgium]	Noted. But the chapter is already very long.
14625	20	8	20	9	Marine related extremes are also assessed in Ch 12 [Roshanka Ranasinghe, Netherlands]	Noted.
108331	20	8	20	11	inconsistence confidence statement on observed trend with chapter 12 and Atlas [Nana Klutse, Ghana]	Noted. In FGD, chapter 12 takes assessment from this chapter and Atlas does not assess changes in extremes.
108333	20	8	20	11	generalbecause' should be 'general because' [Nana Klutse, Ghana]	Noted, typo is corrected.
68085	20	8	20	11	Table 11.2: I realize this table is a placeholder, but shouldn't the reference baseline be 1850-1900, as adopted in other chapters? And shouldn't it come after section 11.2? [MIchael Evans, United States of America]	Noted. We assume the comment related to Table 11.1 rather than 11.2. The main assessment of the chapter is based on information since 1950s as literatures analysing changes in these extreme since 1900 is more limited.
84897	20	8	21	1	I would like to see an entry for extratropical cyclones in Table 11.1 [Turner Jessica, United Kingdom (of Great Britain and Northern Ireland)]	Noted but assessment of extratropic cyclones is not within the scope of Chapter 11
62779	20	8	22	1	I would specify, when possible, which regions. It could greatly simplify the magnitude and extension of this table. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. Regional information is summarized in Section 11.9.
93787	20	8	22	1	It would be beneficial to be more explicit in Table 11.1 about why wildfires are considered as compound events. This classification is not straightforward and its justification in Section 11.8 is rather succint and occurs in any case late in the chapter. [Quentin Lejeune, Germany]	Considered. Only fire weather (rather than wildfire) assessed.
11113	20	8	22	1	Table 11.1, regarding Poleward migration of tropical cyclones. It seems that there is high confidence, rather than medium confidence, for the migration in the western North Pacific. Many studies support such a migration (see section 12.4.2.3 of Chapter 12). [Wen Wang, China]	Considered. This is recalibrated with available evidence.
125877	20	8	22	1	The meaning of the parenthetical in the headers of columns 2 and 3 of Table 11.1 is not clear. [Trigg Talley, United States of America]	Noted. This represents global mean temperature increase by 0.5C or more.
125879	20	8	22	1	[DROUGHT] In Table 11.1 ("Drought events"), to what types of drought and drying tendency do these various statements apply? Clearly not "meteorological" (i.e., precipitation). But is it surface soil moisture, root-zone soil moisture, runoff, ecological, agricultural? [Trigg Talley, United States of America]	Noted. Only agricultural drought is included in the table in FGD.
125881	20	8	22	1	What is the assessment for (regional) decrease in flood events? [Trigg Talley, United States of America]	Noted. Not applicable anymore as the entry is removed in FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
125883	20	8	22	1	Why is there high confidence in changes of flood seasonality, but low confidence in attribution? Or does the attribution statement refer only to the low-confidence part of column 2? Changes in seasonality in snow-dominated basins are strongly associated with changes in temperature, whose attribution is relatively strong. More specificity would help. [Trigg Talley, United States of America]	Noted. Not applicable anymore as the entry is removed in FGD.
125885	20	8	22	1	[DROUGHT] A high-confidence statement about compound drought/heatwave needs a specification of what kind of drought is being referred to. [Trigg Talley, United States of America]	Noted. Details provided in Section 11.8.
51587	20	8	25	2	I really like the use of these tables, which clearly summarise and present the key findings of this chapter. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted, no action taken.
100115	20	8			Change "Synthesis table on observed changes" to "Synthesis table of observed changes" [Ronadh Cox, United States of America]	Noted, text edited.
80697	20	11	20	11	In the table 11.1, there is a mention of Australasia. Does this region also include the Pacific SIDS? If not, where can the information about this countries be presented? [Helene Jacot Des Combes, Marshall Islands]	Noted. Assessment for small islands is in a Rejected. in Atlas.
13679	20	11	20	11	change contributoron by contributor on, and verylikely by very likely [Maria Amparo Martinez Arroyo, Mexico]	Noted. Typos fixed.
52651	20	11	20	11	second row of table: include space between countributoron, likelymain, generalbecause [Mary-Jane Bopape, South Africa]	Noted. Typos fixed.
9141	20	11	20	11	Fewer cold spells and cold waves are virtually certain on a global scale but there are no regional examples with very high likelihood. This seems strange. [Kevin Hennessy, Australia]	Noted. This is now consistent in FGD.
38365	20	11	20	11	The text “Observed/detected trends since 1950 (for +0.5°C global warming or higher)” in line 1, column 2 in Table 11.1, is not accessible. The author team is suggested to specify or modify it. [Yaming LIU, China]	Noted.
107389	20	11	22	1	This table 11.1 seems to be lacking a synthesis of extratropical cyclones / synoptic-scale mid-latitude storms [Markus Donat, Spain]	Noted. These are not in the scope of this chapter.
55169	20	11	25	1	This table contains *many* confidence and likelihood statements. According to the IPCC guidance document on uncertainty, such assessment statements are to be accompanied by a traceable account that provides the reader with the basis (evidence, agreement, etc) upon which the confidence level or likelihood value is based. In a large table like this, such traceability is not possible, and has not even been attempted. Since there are no citations to the primary literature provided in the Tables, it is not even possible for a reader to identify what information was used in the assessment and therefore no individual entry in the table can be independently verified. This is contrary to the spirit of IPCC assessment. [Nancy Hamzawi, Canada]	Noted. The table is a summary of underlying sections. Section/sub-section underlying these assessment highlighted in FGD.
62795	20		20		Specific comment for confidence assesment: Regarding "Cold spells/cold waves: decreases in frequency or intensity over most land areas", it is stated that in South America there are "Low evidence and medium agreement". Would be possible to replace the phrasing by a confidence statement as in the other assesments for Table 11.1? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. Text edited
117077	20		20		Please highlight what is new since AR5 (SR15/SROCC/SRCCL?) in the tables [Valerie Masson-Delmotte, France]	Considered. While it is desirable to highlight what is new, there is also not space to do so.
62863	20		20		remove "contributor" in first line for human contribution column to be consistent with following lines in this column and remove bold for regional statements [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. Wording is now consistent.
62867	20		22		Table 11.1: The different extreme types along the table are not presented in uniform wording, the degree of provided information and description as well as small differences in layout for global vs. regional. Consider homogenizing the table so the focus will be on differences in the available information on extreme events rather than editorial and presentation differences. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. Wording is unified now.
100117	20				Table 11.1, row 2, column 3: add space and comma: "generalbecause" should be "general, because" [Ronadh Cox, United States of America]	Considered. Text edited
96121	21	0	23		Different labels for identical context. Please adjust one of them: Table 11.1: "Drought events: Increases in frequency, intensity and/or duration" Table 11.2: "Increases in intensity and/or duration of drought events" [Nicole Wilke, Germany]	Noted. The labels are adjusted.
80699	21	1	21	1	The information on droughts is very general. Is there some more localised information to be added in the table? [Helene Jacot Des Combes, Marshall Islands]	Considered. Regional information is summarized in Section 11.9.
9143	21	1	21	1	Regarding drought, text on observed trends is consistent with the Executive Summary but not very policy-relevant. Consider using alternative text from the Executive Summary: "There is high confidence (medium confidence) that precipitation deficits have increased since the mid 20th century in west Africa, central Africa, and southern Africa (Northeastern Brazil). There is medium confidence that soil moisture deficits have increased in east Asia, central Europe, the Mediterranean region, and northwest North America. There is medium confidence that some regions show more frequent hydrological droughts (e.g., southern Africa, southern North America, the Mediterranean region)". I think southern Australia should be included in the statement about precipitation deficits. Regarding attribution statement for floods and waterlogging, change "seasonality" to "seasonal variability". Regarding compound events, replace "co-occurent" with "concurrent" and delete "under ehanced greenhouse forcing" [Kevin Hennessy, Australia]	Considered. Text edited
11661	21	1	21	70	second box on left side: again, “water logging” is a strange term. Do you mean soil saturation? [Amy East, United States of America]	Considered. This entry is removed from the table.
125887	21	1			[CONFIDENCE] ""High confidence in changes of flood seasonality, mostly in snow-dominated regions."" Clarify that this is just confidence in an increase but not confidence that the increase is detectable (unusual compared to expected natural variability). The table heading is ambiguous: ""Observed/detectable trends ..."" Which is it? These are two very different concepts. One can have high confidence that something has increased but no confidence in whether the increase is unusual compared to natural variability. This needs to be made much clearer here and elsewhere in the report where these concepts are mixed. Confidence is not high for streamflow timing detection over North America regions. Kam et al. (2018) found only marginal evidence for an emerging detectable anthropogenic influence on winter-spring streamflow timing over North America (according to four or five of nine models), and this was found only for the north-central United States and not other regions, which had even weaker indications of detectable anthropogenic influence. Citation: Kam, J., T.R. Knutson, and P.C. Milly, 2018: Climate Model Assessment of Changes in Winter-Spring Streamflow Timing over North America. J. Climate, 31, 5581, https://doi.org/10.1175/JCLI-D-17-0813.1 [Trigg Talley, United States of America]	Noted. This does not applicable anymore as the entry is removed.
27521	21	6	22	1	About row 7, column 2 of Table 11.1: confidence on those informations is not indicated here whereas confidence is indicated in the TABLE SPM.1. We suggest to complete this part of the table with confidence. [Eric Brun, France]	Noted. This does not applicable anymore as the entry is removed.
27523	21	6	22	1	About lignes 13, 14, 15 of Table 11.1: there are much more informations on "tropical cyclone track" in TABLE SPM.1 than in Table 11.1. This is a good thing because those informations are relevant for Policymakers but, as table 11.1 should me more precise compared to summery in policymakers document, this is quite surprising. We recommend to complete this part of the table. [Eric Brun, France]	Noted. The entry is simplified supported by underlying subsections.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71515	21	11	21	11	In table 3. droughts. I find confuse the message. Dry season is highly variable at the regional scale to assess a global average. I would rewrite this as: "Atmospheric evaporative demand displays a global drying tendency over continents, which has contributed to increase the severity of droughts". Also radiation should be net radiation. [Sergio Vicente-Serrano, Spain]	Noted. This entry is modified.
107687	21	15	21	15	I'm a little surprised here that it says floods have increased in the Northwest USA. Please see the maps in "Villarini, Slater (2017) Climatology of flooding in the United States, Oxford Research Encyclopedia of Natural Hazard Science, Oxford University Press." (Figure 1, Trends in flooding across the United States for water years 1965–2015) and also "Slater, Villarini (2016) Recent trends in U.S. flood risk, Geophys. Res.Lett. 43(12) 428-436, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016GL071199". Both these papers find that in much of the western USA, floods have actually decreased. Only in a very small part of the Northwest have floods increased. [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Considered. Flood entry is removed.
39781	21		21		"Low confidence due to little evidence and high seasonality." -> You mean limited evidence and high seasonality? [TSU WGI, France]	Considered. Text edited
13681	21		21		change 1970s.Low by 1970s. Low [Maria Amparo Martinez Arroyo, Mexico]	Noted. Type fixed.
62865	21		21		Line "Increase in tropical cyclone intensity (maximum surface wind speed)", If there is some information (medium confidence) on human contribution for North Atlantic, I assume this would apply as well for the left column "Observed/detected trends since 1950 (for +0.5°C global warming or higher)". Consider including the available informatio to left column. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. The table is now simplified supported by underlying subsections.
71439	21				First row: again, I suggest to make explicit that we don't know much about changes in the persistence of meteorological drought. This is implicit in the statement about precipitation, but a non-expert might not get the link. [Douglas Maraun, Austria]	Noted. But the persistency of drought is not explicitly assessed.
102525	22	0	22	0	Some cells not filled with confidence levels [Philippe Tulkens, Belgium]	Noted. All cells are complete with calibrated language in FGD.
102527	22	0	25	0	General comment to table: There are very little differences between columns (degrees) - could only the differences be listed somehow to avoid repetitions which actually blur the overview due to the extent [Philippe Tulkens, Belgium]	Considered. Different level of certainty/confidence is associated with different levels of warming in FGD Tables
82753	22	1	22	1	It would be more appropriate to speak here of wildfire risk (which is climatically influenced), asthe chapter text does, rather than wildfire occurrence - as the chapter text notes, wildfire occurrence at a global scale is decreasing as a result of non-climatic factors such as land-use change. This change would also remove an ambiguity in the attribution column, which is intended to reflect human influence on the climate but as currently worded would also include other human influences (e.g. number of human-caused ignitions). [Blair Trewin, Australia]	Considered. The assessment if for fire weather, this is now clear.
13683	22	1	22	1	No mention of wildfires in compound events section [Maria Amparo Martinez Arroyo, Mexico]	Considered. Section 11.8 assesses fire weather in the context of compound event.
125889	22	1			[CONFIDENCE] "Medium confidence for human contribution to poleward migration of tropical cyclones in the western North Pacific since 1950." This should be low confidence, since the signal modeled in the CMIP historical runs examined in the Kossin et al. study did not show a significant poleward trend over the historical period. [Trigg Talley, United States of America]	Considered. Section 11.7 provides rational for the assessment
105217	22	4	22	4	Decreases in flooding and “water logging” (whatever that might be – the concept is not defined in the chapter!) might also be of concern in places where proper ecosystem function and health depends on seasonal flooding or water logging, and where this might be an important factor for aquifer recharge. [Francis Zwiers, Canada]	Considered. This entry is removed from the table.
105219	22	4	22	4	Quantified projections of changes in TC related precipitation, TC lifetime and TC frequently seem very overconfident; they should be stated with a much lower level of precision. [Francis Zwiers, Canada]	Considered. Section 11.7 provides rational for the assessment
105215	22	4	22	4	Given the degree of certainty in the assessment on temperature extremes, is it still useful to continue to call out the individual types, as in previous reports? Perhaps it is sufficient to summarize by condensing the first 4 rows into one. [Francis Zwiers, Canada]	Considered. These take one entry now.
93789	22	4	25	2	A statement on wildfires has been included in Table 11.1. Would it be possible to do the same for Table 11.2? [Quentin Lejeune, Germany]	Considered. Fire weather is assessed in both tables 11.1 and 11.2.
39271	22	4	25	2	Please consider having the findings followed by the uncertainty language in table 11.2. This table will indeed be very helpful to policymakers in understanding the storylines and appreciating which of the extremes will be high-impact events. [Lourdes Tibig, Philippines]	Considered. Calibrated language is used in all assessments in the tables.
125891	22	4	25	2	[DROUGHT] What physical variable does "drying tendency" refer to, and how does this relate to "drought events?" This terminology seems too vague. [Trigg Talley, United States of America]	Considered. It is now specific.
125893	22	4	25	2	[DROUGHT] What kind of drought do these refer to? What physical variables? [Trigg Talley, United States of America]	Considered. It is now specific.
125895	22	4	25	2	[DROUGHT] In Compound Events, the meaning of "drought" is ambiguous. [Trigg Talley, United States of America]	Noted. "drought" is a general term here and difficult to specify.
113567	22	6	22	6	Not sure grouping all droughts in one cathegory is useful or even suitable here. [Diego Miralles, Belgium]	Considered. Four types drought are assessed but only agricultural drought is included in the tables 11.1 and 11.2.
113569	22	6	22	6	A general comment but that applies to this table as well: What is meant in the compound extremes sections by e.g. 'co-occurrent heatwaves and droughts will continue to increase'? That they will occur more dispropotionally in a concurrent manner, or just in terms of absolute occurrence? The latter is would be probabilistically quite obvious, since there will be many more heatwaves to potentially concur with droughts and non-drought conditions... This also applies to the previous table (i.e. is this conditional probability?) [Diego Miralles, Belgium]	Noted. This is meant for co-current events.
82165	22	6	22	6	Table 11.2: if the text is the same in the 3 columns, the columns could be merged instead of having 3-times the same content [Borbála Gálos, Hungary]	Considered. Text is simplified and reduced
14627	22	6	22	7	Marine related extremes are also assessed in Ch 12 [Roshanka Ranasinghe, Netherlands]	Noted. The text is specific for Ch9 assessment
112833	22	6	22	8	The caption should more explicitly articulate the meaning of the numbers in the table: in particular relative to which baseline the changes are specified (I assume pre-industrial). Note that this can be especially important given that these numbers include absolute warming of hot days, and percentage increase of TC rain rates and wind intensities (and the current prhrasing of "rate of increase" in the TC precipitation intensity is especially confusion -- see comment on that point) [Maarten van Aalst, Netherlands]	Considered in revising the tables.
84899	22	6	25	2	Same as above I would like to see an entry for extratropical cyclones in Table 11.2 [Turner Jessica, United Kingdom (of Great Britain and Northern Ireland)]	Considered. Extratropical cyclones are assessed in Chapter 8.
107391	22	9	25	1	This table 11.2 seems to be lacking a synthesis of extratropical cyclones / synoptic-scale mid-latitude storms [Markus Donat, Spain]	Considered. But extratropical cyclones are assessed in Chapter 8.
44389	22		22		Table 11.1 avoid using the term "risk" in this context. Replace by "probability or occurence" of the respective climatic impact driver. [Jana Sillmann, Norway]	Considered. "Risk" does not appear in the tables anymore.
42449	23	0	23	0	Table 11.2 Search typos (blank missing): likelycompared -> likely compared [Joan Bech, Spain]	Noted. Typos fixed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9145	23	1	23	1	Regarding heavy precipitation events, there is high confidence for an increase over Australasia. See section 7.2.2 of CSIRO and BoM (2015) Climate change in Australia Technical Report at www.climatechangeinaustralia.gov.au/publications which says "extreme rainfall events are projected to increase in intensity (high confidence)". See NZ MfE (2018) Climate Change Projections for New Zealand http://www.mfe.govt.nz/publications/climate-change/climate-change-projections-new-zealand] which projects "Increased intensity, particularly for shorter duration (sub-daily) rainfall, and more extreme (longer return period) rainfall". Regarding drought projections, the time in drought is projected to increase over southern Australia (high confidence) and increase over eastern and northern New Zealand (medium confidence) - see CSIRO and BoM (2015) and MfE (2018). [Kevin Hennessy, Australia]	Noted. This assessment considers all (and also more recent) literature. The assessment may or may not differ from national assessment.
10071	23	1	23	2	Why is heavy precipitation "high confidence" for 1.5 C but "likely" for 2.0 C? Why the shift of scales? [Robert Kopp, United States of America]	Noted. High confidence is implied for likely which additionally indicates the possibility of quantifying probability.
42451	24	0	24	0	Table 11.2 Search typos (blank missing):alarger -> a larger [Joan Bech, Spain]	Noted. Typos fixed.
11663	24	1	24	70	again, “saturation” preferable to “water logging” [Amy East, United States of America]	Noted. This does not apply anymore as "water logging" is removed from the table.
125897	24	1			[CONFIDENCE] High confidence in a projected increase of TC rain rates at the global scale? At least for TCs, this should be medium-to-high confidence (as in the WMO TC/climate assessment, Knutson et al. 2019b). If this is not available due to IPCC rules, then medium confidence. This projection has high level of agreement among existing modeling studies (although not as many studies have examined this as TC frequency change), the mechanistic understanding is strong, as is support for anthropogenic increases in total precipitable water, a key ingredient. What remains missing is a clear detection of an observed increase (i.e., that an observed increase is highly unusual compared to expected changes realizable from natural variability only). This is an essential part of a case for high confidence in a projection (don't just rely on models and theory for confidence, but actually see the change unambiguously in the data, and it's clearly distinguishable from natural variability). Recent detection/attribution studies for the Harvey event by Risser and Wehner and van Oldenbourgh et al., while of high quality, analyze observed long-term changes in extreme precipitation in general, not tropical cyclone precipitation. Additionally, the Harvey event was mainly due to the multi-day stall-out of the hurricane in the region. [Trigg Talley, United States of America]	Considered. The guidance about the use of calibrated language does not require detection of changes in the observation in order for projection to have high confidence.
107689	24	28	24	28	"lead to alarger" should be "lead to a larger" [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Typos fixed.
39713	24		24		"… the median projected rate of increase is about 11%." -> 11% isn't a rate [TSU WGI, France]	Noted, but percentage increase is a rate.
112835	24		24		Note that there is a confusing apparently dual use of the word "rate" in the boxes on Increase in precipitation associated with tropical cyclones. There is a reference to an increase in rain rates, but then also a reference to a rate of increase (which suggests that you are describing a change in the pace of change -- I presume that is not is what is intended). I would really restrict the use of the word "rate" to the context of "rain rate", or avoid it altogether and just speak about precipitation intensity or rain intensity. [Maarten van Aalst, Netherlands]	Noted. The meaning of "rain rate" and "rate of increase" are clear.
42453	25	0	25	0	Table 11.2 Typo : 10%.. -> 10%. [Joan Bech, Spain]	Noted. Typos fixed.
11665	25	1	25	1	top row, second box: delete the extra “.” [Amy East, United States of America]	Noted. Deleted.
9147	25	1	25	1	Regarding projected changes in TC frequency, the insurance and disaster risk management sectors need a statement about the total number of TCs and the total number of Cat 4-5, not the proportion of Cat 4-5. Regarding severe convective storms, the 3 studies cited on page 108 lines 2-4 only apply to the USA: 1 paper says there's an increase in spring and autumn, the 2nd paper says spring, and the 3rd paper says spring and summer. Therefore, I suggest revising the statement to "the frequency of severe convective storms increases in spring (medium confidence), summer and autumn (low confidence) in the USA". Regarding compound events, replace "co-occurent" with "concurrent" and consider replacing "heatwaves and droughts" with "heatwaves, droughts and fires" [Kevin Hennessy, Australia]	Noted. This does not apply anymore as convective storm is removed from the table.
62643	25	1	65	60	CMIP5, CMIP6, GCM –terms are defined multiple times. These need to be defined at the first use only to avoid confusion. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted, this is corrected.
20729	25	13	25	13	It is hard to believe that no mention at all is made of meteorological radars in this chapter! Please explain. [philippe waldteufel, France]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed. Chapter 2 (Section 2.3) and Chapter 10 (Section 10.2) assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
1435	25	13	28	39	The discussion about observations overlaps with several other chapters. These could be merged to save space and make the overall report structure easier for the readers. [Rasmus Benestad, Norway]	Agree with reviewer. This the section has been substantially modified from SOD to FGD
108983	25	13			There are similarities in the observation sections in Ch11, and Ch10 and the Atlas. These obervations sections in the three chapters can be better connected by clarifying what aspects of observations is being covered by each chapter. [Gemma Teresa Narisma, Philippines]	Agree with reviewer. This the section has been substantially modified from SOD to FGD
105221	25	15	25	16	I’m wondering whether the chapter has taken a broad enough view of what is considered to be an “extreme” or an “observation”. While there is some consideration of longer timescale phenomena, the primary focus is on short term variability. Nevertheless, every statistic that is derived from a sample of observations has a distribution that is inherited from the sampling process and the inherent chaotic variability that is reflected in the individual daily or sub-daily observations used to calculate that statistic. These sampling distributions also have tails, and the occurrence of a value in the tail (e.g., of a regional seasonal mean temperature or a season moisture deficit) is often associated with strong impacts. For example, the seminal 2004 paper of Stott et al reporting an event attribution study of the 2003 European heat wave did not consider the heat wave per se, but rather the extremely warm summer during which the heat wave occurred (i.e., the event was defined in terms of a seasonal mean, not in terms of the extremes of daily observations). [Francis Zwiers, Canada]	Agree with reviewer. This section has been substantially modified from SOD to FGD and the definition of extremes has been clarified with examples
84051	25	35	25	36	We recommend Sutton 2019 (DOI: 10.1175/BAMS-D-18-0280.1) as means to highlight the importante of considering the Risk Assessments approach. [Marco Tulio Cabral, Brazil]	comment does not appear to apply to text at this location
66331	25		25		Section 11.2.1 overlaps with Atlas section on observations [Erika Coppola, Italy]	Noted. This section is shortened.
105223	26	1	26	1	I don’t think I would characterize reanalyses as “observations” – these are model products that are observationally constrained. Some aspects are often considered to be essentially equivalent to observations, such as the large-scale circulation, but most aspects that are of interest to this chapter, including key surface variables, reflect a substantial influence of the atmospheric and land surface models that are used in the frozen analysis systems, with the model being the primary influence for some variables, including precipitation in most cases (the NARR is a possible exception, as noted later in Section 11.2). The classification of reanalysis variables by strength of observational constraint that is provided in the 1996 Kalnay et al paper in BAMS (doi: 10.1175/1520-0477(1996)077<0437:TNYRP>2.0.CO;2) describing the original NCEP 40-year reanalysis remains largely valid. [Francis Zwiers, Canada]	Taken into account. This section has been substantially modified from SOD to FGD and the relevant sentence was removed
71441	26	1	26	3	Chapter 10 discusses observations for regional information, not in general. So “overall climate monitoring” is misleading here. [Douglas Maraun, Austria]	Taken into account. This section has been substantially modified from SOD to FGD and the relevant sentence was removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
107393	26	9	25	11	Wording is a bit confusing here, mixing up what requires daily and what requires sub-daily data. In particular heatwaves/cold spells last for several days, so do not necessarily require sub-daily data for analysis [Markus Donat, Spain]	Taken into account. Text revised and moved to a new section "11.2.2. Data"
102529	26	9	26	9	Somewhat agree but evaporation processes, relevant to the water balance, operate at the second scale. This should be added since otherwise the sentence is erroneous [Philippe Tulkens, Belgium]	Noted. Unclear what revision is requested here
23097	26	10	26	13	As noted to chapter 10 such a view does not fairly reflect the substantive efforts that have been made since AR5 leading to more open data policies being enacted or the initiation of efforts to improve curation and access to sub-daily and daily surface records. See https://journals.ametsoc.org/doi/10.1175/BAMS-D-16-0165.1 [Peter Thorne, Ireland]	Taken into account. This section has been substantially modified from SOD to FGD and the relevant sentence was removed
105225	26	12	26	12	Replace “are too short (less than 10 years) in many regions” with “are often short (less than 10 years)”. This is not a problem that is isolated to individual regions; it’s ubiquitous. Long, high quality, sub-daily records tend to be the exception rather than the rule. [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed
107395	26	12	26	12	Also here it is not clear if the statement about too short (<10 years) records refers to daily or sub-daily data [Markus Donat, Spain]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed
68771	26	13	26	13	Replace 'On the other side of' with 'At the other end of'. [Bodeker Greg, New Zealand]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed
105227	26	13	26	16	The chapter seems to consider all drought events as being “extreme”, but is that really an appropriate view? It might not be from a climatological perspective, even if drought events have impacts. The impacts that draw attention to “drought” might simply be the result of agricultural or other development in a given area has tried to push the boundary of reasonable and acceptable risks in that area – in which case the occurrence of the impact could be more indicative of maladaptation a truly extreme event. In short, how do we objectively identify drought events that are truly rare, and thus “extreme”, from a climate perspective? [Francis Zwiers, Canada]	Drought is assessed in different chapters of AR6 WGI report from different perspectives. But the decision of the WGI report has ben that the main assessment of drought resides in Chapter 11. As a result, the assessment of drought in Chapter is not just focused on extreme drought, but all droughts. This is now made clear in the introduction section of this chapter when we explain the scope of this chapter in section 11.1
105229	26	13	26	16	This passage evokes two reactions. Secondly, one has the impression that the chapter has a relatively narrow view of what constitutes an extreme. [Francis Zwiers, Canada]	Taken into account. This section has been substantially modified from SOD to FGD. A new section "11.2.1 Definition of extremes" has been added
68773	26	17	26	17	Replace 'very unique' with 'unique'. Something is either unique or it is not i.e. unique is binary. One cannot be 'very unique' any more than one can be 'very pregnant'. [Bodeker Greg, New Zealand]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed
74363	26	19	26	20	need to provide reference(s) for this statement [Yulizar Yulizar, Indonesia]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed
6781	26	19	26	20	This statement should be deleted if it cannot be backed up by a reference and also be explained how the statement can be reconciled with the the conclusions reached by GCOS. In its 2015 Status Report on the Global Observing System (GCOS Publication no. 195 available from WMO), GCOS documented an increase in synoptic observations between 2002 and 2014. The increase came from both an increase in the number of stations reporting and an increase in the frequency of reporting. Data numbers have increased further since 2014, as can be seen for example in time series showing the number of surface pressure and 2m relative humidity observations assimilated in the ERA5 reanalysis, presented by Hersbach et al. (2020). [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This section has been substantially modified from SOD to FGD and the relevant sentence was removed
23099	26	21	26	26	This is very confused text. The ISTI databank is a monthly resolution product not daily and the reference should be to Rennie et al., 2014 https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/gdj3.8. GHCND remains the daily product and should be cited. It has seen substantive improvements. [Peter Thorne, Ireland]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed
8703	26	24	26	24	I think the Karl 2015 reference isn't quite correct. For ISTI, Rennie et al, 2014, https://doi.org/10.1002/gdj3.8 is more appropriate, though I am aware that the Karl reference is useful in relation to the "hiatus" [Robert Dunn, United Kingdom (of Great Britain and Northern Ireland)]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed
6783	26	26	26	27	This sentence needs qualification. Willett et al. (2014) give data counts for the observations used in construction of a dataset for climate monitoring that employs strict quality control and requires that data be used only for stations for which there are sufficient data to define a climatological value for that station. This is because the dataset construction uses anomalies to perform homogenisation. Such requirements are not necesssarrily needed for other uses of humidity observations, for example in reanalysis, as there the processing works with the difference between the observation and a background forecast rather than the difference between the observation and its climatological equivalent. There are many more humidity observations available than were used by Willett et al.. Again, one can refer to the GCOS Status Report, which (in addition to discussing the use of data in Willett et al.'s study) notes that 80% more synoptic humidity data were received at ECMWF in October 2014 than in October 2002, and notes also that the percentage of synoptic observations of dry bulb temperature that were accompanied by an observation of dew point temperature rose slightly from 97 to 98%. In other words there are almost as many synoptic observations of humidty as of temperature supplied each day by national meteorological services. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed
109815	26	26	26	27	should introduce new sub-daily global precipitation dataset somewhere in this section: see Lewis, E., Fowler, H.J., Alexander, L., Dunn, R., McClean, F., Barbero, R., Guerreiro, S., Li, X-.F., Blenkinsop, S. 2019. GSDR: A global sub-daily rainfall dataset. Journal of Climate, 32(15), 4715-4729, DOI: 10.1175/JCLI-D-18-0143.1. [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed. Chapter 2 (Section 2.3) and Chapter 10 (Section 10.2) assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
23101	26	26	26	28	As noted in an earlier comment substantive efforts are ongoing to improve access to sub-daily data holdings. See the BAMS paper linked in an earlier comment. [Peter Thorne, Ireland]	Taken into account. This section has been substantially modified from SOD to FGD and the relevant sentence was removed
71517	26	27	26	27	In the frame of the Ch. 11, the humidity is more relevant to determine the atmospheric evaporative demand and atmospheric drying. [Sergio Vicente-Serrano, Spain]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed. Chapter 2 (Section 2.3) and Chapter 10 (Section 10.2) assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
105231	26	30	26	30	The chapter and its tables refer to “water logging” in various places, but the notion is never defined! What is meant by water logging? [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
11667	26	30	26	30	again, “saturation” preferable to “water logging” [Amy East, United States of America]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
32933	26	31	26	31	add:"measurement errors due to sensor malfunction" [Tomasz Walczykiewicz, Poland]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23103	26	36	26	46	This misses the far more important aspect that this approach can add spurious trends if the station mix contributing changes substantively over time as was shown for DTR in https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2015JD024584 where the difference in ordering of operations was highlighted as fundamentally altering apparent multi-decadal variations. [Peter Thorne, Ireland]	Taken into account, this paragraph has been substantially modified from SOD to FGD version
33245	26	36	26	50	It could be added that not only the station density affect the interpretation of gridded products but also the complexity of the orography (Lundquist et al. 2019; Lundquist, J., M. Hughes, E. Gutmann, and S. Kapnick, 2019: Our Skill in Modeling Mountain Rain and Snow is Bypassing the Skill of Our Observational Networks. Bull. Amer. Meteor. Soc., 100, 2473–2490, https://doi.org/10.1175/BAMS-D-19-0001.1). [Gonzalez Sergi, Spain]	Taken into account, this paragraph has been substantially modified from SOD to FGD version
62869	26	41	26	47	Consider to provide examples of regions with high and low station densities. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account, this paragraph has been substantially modified from SOD to FGD version. Examples are provided in the text.
72195	26	44	26	50	the issue of influence of gridding procedures on extremes should be commented (Haylock et al, 2008,J. Geophys. Res. 113, D20119; Hofstraet al., 2008, J. Geophys. Res. 114,D21101, doi:10.1029/2009JD011799.; Wibig et al, 2014, Met Zeit, 23:181-187) [Joanna Wibig, Poland]	Taken into account, this paragraph has been substantially modified from SOD to FGD version
109817	26	44	26	50	nice paper on this is: Alexander, L., Fowler, H.J., Bador, M., Behrangi, A., Donat, M., Dunn, R., Funk, C., Goldie, J., Moon, H., Seneviratne, S.I., Venugopal, V. 2019: On the use of indices to study extreme precipitation on sub-daily and daily timescales. Environmental Research Letters, 14, 125008, DOI: 10.1088/1748-9326/ab51b6. [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. But we added other relevant literature as well.
71443	26	47	26	49	This statement is not backed up by literature. Herrera et al. Int. j. Climatol 2019 (DOI: 10.1002/joc.5878) discusses this issue and shows that the lower the station density, the more the area average 50-year return level of daily precipitation is overestimated. [Douglas Maraun, Austria]	Taken into account. This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
107397	26	48	26	48	I think the statement about the suitability for model evaluation needs to be more specific/nuanced. These described effects have been shown to primarily affect the MAGNITUDE of the gridded extremes (which is very uncertain anyway in gridded products). However, the (e.g. inter-annual) VARIABILITY of the time series should not be affected by these order of operations effects. There is therefore no reason why the datasets should be less suitable to evaluate e.g. the response to (internal or external) forcing, or teleconnections in the models. So in summary: yes evaluation of absolute magnitude values is tricky anyway, and order of operations contributes, but evaluation of variability-related effects or long-term changes is certainly possible. [Markus Donat, Spain]	Taken into account. This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
105233	26	52	26	53	This is also true for mean temperature and precipitation. [Francis Zwiers, Canada]	noted
102531	26	52	26	53	this statement is important - elaborate why this is so. [Philippe Tulkens, Belgium]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
42455	27	0	27	0	The chapter does not mention the emerging contribution of ground-based weather radar data to climatological studies (see for example the review of Saltikoff et al. 2019, (Saltikoff, E., Friedrich, K., Soderholm, J., Lengfeld, K., Nelson, B., Becker, A., ... & Tassone, C. (2019). An Overview of Using Weather Radar for Climatological Studies: Successes, Challenges, and Potential. Bulletin of the American Meteorological Society, 100(9), 1739-1752. https://doi.org/10.1175/BAMS-D-18-0166.1), or the study by Voormansik et al 2020, (Voormansik, T., Cremonini, R., Post, P., & Moisseev, D. (2020). Use of dual-polarization weather radar quantitative precipitation estimation for climatology. Hydrology and Earth System Sciences Discussions, 1-14.https://doi.org/10.5194/hess-2019-624). This could be solved by renaming section 11.2.1.2 to cover also ground-based weather radar data, so instead of "Satellite-based instrumental record" it could be renamed to "Remote sensing-based instrumental record". In any case I strongly recommend to add a paragraph about this aspect in current section 11.2.1.2. [Joan Bech, Spain]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed. Chapter 2 and Chapter 10 assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
105235	27	1	27	1	Is there a more recent update on the availability of indices? [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed. And we have provided a reference (Dunn et al 2020) for the updated indices where relevant.
105237	27	2	27	2	Delete “very” – I don’t think the emphasis is required. [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
51589	27	3	27	4	Suggest that here you could make reference to the two types of reanalysis available (full input - e.g. ERA5, and surface input - e.g. 20CRv3) [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	This section has been substantially modified from SOD to FGD. Chapter 2 (Section 2.3) and Chapter 10 (Section 10.2) assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
113571	27	6	27	6	Read carefully this section because it containsmultple typing errors. [Diego Miralles, Belgium]	Taken into account. This section has been substantially modified from SOD to FGD.
72101	27	6	27	49	This section is Africa-biased and middle East-biased. Africa most data sparse areas. Many papers used, and sometimes compare satelite products. Yet none of them is cited here. The cited literature should be balanced acrossed continents. Here are some literature that can be taken into account: 1. Dinku et al. 2008: Validation of high-resolution satellite rainfall products over complex terrain. International Journal of Remote Sensing. DOI: https://doi.org/10.1080/01431160701772526; Harrisson et al. 2019: Identifying changing precipitation extremes in Sub-Saharan Africa with gauge and satellite products. Environmental Research Letters. DOI: https://doi.org/10.1088/1748-9326/ab2cae; Sylla et al. (2013): Uncertainties in daily rainfall over Africa: assessment of gridded observation products and evaluation of a regional climate model simulation. International Journal of Climatology. DOI: https://doi.org/10.1002/joc.3551; Chamberlain et al.(2019): Evaluation of remotely sensed rainfall products over Central Africa. https://doi.org/10.1002/qj.3547 There are many other sections like this where research done in Africa regions is completely overlooked. [Mouhamadou Sylla, Rwanda]	Taken into account. This section has been substantially modified from SOD to FGD. Chapter 2 and Chapter 10 assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
106749	27	7	28	39	There are many non-referred papers which, have used and assessed satellite products with regard to ground-truth data, and some even compared latest ECMWF atmospheric reanalyses within data assimilation systems in Africa. I think they should be considered; here are some: 1°/ Harrisson et al. 2019: Identifying changing precipitation extremes in Sub-Saharan Africa with gauge and satellite products. Environmental Research Letters. DOI: https://doi.org/10.1088/1748-9326/ab2cae 2°/ Asessment of ERA5 precipitation over Burkina Faso (in West Africa) highlighted in this paper: Tall, M.; Albergel, C.; Bonan, B.; Zheng, Y.; Guichard, F.; Dramé, M.S.; Gaye, A.T.; Sintondji, L.O.; Hountondji, F.C.C.; Nikiema, P.M.; Calvet, J.-C. Towards a Long-Term Reanalysis of Land Surface Variables over Western Africa: LDAS-Monde Applied over Burkina Faso from 2001 to 2018. Remote Sens. 2019, 11, 735. 3°/ Chamberlain et al. (2019): Evaluation of remotely sensed rainfall products over Central Africa. https://doi.org/10.1002/qj.3547 [Moustapha Tall, Rwanda]	Taken into account. This section has been substantially modified from SOD to FGD. Chapter 2 and Chapter 10 assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.

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105239	27	10	27	13	I think this is a point that needs assessment as opposed to simply stating that the availability of sub-daily data is a “key advantage”. Polar orbiting satellites may indeed provide sub-daily resolution, but perhaps not without inducing some serious “aliasing” problems. Suppose, for example, that a satellite observing system re-observes a given location every 18 hours, and suppose that the element of interest has significant variability at semi-diurnal (12 hour) time scales. That 12-hour variability, sampled at 18-hour intervals, will appear as variability at the 36-hour time scale, a phenomenon known as aliasing (see virtually any text on time series analysis) – variability at shorter than the temporal scale that can be resolved in the measurements is effectively “folded” back onto lower frequency parts of the power spectrum. This is a real concern given the strong influence of the diurnal cycle, which has a shape that is more complex than purely sinusoidal for most variables. It is also of direct concern with measurements of things like surface pressure, in which a semi-diurnal thermal “tide” is clearly evident (this is, in fact, the dominant source of sort term surface pressure variability in the tropics). [Francis Zwiers, Canada]	Taken into account. This section has been substantially modified from SOD to FGD. Chapter 2 and Chapter 10 assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
7203	27	12	27	23	TRMM example is taken from a paper and it is not accurate. In fact that should be TMPA product (reference : Huffman et al. 2007). The more updated reference should be IMERG (Huffman et al. 2019). In any case Maggioni et al. (2016) is just using this product and should not be used as a reference for this product. It would be a WRONG reference. - Huffman, G. J., R. F. Adler, D. T. Bolvin, G. J. Gu, E. J. Nelkin, K. P. Bowman, Y. Hong, E. F. Stocker, and D. B. Wolff (2007), The TRMM multisatellite precipitation analysis (TMPA): Quasi-global, multiyear, combined-sensor precipitation estimates at fine scales, Journal of Hydrometeorology, 8(1), 38-55. -- Huffman, G. J., E. F. Stocker, D. T. Bolvin, E. J. Nelkin, and J. Tan, 2019: GPM IMERG Final Precipitation L3 1 day 0.1 degree x 0.1 degree V06. 6 ed., NASA, https://gpm.nasa.gov/sites/default/files/document_files/IMERG_ATBD_V06_0.pdf [Ali Behrangi, United States of America]	Taken into account. Text has been revised.
6837	27	15	27	16	The sentense "Hence their ability as substitute … is limiited" does not reflect reality as far as geostationary satellites (e.g. METEOSAT) is concerned as they provide high frequency observations at time scales of the order of 5 minutes (rapid scan) to 15 minutes (e.g. clouds, convenction, storm clouds,...). [Constantinos Cartalis, Greece]	Noted. The emphasis here is that the satellite-based products are of indirect nature and are subjective to substantial error and biases and as such are limited as substitute of direct measurement.
62871	27	16	27	16	Consider changing "pentad" to "5 days" or similar wording to clarify that the authors mean 5 days and not e.g. 5 years or other 5 entities of a time window. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text has been revised.
51591	27	17	27	17	Perhaps you could name the two reanalyses which extend to 1900 and beyond - note they are named further down in the paragraph, but it would be useful to include this information further up to help the reader. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	comment does not appear to apply to text at this location. This section has been substantially modified from SOD to FGD. Chapter 2 and Chapter 10 assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
13685	27	21	27	21	Remove the second parentheses in (Alexander et al., 2019, submitted); [Maria Amparo Martinez Arroyo, Mexico]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
43323	27	21			Read "(Alexander et al., 2019, submitted; Bador et al., submitted)" rather than "(Alexander et al., 2019, submitted); Bador et al., submitted)" [Cyriaque Rufin Nguimalet, Central African Republic]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
51593	27	22	27	22	Please could you provide time range for this first comparison with reanalyses (1979-2008/2010?), to distinguish against the century long comparison which comes thereafter. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
20731	27	24	27	27	Of course homogeneity of time series is complicated to achieve for satellites, same as other measuring vectors. The sentence concerning dry versus wet precipitation is mysterious, a reference is necessary. Finally, one should not limit climate assessments to precipitations as implied here. [philippe waldteufel, France]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
7205	27	26	27	26	modify "…. Orographic precipitation …" to "Orographic precipitation and snowfall (Behrangi et al. 2018)…" Behrangi, A., K. J. Bormann, and T. H. Painter (2018), Using the Airborne Snow Observatory to Assess Remotely Sensed Snowfall Products in the California Sierra Nevada, Water Resources Research, https://doi.org/10.1029/2018WR023108(0), doi: doi:10.1029/2018WR023108. [Ali Behrangi, United States of America]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
82301	27	28	27	28	Please change "Masunuga et al., submitted" into "Masunaga et al. (2019)". [Schröder Marc, Germany]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
105241	27	30	27	30	There is in fact, much more data in existence in many jurisdictions, including at higher latitudes, than this statement would imply, but this data might not be gathered by national met services, might not be obtained using instruments and instrumental siting that meets all WMO standards, and might not be openly available for public use and exchange. Nevertheless, there is a lot of data that can potentially fill gaps. Some organizations, like my own, have been working to make that data available for public use in the jurisdictions that they serve – e.g., see https://data.pacificclimate.org/portal/pcds/map/. [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
42457	27	34	27	34	Typo: observations(Harrison -> observations (Harrison [Joan Bech, Spain]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
51595	27	34	27	37	If possible, suggest that you introduce the Timmermans work in the same way as the previous two studies, and outline that as part of their assessment, they compare two reanalyses (ERAI & NARR [which isn't used in the other two at all]) to observational products. And then state the conclusions and likely cause. I found the current phrasing difficult to place in context when following on from the previous sentences. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
23105	27	35	27	36	This sentence makes no sense as written. What was intended here? [Peter Thorne, Ireland]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
7207	27	37	27	37	At the end of this paragraph you may want to add " … and depend on the period delected for extreme analysis our assessment of intensity of extreme events can be different (Golian et al. 2019) Golian, S., M. Javadian, and A. Behrangi (2019), On the use of satellite, gauge, and reanalysis precipitation products for drought studies, Environmental Research Letters, 14(7), 075005, doi: 10.1088/1748-9326/ab2203. [Ali Behrangi, United States of America]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
105445	27	38	27	38	add in term of publications, another techniques in sout america to improve the data for analysis of extrems events. In Perú Aybar et.al 2020, in the paper: "Construction of a high-resolution gridedd rainfall datasetfor Peru from 1981 to the present day", talk about the use of the geostatistical and deterministic interpolation methods, including 1) national quality controlled and infilled reindgauge dataset, 2) radar.gauge merged precipitation climatologiesand 3) the Climate Hazardsgroup Infrared Precipitation (CHIRP) [Elizabeth SILVESTRE, Peru]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
102533	27	39	27	39	"Shorter duration satellite products…" maybe? [Philippe Tulkens, Belgium]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
105243	27	39	27	49	I agree that shorter satellite products may provide useful insights, but I don’t find the examples that are provided to particularly compelling (uncertain information about trends doesn’t, frankly, help a lot). Such products are, presumably, useful for process studies, and could also be used to delineate the spatial extent of an extreme event that, based on in situ observations or documentary evidence, is understood to be extreme in a particular location. [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.

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23107	27	41	27	43	Perhaps note that GRACE-FO is now in operation and that due to the self-calibrating system the data gap is not fatal to long-term characterisation so assuming continuity of future missions offers a long-term high quality product into the future? [Peter Thorne, Ireland]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
113573	27	52	27	52	I would add a mention in this section to MSWEP, which is becoming one of the most widely used precipitation datasets lately, and is used in Chapter 2. Maybe something like. 'Precipitation datasets combining satellite, in situ and reanalysis data have also been developed and applied to study extremes in recent years (Beck et al. 2016, 2019)' Beck, H. E., van Dijk, A. I. J. M., Levizzani, V., Schellekens, J., Miralles, D. G., Martens, B. and de Roo, A.: MSWEP: 3-hourly 0.25° global gridded precipitation (1979–2015) by merging gauge, satellite, and reanalysis data, Hydrol. Earth Syst. Sci. Discuss., 1–38, doi:10.5194/hess-2016-236, 2016. Beck, H. E., Wood, E. F., Pan, M., Fisher, C. K., Miralles, D. G., van Dijk, A. I. J. M., Mcvicar, T. R. and Adler, R. F.: MSWEP V2 Global 3-Hourly 0.1° Precipitation: Methodology and Quantitative Assessment, Bull. Amer. Meteor. Soc., 100(3), 473–500, doi:10.1175/BAMS-D-17-0138.1, 2019. [Diego Miralles, Belgium]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed. Chapter 2 and Chapter 10 assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
23109	27	54	28	19	This was covered in some depth in chapter 1 and repetition here is not helpful. This text should be exchanged with chapter 1 and the text replaced with a brief citation to chapter 1 for the interested reader to find out more placed at the start of the next paragraph. [Peter Thorne, Ireland]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed. Chapter 2 and Chapter 10 assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
52653	28	1	28	19	Beck et al. (2019) compared different types of observations and provide information on where satellite estimates are better than reanalyses and the other round. https://www.hydrol-earth-syst-sci.net/23/207/2019/ [Mary-Jane Bopape, South Africa]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed. Chapter 2 and Chapter 10 assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
62611	28	2	28	3	The statement is misleading. Reanalysis data is not limited to atmospheric variables, rather we also have ocean reanalysis such as ECMWF ORAS5, SODA etc. Therefore, sentence needs modification. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed. Chapter 2 and Chapter 10 assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
13833	28	5	28	8	Due to the nature of extreme events (spatial and temporal scale) and since reanalysis data have difficulties in representing physical processes in regions with complex orography, How have these constraints been handled when using reanalysis data? [Maria Amparo Martinez Arroyo, Mexico]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed. Chapter 2 and Chapter 10 assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
33247	28	8	28	8	It could be added that reanalyisis have similar limitations than gridded datasets to study extremes (not only inhomogeneities) such as smothing, specially for extreme rainfall. This effect is even greater for in mountanious terrain where current reanalysis cannot properly reperesent orography [Gonzalez Sergi, Spain]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed. Chapter 2 and Chapter 10 assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
74513	28	10	28	10	inhomoheneities to correct on inhomogeneities [Moulay Driss HASNAOUI, Morocco]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed. Chapter 2 and Chapter 10 assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
105245	28	10	28	10	The bit in parentheses misses the key point (discussed further down in the paragraph), that a dominant source of inhomogeneity in reanalyses is from the wide scale introduction of new observing systems (e.g., radiosondes, satellite products and maybe to a lesser extent, the direct assimilation of satellite radiances rather than the derived observational products). [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed. Chapter 2 and Chapter 10 assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
6785	28	16	28	16	Radiosondes were not "introduced in 1958". The radiosonde network (mainly over land but including the ocean weather ships that preceded observations by satellite) was established in the second half of the 1940s and in the 1950s, and was developed further thereafter. Some extra stations were established in 1957, particularly in the southern hemisphere, in preparation for the Internationational Geophysical Year, 1958. To illiustrate the growth in numbers, I have taken snapshots of the number of radiosonde temperature data used between 450 and 550hPa in January by the ERA5 (1950-present) reanalysis for various years. The numbers are: 1950 22526; 1957 51085; 1958 55691; 1968 66124; 1978 93835. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed. Chapter 2 and Chapter 10 assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
6787	28	16	28	16	Satellite data were not introduced in 1979. Operational sounding of temperature began with the first pair of VTPR instruments, which were launched in October 1972. BUV ozone data are available from 1970. The VTPR data have been assimilated in the ERA-40, JRA-55 and ERA5 reanalyses, and ERA5 also assimilated the BUV ozone data. The first of the next generation of sounding satellites was launched in October 1978, and its data were assimilated in ERA5 around the beginning of December 1978, i.e. prior to 1979. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed. Chapter 2 and Chapter 10 assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
62613	28	16	28	16	Radio sonde should be writeen as “Radiosonde”, otherwise it’s confusing! [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed. Chapter 2 and Chapter 10 assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
105247	28	17	28	19	It would be useful to discuss the use of ensembles for quantifying the strength of the observational constraint on reanalyses such as 20CR and ERA-20C. The 20CR is a true ensemble product (56 members), allowing users to assess uncertainty through the spread of the ensemble. In some parts of the world (notably the southern hemisphere), that spread remains large until very near the present when considering a storm activity index (e.g., see Figure 4 in Wang et al., 2012, doi: 10.1007/s00382-012-1450-9. ERA-20C used an ensemble to obtain information about the spatio-temporal evolution of background errors, and thus there is presumably some information that is also available about the strength of the observational constraint for different fields (see the ERA-20C landing page at https://www.ecmwf.int/en/forecasts/datasets/reanalysis-datasets/era-20c. [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed. Chapter 2 and Chapter 10 assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
62873	28	19	28	19	Consider adding records from natural paleo archives and documentary data time-series as possible input data for reanalysis encompassing longer time frames and consider adding a reference to Box 11.2. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed. Chapter 2 and Chapter 10 assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
13835	28	21	28	22	It's suggested to explain briefly which indicators representing temperature and precipitation extremes were evaluated and for which periods the evaluations were done. [Maria Amparo Martinez Arroyo, Mexico]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed. Chapter 2 and Chapter 10 assess various aspects of data sources and data products from the perspective of their general use and in the analysis of changes in the mean state of the climate in particular.
69933	28	23	28	23	ONOGI should be Onogi [Masayoshi Ishii, Japan]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
6789	28	26	28	26	To write "during the pre-satellite era" is not wrong, but is maybe a bit misleading. This is partly because what constitutes the pre-satellite era is different for different people (see preceding comment) and partly because as one goes back before the 1970s there begin to be gaps in conventional observational coverage from particular countries (pending data rescue). So the lack of consistency may come both from the lack of satellite data and from the (to some extent remediable) lack of conventional data. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
14817	28	28	19	20	This sentence would benefit from being reworded [Marie-France Loutre, Switzerland]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
23111	28	28	28	28	Naming nomenclature for these should follow lead of chapters 1 and 2. [Peter Thorne, Ireland]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
62607	28	30	28	33	Plagiarism issue: the lines are fully copied from https://www.sciencedirect.com/science/article/pii/B9780128148952000033?via%3Dihub . The lines should be within double quotation otherwise should be reprhased/modified. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
105249	28	36	28	37	This assessment of the success of station precipitation data assimilation into the NARR might be appropriate for the continental USA, but a different, much lower resolution data source was used for Canada, such that is it possible to see the political boundary between the two countries. It is also not quite correct to say precipitation observations where directly assimilated – my understanding is that they were used to constrain the atmospheric latent heating profile associated with precipitation. See for example, https://www.ncdc.noaa.gov/sites/default/files/attachments/narr-nomads-presentation.pdf or the Messinger et al paper describing the NARR. [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
62781	28	42	31	17	I do not understand why include this box here. The two previous subsections talk about ground and remote-sensing data to characterize extreme events, whereas table describes comparisons between paleolimnological records and instrumental ones. Maybe such information could be merged in the instrumental records subsection, presenting that in many cases, the current data obtained with specific instruments are also used to make inferences about past changes and even intercalibrations. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. This box contains more than data. It assesses paleo-extremes with an aim to place historical extremes into paleo perspective. As it assesses different variables, it is more suited to appear before assessment to different extremes are presented in the subsequent sections.
106533	28	42			WGII has a CCB PALEO (housed in WGII ch3) that is relevant to BOX 11.2. Care should be taken to ensure consistency between WGs in messages and uncertainty assessments of those messages. [camille parmesan, France]	Noted.
105251	28	44	28	44	This box seems rather long and ponderous to me, and I'm left wondering, after reading it, whether it actually provides us with insight concerning the frequency and intensity of extremes in the modern observed period relative to those that happened during the Common Era. We learn, for example, that there is high confidence that long duration severe drought occurred in some locations – but how do I use that information to understand drought in the current climate or in the future climate? [Francis Zwiers, Canada]	Considered. While it is very desirable to be able to compare both frequency and intensity of paleo extremes with those in historical observations, existing literature does not provide sufficient details to allow confident assessment.
4649	28	44	31	15	- statements are approximative and awkward, with sometimes even inappropriate / wrong references, Proposed changes should help to limit this. - this is reinforced by the fact that many paragraphs are generic for all types of extremes and all types of archives, while each archive and each extreme is specific, - the main knowledge acquired since 2013 (AR5) is not well highlighted but cannot given the very few place let to paleoarchives, Indeed, 2 on 271 pages is a very small contribution, while a large peer-review literature on paleo-extremes has been published since 2013. - Therefore, this box does not properly reflect science developped by hundreds of researchers in the field of paleo-extremes and the associated results published since 2013. The absence of researcher from the paleo-community among the lead and contributing authors certainly explain this. [Bruno Wilhelm, France]	Considered. Specific comments by this reviewers are addressed in the following.
11639	28	44	31	15	This box comprehensively evaluates our knowledge of extreme events in the Common Era. However, since this is the only text in the chapter devoted to paleoclimate reconstructions, it seems unfortunate to restrict the text to the Common Era. While this period is undoubtedly the most robust in terms of extreme event reconstruction, there are other time periods for which a growing body of literature addresses extreme events earlier in the Holocene. This is particularly relevant if the definition of "extreme events" includes those events that occur on centennial, rather than just annual or decadal timescales. These centennial-scale events are often considered extreme in the context of longer term (millennial-scale) climate conditions. For example, both the 4.2 ka and 8.2 ka events have been proposed as rapidly occuring climate shifts, either regionally or globally, with numerous impacts on temperature, hydroclimate, and human systems. The paleoclimate contribution to this chapter could be strengthened and broadened by adding a short discussion of recently published literature on both these events. For 4.2 ka, recent studies include Zhang et al. (2018, doi:10.5194/cp-14-1805-2018), Carolin et al. (2019, doi:10.1073/pnas.1808103115), and Isola et al. (2019, doi:10.5194/cp-15-135-2019), among many others. For 8.2 ka, recent studies include Oster et al. (2017, doi:10.1038/s41598-017-04215-5), Matero et al. (2018, doi:10.1016/j.epsl.2017.06.011), and Porinchu et al. (2019, doi:10.1016/j.quascirev.2019.07.024), among many others. [Ellie Broadman, United States of America]	Considered. The main aim of the box is to place historical extremes assessed in the chapter in a longer-term perspective rather than assessing all extremes that may have been recorded in all paleo records. For this reason, Common Era has been the focus of the box as the information is the most robust. Extremes in the historical time assessed in the chapter are of relatively short time scale in particular for extreme precipitation and extreme temperatures. For this reason, extremes of longer time scales than those on the historical records are in general not assessed.
62881	28	44	31	15	Consider adding the contribution of ice archives (polar as well as high alpine ice cores) to extend temperature records of extremes beyond the observation period. The Box does not mention ice cores despite them being one of the most widely used climate archives for yearly resolution records. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. The main aim of the box is to place historical extremes assessed in the chapter in a longer-term perspective rather than assessing all extremes that may have been recorded in all paleo records. For this reason, Common Era has been the focus

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
62883	28	44	31	15	Please emphasize the importance of natural archives to estimate past extremes for regions where instrumental data is scarce and where instrumental and observation data are not reaching far back in time and documentary data is scarce. The current Paleobox focuses mainly on restrictions rather than the fact that for some regions paleo archives provide the only long-term data available for climate and climate extremes and are thus extremely valuable despite the restrictions they inherit [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. The box is not about how important or useful of the natural archives, it is about confidence about changes that may be infered from natural archives. Mentioning limitation is relevant for assigning confidence in the assessment, but mentioning "importance" is not very relevant to the assessment. For this reason, text is not revised.
14825	28	44	31	15	I suggest to re-write the box as suggested in the introduction, i.e. iii) droughts, iv) temperature extremes, v) palaeofloods and vi) palaeotempests, instead of going back and forth on the topic, in particular for the part of the box before p29 line52 [Marie-France Loutre, Switzerland]	Considered. The materials in the box are reorganized.
68087	28	44	31	15	Box 11.2: I think the content of this box subverts its summary (pg 31, l. 9-10). In parallel with other Chapters of this WG report, I would move it out of a box and make it a section. There are certainly large uncertainties [:-)] in identifying extremes from paleoclimate data, but most of these uncertainties, with the exception of chronology, are present in historical data as well. I also recognize that the scope of the chapter, as defined, focuses on time and event scales that are not amenable to paleoclimatology. But here are some reasons why the paleoclimate record is valuable and should be part of the review. (1) They permit us to access extremes that persist on longer timescales than are possible in the historical period, for example, the probability of occurrence of drought in a region that persists for decades, or events that would be termed 'Black Swans' if viewed solely within the lens of the satellite era. In other words, the "tails of the distribution" we haven't yet seen; pg 31, l. 22-23.: the data set has to be of sufficient temporal coverage. (2) They permit the detection of large-impact, low probability events (e.g. Cook et al 2004; 2010; 2014) which could produce great societal risk. (3) In combination with realistically or idealistically forced simulations, they provide out-of-sample hindcasting targets, and the means to link cause and effect for such events (e.g. Haywood et al 2004; Graham et al 2007; Neukom et al 2019). (4) In combination with realistic weather generators, emulators, and downscaling, they provide the potential to link from the global to the regional (e.g. Gomez-Navarro et al 2012). (5) Paleoclimatic observations are available for periods not influenced by anthropogenic forcing, and therefore permit estimates of the natural occurrence of extreme events that may be distinct and possibly larger than, the anthropogenically forced extrema (Wittenberg et al 2009; Cobb et al 2013). In contrast to what is here, I would say: paleoclimatic records provide information on the possibility and probability of extreme events of amplitudes and timescales not directly observable in the past century. They provide estimates of extremes that would occur in the absence of anthropogenic forcing, and upon which anthropogenically forced extremes may sit (e.g. Seager et al 2004). In conjunction with global and regional earth system modeling, they permit study of the extent to which different mean states affect the likelihood and the amplitude of extremes (Dutton et al 2015; Fischer et al 2018), and the potential to study extrema within a dynamic and nonstationary earth system. [MIchael Evans, United States of America]	Considered. The main aim of the box is to place historical extremes assessed in the chapter in a longer-term perspective rather than assessing all extremes that may have been recorded in all paleo records. But most relevant aspects are still assessed (e.g. the droughts that lasted longer than those in historical records).
4711	28	46	28	47	It is a restricted view. It also helps e.g. to understand "long term" variations of extremes that cannot be identified from the short instrumental series. [Bruno Wilhelm, France]	Noted. But "long term" variations would have been part of the "longer-term context".
4651	28	48	28	49	"palaeoreconstructions" is unusual and somehow an oxymoron. "in historical and natural evidence" should be prefered. [Bruno Wilhelm, France]	Considered. Changed to 'paleoclimate reconstructions'
62875	28	49	28	49	Both, Oxford and Cambridge dictionary relate ecclestiastical to the Christian church in their definitions. Consider replacing the word "ecclesiastical" with a term more inclusive for other religious documents and simpler for the understanding of the broad IPCC audience. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. Changed to 'religious'
42459	28	49	28	49	I suggest to change "ecclesiastical" by "religious", as not only Christian documentary sources are available as climate proxy data - see for example O'Hara & Metcalf, 1997 (O'Hara, S. L., & Metcalfe, S. E. (1997). The climate of Mexico since the Aztec period. Quaternary International, 43, 25-31. https://doi-org.sire.ub.edu/10.1016/S1040-6182(97)00017-7) [Joan Bech, Spain]	Changed to 'religious'
23937	29	2	29	4	This first sentence is confusing since the instrumental period of course occurred within the last millennium. I suggest changing the wording to "during the preceding millennium" or "in the rest of the last millennium". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Changed to "preceding millennium'
76681	29	4			edvidence [Piero Lionello, Italy]	Changed
20241	29	5	29	5	Evidence [philippe waldteufel, France]	Changed
4653	29	5	29	5	Delete the first "d" in "evdidence". [Bruno Wilhelm, France]	Changed
4655	29	5	29	5	Delete "assessed". [Bruno Wilhelm, France]	Changed
82755	29	7	29	7	Add "instrumentally" after "observed" [Blair Trewin, Australia]	Added
62759	29	7			More can be included after "those observed", reiterate when the observations occurred and how they were collected. Ie. In the instrumental period [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Text edited.
23939	29	7			Tense not consistent with rest of paragraph. Change "report" to "reported". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Changed
4657	29	14	29	29	Another important factor is not given. The lack of (long) instrumental series also affect the confidence of reconstructions since it makes the quantitative calibration of the reconstructed signal / proxy difficult or even impossible. This is particularly true when reconstructing past flood discharges. [Bruno Wilhelm, France]	Already noted in this paragraph
45679	29	15	29	18	The second and third sentences appear at odd with the other statements in the paragraph. [Christophe Deissenberg, Luxembourg]	Rejected -not clear how
4659	29	16	29	16	The review from Wilhelm et al., 2019, WIREs (already cited elsewhere) can be cited here regarding the geographical coverage of palaeoflood records. [Bruno Wilhelm, France]	Rejected - references not used for each event type
4661	29	17	29	18	It is also depending on means (e.g. financial) of the different countries/continent. See e.g. the lack of data in Africa or Southern America. [Bruno Wilhelm, France]	Already noted by ('also the differing attention and focus from the scientific community. ')
62877	29	22	29	23	Sigl et al. (2015) provide the most comprehensive summary of large paleo eruptions and their climate forcing and should be included here: Sigl, M., Winstrup, M., McConnell, J. R., Welten, K. C., Plunkett, G., Ludlow, F., ... & Fischer, H. (2015). Timing and climate forcing of volcanic eruptions for the past 2,500 years. Nature, 523(7562), 543-549. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected- text is on impact of volcanic eruptions, not eruptions themselves
23113	29	24	29	26	The example does not support the point about seasonality. [Peter Thorne, Ireland]	Deleted
17811	29	24	29	26	The text in the parenthesis does not match the statement in the sentence. The sentence is about seasonality, the parenthesis about meteorological variables. Re-write the parenthesis to: e.g. dendrochronlogical archives mostly record summer conditions. Or include another sentence to discuss variable dependence of proxy data. [Raphael Neukom, Switzerland]	Deleted
82757	29	25	29	25	The example doesn't match the text here. Better as "one season or one variable"? [Blair Trewin, Australia]	Deleted
4663	29	25	29	26	space to be deleted before "e.g." [Bruno Wilhelm, France]	Deleted
4665	29	25	29	26	the example given in brackets is not relevant since it is not related to the season. In addition, seasonally reconstructed signals are also highly valuable since changes in timing of extremes is a key question in the context of the global warming. As a result, the sentence should not be written in a negative way. [Bruno Wilhelm, France]	Deleted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4667	29	26	29	27	The first part is an obvious statement, while the second part ("in the absence of trends") is unclear. Remove it or make it clearer. [Bruno Wilhelm, France]	Changed
23115	29	27	29	29	This sentence makes no sense to me as written. Can you please clarify intent here? [Peter Thorne, Ireland]	Changed
4669	29	29	29	29	Add a "," between records and which such as "records, which". [Bruno Wilhelm, France]	Changed
109819	29	31	29	31	good e.g. of this is: Archer, D., O'Donnell, G.M.O., Lamb, R., Warren, S., Fowler, H.J. 2019. Historical flash floods in England: new regional chronologies and database. Journal of Flood Risk Management, 12 (Suppl. 1):e12526, DOI: 10.1111/jfr3.12526. [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
4671	29	31	29	43	The whole paragraph is very approximative and awkward and needs to be rewritten. [Bruno Wilhelm, France]	Rejected - Unclear what is wanted by reviewer
62615	29	33	29	33	minutes to hours or day -> minutes to days [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Changed
4673	29	34	29	34	"Natural archives may be sensitive only to intense environmental disturbances" -> "Intense environmental disturbances" are extreme events. So what's the matter? [Bruno Wilhelm, France]	Noted in paragraph that sources and mechanisms also required
4675	29	34	29	35	"so only sporadically record short duration or small spatial scale extremes" -> this is fully wrong for e.g. paleoflood records, which record floods on a given river. On a small river, those floods occurred in a small catchment and in a very short time. The same for e.g. paleotempests if they would happen at local scale. [Bruno Wilhelm, France]	Disagree. The sentence is not wrong if one reads the whole sentence.
4677	29	35	29	40	Paleoflood hydrology is a science that knows large developments since the 70s (except for the very new archive of speleothems). So the interpretation of the sedimentary records is quite well constrained now. For such a review of all types of palaeflood archives and methods recently published by the paleoflood community, please refer to the following references already cited: - Wilhelm et al., 2018, Water Security - Wilhelm et al., 2019, WIREs [Bruno Wilhelm, France]	Considered. Wilhelm et al. 2019 is cited.
83521	29	37	29	37	Shelf (marine) sediments should be added to the list of geological archives. Both Abrantes and co-authors studies listed in the comment above use shelf sediments near river mouths for their studies. [Antje H. L. Voelker, Portugal]	Rejected addition - paragraph refers to methodologies, not individual studies
24205	29	37	29	37	the abbreviation "e.g." is missing a period. [Rhawn Denniston, United States of America]	Changed
83519	29	37	29	40	As a study combining geologcial (sedimentological), faunal (freshwater diatoms) and historical evidence for flooding events you could add as reference: Abrantes, F., Rodrigues, T., Montanari, B., Santos, C., Witt, L., Lopes, C., Voelker, A.H.L., 2011. Climate of the last millennium at the southern pole of the North Atlantic Oscillation: an inner-shelf sediment record of flooding and upwelling. Climate Research 48, 261-280, doi: 10.3354/cr01010. Or building on the 2011 paper Abrantes, F., Rodrigues, T., Rufino, M., Salgueiro, E., Oliveira, D., Gomes, S., Oliveira, P., Costa, A., Mil-Homens, M., Drago, T., Naughton, F., 2017. The climate of the Common Era off the Iberian Peninsula. Clim. Past 13, 1901-1918, doi: 10.5194/cp-13-1901-2017 - which does not use diatom evidence, but instead biomarker and pollen data. [Antje H. L. Voelker, Portugal]	Rejected addition - paragraph refers to methodologies, not individual studies
43325	29	37			Read " (eg. river and lake sediments), " rather than " (eg. river and lake sediments, " [Cyriaque Rufin Nguimalet, Central African Republic]	Changed
62879	29	39	29	39	diatoms (algae) are not consedered part of fauna. Fauna mostly refers to multicellular eucaryotes while diatoms have only one cell and share a large part of their genom with bacteria, see e.g. Mock, T., Samanta, M. P., Iverson, V., Berthiaume, C., Robison, M., Holtermann, K., ... & Kallas, T. (2008). Whole-genome expression profiling of the marine diatom Thalassiosira pseudonana identifies genes involved in silicon bioprocesses. Proceedings of the National Academy of Sciences, 105(5), 1579-1584. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Changed
99255	29	39			if the evidence should be restricted to diatoms, then it is a flora. I think it would be better to make this fauna and flora and e.g. fossil assemblages are floods in coastal setting for examples are done with other species than diatoms as well [Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Changed
4679	29	42	29	42	End of sentence not understood, please clarify. [Bruno Wilhelm, France]	Changed
4681	29	43	29	43	This statement relies on a case study, while a complete literature exists. What is the interest to mention one isolated unsuccessful study in this context? [Bruno Wilhelm, France]	Noted.
62617	29	45	29	45	High-duration is confusing term. It should be "longer-duration"; Further this confusing term is used in many other places in the chapter. It should be corrected for all occurences. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Changed
4683	29	45	29	47	This sentence is partly wrong since it is depending on the type of extreme events. May be true for droughts but not for floods that rarely last more than a week (and last hours to days most of the time). [Bruno Wilhelm, France]	Rejected. It means that for events like floods, the pre-instrumental evidence is not as complete.
4685	29	45	29	52	This paragraph is ok when talking about temperature but not for other types of extremes. So clarify this at the beginning of the paragraph. [Bruno Wilhelm, France]	Rejected - may hold for other extremes too
23117	29	54	29	54	I assume you mean long-duration not high-duration as duration is a length not a magnitude. Also applies to box summary statement [Peter Thorne, Ireland]	Changed
29913	30	3	30	3	For the Andes region, I would include the following paper that focus on the recent megadrought over Chile: https://doi.org/10.5194/hess-21-6307-2017 [Juan Rivera, Argentina]	Considered. The paper is cited.
4687	30	4	30	5	Start a new sentence with "Recent observed drought extremes.." [Bruno Wilhelm, France]	Changed
45681	30	7	30	7	"but Africa still lags in its contribution" sounds almost like a reproach against African nations. Is that intended? Would "but less so in what concerns Africa" capture your thinking? [Christophe Deissenberg, Luxembourg]	Rejected. There was no mentioning about Africa at the particular page/line number
42461	30	7	30	7	Please check "exceed" or "exceeded"? [Joan Bech, Spain]	Changed
82759	30	14	30	14	Presumably this refers to extreme high temperatures? [Blair Trewin, Australia]	Changed
62621	30	18	30	18	the sentence indicates recent years, however examples are given for two years. This is confusing. Sentence needs modification to better reflect the the information. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Changed
4689	30	23	30	23	Add a space after "temperatures". [Bruno Wilhelm, France]	Changed
42463	30	23	30	23	Typo: temperatures(Orth -> temperatures (Orth [Joan Bech, Spain]	Changed
4691	30	27	30	27	Clarify what are exactly the AR5 assessments you mentionned. [Bruno Wilhelm, France]	Changed
4693	30	27	30	27	The reference Wilhelm et al., 2018 is not appropriate here since those AR5 assessments are not discussed therein. [Bruno Wilhelm, France]	Changed
4695	30	30	30	30	The case of the Rhône River (reconstructed peak discharge more than twice higher than observed ones) can be added for Europe: Evin G., Wilhelm B., Jenny J.P. (2019) Flood hazard assessment of the Rhône River revisited with reconstructed discharges from lake sediments, Global and Planetary Change 172, 114-123 [Bruno Wilhelm, France]	Rejected as not all studies cited individually
4697	30	32	30	32	Precise "European Alps" (e.g. Alps also exist in New Zealand). [Bruno Wilhelm, France]	Changed
4699	30	32	30	32	Put "e.g." before the references of Swierzynski et al. and Amann et al. since a very long list of studies could be cited here. [Bruno Wilhelm, France]	Changed

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4701	30	33	30	35	This statemetn is true for any combination of archives. To highlight this statement between historical evidence and lake sediments, Evin et al. (2019) can be cited. For a full combination of archives, Schulte et al. (2019) can be cited. - Evin G., Wilhelm B., Jenny J.P. (2019) Flood hazard assessment of the Rhône River revisited with reconstructed discharges from lake sediments, Global and Planetary Change 172, 114-123. - Schulte L., Wetter O., Wilhelm B., Peña J.C., Amann B., Wirth S.B., Carvalho F., Gómez-Bolea A. (2019) Integration of multi-archive datasets for the development of a 1 four-dimensional paleoflood model of alpine catchments, Global and Planetary Change 180, 66-88. [Bruno Wilhelm, France]	Noted
4703	30	35	30	36	Wilhelm et al 2019 is inappropriately cited for China since only a few historical evidence are reported for Asia (see fig. 6). [Bruno Wilhelm, France]	Changed
4705	30	35	30	37	This statement is wrong since it is not restricted to historical records, e.g. many sedimentary records reveal the same trend. See for instance Evin et al. (2018) or Wilhelm et al. (2015): - Evin G., Wilhelm B., Jenny J.P. (2019) Flood hazard assessment of the Rhône River revisited with reconstructed discharges from lake sediments, Global and Planetary Change 172, 114-123. - Wilhelm B. , Vogel H., Crouzet C., Etienne D. and Anselmetti F.S. (2016) Frequency and intensity of palaeofloods at the interface of Atlantic and Mediterranean climate domains Climate of the Past 12, 299-316. [Bruno Wilhelm, France]	Noted. The meaning of this comment is not clear as historical records are not discussed here.
4707	30	41	30	42	This statement ("further prevent long term assessments..") is fully wrong since historical and paleo flood records show what already happened and, thereby, what could happen again (if protection infrastructure have not been built or inappropriatlely built). The example of Fukushima is e.g. a relevant example well documented by geological archives but NOT taken into account by stakeholders. Please read section 3.2 ofWilhelm et al., 2019, WIREs (cited many times in this box) to get a deeper insight on how risk assessments can be improved from historical and paleo data. In a few cases, it also makes a comprehensive understanding of flood management plan possible. See for instance the paper of Munoz et al, published in Nature showing how protection infrastructure increased natural flood magnitude: - Munoz, Giosan, Therrell, Remo, Shen, Sullivan, Wiman, O'Donnell & Donnelly (2019) Climatic control of Mississippi River flood hazard amplified by river engineering. Nature. doi:10.1038/nature26145 [Bruno Wilhelm, France]	Noted. The statement if "further present long-term assessments of flood changes …". What was meant is that while these important pre-instrument records provide information about past occurrence of the floods, they do not provide sufficient information to assess if and by how much historically floods may have changed.
62739	30	44	30	45	I don't understand "...that periods of both more and less tropical cyclone activity …" It doesn't clearly express whether it refers to the periods or the number of the tropical cyclone activity [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Changed
20733	30	44	30	53	The WG1 outline does not limit the perimeter of chapter 11 to land surfaces and coastal regions. What about TC on the open sea and the information brought by ship documentation and damages? [philippe waldteufel, France]	Changed
44391	30	52	30	52	replace "risk" with "probability". See IPCC guidance on risk for appropriate use of the term "risk" throughoput the IPCC report. [Jana Sillmann, Norway]	Changed
44387	31	5	31	5	correct to "worldwide occurence (or probability) of droughts", see IPCC guidance on risk [Jana Sillmann, Norway]	Changed
39273	31	9	31	11	It is being suggested that this statement be improved; it is incongruous. [Lourdes Tibig, Philippines]	Considered. The sentence is edited.
14827	31	9	31	15	The conclusion is far from clear. It first says that there is a low confidenc and then explains that there is high confidence! [Marie-France Loutre, Switzerland]	Rejected - there is high confidence in some facets and low in others
4709	31	12	31	13	This statemetn should be nuanced since it is not always true. See for instance Evin et al. (2018) or Munoz et al. (2019) that produced (paleo)flood record encompassing modern floods, making the comparison between modern and ancient floods possible: - Evin G., Wilhelm B., Jenny J.P. (2019) Flood hazard assessment of the Rhône River revisited with reconstructed discharges from lake sediments, Global and Planetary Change 172, 114-123. - Munoz, Giosan, Therrell, Remo, Shen, Sullivan, Wiman, O'Donnell & Donnelly (2019) Climatic control of Mississippi River flood hazard amplified by river engineering. Nature. doi:10.1038/nature26145 [Bruno Wilhelm, France]	Rejected. What is stated is "difficult" not "impossible". While it is possible to make comparison in some cases, even in these cases it would not be easy.
68089	31	20	31	38	How good are the assumptions behind the parametric approach for events that occur less than once every two decades? What is the null hypothesis for change detection for analysis of observations or simulations? [MIchael Evans, United States of America]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
1437	31	22	31	38	It is also possible to estimate the probability of moderately extreme precipitation (heavy precipitation) in terms of the product between the wet-day frequency and a cumulative probability function assuming an exponential distribution of wet day 24-hr precipitation. This "rain equation" has been evaluated against a large volume of historical rain gauge data with demonstrated skill (Benestad et al., 2019; DOI: 10.1088/1748-9326/ab2bb2). Unlike EVT, which assumes a constant number of rainy days for each year, the "rain equation" predicts increased probability for heavy rainfall both when the wet-day frequency increases and when the mean precipitation intencity (wet-day mean precipitation) increases. It is furthermore instrumental for explaing the reason for increase in heavy rainfall amount or frequency of extremes in terms of more rainy days of more intense 24-hr rains. [Rasmus Benestad, Norway]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
79637	31	26	31	29	It may be fair indicating that one disadvantage of the ETCCDI indices is that few of them are specifically sector-relevant. While some of these indices may be useful for sector applications (e.g. number of days with frost for agricultural applications, heat waves for health applications). For this reason, WMO has involvedsectors in the development of the new core set of 34 indices from the ET-SCI (Expert Team on Sector-specific Climate Indices), so that more application-relevant indices could be developed to better support adaptation (Alexander et al. 2019; Mistry 2019; Chisanga et al. 2017) [Wilfran MOUFOUMA OKIA, Switzerland]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
3649	31	32	31	33	You should add reference to the work of R. Katz - at least this one: Katz, R. W. (2010). Statistics of extremes in climate change. Climatic Change, 100, 71-76. doi:10.1007/s10584-010-9834-5 and this one: Katz, R. W., Parlange, M. B., & Naveau, P. (2002). Statistics of extremes in hydrology. Advances In Water Resources, 25, 1287-1304. doi:10.1016/S0309-1708(02)00056-8 [Valerio Lucarini, United Kingdom (of Great Britain and Northern Ireland)]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
74515	31	34	31	34	ETCCDI includes indices we don't need to keep the word indices in behind. [Moulay Driss HASNAOUI, Morocco]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
105253	31	35	31	35	It would also be appropriate to cite Li, et al., submitted (Li, C., F.W. Zwiers, X. Zhang, G. Li, Y. Sun and M. Wehner, 2019: Changes in temperature and precipitation extremes in the new-generation CMIP6 models. Submitted, Journal of Climate). [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
105255	31	35	31	38	I think the pitch for a stronger involvement of the statistical community is not appropriate here, and that is should be deleted. This casts doubt on the methods used in climatology without providing any specific basis for that criticism. [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
23119	31	35	31	38	This could well be seen as editorialising particularly as there is no supporting reference. It should, anyway, perhaps belong in the limitations to the assessment section instead? [Peter Thorne, Ireland]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
44435	31	41	32	32	In section 11.2.3 reference should also be made to Ch9 in AR5 which contained an assessment of extreme events, and advances from that assessment to AR6 should be outlined. [Jana Sillmann, Norway]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections

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29461	31	43	32	32	to observe trends and changes in extremes, more models should be considered. For instance the Met Office HadGEM2 can be used for more trends both hostorical and projections. Especially for africa where the meteorological data is very scares. Also high priority should be given to Tropical regions because most countries in this region are at high risk of extremes but having little or no data [Babatunde Oyekan, Nigeria]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections
105257	31	49	31	49	I think the statement that the “appropriate meteorological patterns can be simulated well” is a bit of an over statement given, for example, continuing challenges in simulating the frequency and duration of blocking events. The discussion on essentially the same topic just below at lines 52 and 53 provides a somewhat more nuanced assessment that seems more appropriate. [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections
13687	31	51	31	51	indicate if it is a section, table or box 11.5/11.6 [Maria Amparo Martinez Arroyo, Mexico]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections
105259	32	2	32	3	How can we compare a bias in intensity with the bias in frequency given that these are measured in different units? Some kind of normalization might help, but impact of, say, a 10% bias in intensity might be very different from a 10% bias in frequency. [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections
39877	32	10	32	10	"underestimation of multi-year drought events" -> Underestimation of what aspect? [TSU WGI, France]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections
105261	32	14	32	14	This enthusiasm for downscaling probably needs to be tempered a bit since it would be very hard to judge whether there is improvement in the deep tails, since there is a dearth of information from both observations and models exactly where it counts most. [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections
105263	32	18	32	18	Seiler et al (2017) could also be cited. They show in the modelling system they considered that dynamical downscaling reduces, but certainly does not eliminate, biases in the simulation of explosive extra tropical cyclones on the east coast of North America. Seiler, C., F.W. Zwiers, K.I. Hodges, J.F. Scinocca, 2017: How does dynamical downscaling affect model biases and future projections of explosive extratropical cyclones along North Americas Atlantic coast? Climate Dynamics, doi:10.1007/s00382-017-3634-9. [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections
35913	32	19	32	19	Additional reference for "precipitation in complex orography areas" - Lee and Hong (2014), which is examining RCM added value on extreme precipitation over the complex mountains in Korea. Lee, J.W. and Hong, S.Y., 2014. Potential for added value to downscaled climate extremes over Korea by increased resolution of a regional climate model. Theoretical and applied climatology, 117(3-4), pp.667-677. [Jiwoo Lee, United States of America]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections
13689	32	20	32	20	change 4km by 4 km [Maria Amparo Martinez Arroyo, Mexico]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections
42307	32	22	32	22	Multi-decadal simulations at regional scale can be conducted and can inform on changes in extremes. [robert vautard, France]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections
71305	32	24	32	24	very-hich -> very-high [Kenji Taniguchi, Japan]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections
62619	32	24	32	24	Hich -> high [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections
125899	32	24	32	24	Spelling error: "very-high resolution". [Trigg Talley, United States of America]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections
42465	32	24	32	24	Typo: very-hich -> very-high [Joan Bech, Spain]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections
109821	32	24	32	25	note the new UKCP18 12-member ensemble of 2.2km convection permitting climate model simulations by Kendon et al. 2019 - these are 100 years long as well 1980-2080. So this is starting to happen. Also the CORDEX-FPS multi-model simulations over Europe with CPMs are doing comparisons now. [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections
66375	32	24	32	32	This one way judgment toward negative direction of the convection permitting and dynamical downscaling has to be amended since this is not the place to discuss the method and here the many papers that show added values on extreme by using dynamical downscaling are not acknowledged. They refer to only two examples well know and documented in literature but all that it is written in the Atlas and CH10 and CH12 going in the opposite positive direction is ignored. See for example Atlas section 5.6.3. [Erika Coppola, Italy]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections
42309	32	27	32	28	This is not true for all models. A few have changing aerosols [robert vautard, France]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections
105265	32	28	32	29	Is this true of land surface models in all RCMs? [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections
105267	32	30	32	30	“are likely to explain” sounds over-confident to me. Suggest replacing this with “may have contributed to” so as not to rule out other explanations. [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections
42311	32	31	32	32	There are also other processes that can come into play which remain largely unexplored, such as difference in cloudiness and precipitation with higher-resolution models (see Kotlarski et al 2014, Vautard et al 2020), which can also play a role; I do not think we can easily attribute currently the difference between RCM and GCM response. [robert vautard, France]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed. Details of model evaluation are provided in all other subsequent sections
102535	32	35	32	54	Better wording than "surprises" ? Entire paragraph. [Philippe Tulkens, Belgium]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
113575	32	37	32	37	low confidence to' add 'the occurrence of' [Diego Miralles, Belgium]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.

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45577	32	37	32	38	Does the word “surprise” really convey the right message here? It may be worth considering “unpredictable” or introducing the black/green swan terminology, but really these events won't be surprises, and won't be unforeseeable either, right? Either way surprises sounds a bit careless [Laura Suarez-Gutierrez, Germany]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
39275	32	37	32	50	May I refer you to Chapter 6 (Extremes, Abrupt Changes and Managing Risks) of SROCC. There is an entire chapter that should have updates of the SREX. [Lourdes Tibig, Philippines]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
32935	32	37	32	50	risk is also defined as combination of probability of the event and associated losses [Tomasz Walczykiewicz, Poland]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
45579	32	37	33	20	The use of large ensembles to try to simulate very low-probability events should be discussed here. For example in Suarez-Gutierrez et al. 2020b (https://link.springer.com/article/10.1007/s00382-020-05263-w) evaluates once in hundreds of years heat extreme events based on MPI-GE. [Laura Suarez-Gutierrez, Germany]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
13691	32	40	32	40	change SR15 by SR1.5 [Maria Amparo Martinez Arroyo, Mexico]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
11669	32	42	32	42	“low” should all be in italics [Amy East, United States of America]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
113577	32	43	32	44	Not sure why someone would think that low confidence implies affirming they will occur. [Diego Miralles, Belgium]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
113579	32	44	32	44	the poor state of knowledge' for 'a poor state of knowledge'. Imagine how the former can be interpreted by someone with bad intentions… [Diego Miralles, Belgium]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
44393	32	45	32	46	remove "considering that risk is equal to the probability of an outcome times the impact of that outcome". This part can be misinterpreted that risk can be calculated by a simple equation. In the risk definition no such statement can be found. [Jana Sillmann, Norway]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
113581	32	45	32	46	I may not recall right but I think Ch1 talks about risk as a function of hazard, exposure and vulnerability. That sounds like probabilty is not comprised, just impact. My lack of knowledge maybe. [Diego Miralles, Belgium]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
39277	32	52	33	10	It is being suggested that these "surprises" be clearly defined, including "grey swans"or grey-swan events". Do these qualify as abrupt changes? [Lourdes Tibig, Philippines]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
105967	32	53	33	10	McCollum, et al. (2020) offers a taxonomy of extremes that could help with clarifying the framing here: transient extreme events, disruptive drivers, and unexpected outcomes. Transient extreme events are temporary events that are “anticipated but not necessarily well planned for,” such as an extreme storm that tests record windspeeds. These are events that exceed our expectations of probable weather patterns, but are still within a range that we’d consider reasonably possible — akin to "grey-swan events." Disruptive drivers are trends that enable events “beyond common perceptions of a probable future.” These aren’t extreme risks themselves, but rather gradual risks like sea-level rise which unexpectedly push what would normally be a transient extreme event out beyond probable expectations of weather patterns, to the edges of what we’d consider possible. And finally, unexpected outcomes: extreme events which fall outside the range of what we’d consider possible. McCollum, et al. describe these events as “diverging so fundamentally from the status quo, they could push society to states where it has never been, or ever imagined being" — akin to "black-swan events.” McCollum, David L., Ajay Gambhir, Joeri Rogelj, and Charlie Wilson. “Energy Modellers Should Explore Extremes More Systematically in Scenarios.” Nature Energy 5, no. 2 (February 2020): 104–7. https://doi.org/10.1038/s41560-020-0555-3. [Sohum Pawar, United States of America]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
102537	32	54	33	1	This statement needs examples [Philippe Tulkens, Belgium]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
32937	33	6	33	8	statistical flood scenarios are examples [Tomasz Walczykiewicz, Poland]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
105269	33	7	33	7	It would be useful to explain the distinction between “grey swan” and “black swan” events. These are concepts that apparently come from the world of financial risk analysis, where a “grey swan” is a very rare extreme event that is possible and known, while a “black swan” is impossible to “predict” (where I think “predict” would imply an event so unusual that even the possibility could not be anticipated). The current pandemic is presumably a grey swan event, as are possibly the events listed at lines 25-31. The story line approach, in its various guises, presumably also deals with grey swan events (we study them because they could plausibly occur given our state of knowledge). I’m not sure if we would know how to potential black swan events (perhaps things like nuclear winter, an asteroid strike, complete social breakdown leading to unconstrained emissions, an unimaginably destructive pathogen that impacts virtually all crop production and massively disrupts the natural carbon cycle ….). [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
44433	33	11	33	13	"nudging physical climate models into an extreme" is not the only way to create a non-probabilistic, physically self-consistent storyline. The references cited also don't necessarily include the nudging part. Alternatives include finding the respective extreme (i.e. conditioned) in climate simulations (e.g. Schaller et al. 2020 now accepted in WACE). so the sentence should be phrase differently. [Jana Sillmann, Norway]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
74517	33	14	33	15	for ref. Wehrli et al., submitted to check if it isn't published in between. [Moulay Driss HASNAOUI, Morocco]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
85071	33	15	33	15	Comment provided by Stacey New: An example of scientific research of these extreme events could be these studies which use the UNSEEN method: 1. Risk of coincident maize yield shocks in USA and China is 6% - Kent et al. (2017), Using climate model simulations to assess the current climate risk to maize production 2. Chance of experiencing unprecedented drought is 5% is China – Kent et al. (2019), Maize Drought Hazard in the Northeast Farming Region of China: Unprecedented Events in the Current Climate. 3. In south east England there is a 7% chance of exceeding the current rainfall record in at least one month in any given winter - Thompson et al. (2017)High risk of unprecedented UK rainfall in the current climate. [Stacey New, United Kingdom (of Great Britain and Northern Ireland)]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
42467	33	15	33	15	Typo: submitted;Hazeleger -> submitted; Hazeleger [Joan Bech, Spain]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.

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105271	33	17	33	18	The chances at any one location are small, but as the text goes onto discuss, the chance of such an event occurring somewhere could be larger (provided the scale of spatial dependence is small enough that spatial variation can to some extent be interchanged with temporal variation). [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
62885	33	17	33	24	Rare events are captured in documentary data such as nile flood events for which high-stands are documented over millennia or the 1540 Megadrought for which documents report low river and lake stands, fires across europe and more (see Wetter et al. (2014). The importance of such data to better understand occurrence and scale of rare events in the future should be mentioned here as they give important insights into frequency, magnitude and societal impacts of rare events that are not covered within the observational period, see e.g.: Wetter, O., Pfister, C., Werner, J. P., Zorita, E., Wagner, S., Seneviratne, S. I., et al. (2014). The year-long unprecedented European heat and drought of 1540 - a worst case. Clim. Change 125, 349–363. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
34955	33	17	33	31	The SOD lists “grey swan” weather events (such as Hurricane Harvey in 2017, Queensland Floods in 2010/2011 and the Australian fires of 2019/2020), implying these to be related to climate change. Objective analysis of historical incidences over recent decades and centuries of rainfall, floods, droughts, hurricanes, tornados, heat-waves, forest fires and coral bleaching reveal surprisingly little evidence of any upward trends, even a decline is some cases. See general comment #12 above. [Jim O'Brien, Ireland]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
51599	33	21	33	31	This list of events should have clear references, and it should be made clear in the text what makes these grey swans (I've never heard this phrase before) [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
11671	33	25	33	25	spell out “Texas” rather than TX, for international readers who won’t be familiar with this abbreviation [Amy East, United States of America]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
42959	33	25	33	31	Post-tropical cyclones that reach with strong intensity higher latitudes, like Ophelia and Sandy, could also be considered as grey-swan examples [Rein Haarsma, Netherlands]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
82761	33	26	33	26	While they were certainly very significant, it's arguable whether the 2010-11 Queensland floods are appropriately in this list, as (at least on a large scale) they could not be considered far outside the range of historical experience (compared, for example, with 1974). A better example may be the January-February 2019 Townsville region floods, where 10-day rainfalls exceeded 2000mm, something never previously observed in the region but with two precedents on other parts of the Queensland coast. There's a report documenting these at http://www.bom.gov.au/climate/current/statements/scs69.pdf. [Blair Trewin, Australia]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
74519	33	28	33	28	for tropical cyclone Idai in Mozambique we need to add the year its of course "March 2019" [Moulay Driss HASNAOUI, Morocco]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
15153	33	29			The major fire years in Californa were 2017 and 2018 nor 2018 and 2019 as stated [John Abatzoglou, United States of America]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
74521	33	30	33	30	for ref. Vautard et al., submitted to check if it isn't published in between. [Moulay Driss HASNAOUI, Morocco]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
113583	33	31	33	31	The Millenium Drought and the Russian Mega-heatwave deserve to enter this hall of fame. [Diego Miralles, Belgium]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
62887	33	32	33	32	The fires in Portugal 2017 (e.g. Osmont et al. in review) were another never seen event in Europe caused by drought that could be mentioned here, see Osmont, D., Brugger, S., Gilgen, A., Weber, H., Sigl, M., Modini, R. L., Schwörer, C., Tinner, W., Wunderle, S., and Schwikowski, M.: Tracing devastating fires in Portugal to a snow archive in the Swiss Alps: a case study, The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-58, in review, 2020. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
105273	33	33	33	34	I don’t think I really buy that non-stationary would necessarily lead to surprises. From a physical perspective, non-stationarity could be associated with the occurrence of tipping points, which are largely unanticipated. On the other hand, the point that something that is rare today could become common in the future is far from new and is well understood – with quite a bit of literature (not just the recent papers listed here) making that point in different ways. In almost all cases that have been studied, I would think that the projected change in frequency is understood and anticipated, and thus not a surprise. [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
39279	33	33	33	34	We now live in a non-stationary climate-only now? [Lourdes Tibig, Philippines]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
90849	33	33			Add glossary for "non-stationery climate" [Vivien How, Malaysia]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
62623	33	44	33	44	AR6 is mostly focussed on CMIP6, however, describing future scenarios in terms of RCP is not appropriate. RCP8.5 should be “SSP5” here and elsewhere in the chapter. All RCPs should be represented in terms of SSPs. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
105275	33	47	33	47	This subsection is a generally nice exposition, and does allude to some of the criticism and limitations of event attribution, but I don’t think it takes a sufficiently hard look to serve as an assessment at the level of an IPCC report. In particular, I think it should address questions about the reliability of event attribution, in the technical sense of that word. While questions can focus on either frequency or intensity, most work focuses on the former and indeed, it is the former that is best supported by a theoretical framework that underpins the making of causal inferences (I’m talking about the foundational work of Alexis Hannart and Judea Pearl, which is not even cited in this chapter). With regard to inferences about frequency, a key question is whether our estimates of event probability are reliable (that is, do we know whether our estimated probabilities are reasonable estimates of actual probabilities – are our estimates reliable in the sense that is required of weather forecasting systems that make probabilistic weather forecasts? See Murphy and Winkler, 1984, J. Amer. Statist. Assoc., doi:10.2307/2288395). There is a little bit of work that deals with this (done as part of the EUCLEIA project), but I think this is still largely an open question. The question is particularly concerning for the counterfactual probability – which is critical because of its position in the denominator of the probability (or risk) ratio – since there is very often no verifying information. That uncertainty must surely limit confidence in event attribution results. [Francis Zwiers, Canada]	Considered. We have substantially rewritten this section. The introduction to the methods is now only in Cross-Working-Group-Box 1, located in Chapter 1 of WGI report, while the section in this chapter focusses on the limitations of the different approaches to event attribution including, as suggested, a discussion on reliability.
125901	33	47	36	7	Is selection bias a factor that should be addressed here? Rare events tend to get all the attention, but rare events will occur somewhere, sometime in a stationary climate system. Perhaps the methods discussed are not vulnerable to this effect but, even if they aren't, it would be good to raise then dismiss the idea, so that the reader is not left wondering. [Trigg Talley, United States of America]	Considered. This is now addressed in summary statement.
51597	33	49	33	50	"… causes for given features of the climate system (e.g., trends, single extreme events)". Attribution aims to identify causes for changes in the climate, rather than features. We do not try to assess if climate change is the cause of an extreme, but rather how climate change may have modified its characteristics (likelihood, intensity etc). Suggest this is amended to capture this. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
110705	33	49	36	7	Inherent to event attribution is the question of whether the models used are able to appropriately represent the relevant global models of variability and states that underly key events where dynamics play a key role, especially persistent circulation features (as, for exampl,e the Cape Town drought). This section seems short on assessing this issue. [Bruce HEWITSON, South Africa]	Considered. It is indeed true that model evaluation is a key aspect of every event attribution study (we state in this section that it is of the "utmost importance"), but the same holds for projections. Therefore we refer to the section on model evaluation.
117083	33		33		What about local knowledge, oral traditions, historical sources ? I have seen literature on past droughts based on local knowledge in several places incl in Africa. [Valerie Masson-Delmotte, France]	This section has been substantially modified from SOD to FGD and the relevant subsection was removed.
105277	34	6	34	8	Some of those “more recent studies” date back to at least as early as 2011 (e.g., Zwiers et al., 2011, J. Climate, doi: 10.1175/2010JCLI3908.1), which is something that I think should be noted. [Francis Zwiers, Canada]	Taken into account. Sentence has been rephrased
90851	34	6			Cross citing "Non-statioary extreme value analysis in a changing climate" by Cheng et al., 2014 [Vivien How, Malaysia]	Considered. This paper mainly introduces a statistical software package that implements non-stationary extreme value theory. It does not address attribution question and is not suitable for cross citing here.
105279	34	7	34	7	It is not at all obvious that the use of non-stationary extreme value distributions is the key to allowing “detailed detection and attribution of regional trends”. More efficient statistical methods help a bit (recall that the early D&A literature focused heavily on “optimization”, which was intended to increase detection power), but often only marginally. The fundamental constraint remains the lower signal to noise ratio at smaller scales – something that is very hard to overcome. [Francis Zwiers, Canada]	Considered. Wording "more appropriate" is removed.
37703	34	10	36	7	This part comprehensively discusses event attribution approach, which is new and important. It is desirable to make it easier for readers to find this place, e.g., by making it a subsection. [Masahide Kimoto, Japan]	Considered. But event attribution as methodology was assessed in AR5 (Chapter 10). Separating event attribution as a subsection leaves the section unbalanced in length.
38409	34	13	34	13	The reference "event (National Academies of Sciences, Engineering, 2016)" needs to correct. The reference of the article is "National Academies of Sciences, Engineering, and Medicine. 2016. Attribution of Extreme Weather Events in the Context of Climate Change. Washington, DC: The National Academies Press. https://doi.org/10.17226/21852". [Mansour Almazroui, Saudi Arabia]	Taken into account, proper form of reference is used in the FGD version.
105281	34	22	34	22	These approaches are not distinct. As was illustrated very nicely in Figure 4 of Otto et al., 2012 (doi:10.1029/2011gl050422), the question of intensity versus frequency is one of whether to compare the quantiles of counterfactual and factual distributions for a given frequency or to compare frequencies for a given quantile. The same two estimated probability distributions are used to answer both questions. [Francis Zwiers, Canada]	Taken into account. Sentence has been rephrased
51603	34	25	34	27	Important work on the effect of framing on attribution assessments was published recently that the report could cite here: Christidis et al. 2018 (Journal of Climate, vol 31, 4827-4845). [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Considered. This paper is cited.
105283	34	29	34	29	I think an assessment is needed here! What characterizes these “key methodologies”, how does this chapter assess them, and what are the implications for previous work not using these methodologies? [Francis Zwiers, Canada]	Considered. This formulation was slightly misleading, it was not intended to suggest that studies using a different methodology could not be included in the assessment. The subsection has been rewritten substantially, focussing explicitly on the limitations of all methods and concluding that they provide important, different lines of evidence for the regional assessment presented in section 11.9.
13693	34	30	34	30	change submitted)as by submitted) as [Maria Amparo Martinez Arroyo, Mexico]	Noted. Typos corrected.
6839	34	30	34	30	separate parenthesis to the word that follows: Philip et al., submitted) as [Constantinos Cartalis, Greece]	Noted. Typos corrected.
42469	34	30	34	30	Typo: submitted)as -> submitted) as [Joan Bech, Spain]	Noted. Typos corrected.
51601	34	32	34	33	The report could provide references to optimal fingerprinting studies by Hadley Centre scientists, who did pioneering work in this field (including the first event attribution study of the European heatwave of 2003). Some relevant references are: Stott et al. 2004 (Nature, vol 432, 610-614); Christidis et al. 2015 (Nature Clim Change, vol 5, 46-50); Christidis et al. 2015 (Climate Dyn, vol 45, 1547-1564). [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Considered. Only post-AR5 papers are assessed.
42471	34	32	51	32	Typo (remove extra blank space, typo found 9 times): (e.g., -> (e.g., [Joan Bech, Spain]	Editorial, the final draft will undergo professional copy-editing prior to publication
51605	34	35	34	37	Here (and also L23) the report makes a distinction between likelihood- and magnitude- approaches, which is not a common, or widely accepted classification in the literature. The majority of published studies assess likelihood changes, as also reflected by the fact that the report itself provides no references for the alternative magnitude-approaches. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sentence has been rephrased
102539	34	41	34	41	Defince "coupled" [Philippe Tulkens, Belgium]	Rejected. Coupled climate models are defined in chapter 1 & the glossary.
51607	34	44	34	45	Recent relevant work with AMIP models could be cited here too. For example: Ciavarella et al. 2018 (Weather Clim. Extremes, vol 20, 9-32). [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	noted
13695	34	46	34	46	remove the parenthesis at (Pall [Maria Amparo Martinez Arroyo, Mexico]	Editorial, the final draft will undergo professional copy-editing prior to publication
42473	34	46	34	46	Typo: (Pall -> Pall [Joan Bech, Spain]	Editorial, the final draft will undergo professional copy-editing prior to publication
42313	34	46	34	47	Analogue approachs can considered in this category as we seek responses given analogue situation of circulation pattern. Cattiaux et al 2011 can be cited for instance, but there are several other papers [robert vautard, France]	Considered. Only post-AR5 papers are assessed.
71445	34	47	35	1	This discussion is somewhat misleading, a couple of issues need to mentioned: further reasons for using conditional attribution are (1) to eliminate internal variability by rephrasing the attribution question into (how would this event have unfolded in a cooler climate), (2) to manage errors in the representation of the large-scale circulation, (3) to enable the use of very-high resolution simulations to properly simulate local extremes and (4) to separate the attribution of changes in severity (conditional) and frequency (unconditional), such that different (purpose specific) model types can used to address each part of the question. When combining both approaches, better statements also including frequency statements can be drawn. This is at least partly discussed in Trenberth et al NCC 2015, Shepherd, CCCR, 2016 and Shepherd et al., Clim Change, 2018 (all cited already in the chapter). I am happy to discuss this issue in the following months. We are currently preparing a paper for a regional attribution case where we consider different types of models, but I am afraid it will be too late for the AR. [Douglas Maraun, Austria]	Considered. Some of these are discussed.
51609	34	54	35	1	"… thus precluding any attribution statements about the change in the frequency". Conditioning does not preclude any attribution statements. The statements are simply conditioned on certain factors. For example, a conditioned statement on ENSO could be that in La Niña years anthropogenic influence increases the likelihood of floods in Australia by a factor of X. This is still a useful and valid attribution statement. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sentence has been rephrased

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24095	35	3	35	4	This statement is problematic as it is not supported by the literature as indicated by the fact the sentence comes with no citations. Framing is not an uncertainty - rather an issue of interpretation, and the definition of the event is not an uncertainty either in the commonly assumed notion of scientific uncertainty - ie if you set out to measure something (x) then the uncertainty in that measurement of x does not include the possibility somebody thinks you should have measured y instead. Rather this uncertainty associated with the "definition of the event" is associated with the uncertainty in how best to characterise an "event" - if the "event" is defined as the maximum 3-day temperature in Toulouse say, then there is no uncertainty in the calculation resulting from that definition. So this sentence is trying to allude to the concept that the "event" is some conceptual thing that different definitions of "events" as thresholds of climate variables are seeking to capture appropriately, This incertitude speaks to a continuing research need to elucidate what we mean by the events that we are trying to attribute and how best to capture them- eg meteorological events a weather forecaster would understand or eg humanitarian weather related disasters much closer to impacts. The problem I have with the framing this chapter has adopted on event definition and event attribution uncertainty is it is too closely tied to current attempts to define pragmatic protocols for operational attribution rather than a comprehensive assessment of the current understanding in event attribution and a full appreciation of the remaining conceptual difficulties. Thus Figure 11.4 is proposed as the IPCC schematic on event attribution uncertainty when there is not currently a widely accepted consensus on this and when it seems it contains important flaws if applied too literally, ie without other considerations (fidelity of a single model vs errors in multi-model ensembles for example). [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Considered. Some texts are rephrased and Fig 11.4 is not included in FGD.
51613	35	3	35	4	This statement is problematic as it is not supported by the literature as indicated by the fact the sentence comes with no citations. Framing is not an uncertainty - rather an issue of interpretation, and the definition of the event is not an uncertainty either in the commonly assumed notion of scientific uncertainty - ie if you set out to measure something (x) then the uncertainty in that measurement of x does not include the possibility somebody thinks you should have measured y instead. Rather this uncertainty associated with the "definition of the event" is associated with the uncertainty in how best to characterise an "event" - if the "event" is defined as the maximum 3-day temperature in Toulouse say, then there is no uncertainty in the calculation resulting from that definition. So this sentence is trying to allude to the concept that the "event" is some conceptual thing that different definitions of "events" as thresholds of climate variables are seeking to capture appropriately, This incertitude speaks to a continuing research need to elucidate what we mean by the events that we are trying to attribute and how best to capture them- eg meteorological events a weather forecaster would understand or eg humanitarian weather related disasters much closer to impacts. The problem I have with the framing this chapter has adopted on event definition and event attribution uncertainty is it is too closely tied to current attempts to define pragmatic protocols for operational attribution rather than a comprehensive assessment of the current understanding in event attribution and a full appreciation of the remaining conceptual difficulties. Thus Figure 11.4 is proposed as the IPCC schematic on event attribution uncertainty when there is not currently a widely accepted consensus on this and when it seems it contains important flaws if applied too literally, ie without other considerations (fidelity of a single model vs errors in multi-model ensembles for example). [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Considered. Some texts are rephrased and Fig 11.4 is not included in FGD.
51617	35	3	35	4	"The key sources of uncertainty in event attribution are… the uncertainty resulting from the framing … aproach". I do not view framing as a source of uncertainty. Framing simply states what the attribution question is. As long as one is clear about what question they try to answer, there is no uncertainty. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Considered. Some texts are rephrased.
104585	35	10	35	10	A recent study on the attribution uncertainties from different modelling approaches should be cited here. Text would be added before 'In general, ...': 'Compared with a atmosphere-ocean general circulation model, Zhou et al., (2018) found that a fully coupled model tends to have larger responses of precipitation extremes to GHGs-warming and show larger uncertainties in the modes of internal variability.' Reference: Zhou, C., K. Wang, and D. Qi, 2018: Attribution of the July 2016 extreme precipitation event over China's Wuhan. Bull. Am. Meteorol. Soc., 99, 107-112. [Chunlüe Zhou, United States of America]	Noted. The suggested text is very specific description of the paper and is not used.
71447	35	13	35	35	This paragraph is basically silent about a major issue in event attribution and one of the reasons why conditional attribution has been proposed (the paper by Trenberth is cited, but the point is not explained): current climate models have severe shortcomings in realistically simulating, e.g., the atmospheric circulation states underlying persistent drought such as the Northern/Central European drought in 2018. Thus current approaches use as a fallback the attribution of, e.g,. the warmest 3-day period of the event, which does not capture the actual nature of the event at all. In other words: current climate models are not fit for event attribution in many cases where atmospheric dynamics play a crucial role. There is plenty of relevant literature on this issue from a wider angle, such as Dawson et al 2012 (doi:10.1029/2012GL053284), Davini and D'Andrea, 2016 (DOI: 10.1175/JCLI-D-16-0242.1), Woollings et al. 2018 (https://doi.org/10.1007/s40641-018-0108-z), Sheperd, 2014 (DOI: 10.1038/NGEO2253) and many others. Note also that this discussion here is currently inconsistent with Chapter 10, where we highlight deficiencies of GCMs in modelling the large-scale circulation relevant for regional weather and climate (10.3.3.4). A link to that Section should be added here. I am happy to discuss this issue. [Douglas Maraun, Austria]	Considered. This subsection has been rewritten substantially, focussing explicitly on the limitations of all methods, including an assessment of the models' shortcomings. Appropriate section in Chapter 10 is also referred now.
62761	35	14			Very extreme event should be described. As previously an extreme event is decribed as meeting a certain threshold, but there is no threshold stated beyond this that would describe a very extreme event. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. Sentence is rephrased.
23121	35	18	35	18	of event attribution [Peter Thorne, Ireland]	Editorial, the final draft will undergo professional copy-editing prior to publication
42475	35	22	35	22	Typo: 2017;Philip -> 2017; Philip [Joan Bech, Spain]	Editorial, the final draft will undergo professional copy-editing prior to publication
43327	35	22		23	Read " (e.g., droughts: Hauser et al. 2017;Philip et al. 2018; Otto et al. 2018a, and floods: Philip et al. 2019), " rather than " e.g., droughts: Hauser et al. 2017;Philip et al. 2018; Otto et al. 2018a, floods: Philip et al. 2019, [Cyriaque Rufin Nguimalet, Central African Republic]	Editorial, the final draft will undergo professional copy-editing prior to publication
42315	35	23	35	24	Can mention storms eg Vautard et al (2019); Stagnations also [robert vautard, France]	Noted, this comment is incomplete.
125903	35	23			Reference for flood attribution: Villarini, G., W. Zhang, F. Quintero, W.F. Krajewski, and G.A. Vecchi, Attribution of the impacts of the 2008 flooding in Cedar Rapids (Iowa) to anthropogenic forcing, submitted to Nature Communication, 2019. [Trigg Talley, United States of America]	Noted. This paper was not available at the time of revising the text
109365	35	29	35	29	Change "risk ratio" to "probability ratio". [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Considered. Probability ratio is used.
40903	35	30	35	30	The chapter mentions 'human influence' a lot. Might be useful to clarify that this refers to human influence on climate specifically, through emissions and land use change, rather than on exposure and vulnerability. [TSU WGI, France]	Rejected. The term is defined in the glossary.
38411	35	32	35	32	The reference (National Academies of Sciences, Engineering, 2016)" needs to correct. The reference of the article is "National Academies of Sciences, Engineering, and Medicine. 2016. Attribution of Extreme Weather Events in the Context of Climate Change. Washington, DC: The National Academies Press. https://doi.org/10.17226/21852". [Mansour Almazroui, Saudi Arabia]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
62625	35	32	35	32	Stating 1 C climate change is misleading. It should be 1 C global warming. Global warming and climate change are not the same. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
105285	35	32	35	35	From a methodological perspective, large (perhaps unbounded) probability ratios are not the fundamental issue. The key point that is missed here (that readers will likely identify it as an unassessed issue) concerns the uncertainty (and reliability) of estimated probabilities under pre-industrial conditions. See also my comment for page 33, line 47. [Francis Zwiers, Canada]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
109367	35	34	35	35	It is not clear why this is a particular challenge. Please explain. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
62889	35	34	35	35	This sentences is grammatically incomplete and the meaning unclear. Consider clarifying: "This poses particular challenges for attribution science as the calculated probability ratios become infinite." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
10927	35	37	35	38	I would challenge the logic in this sentence. "Event attribution" (11.3.4) tells us about how how much anthropogenic influences changed the frequency/magnitude of a specific event, given assumptions of anthropogenic influences on that type of event. This analysis of a specific event cannot be be said to then provide evidence for how anthropogenic influences effect that type of event. That would be circular reasoning. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
62763	35	37			"Event attribution studies now provide" is preferable wording. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
23123	35	38	35	40	But earlier text had spoken to the emergence of best-practice methodologies even if very recent? [Peter Thorne, Ireland]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
51615	35	38	36	7	I am sceptical about the proposed assessment of the "quality of evidence" following the way the authors recommend here. A single study with good quality, well-evaluated, data would still provide high-quality evidence, even if other studies on the same topic are of poorer quality. I think the authors here prescribe ways that reflect protocols used to synthesise information for the purposes of an operational attribution service. While such protocols are indeed necessary for a service, I would be wary of introducing them into scientific research practice, as this could risk penalising really useful research work, simply because it does not fully comply to a protocol. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
109369	35	39	35	40	Wich "assessment process" is being referred to, please clarify, and there is a missing end of the sentence. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
24093	35	42	36	7	This decision tree of Otto et al is just one suggestion for assessing confidence in attribution studies yet is at risk of being elevated here to the official IPCC prescription despite the contradiction with the lines just before (lines 38,39 - "no best-practice methodologies exist yet") and despite the fact that strength of confidence needs to come from mulitple lines of evidence. But here we have just the one line of evidence given by the use of this flow chart from an as yet unpbulished paper. It is not fundamentally the case that a result from only one model is necessarily more scientifically robust than a result from multiple models if for example the one model is of high quality (eg in terms of resolution, processes) and has been shown to accurately simulate the type of event being attributed and its past frequency etc versus multiple poor models that all fail to capture the processes involved and the statistics of the event. So ithis schematic is flawed if used as an overall IPCC procedure (even though it may have merit in an operational attribution context if carefully applied). This flow chart is too much of a hostage to fortune and should be deleted. [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
51611	35	42	36	7	This decision tree of Otto et al is just one suggestion for assessing confidence in attribution studies yet is at risk of being elevated here to the official IPCC prescription despite the contradiction with the lines just before (lines 38,39 - "no best-practice methodologies exist yet") and despite the fact that strength of confidence needs to come from mulitple lines of evidence. But here we have just the one line of evidence given by the use of this flow chart from an as yet unpublished paper. It is not fundamentally the case that a result from only one model is necessarily more scientifically robust than a result from multiple models if for example the one model is of high quality (eg in terms of resolution, processes) and has been shown to accurately simulate the type of event being attributed and its past frequency etc versus multiple poor models that all fail to capture the processes involved and the statistics of the event. So this schematic is flawed if used as an overall IPCC procedure (even though it may have merit in an operational attribution context if carefully applied). Suggest that this flow chart is deleted. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	This section has been substantially modified from SOD to FGD and the relevant sentence/fig was removed.
62891	35	45	35	45	Remove ", a" in reference of figure caption to be consistent with the same figure caption of Fig 11.4 on page 242. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - figure removed
42477	35	46	35	46	Typo: assing -> assessing ? Please check. [Joan Bech, Spain]	Not applicable - figure removed
43329	35	46		47	when "assing" the quality of evidence or when "assessing" the quality of evidence? [Cyriaque Rufin Nguimalet, Central African Republic]	Not applicable - figure removed
109371	35	52	35	53	This sentence is not clear. Is there meant to be some text before this to provide context? Please clarify. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	This section has been substantially modified from SOD to FGD and the relevant sentence/fig was removed.
117085	35		35		please update the level of warming (here 1°C) consistent with Ch 2 [Valerie Masson-Delmotte, France]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
125905	36	3	36	5	For the criteria for high quality evidence, the authors should also consider whether there is strong signal-to-noise ratio in detection/attribution studies (i.e., is the signal clearly detectable or not), and the level of physical/process understanding of the anthropogenic influence on the phenomenon being assessed. [Trigg Talley, United States of America]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
102541	36	5	36	5	This is understood about study design, and not results, but shouldn't a statement be made on whether the models then agree? [Philippe Tulkens, Belgium]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
11673	36	6	36	6	“data are poor”, plural [Amy East, United States of America]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
11675	36	7	36	36	change “dependency” to “dependence” (in lines 7 and 36) [Amy East, United States of America]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
45581	36	10	37	20	The results in Suarez-Gutierrez et al. 2020b (https://link.springer.com/article/10.1007/s00382-020-05263-w) should be included in the discussion of changing extremes for different levels of warming. In particular, there is one aspect that continues to be somewhat ignored in this discussion, regarding the irreducible range of events that could be possible albeit unlikely for each warming level. This aspect is discussed in Suarez-Gutierrez et al. 2020b (https://link.springer.com/article/10.1007/s00382-020-05263-w) and more in depth evaliated in Suarez-Gutierrez et al. 2018 (https://iopscience.iop.org/article/10.1088/1748-9326/aaba58/meta. This potential overlap between the events that are possible in the climate conditions could lead to events characteristic of much higher warming levels occuring earlier as though, particularly in regions of high variability. This aspect must be addressed in order to craft adaptation and mitigation measures that are sufficient. [Laura Suarez-Gutierrez, Germany]	Considered. The mentioned paper is assessed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23991	36	10	38	8	Much of the chapter is framed in terms of understanding extremes at global warming levels of interest to the policy maker (rather than, for example, various time horizons in the different scenarios, which may mean very different things in terms of GMST change). For high warming levels, there is likely to be a fair degree of confidence, however low warming levels (e.g. 1.5degC) may arise in the near term (e.g. to 2040) or mid-term (to 2060). At such time horizons, multiple large ensemble (LENS) (a.k.a. grand ensembles or initial condition ensembles) studies have shown that for regional climates, internal (multi-decadal) varability can sometimes overcome GHG-related signals, a finding that is not possible using the small multi-model ensembles of CMIP. For example, mean temperature or precipitation trends expected from GHG warming may be counteracted partially or completely, yielding a trend of the opposite sign. Just one example is that of Huang et al. (2020) in Science Advances, whereby in several ensemble members the mean monsoon precipitation is shown to decrease out to 2040 instead of the expected GHG-related increase. This occurs due to the behaviour of the IPO in this case. (See DOI: 10.1126/sciadv.aay6546). If a region undergoes decadally-forced changes in temperature and precipitation then it is quite conceivable that the extremes in that region may not undergo the expected scaling according to GMST. Are there studies available for assessment that take into account large-ensembles approaches for assessing extremes at the regional level? The use of large ensembles represents one of the tools listed in Chapter 10 for distilling multiple lines of evidence for climate changes at the regional scale. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Considered. While it is useful to include assessment from both time horizons and warming level perspective, there is not space to include both in one Chapter. Chapter 11 focuses warming level but Chapter 12 does assess based on time horizons especially for the near-term and mid-term.
23125	36	10			Section is cast in terms of GMST which is at odds with the decision communicated by chapter 2 to use GSAT as the primary metric in subsequent chapters. [Peter Thorne, Ireland]	Taken into account. Section has been modified.
90853	36	10			Repeated info appear in Chapter 11.2.6 and Chapter 11.3 [Vivien How, Malaysia]	Taken into account. This section has been substantially modified from SOD to FGD.
62783	36	12	36	12	Are you refering to "variable"?. I would change "quantity" by the term mentioned before. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. We are referring to a quantity of global warming level.
113585	36	12	36	13	This sentence feels too categorical. Maybe add a reference or clarify this conclusion. [Diego Miralles, Belgium]	Taken into account. Sentence has been rephrased
113587	36	13	36	14	Repetition 'On the other hand'. Too many hands. [Diego Miralles, Belgium]	Rejected. There is only two hands
74523	36	16	36	16	to correct SR15 by SR1.5 [Moulay Driss HASNAOUI, Morocco]	Editorial, the final draft will undergo professional copy-editing prior to publication
13697	36	16	36	16	change SR15 by SR1.5 [Maria Amparo Martinez Arroyo, Mexico]	Editorial, the final draft will undergo professional copy-editing prior to publication
105287	36	18	36	18	I recognize that it's a personal thing, but I don't think the idea of temperature goals (or targets, which is also frequently used) is helpful. A goal or a target can be missed by either warming too much or too little (albeit with different consequences). I prefer talking about warming limits. [Francis Zwiers, Canada]	Taken into account. Sentence has been rephrased
23941	36	22	36	25	The statement here is reasonable unless one considers that the same warming level (or global mean radiative forcing imbalance) may have been achieved by different emissions pathways, particularly of aerosol emissions which could be very different at the regional level. For example, different SSP may consider different air quality policies at the sub-regional level, in which major nations take different approaches. See the example for India and China in Figure 1b of Samset et al. (2019) and the possibility for consistent aerosol reductions for India and China, or divergent approaches. https://doi.org/10.1038/s41561-019-0424-5 These could have quite different impacts on local temperature and precipitation. Such eventualities have been acknowledged as an issue later in the section (lines 40-42 on p36) but it is not clear that this has been reiterated or quantified for the later regional discussions. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Considered. More detailed discussion is now provided in the new Rejected. 11.
74525	36	24	36	24	for ref. Seneveratne and Haussen, submitted to check if it isn't published in between. [Moulay Driss HASNAOUI, Morocco]	Taken into account. The correct reference has been included
45683	36	27	36	27	30 mm ==>3.0 mm [Christophe Deissenberg, Luxembourg]	Noted, but comment does not appear to apply to text at this location
74527	36	28	36	28	to correct SR15 by SR1.5 [Moulay Driss HASNAOUI, Morocco]	Editorial, the final draft will undergo professional copy-editing prior to publication
51619	36	29	36	31	Is there information available for intermediate warming e.g. the very policy-relevant 3°C. If so it would be useful to include here. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Space limitation does not allow the inclusion of 3C warming level but it can be inferred from the assessments for 2C and 4C warming levels.
23127	36	30	36	30	Are these relative to 1850-1900 as a proxy for PI or true PI and are they in GMST or GSAT? [Peter Thorne, Ireland]	Considered. This is addressed in the new Rejected. 11.1
102543	36	30	36	30	"+4" deg C. is not in line with table 11.2 [Philippe Tulkens, Belgium]	Considered. The revised Table 11.2 includes 4C warming.
125907	36	31	36	34	This sentence seems to misrepresent the Paris Agreement temperature goals and interprets how to achieve the 1.5 and 2°C goals. Moreover, the sentence references an "aim of the Paris Agreement (1.5°C)" and also "a scenario overshooting the aims of the Paris Agreement (+2°C)". It is unclear what exact "aims" are being described. [Trigg Talley, United States of America]	Taken into account. It has been now clarified that 1.5°C is the lowest limit of the Paris agreement: "These encompass a scenario compatible with the lowest limit of the Paris Agreement (+1.5°C)"
102545	36	33	36	33	"+4" deg C. is not in line with table 11.2 [Philippe Tulkens, Belgium]	Considered. The revised Table 11.2 includes 4C warming.
105289	36	33	36	35	Noise is one issue, but another question is whether global mean temperature variation remains useful as a way of predicting variation in the intensity or frequency of extremes once stabilization occurs. [Francis Zwiers, Canada]	Noted. The difference in the projected changes considered here between transit and stabilized temperatures seems to be small.
1439	36	34	36	34	It is not quite correct to say that RCP2.6 is more subject to noise than high-emission scenarios, eventhough the signal-to-noise ratio is lower (merely because the signal is weaker while the rest may stay the same). [Rasmus Benestad, Norway]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
125909	36	35	36	37	This statement assumes that the information will be used for "action" such as policymaking, but there is no distinction among other types of "actionable" information such as adaptation planning, so maybe there is some syntax missing. [Trigg Talley, United States of America]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
23129	36	37	36	37	Except for storm surge and coastal flooding for which they are clearly key. [Peter Thorne, Ireland]	This section has been substantially modified from SOD to FGD and the relevant sentence was removed.
105291	36	40	36	40	Replace "In particular" with "For example". It's an example, but might not be the dominant cause of regional variation in the relation. [Francis Zwiers, Canada]	Taken into account. Sentence has been rephrased
104919	36	44	36	44	Can ETCCDI or equivalent index acronym be included where each index is referred to in the text? Is many cases it is, but should this example be TXx? [John Caesar, United Kingdom (of Great Britain and Northern Ireland)]	Considered. While it could be ideal to use one acronym for one index for the whole chapter, this can be difficult to do and may not always the best to do given the lingth of the chapter. For this reason, some index are spelled out from place to place in the chapter.
38413	36	51	36	51	Are not the terms "SR1.5" and "SR15" same? Here and at many places it is written SR1.5 while other places (page 36, Line 28; Page 37, Line 18; Page 37, Line 24 and many more places) written as SR15. Needs similar at all places. [Mansour Almazroui, Saudi Arabia]	Editorial, the final draft will undergo professional copy-editing prior to publication
105293	37	1	37	1	Replace "climate variables with large inertia" with "climate variables describing components of the climate system the large inertia". It's not the variables, per se, that have the inertia. [Francis Zwiers, Canada]	Accepted. The text has been revised as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70947	37	2	37	4	This is not true for aspects of extremes tied to midlatitude circulation, where there is quite a difference between a transient and an equilibrated level of warming: see Ceppi et al. (2018 doi: 10.1175/JCLI-D-17-0323.1) for circulation and Zappa et al. (2020 doi: 10.1073/pnas.1911015117) for precipitation and for P-E. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted. However, this sentence is about "conditions within the next decades" not conditions at equilibrium. See also more in depth assessment in the new Rejected. 11.1
10073	37	6	37	9	Clarify whether the quite temperatures of emergence are with respect to pre-industiral or current baseline. [Robert Kopp, United States of America]	Noted. The text is modified to indicate it is with regard to global warming level.
105295	37	6	37	18	It is not evident that Figure 11.6 is calculated in the same way as Figure 11.5, or how the method used for Figure 11.5 would be adapted to a multi-model analysis, in which a large part of the variation between simulations reflects difficult to quantify epistemic (structural) uncertainty. [Francis Zwiers, Canada]	Considered. Figure 11.5 is removed.
105297	37	6	37	18	Figure 11.5 is interpreted as reflecting an aspect of emergence. While this is the word that is used by the authors of the paper that described the figure and its calculation (Kirchmeier-Young et al., 2019), I don't think that they actually performed an emergence calculation. Emergence refers to the separation of a future distribution of some aspect of climate variability as compared to the historical distribution of variability, such that it is clear that the future distribution, say of the interannual variability in TXx, has a distinctly different range of variation than the historical distribution. The calculation performed by Kirchmeier-Young et al. was performed as follows. An extreme event of interest is defined on the basis of simulated variability in a large ensemble simulation for the period 1961-2010. Using this event definition, the probably of the event is estimated under counter factual and future factual conditions. In the case of CanESM2, these probabilities are estimated based on 10-year periods obtained from historical NAT and RCP8.5 large ensemble simulations; 50 ensemble members are available, so probability estimates are based on samples of 500 annual values under each forcing regime for a given 10-year window. A probability ratio (risk ratio RR) is calculated from the two probability estimates, and the sampling uncertainty of the ratio is determined via bootstrapping. A "time of emergence" is determined by testing the null hypothesis that RR=1 against the alternative hypothesis that RR>1 for different moving 10-year windows at the 5% significance level (the testing procedure is described in terms of confidence intervals, but that is simply a way to implement a test). The global mean temperature anomaly corresponding to the time when rejection begins to occur consistently from one 10-year window to the next is noted. This is the temperature plotted in Figure 11.5. Unfortunately, I don't think we can interpret this as the temperature at which emergence occurs. The difficulty is that the "power" of the test that is used depends on the size of the available ensemble, with the result that risk ratios that are only marginally larger than 1 will be become reliably detectable for large enough ensembles. In the case of TXx, the event of interest is defined as a 20-year event (annual probability of occurrence of 0.05 in the reference climate). If the probability of occurrence were 0.0476 in the NAT climate (corresponding to a 21-year event), and 0.053 in a future decade (corresponding to a 19-year event), the risk ratio would be just over 1.1. This would be reliably detectable with a large enough ensemble, and this kind of change in event frequency might be important from an actuarial perspective if you were setting the price of an insurance policy – but a change of this nature would not indicate that the distribution of TXx variability has shifted to a substantially different range. Rather, this would be a way to detect what could be quite subtle changes in the upper tail. [Francis Zwiers, Canada]	Considered. The figure is removed from Chapter 11
108909	37	6	37	32	In this whole paragraph it remains unclear how time/temperature of emergence is defined here. I find it very surprising that Rx1day is emerging at a level of warming of only 0.25°C. This seems to be inconsistent with King, A. D., M. G. Donat, E. M. Fischer, E. Hawkins, L. V. Alexander, D. J. Karoly, A. J. Dittus, S. C. Lewis, and S. E. Perkins (2015), The timing of anthropogenic emergence in simulated climate extremes, Environmental Research Letters, 10(9), 094015 (094019 pp.). I suspect that thr result is sensitive to the definition. If you compare two 20 year periods of regional average Rx1day in two climates that differ by 0.25°C it is very hard to believe that they are significantly different. At least in observations even 1°C warming is often not enough to detect a significant difference in the regional averages. I am not sure if you are averaging across different ensemble members or CMIP6 models. That would not make sense because the real world will only be following one realization. Also the definition of time of emergence should not be dependent on the number of models or initial condition members. Note that in Schleussner et al. 2017 the difference due to 0.5°C warming was only significant at global scale and not at the regional scale. [Erich Fischer, Switzerland]	Considered. Reference to Kirchmeier-Young et al. 2019 do not discuss the emergence in a way as defined traditionally and is removed.
62741	37	7	37	7	I think the definition of climate noise should be added------climate noise:" Variations in the state of the climate system that have little or no organized structure in time and/or space."(http://glossary.ametsoc.org/wiki/Climate_noise) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. But this is a common term.
13699	37	18	37	18	change SR15 by SR1.5 [Maria Amparo Martinez Arroyo, Mexico]	Editorial, the final draft will undergo professional copy-editing prior to publication
74529	37	18	37	29	to correct SR15 by SR1.5 [Moulay Driss HASNAOUI, Morocco]	Editorial, the final draft will undergo professional copy-editing prior to publication
13701	37	23	37	23	change SR15 by SR1.5 [Maria Amparo Martinez Arroyo, Mexico]	Editorial, the final draft will undergo professional copy-editing prior to publication
42479	37	36	37	36	Typo: Regading -> Regarding [Joan Bech, Spain]	Editorial, the final draft will undergo professional copy-editing prior to publication
109373	37	44	37	44	Change "risk ratio" to "probability ratio". [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - figure removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19479	38	11	52	42	Section 11.3 The section examines mechanisms, drivers, observed trends and projections of temperature extremes. The temperature related variables are mostly those from ETCCDI which independently account for three aspects: frequency, magnitude and duration, but the section is missing other relevant characteristics such as areal extent and seasonal features (e.g. timing of occurrence). It has been shown (Perkins 2015) that ETCCDI indices provide a limited description of heatwave episodes, missing aggregated effects of duration and intensity (e.g. Heat Wave Magnitude Intensity, Russo et al. 2015) that are relevant for associated impacts, or areal extent, a determining aspect of emerging events such as megaheatwaves. Several papers account for areal extent (e.g. Stefanon et al. 2012) and provide indices integrating different heatwaves characteristics such as duration, intensity and extension (Sánchez-Benitez et al. 2020). This should be acknowledged in the text. On the other hand, an earlier onset of the summer season in Europe has been reported in observations (ranging between -5 to -10 days decade-1 from 1979 to 2012; Peña-Ortiz et al., 2015). This trend is largely due to increasing greenhouse gases concentrations (Park et al., 2018), but natural sources of variability also play a role (Peña-Ortiz et al, 2015) and it is expected to continue in the future (Cassou and Cattiaux, 2016). It should be expected to involve an advance in the calendar day of heatwaves occurrence when compared to the historical record, and several recent cases have been reported (Morabito et al., 2017; Sánchez-Benítez et al, 2018, Sousa et al 2019). This has relevant implications for health impacts, since the first heatwave of the year usually has an increased mortality (the so-called 'harvesting effect'), and socio-economic effects (by occurring during working time of the year), therefore implying an earlier implementation of early warning systems. This issue is not included in the section at all. Cassou, C., Cattiaux, J., 2016. Disruption of the European climate seasonal clock in a warming world. Nat. Clim. Chang. 6, 589–594. https://doi.org/10.1038/nclimate2969 Morabito, M., Crisci, A., Messeri, A., Messeri, G., Betti, G., Orlandini, S., Raschi, A. Maracchi, G., 2017. Increasing Heatwave Hazards in the Southeastern European Union Capitals. Atmosphere 8, 115. https://doi.org/10.3390/atmos8070115 Peña-Ortiz, C., Barriopedro, D., Garcia-Herrera, R. (2015): Multidecadal variability of the summer length in Europe. Journal of Climate, doi: http://dx.doi.org/10.1175/JCLI-D-14-00429.1 Perkins, S.E. (2015): A review on the scientific understanding of heatwaves-Their measurement, driving mechanisms, and changes at the global scale. Atmos. Res. 164–165, 242–267. https://doi.org/10.1016/j.atmosres.2015.05.014 Sánchez-Benitez, A., García-Herrera R., Barriopedro D., Sousa P.M., Trigo R.M. (2018): June 2017: The Earliest European Summer Mega-heatwave of Reanalysis Period. Geophys. Res. Lett., 45, 1-8, doi:10.1002/2018GL077253. Sánchez-Benitez, A., Barriopedro, D., García-Herrera, R. (2020): Tracking Iberian heatwaves from a new perspective. Weather and climate extremes, doi:10.1016/j.wace.2019.100238. Sousa, P.M., Barriopedro, D., Ramos, A.M., García-Herrera, R., Espírito-Santo, F., Trigo, R.M. (2019): Saharan air intrusions as a relevant mechanism for Iberian heatwaves: The record breaking events of August 2018 and June 2019. Weather Clim. Extrem. 26, 100224.	Taken into account. Parts of the comments are accepted and text revised
14629	38	11	109	40	All of the regional projections in Ch 11 (supported by 6 excellent tables Table 11.4-11.9) are by scenario (unless a scenario to GWL mapping has been done). Only the global scale projections are by GWL (supported by the new figures in Ch 11). Thus part of the bold face paras (and parts of those elevated to the ES) that refer to specific regions (e.g Pg. 91, lines 23-24) in the projections sections in 11.3 to 11.7 must surely be in relation to scenarios? But the bold face statements/ES statements do not refer to scenarios at all. Or have I misunderstood something? [Roshanka Ranasinghe, Netherlands]	Taken into account. Text revised
107401	38	13	38	22	this paragraph mentions 3 perspectives (frequency, intensity , duration), but the metrics described in this paragraph do not address the 3rd perspective: duration [Markus Donat, Spain]	Accepted. Text revised
107403	38	13	38	22	this paragraph should be over "temperature extremes" in general, but only seems to mention measures of hot temperature extremes, not cold extremes [Markus Donat, Spain]	Accepted. Text revise
107399	38	16	38	16	the ETCCDI is acronym for an Expert TEAM (not group) [Markus Donat, Spain]	Not Applicable. It is no longer included here
62627	38	17	38	17	ETCCDI is already defined earlier in the chapter. Please avoid defining it again. Defining same thing multiple times creates confusion. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The text was changed
23131	38	18	38	22	This was already introduced in an earlier section. Is this redundancy really necessary here? [Peter Thorne, Ireland]	Noted. The text was changed
62677	38	27	40	29	The struture of this section is a little bit messy. Based on the opening paragraph (P38 L34-38), the orders of the following paragraph would be: (1) factors at regional scale (feedbacks, land-use change, and change in aerosol); (2) direct impacts from large-scale circulations; (3) drivers of large-scale circulations through ocean-atmosphere interactions, land-atmosphere feedbacks, and local/regional forcings. However, in P39 L39 - P40 L12, the description of the impacts of feedbacks, or local land-use changes or anthropogenic aerosol change are not connected to large-scale circulations. They are purly regional or local impacts, making these two paragraphs seems to be overlapping with the paragraph P38 L40-L54. Thus, the structure of this section becomes jumping from regional impacts to large-scale impacts and back to regional impacts. I would suggest that the authors could re-arrange the orders of paragraphs, focusing on regional impacts (Feedbacks, land-use) first then large-scale impacts (circulations & climate variability...). Also, combine or synthesize the content in P39 L39-L48 and P38 L40-54, they are repetitive in some senses. The summary of this section (P40 L31-38) actually lays out a very nice structure of this section. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The structure of Section 11.3.1 is modified according to the comment.
62679	38	40	38	54	Is the purpose of this paragraph: change in regional temperature extreme is modulated by several other factors (other then anthropogenic influence) and thereby shows heterogenously spatial distribultion of change in tempeature extreme? If this is the purpose, it would be nice to lay out clearly the purpose at the beginning of the paragraph. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The structure of Section 11.3.1 is modified according to the comment.
82763	38	42	38	44	While it appears to be well established that TXx is rising faster than GMST, perhaps of as much interest is how its rate of increase compares with annual, or even summer, mean T on land (so, comparing the rate of increase in extremes with the increase in means in the same place). If there is not enough evidence on this for an assessment finding, it would be worth noting as a knowledge gap. (Figure 11.1 suggests that in fact TXx and T seem to track each other quite closely over land areas). [Blair Trewin, Australia]	Noted. The evidence is shown in later section, so here we just refer to the section.
38415	38	43	38	43	coldest nights, are shown to increase more than GMST. Is the statement "coldest nights are in incrasing trend" is correct? [Mansour Almazroui, Saudi Arabia]	Noted. The structure of Section 11.3.1 is modified and the related sentence was deleted.
62687	38	46	39	2	The descriptions seem to be inconsistent: P38 L46 "snoe/ice-albedo-temperature feedbacks is one of several reasons why the change in temperature extreme increases more thant GMST in several regions", but in P38 L54 "Although the snow/ice-albedo feedback plays an important role ... the effect on temperature extremes is still unclear." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The sentence was modified not to be inconsistent between the two.
113589	38	48	38	51	Add increased stomata resistance following the VPD increase. Maybe cite on that Vicente-Serrano, S. M., Mcvicar, T. R., Miralles, D. G., Yang, Y. and Tomás-Burguera, M.: Unraveling the influence of atmospheric evaporative demand on drought and its response to climate change, WIREs Clim Change, 11(2), 1–31, doi:10.1002/wcc.632, 2020. [Diego Miralles, Belgium]	Accepted. The text is changed according to the suggested change.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79113	38	49	38	50	No so sure how general this statement can apply [Andong Shi, Sweden]	Noted. The text was not changed.
62681	38	51	38	51	"At regional scale, changes in temperature extreme. ..." change in what quantity? Change in magnitude of delta T or change in the frequency of extreme temperature events? The description should be clarified. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The structure of Section 11.3.1 is modified and the related sentence was deleted.
70951	38	52	38	54	I don't know where this statement comes from, and it certainly doesn't come from Tamarin-Brodsky et al. (2019). That study focused on the SH, and showed that the temperature skewness changes could take either sign, depending on their relation to storm-track location. A subsequent study (Tamarin-Brodsky et al. 2020 doi: 10.1038/s41561-020-0576-3) addressed the NH, where the situation is quite different. In the NH it is true that the skewness change in winter is mostly positive, but in summer it can take either sign, depending on location.The spatial structure is actually quite rich. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This statement was removed, and the reference to Tamarin-Brodsky et al. (2019) is removed.
82765	38	53	38	54	The Tamarin-Brodsky 2019 results are for 850 hPa temperatures, not surface ones. Without further evidence, it could not necessarily be assumed that surface temperatures will have the same frequency distribution properties as 850 hPa ones (in coastal locations and in complex topography, in fact, there's a high chance they will differ, as 850 hPa temperatures will not capture lower-level phenomena such as the moderating effect of sea breezes on maximum temperature). [Blair Trewin, Australia]	Taken into account. Removed the reference to Tamarin-Brodsky et al. (2019).
38417	39	1	39	1	A relevant reference "Revadekar J. V., Hameed S., Collins D. et al. (2012). Impact of altitude and latitude on changes in temperature extremes over South Asia during 1971–2000. Int. J. Climatol. 33: 199–209. https://doi.org/10.1002/joc.3418" may add with (Diro et al. 2018). [Mansour Almazroui, Saudi Arabia]	Noted. The structure of Section 11.3.1 is modified and the related sentence was deleted.
107405	39	1	39	2	"effect on temperature extremes unclear" - the effect of snow-albedo effect on cold temperature extremes is discussed in Gross et al (2020): Gross, M. H., Donat, M. G., Alexander, L. V., and Sherwood, S. C.: Amplified warming of seasonal cold extremes relative to the mean in the Northern Hemisphere extratropics, Earth Syst. Dynam., 11, 97–111, https://doi.org/10.5194/esd-11-97-2020, 2020. [Markus Donat, Spain]	Taken into account. Gross et al. (2020) is referred to, and the related texts are added.
70953	39	4	39	24	What this paragraph seems to be missing are the changes in temperature variability associated with regional (thermodynamic) patterns of temperature changes, acted on by atmospheric dynamics which in the NH, to first order, appears to be little changed (Tamarin-Brodsky et al. 2020 doi: 10.1038/s41561-020-0576-3). This is a robust effect. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The structure of Section 11.3.1 is modified.
62683	39	4	39	24	This paragraph could be more concise. According to my understanding, from L9 to L14, they are talking about a similar idea that a persistent anomalous anticyclone could increase the probablity of persistent temperature extreme events (such as heat waves). The authors could synthesize these descriptions into one well-organized description. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The paragraph was arranged and shortened.
4187	39	9	39	10	A research by Yao et al. (2017) indicated the quasi-stationary Eurasian blocking seems to be more favorable to Eurasian mid-latitude cold anomaly. According to this study, in the past two decades, the increasing quasi-stationary Eurasian blocking associated with BKS sea ice decline could lead to continuous cold anomalies over centre Aisa. Hence, it's valuable to understand what characteristics of blocking could affect the mid-latitude weather and I think it is worthy of reference here. (Recommend: Yao et al. 2017, JC, Increased Quasi Stationarity and Persistence of Winter Ural Blocking and Eurasian Extreme Cold Events in Response to Arctic Warming. Part I: Insights from Observational Analys.) [Wenqi Zhang, China]	Taken into account. Yao et al. (2017) is referred to. It may be related to Rejected.10.1 on the meandering jet.
42319	39	9	39	16	Horton et al 2015 (Nature) has identified changes in frequencies in some circulation patterns linked to temperature extrems; reference to be added [robert vautard, France]	Taken into account. Horton et al. (2015) is added as a reference.
4189	39	11	39	24	This section talkes about the the temperature anomaly associated with atmospheric circulation under the warming climate, involved the Australia, Europe and Asia, but no America. Recently, Zhang et al. found the Arctic sea ice loss could weaken the zonal wind and associated potential vorticity gradient, further prolong the Grenndland blocking lifespan and accelebrate the blocking moveing westward, which can cause the significant cold anomaly over eastern America. Maybe it count for the cold trend partly over eastern America during last two decades. So I think it's meaningful and desirable mentioned here. (Zhang and Luo, 2020, JAS, "A Nonlinear Theory of Atmospheric Blocking: An Application to Greenland Blocking Changes Linked to Winter Arctic Sea Ice Los") [Wenqi Zhang, China]	Taken into account. Zhang et al. (2020) is referred to. It may be related to Rejected.10.1 on the meandering jet.
9227	39	13	39	13	Between "… Rohini et al., 2016)." and "Mid-latitude planetary …", please add the following highly relevant result: , and North America (Yu et al., 2017, 2019). References to add: Yu B., H. Lin, Z. Wu, and W. Merryfield, 2017: The Asian-Bering-North American teleconnection: Seasonality, maintenance, and climate impact on North America. Clim. Dyn., doi: 10.1007/s00382-017-3734-6. Yu B., H. Lin, and N. Soulard, 2019: A comparison of North American surface temperature and temperature extreme anomalies in association with various atmospheric teleconnection patterns. Atmos., 10, 172; doi:10.3390/atmos10040172. [Bin Yu, Canada]	Taken into account. Yu et al. (2017, 2019) are referred to.
55171	39	13			Between "… Rohini et al., 2016)." and "Mid-latitude planetary …", please add the following relevant result: , and North America (Yu et al., 2017, 2019). References to add: Yu B., H. Lin, Z. Wu, and W. Merryfield, 2017: The Asian-Bering-North American teleconnection: Seasonality, maintenance, and climate impact on North America. Clim. Dyn., doi: 10.1007/s00382-017-3734-6. Yu B., H. Lin, and N. Soulard, 2019: A comparison of North American surface temperature and temperature extreme anomalies in association with various atmospheric teleconnection patterns. Atmos., 10, 172; doi:10.3390/atmos10040172. [Nancy Hamzawi, Canada]	Taken into account. Yu et al. (2017, 2019) are referred to.
10993	39	16	39	17	Overall, there is low confidence that there have been any changes in jet meandering. The section cited (2.3.1.3.3) doesn't seem relevant. The best reference is probably Cross-Chapter Box 10.1. [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The reference is changed.
79115	39	18	39	18	There are quite some studies on the northwards shift due to warming, but not so sure about the southward shift. Maybe need to be more specific here. [Andong Shi, Sweden]	Noted. The structure of Section 11.3.1 is modified and the related sentence was deleted.
62685	39	18	39	19	The descriptions should consistently have both region (which hemisphere) and season mentioned. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. This assessment is referring to Chap 3.
100875	39	21	39	21	In chapter 4 Section 4.5.1.6 it was assessed with medium confidence a decrease of winter blocking frequency in the regions with the highest blocking frequency in the historical period. This is in partial contradiction with the assessment here –low confidence in human influence on storm tracks and blocking activity". The two assessments are not totally consistent. [Corti Susanna, Italy]	Noted. This assessment is referring to Chap 4.
62689	39	22	39	22	Could the authors expand the description how the Arctic warming affect mid-latitude temperature extreme "through large-scale circulations"? Since this paragraph is about the impacts of large-scale circulation, the description should be connected to the main topic of this paragraph. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. May consider connection if there are studies.

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82767	39	23	39	24	A further limitation on the likelihood of global-scale influence of circulation changes on temperature extremes is that the impact of any circulation changes is likely to be local/regional - particularly as some extremes are primarily the result of advection of hot air from elsewhere (e.g. many southern Australian heatwaves), others will develop in situ in blocking situations. [Blair Trewin, Australia]	Noted. The text was modified.
100823	39	26	39	37	Here a number of modes of variability are introduced (NAO/AO, SAM, ENSO, PDO, AMO) with no reference to the technical annex VI where the modes are defined. The reference to Annex VI should be added. Also, ideally we should stick as much as possible to the acronyms and names reported in Annex VI (or, at least, both acronyms should be mentioned. For example: AMO/AMV, PDO/PDV AO/NAM…). [Corti Susanna, Italy]	Accepted. Annex IV is appropriately referred to here.
62691	39	26	39	37	L34-36 how is this description different from L27-30? If they are talking about different region, the differences should be distinctly mentioned. Also these two descriptions should be put together. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. These sentences are modified.
23133	39	27	39	29	A cross-reference to the annex on modes of variability should be added here [Peter Thorne, Ireland]	Accepted. Annex IV is appropriately referred to here.
71113	39	28	39	28	The cross-chapter working group on MoV uses Northern Annular Mode (NAM) instead of AO. Also please cite the Technical Annex VI. [Yu Kosaka, Japan]	Accepted. Annex IV is appropriately referred to here.
71115	39	29	39	29	The cross-chapter working group on MoV uses Pacific Decadal Oscillation (PDV) instead of PDO. Unless you want to restrict the focus to the extratropical North Pacific, please use PDV. The same applies repeatedly. Also please cite the Technical Annex VI. [Yu Kosaka, Japan]	Accepted. Annex IV is appropriately referred to here.
102547	39	30	39	30	"Yes, a large…" Insert "a" [Philippe Tulkens, Belgium]	Accepted.
13703	39	31	39	32	Change (Wan et al., 2019)(Kamae et al., 2017b) by (Wan et al., 2019; Kamae et al., 2017b) [Maria Amparo Martinez Arroyo, Mexico]	Accepted. The references are arranged.
43331	39	31		32	Read "(Wan et al., 2019; Kamae et al., 2017b)" rather than "(Wan et al., 2019)(Kamae et al., 2017b)" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. The references are arranged.
71117	39	32	39	32	Denoting "hiatus" without quotation may attract criticism. Please rephrase e.g. "during the so-called global warming "hiatus" period, ..." [Yu Kosaka, Japan]	Noted. Here we refer to Rejected. 3.1 and noted as the "slower surface global warming" period, and moved to 11.3.2.
82769	39	32	39	34	During this period, GMST was still warming, just more slowly than before or after; do these papers show any evidence of a slowdown in the warming rate of extremes or not? (also affects p41 line 9-12). It may be relevant in this context that the warming of GMST over this period in the latest generation of data sets is stronger than it was in the data set versions available at the time of the Seneviratne et al 2014 paper. [Blair Trewin, Australia]	Noted. Here we refer to Rejected. 3.1 and noted as the "slower surface global warming" period, and moved to 11.3.2.
62629	39	32	39	34	The sentence “An increase ....Imada et al....” and associated references should be removed, as the interpretation is WRONG. Eventhough there are many papers on hiatus, research has found that there was no real hiatus”, rather it was just a misinterpretation. IPCC reports should NOT include such misinterpretation. Please see the literation well. Karl, T. R., Arguez, A., Huang, B., Lawrimore, J. H., McMahon, J. R., Menne, M. J., ... & Zhang, H. M. (2015). Possible artifacts of data biases in the recent global surface warming hiatus. Science, aaa5632. Yan, X. H., Boyer, T., Trenberth, K., Karl, T. R., Xie, S. P., Nieves, V., ... & Roemmich, D. (2016). The global warming hiatus: Slowdown or redistribution?. Earth's Future, 4(11), 472-482. Medhaug, I., Stolpe, M. B., Fischer, E. M., & Knutti, R. (2017). Reconciling controversies about the ‘global warming hiatus’. Nature, 545(7652), 41. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Here we refer to Rejected. 3.1 and noted as the "slower surface global warming" period, and moved to 11.3.2.
62693	39	32	39	34	Was the hiatus caused by any of these climate variability? If so, could the authors provide a brief description? If not, is this a good place to include this description? Either way, this description seems odd in the context, and is requried to rephrase (or adjust the place in the context). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Here we refer to Rejected. 3.1 and noted as the "slower surface global warming" period, and moved to 11.3.2.
71119	39	33	39	33	Cross-Chapter Box 3.1 instead of Box 3.1. [Yu Kosaka, Japan]	Accepted.
35079	39	34	39	34	Can confidence language be assigned to "it is suggested"? [David Gutzler, United States of America]	Noted. The text was modified.
9229	39	34	39	37	Suggest revising the sentence as: It is suggested that cold and warm extremes in mid-latitudes are associated with atmospheric circulation patterns, such as the Pacific-North American pattern (PNA) and the Asian-Bering-North American (ABNA) pattern, as well as atmosphere-ocean coupled modes such as the Pacific Decadal Oscillation (PDO), the North Atlantic Oscillation (NAO), and the Atlantic Multidecadal Oscillation (AMO) (Kamae et al., 2014; Johnson et al., 2018; Ruprich-Robert et al., 2018; Yu et al., 2017, 2019). Reference to add: Yu B., H. Lin, Z. Wu, and W. Merryfield, 2017: The Asian-Bering-North American teleconnection: Seasonality, maintenance, and climate impact on North America. Clim. Dyn., doi: 10.1007/s00382-017-3734-6. [Bin Yu, Canada]	Taken into account. The reference is added, and the text was modified.
55173	39	34	39	37	It is suggested that cold and warm extremes in mid-latitudes are associated with atmospheric circulation patterns, such as the Pacific-North American pattern (PNA) and the Asian-Bering-North American (ABNA) pattern, as well as atmosphere-ocean coupled modes such as the Pacific Decadal Oscillation (PDO), the North Atlantic Oscillation (NAO), and the Atlantic Multidecadal Oscillation (AMO) (Kamae et al., 2014; Johnson et al., 2018; Ruprich-Robert et al., 2018; Yu et al., 2017, 2019). Reference to add: Yu B., H. Lin, Z. Wu, and W. Merryfield, 2017: The Asian-Bering-North American teleconnection: Seasonality, maintenance, and climate impact on North America. Clim. Dyn., doi: 10.1007/s00382-017-3734-6. [Nancy Hamzawi, Canada]	Taken into account. The reference is added, and the text was modified.
71121	39	36	39	36	Please use PDV instead of PDO as above. [Yu Kosaka, Japan]	Accepted.
71123	39	36	39	36	Likewise, please use Atlantic Multidecadal Variability (AMV) in place of AMO. [Yu Kosaka, Japan]	Accepted.
45583	39	36	39	37	Also results from Müller et al 2020 (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019GL086043) Borchert et al. 2019 (https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2019GL085385) and Qasmi et al. (https://arxiv.org/abs/2001.04195) support the AMO and NA gyre connection to European heat extremes [Laura Suarez-Gutierrez, Germany]	Taken into account. The references (Muller et al., Qassim et al) are added, and the text was modified.
45585	39	39	39	48	Suarez-Gutierrez et al 2020 (https://link.springer.com/article/10.1007/s00382-020-05233-2) adds a relevant aspect to the discussion of the driving mechanisms of heat extremes, and why all sources of influence must be considered simulatenously to account for their multicorrelation. Also the dynamic adjustment literature such as Merrifield et al. 2019 and other studies using this method add important information to the dynamic vs. thermodynamic discussion, and add a different methodological perspective. [Laura Suarez-Gutierrez, Germany]	Noted. This paper is not directly relevant to the cited paragraph. Suarez-Gutierrez et al. (2020) "Dynamical and thermodynamical drivers of variability in European summer heat extremes"

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29463	39	39		48	since there is high confidence in land-atmosphere feedback, there should be more research (future publications) on the percentage effect of this feedback on temperature extremes at regional scale. This will help in knowing the contribution of this feedback in temperature extremes. to what extent is this feedback causing temperature extremes. [Babatunde Oyekan, Nigeria]	Noted.
113591	39	43	39	43	was shown to be relevant' change for 'was shown to affect the growth and state of the atmospheric boundary layer' [Diego Miralles, Belgium]	Noted. The text was modified.
79167	39	43	39	48	include also cowan et al., NatCC, 2020 (https://www.nature.com/articles/s41558-020-0771-7) here and maybe elsewhere in the chapter [Sabine Undorf, Sweden]	Taken into account. The reference is added.
24511	39	43	39	48	Sato and Nakamura (2019) seems to be a suitable reference here. Sato, T. and T. Nakamura, 2019: Intensification of hot Eurasian summers by climate change and land-atmosphere interactions. Scientific Reports, 9, 10866(2019), DOI: 10.1038/s41598-019-47291-5 [Tomonori Sato, Japan]	Taken into account. The reference is added.
79117	39	45	39	46	Feels like it is important to discuss a bit about what are these uncertainties… [Andong Shi, Sweden]	Noted. It is not a scope of this section to discuss the discrepancy between the models and the observations.
113593	39	47	39	48	The soil moisture-temperature feedback also has non-local effects (Vautard et al., 2007; Stéfanon et al., 2014)' for 'The soil moisture-temperature feedback may also affect regional circulation patterns (Vautard et al., 2007; Zampieri et al., 2009)'. Zampieri, M. et al. Hot European summers and the role of soil moisture in the propagation of Mediterranean drought. J. Clim. 22, 4747–4758 (2009). [Diego Miralles, Belgium]	Noted. These references are not recent and inadequate for AR6.
113595	39	48	39	48	Please add after this: 'Moreover, temperature extremes have been shown to propagate downwind via land-atmosphere feedbacks and their effects on heat advection (Miralles et al., 2019; Schumacher et al., 2019)'. Miralles, D. G., Gentine, P., Seneviratne, S. I. and Teuling, A. J.: Land-atmospheric feedbacks during droughts and heatwaves: state of the science and current challenges, Ann. N.Y. Acad. Sci., 2019. Schumacher, D. L., Keune, J., Heerwaarden, C. C., de Arellano, J. V. X. G., Teuling, A. J. and Miralles, D. G.: Amplification of mega-heatwaves through heat torrents fuelled by upwind drought, Nature Geosci, 1–8, doi:10.1038/s41561-019-0431-6, 2019. [Diego Miralles, Belgium]	Taken into account. The references are added.
62695	39	48	39	48	Here is another reference about the soil moisture-atmosphere feedback can have non-local effects: Koster, R. D., Y. Chang, H. Wang, and S. D. Schubert, 2016: Impacts of local soil moisture anomalies on the atmospheric circulation and on remote surface meteorological fields during boreal summer: A comprehensive analysis over North America. J. Climate, 29, 7345–7364, https://doi.org/10.1175/JCLI-D-16-0192.1. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The reference is added.
117091	39		39		on the role of deforestation on extremes, please provide an assessment (this is currently juste a quote from one paper). [Valerie Masson-Delmotte, France]	Taken into account. The sentence was modified.
9257	40	1	40	1	The literature cited in this sentence should be Li et al. (2018h), which refers to "Li, X., Chen, H., Wei, J., Hua, W., Sun, S., Ma, H., et al. (2018h). Inconsistent Responses of Hot Extremes to Historical Land Use and Cover Change Among the Selected CMIP5 Models. J. Geophys. Res. Atmos. 123, 3497–3512. doi:10.1002/2017JD028161." [Xing Li, China]	Noted. This sentence was removed.
45593	40	1	50	1	I think this section would benefit for including some paragraphs on other conditions that exacerbate heat stress beyond absolute temperatures, such as hot and humid conditions or tropical nights. Some examples to consider: Suarez-Gutierrez et al. 2020b (https://link.springer.com/article/10.1007/s00382-020-05263-w), Coffel et al 2018 (https://iopscience.iop.org/article/10.1088/1748-9326/aaa00e), Buzan & Huber 2020 (https://www.annualreviews.org/doi/full/10.1146/annurev-earth-053018-060100) or Raymond et al. 2020 (https://advances.sciencemag.org/content/6/19/eaaw1838) [Laura Suarez-Gutierrez, Germany]	Rejected. Heat stress is assessed in CH12
79119	40	3	40	3	crop intensification?? Not sure, better provide reference [Andong Shi, Sweden]	Taken into account. Changed to "cropland intensification" and added a reference: Mueller et al. (2016).
39775	40	3	40	3	"likely" -> Does this have the IPCC quantitative meaning? [TSU WGI, France]	Taken into account. The text was modified.
62631	40	4	40	4	ongoing CMIP6 simulations -> CMIP6 simulations; it's already complete..so statement like is is misleading. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. "on-going" was deleted.
90855	40	5			Cross citing "A new methodology to map double-cropping croplands based on continuous wavelet transform" by Qiu et al., (2014) [Vivien How, Malaysia]	Noted. This reference shows a methodology, and is not directly relevant to this paragraph.
42317	40	14	40	15	Can refer to the Uban CC Box [robert vautard, France]	Accepted. Box10.3 is referred to.
72197	40	14	40	29	urban heat island impact on heat waves more because of high minimum temperature than high daily temperature. Daily maximum is usually the same asin the surroundings, but daily minimum in the urban environment is much higher than in the surroundings. So there is not time for rest. see Matzarakis and Nestos 2011, Theor. Appl. Clim., 105:99-106, Robinson,2001, J. Appl. Meteorol., 40:762-775) [Joanna Wibig, Poland]	Noted. This reference is not directly related to UHI.
69907	40	14	40	29	The urban heat island effect amplifies the demand for cooling. increased demand for air conditioning will increase energy demand that will thus require additional energy production. Energy efficiency, including in equipment efficiency like air conditioners, can reduce this demand and help limit additional emissions that would further exacerbate climate change. Dreyfus G., et al. (2020) ASSESSMENT OF CLIMATE AND DEVELOPMENT BENEFITS OF EFFICIENT AND CLIMATE-FRIENDLY COOLING; Sachar et al. (2018) Solving the Global Cooling Challenge: How to Counter the Climate Threat from Room Air Conditioners. Rocky Mountain Institute; Shah, N., Wei, M., Letschert, V. and Phadke, A. (2019). Benefits of Energy Efficient and Low-Global Warming Potential Refrigerant Cooling Equipment. U.S.A: Lawrence Berkeley National Laboratory; Shah N., et al. (2015) Benefits Of Leapfrogging To Superefficiency And Low Global Warming Potential Refrigerants In Air Conditioning, Ernest Orlando Lawrence Berkeley National Laboratory; IEA (2018) Future of Cooling; Sustainable Energy for All (2018) Chilling Prospects: Providing Sustainable Cooling for All; and Birmingham Energy Institute, University of Birmingham (2018) A Cool World: Defining the Energy Conundrum of Cooling for All; Biardeau, L.T., Davis, L.W., Gertler, P., Wolfram, C., 2020. Heat exposure and global air conditioning. Nature Sustainability 3, 25–28 ("Air conditioning adoption is increasing dramatically worldwide as incomes rise and average temperatures go up. Using daily temperature data from 14,500 weather stations, we rank 219 countries and 1,692 cities based on a widely used measure of cooling demand called total cooling degree day exposure. India, China, Indonesia, Nigeria, Pakistan, Brazil, Bangladesh and the Philippines all have more total cooling degree day exposure than the United States—a country that uses 400 terawatt-hours of electricity annually for air conditioning."). [Gabrielle Dreyfus, United States of America]	Noted. IEA (2018) Future of Cooling: https://www.iea.org/reports/the-future-of-cooling Biardeau, L.T., Davis, L.W., Gertler, P., Wolfram, C., 2020. Heat exposure and global air conditioning. Nature Sustainability 3, 25–28 [Cannot access this literature.]

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66879	40	14	40	29	The urban heat island effect amplifies the demand for cooling. Cities can be 1–3 ºC warmer than surrounding areas, and in the evenings, this difference can be as high as 12 ºC. Urban heat island effects lead to increased energy consumption and elevated emissions of air pollutants and greenhouse gases. To alleviate the additional heat and increased demand for cooling, making roofs and pavements more reflective as well as increasing vegetation can help counteract the urban heat island effect. Further, tree canopies can also lower city temperatures. Akbari H., et al. (2001) Cool surfaces and shade trees to reduce energy use and improve air quality in urban areas, Solar Energy 70(3):295–310; International Energy Agengy (2018). Future of Cooling: Opportunities for Energy Efficient Air Conditioning; US EPA, "Heat Island Effects", https://www.epa.gov/heat-islands; Lawrence Berkeley National Laboratory, "Heat Island Group, Cool Roofs", https://heatisland.lbl.gov/coolscience/cool-roofs; Ziter C. D., et al. (2019) Scale-dependent interactions between tree canopy cover and impervious surfaces reduce daytime urban heat during summer, PROC. NAT'L. ACAD. SCI. 116(15):7575–7580; Al-Obaidi K. M., et al. (2014) Passive cooling techniques through reflective and radiative roofs in tropical houses in Southeast Asia: A literature review, FRONTIERS ARCHITECTURAL RESEARCH 3(3):283–297;Santamouris M. (2014) Cooling the cities – A review of reflective and green roof mitigation technologies to fight heat island and improve comfort in urban environments, Solar Energy 103:682–703; Oliveira S., et al. (2011) The cooling effect of green spaces as a contribution to the mitigation of urban heat: A case study in Lisbon, Building & Env't. 46(11):2186–2194. [Kristin Campbell, United States of America]	Noted. But it is unclear what is suggested/commented here. Future development and adaptation etc. are not within the scope of this chapter. The references are not obtained.
66881	40	14	40	29	In a warming world with a growing population and expanding middle-class, the demand for cooling is projected to rise substantially. Currently, there are 3.6 billion cooling appliances, which is projected to rise to 9.5 billion by 2050, though up to 14 billion would be required to provide adequate cooling for all. University of Birmingham (2018) A Cool World: Defining the Energy Conundrum of Cooling for All ("Considering per capita equipment penetrations at regional level, it becomes clear that 9.5 billion cooling appliances by 2050 will, on the current technology pathways, not be sufficient to deliver universal access to cooling, let alone meet the UN SDGs 2030 targets. Food and medicine loss in the supply chain will still be high; food poisoning from lack of cold chain and domestic temperature management will still be significant; farmers will lack market 'connectivity' or 'access'; hundreds of millions of people will not have safe, let alone comfortable, living or working environments; medical centres will not have temperature-controlled services for post-natal care, etc... By 2050, would require a total of 14 bn cooling appliances – an additional 4.5 bn appliances compared to the baseline forecast – or 4 times as many pieces of cooling equipment than are in use today."); Dreyfus G., et al. (2020) ASSESSMENT OF CLIMATE AND DEVELOPMENT BENEFITS OF EFFICIENT AND CLIMATE-FRIENDLY COOLING. [Kristin Campbell, United States of America]	Noted. But it is unclear what is suggested/commented here. Future development and adaptation etc. are not within the scope of this chapter.
66883	40	14	40	29	At the same time, increased demand for air conditioning will increase energy demand that will thus require additional energy production. Energy efficiency, including in equipment efficiency like air conditioners, can reduce this demand and help limit additional emissions that would further exacerbate climate change. Dreyfus G., et al. (2020) ASSESSMENT OF CLIMATE AND DEVELOPMENT BENEFITS OF EFFICIENT AND CLIMATE-FRIENDLY COOLING; Sachar et al. (2018) Solving the Global Cooling Challenge: How to Counter the Climate Threat from Room Air Conditioners. Rocky Mountain Institute; Shah, N., Wei, M., Letschert, V. and Phadke, A. (2019). Benefits of Energy Efficient and Low-Global Warming Potential Refrigerant Cooling Equipment. U.S.A: Lawrence Berkeley National Laboratory; Shah N., et al. (2015) Benefits Of Leapfrogging To Superefficiency And Low Global Warming Potential Refrigerants In Air Conditioning, Ernest Orlando Lawrence Berkeley National Laboratory; IEA (2018) Future of Cooling; Sustainable Energy for All (2018) Chilling Prospects: Providing Sustainable Cooling for All; and Birmingham Energy Institute, University of Birmingham (2018) A Cool World: Defining the Energy Conundrum of Cooling for All; Biardeau, L.T., Davis, L.W., Gertler, P., Wolfram, C., 2020. Heat exposure and global air conditioning. Nature Sustainability 3, 25–28 ("Air conditioning adoption is increasing dramatically worldwide as incomes rise and average temperatures go up. Using daily temperature data from 14,500 weather stations, we rank 219 countries and 1,692 cities based on a widely used measure of cooling demand called total cooling degree day exposure. India, China, Indonesia, Nigeria, Pakistan, Brazil, Bangladesh and the Philippines all have more total cooling degree day exposure than the United States—a country that uses 400 terawatt-hours of electricity annually for air conditioning."). [Kristin Campbell, United States of America]	Noted. But it is unclear what is suggested/commented here. Future development and adaptation etc. are not within the scope of this chapter..
52809	40	14	40	29	The discussion of the UHI fails to mention that UHI is primarily a night-time phenomenon, and in the list of causal factors the increased heat storage in buildings and other structures (streets) is a key - it is what explains why the UHI amplitude is maximum around the time fo the daily air temperature minium. [Petra Seibert, Austria]	Noted. The intend is not to conduct in-depth assessment of HUI.
131419	40	14	40	29	It appears here that the UHI effect is more or less homogenous for a city in general but there are large temperature differences depending on the type of area, especially the share of trees, green rooftops and parks in one area have a large influence. Dense built-up areas like informal settlement, or large-scale building-complexes are heating up much more than a classical uper middle class residential areas intermingeld with parks and trees for example. This is important because the vulnerability depends a lot on the settlement-form. [Hans Poertner and WGII TSU, Germany]	Noted. Urban climate is more described in BOX 10.3.
131421	40	14	40	29	The UHI is presented as an homogenous effect for the whole city/ urban areas. However, especially in combination with heat waves the particular characteristics of different neighbourhoods (green areas, water, density, scale of buildings etc.) are crucial for the magnitude of the UHI. There have been a number of studies on this especially using remote sensing to illustrate for example the UHI for informal settlements/ low incomne densly build areas. [Hans Poertner and WGII TSU, Germany]	Noted. Urban climate is more described in BOX 10.3.
40541	40	14	40	29	No conclusion given on urban heat island effects (with confidence language) [TSU WGI, France]	Noted. Urban climate is more described in BOX 10.3.
68453	40	14	40	29	The urban heat island effect amplifies the demand for cooling. Cities can be 1–3 ºC warmer than surrounding areas, and in the evenings, this difference can be as high as 12 ºC. Urban heat island effects lead to increased energy consumption and elevated emissions of air pollutants and greenhouse gases. To alleviate the additional heat and increased demand for cooling, making roofs and pavements more reflective as well as increasing vegetation can help counteract the urban heat island effect. Further, tree canopies can also lower city temperatures. Akbari H., et al. (2001) Cool surfaces and shade trees to reduce energy use and improve air quality in urban areas, Solar Energy 70(3):295–310; International Energy Agengy (2018). Future of Cooling: Opportunities for Energy Efficient Air Conditioning; US EPA, "Heat Island Effects", https://www.epa.gov/heat-islands; Lawrence Berkeley National Laboratory, "Heat Island Group, Cool Roofs", https://heatisland.lbl.gov/coolscience/cool-roofs; Ziter C. D., et al. (2019) Scale-dependent interactions between tree canopy cover and impervious surfaces reduce daytime urban heat during summer, PROC. NAT'L. ACAD. SCI. 116(15):7575–7580; Al-Obaidi K. M., et al. (2014) Passive cooling techniques through reflective and radiative roofs in tropical houses in Southeast Asia: A literature review, FRONTIERS ARCHITECTURAL RESEARCH 3(3):283–297;Santamouris M. (2014) Cooling the cities – A review of reflective and green roof mitigation technologies to fight heat island and improve comfort in urban environments, Solar Energy 103:682–703; Oliveira S., et al. (2011) The cooling effect of green spaces as a contribution to the mitigation of urban heat: A case study in Lisbon, Building & Env't. 46(11):2186–2194. [Durwood Zaelke, United States of America]	Noted. Urban climate is more described in BOX 10.3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68455	40	14	40	29	In a warming world with a growing population and expanding middle-class, the demand for cooling is projected to rise substantially. Currently, there are 3.6 billion cooling appliances, which is projected to rise to 9.5 billion by 2050, though up to 14 billion would be required to provide adequate cooling for all. University of Birmingham (2018) A Cool World: Defining the Energy Conundrum of Cooling for All ("Considering per capita equipment penetrations at regional level, it becomes clear that 9.5 billion cooling appliances by 2050 will, on the current technology pathways, not be sufficient to deliver universal access to cooling, let alone meet the UN SDGs 2030 targets. Food and medicine loss in the supply chain will still be high; food poisoning from lack of cold chain and domestic temperature management will still be significant; farmers will lack market 'connectivity' or 'access'; hundreds of millions of people will not have safe, let alone comfortable, living or working environments; medical centres will not have temperature-controlled services for post-natal care, etc... By 2050, would require a total of 14 bn cooling appliances – an additional 4.5 bn appliances compared to the baseline forecast – or 4 times as many pieces of cooling equipment than are in use today."); Dreyfus G., et al. (2020) ASSESSMENT OF CLIMATE AND DEVELOPMENT BENEFITS OF EFFICIENT AND CLIMATE-FRIENDLY COOLING. [Durwood Zaelke, United States of America]	Noted. Urban climate is more described in BOX 10.3.
68457	40	14	40	29	At the same time, increased demand for air conditioning will increase energy demand that will thus require additional energy production. Energy efficiency, including in equipment efficiency like air conditioners, can reduce this demand and help limit additional emissions that would further exacerbate climate change. Dreyfus G., et al. (2020) ASSESSMENT OF CLIMATE AND DEVELOPMENT BENEFITS OF EFFICIENT AND CLIMATE-FRIENDLY COOLING; Sachar et al. (2018) Solving the Global Cooling Challenge: How to Counter the Climate Threat from Room Air Conditioners. Rocky Mountain Institute; Shah, N., Wei, M., Letschert, V. and Phadke, A. (2019). Benefits of Energy Efficient and Low-Global Warming Potential Refrigerant Cooling Equipment. U.S.A: Lawrence Berkeley National Laboratory; Shah N., et al. (2015) Benefits Of Leapfrogging To Superefficiency And Low Global Warming Potential Refrigerants In Air Conditioning, Ernest Orlando Lawrence Berkeley National Laboratory; IEA (2018) Future of Cooling; Sustainable Energy for All (2018) Chilling Prospects: Providing Sustainable Cooling for All; and Birmingham Energy Institute, University of Birmingham (2018) A Cool World: Defining the Energy Conundrum of Cooling for All; Biardeau, L.T., Davis, L.W., Gertler, P., Wolfram, C., 2020. Heat exposure and global air conditioning. Nature Sustainability 3, 25–28 ("Air conditioning adoption is increasing dramatically worldwide as incomes rise and average temperatures go up. Using daily temperature data from 14,500 weather stations, we rank 219 countries and 1,692 cities based on a widely used measure of cooling demand called total cooling degree day exposure. India, China, Indonesia, Nigeria, Pakistan, Brazil, Bangladesh and the Philippines all have more total cooling degree day exposure than the United States—a country that uses 400 terawatt-hours of electricity annually for air conditioning."). [Durwood Zaelke, United States of America]	Noted. Urban climate is more described in BOX 10.3.
24513	40	14	40	29	Another important aspect, but less commonly known, is the effect of snow cover in urban area. For snowy cities UHI is mitigated by snow cover since the artificial surface will be masked and more reflection of shortwave radiation at the surface. The warming will reduce snow cover, which increases exposure of urban artificial surfaces leading to stronger UHI than present climate. Therefore, snowy urban area may experience additional warming due to snow cover change. Mori and Sato (2015) assessed that this effect on daily maximum temperature is comparable with the magnitude of artificial heat release. Mori, K., and T. Sato, 2015: Evaluating the Role of Snow Cover in Urban Canopy Layer on the Urban Heat Island in Sapporo, Japan with a Regional Climate Model. J. Meteor. Soc. Japan, 93, 581-592, DOI: 10.2151/jmsj.2015-039. [Tomonori Sato, Japan]	Noted. Urban climate is more described in BOX 10.3.
44283	40	15	40	18	The main Urban Heat Island driver is the three dimensional urban surface and the resulted Sky View Factor that regulates the net all-wave radiation and therefore the Urban Energy Balance (Oke, T.R., et al., 2017: Urban Climates. Cambridge University Press, Cambridge. Chapter 7 Urban Heat Island, pp. 197 - 237). [Nektarios Chrysoulakis, Greece]	Noted. Urban climate is more described in BOX 10.3.
44285	40	17	40	18	See above comment No 20 on the bulk albedo of the urban areas that is lower than their surroundings, as well as the role of the three dimensional urban surface on reducing the albedo at city scale. [Nektarios Chrysoulakis, Greece]	Noted. Urban climate is more described in BOX 10.3.
20243	40	18	40	20	While this sentence does include a verbatim quote of Mohajerani et al.'s paper, reading "seasonal variations of a city's particular location" reminds one of S.F. novels. [philippe waldteufel, France]	Noted. The text was modified.
39817	40	20	40	20	"seasonal variations of a city's particular location" -> Sounds like the city moves around [TSU WGI, France]	Noted. The text was modified.
125911	40	20	40	26	UHI is also largely a function of urban planning, utilmately the urban land use and land cover (Liang and Keener, 2016). [Trigg Talley, United States of America]	Noted. Urban climate is more described in BOX 10.3.
44287	40	22	40	23	The interaction between the UHI and heatwaves is a function of the Urban Energy Balance and more specifically on how the available energy is partitioned into the turbulent fluxes of sensible heat and latent heat, which can be estimated at city scale by synergistically exploiting satellite observations with standard meteorological measurements (Chrysoulakis et al. 2018: Urban energy exchanges monitoring from space. Scientific Reports, 8, 11498). [Nektarios Chrysoulakis, Greece]	Noted. Urban climate is more described in BOX 10.3.
6841	40	26	40	26	European cities instead of Europe cities [Constantinos Cartalis, Greece]	Noted. The text was modified.
11413	40	26	40	27	"with cooler cities more affected by additional heat" What does this mean? [Strandberg Gustav, Sweden]	Noted. The text was modified.
44289	40	27	40	29	The implementation of Nature-Based Solutions (NBS) in urban areas is expected to give urban planning the opportunity to play an important role in climate change mitigation/adaptation, at both neighbourhood and city scales. NBS are defined as "locally adapted actions inspired by, supported by or copied from nature" (European Commission, 2015: Towards an EU Research and Innovation policy agenda for Final Report of the Horizon 2020 Expert Group on 'Nature-Based Solutions and Re-Naturing Cities'. Directorate-General for Research and Innovation, Climate Action, Environment, Resource Efficiency and Raw Materials. Luxembourg: Publications Office of the European Union. ISBN 978-92-79-46051-7) and constitute a pertinent category of spatial practices, which are strongly adopted and supported by the European Commission and other international organisations (European Environment Agency, United Nations Environment Programme, International Union for Conservation of Nature, etc.), as a sustainable and resilient technique creating balance between built and natural systems (Somarakis, G., et al., 2019. Thinknature Nature-Based Solutions Handbook, doi:10.26225/jerv-w202). [Nektarios Chrysoulakis, Greece]	Noted. Urban climate is more described in BOX 10.3. Checked the reference: European Commission, 2015: Towards an EU Research and Innovation policy agenda for Final Report of the Horizon 2020 Expert Group on 'Nature-Based Solutions and Re-Naturing Cities'. Directorate-General for Research and Innovation, Climate Action, Environment, Resource Efficiency and Raw Materials. Luxembourg: Publications Office of the European Union. ISBN 978-92-79-46051-7 https://op.europa.eu/en/publication-detail/-/publication/fb117980-d5aa-46df-8edc-af367cddc202
9121	40	27	40	29	We suggest the addition of the paper on the cooling effect of vegetation. Tonosaki K, Kawai S, T.K., 2014: Cooling Potential of Urban Green Spaces in Summer. Designing Low Carbon Societies in Landscapes. [Nakagoshi N, Mabuhay AJ. (eds.)]. Springer, Tokyo, pp. 15–34. [Kochi Tonosaki, Japan]	Noted. Urban climate is more described in BOX 10.3. The reference is not cited. Tonosaki K, Kawai S, T.K., 2014: Cooling Potential of Urban Green Spaces in Summer. Designing Low Carbon Societies in Landscapes. [Nakagoshi N, Mabuhay AJ. (eds.)]. Springer, Tokyo, pp. 15–34. (NOT accessible.)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6843	40	29	40	29	Replace the sentense to read as "These effects may be partially mitigated through the removal of heat sources, the implementation of cool materials or increased blue/green infrastructure in cities, which could potentially reduce mean warming and hot extremes (Akbari et al., 2016; Li et al., 2014a; Seneviratne et al., 2018a) H. Akbari, C. Cartalis, D. Kolokotsa, A. Muscio, A. L. Pisello, F. Rossi, M. Santamouris, A. Synnefa, N.H. Wongf, M. Zinzig, 2016, Local Climate Change and Urban Heat Island Mitigation Techniques – The State of the Art, Journal of Civil Engineering and Management, 22 (1), pp. 1-16. [Constantinos Cartalis, Greece]	Noted. Urban climate is more described in BOX 10.3.
66695	40	31	40	36	Suggest a rewrite to bring out the dominance point: "Summary: Greenhouse gas forcing provides the dominant driver behind changes in extreme temperatures, though multiple mechanisms are involved. At regional scales, long-term changes in temperature extremes are modulated by changes in circulation patterns and soil moisture-evapotranspiration-temperature or snow/ice-albedo-temperature feedbacks. The short-term behaviour of extremes is also affected by decadal and multi-decadal natural variability and shorter-lived anthropogenic forcers. " [Dave Frame, New Zealand]	Taken into account. The summary paragraph is modified according to the suggestion.
66885	40	31	40	38	Speed is the metric of concern because of our proximity to 1.5C and drastic mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). [Kristin Campbell, United States of America]	Noted. This is not within the scope of Chapter 11
66887	40	31	40	38	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescales like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Kristin Campbell, United States of America]	Noted. This is not within the scope of Chapter 11
68459	40	31	40	38	Speed is the metric of concern because of our proximity to 1.5C and aggressive mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). Aggressive mitigation of SLCPs can cut the rate of warming in half, Arctic warming by two-thirds, and avoid up to 0.6C of warming by 2050. UNEP & WMO (2011) Integrated Assessment of Black Carbon and Tropospheric Ozone; Shindell D., et al. (2012) Simultaneously Mitigating Near-Term Climate Change and Improving Human Health and Food Security, Science 335(6065):183–189; Xu and Ramanathan (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, Proc. Natl. Acad. Sci. 114(39):10315–10323. [Durwood Zaelke, United States of America]	Noted. This is not within the scope of Chapter 11
68461	40	31	40	38	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescale like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Durwood Zaelke, United States of America]	Noted. This is not within the scope of Chapter 11
11677	40	32	40	32	"greenhouse-gas forcing" [Amy East, United States of America]	Taken into account. Changed to "Greenhouse gas forcing".
70955	40	32	40	33	In the NH, it is less the changes in circulation patterns than the effect of circulation patterns in mediating regional patterns of temperature change, which is important for changes in temperature variability (Tamarin-Brodsky et al. 2020 doi: 10.1038/s41561-020-0576-3) [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The text was modified.
70957	40	37	40	38	I think this low confidence statement applies only to the NH, doesn't it? In the SH, if we accept the poleward shift of the storm tracks as a robust projection from climate models, then there are robust changes in temperature variability that accompany this (Tamarin-Brodsky et al. 2019, already cited). [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The text was modified.
113597	40	41	43	20	To me the most interesting part of these results and figures is that cold extremes are becoming more warm than hot extremes are. This implies a narrowing of the base of the temperature histogram. This is not seem in the histogram depicted in the FAQs. It was not reported this way in previous reports neither, if I recall right; cold extremes were typically less drastically and confidently changing. [Diego Miralles, Belgium]	Noted. We do not see any action requested. Please note that the figures of the FAQs have been completely revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38435	40	41	51	10	Climate extremes in the Middle East are not much assessed in this chapter (Chapter 11), though this region is highly vulnerable to climate change. Section 11.3 focused on temperature extremes and examined observed and simulated temperature extremes over different regions but the information about extremes over the Middle East region including Arabian Peninsula is missing in this section. Some recent studies examined the extreme temperature and precipitation events over 1- Arabian Peninsula Almazroui_2020_AJGS_Extreme_Temperature.pdf (in SOD submitted literature, and currently in press) and 2- MENA region Driouech_et_al_2020_Changes_CORDEX_MENA_ESV (already in DM under Home/SOD submitted literature) The information available in these studies could be used to fill this gap in section 11.3 and 11.4 In addition, there is not much information about the assessment of extremes over Africa. [Mansour Almazroui, Saudi Arabia]	Considered. Assessments are provided for all AR6 regions in Section 11.9.
38473	40	41	51	10	Climate extremes in the Middle East are not much assessed in this chapter (Chapter 11), though this region is highly vulnerable to climate change. Section 11.3 focused on temperature extremes and examined observed and simulated temperature extremes over different regions but the information about extremes over the Middle East region including Arabian Peninsula is missing in this section. Some recent studies examined the extreme temperature and precipitation events over the region. [Mansour Almazroui, Saudi Arabia]	Considered. Assessments are provided for all AR6 regions in Section 11.9.
108865	40	47	43	9	In this whole section it is often unclear over what period the trend has been observed. This is critical because internal variability is large and the statements may strongly dependent on the time period. For the US it is fair to acknowledge that the picture changes quite substantially depending whether you start the analysis in the 1930s or 1950s. I would avoid choosing periods for which you could be accussed of cherry-picking [Erich Fischer, Switzerland]	Taken into account. Text revised
13705	40	52	40	52	Indicate if 11.2 is section, table or box [Maria Amparo Martinez Arroyo, Mexico]	Not Applicable. Text was changed
39281	40	52	41	14	, ; [Lourdes Tibig, Philippines]	Noted.
13707	40	54	40	54	change of nights by of cold nights [Maria Amparo Martinez Arroyo, Mexico]	Not Applicable. Text was changed
117093	40		40		findings from the chapter on urban aspects coould be integrated with the ccbox in chapter 10 [Valerie Masson-Delmotte, France]	Taken into account. BOX 10.3 "Urban Climate" is referred to here.
8695	41	1	41	1	Could add Dunn et al, 2020, submitted using HadEX3 to support and update the statement from Alexander 2016 [Robert Dunn, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference added
108145	41	2	41	3	I suggest adding the following citation: Olmo M, Bettolli ML, Rusticucci M. Atmospheric circulation influence on temperature and precipitation individual and compound daily extreme events: spatial variability and trends over southern South America. Weather and climate extremes. Submitted December 2019. Olmo et al 2019 found significant positive trends for TX90p over the whole Southern South America region based on station data and CPC gridded data. TN90p also showed significant trends across the region but TX90p presented more homogeneous trends that TN90p. TX10p also showed significant negative trends over the region. [Maria Bettolli, Argentina]	Accepted. Reference added
62699	41	3	41	4	The sentence correctly summarizes the study by Rusticucci et al. 2017 which focus on south America. However on the global level there appears to be regions with a negative trend in TX90p also in North America, as shown in Figure11.7 middle and also in the study by Alexander et al 2017. However the trend for the region in NA is not significant as opposed to the region in South America. However, I interpreted the sentence as the only region is found in South America and this was contradicting Figure 11.7 (middle). But this could also be due to a wrong interpretation of the sentence on my side. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. Regional information is grouped by continents 11.3 and assessments for individual regions are provided in Section 11.9.
108861	41	5	41	9	Consider citing this study, which shows that TXx and TNn have warmed over the vast majority of global land regions with an area land fraction that is much higher than expected by chance. Fischer, E. M., and R. Knutti (2014), Detection of spatially aggregated changes in temperature and precipitation extremes, Geophysical Research Letters, 41(2), 547-554. [Erich Fischer, Switzerland]	Accepted
23137	41	9	41	10	Is a period ending a decade ago really 'recent'? [Peter Thorne, Ireland]	Taken into account. Text revised
62697	41	40	41	40	The Reference to Figure 11.1 is probably wrong. Figure 11.1 only shows Txx and Txxp95 global. But the paragraph refers to the significance increase in Tnn and Txx for Africa. Maybe the intention was to refer to Figure 11.7 but there is no statement about significance in that figure. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Changed to Figure 11.9
23943	41	44	41	45	Change "during the last decades" to "in recent decades" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text revised
38143	41	44	41	49	For East Asian temperature extemes, only a few Chinese studies are cited. Studies for Korea and Japan need to be included here for better coverage. [Junhee Lee, Republic of Korea]	Take into account. Additional references are added.
23945	41	44	41	54	The authors should consider whether the heavy use of the bracket structure for alternatives in this paragraph is the best way to make the text readable. For example, see Alan Robock's commentary on this at: https://eos.org/opinions/parentheses-are-are-not-for-references-and-clarification-saving-space. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Take into account. Text revised.
42961	41	44	42	46	I find these continental summaries not very informative due to the large differences in climate of each continent. Why not group it by climatic regions? [Rein Haarsma, Netherlands]	Considered. This is to address the need of assessing individual continents and regions.
23139	41	45	41	49	This passage is a real mind twister. I think the two final segments of the sentence may be saying the same thing. But even after several reads I am very very confused. Please redraft for clarity. [Peter Thorne, Ireland]	Accepted. Text revised
23947	41	47			Change "in the" to "on the" in both cases. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not Applicable. Text was changed
39283	41	51	41	54	Here are a few studies in a part of Asia (Southeast Asia) you may want to consider-Cheong et al, 2018 (Observed and modelled temperature and precipitation extremes over Southeast Asia); Dong et al, 2018 (Observed changes in temperture exremes over Asia and their attribution); and Marjuki et al, 2016 (observed trends and variability in climate indices relevant for crop yields in Southeast Asia). [Lourdes Tibig, Philippines]	Considered. The listed papers are cited when relevant.
38437	41	53	41	53	Chapter 11 mentioned heavy precipitation trends in the Himalayas. Whereas chapter 10 (Chapter box 10.3) assesses the climate change over the Hindu Kush Himalaya region. For consistency, chapter 11 should also adopt the same terminology for naming the region. For example, the study of Sun et al., 2017, cited in chapter 11, examined changes in the extreme temperature over Hindu Kush Himalaya during 1961-2015 but in the text, the region is referred to as Himalaya and Tibetan Plateau. Similarly, the text in some places in page 55 (line 1 to 10) refers the region as northwest Himalaya, eastern Himalayas and western Himalayas etc. I think we should be consistent in naming the region in both chapters. [Mansour Almazroui, Saudi Arabia]	Take into account. Text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32779	41	54	41	54	please add Western Asia (Sivakumar et al.2013) Sivakumar, M.V.K., A.C. Ruane, and J. Camacho, 2013: Climate change in the West Asia and North Africa region. In Climate Change and Food Security in West Asia and North Africa. M.V.K. Sivakumar, R. Lal, R. Selvaraju, and I. Hamdan, Eds. Springer Netherlands, pp. 3-26, doi:10.1007/978-94-007-6751-5_1. [sadegh zeyaeyan, Iran]	Considered, there are a few papers already cited for the region in Table 11.5. For this region, this book chapter was not cited.
33109	41	54	41	54	please add Western Asia (Sivakumar et al.2013) Sivakumar, M.V.K., A.C. Ruane, and J. Camacho, 2013: Climate change in the West Asia and North Africa region. In Climate Change and Food Security in West Asia and North Africa. M.V.K. Sivakumar, R. Lal, R. Selvaraju, and I. Hamdan, Eds. Springer Netherlands, pp. 3-26, doi:10.1007/978-94-007-6751-5_1. [Sahar Tajbakhsh Mosalman, Iran]	Considered, there are a few papers already cited for the region in Table 11.5. For this region, this book chapter was not cited.
21089	41	54	41	54	please add Western Asia (Sivakumar et al.2013) Sivakumar, M.V.K., A.C. Ruane, and J. Camacho, 2013: Climate change in the West Asia and North Africa region. In Climate Change and Food Security in West Asia and North Africa. M.V.K. Sivakumar, R. Lal, R. Selvaraju, and I. Hamdan, Eds. Springer Netherlands, pp. 3-26, doi:10.1007/978-94-007-6751-5_1. [Farnaz Pourasghar, Iran]	Considered, there are a few papers already cited for the region in Table 11.5. For this region, this book chapter was not cited.
82773	42	1	42	5	Notwithstanding these results over the post-1950 period, some parts of southern Australia have shown stable or increased numbers of frosts since the 1980s as the influence of drying trends and strengthening of the subtropical ridge has become apparent (Dittus et al 2014, http://www.bom.gov.au/jshess/docs/2014/dittus.pdf). (There are also Crimp et al papers in this space, but those use unhomogenised data and are less useful in this context). This is covered at P44 L16-20 but is probably also worth mentioning here to reconcile what might otherwise appear to be an inconsistency between the two sections. [Blair Trewin, Australia]	Considered. Mentioning about frost days is added in this paragraph.
9221	42	3	42	5	add "but the converse is found for trends since 1960 (Trewin et al, submitted)". Cite Blair Trewin, Karl Braganza, Robert Fawcett, Simon Grainger, Branislava Jovanovic, David Jones, David Martin, Robert Smalley, Vanessa Webb. An updated long-term homogenised daily temperature data set for Australia. Submitted to JDG. [Kevin Hennessy, Australia]	Noted
9149	42	5	42	7	In Australia, very high monthly maximum or minimum temperatures that occurred around 2 per cent of the time in the past (1951–1980) now occur around 12 per cent of the time (2003–2017) (BoM and CSIRO, 2018: http://www.bom.gov.au/state-of-the-climate/). In New Zealand, number of frost days (below 0 degrees Celsius) decreased and the number of warm days (over 25 degrees Celsius) increased at around one-third of measured sites over the period 1972–2016 (MfE, 2017 http://www.mfe.govt.nz/sites/default/files/media/Environmental%20reporting/our-atmosphere-and-climate-2017.pdf) [Kevin Hennessy, Australia]	Noted
82775	42	7	42	9	A more up-to-date (1951-2015) reference for this result is McGree et al 2019, https://doi.org/10.1175/JCLI-D-18-0748.1 [Blair Trewin, Australia]	Accepted
108863	42	11	42	17	Add reference to the following study, which demonstrates that there is dectable signal in frequency and intensity of hot and cold extremes in Europe. Lorenz, Ruth, Zélie Stalhandske, and Erich M. Fischer. "Detection of a climate change signal in extreme heat, heat stress, and cold in Europe from observations." Geophysical Research Letters 46.14 (2019): 8363-8374. [Erich Fischer, Switzerland]	Accepted
6879	42	15	42	15	After "Nastos and Kapsomenakis, 2015; Fioravanti et al., 2016; Ruml et al., 2017)" please add "and in southeast Europe (Kuglitsch, F.G., A. Toreti, E. Xoplaki, P.M. Della-Marta, C.S. Zerefos, M. Türkes, J. Luterbacher "Heat wave changes in the eastern Mediterranean since 1960", Geophys. Res. Lett., 37, L04802, doi: 10.1029/2009GL041841, 2010). [Christos Zerefos, Greece]	Noted
55495	42	19	42	28	please consider these references: Piticar, A., 2018:, doi:10.1016/j.gloplacha.2018.08.007. Ceccherini, G. et al., 2016: , doi:10.5194/nhess-16-821-2016. Vicente-Serrano, S. M. et al., 2018: , doi:10.1002/joc.5176.Meseguer-Ruiz, O. et al., 2018:doi:10.1007/s00477-018-1557-6. [Matilde Rusticucci, Argentina]	Considered. References are cited in FGD.
108147	42	24	42	26	Please, revise this statement. It is contradictory to the information in Table 11.7: Regional assessments for Central and South America, Region: Southeastern South America (SES) Section, Column: Temperature Extremes, Observed Trends, where it says "Increase in the frequency of warm extremes and decrease in the frequency of cold extremes over most of SES …" The frequency of warm extremes (TX90p and TN90p) have increased. [Maria Bettolli, Argentina]	Noted, text revised.
108149	42	24	42	26	I suggest adding the following citation: Olmo M, Bettolli ML, Rusticucci M. Atmospheric circulation influence on temperature and precipitation individual and compound daily extreme events: spatial variability and trends over southern South America. Weather and climate extremes. Submitted December 2019. Olmo et al 2019 found significant positive trends for TX90p and TN90p and significant negative trends for TX10p over SES region based on station data and CPC gridded data (1979-2015). [Maria Bettolli, Argentina]	Accepted
38419	42	33	42	33	In the US, some stations show a cooling in monthly maximum temperatures. Is this statement from station data or any processed data? For example Lee et al. (2014) uses Head-banging smoothed GEV trends in their Fig. 11 as "Head-banging smoothed GEV trends of U.S. monthly MaxTmax series (°C century−1). The eastern United States shows cooling and the western United States shows warming". [Mansour Almazroui, Saudi Arabia]	Not Applicable. Text removed
62703	42	34	42	35	In my opinion the sentence would benefit from a reference. I am also not sure whether the sentence is really intended to refer to increase in monthly temperatures or in monthly maximum temperatures as talked about in the previous sentence? I would suppose the sentence refers to the last sentence of the Abstract in Lee et al. 2014, in that case I assume this statement refers to the increase in monthly maximum temperatures [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not Applicable. Text removed
100491	42	37	42	38	To highlight recent new evidence in this context, this sentence could be reworded to: 'In addition, observational and model evidence suggests that irrigation expansion and cropland intensification also played a role in masking the warming of hot extremes in this region (Mueller et al., 2016; Thiery et al., 2017; Thiery et al., 2020; Chen and Dirmeyer, 2019).'. REF: Mueller, N. D., Butler, E. E., McKinnon, K. A., Rhines, A., Tingley, M., Holbrook, N. M., & Huybers, P. (2016). Cooling of US Midwest summer temperature extremes from cropland intensification. Nature Climate Change, 6(3), 317-322.; Thiery, W., Visser, A. J., Fischer, E. M., Hauser, M., Hirsch, A. L., Lawrence, D. M., ... & Seneviratne, S. I. (2020). Warming of hot extremes alleviated by expanding irrigation. Nature Communications, 11(1), 1-7.; Chen, L., & Dirmeyer, P. A. (2019). Global observed and modelled impacts of irrigation on surface temperature. International Journal of Climatology, 39(5), 2587-2600. [Wim Thiery, Belgium]	Not Applicable. The content is no longer included here
62431	42	69	42	69	Huang et al. (2017) missing in the reference perhaps a missing letter here page 69 line 42 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. There is no Page 42 Line 69
38145	43	1	43	5	Studies on observed heat wave trends in Korea can be included here for better representation of its changes in Asia. [Junhee Lee, Republic of Korea]	Noted
6865	43	11	43	11	Delete the word "virtually" [Christos Zerefos, Greece]	Rejected. "virtually certain" is a calibrated phrase.
62707	43	11	43	12	I am wondering on which basis the term "virtually certain" is used here in the summary. The study by Alexander at al. 2016 only shows statistically significant at the 5% level which would correspond to very likely. Also Figure 11-7 from the submitted study by Dunn et al. Doesnt include statistical significance so far. But maybe I missed something ? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. This is expert judgement based on many papers.
3151	43	13	43	13	"likelythat" should be changed to "likely that" [Hui Wang, China]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42481	43	13	43	13	Typo: likelythat -> likely that [Joan Bech, Spain]	Accepted
43333	43	13			Read "It is very likely that " rather than "It is very likelythat " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
23141	43	15	43	16	I'm not sure that there was an adequately clear trace as written in the preceding text to support this finding. [Peter Thorne, Ireland]	Not Applicable. Text removed
6867	43	20	43	20	Something should be said on north America as well. There are a number of papers which studied trends. [Christos Zerefos, Greece]	Accepted
106751	43	23	45	50	This section highlights an important disproportion in literature citation. Results for Africa (compared to other regions) have been under cited (if only one); and indeed, they are numerous related to CMIP5 and CORDEX outputs evaluation. They should be considered, in order to balance litterature citation between regions. [Moustapha Tall, Rwanda]	Taken into account. Many papers are added.
71467	43	23			In Chapter 10 we discuss aspects which are relevant here, e.g., the representation of soil moisture temperature feedbacks in different types of models (mainly section 10.3.3.6). There should be a link somewhere in this section. [Douglas Maraun, Austria]	Noted. Section 10.3.3.6 focuses on under what condition (or what forcing must be included) a model will perform well. While this is relevant, it is not within the scope of discussion here. Ideally, we shall include/discuss everything that is relevant. But a decision still needed to be make about what to focus on. At the end, it was decided not to cite as it would take quite a bit words to make the connection without clear gain by doing so.
108867	43	25	43	54	I am surprised that there is no mention of internal variability. Fully-coupled AOGCMs and observations should not be expected to agree in their representation of regional trends and even in 20-yr means due to internal variability. This is illustrated in many large ensemble studies, e.g. for TXx and TNn in Fischer, E. M., and R. Knutti (2014), Detection of spatially aggregated changes in temperature and precipitation extremes, Geophysical Research Letters, 41(2), 547-554.or for heatwaves in Perkins, S. E., and E. M. Fischer (2013), The usefulness of different realizations for the model evaluation of regional trends in heat waves, Geophysical Research Letters, 40(21), 5793-5797. [Erich Fischer, Switzerland]	Considered. While it is ideal to also include the effect of internal variability on trends and short-term mean, it was decided to have a more narrow focus, discussing mostly on mean but also include trends for long time scale. The level of model agreement in long-term change is also trended in attribution sesstions which is another way of model evaluation.
102549	43	30	43	30	"a few degrees" - > is this really "reasonably well"? Suggest to rephrase. [Philippe Tulkens, Belgium]	Taken into account. Text revised
70959	43	32	43	35	Tamarin-Brodsky et al. have shown (2019, already cited, for the SH; 2020, doi: 10.1038/s41561-020-0576-3, for the NH) that the CMIP5 models do a remarkably good job, overall, in simulating the observed regional distribution of temperature variance and skewness [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted
42483	43	43	43	43	Typo: simulateing -> simulating [Joan Bech, Spain]	Not Applicable. It is no longer included
43335	43	43			Read "the ensemble simulating intensity indices" rather than "the ensemble simulateing intensity indices" [Cyriaque Rufin Nguimalet, Central African Republic]	Not Applicable. It is no longer included
23143	43	44	43	46	Is this another example? If so say so. Otherwise why is this case specifically called out in this manner and at this juncture? [Peter Thorne, Ireland]	Not Applicable. It is no longer included
102551	44	10	44	13	No benefit from CMIP6 what so ever? Suggest to elaborate. [Philippe Tulkens, Belgium]	Taken into account. Text revised
51621	44	10	44	13	By "interchangeable" do you mean similar? I think that word would be easier for non-experts to understand. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
125913	44	23	44	23	Is "... median RMSEmedian,std for CMIP6 ..." an editorial error? [Trigg Talley, United States of America]	Not applicable - panel removed
125915	44	33	46	2	[CONFIDENCE] Additional detail is needed with regard to the summary on page 11-45, line 52. The preceding section describes numerous factors for which models fail to reproduce extreme conditions, yet the summary states there is high confidence that the models reproduce "climatology and overall warming in temperature extremes." "Climatology" is broad and seems too expansive to accurately reflect the preceding discussion. [Trigg Talley, United States of America]	Considered. These are described in the text already.
62705	44	35	44	36	The sentence refers to the “longer Period” which is not introduced in the text. I think they refer to the periods in the study by Sillmann et al. 2014 (1971-2010) and (1996-2010) which might be important to mention since at least for one 15 year Period in the study the global mean Txx falls outside the 5-95 % model range (Figure 3 in Sillmann et al. 2014). However, I am not sure whether it also falls outside of the 0-100% model range, which I assume is meant by “spread of simulated trends in CMIP5”. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not Applicable. Text removed
108869	44	36	44	40	Consider citing the following papers which demonstrate that the global land fraction of positive trends for TXx is overestimated and underestimated for TNn. Fischer, E. M., and R. Knutti (2014), Detection of spatially aggregated changes in temperature and precipitation extremes, Geophysical Research Letters, 41(2), 547-554 and Borodina, A., Fischer, E.M. and Knutti, R., (2017). Potential to Constrain Projections of Hot Temperature Extremes. J. Climate, 30(24), 9949-9964, doi:10.1175/JCLI-D-16-0848.1. [Erich Fischer, Switzerland]	Accepted
23145	44	43	44	45	Why not refer to the substantive assessment in cross-chapter box 3.1 rather than a small random selection of papers and so-called is a little value-laden? [Peter Thorne, Ireland]	Not Applicable. Text removed
108871	44	48	44	49	This paper suggests that there is a systematic underestimation of trends in TNn Fischer, E. M., and R. Knutti (2014), Detection of spatially aggregated changes in temperature and precipitation extremes, Geophysical Research Letters, 41(2), 547-554 [Erich Fischer, Switzerland]	Accepted. Reference added
70961	44	51	44	53	Horton et al. (2015 doi: 10.1038/nature14550) and Kretschmer et al. (2018 doi: 10.1175/BAMS-D-16-0259.1) would also be good references for this point. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Not Applicable. Text removed
117097	44		44		no need to replicate the hiatus assessment, just refer to chapter 2 CCB. [Valerie Masson-Delmotte, France]	Not Applicable. It is no longer included
108873	45	4	45	16	A lot of these statement may be highly sensitive to internal variability. A lot of the recent literature based on large ensembles suggests that models and observations may substantially disagree simuly due to to internal variability. See e.g. Perkins, S. E., and E. M. Fischer (2013), The usefulness of different realizations for the model evaluation of regional trends in heat waves, Geophysical Research Letters, 40(21), 5793-5797. [Erich Fischer, Switzerland]	Noted
11679	45	10	45	11	“which… which” is awkward, recommend rephrasing. [Amy East, United States of America]	Not Applicable. Text removed
62609	45	13	45	16	Plagiarism issue: the lines are fully copied from https://iopscience.iop.org/article/10.1088/1748-9326/aac3e5. The lines should be within double quotation otherwise should be reprhased/modified. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Text revised
107407	45	20	45	20	also the observed lack of warming in large parts of North America is not captured in these model simulations (same reference, Dittus et al 2018) [Markus Donat, Spain]	Accepted
38147	45	27	45	31	There are missing studies from CORDEX East Asia which are worth citing. [Junhee Lee, Republic of Korea]	Taken into account. Added relevant references
72103	45	27	45	46	This is another example of unbalanced literature citation. Here, 9 studies are cited for East Asia, 2 for Europe and 1 for Africa. Please balance. [Mouhamadou Sylla, Rwanda]	Considered. We were able to add a few more papers for Africa.
42321	45	35	45	36	Most models, not all, lack aerosol changes [robert vautard, France]	Taken into account. Text revised
108875	45	35	45	39	See also the following paper for an evaluation of trends in heat extremes in EURO-CORDEX Lorenz, Ruth, Zélie Stalhandske, and Erich M. Fischer. "Detection of a climate change signal in extreme heat, heat stress, and cold in Europe from observations." Geophysical Research Letters 46.14 (2019): 8363-8374. [Erich Fischer, Switzerland]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42323	45	35	45	39	When considering the large EURO-CORDEX ensemble, RCMs do not exhibit specifically different signal from GCMs on heat extremes, but cold extremes are sensitive to RCM formulations • Vautard, R., N. Kadygrov, C. Iles, F. Boberg, E. Buonomo, K. Bülow, E. Coppola, L. Corre, E. van Meijgaard, R. Nogherotto, M. Sandstad, C. Schwingshackl, S. Somot, E. Aalbers, O. B. Christensen, James M. Ciarlo`, M.-E. Demory, F. Giorgi, D. Jacob, R. G. Jones, K. Keuler, E. Kjellström, G. Lenderink, G. Levavasseur, G. Nikulin, J. Sillmann, S. Lund Sørland, C. Solidoro, C. Steger, C. Teichmann, K. Warrach-Sagi, V. Wulfmeyer, 2019: Evaluation of the large EURO-CORDEX regional climate model ensemble, J. Geophys. Res., sub judice. [robert vautard, France]	Noted
23655	45	47	45	50	Regarding the modeling of temperature extremes, Bozkurt et al. (2019) showed that 10 km regional climate model simulations represent a better spatial and temporal variability of frost days compared to the coarser resolutions over the southern Chile where the Patagonian Ice Fields are located. Bozkurt, D., Rojas, M., Boisier, J.B., Rondanelli, R., Garreaud, R., Gallardo, L., 2019. Dynamical downscaling over the complex terrain of southwest South America: Present climate conditions and added value analysis. Climate Dynamics, 53, 6745–6767, doi:10.1007/s00382-019-04959-y. [Deniz Bozkurt, Chile]	Considered, Bozkurt et al. is cited in the FGD version
108151	45	48	45	50	I would suggest adding the following citation: Tencer B., Bettolli ML., Rusticucci M., 2016. Compound temperature and precipitation extreme events in Southern South America: associated atmospheric circulation and simulations by a multi-RCM ensemble. Clim. Res. 68, 183–199. Tencer et al 2016 evaluated the performance of different RCMs in representing TX90p, TN90p, TX10p and TN10p over La Plata Basin Region. [Maria Bettolli, Argentina]	Accepted
39285	46	17	46	34	Please synthesize-It looks like a literature review. [Lourdes Tibig, Philippines]	Taken into account. Text revised
65079	46	24	46	38	It could be considered premature to reference unpublished work [Magnus Joelsson, Sweden]	Noted. Work has been published.
62633	46	25	46	25	statement describes CMIP6 simulation during 1951-2015. CMIP6 historical simulation is up to 2014. So, it is totally confusing why the mentioned study uses one year of projection simulation. And even used one year of projection, the immediate question will come for which SSP? Nothing is mentioned..as a result it does create confusion and validity of this method including just one year of projection data. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Text revised
42485	46	25	46	25	Typo: tempertaures -> temperatures [Joan Bech, Spain]	Not Applicable. Text was reorganized.
8697	46	25	46	25	spelling "temperatures" (first word) [Robert Dunn, United Kingdom (of Great Britain and Northern Ireland)]	Not Applicable. Text was reorganized.
8041	46	27	46	29	Unclear: what does "as much 75% of the … extremes … are about five times higher" mean? [jouni Räisänen, Finland]	Taken into account. Text revised
62709	46	27	46	29	I am not sure whether I understand this sentence correctly. For me the study by Fischer and Knutti 2015 claims that at the current warming of 0.85 degree Celsius, 75 % of all moderate daily hot extremes are attributable to the warming which roughly translates to a five time higher probability of them occurring now than during pre-industrial climate. For me it is not about how “high” the moderate daily hot extremes are, which is how I understand the sentence in the report. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Text revised
108877	46	36	46	38	Add references to the corresponding papers [Erich Fischer, Switzerland]	Accepted. References added
39287	46	36	46	53	Same comment [Lourdes Tibig, Philippines]	Taken into account. Text revised
62635	46	40	46	40	statement describes CMIP6 simulation during 1951-2015. CMIP6 historical simulation is up to 2014. So, it is totally confusing why the mentioned study uses one year of projection simulation. And even used one year of projection, the immediate question will come for which SSP? Nothing is mentioned..as a result it does create confusion and validity of this method including just one year of projection data. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Text revised
42487	46	41	46	41	Typo: Austraila -> Australia [Joan Bech, Spain]	Not Applicable. Text was removed and reorganized
62637	46	50	46	51	“different global region” is confusing. Do you mean regionally different? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Text revised
1441	47	1	47	6	As far as I know, the first analysis of record-breaking statistics in climate science was Benestad (2003; DOI: 10.3354/cr025003) that compared the time series with an iid (independent and identically distributed) process in chronological and reverse chronological order. The record-breaking statistics also offers a means to analyse the tails of the statistical distribution (Benestad 2008; DOI: 10.1029/2008EO410002). The analysis of record-breaking events has since been extended to record-low events in addition to record-high (for temperature). I think it’s fair to credit the work behind the recent studies, which also is in line with traceability and comparison to earlier assessments. [Rasmus Benestad, Norway]	Noted, but pre-AR5 work is generally not assessed in AR6.
39289	47	1	48	46	A synthesis wth uncertainty language is preferred to literature survey/review, please. [Lourdes Tibig, Philippines]	Taken into account. Text revised
104589	47	3	47	3	A paper is worth quoting here on attributing a century-record-breaking heatwave event in summer 2017 over Shanghai region with highly dense population. Rather than other papers, this paper used the longest 145 years of observations from 1873 to 2017 to study the summer heat. Reference: Zhou, C., K. Wang, D. Qi, and J. Tan, 2019: Attribution of a record-breaking heatwave event in summer 2017 over Yangtze River Delta. Bull. Am. Meteorol. Soc., 100, 97-103. [Chunlüe Zhou, United States of America]	Accepted. Reference added
11681	47	5	47	5	the King et al. 2017 reference appears to be incomplete in the reference list. [Amy East, United States of America]	Accepted. Added
1443	47	14	47	15	The "law of small numbers" implies that model studies "based on simulations by two climate models" does not give a representative picture if that also implies only two simulations. Deser et al (2012; DOI: 10.1038/nclimate1562) has demonstrated that stochastic/chaotic regional internal variations may swamp the signal on time scales up to decades. Hence, we must expect a large ensemble to be able to discern a signal. This is analogous to working with a tiny sample with large random sampling fluctuations in statistics. There is not much to learn from small samples and it's easy to get mislead (e.g. Kahnemann, "Thinking fast and slow"). [Rasmus Benestad, Norway]	Taken into account. Text revised
104591	47	16	47	16	A new additional evidence could be adopted here with specific text written after '... multiple model simulations were used.' as: 'Zhou et al., (2020) found anthropogenic effects in severe cold outbreaks in the eastern United States from human-induced dynamic and thermodynamic perspectives by using ten CMIP6 models.' Reference: Zhou, C., A. Dai, J. Wang, and D. Chen, 2020: Quantifying human-induced dynamic and thermodynamic contributions to severe cold outbreaks like November 2019 in the eastern United States. Bull. Am. Meteorol. Soc., under review. [Chunlüe Zhou, United States of America]	Noted, paper cited but text is not added as suggested.
81669	47	17	47	20	The driver of an increase in frost events is certainly linked in part to decrases in cloud and rainfall. But the nature of the circulation driver is explained by an increase in the intensity of the subtropical ridge only in a general sense. It would be useful here to mention the potential changes to the speciifc drivers of frost, such as described in Ashcroft, L., Pezza, A. and Simmonds, I. (2009). Cold Events over Southern Australia: Synoptic Climatology and Hemispheric Structure. Journal of Climate 22: 6679-6698. and Pook, M.J., Risbey, J.S. and McIntosh, P.C. (2011). The Synoptic Climatology of Cool-Season Rainfall in the Central Wheatbelt of Western Australia. Monthly Weather Review 140: 28-43. [Michael Grose, Australia]	Noted, but pre-AR5 work is generally not assessed in AR6.
13709	47	18	47	18	change & by and [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Text revised
42489	47	18	47	18	Citation format: Dittus, Karoly, Lewis, & Alexander, 2014 -> Dittus et al. 2014 [Joan Bech, Spain]	Accepted. Text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42491	47	22	47	22	English: There are a large number of studies -> There is a large number of studies [Joan Bech, Spain]	Taken into account. Text revised
82779	47	36	47	42	As other humidity-related findings are in section 11.8.3, this might sit better there too? [Blair Trewin, Australia]	Noted, attribution of changes in WBGT is used as supporting evidence here.
104593	47	42	47	42	A novel result would be better to place before 'In addition to ...' as: 'Zhou et al. (2020) found anthropogenic contributions in the increased probability of summer extreme heat in Northeast China from dynamic and thermodynamic point of view, which provides a physical way to interpret formation and evolution of temperature extremes.' Reference: Zhou, C., D. Chen, K. Wang, A. Dai, and D. Qi, 2020: Conditional attribution of the 2018 summer extreme heat over Northeast China: Roles of urbanization, global Warming, and warming-Induced circulation changes. Bull. Am. Meteorol. Soc., 101, 71-76. [Chunlüe Zhou, United States of America]	Considered. Paper is cited.
38149	47	42	48	4	Need to include recent event attribution studies of warm extremes over Korea. Also, many recent studies including Herring et al. 2019 and 2020 now use rigorous methods for uncertatiny estimation of the risk ratio or probability ratio. [Junhee Lee, Republic of Korea]	Taken into account. Text revised
117099	47		47		please see chapter 4 regarding heat stress to check consistency [Valerie Masson-Delmotte, France]	Taken into account. Text revised
1445	48	6	48	18	Perhaps explain more carefully why we have seen such cold spells over e.g. North America. Due to the polar vortex and advection of cold polar air? [Rasmus Benestad, Norway]	Taken into account. Text revised
39801	48	8	48	8	"On small spatial scales..." -> Specify the spatial scale [TSU WGI, France]	Taken into account. Text revised
38151	48	10	48	16	Regarding cold extremes over the eastern US and East Asia, some recent studies suggest possible anthropogenic influences via Arctic warming or Arctic sea-ice melting. This needs to be noted for more complete assessments. [Junhee Lee, Republic of Korea]	Noted. The influence of Arctic warming or Arctic sea-ice melting is assessed in 11.3.1
104595	48	13	48	13	An important and different result based on cold outbreaks and the use of CMIP6 models would be cited after '... Bellprat et al., 2016).' as: 'In contrast, Zhou et al. (2020) found the appearance of anomalous norherly winds often leads to severe cold outbreaks in Novermber in eastern US and anthropogenic forcings as a whole might increase the prbability by a combination of anthropogenic dynamic contribution to the increased probability and anthropogenic thermodynamic contribution to the decreased probability via the use of ten CMIP6 models. This case study shows anthropogenic dynamic contribution might dominates over anthropogenic thermodynamic contribution in the probability changes in cold extremes. The separation of anthropogenic dynamic and thermodynamic could be helpful to reconcilel the different interpretations of cold and warm extremes under global warming.' Reference: Zhou, C., A. Dai, J. Wang, and D. Chen, 2020: Quantifying human-induced dynamic and thermodynamic contributions to severe cold outbreaks like November 2019 in the eastern United States. Bull. Am. Meteorol. Soc., under review. [Chunlüe Zhou, United States of America]	Noted
44395	48	17	48	18	replace "risk" with "probability". See IPCC guidance on risk for appropriate use of the term "risk" throughoput the IPCC report. [Jana Sillmann, Norway]	Accepted. Text revised
23949	48	34	48	35	This sentence does not seem to have considered much regional evidence for India. For example, Rohini et al. (2016, https://www.nature.com/articles/srep26153) suggests that heatwave intensity and duration have increased since the 1950s over central and NE India, which contradicts the findings stated here. (Infact, your chapter has already cited the Rohini study in the table on page 125, with the wording, "Increase in the intensity and frequency of heatwaves", which contradicts the sentence here.) See also several references in the introduction of Mishra et al. (2017; https://iopscience.iop.org/article/10.1088/1748-9326/aa9388/meta). [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Considered. The relevant wording is revised in FGD. Note that this paragraph is focused on attribution.
6869	48	36	48	36	After (Wehner et al., 2018c). please add "Global dimming and brightening due to aeorsols have been discussed at Zerefos, C.S., K. Eleftheratos, C. Meleti, S. Kazadzis, A. Romanou, C. Ichoku, G. Tselioudis, A. Bais, "Solar dimming and brightening over Thessaloniki, Greece, and Beijing, China", Tellus B, DOI: 10.1111/j.1600-0889.2009.00425.x, 2009. [Christos Zerefos, Greece]	Rejected. Not related to the content of this section and the published year of this reference is before 2013
76683	48	37		38	This was actually already shown by Zampieri M, Lionello P (2011) Anthropic landuse cools down summer season in Europe. Clim Res , 46:255-268. doi:10.3354/cr00981 [Piero Lionello, Italy]	Noted
8001	48	41	48	41	Alessandri et al (in review) point at deforestation effects to be dependent on the albedo contrast between canopy and soil. For some (high latitude) soils the albedo is lower than the canopy albedo, which would lead to an opposite warming effect than when the albedo is higher than vegetation (as normally assumed) [Bart van den Hurk, Netherlands]	Noted
38153	48	50	48	50	It's uncelar what "other relevent characteristics" represent other ethan intensity, frequency, duration. This would be important to identify what's new since AR5. Please clarify for clearer communication. [Junhee Lee, Republic of Korea]	Taken into account. Text revised
24097	48	51	48	53	"Event attribution studies on temperature extremes point to human inlfuence on extreme heat-related events, regardless of framing, methods, definitions of events and regions", This conclusion is not sufficiently traceable and is poorly expressed. I would expect such a conclusion to be supported by an inventory of events for which studies find a human influence and for which this assessment is that there is high confidence (for example because there are independent studies assessing the same event) but from the text it is hard to see what such an inventory would look like. Also the "regardless of framing, methods, definitions of events and regions" phrase is poorly expressed because one event can't be in more than one region, yet framing, methods and definitions could apply to an event. Is the sentence meant to express that studies find human influence in all regions? But then that wouldn't be supported by literature on Indian events that appears to not find human influence there (lines 34-35). In any event "studies on temperature extremes point to human influence" is a pretty vacuous statement, when what is needed here is an assessment like statement on whether the likelihood or magnitude of specific extreme heat events (in which regions?) has been assessed to have been increased (significanty or by some amount?) by human influence on climate. I think a concluding statement on events would be valuable but it needs to be re-drafted. It looks like the Tables in Section 11.9 provide details of specific events and could be referred to. A summary statement on events could provide a "for example" clause (then listing some events for which there is high confidence in attribution). [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51623	48	51	48	53	"Event attribution studies on temperature extremes point to human inlfuence on extreme heat-related events, regardless of framing, methods, definitions of events and regions", This conclusion is not sufficiently traceable or clearly expressed. I would expect such a conclusion to be supported by an inventory of events for which studies find a human influence and for which this assessment is that there is high confidence (for example because there are independent studies assessing the same event) but from the text it is hard to see what such an inventory would look like. Also the "regardless of framing, methods, definitions of events and regions" phrase is poorly expressed because one event can't be in more than one region, yet framing, methods and definitions could apply to an event. Is the sentence meant to express that studies find human influence in all regions? But then that wouldn't be supported by literature on Indian events that appears to not find human influence there (lines 34-35). In any event "studies on temperature extremes point to human influence" is a pretty vacuous statement, when what is needed here is an assessment like statement on whether the likelihood or magnitude of specific extreme heat events (in which regions?) has been assessed to have been increased (significanty or by some amount?) by human influence on climate. I think a concluding statement on events would be valuable but suggest it needs to be re-drafted. It looks like the Tables in Section 11.9 provide details of specific events and could be referred to. A summary statement on events could provide a "for example" clause (then listing some events for which there is high confidence in attribution). [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text revised
10929	48	51	48	53	"Event attribution" usually requires the reasonable assumption that anthropogenic influences have an influence on the frequency and magnitude of a number of different types of extreme events. So an "event attribution" analysis can't be used to support the assumption used in the first place. Can it? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text revised
125917	48	53			"... extremely likely that human influence is the main contributor ... to observed increase in hot extreme and decrease in cold extremes ..." This seems like too strong a statement given the uncertainties. AR5 was able to say this for global mean temperature, but there they had multiple studies estimating the attributable anthropogenic contribution and could build a good pseudo-pdf on which to base the statement. That seems to be missing here, yet a similar "extremely likely, main contributor" language is being used. This seems not justified. [Trigg Talley, United States of America]	Considered. The statement is backed up by multiple post-AR5 studies that used both CMIP5 and CMIP6 data, more recvent observations since AR5. No action is taken
39883	49	4	49	4	"Urbanization has exacerbated the effects of global warming in cities."-> urbanisation is covered in 11.3.4. either remove or move to other chapters/sections where this is covered [TSU WGI, France]	Accepted
38155	49	4	49	6	It would be more useful to be able to specify which regions are affected by aerosol forcing and possibly how much weakening of hot extremes. [Junhee Lee, Republic of Korea]	Considered. While it is ideal to have quantitative statement here, there is not enough space in the summary paragraph to provide such details and sometimes it may not be possible to provide robust estimate. But the text provides more details.
39481	49	6	49	8	Consider to add the words 'no-till farming' to 'irrigation, and crop expansion', which also refers to page 40, line 2 to 3 where it is stated that 'Some aspects of agricultural management, including no-till farming, irrigation and oerall crop intensification are likely to cool hot temperature extremes' and also see page 18 line 25 to 28 also refers to 'no-till farming', which may cool hot days. [Tamara van 't Wout, Qatar]	Accepted
11683	49	7	49	7	delete “the” before “central North America” [Amy East, United States of America]	Accepted
18367	49	11	52	42	section 11.3.5: Chen et al. (2019) found that future PDFs of daily near-surface air temperature anomalies (relative to their own climatology for the future and current periods) may become wider in the low-latitudes while they may become narrower at high latitudes. Such changes have major implifications for temperature extremes and other related extremes such as water vapor extremes. For example, the PDFs changes are associated with increased frequency of both extreme cold and extreme hot temperatures at low latitudes but reduced frequencys of cold and hot temepratures when the mean temperature change is excluded. These findings seem to be relevan to this section. Chen, J., A. Dai, and Y. Zhang, 2019: Projected changes in daily variability and seasonal cycle of near-surface air temperature over the globe during the 21st century. J. Climate, 32, 8537-8561. https://doi.org/10.1175/JCLI-D-19-0438.1. [Aiguo Dai, United States of America]	Noted. The chapter does not explicitly assess changes in variability, but changes in variability would have be reflected in projected changes in extreme temperature assessed here.
108879	49	13	49	21	I strongly suggest to highlight that internal variability maybe regionally offsetting or amplifying the forced response. For 1.5 and 2°C there may be decades of no trend or a decrease over some areas simply due to internal variability. It is relevant to point this out in order to avoid misinterpretation as it had happened with the global warming hiatus. [Erich Fischer, Switzerland]	Noted. Inference of internal variability for near-term or lower level of global warming would have been reflected in the uncertainty/spread of the projection. What is discussed here is about median/mean changes but what you are suggesting is about a specific realization.
13711	49	15	49	19	change SR15 by SR1.5 [Maria Amparo Martinez Arroyo, Mexico]	Noted. Use SR15 consistently in the text body
62639	49	23	49	24	GCM and RCM are already defined earlier. Do not define multiple times in the text, it will confuse the readers [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not Applicable. Text was changed
108883	49	23	49	34	Note that some recent studies that projected changes in hot extremes may be overestimated and provide constraint, e.g. Sippel, Sebastian, et al. "Refining multi-model projections of temperature extremes by evaluation against land-atmosphere coupling diagnostics." Earth System Dynamics 8.2 (2017): 387-403., Vogel, M. M., Orth, R., Cheruy, F., Hagemann, S., Lorenz, R., van den Hurk, B. J., & Seneviratne, S. I. (2017). Regional amplification of projected changes in extreme temperatures strongly controlled by soil moisture-temperature feedbacks. Geophysical Research Letters, 44(3), 1511-1519. Borodina, A., Fischer, E.M. and Knutti, R., (2017). Potential to Constrain Projections of Hot Temperature Extremes. J. Climate, 30(24), 9949-9964, doi:10.1175/JCLI-D-16-0848.1 [Erich Fischer, Switzerland]	Accepted. Text revised
79801	49	23	49	34	In contrast to Vogel et al., Brown (submitted) shows the sign of heatwave changes, when defined against future climatologies, are regionally dependant with heatwave severities (the time integrated temperature anomaly) not changing for 11 out of 20 populous cities, increasing for 7 and decreasing for 2. This shows that for many regions the future increase in heatwave temperature is due or mostly due to the whole body of the temperature distribution increasing (or more accurately the threshold defining a heatwave) and not due to the rare events themselves being enhanced. Durations are also, when heatwaves are defined agains future climatologies, found not to change for most of the cities studied (14 out of 20) with only 5 showing increases and one a decrease in heatwave duration. Brown (submitted) also finds the absolute magnitude of temperature increases (ie wrt present day) are independent of heatwave rarity and duration, ie there is no further enhancement the rarer or longer the heatwave. Brown (submitted, Weather and Climate Extremes) is currently in review with the revision due end of July 2020. Xuebin Zhang is the editor overseeing the submission and has a copy of the paper. I can, of course, provide further copies simon.brown@metoffice.gov.uk. or from https://www.dropbox.com/s/xrbzfxtxaoiumt9/futureChangesCityHeatwaves.pdf?dl=0 [Simon Brown, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text revised

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45587	49	31	49	33	Suarez-Gutierrez et al 2020 (https://link.springer.com/article/10.1007/s00382-020-05233-2) also elaborates on the fixed versus moving threshold extreme event definition, this is an aspect that not only affects the characteristics of extremes, it affects the fidelity of our projections and impact analysis. Considering that what is extreme now would be equally as extreme in the future is not very realistic, instead considering that the threshold for extremes changes somehow with the mean climate conditions would bring interesting and necessary discussions to the impact and adaptation literature too. It would be great to elaborate on this further, also to give pointers to future studies on what would be most relevant to look at in terms of thresholds. [Laura Suarez-Gutierrez, Germany]	Taken into account. Text revised
104921	49	36	49	36	Clarify 'extreme temperatures' - just high temperatures? [John Caesar, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text revised
23951	49	36	49	54	The regional paragraphs on this page do not make reference to the time horizon involved, likely given the focus of the chapter in terms of warming levels but this should be restated clearly here. Nor is it clear whether the studies assessed (e.g. in Tables 11.4-11.6) assess only warming levels or particular time horizons, or, if traditional scenarios/time horizons have been converted to warming levels by the chapter authors and if so how this has been done. No doubt the consideration of whether the projection is being made mid- or end-21st century will affect the confidence of the statement being made. The issue of the impact of differing local forcings (such as aerosols) under the same global radiative forcing (and therefore approximate same warming level) is at least acknowledged (e.g. lines 40-42 on page 36) but it is not clear how this has been quantified at the regional scale. For example, see Fig. 1b in https://www.nature.com/articles/s41561-019-0424-5 to compare the different pathways of aerosol emissions per SSP for India and China, in which they can either be consistent or quite divergent. Such different emissions pathways, within the same overall global radiative forcing, would undoubtedly lead to different manifestations of extremes. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Considered. This is now discussed in 11.2.4
66381	49	36	50	18	This paper could be revised and added as a reference Coppola, E., Raffaele, F., Giorgi, F., Giuliani, G., Xuejie, G., Ciarlo, J., et al. (submittedc). Climate hazard indices projections based on CORDEX-CORE, CMIP5 and CMIP6 ensemble. Clim. Dyn. (submitted). [Erika Coppola, Italy]	Accepted
104925	49	36	50	18	Where possible, can the language and metrics used in the regional summaries be made more consistent to make inter-regional comparison more straightforward? [John Caesar, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text revised. Regional summaries are provided in Section 11.9 which use consistent metric/languages across regions
40581	49	42	49	42	Not clear what a 'hot event' is [TSU WGI, France]	Taken into account. Text revised
104923	49	42	49	43	Frequency and/or magnitude of hot and cold events? [John Caesar, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text revised
39291	49	42	49	46	I am suggesting you take a look at Zhu et al,2020 (Conspicuous temperature extremes over Southeast Asia: sesonal variations under 1.5 °C and 2°C global warming). [Lourdes Tibig, Philippines]	Accepted
9151	49	49	49	49	also cite CSIRO and BoM (2015). See Section 7.1.2 of CSIRO and BoM (2015) Climate change in Australia Technical Report at www.climatechangeinaustralia.gov.au/publications [Kevin Hennessy, Australia]	Accepted
42493	49	54	49	54	Typo: confidenceof -> confidence of [Joan Bech, Spain]	Not Applicable. Text was changed
66379	49	54	50	3	This paper could be revised and added as a reference Coppola, E., Nogherotto, R., Ciarlo, J. M., Giorgi, F., van Meijgaardm, E., Iles, C., et al. (submitted, a). Assessment of the European climate projections as simulated by the large EURO-CORDEX regional climate model ensemble. J. Geophys. Res. - Atmos. (submitted) [Erika Coppola, Italy]	Accepted
98867	49	54	50	5	Bibliography: heatwave projections over Europe from EuroCORDEX simulations: https://doi.org/10.1038/s41598-020-65663-0 [Enrique Sanchez, Spain]	Accepted
31451	49	54	50	6	Projected climate change impact has been assessed on a regionalscale for central Europe and 11 urban areas respectively based on climate indices for the period 2021–2050 using RCM 7km-simulations. Amongst others, hot days and tropical nights, heat waves and heavy preciptation events have been assessed. in line with the report, the number of heat waves, as well as the number of single hot days, tropical nights and heavy precipitation events is projected to increase in the near future. In addition, the number of frost days is significantly decreased. For most urban regions investigated the 95 percentile of air temperature is increased by 1-3°C. Literature: Fallmann, J., Wagner, S., & Emeis, S. (2017). High resolution climate projections to assess the future vulnerability of European urban areas to climatological extreme events. Theoretical and Applied Climatology, 127(3-4), 667-683. [Joachim Fallmann, Germany]	Noted
43337	49	54			Read "there is high confidence of a projected" rather than "there is high confidenceof a projected" [Cyriaque Rufin Nguimalet, Central African Republic]	Not Applicable. Text was changed
33251	50	1	50	1	Consider including as example of great heat waves in Europe the one ocurred in 2019 [Gonzalez Sergi, Spain]	Not Applicable. Text was changed
45589	50	1	50	7	Also Suarez-Gutierrez 2018 (https://iopscience.iop.org/article/10.1088/1748-9326/aaba58/meta) [Laura Suarez-Gutierrez, Germany]	Accepted
76685	50	1		3	You might add to the refs: Lionello, P. and Scarascia L. (2020) The relation of climate extremes with global warming in the Mediterranean region and its North versus South contrast Reg Environ Change 20, doi: 10.1007/s10113-020-01610-z [Piero Lionello, Italy]	Accepted
20735	50	3	50	4	Ice-free arctic summers do not seem to belong to the category of extreme events, do they? [philippe waldteufel, France]	Accepted. Text removed
42495	50	4	50	4	Typo: confidence(Laliberté -> confidence (Laliberté [Joan Bech, Spain]	Not Applicable. Text removed
11685	50	5	50	5	does this refer to warming of both the hot and the cold extremes? The way it's written, this is unclear [Amy East, United States of America]	Not Applicable. Text removed
62711	50	8	50	12	For the whole paragraph about Central and South America there are no confidence levels assigned to the statements. However, in Table 11.7 the statements are made with"high confidence" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Text revised
62829	50	14	50	15	Cross comentary for section 11.3.5 and Table 11-9 (Projections on temperature extremes, CNA). Please consider a better phrasing for the following sentence: "(...) warm (cold) days and warm (cold) nights are very likely to increase (decrease) in all regions." Maybe, re-phrase it as used in Table 11.9 (CNA), for a better understanding. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Text revised
102553	50	14	50	18	Cold regions -> Suggest to add the influence of melt and the geographical movement of the zero-degree border affecting the surface mass balance on the yearly/decadal scales [Philippe Tulkens, Belgium]	Rejected. This would be part of assessment on CID's in Chapter 12.
13713	50	20	50	20	change SR15 by SR1.5 [Maria Amparo Martinez Arroyo, Mexico]	Not Applicable. Text removed
20245	50	23	50	23	Figures 11.9 and 11.10: Is it possible to match more efficiently the colour code to the range of TXx and TNn changes, so as to observe in more detail the evolution with increasing global warming? [philippe waldteufel, France]	Noted, these figures are not used in the final version and no action is taken.
23147	50	55	51	2	This is figure caption material and should be deleted from the main text. Figures have already been introduced. [Peter Thorne, Ireland]	Accepted. Text revised
23149	50	55	51	19	This feels very repetetive with the paragraph prior to the figures. Suggest to merge and reconcile. [Peter Thorne, Ireland]	Accepted. Text revised
108881	51	5	51	5	See also Fischer, E. M., J. Sedlacek, E. Hawkins, and R. Knutti (2014), Models agree on forced response pattern of precipitation and temperature extremes, Geophysical Research Letters, 41(23), 8554-8562. [Erich Fischer, Switzerland]	Accepted
42497	51	10	51	10	Typo: GMSTi.e. -> GMST i.e. [Joan Bech, Spain]	Not Applicable. Text was changed
42499	51	11	51	11	Typo: warming(Appendix -> warming (Appendix [Joan Bech, Spain]	Accepted

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1447	51	12	51	13	The model differences may be due to different representation of sea-ice. Some models simulate too much and others too little, and it is in the regions where the sea-ice retreats (and stronger thermodynamical coupling with the ocean) where the warming is the greatest. [Rasmus Benestad, Norway]	Not Applicable. Text removed
71133	51	21	51	22	The quasi-linearity in warming of extremes as a function of global warming is only applicable to transient warming and not stabilised warming levels. I suggest adding the word "transient" before "global warming" in this sentence. [Andrew King, Australia]	Not Applicable. Text was changed
20737	51	21	51	23	In cases where a function y(x) varies more steeply that if y were proportional to x, it does not follow necessarily that the relation is an exponential one. Is it established that the data cannot be fitted by a power law with an exponent larger than one? [philippe waldteufel, France]	Taken into account. Text revised
51625	51	21	51	33	It's good to see a more risk-based approach being taken as laid out here and seen throughout this chapter - thank you to the authors. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted
1449	51	22	51	23	It is impossible that the probability shows an exponential increase since it has to be bound by the interval [0,1]. It may however converge towards 1 ("S-shape" type behaviour). The frequency may multiply, but it also has an upper bound when the event repeats all the time. It's, however, easier for the frequency of rare events (e.g. extremes) to multiply several times than for more frequenct events. [Rasmus Benestad, Norway]	Taken into account. Text revised
42501	51	27	51	27	Typo: 2018).Such -> 2018). Such [Joan Bech, Spain]	Not applicable. Text was changed
76687	51	29			You might add to the refs: Lionello, P. and Scarascia L. (2020) The relation of climate extremes with global warming in the Mediterranean region and its North versus South contrast Reg Environ Change 20, doi: 10.1007/s10113-020-01610-z [Piero Lionello, Italy]	Accepted. Reference added
23151	51	30	51	33	I do not follow the logic of this statement, paerhaps because I misunderstood some point made earlier in the paragraph? [Peter Thorne, Ireland]	Taken into account. Text revised
96123	51	35	51	36	It may be confusing for non-scientists to read about decreases in return times saying that extreme events occurring more often. A reference to FAQ 11.3, Figure 1 might be helpful. [Nicole Wilke, Germany]	Taken into account. Text revised
45591	51	35	51	44	It would be interesting to compare this CMIP6/CMIP5 estimates with the most extreme events simulated by a low ECS lrge ensemble as MPI-GE in Suarez-Gutierrez et al. 2020b (https://link.springer.com/article/10.1007/s00382-020-05263-w) , to give some frame of reference to the scenario/model versus internal variability uncertainty. [Laura Suarez-Gutierrez, Germany]	Taken into account. Text revised
1453	51	46	51	53	Empirical-statistical downscaling has also been used to get more robust estimates for future heatwaves than RCMs based on large multi-model ensembles (RCPs 2.6, 4.5, 8.5), e.g. in India. The number of heatwaves can be modelled as a Poisson process and the probability their duration exceeding (DOI: 10.3354/cr00924, DOI: 10.1016/j.apgeog.2013.11.008, and DOI: 10.1007/s00382-014-2287-1) e.g. 5 days can be estimated through the means of the geometric distribution. Benestad et al. (2018; DOI: 10.5194/ascmo-4-37-2018) appplied these statistical techniques to make projections for future heatwaves in India with temperature exceedin 35C and lasting more than 5 days (however, the data quality from India was questionable). [Rasmus Benestad, Norway]	Taken into account. Text added.
66383	51	46	52	10	This paper could be revised and added as a reference Coppola, E., Raffaele, F., Giorgi, F., Giuliani, G., Xuejie, G., Ciarlo, J., et al. (submittedc). Climate hazard indices projections based on CORDEX-CORE, CMIP5 and CMIP6 ensemble. Clim. Dyn. (submitted). [Erika Coppola, Italy]	Accepted
82781	51	51	51	53	This wording suggests the behavior of extremes in the two regions is different - is this correct, and if so how? [Blair Trewin, Australia]	Noted. Much of the text is removed in FGD and the comment is not applicable anymore.
20247	52	15	52	20	Figure 11.11: It is necessary to list in this legend the meaning of the acronyms for every subplot (or at least to supply a reference to such a list somewhere in the report); moreover, the world map included in each subplot is hardly readable. [philippe waldteufel, France]	Accepted - now pointing to the Atlas where the regions are written out. Figure redone and small maps where removed. (Note is now Figure 11.3)
6871	52	25	52	25	Replace "virtually certain" with "observed" [Christos Zerefos, Greece]	Not Applicable. Text removed
71307	52	25	52	42	Non-linearity in P.11-51 should be mentioned in the summary. [Kenji Taniguchi, Japan]	Taken into account. Text revised
113599	52	25	52	42	Revise typing errors in this paragraph; there are plenty. [Diego Miralles, Belgium]	Taken into account. Text revised
6873	52	28	52	28	Replace "virtually certain" with "observed" [Christos Zerefos, Greece]	Not Applicable. Text removed
43339	52	30			Read " over most land areas. In most" rather than " over most land areas.In most" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
1451	52	32	52	32	The likelihood, i.e. the probability, is bound in the interval [0,1] and cannot increase exponentially. It can converge to 1 which means towards a state where the events in question take place all the time. One can say that the number of events will multiply, although there is also an upper limit if we have e.g. daily observations - the number of events cannot exceed the number of observations made. The phrase 'exponential increase' may be regarded as a common and loose term, but in this part of the report, it's better to use strict scientific definitions (save the common terms for the key messages). [Rasmus Benestad, Norway]	Taken into account. Text revised
40551	52	32	52	33	No mention in the SPM of the surprising finding from Ch11 that "The likelihood of temperature extremes generally increases exponentially with increasing global warming levels (high confidence)." This seems very worthy of appearing in the SPM. [TSU WGI, France]	Noted. The magnitudes of change in the regions are different.
42503	52	34	52	34	Typo: inprojections -> in projections [Joan Bech, Spain]	Accepted
43341	52	34			Read "high confidence in projections" rather than "high confidence inprojections" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
28951	52	36			also Wilcox et al. (2020) https://doi.org/10.5194/acp-2019-1188, in review but may be accepted in time. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Not Applicable. Text removed
42505	52	40	52	40	Typo: high-latituderegions -> high-latitude regions [Joan Bech, Spain]	Not Applicable. Text removed
42507	52	40	52	40	Typo: regionsis -> regions is [Joan Bech, Spain]	Not Applicable. Text was changed
43343	52	40			Read "and some mid-latitude regions is " rather than "and some mid-latitude regionsis " [Cyriaque Rufin Nguimalet, Central African Republic]	Not Applicable. Text was changed
82787	52	47	53	1	Somewhere in the introduction (or elsewhere), it should be mentioned that an additional challenge in assessing extreme precipitation (relative to temperature) is that the length scales of extreme events are generally shorter than they are for temperature, making it less likely that trends will be spatially coherent and less likely that extremes will be resolved by the observation network. [Blair Trewin, Australia]	Noted but no action is taken. The introduction section is shortened. Also, while there is challenge in identifying trends in extreme precipitation due to lower signal to noise ratio (when compared with temperature), the challenge to the assessment is the availability of relevant literature rather than spatial representativeness of observing network/station.
105447	53	2	53	2	ADD, the results about Peruvian Altiplano, in the paper: (huerta and Lavado, 2020) "Trends and variability of precipitacion extremes in the Peruvian Altiplano", like a results for Andeas Regiona. [Elizabeth SILVESTRE, Peru]	Considered. This paper is cited.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106753	53	4	53	52	Missing literature citations for Africa is also noticed here. In fact, numerous studies have investigated and tried to explain the mechanisms that drive heavy precipitation over the region. Just to name a few: 1°/ Hoell et al, 2018 (https://doi.org/10.1007/s00382-017-3801-z). This paper shown that extreme wet seasons are the consequence of an anomalous lower tropospheric cyclone over south Africa,which sustains convergence and moisture fluxes, while extreme dry seasons are a result of an anomalous lower tropospheric anticyclone that decreases convergence and moisture fluxes into the region. 2°/ Sylla et al. 2015 in Journal of Climate (https://doi.org/10.1175/JCLI-D-14-00854.1) demonstrated a strong link between increased intensity of extreme wet episodes caused by the manifestation of stronger moisture convergence in the boundary layer. etc. [Moustapha Tall, Rwanda]	Noted, no action is taken. This subsection does not refer to details of specific regions. Also, the main focus about the mechanisms is those related to response to global warming.
72105	53	4	53	52	Here also literature for Africa is completely overlooked. Mechanisms of heavy precipitation occurrence have also been studied in Africa. For example over West Africa "Sylla et al. 2015, doi: https://doi.org/10.1175/JCLI-D-14-00854.1" found that increased intensity of very wet events is due to the occurrence of stronger moisture convergence in the boundary layer that sustains intense precipitation once convection is initiated. There are other examples: Batebana et al. 2015: Investigation of the atmospheric circulation anomalies associated with extreme rainfall events over the Coastal West Africa, Journal of the Earth and Space Physics 41(4):141-149. DOI: 10.22059/jesphys.2015.55173; There are others. [Mouhamadou Sylla, Rwanda]	Noted. This subsection does not refer to details of specific regions.
102555	53	6	53	6	Reference need for this statement [Philippe Tulkens, Belgium]	Noted. The first sentence has been removed to save space. This sentence is a repeat of SREX Ch3 and BOX 11.1.
1455	53	6	53	6	The discussion of the two main drivers of extreme preciptation is a bit limited, although these are clearly two important factors. In addition, the cloud height/thickness play a big role when the raindrops are able to collect more moisture during their fall to the ground. There are also other perspectives, and in a statistical sense, the two local controlling factors for heavy rainfall events are the local wet-day frewquency and the mean precipitatin intensity (Benestad et al, 2019; DOI: 10.1088/1748-9326/ab2bb2) whereas on a global scale, the ratio of global area of evaporation to the global area of daily rainfall sets a frame for the rainfall statistics (Benestad, 2018: DOI: 10.1088/1748-9326/aab375) in addition to of course the rate of evaporation (surface temperature and wind condition). [Rasmus Benestad, Norway]	Noted, no action is taken. The statistical aspects of extreme precipitation is not directly related to the concept of driver, and is not included in this section.
70175	53	6	53	32	There is a substantial gap in discussing dynamical processes' contributions to extreme precipitation, except moonson. But storms like tropical cyclones, extratropical cyclones, fronts are all important meterolofical causes of extreme precipitation. Section 11.7 rarely talks about the linkages between precipitation and dynamical systems. Thus, I suggest adding more discussions around the theme, as indicated by the following two references: 1. Kunkel, K. E., Easterling, D. R., Kristovich, D. A., Gleason, B., Stoecker, L., & Smith, R. (2012). Meteorological causes of the secular variations in observed extreme precipitation events for the conterminous United States. Journal of Hydrometeorology, 13(3), 1131-1141. 2. Huang, H., Winter, J. M., & Osterberg, E. C. (2018). Mechanisms of abrupt extreme precipitation change over the Northeastern United States. Journal of Geophysical Research: Atmospheres, 123(14), 7179-7192. [Huanping Huang, United States of America]	Noted, no action is taken. This aspect is too specific for this subsection.
109603	53	6	53	52	This section seems to virtually ignore the link between natural oscillations (e.g. AMO, PDO, NAO, ENSO etc.) and precipitation patterns worldwide (see previous references above). [Reynold Stone, Trinidad and Tobago]	Taken into account. Only references which address extreme precipitation and large scale modes are considered. #109595: Macdonald, N. and H. Sangsteer. 2017. High-magnitude flooding across Britain since AD 1700. Hydrol. Earth Syst. Sci., vol. 21, pp. 1631-1650; : extreme precipitation is not addressed, only on extreme flood; AMO, NAO Malik et al. 2017. Decadal to multi-decadal scale variability of Indian summer monsoon rainfall in the coupled ocean-atmosphere-chemistry climate model SOCOL-MPIOM. Climate Dynamics, vol. 49, pp. 2551-3572; Indian summer monsoon rainfall; AMO, PDO, ENSO, mainly on annual rainfall, not on extreme precipitation. Valdes-Pineda, R. et al. 2018. Multi-decadal 40- to 60-year cycles of precipitation variability in Chile (South America) and their relationship to the AMO and PDO signals. Journal of Hydrology, vol. 556, pp. 1153-1170; : AMO, PDO, not on extreme Riechelmann, S. et al. 2017. Sensitivity of Bunker Cave to climatic forcings highlighted through multi-annual monitoring of rain-, soil-, and dripwaters. Chemical Geology, vol. 449, pp. 194-205; : NAO, not on extreme Lapointe, F. et al. 2017. Influence of North Pacific decadal variability on the western Canadian Artic over the past 700 years. Clim. Past, vol. 13,
62665	53	6	54	7	This section "11.4.1 Mechanisms and drivers" means to include the mechanism that control extreme precipitation. However, most of the key processes are mentioned in the BOX 11.1. I understand the authors don't want to repeat the processes/mechanisms here, but it would be nice to list or briefly describe the key processes here, otherwise, this section seems to be empty. Even the authors indicate that the details have been addressed in other sections (L12-13), without any indications or hints, readers have no idea what to expect from those sections. For example, many details provided in the summary of this section (P53 L54 - P54 L7) are not really clearly mentioned in this section, but were mentioned in the BOX 11.1. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted, no action is taken. Space limitation does not permit repeating same materials more than once.
28947	53	6			Suggest adding "with microphysical factors that influence precipitation efficiency a secondary contributor." [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Microphysical contribution on extreme precipitation is addressed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
109823	53	8	53	15	I think thermodynamic changes occur on average over large enough regions. You could reference some of geert lenderink's papers here - or Loriaux (Lenderink is also an author). I think they were the first to find this latent heating mechanism for dynamical enhancement. [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Noted, no action is taken. This is addressed in Box 11.1 which assesses relative importance between thermodynamic and dynamic changes in extreme precipitation.
69537	53	10	53	11	It is not the C-C relationship that breaks down, but the fact that precipitation extremes follow the simple thermodynamic scaling. [Martin Singh, Australia]	Taken into account. The text was modified.
62667	53	14	53	19	Is latent heating one of the thermodynamic processes? Are changes In large-scale modes one of the dynamic changes? As the topic sentence of this paragraph emphasizess the two main drivers (thermodynamics and dynamics) of extreme precipitation, it would be good to connect latent heating and large-scale modes to these two main drivers. If the purpose that authors mention latent heating and large-scale modes is to provide other factors (other than the two main drivers), transition sentence(s) is needed. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The sentences were restructured.
39293	53	21	53	32	Uncertainty language is generally missed out. [Lourdes Tibig, Philippines]	Noted. The summary paragraph is written with the uncertainty language.
62669	53	21	53	32	The purpose of this paragraph is confusing. This section is "Mechanism and drivers of extreme precipitation". And the opening sentence of this paragraph "thermodynamic and dynamic processes are important in driving heaving precipitation change associated with monsoon circulation". Are the thermodynamic and dynamic processes for heavy precipitation "associated with monsoon circulation" different from other kind of heavy precipitation? Why is the heavy precipitation "associated with monsoon circulation" special that it owns an individual paragraph? Or the authors want to emphasize that monsoon circulations can drive/affect extreme precipitation (L30-32)? If this is the case, the first part of the paragraph (L21-26) should be re-addressed. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The sentences were restructured.
45537	53	21	53	33	I agree with your point. And a recent work published in GRL suggests that the projected precipitation over Asia monsoon region increases robustly across different scenarios in the long-term projection. It further indicates that the enhancement of global and regional monsoon precipitation is mainly caused by thermodynamic responses due to increased moisture, while the uncertainty of monsoon precipitation projection arises from dynamic processes due to the circulation changes. You may cite this paper: Chen et al. (2020). Global land monsoon precipitation changes in CMIP6 projections [Wenqi Zhang, China]	Noted, no action is taken. This subsection does not refer to details of specific regions. Details of monsoon changes are assessed in CH8.
29465	53	23		32	the same thing can also occur in west Africa due to complex systems producing precipitation in this region such as AEJ, AEW and TEJ. Some of which horizontal temperature gradient is much needed for the formation. Increase in global can lead to huge development of these systems thereby producing more precipitation. for example, TEJ which modulate precipitation over west Africa can be affected by global warming. [Babatunde Oyekan, Nigeria]	Noted, no action is taken. This subsection does not refer to details of specific regions.
28949	53	23			Discussion could be limited to the mechanisms driving monsoon changes rather than observations and projections dealt with elsewhere. The basic mechanism stated in AR5 (Collins et al. 2013) are that thermodynamic processes amplify the monsoon but the slowing tropical circulation offset this while processes involving land-ocean temperature contrast, amplification of heat lows and aerosol-cloud-surface interaction complicate the regional changes (e.g. Section 8.2). [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Considered. The text was modified.
23153	53	24	53	25	You should leave the assessment of understanding of monsoon circulations to chapter 8, cross-reference to it and avoid making a very general statement that could be used to discredit the substantive analysis performed by chapter 8. This aspect is their remit and not yours. [Peter Thorne, Ireland]	Taken into account. We refer to Section 8.3.2.4 for the monsoon changes.
62671	53	27	53	29	Is this impact of warming SST specifically to the monsoon region? If this paragraph is meant to emphasize how monsoon circulations drive extreme precipitation, the impact of SST should be connected to monsoon circulation specifically. If it is the general impact of warming SST, then this description should not be included this paragraph. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. The text was modified.
17701	53	28			"near the coasts of the continents" is unclear. Is it Asian continent? Or all continents across the globe? [Sridhara Nayak, Japan]	Taken into account. The area of the continents are clarified.
23953	53	28			Change "rainfalls" to "rainfall" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
23955	53	29			"areas of torrential rain" would be better than "the torrential areas" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
23957	53	30			I'm not sure what "with moisture surge" adds to this sentence. Perhaps it would be better phrased as, "The warming of the western Indian Ocean is associated with increases in moisture surges on the low-level monsoon westerlies towards the Indian subcontinent, which may lead to an increase in the occurrence of precipitation extremes over central India." [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text was modified.
80629	53	34	53	40	There are recent modelling studies linking aerosols to changes in extremes, via processes that are likely distinct from those of GHG forcing and overall global warming. E.g. Sillmann et al. 2019, npj CAS, https://doi.org/10.1038/s41612-019-0079-3, shows that rapid adjustments associated with black carbon affects extreme precipitation differently to other mechanisms (in CMIP5-type models; this is a PDRMIP paper). Also, Samset et al. 2018, GRL, https://doi.org/10.1002/2017GL076079, shows that in regions currently dominated by aerosol forcing (Asia notably) the dX/dT ratio (where X is e.g. RX5day) is higher for aerosol induced dT than for GHG induced dT. This sets the stage for quite marked changes in wet extremes in Asia as they clean up their aerosol emissions (again, according to present global models, which of course have their limitations). [Bjorn Samset, Norway]	Taken into account. These references are added.
70173	53	34	53	52	It is strange to talk about aerosol and land use forcings without having a dedicated paragraph to discuss the influence of greenhouse gases first. [Huanping Huang, United States of America]	Noted, no action is taken. The influence of greenhouse gases is implicitly discussed as the thermodynamic and dynamics changes in first paragraph and Box 11.1.
42509	53	36	53	36	Typo: aersol -> aerosol [Joan Bech, Spain]	Accepted.
20249	53	36	53	36	aerosol-cloud [philippe waldteufel, France]	Accepted.
74531	53	36	53	36	to correct the word aersol by aerosol [Moulay Driss HASNAOUI, Morocco]	Accepted.
62673	53	36	53	40	L36-37 describes how important aerosol-cloud interactions to extreme precipitation, specifically over Indian and China. However, in L38-40, the authors only list one possible effect of aerosol on extreme precipitation (via change in tropical cyclone). Arent they any other possible effects? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. The texts are modified for clarification.
39295	53	42	53	52	A synthesis with uncertainty language , if possible, is preferred. [Lourdes Tibig, Philippines]	Considered. Generally, assessment with uncertainty language is given in summary section. Here, we use one from BOX10.2.
109825	53	44	53	44	including sub-daily extremes increasing with urbanisation: see Li, Y., Fowler, H.J. Argüeso, D., Blenkinsop, S., Evans, J.P., Lenderink, G., Yan, X., Guerreiro, S.B., Lewis, E., Li, X-.F. 2019: Strong intensification of hourly rainfall extremes by urbanization. Geophysical Research Letters, in press. [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Considered. Effect of urbanization is assessed in detail in Box 10.3 and for this reason, it is not discussed here in any details.
11687	53	50	53	50	"reservoir" should be plural, presumably [Amy East, United States of America]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13837	53	50	53	52	In this statement, is the remote effect of the land use change recently proposed by various authors being considered (Hasler et al, 2009, for example)? Hasler, N., D. Werth, and R. Avissar (2009), Effects of tropical deforestation on global hydroclimate: A multimodel ensemble analysis, J. Climate, 22, 1124e1141, doi:10.1175/2008JCLI2157.1. [Maria Amparo Martinez Arroyo, Mexico]	Considered, no action is taken. This aspect is already described as "large-scale land use and land cover change". This study does not refer to heavy precipitation.
40547	53	54	54	7	No IPCC uncertainty language used [TSU WGI, France]	Considered. Uncertainty language is used in the summary when applicable.
71309	53	54	55	7	It seems better to mention briefly about effects of aerosol and urbanization in summary. [Kenji Taniguchi, Japan]	Considered. Effects of aerosol and urbanization are included in the summary.
109605	54	1	54	7	This section seems to virtually ignore the link between natural oscillations (e.g. AMO, PDO, NAO, ENSO etc.) and precipitation patterns worldwide (see previous references above). [Reynold Stone, Trinidad and Tobago]	Taken into account. These modes are referred to with citing Annex IV.
89135	54	1		2	More precise than the C-C relationship itself is its scaling along moist adiabats, which is somewhat smaller O'Gorman, P. A., & Schneider, T. (2009). The physical basis for increases in precipitation extremes in simulations of 21st-century climate change. Proceedings of the National Academy of Sciences, 106(35), 14773–14777. https://doi.org/10.1073/pnas.0907610106 [Angeline Pendergrass, United States of America]	Noted. This paper is for theoretical study for idealized cases.
42511	54	4	54	4	Typo: widerange -> wide range [Joan Bech, Spain]	Noted. The text was modified.
109827	54	5	54	7	The smaller scale dynamic enhancements are easier to predict I think - enhancing short duration extremes : see summary of these in Fowler, H.J., Lenderink, G., Prein, P., Westra, S., Allan, R.P., Ban, N., Barbero, R., Berg, P., Blenkinsop, S., Do, H.X., Guerreiro, S., Haerter, J., Kendon, E., Lewis, E., Schaer, C., Sharma, A., Villarini, G., Wasko, C., Zhang, X. Intensification of short-duration rainfall extremes with global warming and implications for flood hazard. Submitted to Nature Reviews Earth and Environment, minor revisions. [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This aspect is described in 11.4.2.
64847	54	12	54	34	Ménégoz et al. (in review) are suggesting that even if the increase in Rx1day in the European Alps is currently strong, timseries with a lenghts of minimum 50 to 80 years is required to get significant linear trends. This does not mean that this signal is small, but that the interannual variability is very large. Reference: Ménégoz, M., Valla, E., Jourdain, N. C., Blanchet, J., Beaumet, J., Wilhelm, B., Gallée, H., Fettweis, X., Morin, S., and Anquetin, S.: Contrasting seasonal changes in total and intense precipitation in the European Alps from 1903 to 2010, Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-690, in review, 2020. [Martin Ménégoz, France]	Noted, while the paper is relevant to the paragraph, there is not a lot space to discuss contrast between signal magnitude and variability.
1459	54	12	54	34	It is extremely difficult to get a comprehensive picture when the series of available rain gauge data have different lengths and start and stop at different times. To make is more difficult, there are many countries where climate data is not openly accessible, and there is the question whether the series are homogeneous. There are also important caveats when it comes to gridding 24-hr precipitation, which results in spatial inhomogeneity (points between sites are weighted means of the surrounding sites, however, statistical properties, such as R1xday or even trends in R1xday, are less affected than daily values) and also is limited by rain gauge data becoming available/unavailable over time. [Rasmus Benestad, Norway]	Noted, but it is unclear what specific aspects of this comment needs to be addressed.
41067	54	12	58	33	There's a general lack of quantification in this section. Would be useful to provide numbers, in addition to 'increase' or 'decrease', where possible. Else the reader is left to guess how large the changes are. Should check this throughout the chapter. [TSU WGI, France]	Considered. Given very high spatial heterogeneity and uncertainty in heavy precipitation, different length of available datasets and analysis period, the quantifications are provided only at a very few places to avoid debate and confusion. Quantification is given in terms of statistical significance as well as overall magnitude of change in the rate of extreme precipitation (that consistent with CC-scaling etc.).
102557	54	14	54	14	"..in many locations that are not statistically..." [Philippe Tulkens, Belgium]	Accepted
23155	54	14	54	15	This stub sentence makes no sense and can be removed. [Peter Thorne, Ireland]	Accepted
74533	54	15	54	15	Du et al. (2019) found ... in place of (Du et al. ... [Moulay Driss HASNAOUI, Morocco]	Accepted
13715	54	15	54	15	change (Du et al., 2019) by Du et al. (2019) [Maria Amparo Martinez Arroyo, Mexico]	Accepted
39297	54	15	54	34	Same comment as above [Lourdes Tibig, Philippines]	Noted. Calibrated language is used when needed, in particular in the summary paragraph.
43345	54	15			Read "Du et al. (2019) found a significant " rather than "(Du et al., 2019) found a significant " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
1457	54	17	54	17	Benestad et al (2019; DOI: 10.1088/1748-9326/ab2bb2) found increasing trends for 76% of 1875 rain gauges distributed world-wide with more than 50-years of valid data in the interval 1961-2018. Most (95%) of this increase could be attributed increasing tends in the wet-day mean precipitation while 72% of these also were associated with increasing wet-day frequency. The analysis also revealed increasing trends in the daily precipitation variance, σ^2, for 79% of the sites, as well as increasing trends in the 10-year-return period (also 79% of the sites), and the number of days with record-breaking 24-hr precipitation amounts was greater than the expected number for a stable by 20%. [Rasmus Benestad, Norway]	Accepted
82783	54	17	54	26	It would be useful to clarify what time periods these changes are over (or, if they are over a range of periods, say so). [Blair Trewin, Australia]	Taken into account, The period is 1951-2018 and is mentioned
20251	54	21	54	21	increasing [philippe waldteufel, France]	Accepted
8699	54	21	54	21	spelling "increases" (third word) [Robert Dunn, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
43347	54	21			"increasins" or "increasing"? [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
108885	54	24	54	25	Consider citing Fischer, E. M., and R. Knutti (2014), Detection of spatially aggregated changes in temperature and precipitation extremes, Geophysical Research Letters, 41(2), 547-554 [Erich Fischer, Switzerland]	Noted, but this reference does not fit to the section on observed trends.
104927	54	25	54	26	Unclear what is meant by a decrease is not significant - in regions with a decrease? [John Caesar, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, sentence is modified
23157	54	26	54	26	End of this sentence makes no sense - what decrease? [Peter Thorne, Ireland]	Taken into account, sentence is modified
109829	54	30	54	31	This is also shown in Westra et al. 2013 and in Fischer and Knutti 2016 NCC [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
23743	54	36	54	49	You may consider including the finding of this paper: "Chang'a, L.B., Kijazi, A.L., Luhunga, P.M., Ng'ongolo, H.K. and Mtongori, H.I. (2017): Spatial and Temporal Analysis of Rainfall and Temperature Extreme Indices in Tanzania. Atmospheric and Climate Sciences, 7, 525-539. https://doi.org/10.4236/acs.2017.74038" The paper has given information on the observed trends in extreme temperature and rainfall in Tanzania (East Africa) using observed station data. [Ladislaus Chang'a, United Republic of Tanzania]	Noted, the article is more on extreme temperature and SPI.
23745	54	36	54	49	Also this paper can be included "Omondi, P. A., and Coauthors, 2014: Changes in temperature and precipitation extremes over the Greater Horn of Africa region from 1961 to 2010. International Journal of Climatology, 34, 1262-1277." This also has assessed trends in extreme events over the East African region. [Ladislaus Chang'a, United Republic of Tanzania]	Noted and added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23747	54	36	54	49	Also this paper can be considered Chang'a, L.B., Kijazi, A.L., Mafuru, K.B., Kondowe, A.L., Osima, S.E., Mtongori, H.I., Ng'ongolo, H.K., Juma, O.H. and Michael, E. (2020) Assessment of the Evolution and So- cio-Economic Impacts of Extreme Rainfall Events in October 2019 over the East Afri- ca. Atmospheric and Climate Sciences, 10, https://doi.org/10.4236/***.2020.***** [Ladislaus Chang'a, United Republic of Tanzania]	Noted, but this paper is not specifically related to this section
62715	54	42	54	42	SDII gets introduced here for the first time without further explanation. The acronym is explained later in the text on page 61 line 37-38. I would suggest to give the explanation the first time the abbreviation is used. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
29467	54	43		45	based on a model using HadGEM2 simulations(unpublished), it was gathered that there is increase in precipitation in west Africa. [Babatunde Oyekan, Nigeria]	Noted, however unpublished work could not be assessed
52655	54	48	54	48	Kruger and Nxumalo 2017 https://journals.co.za/content/journal/10520/EJC-710b403bc and Mackellar et al 2014 https://www.sajs.co.za/article/view/395 found same. [Mary-Jane Bopape, South Africa]	Noted, the recent work by Kruger on the same is added, the second link is not working on 04-10-2020
39299	54	51	55	14	You may want to consider the ff papers: Kim et al, 2018 (Evaluation of precipitation extremes over the Asian domain: Observation and modelling ; Mandapaka et al, 2017 (Analysis of spatial patterns of daily precipitation and wet spell extremes in Southeast Asia); Marjuki et al, 2016 (Observed trends and variability in climate indices relevant for crop yield in Southeast Asia); Cheong et al, 2018 (Observed and modelled temperature and precipitation extremes over southeas Asia from1972 to 2010); and Xiao et al, 2016 (Robust increase in extreme summer rainfall intensity during the past four decades observed in China). [Lourdes Tibig, Philippines]	Noted and added
83883	54	53	54	53	Reference Missing: Goswami etal. , 2006, Increasing Trend of Extreme Rain Events Over India in a Warming Environment, Science, doi: 10.1126/science.1132027 [Ajaya Mohan Ravindran, United Arab Emirates]	Rejected, only post-AR5 papers are assessed.
83885	54	53	54	53	Reference Missing: Ajayamohan et al., 2010, Increasing trend of synoptic activity and its relationship with extreme rain events over central India, Journal of climate, doi: 10.1175/2009JCL2918.1 [Ajaya Mohan Ravindran, United Arab Emirates]	Rejected, only post-AR5 papers are assessed.
11689	55	2	55	2	delete extra parenthesis [Amy East, United States of America]	Accepted
23159	55	2	55	3	Which is it - no trend or contrasting estimates? It can't be both as implied so choose one here. [Peter Thorne, Ireland]	Accepted, rewritten
23959	55	2			Double bracket needs correcting [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
130559	55	3	55	6	I do not agree the conclusion for China's extreme precipitation change. It is too vague and too general, neglecting most studies conducted in China. [Panmao Zhai, China]	Noted, there is large disagreements across literature and dataset for trends in China. This is now reflected.
17703	55	9	55	11	Sentence is not clear. [Sridhara Nayak, Japan]	Noted and modified
23961	55	11			Change "activties" to "activity" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
11691	55	12	55	12	what is meant by "the Maritime Continent"? where? [Amy East, United States of America]	Noted, Part of south east Asia and a common terminology
26127	55	13	55	14	Finding of National Climate Center confirmed thatt mean temperature of Iran in the recent decade of 2008-2017 has been increased by 1.2oC(0.4)comparing to 1958-1988 reference period. Also , in the same period mean annual precipitation of Iran in the recent decade has been decreased by about 40mm(15%) comparing to the reference period of 1958-1988(Abbasi, F., Malbusi, Sh., koohi, M., Javanshiri Z., Habibi M., Falamarzi Y. (2018). Climate change detection pdate over Iran during 1958-2017. (Report No. C200A001M). Mashahd (Iran): Climatological ResearchInstitute.). URL: https://cri.ac.ir/index.php/fa/2018-09-29-15-04-19/2018-09-29-18-09-07/2018-09-29-18-09-49/2019-01-24-07-12-00 [Iman BABAEIAN, Iran]	Rejected, Not related to precipitation extremes
33087	55	13	55	14	It is better to add figure for stations distribution [Sahar Tajbakhsh Mosalman, Iran]	Noted, however, due to space constraints, it is difficult to add such detailed regional figures
19525	55	13	55	14	it is better to add plot of stations [Hamideh Dalaei, Iran]	Noted, however, due to space constraints, it is difficult to add such detailed regional figures
19527	55	13	55	14	add refrence (Dalaei, et al,Acta Tropica,166(2017-l57)45-53 [Hamideh Dalaei, Iran]	Rejected, the paper is not related to extremes: https://www.sciencedirect.com/science/article/pii/S0001706X16304508
82785	55	13	55	14	What are the increases of? (presumably not the frequency). Also, unless the 50% refers to significant increases, 50% showing increases doesn't indicate a strong skew towards positive trends. [Blair Trewin, Australia]	Noted, the sentence is removed.
23161	55	13	55	14	This is confusing as written presumably you mean an increase in the intensity of extreme events but a derecrease in the frequency of occurrence? If so please say so more clearly here. [Peter Thorne, Ireland]	Noted, the sentence is removed.
32757	55	13	55	14	It is better to add figure for stations distribution [sadegh zeyaeyan, Iran]	Noted, however, due to space constraints, it is difficult to add such detailed regional figures
33085	55	13	55	15	Finding of National Climate Center confirmed that mean temperature of Iran in the recent decade of 2008-2017 has been increased by 1.2oC(0.4)comparing to 1958-1988 reference period. Also , in the same period mean annual precipitation of Iran in the recent decade has been decreased by about 40mm(15%) comparing to the reference period of 1958-1988(Abbasi, F., Malbusi, Sh., koohi, M., Javanshiri Z., Habibi M., Falamarzi Y. (2018). Climate change detection pdate over Iran during 1958-2017. (Report No. C200A001M). Mashahd (Iran): Climatological ResearchInstitute.). URL: https://cri.ac.ir/index.php/fa/2018-09-29-15-04-19/2018-09-29-18-09-07/2018-09-29-18-09-49/2019-01-24-07-12-00 [Sahar Tajbakhsh Mosalman, Iran]	Rejected, Not related to precipitation extremes
32755	55	13	55	15	Finding of National Climate Center confirmed that mean temperature of Iran in the recent decade of 2008-2017 has been increased by 1.2oC(0.4)comparing to 1958-1988 reference period. Also , in the same period mean annual precipitation of Iran in the recent decade has been decreased by about 40mm(15%) comparing to the reference period of 1958-1988(Abbasi, F., Malbusi, Sh., koohi, M., Javanshiri Z., Habibi M., Falamarzi Y. (2018). Climate change detection pdate over Iran during 1958-2017. (Report No. C200A001M). Mashahd (Iran): Climatological ResearchInstitute.). URL: https://cri.ac.ir/index.php/fa/2018-09-29-15-04-19/2018-09-29-18-09-07/2018-09-29-18-09-49/2019-01-24-07-12-00 [sadegh zeyaeyan, Iran]	Noted but the paper is not related to extreme precipitation and hence not cited.
106769	55	14			decline in the frequency (Najafi and Moazami, 2016). Tabari et al. 2014 analyzes temporal oscillations in precipitation series in several sub-regions of Iran over a 31-year period (1980-2010).The results of this study indicated significant anomalies in precipitation extremes in the northwest and southeast regions of Iran. Analysis of extreme precipitation perturbations reveals that perturbations for the monthly aggregation level are generally lower than the annual perturbations. Furthermore, high-oscillation and low-oscillation periods are found in extreme precipitation quantiles across different seasons. In all selected regions, a significant anomaly (i.e., extreme wet/dry conditions) in precipitation extremes is observed during spring. Tabari, H., AghaKouchak, A., & Willems, P. (2014). A perturbation approach for assessing trends in precipitation extremes across Iran. Journal of Hydrology, 519, 1420-1427. [Mansoureh Kouhi, Iran]	Considered but no action is taken. Space limitation does not permit getting into such details.
13717	55	25	55	25	change Rx1d by Rx1day [Maria Amparo Martinez Arroyo, Mexico]	Accepted
42513	55	27	55	27	English: has remain -> has remained [Joan Bech, Spain]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
88483	55	27	55	27	Could perhaps note something like "Gridded observations also show a significant trend towards more intense convective rainfall events in Australia (Dowdy 2020)." Reference: Dowdy, A.J., 2020. Climatology of thunderstorms, convective rainfall and dry lightning environments in Australia. Climate Dynamics, 54(5), 3041-3052, https://doi.org/10.1007/s00382-020-05167-9 [Andrew Dowdy, Australia]	Considered , no action is taken. Space limitation does not permit such details.
9153	55	28	55	32	In New Zealand, there is no clear evidence that intense rainfall events have changed from 1960-2016, but there are trends at some locations: proportion of annual rainfall occurring in intense events (in the 95th percentile) decreased at 4 of 30 locations (Auckland, New Plymouth, Rotorua, and Taupo) and increased at two (Napier and Timaru) annual maximum one-day rainfall amounts decreased at 4 of 30 locations (Auckland, Hamilton, Taupō, and New Plymouth) and increased at two (Timaru and Dunedin) (MfE, 2017: http://www.mfe.govt.nz/sites/default/files/media/Environmental%20reporting/our-atmosphere-and-climate-2017.pdf) [Kevin Hennessy, Australia]	Noted, but no action is taken. Space limitation does not permit such details.
108887	55	34	55	36	See also Zeder and Fischer, 2020, Observed extreme precipitation trends and scaling in Central Europe, Weather and Climate Extremes, in press. [Erich Fischer, Switzerland]	Accepted
42325	55	34	55	50	Chapter 12 also has region-by-region material in particular for mediterranean areas and other regions which could be moved from there to this section. This should be handled by an "extreme precipitation and flood" CH8-CH11-CH12 LA group [robert vautard, France]	Taken into account. The materials related to heavy precipitation from Chapter 12 is now included in 11.4
76673	55	36		38	The lack of consistency at regional scale does not allow to reach a robust conclusion on trends of extreme precipitation at regional scale in the Mediterranean. It is not only because of lack of data, but becuase the interannual variability is too large to identify the signal. Besides the cited studies, Reale and Lionello (2013) considered 15 coastal stations along the whole Mediterranean coastline and briefly mention they did not find any statistically significant change in the number of intense precipitation events (Reale M, P Lionello P (2013) Synoptic climatology of winter intense precipitation events along the Mediterranean coasts. Nat Hazards Earth Syst Sci, 13:1707–1722. doi:10.5194/nhess-13-1707-2013) ... [Piero Lionello, Italy]	Considered, the paper is cited.
24047	55	38	55	39	Please, consider adding this reference: Serrano-Notivoli et al. (2018, https://doi.org/10.1002/joc.5562) [Roberto Serrano-Notivoli, Spain]	Accepted
87961	55	39	55	40	change "Portugal, where a mixed trend is observed (Pedron et al., 2017)" in "Portugal, where a mixed trend is observed (Pedron et al., 2017) and Italy, with the same mixed trend obtained on sub-daily precipitation extremes (Libertino et al, 2019) [Pierluigi Claps, Italy]	Noted, but sub-daily precipitation trends are assessed in different paragraphs
8043	55	40	55	40	the total precipitation contributed from extremes: what is the threshold for extreme precipitation in this case? [jouni Räisänen, Finland]	Accepted and added
13719	55	40	55	40	Include what degree C refers because it is not understood [Maria Amparo Martinez Arroyo, Mexico]	Rejected, Degree C is a common term
43349	55	49		50	Incorrect writting of quoted references "(Yiou and Cattiaux 2013, BAMS, Dong et al. 2013 BAMS, (Held and Soden, 2006; Grams et al., 2014; Madsen et al., 2014; Helama et al., 2018) " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted and corrected
87959	55	51	56	4	Trends in precipitation daily extremes in the US are sensitive to the time scale examined. In McKitrick, Ross R. and John Christy (2019) Assessing Changes in US Regional Precipitation on Multiple Time Scales Journal of Hydrology vol. 578 Nov 2019, https://doi.org/10.1016/j.jhydrol.2019.124074 we examined 2,000 year drought proxies and 150 year-long daily precip records from the US Pacific and Southeast region. On the long time scales there were no significant trends, and the apparent upward trend using a 100 year 20th-century sample doesn't hold up either when the sample is extended back to the 1800s (because there were intervals in the mid-1800s with very high extreme precip levels) or when the sample is confined to the post-1970 interval during which the signs of the trends reverse. [Ross McKitrick, Canada]	Considered, but no action is taken. The assessment focuses on changes since 1950s.
70163	55	52	55	52	Among the three countries in North America, only US has a robust and detectable increase in heavy precipitation, supported by a rich set of publications. But this line suggests that the medium and high confidence in North America heavy precipitation change is solely determined by the US (as indicated by "specifically"), without considering Canada and Mexico. Thus, I recommend to restructure the topic sentence and first highlight the diversity of changes in North America countries, instead of giving the audience an impression that the US represents the North America as a whole. And then describe the change in each country separately. [Huanping Huang, United States of America]	Considered. Different continents have the same structure for their assessments.
125919	55	52	55	53	Specify "at the daily scale". Is the "increase in heavy precipitation" of daily total? Or the heavy precipitation at return interval of one day? [Trigg Talley, United States of America]	Taken into account and specified
55493	56	6	56	18	please consider these references Domínguez-Castro, F., R. 2018: doi:10.1002/joc.5312. Morán-Tejeda (2016) DOI: 10.1002/joc.4597 . Du, H., et al. (2019). . https://doi.org/10.1029/2019GL081898. Carril, (2016) doi: 10.3354/cr01374 [Matilde Rusticucci, Argentina]	Noted. The first two are not related to daily heavy precipitation, and the last one is about multiple-day heavy precipitation, which is added at appropriate place.
29915	56	6	56	18	Skansi et al (2013) is mentioned 5 times in 8 sentences. I would add Carvalho (2020) https://onlinelibrary.wiley.com/doi/abs/10.1002/wcc.627 and Carril et al. (2016) for South America https://www.int-res.com/abstracts/cr/v68/n2-3/p95-116/ [Juan Rivera, Argentina]	Accepted and added
13721	56	14	56	14	Standardize the format of Rx1day or RX1day [Maria Amparo Martinez Arroyo, Mexico]	Accepted
57451	56	16	56	18	The term "Central America" is used with different meanings along Chapter 11. In this case, the reference refers to the Central America Isthmus, but in other parts of the Chapter, the cited reference includes Mexico and the Caribbeani. I suggest to be specific in each case, considering Mexico (NorthernCentral America), the Centra america Insthmus) (Southern Central America) and the Caribbean as different regions.. [Daniel Martinez Castro, Cuba]	Accepted
66019	56	20	56	20	Suggest defining "long" in this context, noting that some of the papers referenced only go back as far as 1998, as in the example of Sen Roy and Roualt (2013). [Kushla Munro, Australia]	Noted. Long term means multi-decadal
109831	56	20	56	47	Note that there is a great summary of observed trends in sub-daily precipitation extremes globally (and a nice figure that could be used in this report) in Fowler et al. : Fowler, H.J., Lenderink, G., Prein, P., Westra, S., Allan, R.P., Ban, N., Barbero, R., Berg, P., Blenkinsop, S., Do, H.X., Guerreiro, S., Haerter, J., Kendon, E., Lewis, E., Schaer, C., Sharma, A., Villarini, G., Wasko, C., Zhang, X. Intensification of short-duration rainfall extremes with global warming and implications for flood hazard. Submitted to Nature Reviews Earth and Environment, minor revisions. There is a significant amount of literature that has been missed here. please use the refs within this paper (I can send this to you) [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Added
66017	56	20	56	47	We appreciate that the report acknowledges that long term analysis of sub daily precipitation extremes have only been conducted in a few regions. Suggest additional references: - Busuioc et al (2016), 81 stations in Romania from 1961-2010. - Mishra et al (2012), 6000 stations across the USA from 1950. - Lenderink et al (2010), 27 stations Netherlands from 1995, 3 from Switzerland from 1981 and 1 from Belgium from 1950. Includes the record from De Bilt since 1906. - Lenderink et al (2011), compares the long data records from De Bilt since 1906 and Hong Kong since 1885. - Park and Min, (2017), 26 stations from 1980, Korea. [Kushla Munro, Australia]	Noted. Thank you. We assess post-AR5 papers and as such, pre-2012 articles are not included. Busuioc et al(2016) is added. Morrison et al is added in the projection section paragraph.
43351	56	21			Read "Sen Roy and Rouault (2013) showed an increase " rather than "(Sen Roy and Rouault, 2013) showed an increase " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23963	56	22	56	24	Over what time period is the tend in urban sub-daily extremes? And can the role of aerosol pollution (and hence cloud microphysics) be ruled out, rather than simply blaming warming? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Aerosols can not be ruled out but the specific literature did not discuss it
42515	56	31	56	31	Typo: guage -> gauge (please check meaning) [Joan Bech, Spain]	Accepted
43353	56	31			Read "gauge stations" rather than "guage stations" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
66021	56	35	56	35	Suggest including additional countries in discussions on studies on European regions. See: Romania (Busuioc et al, 2016), Netherlands, Belgium and Switzerland (Lenderink et al, 2010) and Germany (Berg et al, 2013). [Kushla Munro, Australia]	Noted. Busuioc et al. (2016) is added. Lenderink et al (2010) is pre-AR5 article and is not assessed. Berg et al (2013) is more on evaluation of hourly precipitation
87963	56	36	56	36	change "regions. An increase in hourly extreme precipitation was observed in Sicily (Arnone et al., 2013)" in "regions. In Italy, hourly extremes showed clear increase in some regions and decrease in others (Libertino et al., 2019), confirming previous local findings (Arnone et al., 2013)" [Pierluigi Claps, Italy]	Accepted
109833	56	36	56	38	This is wrong. Chan et al 2016 looked at convection permitting model outputs - they found that intensity and duration increases in a future warmer climate. I am not sure where this statement comes from. [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and the sentence is deleted
10945	56	36	56	38	A similar study over Sweden (but a bit longer period; 1996-2018) did not show any clear change in magnitude or frequency of sub-hourly extremes: Olsson, J., Södling, J., Berg, P., Wern, L., and A. Eronn (2019) Short-duration rainfall extremes in Sweden: a regional analysis, Hydrol. Res., nh2019073, doi: 10.2166/nh.2019.073. [Jonas Olsson, Sweden]	Accepted
1461	56	41	56	43	There is a need to specify what the level of siginficance was. If it is 5%, then one should expect to see 5% of stations with a significant increase as a consequence of the definition of statistical significance, so it's not clear why the rest of the sentence says otherwise. Is there additional finormation relevant for this judgement? Also what is meant by 'increase'? The annual maximum hourly rainfall amount? Also, over what interval and were 6000-733 sites of low quality or too short? [Rasmus Benestad, Norway]	Accepted and modified
1463	57	1	57	7	There is another factor explaining the extremes and that is how many rainfall events that are considered each year. This is equivalent to their sample size and if there are years with small or large samples, it will affect the probability of seing extreme values. It's a sampling effect that easily can be demonstrated through simple Monte-Carlo simulations with identical pdfs of different sample size. [Rasmus Benestad, Norway]	Noted. This passage has been rewritten taking new literature into account. This comment does not apply to the new text.
89137	57	1		26	Bao et al (2017) shows that the interpretation of observed extreme precipitation scaling is not appropriate. The correct way that these relationships should be interpreted is illustrated in Fig 10 of Drobinski et al (2016). Drobinski, Philippe, Nicolas Da Silva, Gérémy Panthou, Sophie Bastin, Caroline Muller, Bodo Ahrens, Marco Borga, et al. "Scaling Precipitation Extremes with Temperature in the Mediterranean: Past Climate Assessment and Projection in Anthropogenic Scenarios." Climate Dynamics, 2016, 1–21. https://doi.org/10.1007/s00382-016-3083-x. [Angeline Pendergrass, United States of America]	Considered. This passage is rewritten, taking new literature into consideration.
89139	57	1		26	This topic is also addressed in Chapter 8, section 8.2.3.2 - it should be consistent in both places. [Angeline Pendergrass, United States of America]	Noted, Section 8.2.3.2 is cited at 11.4.1
66023	57	10	57	10	Suggest correcting the reference for (Lewis et al., 2019). There are currently two references for Lewis et al., (2019). Suggest the intended reference be: Lewis, E., Fowler, H., Alexander, L., Dunn, R., McClean, F., Barbero, R., ... & Blenkinsop, S. (2019). GSDR: A Global sub-daily rainfall dataset. Journal of Climate, 32(15), 4715-4729. This citation does not appear in the references. [Kushla Munro, Australia]	Accepted
109835	57	11	57	13	Note that there is super CC scaling for individual and small pools of stations but at regional/continental scales it converges to CC apart from for Europe where it remains super CC. This paper has only just been submitted so might want to remove this section. Not sure if it will be published in time. [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This passage has been rewritten taking new literature into account.
66025	57	11	57	13	Suggest including a reference for the sentence starting: "There is a super-C-C". Perhaps one of the other Lewis 2019 papers was intended for this sentence? [Kushla Munro, Australia]	Noted. This passage has been rewritten taking new literature into account.
66027	57	11	57	13	Suggest including a more detailed and robust review of the literature detailing where super-C-C has been observed and under what conditions, e.g., temperature or dewpoint temperature ranges. Suggest that this discussion should also include the results from the longest operational gauges such as at De Bilt (Lenderink and Van Meijgaard, 2008) and Hong Kong (Lenderink et al 2011) as well as the results from Busuioc et al (2016), Mishra et al (2012), Lenderink et al (2010), Park and Min, (2017) and Berg et al (2013). Additionally, we suggest that it be noted that super-C-C does not occur over the entire dewpoint temperature range and scaling often shifts from C-C scaling to super-C-C. Also, C-C scaling and super-C-C behaviour can occur at the same site depending on conditions - as such, we suggest caution when reporting a single scaling rate for a single location. [Kushla Munro, Australia]	Noted. This passage is rewritten taking new literature into account. Pre-AR5 literature is not assessed. Busuioc (2016) is added at proper place
66031	57	11	57	13	Suggest clarification on the role of convective precipitation driving super-C-C behaviour at higher dewpoint temperatures. Several studies are now concluding that super-C-C is a result of convective precipitation including, Berg et al (2013), Park and Min, (2017) and Loriaux et al (2013). [Kushla Munro, Australia]	Noted. This passage has been rewritten taking new literature into account.
109839	57	15	57	17	Note that a paper by Geert Lenderink just submitted to a special issue of the proc roy soc ser A seems to show this relation. [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Noted, but the author team does have access to the paper mentioned here.
108889	57	17	57	18	Add references to this statement [Erich Fischer, Switzerland]	Accepted and added
109837	57	18	57	20	Note that Bao et al 2017 used dry bulb temperature, not dew point and look at projections from an RCM (with parameterised convection) so would not expect them to match as only CPMs can reproduce observed scaling relations (see Chan et al. 2016 in Nature Geoscience for e.g.) [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This passage has been rewritten taking new literature into account.
66029	57	18	57	20	Suggest clarification. While Bao et al. (2017) did find negative scaling rates for the tropics as a function of temperature, Barbero et al (2017) responded directly to the result and explained in detail the importance of using dewpoint temperature instead of temperature and reported scaling rates for Darwin of -38.83% as a function of temperature and 9.38% as a function of dewpoint temperature, arguing as many others have of the importance of using moisture for the estimation of scaling rates instead of temperature. Hardwick Jones et al (2010) also discusses the importance of including humidity in the determination of the scaling rates and also explicitly studies the scaling rates at Darwin. Suggest including a specific paragraph committed to the discussion of how negative scaling rates in the tropics have been observed if temperature is used and detailing that this is due to the fact that humidity decreases as temperature increases at high temperatures, as presented by Hardwick Jones et al (2010) and Barbero et al (2017). Suggest further describing how positive scaling rates do exist when dewpoint temperature is used instead. [Kushla Munro, Australia]	Noted. This passage has been rewritten taking new literature into account.
1465	57	28	57	35	Is it possible to say something of the cloud climatology? E.g. whether there has been a change in the convective activity, statistics of frontal systems or cyclones? There is also additional information from insurance claims. Would it be possible to combine information from multiple independent scources to get a firmer picture? [Rasmus Benestad, Norway]	Noted. These are more related to processes and better fit to Chapter 8

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
125921	57	28			The following references may be relevant: Morrison, A., G. Villarini, W. Zhang, and E. Scoccimarro, Projected changes in extreme precipitation at sub-daily and daily time scales, Global and Planetary Change, 182, 1-11, 2019. Zhang, W., G. Villarini, E. Scoccimarro, and G.A. Vecchi, Stronger influences of increased CO2 on sub-daily precipitation extremes than at the daily scale, Geophysical Research Letters, 44, 7464-7471, 2017. [Trigg Talley, United States of America]	Accepted and added
42517	57	41	57	42	Check reference format: Sun et al. 2019 JCLI-0892 -> Sun et al. 2019 ? [Joan Bech, Spain]	Accepted
38421	57	42	57	42	There are also some regional studies indicating an increase. Increase of what? [Mansour Almazroui, Saudi Arabia]	Taken into account. RX5day precipitation
13723	57	42	57	42	What is JCLI-0892? It's not understood [Maria Amparo Martinez Arroyo, Mexico]	Taken into account. Corrected
11693	57	42	57	42	looks like a problem with reference citation, delete “JCLI-0892” [Amy East, United States of America]	Taken into account. Corrected
23965	57	42	57	43	Please clarify if the increase referred to here represents an increase in the duration by 5.17%, or of an increase in the frequency of events lasting longer than 5 days by 5.17%? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. RX5day precipitation
109841	57	43	57	45	I found sig increases in 5 day extremes for the UK many years ago now in Fowler et al. 2003a,b. Fowler, H.J. and Kilsby, C.G., 2003: Implications of changes in seasonal and annual extreme rainfall. Geophysical Research Letters, 30(13), 1720, doi:10.1029/2003GL017327. Fowler, H.J. and Kilsby, C.G., 2003: A regional frequency analysis of United Kingdom extreme rainfall from 1961 to 2000. International Journal of Climatology, 23(11), 1313-1334. [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Pre-AR5 papers are in general not assessed in AR6 report.
42519	57	50	57	50	Typo: in an warming -> in a warming [Joan Bech, Spain]	Accepted and corrected
117103	57		57		Give an uncertainty range with the monsoon increase ratio number (5.17). [Valerie Masson-Delmotte, France]	Considered, sentence is modified.
71311	58	1	58	1	Place Fig.11.12 near the first description (in P.11-54) [Kenji Taniguchi, Japan]	Accepted
96125	58	3	58	4	It should please be explained how exactly "sufficient data" has been defined: Are there, for example, gaps in the data and, if so, how has it been dealt with? Or do all selected 8345 stations have a continuous record during 1950-2018? [Nicole Wilke, Germany]	Accepted. 70% data available, now clarified
42963	58	3	58	13	I find this figure difficult to interpret and not very informative due to lack of data over most of the globe. [Rein Haarsma, Netherlands]	Noted. The caption is edited to improve readability.
6791	58	4	58	4	The end of the first sentence of this figure caption is garbled. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The caption is edited to improve readability.
43355	58	12		13	Read " Adapted from Sun 12 et al. (submitted)." rather than " Adapted from (Sun 12 et al., submitted)." [Cyriaque Rufin Nguimalet, Central African Republic]	Editorial
39301	58	18	58	34	Uncertainty language must be italicized. [Lourdes Tibig, Philippines]	Taken into account and Corrected
38159	58	20	58	33	Considering its importance for meteorological disasters like flash floods, adding a statement on sub-daily extreme precipitation in the summary would be useful to readers. [Junhee Lee, Republic of Korea]	Considered. There is now a sentence about sub-daily precipitation.
28953	58	21			Coordinate with Section 8.3.1.3 which may need correcting from "Also, it is very likely that the frequency and intensity of precipitation extremes have increased since 1951 in a majority of land regions with good observational coverage, and there is high confidence that such an increase is partly due to anthropogenic forcings." [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account and modified.
80701	58	22	58	22	are the Pacific SIDS included in these land region? In figure 11.6, the region where they are located is not included in the land regions [Helene Jacot Des Combes, Marshall Islands]	Noted. This paragraph is more on high level message, and not really on very region specific assessments. Fig. 11.6 is on extreme temperature.
1467	58	23	58	23	Statistically significant at what level? [Rasmus Benestad, Norway]	Noted. By default they are at 0.05 level, if not mentioned
23165	58	24	58	25	At several points in the text you had maintained it was significantly less than expected by chance. Why do you row back on this in the assessment text? This makes no logical sense and introduces a disparity between text and summary? [Peter Thorne, Ireland]	Considered. The summary text has been rewritten.
76671	58	32		33	It is not only an issue of data limitation. Internal variability plays a role as well ins everal regions (see comment above) [Piero Lionello, Italy]	Considered. The summary text has been rewritten.
78761	58	36	59	23	Facing the issues in evaluating model output in the context of extreme precipitation, a paper (titled "How Well Can a Climate Model Simulate an Extreme Precipitation Event: A Case Study Using the Transpose-AMIP Experiment") might be inclued here. Previous studies focusing on the model performance for extreme events generally evaluate the quantitative statistical features of extreme precipitation based on longterm model outputs. This paper evaluated the performance of a climate model (also a CMIP6 model) in simulating the spatial and temporal distribution of the rainfall and the related synoptic circulation in a high-impact extreme precipitation event. [jian li, China]	Noted. But there is no details (name of authors etc.) about this paper provided.
28955	58	36			Could be a link to Section 8.5.1.1.1 on model convection [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
71465	58	36			In Chapter 10 we discuss many aspects which are relevant here, e.g., the representation of convection or fronts by different types of models (mainly section 10.3.3.5). There should be a link somewhere in this section. [Douglas Maraun, Austria]	Accepted, a link is existing in this chapter
66937	58	38	58	39	The first two sentences of the section, while accurate, seem disconnected from each other and made for confusing reading. Suggest rewording. [Mathew Barlow, United States of America]	Considered. Text modified.
72111	58	38	60	14	May be you can check this paper. It did a comprehensive evaluation of extreme precipitation over Africa by looking at CORDEX VS CMIP5: Gibba et al. (2019): State-of-the-art climate modeling of extreme precipitation over Africa: analysis of CORDEX added-value over CMIP5, DOI: https://doi.org/10.1007/s00704-018-2650-y [Mouhamadou Sylla, Rwanda]	Accepted
72107	58	38	60	24	The evaluation here mostly takes into account CMIP3, 5 or 6. That's great but there are several studies that used CORDEX in all regions of the world. I think these should be considered in a more extensive way. [Mouhamadou Sylla, Rwanda]	Considered. This is now expanded.
109375	58	44	58	44	"Smaller" needs clarifying, in what sense? [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account and reworded
84903	58	47	58	48	"However, Risser and Wehner (2020) reversed the order of operations by performing gridding to 25km after fitting extreme value distributions" to station data I presume but this is not stated explicitly. [Turner Jessica, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Some details about individual papers are removed to meet length limit.
84905	58	48	58	49	The statement here "that model skill in evaluation of the CMIP6 HighResMIP models is affected" is confusing to me. Affected in what way? [Turner Jessica, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and modified
109377	58	50	58	50	"… is affected". How, and what are the implications. Please clarify. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and modified
43357	58	51		52	Read " (Sillmann et al., 2013a; Kim et al., submitted; Li et al., submitted)." rather than " ((Sillmann et al., 2013a); Kim et al., submitted; Li et al., submitted)." [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
117105	58		58		the section on model evaluation needs to be better integratied and linked to the findings of ch 3 and 8 [Valerie Masson-Delmotte, France]	Considerd. This section is now carefully checked with Chapters 3 and 8 and relevant sections in other chapters are cited.
43359	59	4			Read "Detection and Indices (ETCCDI) in CMIP5 over CMIP3 " rather than "Detection and Indices (ETCCDI) indices in CMIP5 over CMIP3 " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
42965	59	7	59	9	Over the Gulf stream region the extreme precipitation is also improve with higher resultion. Scher, S., Haarsma, R. J., De Vries, H., Drijfhout, S. S., & Van Delden, A. J. (2017). Resolution dependence of extreme precipitation and deep convection over the G ulf S tream. Journal of Advances in Modeling Earth Systems, 9(2), 1186-1194. [Rein Haarsma, Netherlands]	Accepted and added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66939	59	7			A regional study has shown that CMIP6 models still struggle with reproducing aspects of the basic patterns associated with extreme precipitation: Agel, L, and M. Barlow, 2020: How Well Do CMIP6 Historical Runs Match Observed Northeast US Precipitation and Extreme Precipitation-related Circulation? In review. [Mathew Barlow, United States of America]	Accepted and added
66941	59	7			Evaluation of model circulation is an important aspect of evaulating how realistically models produce extreme precipitation (Barlow et al. 2019). Barlow, M., W.J. Gutowski, J.R. Gyakum, R.W. Katz, Y.K. Lim, R.S. Schumacher, M.F. Wehner, L. Agel, M. Bosilovich, A. Collow, and A. Gershunov, 2019. North American extreme precipitation events and related large-scale meteorological patterns: a review of statistical methods, dynamics, modeling, and trends. Clim. Dyn., 53, 6835-6875. https://doi.org/10.1007/s00382-015-2638-6 [Mathew Barlow, United States of America]	Accepted and added
108891	59	10	59	10	Borodina et al. demonstrates that CMIP5 models underestimate observed global TRENDS in heavy precipitation but does not evaluate the magnitude of heavy rainfall. [Erich Fischer, Switzerland]	Accepted and corrected
125923	59	10	59	10	"Models generally underestimate extreme precipitation (Borodina et al., 2017)." Borodina et al find that the dependence of extreme precipitation on temperature is underestimated (not the extreme precipitation itself). Global models are as likely to overestimate as underestimate extreme precipitation when the comparison is made on the same grid using conservative interpolation prior to calculating the extremes (see. e.g., Figure 4 of Chen and Knutson, J. Climate, 2008). [Trigg Talley, United States of America]	Accepted. The statement is corrected. This is underestimation of global trends
1469	59	16	59	23	This paragraph is not very clear. I'm not sure what the message really is even after several attempts to read it. [Rasmus Benestad, Norway]	Noted and added to the previous paragraph, more on supporting literature on the message from previous paragraph. This also provides some information about performance skill of CMIP5 models before comparing CMIP5 and CMIP6 in the next paragraph
13725	59	20	59	20	Change PRCTOT by PRCPTOT [Maria Amparo Martinez Arroyo, Mexico]	Accepted and corrected
1471	59	25	59	39	There is no reason to expect that the models reproduce the same statistics as seen in the observations based on rain gauges or higher-resolution gridded products. For the same reason that intensity-frequency-duration curves provide different return values for different time scales, the rainfall statistics is expected to vary with spatial scales beyond mesoscale systems. The main message is that extreme rainfall statistics cannot be read directly from GCMs because they represent a different type of information. But when this information is aggregated over time and space, they should hopefully reproduce similar aggregated observations. This is also connected to the model's minimum skillfull scales and to get realistic extremes from GCMs, the results need to be downscaled. [Rasmus Benestad, Norway]	Considered. This is discussed
89143	59	25		28	Another study evaluating CMIP6 extreme precipitation is Akinsanola, A. A., Kooperman, G. J., Pendergrass, A. G., Hannah, W. M., & Reed, K. A. (2020). Seasonal representation of extreme precipitation indices over the United States in CMIP6 present-day simulations. Environmental Research Letters. https://doi.org/10.1088/1748-9326/ab92c1 [Angeline Pendergrass, United States of America]	Accepted. Added
23169	59	26	59	30	These two sentences cannot both be simultaneously true (no difference / CMIP6 wetter) so please which is it? [Peter Thorne, Ireland]	Noted. We have highlighted them as the differences in findings from multiple studies.
20739	59	41	59	43	Does not this indicate basically good consistency between the metrics? [philippe waldteufel, France]	Taken into account. Across observational/ reanalysis datasets, corrected
65085	59	41	60	23	There is a place between the paragraph describing Fig 11.3 model performance, and the paragraph describing RCM performance, where there can be some mention of regional studies using CMIP5/6 to evaluate model performance. For example, we have looked at the ability of CMIP5/6 historical runs to generate realistic Northeast US extreme precipitation, and whether the extreme precipitation was generated by the same large-scale meteorolgocal patterns seen in observations. We found wide variations in the models' abilities, but overall higher-resolution models performed better. Most models generated extreme precipitation for the "right" dynamical reason: that is, they produced regional extreme precipitation in accordance with observed extreme precipitation circulation patterns. Papers: "Agel L, M Barlow, J Polonia, D Coe: Simulation of Northeast US Extreme Precipitation and Its Associated Circulation by CMIP5 Models. Journal of Climate (in revision)", "Agel L, M Barlow: How Well Do CMIP6 Historical Runs Match Observed Northeast US Precipitation and Extreme Precipitation-related Circulation? Journal of Climate (in revision)." [Laurie Agel, United States of America]	Accepted and added
51627	59	50	59	50	By "interchangeable" do you mean similar? I think that word would be easier for non-experts to understand. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	accepted
20253	60	1	60	14	Please indicate a reference for the definition of indices considered in the top chart of figure 11.13; possibly Donat et al (2013a) [philippe waldteufel, France]	Not applicable - panel removed
72109	60	17	60	23	One should pay attention about the literature cited for Africa about evaluation of heavy precipitation. For example Gbobaniyi et al. (2014) did not analyse heavy or extreme rainfall. [Mouhamadou Sylla, Rwanda]	accepted
106755	60	18	60	18	It should be corrected that Gbobabniyi et al, 2014 did not focuis on extreme rainfall, but rather on climatological aspects of rainfall (temperature also) and its interannual variability over west africa. However; Gibba et al. 2019 in TAAC (https://doi.org/10.1007/s00704-018-2650-y) performed an interesting analysis of extreme rainfall indices over whole Africa using CORDEX and CMIP5 models. [Moustapha Tall, Rwanda]	accepted
66385	60	19	60	19	These two paper could be added as well as reference for Europe Fantini A, et al., (2016) Assessment of multiple daily precipitation statistics in era-interim driven Med-CORDEX and Euro-CORDEX experiments against high resolution observations. Clim Dyn. https://doi.org/10.1007/s00382-016-3453-4; Vautard, R., Kadygrov, N., Iles, C., Boberg, F., Buonomo, E., and Al, E. (submitted). Evaluation of the large EURO-CORDEX regional climate model ensemble. J. Geophys. Res. (submitted). [Erika Coppola, Italy]	Noted. First literature added, the second one could not be found on 9th October 2020
71313	60	21	60	23	It seems better to explain "host GCMs", "parent GCMs" in studies using RCMs. [Kenji Taniguchi, Japan]	Accepted
66387	60	22	60	23	Please revise this statment by reviewing the paper Coppola, E., Raffaele, F., Giorgi, F., Giuliani, G., Xuejie, G., Ciarlo, J., et al. (submittedc). Climate hazard indices projections based on CORDEX-CORE, CMIP5 and CMIP6 ensemble. Clim. Dyn. (submitted). [Erika Coppola, Italy]	Considered. Text is modified to reflect that is the conclusion of the two specific papers rather than something in general.
23171	60	23	60	23	specifically in S. asia or generically in all regions? As written it implies the latter but I think you mean the former? [Peter Thorne, Ireland]	Accepted
43361	60	23			Read " (Mishra et al., 2014a; Singh et al., 2017). " rather than " (Mishra et al., 2014a; Singh et al., 2017) " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. Done with Mendeley
38423	60	25	60	26	Model evaluation of HighResMIP-class (resolution minimum 50 km in the atmosphere and 0.25° in the ocean) simulations (Haarsma et al., 2016) is incomplete. If this work is incomplete then this statement and Reference is not required. [Mansour Almazroui, Saudi Arabia]	Taken into account and reworded
62645	60	25	60	26	The statement is unsubstanciated. I understand the authors of this chapters did not have access to the results or literature of HighResMIP. But, it would be misleading to the readers when AR6 will be published in late 2020 or in 2021. The HighResMIP is now complete and data is freely available. So, please remove this sentence or replace with convincing and useful information. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. We have reworded this statement
42967	60	25	60	26	Demory et al. https://www.geosci-model-dev-discuss.net/gmd-2019-370/ Makes a comparison between CORDEX and HIghResMIP over Europe. [Rein Haarsma, Netherlands]	Considered. Much of the discussion about HighResMIP models is now removed and as a result, this paper is not cited.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
109379	60	25	60	34	There seems little information here, suggest either deleting or adding any relevant text elsewhere. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Considered. Much of the discussion about HisResMIP models is removed.
90551	60	25	60	34	There is a study reviewing extreme precipitaiton ove rthe US in GCMs at various resolutions (200, 50, 25km atmosphere/land resolution and 1 degree ocean). This work shows that increased resolution improves not only extreme precipitation magnitudes, but also the timing of extreme precipitation. I suggest you add a line after the summary of fvCM5.1 in line 30: "Meanwhile, both the magnitude and timing of extreme precipitation over the U.S. has also been shown to dramatically improve when resolution is increased from 200 to 50 to 25 km in the GFDL CM2.1, FLOR, and HiFLOR models (van der Wiel et al. 2016). Full reference: Van Der Wiel, K., Kapnick, S.B., Vecchi, G.A., Cooke, W.F., Delworth, T.L., Jia, L., Murakami, H., Underwood, S. and Zeng, F., 2016. The resolution dependence of contiguous US precipitation extremes in response to CO2 forcing. Journal of Climate, 29(22), pp.7991-8012. [Sarah Kapnick, United States of America]	Considered. Much of the discussion about HighResMIP models is now removed and as a result, this paper is not cited.
1473	60	25	60	34	Why include this sentence "Model evaluation of HighResMIP-class (resolution minimum 50 km in the atmosphere and 0.25° in the ocean) simulations (Haarsma et al., 2016) is incomplete." and what message does it convey to the reader? Is it important? Also, the rest of the paragraph is very vague and difficult to follow. [Rasmus Benestad, Norway]	Accepted and Reworded
13727	60	27	60	27	standardize the RX5day or Rx5day format [Maria Amparo Martinez Arroyo, Mexico]	Accepted. We used RX5day consistently now
8701	60	27	60	27	capitalisation of Rx5day not correct on this and subsequent page [Robert Dunn, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We used RX5day consistently now
109381	60	32	60	32	In what sense are they "much improved". Also, please comment on the magnitudes of the simulated extremes. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Considered. Much of the discussion about HighResMIP models is now removed.
3685	60	32	60	32	Also improvement with convection-permitting model over East Africa at sub-daily timescale https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-18-0387.1?mobileUi=0 [Declan Finney, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
66389	60	32	60	34	These two submitted papers based on CP model ensembles that are now under revision could be added Ban et al, The first multi-model ensemble of regional climate simulations at kilometer-scale resolution Part I: Evaluation of precipitation, Climate Dynamic, submitted; Pichelli et al, The first multi-model ensemble of regional climate simulations at kilometer-scale resolution part 2: future precipitation projections, Climate Dynamic, submitted [Erika Coppola, Italy]	Noted. But the authors do not have access to the papers as on 9th October 2020
55175	60	32	60	34	Similar improvements in daily and sub-daily precipitation distributions are evident over northeastern North America (Innocenti et al., 2019) and Canada (Cannon and Innocenti, 2019). -- Innocenti, S., A. Mailhot, A. Frigon, A.J. Cannon, and M. Leduc, 2019. Observed and simulated precipitation over northeastern North America: how do daily and sub-daily extremes scale in space and time. Journal of Climate, 32:8563-8582. doi:10.1175/JCLI-D-19-0021.1 -- Cannon, A.J. and S. Innocenti, 2019. Projected intensification of sub-daily and daily rainfall extremes in convection-permitting climate model simulations over North America: Implications for future Intensity-Duration-Frequency curves. Natural Hazards and Earth System Sciences, 19:421-440. doi:10.5194/nhess-19-421-2019 [Nancy Hamzawi, Canada]	Accepted
66537	60	32	60	34	Here, two examples of convective-permitting models are mentioned. There are a growing number of these models and simulations in the literature and examples from the literature should be better reflected here. Examples includes the series of papers by Kendon et al (in the reference list of this chapter) from the MOHC on both UK and African conditions, the papers by Prein et al (also in the reference list of this chapter) on conditions in the US and the papers by Ban et al (also in the reference list) on conditions in the Alpine region. Another paper not included in the reference list confirming the picture of better performance in representing the diurnal cycle of precipitation and extreme conditions both over Europe and over Lake Victoria in Africa in a convective-permitting model is by Belušić, et al 2020 (DOI: 10.5194/gmd-13-1311-2020). [Kjellström Erik, Sweden]	Accepted. Reference added
51629	60	36	60	37	This sentence seems to contract p. 11-59 L50-51 which states that "in general, CMIP5 and CMIP6 are interchangeable in their performance in simulating the observed climatology of extreme precipitation (high confidence)". Could you clarify the text to avoid confusing the reader? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Considered. Text edited.
62713	60	36	60	37	I am not sure how I can put this sentence in relation to other statements in this chapter. It states that ability to simulate climate extremes (in general or here only for precipitaton) has steadily increased since AR5 mainly due to grid refinements. However, as stated e.g on Page 59 Line 50-51 CMIP5 and CMIP6 are interchangeable in their perfomance in simulating the observed climatology and also the summary on Page 60 Line 50-51 states that CMIP6 and CMIP5 models are comparable in their performance. But maybe also the original sentence only refers to High Resolution Models and not to simulations of the CMIP? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. Text edited.
42521	60	38	60	38	Typo (here and similarly elsewhere): 25km -> 25 km [please check and be consistent; in the document sometimes a blank space is left before 'km' sometimes not] [Joan Bech, Spain]	Noted. "25km" is removed in the text.
109383	60	38	60	38	There is no clear evidence for the "considerably better" statement so please substantiate or remove. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Reworded
109385	60	41	60	42	Please provide evidence for the "challenging issue" and "need improvements". [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Considered. The text is removed.
113601	60	47	60	47	Remove and fit paragraph with the rest for consistency with other sections. [Diego Miralles, Belgium]	Considered. Text and headings for all sections now follow the same style.
66377	60	49	60	55	No mention of the added values of RCM compared to CMIP although is it stated in the text above these lines, but it is mentioned the improvement of HIghResMIP models. [Erika Coppola, Italy]	Considered. Text edited.
102559	60	49	61	2	No RCM based conclusions - could be added. [Philippe Tulkens, Belgium]	Considered. RCM summary is embedded in the paragraph.
66943	60	49			I don't agree with the statement, "There is high confidence in the ability of CMIP6 models to capture large-scale features of precipitation extremes." Perhaps because I'm not sure what is meant by "large-scale features." Does that mean the spatial distribution of extremes? If so, maybe. Would be useful to clarify and have clear tracability on this statement. In terms of regional atmospheric circulation, the CMIP6 models still have some substantial struggles, at least in some regions: Agel, L, and M. Barlow, 2020: How Well Do CMIP6 Historical Runs Match Observed Northeast US Precipitation and Extreme Precipitation-related Circulation? In review. [Mathew Barlow, United States of America]	Considered. Text is edited to be specific about "spatial distribution".
109387	60	51	60	53	The evidence for this medium confidence statement is not clear. Please provide or remove this statement. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Much of discussion on HighResMIP is removed.
51631	60	52	60	53	Do you mean "Some CMIP6… somewhat more realistic values of extreme precipitation *than lower resolution CMIP6 models*"? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Summary is shortened and this is not applicable any more.
109843	61	5	61	5	To me this section is not about the causes of the changes, it is all about attribution. To get some of the mechanisms in here as well then see section in Fowler et al. review paper: Fowler, H.J., Lenderink, G., Prein, P., Westra, S., Allan, R.P., Ban, N., Barbero, R., Berg, P., Blenkinsop, S., Do, H.X., Guerreiro, S., Haerter, J., Kendon, E., Lewis, E., Schaer, C., Sharma, A., Villarini, G., Wasko, C., Zhang, X. Intensification of short-duration rainfall extremes with global warming and implications for flood hazard. Submitted to Nature Reviews Earth and Environment, March 2020. [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The focus here is attribution rather than the physical mechanisms.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
62651	61	7	63	8	The structure of "11.4.4 Causes of the observed change" is unorganized. It is difficult to understand the connections between each paragraphs as well as the flow across all the paragraphs. I have a rough sense that the authors want to addres the change in extreme precipitation at large-scale first (P61 L16-49), then address the change at regional scale (P61 L51~), including both anthropogenic influences and other influences. However, the intention of each paragraph is unclear. Sometimes it seems like just saying similar ideas but in different ways. I list some specific comments in the following comments. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. The section is heavily edited to improve flow and to focus on assessment.
62675	61	7	63	8	In general, the descriptions lack of clarity of what "change" they are talking about: change in frequency (for a given magnitude) or change in magnitude (for a particular return period)? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The text is edited to improve clarity.
62653	61	11	61	17	Are these two sentences (L11-12 "limited evidence of anthropogenice influence …." & L16-17 "detection and attribution analyses have provided consistent and robust …") consistent with each other? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The text is edited to improve clarity.
1475	61	14	61	34	It is also relevant to consider trends in the wet-day frequency (the number of rainy days) as well as trends in the intensity. We would expect to see a change in the extremes, purely as a result of changes wet-day frewquency (which affects the effective sample size) even if the pdf (for the wet-days) were to be the same. This is a statistical effect connected to probabilities. The hypothesis is that anthopogenic climate change at least affects the pdf for (wet-day) precipitation in terms of increasing the rate of evaporation, the atmospheric moisture and hence the mean rainfall intensity (the "thermodynamic effect"). The wet-day frequency, on the other hand, is to a large extent controlled by the atmospheric circulation patterns (the "dynamic effect"). [Rasmus Benestad, Norway]	Noted, the section is written based on available literature. This is also restructured now.
39303	61	14	62	50	Please synthesize-It looks like a literature review. [Lourdes Tibig, Philippines]	Considered. The text is heavily edited to synthesize.
13729	61	22	61	22	it is recommended to standardize the units: K or ° C [Maria Amparo Martinez Arroyo, Mexico]	Accepted
11695	61	22	61	22	why is temperature expressed in Kelvins here? It's been degrees C throughout the report until now. [Amy East, United States of America]	Accepted
74535	61	23	61	23	To check if it isn't published [Moulay Driss HASNAOUI, Morocco]	Accepted. Checked and updated
62655	61	25	61	27	"over global land areas, in the mid-to-high latitudes, western and eastern Eurasia, and the global dry regions" This is very confusing. What is the exact area the author mean here? Doesnt "global land area" already cover the rest of area they mentioned (mid-to-high latitude, Euasia, dry region...)? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reworded
42523	61	26	61	26	Typo:gobal -> global [Joan Bech, Spain]	Accepted
74537	61	28	61	28	To check if it isn't published [Moulay Driss HASNAOUI, Morocco]	Accepted. It is not yet published and deleted. The conclusions from this study is same as Paik et al. (2020)
74539	61	30	61	30	Paik et al. In place of Paik and et al. .. And chek if it isn't published. [Moulay Driss HASNAOUI, Morocco]	Accepted. Checked and updated
74541	61	30	61	30	for ref. Kirchmeier-Young and Zhang (submitted) To check if it isn't published [Moulay Driss HASNAOUI, Morocco]	Accepted. Checked and updated
11697	61	30	61	30	Paik et al." (delete "and") [Amy East, United States of America]	Accepted. The sentence is now deleted
43363	61	30			Read "the same results as Paik et al. (submitted)" rather than "the same results as (Paik and et al. (submitted)" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. The sentence is now deleted
62657	61	36	61	42	The purpose of this paragraph is confusing. I would expect that the authors list other factors (other than anthropogenic forcing) that can also drive the observed change in extreme precipitation. However, they only list volcanic impact based on "one" study. How important is this factor (compared to other factors) contributing to the large-scale long-term change in extreme precipitaiton ? And how long did the volcanic impacts remain in the record after volcanic eruptions? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. Text is heavily edited.
62659	61	44	61	49	How is this paragraph different from the paragraph P61 L14-34. It seems to me they are both about the change in Rx5day at large-scale area (e.g. global land area) was mainly caused by anthropogenic forcing. I would suggest to combine this paragraph into L14-34. They are repetitive. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The section is restructured
79121	61	47	61	47	grammar error, also on Line 53 [Andong Shi, Sweden]	Accepted. Corrected
108893	61	47	61	47	A reference to support this statement would be Borodina, A., Fischer, E.M. and Knutti, R., (2017). Models are likely to underestimate increase in heavy rainfall in the extratropical regions with high rainfall intensity. Geophysical Research Letters, 44(14), 7401-7409, doi:10.1002/2017GL074530. [Erich Fischer, Switzerland]	Accepted
117107	61		61		interesting findings linked to volcanic eruptions, could they be relfected in the ES to support integration x chapters for the TS? [Valerie Masson-Delmotte, France]	Noted. This is just a single evidence
13731	62	3	62	3	change mm /day by mm/day [Maria Amparo Martinez Arroyo, Mexico]	Accepted
23967	62	3			Suggest remove "occurrences", since it is not required and makes the sentence more difficult to read. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
84907	62	6	62	6	Why is only North America cited here when strong evidence for anthropogenic influnce has been found in other regions such as the UK. [Turner Jessica, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Could not understand this comment as the specific line number corresponding to a empty line.
62661	62	7	62	23	What is the main topic of this paragraph? The topic sentence (the first sentence) is about the long-term changes in heavy precipitation of longer duration events. However, the rest of paragraph doesn't really mention anything about longer duration. I would guess the topic of this paragraph is that: whether or not anthropogenic influecnes have affected historical heavy precipitation events (in terms of probability or magnitude) depends on the spatial and temporal scales of the targets. If this is the case, I would suggest the authors to re-address the topic sentence of this paragraph. If not, more connection/transition sentences or words are necessary to make the paragraph more complete. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The entire section is restructured now
51633	62	16	62	17	Does this sentence refer to the same specific events referenced in the previous lines 12 - 15? If so could you clarify in the text please as currently this is worded a bit confusingly. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Clarified
42525	62	19	62	19	Typo: eventrs -> events [Joan Bech, Spain]	Accepted
23173	62	19	62	23	I could just barely follow this passage. Edits for clarity would be very useful. [Peter Thorne, Ireland]	Accepted. The entire section is restructured now
62717	62	19	62	23	For me the second sentence is kind of redundant to the first, however the first sentence is not complete or a mixture of two sentences I think. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The entire section is restructured now
70165	62	19			A typo "eventrs", should be "events" [Huanping Huang, United States of America]	Accepted
43365	62	19			"extreme precipitation events" or "extreme precipitation eventrs "? [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
62663	62	25	62	27	I am not sure if this paragraph focues on change in extreme precipitation at regional scale, but I would suggest that the authors consider to move this paragraph forward (e.g. right after the paragraph P61 L14-34) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The entire section is restructured now
24493	62	32	62	36	The description of "very extreme precipitation will be increased 6-7% per degree Celsius" is something strange because the annual precipitation will be increased in 1-3% per degree Celsius in page 149. It is generally opposite. [Nobuhito Mori, Japan]	Noted. Extreme precipitation increases more than total (annual) precipitation for the same amount global warming.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1477	62	33	62	37	I'm not sure if I understood this sentence: "There are multiple cases indicating an increase in very extreme precipitation in relation to temperature above 6-7%/ °C, the Clausius-Clapeyron rate". Does it mean more rapid increase than the CC-rate? Also, when dealing with 'exceedingly rare' events, we are looking at the exceptions to the rule and may get fooled by the law of small numbers, random sampling fluctuations, and so-called 'freak events' (and if they are not selected at random, the analysis may suffer the same caveats as "cherry picking"). I think the paragraph need to be clearer and provide more information about how these cases are representative for the pattern of climate change. [Rasmus Benestad, Norway]	Noted, clarified and reworded
104587	62	34	62	36	A new compelling observation evidence from Zhou and Wang (2017) shows extreme precipitation increased with mean air temperature by above 10%/°C in China. Please include this article in the citation in Lines 36. Reference: Zhou, C., and K. Wang, 2017: Quantifying the sensitivity of precipitation to the long-term warming trend and interannual-decadal variation of surface air temperature over China. J. Clim., 30, 3687-3703. [Chunlüe Zhou, United States of America]	Noted. The conclusion of this paper does not seem to be convincing as it is in sharp contrast with the understanding of significant warming and general lack of significant change in extreme precipitation in China.
70171	62	38			It is not clear to me what this actually means. After checking section 11.4.1, I also don't find much evidence to support the statement. It will be great to futher clarify it. [Huanping Huang, United States of America]	Accepted. The sentence is deleted to avoid confusion
23175	62	41	62	50	You are about to go to a section on floods so why include such a paragraph here? Feels like it is in the wrong place. [Peter Thorne, Ireland]	Considered. Discussion about flood is removed from here.
104397	62	41	62	50	Discussion here omits the evidence that development has dramatically increase resilience to flooding and this increased resilience counters and masks the risk of flooding driven by increase in seasonal/annual precipitation and increases in extreme precipitation. See: USGS publication Flow Modification in the Nations's Streams and Rivers 2019 (https://doi.org/10.3133/cir1461) which reports that due to the "modification of natural flows in streams and rivers... high flows have been reduced in magnitude, are of shorter duration, are less frequent, and vary less from one year to the next than they would naturally." See also Formeta and Feyen, 2019: Empirical evidence of declining global vulnerability to climate-related hazards (https://doi.org/10.1016/j.gloenvcha.2019.05.004) which reports increasing resilience to extreme weather events with increasing development. [Hunter Cutting, United States of America]	Considered. Discussion about flood is removed from here.
107691	62	44	62	45	Shouldn't this be "attribution of the probability of a precipitation to human influence" rather than the other way around? And the same in line 45, "does not by itself directly attribute the flood or the related damages to human influence". Either way, this sentence is a bit unclear and could be clarified. [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reworded
9155	62	50	62	50	Anthropgenic influence was detected in New Zealand for extreme rainfall in 2014 in Northland (Rosier et al., 2015) who state "An event as extreme as that of July 2014 is estimated to change from approximately 1-in-350-yr in the “natural” world to 1-in-200-yr with anthropogenic influences, with a corresponding FAR of 47%. In northwest Australia, the extreme rainfall increase since 1950 can be related to increased monsoonal flow due to increased aerosol emissions, but cannot be attributed to an increase in greenhouse gases (Dey et al., 2018)" [Kevin Hennessy, Australia]	Accepted
40543	62	52	62	52	No confidence language [TSU WGI, France]	Accepted
125925	62	52	62	53	"... it is likely that anthropogenic influence is the main cause of the observed intensification of heavy precipitation in land regions ..." Is this too strong? See Figure S1 of Fischer and Knutti (2015). 18% of moderate daily precipitation extremes over land are attributable to the observed temperature increase which in turn mainly results from human influence. So how do authors get to "main cause" which seems to imply more than 50%, not less that 20%. [Trigg Talley, United States of America]	Noted. This is not too strong in light of Fischer and Kuntti (2015) there 18% of moderate events is attributed to human influence. What is stated here is the portion of "intensification" rather than total amount of increase (in number of events)
39483	62	54	63	1	Recheck whether it should be "to' or 'and' in 'one to five days' on page 61 line 12 and 'one-day and five-day precipitation' on pages 62-63 line 54 and 1. [Tamara van 't Wout, Qatar]	Accepted. One and five days
109607	63	5	63	6	The assertion that there is evidence of human influence on the intensification of exreme precipitation in North America is contradicted by several studies that identify internal decadal ocean variability (Hoerling, M. et al. 2016. Characterising recent trends in U.S. heavy precipitation. Journal of Climate, vol. 29, no. 7, pp. 2313-2332), intrinsic variability (van de Wiel, K. et al. 2016. The resolution dependence of contiguous U.S. precipitation extremes to CO2 forcing. Journal of Climate, vol. 29, no. 22, pp. 7991-8012), and observational and modelling uncertainties (Sarojini et al. 2016. Detection and attribution of human influence on regional precipitation. Nature Climate Change, vol. 6, no. 7, DOI: 10.1038/nclimate2976). [Reynold Stone, Trinidad and Tobago]	Noted. Evidence of human influence does not contradict contribution of other factors including internal variability.
6875	63	6	63	6	Replace "in North America" with "various parts of the world" [Christos Zerefos, Greece]	Considered. Text edited to also include other continents.
24099	63	6	63	8	"There is also new evidence of human contributions to the increase in the probability or magnitude of some individual events" Again, like for extreme heat events, this conclusion statement is stepping back from making an assessment, rather just referring to new studies (as in temperature) or new evidence (as here). It would be good if this could become an assessment statement about whether some extreme rainfall events have actually been affected. Like has been done for flood event attribution (p71 lines 3-5). [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Considered. Relevant text is edited.
51635	63	6	63	8	"There is also new evidence of human contributions to the increase in the probability or magnitude of some individual events" Again, like for extreme heat events, this conclusion statement is stepping back from making an assessment, rather just referring to new studies (as in temperature) or new evidence (as here). It would be good if this could become an assessment statement about whether some extreme rainfall events have actually been affected, such as those done for flood event attribution (p71 lines 3-5). [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Considered. Text is edited.
39305	63	11	63	43	Same comment as above [Lourdes Tibig, Philippines]	Noted
18369	63	11	66	16	section 11.4.5: Please note that Fig. 1 of Dai et al. (2018) showed the change map for heavy precipitation in CMIP5 models. The change pattern of heavy precipitation broadly follow the mean precipiutation change patterns, with increases over most regions except for many subtropical regions whether the mean P decreases. The same change patterns are seen in CMIP3 models (Sun et al. 2007). Refs cited: . Dai, A., T. Zhao, and J. Chen, 2018: Climate change and drought: A precipitation and evaporation perspective. Current Climate Change Reports, 4, 301-312. DOI: 10.1007/s40641-018-0101-6. (http://link.springer.com/article/10.1007/s40641-018-0101-6) Sun, Y., S. Solomon, A. Dai, and R. Portmann, 2007: How often will it rain? J. Climate, 20, 4801-4818. [Aiguo Dai, United States of America]	Noted and added
1479	63	13	63	29	Has there been any update on the analysis of record-breaking monthly precipitation simulated by the GCMs? Some of the first applications of the record-breaking statistics was applied to climate models involved the monthly mean precipitation from 31 simulations of the CMIP3 SRES A1b ensemble (Benestad 2006; DOI: 10.1175/JCLI3656.1), which found trends in extreme monthly rainfall in the high latitudes and parts of the tropics, in addition to decrease in extremes in parts of the subtropics. This study was not cited in the AR5, but this type of analysis nevertheless provides useful information about trends in precipitation on timescales beyond a day (which apparently tends to be lacking). I would be surprised if the conclusion from that study has changed with the new CMIP simulations. [Rasmus Benestad, Norway]	Noted. There does not appear to be new update on this.
11699	63	17	63	18	fix/move parenthesis [Amy East, United States of America]	Accepted
43367	63	17		18	Read " confirmed by Sillmann et 17 al. (2017a)." rather than " confirmed by (Sillmann et 17 al., 2017a)." [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
13733	63	18	63	18	Change (Sillmann et al., 2019) by Sillmann et al. (2019) [Maria Amparo Martinez Arroyo, Mexico]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43369	63	18			Read " Sillmann et al. (2019) further showed" rather than " (Sillmann et al., 2019) further showed" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
71649	63	20	63	21	"CHAPTER 4" is not a forcing agent. [Ryo Mizuta, Japan]	Accepted
42527	63	21	63	21	Typo: that that -> that [Joan Bech, Spain]	Accepted
13735	63	21	63	21	Delete a that [Maria Amparo Martinez Arroyo, Mexico]	Accepted
89145	63	21		24	We showed in Pendergrass et al (2019) that the interpretation in Lin et al (2016) was incorrect and the interpretation in Lin et al (2018) should be taken with skepticism. Specifically, that extreme precipitation change (specificially rx1day annual), responds nonlinear to changes in temperature, such that the respone that they attribute to aerosol forcing would have also resulted from the same amount of warming driven by greenhouse gas increase. Pendergrass, A. G., Coleman, D. B., Deser, C., Lehner, F., Rosenbloom, N., & Simpson, I. R. (2019). Nonlinear Response of Extreme Precipitation to Warming in CESM1. Geophysical Research Letters, 46(17–18), 10551–10560. https://doi.org/10.1029/2019GL084826 [Angeline Pendergrass, United States of America]	Noted, text edited.
43371	63	21			Read "Both Lin et al. (2016) and Lin et al. (2018) found that the " rather than "Both Lin et al. (2016) and Lin et al. (2018) found that that the " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
13737	63	23	63	24	Change Lin et al. (2016) and Lin et al. (2018) by Lin et al. (2016, 2018) [Maria Amparo Martinez Arroyo, Mexico]	Accepted
23179	63	28	63	29	I don't see how this statement is obviously supported by the preceding text as it stands. [Peter Thorne, Ireland]	Accepted. We have now added the supporting literature
1481	63	31	63	32	"Changes in Rx1day during the historic period for half-a-degree warming are consistent with the difference in the projected changes for 1.5°C and 2°C warming scenarios, as simulated by global models" - in what respect? [Rasmus Benestad, Norway]	Taken into account and reworded
1483	63	36	63	37	The mean precipitation is of course the product between the wet-day frequency and the wet-day mean precipitation. If the mean precipitation decreases but the intensity increases, that mean that the frequency also decreases. I.e. more droughts interrupted by intense and violent extreme rainfall events. That is bad news. [Rasmus Benestad, Norway]	Accepted. The sentence is now deleted based on Dai et al. (2018), Figure 1, see comment id 18369
108895	63	37	63	39	In some seasons there may also be reductions over land areas such as southern Europe around the Mediterranean Basin. [Erich Fischer, Switzerland]	Accepted
45685	63	38	63	41	Please check the arguably incomplete sentence [Christophe Deissenberg, Luxembourg]	Considered, sentence is revised.
71651	63	40	63	43	"Projected long-period Rx1day return value changes are larger than changes in mean Rx1day and increase with increasing rarity" This is also shown to be the case in most parts of the world by Mizuta and Endo (2020, GRL) using a high-resolution GCM large ensemble. They show the dynamic process is responsible for the larger increase with increasing rarity. Mizuta, R., and H. Endo, 2020: Projected changes in extreme precipitation in a 60-km AGCM large ensemble and their dependence on return periods, Geophys. Res. Lett., in press. [Ryo Mizuta, Japan]	Accepted
89147	63	41		43	The dependence of extreme precipitation on its definition (its rarity) is also illustrated in Pendergrass (2018). This study also highlights the substantial uncertainty across (CMIP5) models, which should be acknowledged too. Pendergrass, A. G. (2018). What precipitation is extreme? Science, 360(6393), 1072–1073. https://doi.org/10.1126/science.aat1871 [Angeline Pendergrass, United States of America]	Accepted
43373	63	41			Read " (Li et al., submitted; Wehner, submitted)" rather than " (Li et al., submitted; (Wehner, submitted)" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
102561	63	43	63	43	"...with the rarity considered" -> rephrase. [Philippe Tulkens, Belgium]	Accepted
74543	63	21 and 22	63	21 and 22	"found that the rate" in place of "found that that rate" [Moulay Driss HASNAOUI, Morocco]	Accepted
117109	63		63		"independent of the forcing" : is this correct if you compare temperature increase due to reduction in emission of aerosols or increased emissions of CO2? [Valerie Masson-Delmotte, France]	Noted. This is supported by literature (e.g. Sillmann et al. 2019).
100503	64	2	64	2	Lake Victoria in East Africa has been identified as a regional hotspot of extreme precipitation intensification in East Africa under RCP8.5 due to mesoscale processes intensifying the regional-scale pattern (Thiery et al., 2016). REF: Thiery, W., Davin, E. L., Seneviratne, S. I., Bedka, K., Lhermitte, S., & Van Lipzig, N. P. (2016). Hazardous thunderstorm intensification over Lake Victoria. Nature communications, 7(1), 1-7. [Wim Thiery, Belgium]	Accepted. Reference added at appropriate place
100501	64	4	64	4	Please change 'RCP8' to 'RCP8.5' [Wim Thiery, Belgium]	Accepted
71653	64	4	64	6	The sentense is the same as the first sentense of the previous paragraph (P63L31). [Ryo Mizuta, Japan]	Accepted. Deleted
23181	64	4	64	8	You already said all of this earlier in the section. There is no need to repeat points in this manner. Merge and reconcile. [Peter Thorne, Ireland]	Accepted
39307	64	4	64	34	Same comment as above [Lourdes Tibig, Philippines]	Accepted. The references are correctly written
89151	64	4		26	The response of extreme precipitation to increasing warming levels should cross reference and be consistent with the analogous discussion for mean precipitation in Chapter 8, section 8.5.3.1. [Angeline Pendergrass, United States of America]	Accepted. The section from chapter 8 is cited at appropriate place
89149	64	6		17	We showed in Pendergrass et al (2019) that rx1day scales quadratically with temperature in CESM1 simulations, and that there is a nonlinear component to rx1day change in 2/3 of CMIP5 models, potentially contradicting the statements in this paragraph tha extreme precipitation scales linearly with warming. Pendergrass, A. G., Coleman, D. B., Deser, C., Lehner, F., Rosenbloom, N., & Simpson, I. R. (2019). Nonlinear Response of Extreme Precipitation to Warming in CESM1. Geophysical Research Letters, 46(17–18), 10551–10560. https://doi.org/10.1029/2019GL084826 [Angeline Pendergrass, United States of America]	Accepted. The sentence related to linear relationship is now deleted
108897	64	13	64	15	I think somewhere in this section the role of internl variability needs to be discussed. I think the statements about the increase in the forced response could be even stronger but I recommend to acknowledge that these rainfall intensification could be partly offset or amplified due to internal variability. [Erich Fischer, Switzerland]	Accepted
38161	64	24	64	29	Below study looks very relevant to cite here, which examined East Asian and South Asian monsoon responses including extreme precipitations based on HAPPI simulations: Lee D., S.-K. Min, E. Fischer, H. Shiogama, I. Bethke, L. Lierhammer, and J. Scinocca, 2018: Impacts of half a degree additional warming on the Asian summer monsoon rainfall characteristics. Environ. Res. Lett., 13, 044033, doi: 10.1088/1748-9326/aab55d. [Junhee Lee, Republic of Korea]	Accepted
23969	64	26	64	27	Has the HAPPI project been defined somewhere? Perhaps better to say "HAPPI multi-model model data" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
29469	64	29		32	the HadGEM2 model also projected annual increase in west Africa. This model can be used to compare the CORDEX being used and get the required projected increase in west Africa (supporting the existing projection.) [Babatunde Oyekan, Nigeria]	Noted, but no literature found on projections wrt warming levels with CORDEX for this region
108313	64	52	64	52	north Africa' should be North Africa for consistency with the definition [Nana Klutse, Ghana]	Accepted. We have used small letter in the entire paragraph
108335	64	52	64	55	Inconsistence confidence statements for extreme precipitation over Sahara and heavy precipitation over West Africa. It is Southern West Africa? Which extreme, dry or wet? [Nana Klutse, Ghana]	Noted. Reworded to avoid confusion

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3687	64	54	65	2	I don't believe any of these references consider recent conection-permiting climate change simulations over Africa, which greatly modify our view of how extreme precipitation can change. Over East Africa,the convection-permitting model shows widespread and super-CC increases in extreme rainrate, whilst a parametrised convection model does show some large increases in extreme rain rate but these highly-localised around locations of strong mesoscale forcings. https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-19-0328.1?mobileUi=0 . Important results have also been found using the convection-permitting simulation over West Africa https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019GL083544 AND https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-19-0380.1?af=R&mobileUi=0 AND Africa as a whole https://www.nature.com/articles/s41467-019-09776-9 [Declan Finney, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Literature added at appropriate place in this sub-section
28969	64	55			See also Dunning et al. (2018) J. Clim https://doi.org/10.1175/JCLI-D-18-0102.1 which shows increased daily rainfall intensity but later end to the wet season over west Africa and the Sahel, linked to a strengtheining Sahara heat low (a previous comment on the FOD was apparently accepted and added but I could not see an update - you can mark as noted or rejected if you like, I won't be offended!) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, reference is added
74545	64	6 an 7	64	6 an 7	for ref. Li et al. (submitted) To check if it isn't published [Moulay Driss HASNAOUI, Morocco]	Accepted. Deleted, the article is not yet published
74547	64	6 an 7	64	6 an 7	for ref. Seneveratne and Hausen (submitted) To check if it isn't published [Moulay Driss HASNAOUI, Morocco]	Taken into account and Updated
66339	65	1	65	5	There is a conflict in the confidence assessment with CH12 table 12.3; [Erika Coppola, Italy]	Noted. Table 12.3 does not have entry on heavy precipitation
23741	65	1	65	5	You may consider including the finding of this paper: "Mtongori, H., F. Stordal, and R. Benestad, 2015: Evaluation of Empirical Statistical Downscaling models' skill in predicting Tanzanian rainfall and their application in providing future downscaled scenarios. Journal of Climate, 29, 3231-3252 DOI: http://dx.doi.org/10.1175/JCLI-D-15-0061.1" The paper gives information of projection of precipitation over Tanzania (East Africa) in terms of wet day mean, frequency and total rainfal. One of the main finding is that, the projected increase in total rainfal is not necessarily due to increase in intensity and frequency of rainfall events in all locations and seasons. some areas/season the change is due to intensity alone and some area/season due to frequency alone and some due to both. In addition the paper has provided projections in three future periods, 2010-2039, 2040-2069 and 2070-2099 using multimodel ensemble. [Ladislaus Chang'a, United Republic of Tanzania]	Accepted and added
38427	65	4	65	4	RCP8 should be RCP8.5 [Mansour Almazroui, Saudi Arabia]	Accepted
108315	65	4	65	4	RCP 8' should be RCP 8.5 [Nana Klutse, Ghana]	Accepted
13739	65	4	65	4	Change RCP8 by RCP8.5 [Maria Amparo Martinez Arroyo, Mexico]	Accepted
11701	65	4	65	4	should RCP8 be RCP8.5? I think so [Amy East, United States of America]	Accepted
38425	65	4	65	6	Changes in Rx1day during the historic period for half-a-degree warming are consistent with the difference in the projected changes for 1.5°C and 2°C warming scenarios as simulated by the global models (Fischer and Knutti, 2015). This is the repitition of Lines 31-33 on page 64. [Mansour Almazroui, Saudi Arabia]	Accepted and deleted
108317	65	5	65	5	However, western South Africa heavy rainfall amounts are projected to decrease' should have a referencce [Nana Klutse, Ghana]	Accepted and added
66341	65	14	65	15	There is a conflict in confidence statment with CH12 table 12.4; [Erika Coppola, Italy]	Noted. Table 12.3 does not have entry on heavy precipitation
39309	65	14	65	27	Here are additional papers that could be considered in this paragraph-Supri et al, 2020 (Multi-model projections of precipitation extremes in southeast Asia based on CORDEX-SEA simulations); and Mandapaka et al, 2017 (spatio-temporal of rainfall extremes in Southeast Asia: Implications for flood risk assessment [Lourdes Tibig, Philippines]	Noted. Supri et al. (2020) added
66343	65	21	65	22	There is a conflict in confidence statment with CH12 table 12.4; [Erika Coppola, Italy]	Noted. Table 12.4 does not have entry on heavy precipitation
42529	65	24	65	24	Typo: 2018Han -> 2018; Han [Joan Bech, Spain]	Accepted.
38163	65	25	65	25	Given many studies for East Asian extreme precipitation projections, can't the confidence level be promoted? Below paper is missing here, which show consistent intensification of extreme precipitation over East and Southeast Asia: Lee D., S.-K. Min, E. Fischer, H. Shiogama, I. Bethke, L. Lierhammer, and J. Scinocca, 2018: Impacts of half a degree additional warming on the Asian summer monsoon rainfall characteristics. Environ. Res. Lett., 13, 044033, doi: 10.1088/1748-9326/aab55d. [Junhee Lee, Republic of Korea]	Accepted, the reference is added at an appropriate place in the sub-section
96127	65	25	65	25	Please check: For coastal floods "sea level rise" is maybe less important than "strong upland winds". [Nicole Wilke, Germany]	Rejected. Not related to section 11.4, heavy precipitation
96129	65	29	65	29	Please amend: "stream flow" --> "streamflow and water levels". [Nicole Wilke, Germany]	Rejected. The wording "stream flow" does not appear at the referred page/line.
82789	65	29	65	34	"northern" and "southern Australia" are better to avoid possible confusion with the state of South Australia. [Blair Trewin, Australia]	Accepted
9157	65	29	65	34	Seems to conflict with section 7.2.2 of CSIRO and BoM (2015) Climate change in Australia Technical Report at www.climatechangeinaustralia.gov.au/publications which says "extreme rainfall events are projected to increase in intensity (high confidence)". Projected changes in RX1-day and RX1-1day-RV20 are provided for RCP4.5 and RCP8.5 for 2081-2099 relative to 1985-2005 for 4 regions covering Australia, based on 21-24 CMIP5 GCMs. Alexander and Arblaster analysed 22 CMIP5 GCMs and found “most intense precipitation extremes increase substantially, with a separation becoming clear between emissions scenarios … Projected changes in precipitation extremes show increases in the most intense rainfall events across most of the country, though with very few regions of significant and consistent change across the models... Future changes in precipitation extremes are less consistent across the models and most regions show little significant and robust change over the 21stC”. However, Fig 16 and Table 7 show robust increases in RX5-day. The paper by Evans et al (2017) only applies to southeast Australia based on downscaling 4 CMIP3 GCMs where they found “Across a range of metrics, robust increases in the magnitude of precipitation extreme indices are found. While these increases are often in-line with a continuation of the trends present over the last century, they are not found to be statistically significant within the ensemble as a whole”. Dey et al (2019) add nothing new because they simply review published literature, excluding CSIRO and BoM (2015). [Kevin Hennessy, Australia]	Noted. We assessed low confidence because of inconsistencies and disagreements, which is even highlighted in the comment.
89155	65	29		34	One factor that could be mentioned here, at least for western Australia, is the southeast Indian Ocean region of decreasing extreme precipitation - this is one of the only places on land where this is a potentially relevant factor. This is shown in Pfahl et al (2017) [Angeline Pendergrass, United States of America]	Considered. Findings from Pfahl et al (2017) paper are discussed.
76689	65	29			You might add to the refs: Lionello, P. and Scarascia L. (2020) The relation of climate extremes with global warming in the Mediterranean region and its North versus South contrast Reg Environ Change 20, doi: 10.1007/s10113-020-01610-z.. The paper shows contrasting trends within the Mediterranean region. [Piero Lionello, Italy]	Accepted
23183	65	30	65	30	This makes no sense - you do not yet know what future changes shall be as implied here. [Peter Thorne, Ireland]	Accepted. Reworded
42531	65	32	65	32	Typo: models. (Perkins -> models (Perkins [Joan Bech, Spain]	Accepted
66345	65	32	65	34	There is a conflict in confidence statment with CH12 table 12.5 [Erika Coppola, Italy]	Noted. Table 12.5 does not have entry on heavy precipitation

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70965	65	36	65	44	We are submitting a manuscript (E Bevacqua, G Zappa and TG Shepherd: “Shorter cyclone clusters modulate changes in European wintertime precipitation extremes“) which shows across the CMIP5 models that the accumulated precipitation in wintertime cyclone clusters (a key driver for wintertime flooding) generally increases across Europe, although by less than the mean precipitation per cyclone, because of a decrease in the overall number of cyclones within clusters. This modulation of the accumulated precipitation varies substantially between northern and southern Europe. You may contact e.bevacqua@reading.ac.uk for the submitted version of this paper. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The differences in the changes of extreme precipitation across north and south Europe are discussed but the possible causes or related processes are not included due to space limitation.
76695	65	41			Lionello and Scarascia 2020 (doi: 10.1007/s10113-020-01610-z) used global model projections to show the contrast in future trends of precipitation extremes at short spatial scale in the Mediterranean (increasing in the north and decreasing in the south Med) [Piero Lionello, Italy]	Accepted. Added
66349	65	46	65	46	There is a conflict in confidence statment with CH12 table 12.8 [Erika Coppola, Italy]	Noted. Table 12.8 does not have entry on heavy precipitation
41065	65	46	65	52	There's a general lack of quantification in this section. Would be useful to provide numbers, in addition to 'increase' or 'decrease', where possible. Else the reader is left to guess how large the changes are. [TSU WGI, France]	Noted. The magnitude of changes (in relation to warming level) is given in the summary of this section. It would take a lot space to repeat the numbers for individual regions.
42533	65	47	65	47	Typo: 2018f)(Innocenti -> 2018f; Innocenti [Joan Bech, Spain]	Accepted
43375	65	47			Read "(Easterling et al., 2017; Wu, 2015; Zhang et al. 2018f; Innocenti et al., 2019) " rather than "(Easterling et al., 2017; Wu, 2015; Zhang et al. 2018f)(Innocenti et al., 2019) " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
66347	65	51	65	52	There is a conflict in confidence statment with CH12 table 12.6 [Erika Coppola, Italy]	Noted. Table 12.6 does not have entry on heavy precipitation
66391	65	54	66	4	These two submitted papers based on CP model ensembles that are now under revision could be added Ban et al, The first multi-model ensemble of regional climate simulations at kilometer-scale resolution Part I: Evaluation of precipitation, Climate Dynamic, submitted; Pichelli et al, The first multi-model ensemble of regional climate simulations at kilometer-scale resolution part 2: future precipitation projections, Climate Dynamic, submitted [Erika Coppola, Italy]	Noted but the authors don’t have access to the paper as of Dec. 11.2020.
100479	66	1	66	2	Hourly precipitation extremes have also been projected to increase in Northern Europe during summer, and decrease in southern Europe (Hodnebrog, Ø., Marelle, L., Alterskjær, K., Wood, R. R., Ludwig, R., Fischer, E. M., Richardson, T. B., Forster, P. M., Sillmann, J., and Myhre, G.: Intensification of summer precipitation with shorter time-scales in Europe, Environmental Research Letters, 14, 12, 124050, 10.1088/1748-9326/ab549c, 2019). [Øivind Hodnebrog, Norway]	Accepted
10947	66	1	66	2	Olsson and Foster (2014) found a projected increase of 30-min precipitation extremes over Sweden. Reference: Olsson, J., and K. Foster (2014) Short-term precipitation extremes in regional climate simulations for Sweden, Hydrol. Res., 45.3, 479-489, doi:10.2166/nh.2013.206. Also the references in cell I16 above include analyses of sub-hourly rainfall in RCM projections. [Jonas Olsson, Sweden]	Accepted
109845	66	1	66	4	Note that there are projected increases across Europe as well from studies by Kendon, Chan, Ban and Schaer. See for e.g.: Chan, S.C., Kendon, E.J., Berthou, S., Fosser, G., Lewis, E., Fowler, H.J. Europe-wide climate change projections at convection-permitting scale with the Unified Model. Climate Dynamics, in press.There is a good summary again in the Fowler et al. review paper: Fowler, H.J., Lenderink, G., Prein, P., Westra, S., Allan, R.P., Ban, N., Barbero, R., Berg, P., Blenkinsop, S., Do, H.X., Guerreiro, S., Haerter, J., Kendon, E., Lewis, E., Schaer, C., Sharma, A., Villarini, G., Wasko, C., Zhang, X. Intensification of short-duration rainfall extremes with global warming and implications for flood hazard. Submitted to Nature Reviews Earth and Environment, March 2020. [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reported
11703	66	2	66	2	delete “Andreas F”. It’s just “Prein et al. (2016).” [Amy East, United States of America]	Accepted
69541	66	10	66	14	same as comment 36 above. [Martin Singh, Australia]	Rejected. Unclear what this comment is referred to.
39485	66	10	66	16	The summary text could benefit from including that there low confidence in the changes in extreme precipitation in north Africa and the Sahara either due to a lack of agreement among studies on the sign of changes or due to insufficient evidence as well as there is low confidence regarding Australia. Consider to mention this after the end of the sentence in line 11. [Tamara van 't Wout, Qatar]	Accepted
109847	66	12	66	13	This statement seems very precise and I wonder where it comes from: "a slightly smaller rate in the 50-yr event of annual maximum 1-day and 5-day precipitation per 1°C warming" ?? [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Considered. The wording is reformulated.
71655	66	12	66	14	(same as the comment to the Executive Summary P7L35-37) How is the number "7%" and "50-yr" concluded from a number of literatures ? [Ryo Mizuta, Japan]	Noted. The precise values are now avoided
1485	66	14	66	14	Are we sure that it is 'high confidence' for these figures (7%/C and smaller for the 50-year return-value)? We are dealing with a large set of caveats here: (1) the GCMs provide an unaccurate representation of extreme precipitation, (2) 50-year events are fare out in the tails of the distribution, and modelling their probabilities (through GEV and GP) tend to be fraught with large uncertainties, (3) the simulation of clouds are not very accurate, and (4) do the GCMs not indicate that the global rainfall area diminishes (even this is a difficult question since different spatial resolutions implies different ability to represent the rainfall area). I was not convinced by the discussion above that we can use the label 'high confidence' on 7%/C, but I would agree that we can say that about the tendency of the rainfall becoming more intense and the extremes more frequent (also based on the analysis of observations). [Rasmus Benestad, Norway]	Noted. The precise values are now avoided
125927	66	14	66	16	[CONFIDENCE] How can this acceleration be very likely if it has not been clearly detected in observations? This seems to rely on models only for a very likely projection, which does not seem appropriate. [Trigg Talley, United States of America]	Considered. The summary statement is recalibrated.
43377	66	14			Read "extreme precipitation will very likely accelerate with " rather than "extreme precipitation will very likelyaccelerate with " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
102563	66	16	66	16	"..in the increase rate between regions." [Philippe Tulkens, Belgium]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
105299	66	19	66	19	The authors have tried hard to assess information about observed and projected changes in flooding, but having read this section, the primary impression is that they were given an impossibly large task. Many factors contribute to this situation, including, but certainly not limited to, the following. The hydrological literature is extremely heterogeneous, with most of expertise and attention focused at regional and sub-regional (basin) scales, where issues are very often basin specific and driven by the particular requirements of the users in those areas. The models that are used are structurally diverse, and generally need be extensively calibrated since much of the detail governing sub-grid scale processes and variations in land surface properties remains largely unknown, irrespective of the spatial resolutions. Very often, they deal only with water quantity, leaving questions concerning hydrology (water height and velocity) aside, and very often they consider only the naturalized flow in the basins they represent. The driving information for these models and the data used for calibration are also, generally, poorly resolved and uncertain relative to their resolutions, necessitating the use of a wide variety of downscaling techniques for bring that information (and all of its imperfections) to the scales required to drive the models (exceptions may be small, intensely instrumented research basins). This is a discipline that, from an outside perspective, remains highly empirical and does not yet appear to have developed principles and approaches that can be used to organize and synthesize results and thought at a large scale. This context makes the assessment of flooding and streamflow change exceedingly difficult, so much so, that one wonders whether the chapter should even try. I wonder if the evolution of this topic since the SREX might be sufficient to set the stage for a special report on water, with an emphasis on droughts and floods. Having an objective like this in mind might help to scope this section and that on drought, for example, by using these sections to carefully articulate questions for assessment and make a preliminary assessment of whether there might be sufficient literature to proceed. I think having such a focused IPCC report in mind would serve a very useful purpose, both to the users of flood and drought projections, but also to the discipline, by pointing to a path forward that could ultimately, provide a stronger assessment of what we know and don't know about changes in flooding and drought. [Francis Zwiers, Canada]	Accepted: we have used some of the suggested material by the reviewer to stress uncertainties in observations and model simulations. We basically agree with the reviewer's point, but it is necessary to include an assessment of floods in the AR6.
23193	66	19			This section feels weak overall and that it is not comprehensive in the literature being cited. There are numerous key papers missing. Additional CA input would be useful. Possible CAs may include my colleague Conor Murphy, Rob Wilby from Loughbourough or Julia Hall. Furthermore, there is scant coverage of coastal inundation / storm surge and yet there is a body of work out there on this which could / should be better integrated. [Peter Thorne, Ireland]	Accepted: the section has been substantially modified from SOD to FGD
1703	66	19			we have been witnessing flash floods for a couple of years in Jordan (East Mediterranean country), and it is getting more intense every year… I wonder if any model captured this. [Ruba Ajjour, Jordan]	Rejected: this question is not very specific and it does not provide recommendation
107659	66	21	6	21	Suggest adding pluvial floods [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: pluvial floods have been added
20741	66	21	66	29	Are climate scientists not interested in the areas concerned by floods? Such areas are becoming estimated with improved accuracy and reliability by spaceborne microwave sensors. See for example: Tay, C.W.J., Yun, S., Chin, S.T. et al. Rapid flood and damage mapping using synthetic aperture radar in response to Typhoon Hagibis, Japan. Sci Data 7, 100 (2020). https://doi.org/10.1038/s41597-020-0443-5 [philippe waldteufel, France]	Rejected: There is interest in the flooded area but there are not still sufficient studies covering large time-spans to assess flood changes.
82791	66	21	66	29	The extent to which some rivers are regulated (and the fact that this has changed over time) should also be brought into this introduction as a confounding factor. [Blair Trewin, Australia]	Rejected: this is explained in the section 11.5.1 of mechanisms and drivers.
125929	66	21	66	29	Perhaps a definition of "flood" could be given, presumably making reference to the inundation of normally dry land. [Trigg Talley, United States of America]	Accepted: definition has been included
84911	66	21	72	23	Discussion of coastal flooding seems very meager in this section given the robustness of the link between increased coastal flooding and mean sea-level rise. It is hardly mentioned in the body of the text and not at all in the summary paragraphs at the end of the sections. [Turner Jessica, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: reference and comments to coastal floods have been removed from Ch. 11
125931	66	21			The paragraph begins by noting that there are different flood types, and lists some examples. For most types, the reader will likely know what they mean or can deduce it from the discussion that follows. But, what exactly is meant by "flash floods", especially if they are distinguished from "river floods"? Consider clarifying this since flash floods can occur on rivers, especially very small rivers. [Trigg Talley, United States of America]	Accepted: brief description of the flood types has been added
32939	66	22	22	66	time and space scale [Tomasz Walczykiewicz, Poland]	Rejected: removed from SOD manuscript.
105301	66	23	66	23	I'm not a huge fan of the word "driver", which can mean different things to different readers, particularly in the context of the IPCC and UNFCCC, where the key "driver" of interest is the external forcing of the climate system. I would suggest that you discuss "factors" instead of "drivers". This is a more neutral term that would help avoid confusion about the ultimate causes of long-term changes in flooding behavior. [Francis Zwiers, Canada]	Accepted - replaced throughout the entire document.
107693	66	24	66	25	This sentence is a little vague. Perhaps we could be more specific, e.g. rainfall intensity matters for flash floods and pluvial urban floods. The expression "some types of floods" is a bit vague; perhaps you could specifiy (e.g. groundwater flooding etc.) [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: included in the revised document.
105303	66	25	66	26	Should mention other factors as well (e.g., storm surge). Note also that "coastal" is not restricted just to salt water environments; wind setup on large lakes (seiche) and low frequency variability in large lake water balances can also cause coastal flooding affecting many millions of people (e.g., in the Great Lakes basin). [Francis Zwiers, Canada]	Rejected: coastal floods are assessed in Ch. 12
32941	66	27	28	66	Statistical analyzes of future floods are based on existing, often short observations. [Tomasz Walczykiewicz, Poland]	Rejected: not directly related to the text in the section, which is not related to flood projections.
107661	66	27	66	27	"Floods … are difficult to measure…": I'm not sure I really agree with the statement that floods are difficult to quantify - there has been considerable progress in measurement methods, so this sentence is a bit strange to me. [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: we have detailed in the revised document that we refer to flooded area, which is difficult to quantify.
107681	66	27	66	29	The two sentences here "Flood indicators that can be measured, such as runoff or streamflow ... Thus, the assessment... considers changes in flow" are a bit strange to me. Runoff and streamflow are not really "indicators"; flood extent, duration, magnitude, etc. are "indicators" (in my mind). The meaning of these two sentences sentence is not very clear to me, and I don't understand what it is trying to say. What other quantity would be measured to evaluate changes in fluvial flooding, other than streamflow? [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: this sentence has been rewritten in the revised document.
105305	66	29	66	29	This statement makes the scope of this section somewhat unclear, since "flow" would seem to exclude some of the issues that have just been called out (e.g., concerning coastal flooding). With regard to riverine flooding, I think it is reasonable to consider primarily observed and simulated streamflow changes, but it should be made clear that this can only be considered as a rough proxy for changes in riverine flooding behavior. This section needs to reinforce throughout that it is primarily streamflow that is being discussed and assessed. As currently written, despite this one sentence, the section subsequently uses the term flooding rather loosely, which unfortunately, would implicitly invite readers to over interpret the assessments that are provided as pertaining directly to flooding rather than to streamflow. [Francis Zwiers, Canada]	Accepted: This issue has been clarified in the revised document.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
104487	66	32	66	32	Suggest to elaborate more on snowmelt floods, and in particular extensive snowmelt combined with heavy precipitation or long duration precipitation. In regions with a seasonal snow cover this is commonly the main cause of extreme river flooding over large areas. [Lena M Tallaksen, Norway]	Accepted: this issue has been included in the revised document.
100771	66	32	67	27	Suggest to elaborate more on snowmelt floods, and in particular extensive snowmelt combined with heavy precipitation or long duration precipitation. In regions with a seasonal snow cover this is commonly the main cause of extreme river flooding over large areas. [Lena M Tallaksen, Norway]	Accepted: this issue has been included in the revised document.
100773	66	32	67	27	May add a note on groundwater flooding, i.e. groundwater table above the ground, e.g. occured in the UK in winter 2014, as a result of long lasting precipitation higher than normal (>month). [Lena M Tallaksen, Norway]	Accepted: included in the revised document.
62349	66	32			Section 11.5.1: This section could be subdivided according to the main factors that would influence floods, for example: 11.5.1.1_Precipitation; 11.5.1.2_Antecedent soil moisture; 11.5.1.3_Snow pack and snow-melting in cold regions; 11.5.1.4_Surge and tides in coastal regions and 11.5.1.5_Human water management. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected: The connection between factors is difficult to be separated.
103835	66	33	66	33	May add a note on groundwater flooding, i.e. groundwater table above the ground, e.g. occured in the UK in winter 2014, as a result of long lasting precipitation higher than normal (>month). [Lena M Tallaksen, Norway]	Accepted: included in the revised document.
101605	66	33			May add a note on groundwater flooding, i.e. groundwater table above the ground, e.g. occured in the UK in winter 2014, as a result of long lasting precipitation higher than normal (>month). [Lena M Tallaksen, Norway]	Accepted: included in the revised document.
104399	66	34	67	35	Discussion here omits the evidence that development has dramatically increase resilience to flooding and this increased resilience counters and masks the risk of flooding driven by increase in seasonal/annual precipitation and increases in extreme precipitation. See: USGS publication Flow Modification in the Nations's Streams and Rivers 2019 (https://doi.org/10.3133/cir1461) which reports that due to the "modification of natural flows in streams and rivers... high flows have been reduced in magnitude, are of shorter duration, are less frequent, and vary less from one year to the next than they would naturally." See also Formeta and Feyen, 2019: Empirical evidence of declining global vulnerability to climate-related hazards (https://doi.org/10.1016/j.gloenvcha.2019.05.004) which reports increasing resilience to extreme weather events with increasing development. [Hunter Cutting, United States of America]	Rejected: given space limitations this is not included in the final document.
62513	66	35	66	37	It seems this sentence should have citations highlighting the specific studies [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account: the sentence has been rewritten.
107695	66	36	66	36	Perhaps this sentence would be clearer if you wrote "the role of temperature" instead of just "temperature"? Either way, the sentence could be clarified a bit. [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: rewritten in the revised document.
107663	66	38	66	38	For "antecedent moisture", you could cite "Slater, Villarini (2016) Recent trends in U.S. flood risk, Geophys. Res.Lett. 43(12) 428-436, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016GL071199" [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: reference included
125933	66	38			The following references are relevant: Neri, A., G. Villarini, L.J. Slater, and F. Napolitano, On the statistical attribution of the frequency of flood events across the U.S. Midwest, Advances in Water Resources, 127, 225-236, 2019. Slater, L.J., and G. Villarini, Recent trends in US flood risk, Geophysical Research Letters, 43(24), 12428-12436, 2016. [Trigg Talley, United States of America]	Accepted partially: second reference included
62353	66	42	66	53	The level of confidence could be included in the analysis of factors that would increase flooding. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected: general assessment is provided in the last paragraph.
39311	66	42	67	27	Can you consider synthesizing the information/findings here, not just a literature review. [Lourdes Tibig, Philippines]	Rejected: here we show the main mechanisms of flood. This is a synthesis
2173	66	43	66	43	The reference Fitsum and Ashish 2016 should be: Woldemeskel, F., Sharma, A., 2016. Should flood regimes change in a warming climate? The role of antecedent moisture conditions. Geophys. Res. Lett. 43, 7556–7563. https://doi.org/10.1002/2016GL069448 [Conrad Wasko, Australia]	Accepted: corrected in the revised document.
125935	66	43	66	43	Snow-water equivalent and snow density are more directly relevant to river floods than is snow cover. [Trigg Talley, United States of America]	Accepted: replaced in the revised document
13839	66	44	66	45	It's suggested to explicitly address the causes of flooding related to non-climatic processes (hydraulic infrastructure, population, solid waste, etc.), as human settlements grow, these causes represent a potentially significant risk to the population. [Maria Amparo Martinez Arroyo, Mexico]	Rejected: this refers to impacts and vulnerabilities, which is assessed in WGII
107667	66	44	66	45	The expression "physiographic characteristics" is a little confusing. Instead, I would suggest separating the two drivers here: (i) river and catchment engineering (e.g. hydraulic structures) and (ii) changes in the conveyance capacity of river channels. There is a lot of literature on the effect of catchment engineering (e.g. effects of channel straightening, meander cutoffs, etc.). If you include hydraulic structures do you also want to mention the effect of dams on flood magnitudes (as well as on river channel conveyance upstream and downstream)? For stream morphology and channel conveyance capacity, you could cite "Slater, Singer, Kirchner (2015) Hydrologic versus geomorphic drivers of trends in flood hazard, Geophysical Research Letters 42(2), 370-376. https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2014GL062482". This last paper quantifies the influence of changes in river channel conveyance on the frequency of overbank flooding. [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: reorganised in the revised document.
107665	66	46	6	46	For "land use and land cover characteristics", there are many more papers emerging on this topic. One possible reference is "Slater et al. (2019) A dynamical-statistical framework for seasonal streamflow forecasting in an agricultural watershed, Climate Dynamics, 53(12), 7429-7446. https://link.springer.com/article/10.1007/s00382-017-3794-7", or alternatively, reference to "Slater, Villarini (2017) Evaluating the drivers of seasonal streamflow rates in the U.S. Midwest, Water MDPI. 9, 695. https://www.mdpi.com/2073-4441/9/9/695". In both these papers, we used agricultural land cover as a covariate within the models. [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: there are already two references to refer to this issue.
125937	66	46			The following reference may be relevant: Zhang, W., G. Villarini, G.A. Vecchi, and J.A. Smith, Urbanization exacerbated the rainfall and flooding caused by hurricane Harvey in Houston, Nature, 563, 384-388, 2018. [Trigg Talley, United States of America]	Rejected: there are already two references to refer to this issue.
2175	66	51	66	51	I feel that a reference to the complicating factor of the "rarity" of the event considered should be added here (please note that this would be consistent with the text in Chapter 8, page 24, line 50). The following papers show that if you consider events such an annual maxima you are more likely to see decreases in flood magnitude due to factors other than extreme precipitation changes (such an antecedent conditions). But once events are very extreme, say events seen once every hundred years, then change in precipitation dominate. This is a good location to add one sentence on this, because nevertheless it is impressive to see correspondence between flooding and precipitation as stated in the following sentence. The papers referring to this are: Wasko, C., Nathan, R., 2019. Influence of changes in rainfall and soil moisture on trends in flooding. J. Hydrol. 575, 432–441. https://doi.org/10.1016/j.jhydrol.2019.05.054; Wasko, C., Sharma, A., 2017. Global assessment of flood and storm extremes with increased temperatures. Sci. Rep. 7, 7945. https://doi.org/10.1038/s41598-017-08481-1; Ivancic, T.J., Shaw, S.B., 2015. Examining why trends in very heavy precipitation should not be mistaken for trends in very high river discharge. Clim. Change 133, 681–693. https://doi.org/10.1007/s10584-015-1476-1 [Conrad Wasko, Australia]	Accepted: a sentence and references included in the revised document.

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105307	66	51	66	52	I think this is a good illustration of the use of appropriately cautious language, in contrast to statements in the Executive Summary that seem less well nuanced. [Francis Zwiers, Canada]	Noted with thanks for the complement.
107697	66	53	66	53	This paper also clearly shows the correspondence between long-term changes in flooding and precipitation across the USA: "Slater, Villarini (2016) Recent trends in U.S. flood risk, Geophys. Res.Lett. 43(12) 428-436, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016GL071199" [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: the reference has been included.
62463	66	53	66	53	Line 53 page 66 “Peterson et al. 2013” Referece not found in the list author probably forgot to put “a” or “b” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: correct link to the reference has been included.
105309	67	1	67	1	I think this has a stronger basis than is indicated by the word “speculated”, and indeed, is a topic that has been studied for at least 15 years now. See, for example, the detection and attribution study of Gedney et al, 2006 (Nature, doi:10.1038/nature04504) claiming to have detected this effect in historical continental runoff data, and the subsequent discussion in Nature (doi:10.1038/nature05480 and doi:10.1038/nature05481) about whether the runoff data they used was up to the task. [Francis Zwiers, Canada]	Rejected: these studies are related to mean streamflow but not on flood, which is the focus of this section. Effects of CO2 on water resources are discussed in chapter 8. The term speculated has been replaced.
66421	67	1	67	5	The wording of this, with phrasing like "it has been speculated" reflects a strong viewpoint that I don't think is warranted. The strong physioloogical basis, documented in robust numerical models, goes beyond speculation. Maybe more neutral to rewqord as saying that evidence exists but currently only from models? [Charles Koven, United States of America]	Accepted: the word has been replaced.
96131	67	1	67	5	One could imagine, that this mechanism is a valid "speculation" for some regions but not for whole the globe. Moreover, it is presumably much less important than other factors mentioned here (with less lines of text). Please revise. [Nicole Wilke, Germany]	Accepted: the word has been replaced.
125939	67	1	67	9	Briefly raise the potential effects of changes in plant phenology and forest species composition on flood magnitude and/or frequency (e.g., Peuelas and Filella, 2001; Knighton et al., 2019). Citations: Knighton, J., Conneely, J., and Walter, M.T. (2019). Possible increases in flood frequency due to the loss of Eastern Hemlock in the northeastern United States: Observational insights and predicted impacts. Water Resour. Res. 55(7), 5342-5359. doi:10.1029/2018WR024395 Peuelas, J., and Filella, I. (2001). Responses to a warming world. Science, 294(5543), 793-795. https://doi.org/10.1126/science.1066860 [Trigg Talley, United States of America]	Accepted: the reference has been including when referring to land cover changes.
5689	67	7	67	7	The study from Evaristo & McDonnell was retracted and should not be cited. [Joachim Rock, Germany]	Accepted: the reference has been replaced.
66423	67	8	67	9	The phrasing "there is low confidence in the overall effects on future floods caused" seems quite weak, as it does not even specify what the sign of the expected response is that the assessment has low confidence in. Since the models that do exist point to increased flood risk, I suggest reword to something like: "there is low confidence that the physiological response of plants to increased CO2 concentrations will lead to increased future flood risk." [Charles Koven, United States of America]	Accepted: text modified to gain clarity.
69245	67	11	67	18	The urban flashflood should be separated from flash flood in this section. It is suggested to be reworded as urban inland flood. The urban inland flood occurs in cases of heavy precipitation due to the over capacity of the drainage system. This is different from other flash floods and important for both developed and developing countries. [Kaoru Magosaki, Japan]	Accepted: modified in the revised document.
62355	67	11	67	18	The level of confidence is not included. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected: this paragraphs does not include an assessment so confidence level is not needed.
32943	67	12	13	67	including errors in dam resorvoir construction, service and management (Oroville Dam) [Tomasz Walczykiewicz, Poland]	Rejected: dam breaks is including, independently of the reason of the break.
104401	67	13	67	17	The reference provided to argue that urbanization is a significant factor is driving observed trends in flooding is contradicted by the United States Geological Service (USGS) publication The 100-year Flood (URL reference below) that reports: "The effects of development (conversion of land from forested or agricultural uses to commercial, residential, or industrial uses) on peak flows is generally much greater for low-recurrence interval floods than for high-recurrence interval floods, such as 25- 50- or 100-year floods. During these larger floods, the soil is saturated and does not have the capacity to absorb additional rainfall. Under these conditions, essentially all of the rain that falls, whether on paved surfaces or on saturated soil, runs off and becomes streamflow." Reference: https://www.usgs.gov/special-topic/water-science-school/science/100-year-flood?qt-science_center_objects=0#qt-science_center_objects. [Hunter Cutting, United States of America]	Rejected: this statement is not contradictory with the text included. We agree that urbanization increases the risk of pluvial flooding and this is stated in the document.
31637	67	20	67	27	Here, there could be a distinction between permanent flooding, chronic flooding, and flooding during storms, projected sea-level rise being a major driver of change for the three of them. [Gonéri Le Cozannet, France]	Rejected: coastal floods have been removed from Ch. 11
62357	67	20	67	27	The level of confidence is not included. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected: coastal floods have been removed from Ch. 11
14631	67	22	67	22	Ch 12 also assessses coastal flooding extensively [Roshanka Ranasinghe, Netherlands]	Accepted: coastal floods have been removed from Ch. 11
105311	67	23	67	23	Not just tropical cyclones – this is also a serious concern when mid-latitude extra-tropical cyclones affect coastal regions. [Francis Zwiers, Canada]	Rejected: coastal floods have been removed from Ch. 11
105313	67	24	67	24	There are other factors as well as heavy precipitation, including heavy discharge, which could be produced for reasons other than heavy precipitation in the immediate area affected by coastal flooding, and coincidence with high tides (particularly, so-called “king tides”). [Francis Zwiers, Canada]	Rejected: coastal floods have been removed from Ch. 11
66425	67	29	67	35	It seem slike there should be some mention of a possible contribution of increased flood risk from CO2 fertilization, as per the discussion above, in the summary here? [Charles Koven, United States of America]	Accepted: assessment included in the revised document.
62359	67	29	67	35	The level of confidence is not included in Summary. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected: we do not think necessary since it includes facts widely known and accepted.
28957	67	31			Could link to and coordinate with 8.2.3.2 (which currently summarises "there is low confidence in changes in the frequency of flooding regionally which is strongly dependent upon complex catchment characteristics, antecedent conditions and how atmospheric circulation systems respond to climate change, which is less certain than thermodynamic drivers (see Section 11.5)." but can be modified) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: linked with 8.2.3.2.
74549	67	46	67	46	to correct SR15 by SR1.5 [Moulay Driss HASNAOUI, Morocco]	rejected: SR15 is how this is referred in the report.
31639	67	47	67	47	In fact, many studies on coastal flood exist in the area of coatal engineering, but many of them have not been published. I can suggest the following examples on city-scale coastal flooding addressing observed trends: Idier, D., Rohmer, J., Pedreros, R., Le Roy, S., Lambert, J., Louisor, J., Le Cozannet, G. and Le Cornec, E., 2020. Coastal flood: a composite method for past events characterisation providing insights in past, present and future hazards—joining historical, statistical and modelling approaches. Natural Hazards, 101(2), pp.465-501; Sweet, W. V., & Park, J. (2014). From the extreme to the mean: Acceleration and tipping points of coastal inundation from sea level rise. Earth's Future, 2(12), 579-600 There are other examples that can be extracted from section 2 and 3,3 of Le Cozannet, G., Nicholls, R. J., Hinkel, J., Sweet, W. V., McInnes, K. L., Van de Wal, R. S., ... & White, K. D. (2017). Sea level change and coastal climate services: The way forward. Journal of Marine Science and Engineering, 5(4), 49., in particular on chronic flooding and seasonnal extreme water levels, plus references to existing coastal engineering studies. Work by Barnard et al. on COSMO in the Bay of San Francisco could also be relevant as example here. [Gonéri Le Cozannet, France]	Rejected: coastal floods have been removed from Ch. 11

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107679	67	47	67	47	Here, in addition to the problem of spatial sparsity, I would add that many stream gauge records also have gaps in observed data, which leads to difficulties detecting trends in flooding. If you want a reference you could cite: "Slater, Villarini (2016) On the impact of gaps on trend detection in extreme streamflow time series, International Journal of Climatology. 37(10), 3976-3983". [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: included in the revised document.
105315	67	53	67	53	It's not obvious to me that there is enough here to warrant subdivision into three subsections, each only a single paragraph. [Francis Zwiers, Canada]	Accepted: this section has been completely reorganised and shortened.
39313	67	53	68	26	Same comment as above [Lourdes Tibig, Philippines]	Rejected: we include here an assessment based on the available literature.
105317	68	1	68	6	This is an example of a discussion where assessment seems to be completely lacking. Does the chapter have a view on the data that were used and whether they are of a suitable quality to allow trend calculation? If, for example, regulation was introduced upstream of many of the stations during the period of record, there would very likely be an impact on annual maximum peak flow that is unrelated, or at least would be confounded with, any impact of climate change, even if the implementation of regulation had little impact on annual mean streamflow. If the data are suitable for the purpose that they have been applied to, then are the authors comfortable with reporting results to within the nearest 1/10th of a percent? Surely, they shouldn't be! Also, how do they interpret a finding that there is "significant" decrease at a larger proportion of stations than there is increase? Is this an indication of regionally varying responses to climate change, or is this perhaps only an artefact of sampling (there might simply be more stations per unit area in drying regions than elsewhere …)? Are the authors comfortable that the statistical analysis has been well done, and that there are not inflated rates of detection of trends because methods have not considered factors such as serial dependence (autocorrelation) of the data? In summary, what is required is an assessment, not simply a review indicating what is in the literature. [Francis Zwiers, Canada]	Accepted: uncertainties on the data sources and availability of studies are assessed: Streamflow measurements are not evenly distributed over space, and coverage in many regions of Africa, South America, and parts of Asia is poor (e.g. Do et al., 2017). Moreover, many stream gauge records have gaps in observed data, which leads to difficulties detecting trends in flooding (Slater and Villarini, 2017). Also possible uncertainties related to water management are assessed.
105319	68	1	68	26	See my comment concerning page 68, lines 1-6. This whole paragraph, in fact, fails to provide an assessment. [Francis Zwiers, Canada]	Accepted: This issue is stressed in the revised document.
125941	68	1	68	54	These two sections summarize studies that examine trends in the magnitude of historical annual peak flows and the frequency of annual peaks or peaks-over-threshold. This is good and necessary, however, there is a study that examined historical trends in only the largest floods (such as the 100-year flood), floods with the most societal impacts (Hodgkins et al., 2017). This study should be summarized in one of these two sections. It shows that there are not more temporal trends than expected due to chance in the number of major floods, and that the major floods for some regions in both North America and Europe are related to the Atlantic Multidecadal Oscillation. Citation: Hodgkins, G.A., Whitfield, P.H., Burn, D.H., Hannaford, J., Renard, B., Stahl, K., Fleig, A.K., Madsen, H., Mediero, L., Korhonen, J. and Murphy, C., 2017, Climate-driven variability in the occurrence of major floods across North America and Europe: Journal of Hydrology, v. 552, p. 704-717, http://dx.doi.org/10.1016/j.jhydrol.2017.07.027. [Trigg Talley, United States of America]	Accepted: Reference to this relevant study has been included.
62515	68	2	68	2	please replace north Europe with northern Europe [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
33253	68	5	68	6	I suggest: "This suggests there is a regional variability in peakflow trends. [Gonzalez Sergi, Spain]	Accepted: replaced in the revised document
62517	68	8	68	8	please replace north Asia with northern Asia [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
2171	68	10	68	10	The following manuscript attributed the negative annual maxima flood trends in the south of Australia to decreases in antecedent soil moisture. It also observed increases in flooding for more extreme events than annual maxima: Wasko, C., Nathan, R., 2019. Influence of changes in rainfall and soil moisture on trends in flooding. J. Hydrol. 575, 432–441. https://doi.org/10.1016/j.jhydrol.2019.05.054 [Conrad Wasko, Australia]	Accepted. Comment and reference included in the revised document.
38367	68	11	68	13	The paragraph addresses the observed runoff change instead of the cause and attribution, and gives only factual expressions for other regions. To maintain consistency, it is suggested to delete 'that is linked to a decrease in precipitation intensity and an increase in the number of dams'. In addition, considering that the research of Bai et al (2016) covers no more than 'Yellow River basin', the term 'central China', which is too wide in indication, is suggested to be reworded as 'northern central China'. [Yaming LIU, China]	Rejected: this section has been completely reorganized and rewritten.
62427	68	13	68	13	Zhang et al. (2015) missing in the reference perhaps a missing letter here line 13 page 68 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: linked in the revised document.
3207	68	14	68	15	I suggest to modify this sentence as follow (including two references "In the Amazon basin, there is a significant increase in extreme flow associated with the strengthening of the Walker and Hadley circulations (Barichivich et al., 2018; Wang et al., 2018. https://doi.org/10.1088/1748-9326/aadbb9; Espinoza et al 2019. doi: 10.1007/s00382-018-4462-2)" [Jhan Carlo Espinoza, France]	Rejected: these papers are not on floods but on extreme precipitation.
62519	68	16	68	16	please replace west Africa with western Africa [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: replaced in the revised document
11705	68	16	68	16	fix parentheses [Amy East, United States of America]	Accepted: replaced in the revised document
125943	68	16	68	18	"Peterson et al. (2013a) documented strong spatial differences in the trends over North America, with an increase in the northwest US and a decrease in the southeast US." That description does not match Figure 3a of that publication or the corresponding text. The sentence should say: "... with an increase in the northeast US and a decrease in the southwest US." That correction will also align the sentence with the next one, as intended. [Trigg Talley, United States of America]	Accepted: corrected in the revised document.
107669	68	16	68	19	Here I think you could start with a more recent paper than Peterson 2013, because it is strange to say "their finding is consistent with" if their paper is the oldest. [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: this section has been completely reorganized and rewritten.
107683	68	16	68	19	"increase in the northwest and decrease in the southeast": I don't think this is correct - many studies are showing increases in the northeast and decrease in the west/southeast. For clearer spatial patterns since 1965, see the maps in "Villarini, Slater (2017) Climatology of flooding in the United States, Oxford Research Encyclopedia of Natural Hazard Science, Oxford University Press. [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: corrected in the revised document.
125945	68	16	68	20	There is a newer national U.S. flood-trends study than Peterson et al. (2013a; data ending in 2008); it is Hodgkins et al. (2019; data ending in 2015). This study should be referenced. It also shows spatial differences in North American trends but shows few catchments with significant change at minimally altered catchments in the last 50 years. Citation: Hodgkins, G.A., Dudley, R.W., Archfield, S.A., and Renard, B., 2019, Effects of climate, regulation, and urbanization on historical flood trends in the United States: Journal of Hydrology, v. 573, p. 697-709, https://doi.org/10.1016/j.jhydrol.2019.03.102. [Trigg Talley, United States of America]	Accepted. Assessment of this relevant study has been included.
43379	68	16			Read " (Nka et al., 2015)" rather than " (Nka et al. (2015))" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted: replaced in the revised document
3891	68	17	68	18	I think the text here has a typo and the east and west should be swapped. The text at this line: "an increase in the northwest US and a decrease in the southeast US". While Peterson et al has the following: "Flood magnitudes have been decreasing in the Southwest. Long-term data show an increase in flooding in the northern half of the eastern prairies and parts of the Midwest..." in Peterson, T.C., Heim, R.R., Hirsch, R., Kaiser, D.P., Brooks, H., Diffenbaugh, N.S., Dole, R.M., Giovannettone, J.P., Guirguis, K., Karl, T.R., Katz, R.W., Kunkel, K., Lettenmaier, D., McCabe, G.J., Paciorek, C.J., Ryberg, K.R., Schubert, S., Silva, V.B.S., Stewart, B.C., Vecchia, A. V., Villarini, G., Vose, R.S., Walsh, J., Wehner, M., Wolock, D., Wolter, K., Woodhouse, C.A., Wuebbles, D., 2013. Monitoring and understanding changes in heat waves, cold waves, floods, and droughts in the United States: State of knowledge. Bull. Am. Meteorol. Soc. 94, 821–834. https://doi.org/10.1175/BAMS-D-12-00066.1 [Conrad Wasko, Australia]	Accepted: corrected in the revised document.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
125947	68	19			These references may be relevant: Neri, A., G. Villarini, L.J. Slater, and F. Napolitano, On the statistical attribution of the frequency of flood events across the U.S. Midwest, Advances in Water Resources, 127, 225-236, 2019. Slater, L.J., and G. Villarini, Recent trends in US flood risk, Geophysical Research Letters, 43(24), 12428-12436, 2016. [Trigg Talley, United States of America]	Accepted: the reference has been included.
107671	68	20	68	20	It is unclear to me what is meant by "long-term". [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: rewritten in the revised document.
107703	68	20	68	20	Perhaps it is worth mentioning that Berghuijs et al, in "The Relative Importance of Different Flood-Generating Mechanisms Across Europe" (2019), questioned whether the relative importance of different peak flood-generating mechanisms is changing over time across Europe. They found the relative importance of different mechanisms has not changed substantially during five decades. [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: included in the revised document.
125949	68	20			Should the reference here be to Wehner et al. (2017) instead of 2018? [Trigg Talley, United States of America]	Accepted: corrected in the revised document.
62521	68	23	68	23	please replace north Europe with northern Europe [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: corrected in the revised document.
62523	68	26	68	26	please change south to southern [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: corrected in the revised document.
107673	68	39	68	39	For trends in frequency, if you wish to include a paper that is about the whole USA you could cite "Slater, Villarini (2016) Recent trends in U.S. flood risk, Geophys. Res.Lett. 43(12) 428-436, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016GL071199", which looks at changes in flood frequency using the peak-over-threshold approach. [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: Citation to Archfield et al. (2016) has been included.
125953	68	39	68	42	These results were further confirmed and their interpretation significantly expanded in: Neri, A., G. Villarini, L.J. Slater, and F. Napolitano, On the statistical attribution of the frequency of flood events across the U.S. Midwest, Advances in Water Resources, 127, 225-236, 2019. [Trigg Talley, United States of America]	Accepted: assessment of this study has been included.
125955	68	39	68	45	This section should include a summary of the study by Archfield et al. (2016). It is a national US study, as opposed to the regional study by Mallakpour and Villarini (2015). It shows that the frequency of peaks-over-threshold are increasing in some regions of the US and decreasing in others. There are a majority of regions with little significant change. [Trigg Talley, United States of America]	Accepted: this reference has been included and assessed.
125951	68	39	68	45	After discussing the results of Mallakpour and Villarini (2015) at these lines, and before shifting to discuss the European research, consider adding this sentence: ""Armstrong et al. (2012, 2014) also found increases in the annual frequency of high flows in the Northeast US. While they documented upward trends in annual maximum flow magnitudes as well, they too found stronger trends for frequency than for magnitude. Using different measures, Frei et al. (2015) also found stronger evidence for increased frequency of Northeast U.S. high stream flow than for increased magnitude."" Citations: Armstrong et al. (2014) is already in the chapter reference list Armstrong, W.H., Collins, M.J., and Snyder, N.P. (2012). Increased frequency of low-magnitude floods in New England. J Am Water Resour Assoc 48(2), 306-320. doi:10.1111/j.1752-1688.2011.00613.x Frei, A., Kunkel, K.E., and Matonse, A. (2015). The seasonal nature of extreme hydrological events in the northeastern United States. J. Hydrometeorol. 16(5), 2065-2085. doi:10.1175/JHM-D-14-0237.1 [Trigg Talley, United States of America]	Accepted: included in the revised document.
85037	68	40	68	42	The sentence refers to "apparent inconsistency" in the previous statement but the previous statement isn't inconsistent. Is there an error? [Turner Jessica, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: the sentence has been rewritten.
20743	68	40	68	43	Where is the pointed out inconsistency? [philippe waldteufel, France]	Accepted: the sentence has been rewritten.
8045	68	41	68	41	what does "while there is evidence of signficance changes in the annual peak flow" mean? [jouni Räisänen, Finland]	Accepted: the sentence has been rewritten.
11707	68	41	68	41	sounds like “significance” should be “significant” [Amy East, United States of America]	Accepted: replaced in the revised document.
125957	68	41			Replace "while there is evidence of significance changes in the annual peak flow" with "while there is little evidence of significant changes in the annual peak flow magnitude". Making that correction will bring the statement in line with the cited paper's findings and it will make better sense with the following sentence. [Trigg Talley, United States of America]	Accepted: the sentence has been rewritten.
105321	68	42	68	42	The text doesn’t really make clear what the discrepancy is. The authors seem to buy the notion that there is a discrepancy, but as in other parts of this section on observed trends, they don’t seem to assess the supporting evidence. For example, a change in the frequency of high flows could indicate a change in the shape of the annual hydrography, such as that which might come about as a result of regulation or land use change. So the same question emerges – are the data that were used up to the task of making inferences about changes that are likely to be due to climate change and not some other factor? That is, critical assessment is required, not just reporting, and not just speculation about difference in trend methods without demonstrating, at least, that this is plausible. [Francis Zwiers, Canada]	Accepted: the sentence has been rewritten.
125959	68	42			There is not an inconsistency if you have no trend in magnitude with an increase in frequency. If there were a downward trend in magnitude with an increase in frequency that may be more odd, but that is not the case. Consider re-characterizing this phenomenon or just strike the sentence. [Trigg Talley, United States of America]	Accepted: the sentence has been rewritten.
107699	68	43	68	43	This comment "The apparent inconsistency… should be interpreted in proper context" is correct, but not sufficiently clear. I think the author is trying to say that the methods for trend detection for flood frequency are more likely to detect significant changes in flooding than the methods used for detecting changes in flood magnitudes. This is true, but it is not very easy to understand.. Figure 1 in "Villarini, Slater (2017) Climatology of flooding in the United States, Oxford Research Encyclopedia of Natural Hazard Science, Oxford University Press." shows that we systematically detect more significant trends in the frequency (number of flood days) than in the magnitude (AMAX; annual peak maxima). [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: the sentence has been rewritten.
62525	68	44	68	44	consider changing “trend detection for the magnitude and the frequency are different” to “trend detection of magnitude and frequency differ” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected: Sentence rewritten in the revised document
62527	68	47	68	47	please change south Europe to southern Europe [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: Replaced in the revised document.
62529	68	53	68	53	omit “only” before “limited” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: section rewritten in the final document
107701	68	53	68	53	"peak flow frequency": perhaps this would be clearer as just "flood frequency", because some people think that "peak flow" refers to annual maxima. There are some studies of flood frequency such as "Slater, Villarini (2016) Recent trends in U.S. flood risk, Geophys. Res.Lett. 43(12) 428-436, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016GL071199" [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: "peak flood" frequency is maintained.
117111	68		68		first para, coordination is needed wth ch 8 [Valerie Masson-Delmotte, France]	Accepted: There are not overlaps and inconsistencies with Ch. 8 since they assess changes in mean streamflow instead on floods as done here.
109849	69	0	69	0	11.5.3 Model evaluation - this section is so short to be almost reducndant. Needs to be expanded or left to WG2 report. There are loads of other papers available in this area. [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: the section has been expanded with new assessment and new literature

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
105323	69	1	69	1	There is a very strong statement based on this short subsection in the executive summary, but from the description given here, it is evidence that the amount of evidence (amount of literature, diversity and robustness of approaches and results, geographic extent of results, etc) is rather limited. Two papers are mentioned studying Canadian and European data – surely that isn't enough to support "high confidence", particularly given that different mechanisms seem to be implicated in different regions. [Francis Zwiers, Canada]	Accepted: General assessment has been rewritten
125961	69	1			In this section, it would be good to include Villarini (2016) who used circular statistics for seasonality. He did not find strong trends in changing seasonality. Citation: Villarini, G., On the seasonality of flooding across the continental United States, Advances in Water Resources, 87, 80-91, 2016. [Trigg Talley, United States of America]	Accepted: reference assessed.
125963	69	3	69	14	There is a national study from the US on changes in the timing of snowmelt related high flows (Dudley et al., 2017) that should be summarized here. There's currently nothing on US trends, despite several major papers over the last two decades. Dudley et al. (2017) is the most recent and the first to include all regions with substantial snowmelt in the conterminous US. Previous studies mostly analyzed only western US basins. Dudley et al. (2017) uses a metric, the timing of the winter-spring center of volume that is more robust than the timing of peak flows. High flows, including peak flows have a strong influence on the metric. It also uses a seasonal window so that peaks outside the snowmelt season are not included; most studies have used annual peak flows which can conflate flood generation processes. It breaks up the US into three regions, the Northeast, low-elevation areas of the West, and high-elevation areas of the West. Citation: Dudley, R.W., Hodgkins, G.A., McHale, M.R., Kolian, M.J., and Renard, B., 2017, Trends in snowmelt-related streamflow timing in the conterminous United States: Journal of Hydrology, v. 547, p. 208-221, http://dx.doi.org/10.1016/j.jhydrol.2017.01.051. [Trigg Talley, United States of America]	Accepted: the assessment of this reference has been included.
2177	69	3	69	14	The following manuscript shows that changes in antecedent moisture conditions have caused changes in flood timing across the Australian continent as a result of climate change: Wasko, C., Nathan, R., Peel, M.C., 2020. Changes in Antecedent Soil Moisture Modulate Flood Seasonality in a Changing Climate. Water Resour. Res. 56, e2019WR026300. https://doi.org/10.1029/2019WR026300 [Conrad Wasko, Australia]	Accepted: assessment of this paper included.
125965	69	8			Kam et al. (2018) find only marginal evidence for an emerging detectable anthropogenic influence on winter-spring streamflow timing over North America (according to four or five of nine models), and this was found only for the north-central United States and not other regions of North America, which had even weaker indications of detectable anthropogenic influence. Citation: Kam, J., T.R. Knutson, and P.C. Milly, 2018: Climate Model Assessment of Changes in Winter-Spring Streamflow Timing over North America. J. Climate, 31, 5581-5593, https://doi.org/10.1175/JCLI-D-17-0813.1 [Trigg Talley, United States of America]	rejected: this study is not related to floods but to mean streamflow.
125967	69	14			Consider adding this information at the end of the section: ""Collins (2019) and Frei et al. (2015) showed how warm season (Jun-Oct) flood counts in the Northeast US have increased, in association with the seasonality of regional upward trends in extreme precipitation (Huang et al., 2017a)."" Citations: Collins, M.J. (2019). River flood seasonality in the Northeast United States: Characterization and trends. Hydrol. Process. 33(5), 687-698. doi:10.1002/hyp.13355 Frei, A., Kunkel, K.E., and Matonse, A. (2015). The seasonal nature of extreme hydrological events in the northeastern United States. J. Hydrometeorol. 16(5), 2065-2085. doi:10.1175/JHM-D-14-0237.1 Huang et al. (2017a) already in chapter reference list. [Trigg Talley, United States of America]	Accepted: assessment of this study included in the revised document.
20255	69	16	69	16	"that" seems missing [philippe waldteufel, France]	Accepted: rewritten in the revised document.
84909	69	16	69	22	The summary deserves a mention of the areas where we also have low confidence. The tone appears to me that we're more confident in general than the body of the the text being summarised communicates. [Turner Jessica, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Assessment has been rewritten
96133	69	17	69	21	What does "flood proxy" mean? [Nicole Wilke, Germany]	rejected: we are not assessing floods, but flood peaks, although we think flood peak is a good approximation of the flood affected area.
28959	69	17			may need to coordinate with 8.3.1.5 which states "medium confidence that global warming has already led to a general increase in the occurrence of high streamflow values and flooding at the global scale, despite growing evidence at the regional scale." and Box 8.2 which states "high confidence that the seasonality of precipitation, runoff, streamflow and water availability will increase with global warming in many regions." [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: assessment harmonized with Ch 8.
62429	69	20	68	20	Wehner et al., 2018 missing in the reference perhaps a missing letter here line 20 page 68 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Corrected in the revised document.
125969	69	21			Should say "southwest US." [Trigg Talley, United States of America]	Accepted: corrected in the revised document.
107705	69	27	69	27	Here you describe hydrological and hydrodynamic models. Perhaps it is also worth saying something about the increasing popularity of distributional regression models for evaluating nonstationarity in flooding, such as the "Generalised Additive Models for Location, Scale and Shape" (GAMLSS) framework? Using these probabilistic models with observed data can provide important insights... [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: we only assess here physically-based models and not statistical models.
102565	69	27	69	28	Define the difference [Philippe Tulkens, Belgium]	Rejected. The comment is not clear.
31641	69	31	69	31	I would precise that the coastal flood studies lacking adequate validation are are often those performed at large (e.g., global) scales. At local to regional scale, there is a coastal engineering community that has been constantly improving and validating models against observations since at least 20 years (see my comment on page 67 line 47 for suggested references supporting this comment, plus e.g., Chaumillon, E., Bertin, X., Fortunato, A.B., Bajo, M., Schneider, J.L., Dezileau, L., Walsh, J.P., Michelot, A., Chauveau, E., Créach, A. and Hénaff, A., 2017. Storm-induced marine flooding: Lessons from a multidisciplinary approach. Earth-Science Reviews, 165, pp.151-184 [Gonéri Le Cozannet, France]	Rejected: coastal floods have been removed from Ch. 11
23185	69	34	69	48	Should this not be in chapter 8 rather than here? Seems like it would be more germane to the charge given to chapter 8. [Peter Thorne, Ireland]	Rejected: in order to maintain the structure of the different Ch 11 sections we include a section with the assessment of models.
105325	69	38	69	39	This is a broad-brush statement casting widespread doubt that should be solidly backed up with an assessment of the literature on calibration, its pitfalls, and best practices to avoid them, specifically in the context of hydrological models. It's not very useful in an IPCC report to say that some projections are likely to be unreliable because models have been overfitted to the available calibration data, and just leaving it at that. Yes, we can find widely different projections (lines 39-42) – but we presumably trust some more than others. How do we begin to discriminate, or is that even possible? Answering that question would presumably be a place to start an assessment. It sounds like we might be able to take something from Huang et al (lines 42-46), although it seems to me that this is already well known and widely appreciated (i.e., that peak flows are hard to reproduce). The question for the future however, is whether this makes projections of relative changes in peak flows completely unreliable, or is there a basis for nevertheless taking useful information from models that do not perform perfectly? [Francis Zwiers, Canada]	Accepted: this paragraph has been substantially modified according to suggestions by the reviewer.
125971	69	42	69	46	This statement is perhaps true for Huang et al (2017) work, but there has been a long history of statistical and flood engineering practices for extreme rare events. [Trigg Talley, United States of America]	Rejected: comments is too general to be considered.
82793	69	44	69	45	Are these annual exceedance probabilities? If so, then say so. Also affects P70 L12-13. [Blair Trewin, Australia]	Rejected: This section has been substantially rewritten

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8047	69	47	69	48	Misrepresentation of the original paper, which states that "the ensemble mean fails to perform better than any individual model" and " The EM fails to perform better than the best performing model for all maximum and minimum flow return periods (p. 8)." Thus the ensemble means is not the best but still not worse than any individual model. [jouni Räisänen, Finland]	Accepted: corrected in the revised document.
96135	69	50	69	51	The statement "The use of hydrological models for assessing changes in floods, especially for future projections, adds another dimension of uncertainty." is of course true. But hydrological and hydraulic models are currently the only tools that produce data on peak flows and water levels. That should be indicated here as well. [Nicole Wilke, Germany]	Rejected: we agree, but we have assessing the possible confidence of these models and not their availability.
105327	69	50	69	70	I think it is quite well understood that hydrologic modelling is affected by a large cascade of uncertainty, which fans out from the choice of emissions scenario through to the ultimate projections from the hydrologic models. While I'm sure that it is useful to remind readers, again, of this cascade, what needs assessment is the implications of that fan of uncertainty for our understanding of future streamflow extremes and flooding. Is there still useful information that can be obtained (both scientific, and from a user perspective) despite the fan of uncertainty? Is there a suitable way to pick an informative path through that wide delta or are there ways in which the impacts of that uncertainty can be reduced by imposing a constraint at a suitable point along the path of the development of that uncertainty? Are there types of information that we would have more confidence in than others (e.g., changes in frequency rather than magnitude, relative changes rather than absolute changes, direction of change only, or there is really nothing that can be used)? Are there approaches that can be used to increase confidence (e.g., physical reasoning that links related lines of evidence from the climate model simulations that provide input to the hydrologic model with the evidence that comes directly from the latter)? [Francis Zwiers, Canada]	Accepted: the final assessment of the section has recorded better the limitations and capabilities of flood models.
105739	69	51	69	54	Justin Sheffield; Eric Wood; Nathaniel Chaney; Kaiyu Guan; Sara Sadri; Xing Yuan; Luke Olang; Abou Amani; Abdou Ali; Siegfried Demuth; Laban Ogallo, 2013: A Drought Monitoring and Forecasting System for Sub-Sahara African Water Resources and Food Security, BAMS-D-12-00124 [Abou Amani, France]	Rejected: we are here assessing flood models and not drought models.
66393	69		69		This paper could be revised and added as a refernce Di Sante, F., Coppola, E., and Filippo, G. (submitted, a). Future projections of river floods over the European region using EURO-CORDEX simulations. Int. J. Climatol. (submitted). [Erika Coppola, Italy]	Accepted: this study has been assessed in the chapter.
23657	70	1	70	2	(Maier et al., 2018) to Maier et al. (2018) [Deniz Bozkurt, Chile]	Accepted: replaced in the document.
11709	70	1	70	2	move opening parenthesis to just before "2018" [Amy East, United States of America]	Accepted: replaced in the document.
43381	70	1		2	Read "For example, Maier et al. (2018) used a modeling framework" rather than "For example, (Maier et al., 2018) used a modeling framework" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted: replaced in the document.
23187	70	6	70	7	This makes no sense to me as written. Please redraft for clarity. [Peter Thorne, Ireland]	Accepted: the sentence has been removed.
23189	70	11	70	13	I see no defensible trace for such a statement and particularly so given that there is no confidence or likelihood statement attached. [Peter Thorne, Ireland]	Accepted: the final assessment of the section has recorded better the limitations and capabilities of flood models.
62361	70	11	70	17	The level of confidence is not included in Summary. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: the final assessment of the section has recorded better the limitations and capabilities of flood models.
23191	70	14	70	17	Not cast in confidence / likelihood language terms. [Peter Thorne, Ireland]	Accepted: the final assessment of the section has recorded better the limitations and capabilities of flood models.
5599	70	20	70	52	Thera are no informations about the coastal floods and the human role on the temperature and sea level rise and on the coastal flooding, why ? [Benoit Laignel, France]	rejected: Information of coastal floods removed from Ch. 11.
125973	70	20			Add reference to a study that attributed flood peak, inundated areas and economic losses to an anthropogenic signal: Villarini, G., W. Zhang, F. Quintero, W.F. Krajewski, and G.A. Vecchi, Attribution of the impacts of the 2008 flooding in Cedar Rapids (Iowa) to anthropogenic forcing, submitted to Nature Communication, 2019. [Trigg Talley, United States of America]	Considered but the manuscript does not appear to have been published in Nature Communication by the time of deadline.
96137	70	23	70	24	"structural differences in hydrological models are very large compared to climate models": Please add a reference for this statement. [Nicole Wilke, Germany]	Rejected: this is a simple statement and no references are needed here.
105329	70	27	70	31	Should maybe mention Pall et al (2011), even though this paper has also been cited in other IPCC assessments. Also, there are relevant event attribution studies, such as those of Teufel et al (2017, 2019) that do not rely on hydrologic models. [Francis Zwiers, Canada]	Accepted: Teufel et al. (2019) cited in the document.
62531	70	33	70	33	please consider changing "...the anthropogenic signal is different in different regions..." to "...the anthropogenic signal varies in different regions..." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected: section substantially rewritten after SOD
105331	70	34	70	35	It is incorrect to include Teufel et al (2017) as an example in this sentence. They didn't find a discernable human influence on runoff in their study, but that is very different from suggesting that anthropogenic influence reduced runoff in the case of the Calgary floods of 2013 (their model did not allow them to make direct inferences about streamflow or flooding). [Francis Zwiers, Canada]	Accepted: the reference has been removed.
107677	70	37	70	38	This paper performs a statistical attribution of the impacts of land cover: "Slater, Villarini (2017) Evaluating the drivers of seasonal streamflow rates in the U.S. Midwest, Water MDPI. 9, 695. https://www.mdpi.com/2073-4441/9/9/695" [Louise Slater, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: this study is not related to floods but to mean streamflow.
100505	70	44	70	44	The correct referrence for this statement is not 'Gudmundsson et al. (2019)' but 'Gudmundsson et al. (submitted)'. REF: Gudmundsson, L., Boulange, J., Do, H. X., Gosling, S. N., Grillakis, M. G., Koutroulis, A. G., Leonard, M., Liu, J., Müller Schmied, H., Papadimitriou, L., Pokhrel, Y., Seneviratne, S. I., Satoh, Y., Thiery, W., Westra, S., Zhang, X., Zhao, F., Globally observed trends in mean and extreme river flow attributed to man-made climate change, submitted. [Wim Thiery, Belgium]	Accepted: corrected in the revised document.
71519	70	44	70	52	The study does not focus on "extreme" floods but on 90th percentile flows (high flows better), so the term extreme should be removed. The sentence should include reference to the spatial, on the contrary it is confuse : "observed SPATIAL PATTERNS OF trends can be reproduced only if anthropogenic climate change is considered". In the following sentence the same. On the contrary, the reader may consider that temporal trends are explained by the forcing but this is only an assessment of the spatial relationship. [Sergio Vicente-Serrano, Spain]	Accepted: replaced.
62533	70	46	70	46	please replace "...hisotrical forcing or by climate model pre-industrial simulations" with "...historical forcings and with pre-industrial climate model simulations" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: corrected in the revised document.
125975	70	52			Could add here: "However, Knutson and Zeng (2018) find that observed annual precipitation increasing trends in extratropical land regions tended to be detectable and attributable in part to anthropogenic forcing, and also that CMIP5 historical runs failed to simulate precipitation increase trends that are as strong as in observations over 1901-2010." [Trigg Talley, United States of America]	rejected: this is not an study of flood attribution.
105333	71	1	71	3	This assessment should specifically include the caveat that such an assessment is only possible in a few (just one or two?) well-studied regions. [Francis Zwiers, Canada]	Accepted: Confidence assessment has been replaced

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
125977	71	8	73	23	One thing missing from this discussion of projections is how well the models have done at reproducing historical trends at the regional scale. Relevant to this are the consistency assessments of precipitation trends (historical vs. observed) over land regions (Knutson and Zeng, 2019). It may be more difficult to have confidence in future projections where historical runs were already inconsistent with observed trends. One example would be page 11-72, line 32, where Hirabayashi et al. project decreasing floods over a large portion of North America. But Knutson and Zeng show that CMIP5 historical runs simulated decreasing precipitation over the central and southcentral U.S. since 1901, but observations showed a significant increase in precipitation over those regions. Such comparisons can help in the evaluation of future projections (i.e., the proposed decrease in flooding seems less likely to materialize given the model problems over the historical period). More of this type of confrontation of model projections with historical trend consistency tests should be done in this type of context (e.g., for things that depend on regional precipitation trends). [Trigg Talley, United States of America]	Rejected: the assessment by the suggested article is based on mean precipitation but not on floods. In addition, there are limited studies comparing flood trends in observations and models.
102567	71	8	73	23	Please consider bias correction elements for this section [Philippe Tulkens, Belgium]	Rejected: The suggested comment is not specific.
5601	71	8	73	23	Why are the projections focused only on the river floods and not on the coastal floods ? A lot of coastal cities are concerned by the coastal floods which depend of the sea level rise, storm surges, wave, tide and heavy rain and coastal river floods [Benoit Laignel, France]	Rejected: coastal floods have been removed from Ch. 11
125979	71	8			Villarini and Zhang (2020) found that extreme runoff (used as a proxy for flooding) is projected to increase across large areas of the US based on several CMIP6 models. Citation: Villarini, G., and W. Zhang, Projected changes in flooding: A U.S. perspective, submitted to Annals of the New York Academy of Sciences, 2020. [Trigg Talley, United States of America]	Accepted: the reference has been assessed in the revised document.
13741	71	12	71	12	change WG II, Ch. 3 by WGII, Chapter 3 [Maria Amparo Martinez Arroyo, Mexico]	Accepted: replaced in the revised document.
62535	71	14	71	14	please replace south and Southeast Asia with southern and Southeast Asia [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: replaced in the revised document.
96139	71	24	71	25	Many model based climate impact assessments mentioned in the report do not cover adaptation measures or management activities when addressing future changes. So they all use "proxies" instead of running real world simulations. Please clarify this in the framing chapter and wherever appropriate to avoid misinterpretations. [Nicole Wilke, Germany]	Rejected: Really this is stressed in the statement commented, an aspect that introduces uncertainties in the assessment.
9159	71	26	71	26	Alfieri et al. (2016) is not in the references. Is it 2015 or 2017? [Kevin Hennessy, Australia]	Accepted: Replaced in the revised document. It should be 2017.
62433	71	26	71	26	Alfieri et al., (2016) missing page 71 line 26 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: Replaced in the revised document. It should be 2017.
3153	71	29	71	29	please confirm here is 4°C or 3°C? [Hui Wang, China]	Rejected: It is 4ºC
43383	71	29			Read "These results are supported by Paltan et al. (2018)" rather than "These results are supported by (Paltan et al., 2018)" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted: replaced in the revised document.
96141	71	30	71	30	"well-established" is not a validation criterion. Suggest to delete. [Nicole Wilke, Germany]	Accepted: removed in the revised document.
62435	71	30	71	30	Huang et al. (2018) missing page 71 line 30 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: Link to the reference included.
108109	71	31	71	31	Instead of the term “bias-corrected” I suggest to use the term “bias adjusted”, which is explained in Chapter 10 Section 10.3.1.4.2 and used in Chapter 2, 8, 10 and 12. [Claas Teichmann, Germany]	Rejected: this section has been entirely rewritten in the final document
13743	71	32	71	32	Change 1.5, 2.0, and 3.0°C by 1.5°C, 2°C and 3°C [Maria Amparo Martinez Arroyo, Mexico]	Accepted: replaced in the revised document.
13745	71	36	71	36	standardize the format: 2.0°C or 2°C [Maria Amparo Martinez Arroyo, Mexico]	Accepted: replaced in the revised document.
23971	71	41			Replace "grids" with "gridpoints" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: this section has been entirely rewritten in the final document
23197	71	47	71	47	British and Irish isles (also applies in several other spots) [Peter Thorne, Ireland]	Rejected: this section has been entirely rewritten in the final document
62537	71	53	71	53	please replace “south and east Asia” with southern and eastern Asia [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: replaced in the revised document.
13747	72	2	72	2	standardize the format: 2.0°C or 2°C [Maria Amparo Martinez Arroyo, Mexico]	Accepted: format uniform in the revised document.
62539	72	4	72	5	please replace “east Europe and south Canada” with eastern Europe and southern Canada [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: replaced in the revised document.
125981	72	9			The following reference may be relevant: Giuntoli, I., G. Villarini, C. Prudhomme, and D.M. Hannah, Uncertainties in projected runoff over the contiguous United States, Climatic Change, 150(3), 149-162, 2018. [Trigg Talley, United States of America]	Rejected: the suggested article focus on mean river streamflow and not on high or extreme flow.
39721	72	12	72	12	"agreement… is poor" -> Not IPCC uncertainty language - there is low agreement? [TSU WGI, France]	Accepted: replaced in the revised document.
62541	72	13	72	13	please replace South Europe with southern Europe [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Replaced in the revised document.
74551	72	14	72	14	"and Guereiro et al. (2018a)" in place of "and (Guereiro at al. (2018a)" [Moulay Driss HASNAOUI, Morocco]	Accepted: replaced in the revised document.
43385	72	14			Read "and Guerreiro et al. (2018a) projected a decrease" rather than "and (Guerreiro et al., 2018a) projected a decrease" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted: replaced in the revised document.
28961	72	16			May need to check 8.4.1.5 is consistent: "medium confidence for a general increase in flooding, although with high geographical variations in the amount but regional consistency in flooding increases in the West Amazon, the Andes, and northern Eurasia (Chapter 11, Section 11.5.5). There is medium confidence for future increases in urban and coastal floods, generally (Chapter 11, Section 11.5.5)," [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: this has been checked and there is consistency between both sections.
74553	72	18	72	18	; Guereiro et al., 2018a) [Moulay Driss HASNAOUI, Morocco]	Accepted: replaced in the revised document.
108125	72	25	72	25	Instead of the term “bias corrected” I suggest to use the term “bias adjusted”, which is explained in Chapter 10 Section 10.3.1.4.2 and used in Chapter 2, 8, 10 and 12. [Claas Teichmann, Germany]	Rejected: this section has been entirely rewritten in the final document
13749	72	29	72	29	Change RCP 8.5 by RCP8.5 [Maria Amparo Martinez Arroyo, Mexico]	Accepted: replaced in the revised document.
43387	72	41			Read "(Kundzewicz et al., 2017)" rather than "Kundzewicz et al. (2017)" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted: replaced in the revised document.
105335	72	45	72	46	I guess I’m wondering if this is the chapter’s view, or that of Kundzewicz. [Francis Zwiers, Canada]	Rejected: this paragraph has been removed n in the final document
42535	72	46	72	46	Typo: in found -> is found (please check meaning) [Joan Bech, Spain]	Accepted: corrected in the revised document.
62545	72	46	72	46	please change “north Asia” to “northern Asia” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected: this sentence has been removed.
62543	72	46	72	47	This sentence does not make sense…did you mean to say “An increase is found in southeast and north Asia…”? If so, please clarify an increase in what [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected: this sentence has been removed.
42537	72	51	72	51	Please check English: They provide more a realistic -> They provide a more realistic (check meaning) [Joan Bech, Spain]	Accepted: corrected in the revised document.
23199	72	52	72	52	predictably isn't the right word here. Something like inevitably would be better? [Peter Thorne, Ireland]	Accepted. Rewritten in the revised document.
62547	72	53	72	53	please don’t end the sentence with “this”. Either reword the sentence, or clarify what this refers to at the end of the sentence [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected: this sentence has been removed.
43389	72	53			Read "(Kundzewicz et al., 2017)." rather than "(Kundzewicz et al. (2017)." [Cyriaque Rufin Nguimalet, Central African Republic]	Rejected: the sentence has been modified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66395	72		72		This paper could be revised and added as a refernce Di Sante, F., Coppola, E., and Filippo, G. (submitted, a). Future projections of river floods over the European region using EURO-CORDEX simulations. Int. J. Climatol. (submitted). [Erika Coppola, Italy]	Accepted: this study has been assessed in the chapter.
62549	73	1	73	3	Please consider changing "...but available studies project an increase in urban flood potential, for example in cities of North America (Kermanshah et al., 2017; Hettiarachchi et al., 2018), in northern China (Zhou et al., 2018b), and in India (Vemula et al., 2019)" to "...but available studies project an increase in urban flood potential: in North America (Kermanshah et al., 2017; Hettiarachchi et al., 2018), northern China (Zhou et al., 2018b), and India (Vemula et al., 2019)" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected: we prefer the current format of the sentence.
37595	73	1	90	1	Chapter 11 suffers from a lack of consideration of drought extremes in the context of their potential to damage plants (being the source of primary production and key players in atmospheric water and CO2 fluxes). For clear physiological reasons, drought damage to plants is disproportionately accelerated by the combination of hot and dry conditions. In the opinion of this reviewer, this very important fact should warrant specific attention to the likelihood and intensity of such combinations. This combination is partially addressed by the discussion of SPEI, but plant damage is likely to exceed projections based on SPEI due to the unpredictable effects of high temperature on leaf diffusive porosity (leaf diffusive conductance during drought is determined by properties of leaf surface waxes, which are sensitive to denaturation at high temperature and a loss of function). see Brodribb et al 2020 Science for review. [Timothy Brodribb, Australia]	Accepted - This issue has been stressed in cross chapter box on carbon-water nexus and further assessment included in the section 11.6.5
105337	73	4	73	6	See my comment on this topic in the context of Executive Summary. My inclination would be to be a bit more cautious about this, at least in the absence of a discussion of exactly what the chapter means by "flood potential", which I am guessing would vary widely from one urban area to another depending on the details of the urban drainage system that are specific to each area. [Francis Zwiers, Canada]	Rejected: This is related to the increased risk of high precipitation events.
131423	73	4	73	18	What is meant by "developed urban areas"? It sounds like that "undeveloped urban areas" would not been affected like informal settlements, whereas these are often most affected because of lacking infrastructure etc. Maybe "sealed surfaces in urban areas" is meant? [Hans Poertner and WGII TSU, Germany]	Accepted. The text has been removed.
31643	73	6	73	13	"There are few direct projections for changes in coastal floods": in fact there are projections, but may be one should distinguish those at global scales that give the general trends, but can not include yet all processes such as the wave and wind setup or overtopping due to computational and storage reasons (e.g., Hinkel et al 2014), and those performed at local scales that can include all these processes, but often belong to grey litterature and coastal engineering studies (e.g., see sections 2 and 3,3 in Le Cozannet et al 2017, and references therein, with e.g. reference to FEMA reports) Hinkel, J., Lincke, D., Vafeidis, A.T., Perrette, M., Nicholls, R.J., Tol, R.S., Marzeion, B., Fettweis, X., Ionescu, C. and Levermann, A., 2014. Coastal flood damage and adaptation costs under 21st century sea-level rise. Proceedings of the National Academy of Sciences, 111(9), pp.3292-3297. [Gonéri Le Cozannet, France]	Rejected: reference and comments to coastal floods have been removed from Ch. 11
69247	73	6	73	16	The projections of extreme sea levels (mainly storm surges) are also discussed in Chapter 9.6.4.2 but the descriptions in this section and those in 9.6.4.2 seem different. Please check again the corresponding contents in Chapter 9. For example, the changes in extreme sea level (ESL) due to tropical cyclones, extreme waves and compound flooding by coastal and river discharge are discussed in detail in 9.6.4.2. There is also further description of areas to be affected as NW Europe, Patagonia and others. However, the geographical regions described in 9.6.4.2 as the areas to be affected of coastal flooding do not coincide with those described in Chapter 11. Thus, it would be requested that the definition of 'sea level' as well as the cause of coastal flooding would be revisited and that the regional coastal flood risk described in Chapter 11 would be rechecked for consistency. [Kaoru Magosaki, Japan]	Accepted: Reference to coastal floods has been removed from Chapter 11
62551	73	7	73	7	Please consider changing "should" to "will likely" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected: reference and comments to coastal floods have been removed from Ch. 11
40617	73	9	73	16	Overlap with Ch9? Small number of studies cited. [TSU WGI, France]	Accepted: Reference to coastal floods has been removed from Chapter 11
62553	73	11	73	12	this sentence is incomplete and does not makes sense...please revise for clarity [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected: reference and comments to coastal floods have been removed from Ch. 11
8049	73	11	73	12	"Thus, the projections needed in developing countries": something misisng? [jouni Räisänen, Finland]	Rejected: reference and comments to coastal floods have been removed from Ch. 11
13751	73	13	73	13	standardize the format: 2000 - 2050 or 2000-2050 [Maria Amparo Martinez Arroyo, Mexico]	Rejected: reference and comments to coastal floods have been removed from Ch. 11
78313	73	13	73	14	Singapore has an interest to understand extreme sea level events and coastal floods. [Leonie Lee, Singapore]	Rejected: reference and comments to coastal floods have been removed from Ch. 11
105339	73	13	73	16	Findings reported from the literature are given with excessive precision, but by reporting in this way, the chapter implicitly indicates that it thinks these results are reliable. [Francis Zwiers, Canada]	Rejected: reference and comments to coastal floods have been removed from Ch. 11
13753	73	16	73	16	standardize the format: RCP 4.5 or RCP4.5 and RCP 8.5 or RCP8.5 [Maria Amparo Martinez Arroyo, Mexico]	Rejected: reference and comments to coastal floods have been removed from Ch. 11
20745	73	18	73	23	Inasmuch as the general trend for precipitation increasing along with global warming is recognized, one expects that the flood occurrences should follow a parallel evolution; the basic question then is whether this happens, and why it eventually does not. It is a pity that climate scientists do not address this issue clearly. The link between the last and last before one sentence is mysterious. In line 23, "more uncertain" than what? [philippe waldteufel, France]	Accepted: Final assessment has been rewritten and reorganised.
23201	73	18	73	23	It feels odd to lead with the very specific aspect of urban flooding here and I'm not sure I saw enough of a trace in the underlying text to justify as high confidence as is being assigned to urban flooding. [Peter Thorne, Ireland]	Accepted: medium confidence is stated
6877	73	21	73	21	Replace "in the west Amazon, the Andes, and northern Eurasia" with "in large parts of both hemispheres" [Christos Zerefos, Greece]	Rejected: current sentence conveys the desired meaning.
39487	73	21	73	22	Consider to replace the word 'Eurasia' and with the words 'North America and southeast and north Asia' as referred to on page 72 line 46 to 47 as Eurasia does not accurately cover these continents. [Tamara van 't Wout, Qatar]	Accepted: replaced in the revised document.
62555	73	22	73	22	please change "west Amazon" to "western Amazon" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: replaced in the revised document.
66351	73	22	73	23	This statment is not valid everywhere and it is in contrast with the assessment in CH12 that gives high confidence in the direction of changes for regions like Polar Artic regions and Central Europe [Erika Coppola, Italy]	Accepted: the sentence has been deleted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2417	73	26	73	26	This is a comment about the whole section. My comment is that this section could be clearer that the main developments in drought research since the previous IPCC report have focused on the issues that exist when calculating future trends in potential evapotranspiration (PET) under greenhouse warming and interpreting their role for actual hydrological variables (ET, soil moisture, runoff and streamflow). Since future PET trends are used in the calculation of widely used droughts metrics like the PDSI and the SPEI, this has raised some issues regarding the use of such metrics in future drought assessments, in particular concerns that they might be overestimated. This is mentioned at times in the section, and the relevant studies are cited throughout, but this could be made clearer (I made some more comments on the relevant pages/lines). One related issue is that the authors mention the existence of issues with the PDSI, but do not mention the main one, which is that most PDSI-focused studies do not account for CO2-induced changes in stomatal conductance. The authors then explain that they are going to focus more on the SPEI rather than on the PDSI, without mentioning that the SPEI also suffers from the same shortcoming, that is, it relies on future PET trends. Similarly, projections of future hydrological droughts most often rely on offline hydrological model projections, where hydrological models are driven, in part, by PET estimates calculated from climate model outputs. Recent studies have highlighted the risk of such a modeling framework to lead to negatively biased estimates of future runoff and streamflow (e.g., Milly and Dunne 2017). Milly, P. C. D., & Dunne, K. A. (2017). A hydrologic drying bias in water-resource impact analyses of anthropogenic climate change. JAWRA Journal of the American Water Resources Association, 53(4), 822-838. [Alexis Berg, United States of America]	Noted: The CO2 effects of plant WUE have been widely discussed in coordination with Chapters 5 and 8. There is a new Rejected. in which the state of the art on this issue is included (CC-Box 5.1). This point and the other limitations mentioned by the reviewer are indicated in Section 11.6: "Moreover, uncertainties in drought projections are affected by the consideration of 16 plant physiological responses to increasing atmospheric CO2 (Greve et al., 2019; Mankin et al., 2019; Milly and Dunne, 2016; Yang et al., 2020; Chapter 5, Rejected. 5.1), the role of soil moisture-atmosphere feedbacks for changes in water-balance and aridity (Berg et al., 2016; Zhou et al., 2021), and statistical issues related to considered drought time scales (Vicente-Serrano et al., 2020a)." ; "Several studies suggest that increasing atmospheric CO2 could lead to reduced leaf stomatal conductance, which would increase water-use efficiency and reduce plant water needs, thus limiting ET (Chapter 5, Cross-4 Chapter Box 5.1; Greve et al., 2017; Lemordant et al., 2018; Milly and Dunne, 2016; Roderick et al., 2015; 5 Scheff et al., 2017; Swann, 2018; Swann et al., 2016)."; "Another mechanism reducing the ET response to increased AED in projections is the control of soil moisture limitations on ET, which leads to reduced stomatal conductance under water stress (Berg and Sheffield, 26 2018; Stocker et al., 2018; Zhou et al., 2021)."
53545	73	26			It may be also useful here and in other related chapters (including CH8, 10 and 11) to distinguish between drought indices using fixed versus time-dependent thresholds. The later may be more suitable to distinguish changes in variability from changes in mean state, and thus changes in drought frequency/severity from changes in aridity. [Hervé Douville, France]	Rejected - In general, drought indices considered in the chapter are based on standardized approach in order to allow spatial comparability and assessment is based on drought event characteristics: duration and severity.
89167	73	26			This section should cross-reference relevant sections in Chpater 8, in sections 8.2-8.4 [Angeline Pendergrass, United States of America]	Taken into account - coordination and consensus with chapters 8 and 12 has been fully addressed to decide what to include in each chapter and what to cross-reference between the chapters
2419	73	28	73	28	Replace "anomalies" with "negative anomalies", as, in theory, anomalies can also be positive. [Alexis Berg, United States of America]	Rejected - Considered the suggestion by Jacob Scheff in comment 10093
103845	73	28	73	28	Overall miss the spatial aspects of drought; the area affected is an important measure of the severity of an event and several indices have been developed to characterise the spatial aspect of a drought - either just as the area covered at a given time or as the area and dynamic of a given drought cluster (contineous in space) [Lena M Tallaksen, Norway]	Rejected - this section has been substantially revised and rewritten
37597	73	28	73	28	"negative anomalies" [Timothy Brodribb, Australia]	Rejected - Considered the suggestion by Jacob Scheff in comment 10093
62557	73	28	73	30	citation for this definition? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: Included reference to Wilhite and Pulwarty (2017)
105341	73	28	73	44	One of the things that struck me is that this brief chapeau paragraph doesn't really explain why "drought" should be considered in a chapter on extremes. Something that is extreme at a particular location presumably has high impact and occurs infrequently at that location (at least, infrequently enough that local inhabitants, activities, ecosystems, etc., are not well adapted to the occurrence of the kind of extreme in question). But frequency is a notion that seems largely missing in this section – rather, the focus seems to be largely on summary measures of drought with notions of rarity considered mostly implicitly. Perhaps that reflects the state of the development of the science – if so, that would be worth pointing out because, I think it reflects a clear limitation of the application of the science. [Francis Zwiers, Canada]	Accepted - Thanks for the comment. We include in the definition the impact perspective: "negative impacts for various components of natural systems and economic sectors" and included reference to the infrequent character of the phenomenon.
23203	73	28	75	2	Chapter 8 also defined droughts. The definition should be covered once (presumably in chapter 8) and cross-referenced. The current definitions somewhat differ from one another and need to at an absolute minimum be reconciled. Defining droughts in the context of the scoped outline arguably rests with chapter 8 and not chapter 11. [Peter Thorne, Ireland]	Taken into account - coordination and consensus with chapters 8 and 12 has been fully addressed to decide what to include in each chapter and what to cross-reference between the chapters
10093	73	28			"anomalies from average" could be rendered more simply and directly as "below-average". [Jacob Scheff, United States of America]	Accepted - Replaced in the revised document
101607	73	28			Overall miss the spatial aspects of drought; the area affected is an important measure of the severity of an event and several indices have been developed to characterise the spatial aspect of a drought - either just as the area covered at a given time or as the area and dynamic of a given drought cluster (contineous in space) [Lena M Tallaksen, Norway]	Rejected - this section has been substantially revised and rewritten
44033	73	30	73	33	Please include this too. "Although there are different types of droughts, its well known that the meteorological drought is the root cause for all other types of droughts. The rainfall deficits propagate to agricultural drought and then hydrological drought. The combined effect of these three types of droughts are reflected in ecological drought and socio-economic drought." [SABYASACHI SWAIN, India]	Accepted partially - meteorological drought has been included but propagation among drought types is already addressed in Chapter 8.
44035	73	30	73	33	Mukherjee et al. (2018) provided a detailed insight for propagation of droughts from one form to the other, which is addressed in my previous comment (No. 2). Reference: Mukherjee, S., Mishra, A., & Trenberth, K. E. (2018). Climate change and drought: a perspective on drought indices. Current Climate Change Reports, 4(2), 145-163. [SABYASACHI SWAIN, India]	Accepted - the reference has been included
39489	73	30	73	33	Consider to add 'meteorological drought' as a type of drought, also since reference is made to 'meteorological droughts' on page 77 line 50 to 53, on page 85 line 23 to 24 and on page 86 line 1 to 3 as a drought type. [Tamara van 't Wout, Qatar]	Accepted - meteorological drought has been included
11115	73	30	73	33	Drought types mentioned here are not in agreement with those in Table 11.3. [Wen Wang, China]	Accepted - meteorological drought has been included to match the different variables assessed in the table.
44031	73	30	73	33	This misses out the most important form of drought i.e. meteorological drought (precipitation deficits). This should be included. [SABYASACHI SWAIN, India]	Accepted
103839	73	31	73	31	Following the most commonly (and recently) used terminology (also in the SREX report) related to drought propagation, drought is classified into the types: meteorological, soil moisture and hydrological drought (soil moisture instead of agricultural drought which is linked to a specific crop) [Lena M Tallaksen, Norway]	Rejected - Agricultural drought is not only driven by soil moisture deficits but also the atmospheric evaporative demand may affect. This is a classical and well accepted definition of droughts. We have also included ecological droughts.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10095	73	31			Another key figure to cite here, besides Fig. 11.17, is Fig. 8.5! [Jacob Scheff, United States of America]	Taken into account - coordination and consensus with chapters 8 and 12 has been fully addressed to decide what to include in each chapter and what to cross-reference between the chapters
100775	73	31			Following the most commonly (and recently) used terminology (also in the SREX report) related to drought propagation, drought is classified into the types: meteorological, soil moisture and hydrological drought (soil moisture instead of agricultural drought which is linked to a specific crop) [Lena M Tallaksen, Norway]	Rejected - Agricultural drought is not only driven by soil moisture deficits but also the atmospheric evaporative demand may affect. This is a classical and well accepted definition of droughts. We have also included ecological droughts.
104489	73	36	73	36	The different time scales of drought are mentioned, however, I think this part should be elaborated and linked to drought in the different parts of the hydrological cycle. For example, a soil moisture drought commonly has a time scale of weeks or months, whereas hydrological drought normally would last longer. The temporal scale depends on the hydroclimatology of the region under study. For instance are catchments in seasonal snow climates much less prone to multi-year droughts as stores are replenished every winter/spring during snowmelt (see e.g. Brunner and Tallaksen (2019). [Lena M Tallaksen, Norway]	Accepted - This issue has been mentioned in the text and the suggested reference included
101609	73	36	73	37	The different time scales of drought are mentioned, however, I think this part should be elaborated and linked to drought in the different parts of the hydrological cycle. For example, a soil moisture drought commonly has a time scale of weeks or months, whereas hydrological drought normally would last longer. The temporal scale depends on the hydroclimatology of the region under study. For instance are catchments in seasonal snow climates much less prone to multi-year droughts as stores are replenished every winter/spring during snowmelt (see e.g. Brunner and Tallaksen (2019). [Lena M Tallaksen, Norway]	Accepted - This issue has been mentioned in the text and the suggested reference included
20747	73	36	73	40	It is surprizing that little attention is given to the area concerned by a drought [philippe waldteufel, France]	Rejected - this section has been substantially revised and rewritten
103841	73	37	73	37	Suggest to define 'flash drought' - a relatively new term used to characterise a sudden onset and rapid intensification of (soil moisture) drought conditions [Lena M Tallaksen, Norway]	Accepted - Definition was included
101327	73	37	73	37	The different time scales of drought are mentioned, however, I think this part should be elaborated and linked to drought in the different parts of the hydrological cycle. For example, a soil moisture drought commonly has a time scale of weeks or months, whereas hydrological drought normally would last longer. The temporal scale depends on the hydroclimatology of the region under study. For instance are catchments in seasonal snow climates much less prone to multi-year droughts as stores are replenished every winter/spring during snowmelt (see e.g. Brunner and Tallaksen (2019). [Lena M Tallaksen, Norway]	Accepted - This issue has been mentioned in the text and the suggested reference included
89157	73	37			Another study that also discusses potential future changes in flash droughts is Pendergrass et al. (2020) Pendergrass, Angeline G., Gerald A. Meehl, Roger Pulwarty, Mike Hobbins, Andrew Hoell, Amir AghaKouchak, Céline J. W. Bonfils, et al. “Flash Droughts Present a New Challenge for Subseasonal-to-Seasonal Prediction.” Nature Climate Change 10, no. 3 (March 2020): 191–99. https://doi.org/10.1038/s41558-020-0709-0. [Angeline Pendergrass, United States of America]	Accepted - the reference has been included
101323	73	37			Suggest to define 'flash drought' - a relatively new term used to characterise a sudden onset and rapid intensification of (soil moisture) drought conditions [Lena M Tallaksen, Norway]	Accepted - Definition was included
11117	73	40	73	40	several should be "many". [Wen Wang, China]	Accepted - Replaced in the revised document
82045	73	40	73	40	Here the word 'several' is used to refer to drought indices, but should really be 'many' or similar (the Lloyd-Hughes 2014 paper cited in the chapter counts >100 and there will be many more. Sorry this sounds pedantic but quite a change in meaning from one word. There are other instances through thechapter like this that should be checked and changed. [Jamie Hannaford, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Replaced in the revised document
20257	73	47	74	4	In fig 11.17, while the diagram is pleasant and rather easy to understand, additional notations and poor writing of the legend make things more obscure. What does the sentence on lines 49-51 mean when taking it word by word? The meaning of +, -, 0 notations is not straightforward. Since the "*" sign is located between atmospheric dryness and plant water deficit, reference to soil moisture deficit is against logics; a similar remark applies to the "**" sign [philippe waldteufel, France]	Taken into account - The figure 11.17 has been removed from CH 11 but generated a merged common figure with Ch. 8.
70407	73		91		This chapter uses terms like “CO2 fertilization” (and also “vegetation-CO2 feedbacks”, “fertilizing CO2 effects”) conflates multiple competing aspects of how plants respond to increasing CO2, and in particular how those response alter water use and thus water flux. CO2 fertilization does not reflect the standard terminology in the literature. Plant responses to CO2 can influence land evapotranspiration through two mechanisms, which have opposite effects on evapotranspiration: (1) increased rates of photosynthesis and increases in leaf area, which increase transpiration, and (2) stomatal closure, which decreases transpiration. The term “CO2 fertilization” generally only refers to the first process. The term “plant physiological responses to increasing CO2” can encompass both CO2 fertilization and stomatal closure. In order for this chapter to remain clear to the diverse scientific communities interested in projected changes in drought, we recommend outlining each of these two opposing processes and explicitly defining which processes are included in different terms. We have tried to point out specific instances where this unspecific terminology is used in additional comments. [Abigail Swann, United States of America]	Accepted: A new shared figure between Ch 8 and Ch 11 in which these two different mechanisms are included separately.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70409	73		91		The aspects of this chapter related to drought seems inadequately balanced. In particular, the role of plant responses to increasing CO2 are not clearly defined, and generally dismissed as unimportant in the dominant narrative of the chapter, with a skew towards one group of authors (for example, 29 papers from one lead author are referenced). We don't feel that this reflects the state of the literature. The narrative is also inconsistent with the presentation in Chapter 8 on plant controls over evapotranspiration fluxes and drought (sections 8.2.3, 8.4.1.4, 8.4.1.8, 8.5.1.1.3). We have tried to highlight specific issues in additional review comments, but feel that the first order role of plant processes in reducing evapotranspiration under high CO2 and modifying when and where droughts occur is not represented. We recommend adopting some of the language from Chapter 8, and in our specific comments. [Abigail Swann, United States of America]	Rejected: The CO2 effects of plant WUE have been widely discussed in coordination with Chapters 5 and 8. There is a new Rejected. in which the state of the art on this issue is included. There is agreement with Ch. 5 and Ch. 8 on a low confidence on the CO2 fertilizing effects related to a possible WUE. The assessment is not inconsistent with Ch. 8.2.3.1, so we disagree with the statement by the reviewer on the possible inconsistencies with Ch. 8. They overlap with the possible effects on morphological changes (mostly leaf area, root depth, etc.) but also with the radiative CO2 effects, which increase the vegetative growth periods, increases evaporative demand but also affects physiological plant processes (included in the mentioned Rejected.). Also limitations of the models are mentioned since they are also strongly relevant. Assessment of CO2 effects on future projections of hydrological and biophysical droughts are expanded and better clarified in section 11.6, including the existing uncertainties related to the possible CO2 fertilizing effects and the need of separate the possible hydrological effects from the plant stress effects under a warmer world. Finally, the reviewer suggests that the assessment is biased to the possible points of view of some of the authors. She does not indicate who is referring but I suspect it could be Vicente-Serrano (26 citations in the chapter) or Seneviratne (25 citations in the chapter), although they do not coincide with the 29 suggested. I suspect that she refers to Vicente-Serrano with a total 29 over the entire chapter. Let me to say that the section 11.6 shows one of the lower ratios of self-citations among the different sections of the chapter. In addition, the majority of the references to Vicente-Serrano included were related to illustrate some statements but the thesis of the section are not driven by specific
62465	73				In general, for the section on drought the literature seems quite limited, In Sub- Sahara Africa most of the focus was on South Africa with not much mention of the regional or continental changes. In addition this section is citing numerous papers under review. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected: 11.6 is the section that includes more literature in Chapter 11.
101325	73				Overall miss the spatial aspects of drought; the area affected is an important measure of the severity of an event and several indices have been developed to characterise the spatial aspect of a drought - either just as the area covered at a given time or as the area and dynamic of a given drought cluster (contineous in space) [Lena M Tallaksen, Norway]	Rejected - this section has been substantially revised and rewritten
104491	74	9	74	9	Table 11.3 Miss the NVDI as an index of drought (most closely related to soil moisture drought) [Lena M Tallaksen, Norway]	Rejected - we do not include indices based on vegetation metrics. There are several and this is not a mandate of the WGI. More relevant for the WGII.
70967	74	9	74	9	I am surprised not to see any mention of P-E, which would connect to the hydrological cycle (and thereby to Chapter 8). Surely this is important for drought? [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The assessment is also based on studies using P-E metrics, such as Padron et al. (2020), which are used as complementary information to assess change in agricultural and ecological droughts (see also Section 11.9.4). However this table is providing a list of more standard drought metrics from the literature.
105343	74	9	74	9	What does it mean for some feature of drought type to be "critical"? This links to my previous comment concerning the opening paragraph of this section – what makes drought "extreme"? [Francis Zwiers, Canada]	Accepted - We agree, the term critical is ambiguous. Replaced by low-frequency
105345	74	9	74	9	Also, the inclusion of "synthetic measures" in the last row of the table seems a bit out of place since they evidently are not linked specifically to different drought types. A further point is that while I understand that the authors mean metrics that synthesize drought information, I would urge them to find another term since, in English, something "synthetic" is generally understood to be artificial that is distinct from something natural (e.g., acrylic or polyester fiber as opposed to wool or cotton). [Francis Zwiers, Canada]	Accepted - we have replaced the terminology: Atmospheric-based drought indices
105347	74	9	74	9	For the last column of this table, what determines whether a reference is "key"? In deference to the many other people who have worked in these areas (not all of them can be cited in a table), it might be better to use a title like "Representative references". [Francis Zwiers, Canada]	Accepted - We agree, "representative" is much better.
82171	74	9	74	9	Table 11.3: For SPI the basic original reference McKee et al. 193 should be mentioned, similarly in the case of PDSI (Palmer 1965) [Borbála Gálos, Hungary]	Rejected - The report is not generally including references before 2012-2013
82173	74	9	74	9	Table 11.3: All references should be listed only in the "Key references" column [Borbála Gálos, Hungary]	Accepted - the references have been moved there.
101329	74	9	75	1	Table 11.3 Miss the NVDI as an index of drought (most closely related to soil moisture drought) [Lena M Tallaksen, Norway]	Rejected - we do not include indices based on vegetation metrics. There are several and this is not a mandate of the WGI. More relevant for the WGII.
125983	74	9	75	2	This table of literature synthesis is useful and should be emulated throughout the WGI AR6. [Trigg Talley, United States of America]	Rejected - This is specifically developed for Ch 11. Other chapter may have different priorities.
107413	74	9	75	2	Drought can be measured by plant functional traits; e.g., root morphology and depth, plant life span, leaves, … [Rachda Berrached, Algeria]	Rejected - We agree, but there are not well defined metrics based on these physiological characteristics that can be applied worldwide and used to generate spatially and comparable drought indices.
82047	74	10	74	10	I don't really understand what Table 1 is for. From the caption it sounds like it is about the different drought types in general, and is fine in that regard. But the 'Comments' box is a mix of content, some generally about the Drought type and indices, but I was surprised to see the references to 'Low confidence' and so on, whcih seem an odd fit. And not very systematic, i.e. only in the AE and soil moisture box. If this is being done it should apply for all drought types? [Jamie Hannaford, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the structure of the table has been changed following the suggestions.
38429	74		74		Table 11.3, Row 3, Col 3: The references (Hobbins et al., McEvoy et al., 2016) are not required because they are in Row 3, Col 5. [Mansour Almazroui, Saudi Arabia]	Accepted - the references have been removed from the second column.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38433	74		75		Table 11.3: The references from Col 4 (Rows 3, 4 & 5) should be moved to col 5 to avoid duplication. [Mansour Almazroui, Saudi Arabia]	Accepted - the references have been moved to the last column.
10097	74				Table 11.3, "Critical precipitation deficits" row: Another commonly used metric is simple precipitation deciles or precipitation percentiles, e.g. http://www.bom.gov.au/climate/history/rainfall/ https://www.droughtmanagement.info/deciles/ https://niwa.co.nz/static/climate/rain_decile_1month.png?1234 [Jacob Scheff, United States of America]	Accepted - they are included in the table
10099	74				Table 11.3, "Critical increase in atmospheric evaporative demand" row: It's unclear what's meant by "atmospheric dryness". Perhaps "atmospheric dryness" should be replaced by "vapor pressure deficit"? Certainly, "vapor pressure deficit" should be mentioned somewhere in this row. [Jacob Scheff, United States of America]	Rejected - we do not enter to mention possible effects of VPD on plant physiology but a metric of an atmospheric evaporative demand. VPD is one of the variables used to calculate AED but it does not fully capture both radiative and aerodynamic components of the AED so it is not recommended to be a robust metric of the AED.
71521	75	1	75	1	Remove "standardized" at the end of the table and replace the last sentence of the table by: "The SPEI is not intended to be a soil moisture proxy but it allows to identify vegetation stress conditions in which soil moisture and atmospheric evaporative demand play an important role." [Sergio Vicente-Serrano, Spain]	Accepted - Replaced sentence.
82049	75	1	75	1	I think this 'hydrological droughts' box is very light compared to all the others. It is very brief and does not go into much detail. It implies SRI and SSI are the main indices but to my mind these are quite recent developments and used to be quite niche. There is much wider range of indices used, e.g. threshold level methods (see reviews like Van Loon (2015). It also does not do justice to low flows, What about indices for ephemeral/intemittent streams (a growing area). There is a big literature on low flows and many different low flow indicators (7-day miniumum, Q95 and so on. The point about observational data is very vague - there are many papers on the state of global streamflow datasets and the issues with them (see my later point below). Finally, but very importantly, this hydrological box only discusses river flows but groundwater is a fundamental variable (see also below), with a very large literature and, again, very many indices used [Jamie Hannaford, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the threshold method and the SGI are included as well as reference to Van Loon 2015 and Bloomfield and Marchand (2013)
103859	75	1	75	1	Suggest to add Stagge et al., 2015 under Combined sythetic measures of drought. The paper is a benchmark paper, well cited, on candidate distributions for the SPI and the SPEI indices; https://rmets-onlinelibrary-wiley-com.ezproxy.uio.no/doi/full/10.1002/joc.4267 [Lena M Tallaksen, Norway]	Accepted - reference has been included.
37609	75	1	75	1	Table 11.3 describes SPEI as a useful composite of soil and atmospheric deficit. This is a key point in relation to vegetation mortality. The combination of heat and drought appears to be the strongest predictor of vegetation damage (PJ Mitchell, AP O'Grady, KR Hayes, EA Pinkard Exposure of trees to drought-induced die-off is defined by a common climatic threshold across different vegetation types. Ecology and Evolution 4 (7), 1088-1101) [Timothy Brodribb, Australia]	Accepted - included in the table
125985	75	2	75	2	In the last row in Table 11.3, the acronym SPEI seems inconsistent with the associated phrase. [Trigg Talley, United States of America]	Accepted - corrected in the revised document
103863	75	2	75	2	Essential to the SPI and SPEI (in particular) is the flexibility of the SPI to account for different time scale by summing precipitation over k months, termed accumulation periods. Accordingly it has been used as proxies for soil moisture and hydrological drought. It is worth mentioning this feature of the indices as it is much used (even though it is not intended for this use as highlighted in the table). [Lena M Tallaksen, Norway]	Accepted - this essential feature has been included
105349	75	4	75	4	For the remainder of this section, each subsection is further divided in to sub-subsections that address in turn, the rows of Table 11.3. In each case, this is concluded with a summary that highlights the assessments that are made. That structure is fine, although this tidy matrix of paragraphs does sometimes result in small sub-subsections with relatively little content. More importantly, it is not always evident how to link the assessments in the summaries to the evidence that is discussed in the individual sub-subsections. Thus sub-subsections often provide reviews, but without concluding with an assessment, or a statement that an assessment is not possible given the state of the evidence in the sub-subsections. In a few instances, the sub-subsections start with an assessment (e.g., 11.6.1.1), but again it is not always evident where the supporting evidence is evaluated or how the assessment was made. That is, it is often not evident how the reader should identify the “traceable account” (the evidence and associated reasoning) leading to the assessment that is required by the IPCC guide to the use of uncertainty language. [Francis Zwiers, Canada]	Rejected - thanks for the comment. We agree that different ways could be followed in order to organise the section. Nevertheless, we think more logical to assess individually the different drought types and to make a final summary of the assessment. Really there is few space to include a summary in each one of the small subsections.
18375	75	5	75	15	As stated in many previou studies (e.g., Dai 2011a; Dai 2011b), surface aridity level (and thus drought severity) is determined by the surface water balance between the water supply (i.e. P) and demand (i.e., PET). The formation of precipitation is mainly an atmospheric process (related to atmospheric convection, frontal lifting, cloud miscrophisics, low-level water vapor supply, etc.) that has been studied extensively in atmospheric sciences (so you don't want to dive into its details here as the authors of this section are unlikely to be experts on this topic). The main controls on PET are surface solar radiation, wind speed, near-surface water vapor deficit (or relative humidity) and vegatation type, as represented by the Penman-Montheith equation. For future climate change, the main driver of the drought change is the universal increase in PET caused by the increased vapor deficit, plus decreased precipitation over many subtropical land areas (Zhao and Dai 2015). For historical drought changes, the main driver is preciptiation change, which is still dominated by internal climate variations associated with IPO and other decadal modes (Dai and Zhao 2017). These basic aspects of drought changes are clearly described in Dai et al. (2018). This paragraph misses the key aspects of the main drivers of drought changes. I suggest the authors to read Dai et al. (2018, 10.1007/s40641-018-0101-6) and many key refs cited there. Papers cited: . Dai, A., 2011a: Drought under global warming: A review. Wiley Interdisciplinary Reviews: Climate Change, 2, 45-65. doi:10.1002/wcc.81. Dai, A., 2011b: Characteristics and trends in various forms of the Palmer Drought Severity Index (PDSI) during 1900-2008. J. Geophys. Res., 116, D12115, doi:10.1029/2010JD015541. Dai, A., T. Zhao, and J. Chen, 2018: Climate change and drought: A precipitation and evaporation perspective. Current Climate Change Reports, 4, 301-312. DOI: 10.1007/s40641-018-0101-6. (http://link.springer.com/article/10.1007/s40641-018-0101-6) Dai, A. and T. Zhao, 2017: Uncertainties in historical changes and future projections of drought. Part I: Estimates of historical drought changes. Climatic Change, 144, 519–533. DOI: 10.1007/s10584-016-1705-2. Zhao, T., and A. Dai, 2015: The magnitude and causes of global drought changes in the 21st century under a low-moderate emissions scenario. J. Climate, 28, 4490–4512. doi: http://dx.doi.org/10.1175/JCLI-D-14-00363.1 [Aiguo Dai, United States of America]	Accepted: The CO2 effects of plant WUE have been widely discussed in coordination with Chapters 5 and 8. There is a new Rejected. (CC-Box 5.1) in which the state of the art on this issue is included. There is agreement with Ch. 5 and Ch. 8 on a low confidence on the CO2 fertilizing effects related to a possible WUE. The assessment is not inconsistent with Ch. 8.2.3.1. The fertilizing CO2 overlap with the possible effects on morphological changes (mostly leaf area, root depth, etc.) but also with the radiative CO2 effects, which increase the vegetative growth periods, increases evaporative demand but also affects physiological plant processes (included in the mentioned Rejected.). Also limitations of the models are mentioned in the cross chapter box on the carbon-water nexus since they are also strongly relevant. Assessment of CO2 effects on future projections of hydrological and biophysical droughts are expanded and better clarified in section 11.6.5, including the existing uncertainties related to the possible CO2 fertilizing effects and the need of separate the possible hydrological effects from the plant stress effects under a warmer world. In any case, this relevant aspect has been widely discussed and agreed between the different chapters involved (Ch. 5, 8 and 11).
23211	75	5			Much of 11.6.1 is redundant with very similar assessment carried out in chapter 8. There is a need for improved coordination and arguably chapter 8 should be the lead assessment here and chapter 11 should be repeating their assessment then adding any chapter 11 specific additional material. It is unwise to have the degree of redundancy across chapters that presently exists here. [Peter Thorne, Ireland]	Accepted - An important coordination with Ch. 8 and Ch 12 has allowed to remove inconsistencies and overlaps.
11711	75	7	75	8	for most of the report, it’s “dynamic” and “thermodynamic”, so best to delete the “al” at the end of each of these words here. Same comment for p. 79, line 5. [Amy East, United States of America]	Accepted - reworded in the entire section.
39491	75	8	75	10	Consider to indicate more specifically what type of 'moisure' is referred to here. [Tamara van 't Wout, Qatar]	Rejected - It is generic. It refers to the exchange of moisture, and it can be between the soil and the atmosphere, oceans, between air masses, etc. It basically makes reference to the general moisture balance.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
103851	75	9	75	9	Table 11.3 Miss streamflow drought indices derived using the threshold level method (gives drought duration and deficit volume=severity) [Lena M Tallaksen, Norway]	Accepted - Threshold level methods have been included and cited Van Loon (2015) as an updated reference on this issue
103855	75	10	75	10	Table 11.3 Miss Groundwater drought indices, e.g. the SGI (standardised groundwater index; Bloomfield and Marchant 2013; http://nora.nerc.ac.uk/id/eprint/504254/1/analysis%20of%20groundwater%20drought%20building%20on%20the%20standardised%20precipitation%20index%20approach.pdf [Lena M Tallaksen, Norway]	Accepted - SGI included and reference to Bloomfield and Marchant (2013) included
82051	75	13	75	13	Just the use of 'several again when 'a number' or even 'different' would work here. I won't highlight these any further. [Jamie Hannaford, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - word replaced
29917	75	13	75	14	Maybe "megadroughts" instead of "decadal droughts"? [Juan Rivera, Argentina]	Accepted. The word "megadrought" is also used in the glossary.
82053	75	14	75	15	This sentence sounds like quite a big assertion that should be referenced? I cannot think of papers to the contrary here but I do know of attribution studies done for flood events that do suggest GHG fingerprints on circulation leading to flooding (e.g. Schaller et al. 2014 in ref list) so surely there are some drouight attribution studies that support the converse of this title. Please provide a ref to support this. [Jamie Hannaford, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - reference to Chapter 2 is included and sentence reworded
18385	75	18	76	28	ENSO can greantly affect preciptiation and thus drought on interannual time scales (e.g., Dai and Wigley 2000; Dai 2011; Dai and Zhao 2017); however, it is the IPO (Interdecadal Pacific Oscillation) that can cause decadal preciptiation deficits and thus decadal droughts over the Southwest U.S. (Dai 2013), Eastern Australia, Southern and West Africa, and many other regions (Dong and Dai 2015). I'm supprised to see only ENSO is mentioned in this paragraph. One can think of the IPO as the decadal variations in the ENSO activity, but it is not the same thing as the conventional ENSO. Another mode that can cause multi-decadal drought is the AMO, which caused Sahel drought in the 1970s and 1980s (Dai et al. 2004) and also affects Amazon rainfall and drought (Hua et al. 2019). It seems that most discussions on P, PET and other drivers ignored their projected changes in the 21st century, which are the focus of this chapter (extremes under chaning climate) and have been the subject of many recent studies (see Dai et al. 2018 for relevant papers on model-projected P, PET and soil moisture changes). Suggest add some discussions on how P, PET and soil moisture may change under GHG-induced global warming. Papers cited: Dai, A. and T. M. L. Wigley, 2000: Global patterns of ENSO-induced precipitation. Geophys. Res. Lett., 27, 1283-1286. Dai, A., P. J. Lamb, K. E. Trenberth, M. Hulme, P. D. Jones, and P. Xie, 2004: The recent Sahel drought is real. Intl. J. Climatology, 24, 1323-1331. Dai, A., 2011: Characteristics and trends in various forms of the Palmer Drought Severity Index (PDSI) during 1900-2008. J. Geophys. Res., 116, D12115, doi:10.1029/2010JD015541. Dai, A., 2013: The Influence of the Inter-decadal Pacific Oscillation on U.S. precipitation during 1923-2010. Climate Dynamics, 41: 633-646, doi:10.1007/s00382-012-1446-5. Dai, A. and T. Zhao, 2017: Uncertainties in historical changes and future projections of drought. Part I: Estimates of historical drought changes. Climatic Change, 144, 519–533. DOI: 10.1007/s10584-016-1705-2. Dong, B., and A. Dai, 2015: The influence of the Inter-decadal Pacific Oscillation on temperature and precipitation over the globe. Climate Dynamics, 45, 2667–2681. DOI 10.1007/s00382-015-2500-x. Hua, W., A. Dai, L. Zhou, M. Qin*, and H. Chen, 2019: An externally-forced decadal rainfall seesaw pattern over the Sahel and southeast Amazon. Geophys. Res. Lett., 46, 923-932. https://doi.org/10.1029/2018GL081406. [Aiguo Dai, United States of America]	Accepted - reference to the AMO and IPO have been included
105351	75	20	75	21	Here, there seems to be an assessment which might be correct, but on the face of it, is supported by only one review paper. Presumably the argument that the occurrence of precipitation deficits is largely controlled by variations in circulation (dynamics) needs to be built so that the assessment is well supported. If you are going to say that, then feedback mechanisms presumably have to be understood as amplifying mechanisms, triggered in the first instance, by changes in circulation, rather than as a separate contributor. [Francis Zwiers, Canada]	Taken into account - the assessment based on circulation mechansisms is well developed and referenced in the final document (see assessment in 11.6.1)
37611	75	20	75	21	Precipitation deficit may provide an easily defined metric of drought, but this might not be the best index of damage potential to forest/crop systems. Plants are vulnerable to damage by the combination of high temperature, high ED and water deficit. This needs greater recogniion (see Brodribb et al 2020 Science) [Timothy Brodribb, Australia]	Accepted: The CO2 effects of plant WUE have been widely discussed in coordination with Chapters 5 and 8. There is a new Rejected. (CC-Box 5.1) in which the state of the art on this issue is included. There is agreement with Ch. 5 and Ch. 8 on a low confidence on the CO2 fertilizing effects related to a possible WUE. The assessment is not inconsistent with Ch. 8.2.3.1. The fertilizing CO2 overlap with the possible effects on morphological changes (mostly leaf area, root depth, etc.) but also with the radiative CO2 effects, which increase the vegetative growth periods, increases evaporative demand but also affects physiological plant processes (included in the mentioned Rejected.). Also limitations of the models are mentioned in the cross chapter box on the carbon-water nexus since they are also strongly relevant. Assessment of CO2 effects on future projections of hydrological and biophysical droughts are expanded and better clarified in section 11.6.5, including the existing uncertainties related to the possible CO2 fertilizing effects and the need of separate the possible hydrological effects from the plant stress effects under a warmer world. In any case, this relevant aspect has been widely discussed and agreed between the different chapters involved (Ch. 5, 8 and 11).
8051	75	21	75	23	What is the distinction between "atmospheric dynamics" and "anomalies in the moisture transport"? Is the moisture transport also not affected by the atmospheric dynamics, i.e. the wind field? [jouni Räisänen, Finland]	Rejected: this has been merged with circulation under the term of dynamic.
82055	75	21	75	23	Atmospheric dynamics is a bit of a vague term. I don't really follow this distinction between At. Dyn and moisture transport. Surely the latter can be seen as part of atmospheric dynamics? Overall it seems a bit of an empty sentence and I'm not that sure what it's trying to convey. [Jamie Hannaford, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: this has been merged with circulation under the term of dynamic.
69543	75	21	75	23	I don't understand how "anomalies in the moisture transport" can be separated from "atmospheric dynamics" in terms of contribution to precipitation deficits. The moisture is transported by the atmospheric flow. Similarly, soil-moisture precipitation feedbacks act, at least partially, by altering the atmopsheric flow (i.e., atmospheric dynamics), so I am not sure how one can separate these effects from those of atmospheric dynamics. [Martin Singh, Australia]	Rejected: this has been merged with circulation under the term of dynamic.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
82057	75	21	76	5	Following on from that, I find this whole section misses a key point that all these different scales are not independent and there is considerable nesting. It refers to drivers at the different scales somewhat distinctly but omits to refer to the overlap between all these drivers, and how this also manifests on different timescales (synpotic scale features like anticylones being a proximal influence on a given drought event, while thier time evolution being influenced by much larger scale and circulation itself like the NAO on interannual scales, itself influenced by larger scale atmosphere-ocean features like AMO on decadal scales. The current wording gives a feeling of these drivers being mutually exclusive in some way rather than interdepent manifestations of the same processes on different scales [Jamie Hannaford, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Good point. This has been included in the revised manuscript.
38431	75		75		Table 11.3, Row 6, Col 3: Precipitation Evapotranspiration Index (SPEI) should be the Standardised Precipitation-Evapotranspiration Index (SPEI) and , Standardized should be deleted from the end. [Mansour Almazroui, Saudi Arabia]	Accepted - corrected in the revised document
18371	75		75		Table 11.3: The last row: Dai (2011) describes the various forms of the PDSI and probably should be cited here. . Dai, A., 2011: Characteristics and trends in various forms of the Palmer Drought Severity Index (PDSI) during 1900-2008. J. Geophys. Res., 116, D12115, doi:10.1029/2010JD015541. [Aiguo Dai, United States of America]	Rejected - We are not using references published before 2012 for the entire report. Thus, the more suited reference here (Palmer, 1965) is not cited
101331	75		75		Table 11.3 Miss streamflow drought indices derived using the threshold level method (gives drought duration and deficit volume=severity) [Lena M Tallaksen, Norway]	Accepted - Threshold level methods have been included and cited Van Loon (2015) as an updated reference on this issue
101333	75		75		Table 11.3 Miss Groundwater drought indices, e.g. the SGI (standardised groundwater index; Bloomfield and Marchant 2013; http://nora.nerc.ac.uk/id/eprint/504254/1/analysis%20of%20groundwater%20drought%20building%20on%20the%20standardised%20precipitation%20index%20approach.pdf [Lena M Tallaksen, Norway]	Accepted - SGI included and reference to Bloomfield and Marchant (2013) included
101335	75		75		Suggest to add Stagge et al., 2015 under Combined sythetic measures of drought. The paper is a benchmark paper, well cited, on candidate distributions for the SPI and the SPEI indices; https://rmets-onlinelibrary-wiley-com.ezproxy.uio.no/doi/full/10.1002/joc.4267 [Lena M Tallaksen, Norway]	Accepted - the reference has been included in the table
101337	75		75		Essential to the SPI and SPEI (in particular) is the flexibility of the SPI to account for different time scale by summing precipitation over k months, termed accumulation periods. Accordingly it has been used as proxies for soil moisture and hydrological drought. It is worth mentioning this feature of the indices as it is much used (even though it is not intended for this use as highlighted in the table). [Lena M Tallaksen, Norway]	Accepted - this essential feature has been included
10101	75				Table 11.3, "Combined synthetic measures of drought" row: Should include the recent work by Yang et al. (2020), HESS, https://doi.org/10.5194/hess-2019-701 showing that physiological r_s changes must be accounted for in the evaporative-demand input to PDSI and/or SPEI, in order for those indices to be even qualitatively accurate under climate change conditions. [Jacob Scheff, United States of America]	Accepted .- the reference has been included
10103	75				Table 11.3, "Combined synthetic measures of drought" row: More generally, should cite more of the work suggesting that these measures are inaccurate and/or misleading under climate change despite their success for climate variability, e.g. Swann et al. (2016) and/or Scheff (2018). [Jacob Scheff, United States of America]	Taken into account. The associated limitations with these metrics are considered in Section 11.6. In this table, which is now in the Appendix (11.A.1), only a few general aspects could be listed. The following sentence is also included in the table: "These indices are not intended to be a soil moisture or water-balance proxy"
6881	75				In the table 11.3 Please move "Standardized" before "Precipitation Evapotranspiration Index" [Josep Penuelas, Spain]	Accepted - corrected in the revised document
62559	76	1	76	5	there is an orphaned open parenthesis starting on line 2. In general this sentence is fragmented and hard to read from the multiple digressions and citations. Consider revising [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - parenthesis removed.
62397	76	4	76	4	Only ENSO is mentioned as a global ocean atmosphere coupled pattern that causes precipitation deficits, however there are other large scale drivers such as the Indian Ocean Dipole, which just caused major floods in East Africa and is known to drive droughts over Southern Africa and Australia. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - IOP and AMO have been included
2421	76	5	76	5	It seems to me that the title of this section does not really match the content: subsections 11.6.1.X do not really describe mechanisms so much as the different metrics and variables used to characterize droughts. [Alexis Berg, United States of America]	Noted - The section has been revised to provide more background on mechanisms. The description of the different drought metrics and associated variables also belong in this discussion.
62399	76	7	76	8	ENSO impact on drought is mentioned to be over South Africa, this section can be expanded to include other areas in Southern Africa. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable: This section has been substantially shortened in the final document
23205	76	7	76	11	As the 2019/20 drought in Australia showed the IOD is also important in Australia (also characterised in the chapter 10 case study). [Peter Thorne, Ireland]	Not applicable: This section has been substantially shortened in the final document
23973	76	7			Insert "The" at the start of the sentence. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - inserted.
52657	76	8	76	8	Change South Africa to Southern Africa [Mary-Jane Bopape, South Africa]	Not applicable: This section has been substantially shortened in the final document
113603	76	8	76	9	Please add referene to: Miralles, D. G., van den Berg, M. J., Gash, J. H., Parinussa, R. M., De Jeu, R. A. M., Beck, H. E., Holmes, T. R. H., Jiménez, C., Verhoest, N. E. C., Dorigo, W. A., Teuling, A. J. and Dolman, A. J.: El Niño–La Niña cycle and recent trends in continental evaporation, Nature Climate Change, 4(1), 1–5, doi:10.1038/nclimate2068, 2013. [Diego Miralles, Belgium]	Accepted - reference included
105353	76	9	76	12	There are examples of language here that I don't think are careful enough for an IPCC report. Statement such as "droughts are affected" and "are not SST driven" seem rather categorical – they might be correct, but only if one places very high levels of trust in the studies that are cited. [Francis Zwiers, Canada]	Accepted - the sentences have been carefully rewritten.
82795	76	10	76	10	Australia could be added to the list of places where the IOD has an influence on drought (particularly in light of 2019's events). [Blair Trewin, Australia]	Not applicable: This paragraph has been removed from the final document
23975	76	10			The positive IOD is also relevant to Australia also, e.g. the 2019/2020 wildfires. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable: This paragraph has been removed from the final document
82059	76	11	76	13	I would fundamentaklly disagree that precipitation in Europe is not SST driven! I presume this was meaning more that in these locations precipitation is not ENSO driven (although even that is incorrect, while less prominent ENSO does affect european precip. But as it stands it's wrong, precip in europe very strongly affected by atlantic SST variability on a range of timescales as shown in numerous papers (e.g. Sutton and Dong, 2012, many others). While 'other drivers' are indeed important e.g. the circulation patterns identified by Kingston like NAO, SCA, EA, it's a question again of scale, and at large scales and long timescales SST is fundamental. Further info on SST drivers of European rainfall on a global scale in both Atlantic and Pacific (AMO and PDO) and then effects on drought can be found in Svensson & Hannaford (2019) and references therein. https://iopscience.iop.org/article/10.1088/2515-7620/ab42f7 [Jamie Hannaford, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable: This paragraph has been removed from the final document

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
62387	76	11	76	13	"Precpitation in other regions ….. Canada and Middle East are not SST-driven ….". I just like to draw authors attention to that although there is not much literature available for Middle East, but in recent time few studies are conducted over the region. For e.g., Abid et al. (2018) highlight a prominent linkages between ENSO and the Precipitation deficit over the region during summer season using observations as well as model. In Middle East, the summer rainfall mainly happens over the southwestern Arabian Peninsula. The study says that warming in ENSO region drives below normal rainfall over the region (i.e. negative relationship between ENSO and regional rainfall) that may favor drought conditons over the region. This study was picked up by media as highlight story. I hope that the suggested study will provide authors some hint for the MiddleEast drougt conditions driven by SSTs from ENSO. Following is the complete reference of the article: " 1- ENSO relationship to summer rainfall variability and its potential predictability over Arabian Peninsula region; Abid et al. (2018); npj Climate and Atmospheric Science 1, 20171; https://doi.org/10.1038/s41612-017-0003-7". There are some others also which establishes ENSO positive relationship with MiddleEast/Arabian region during wet (Nov-Apr) including winter (Dec-Feb) season. This means that La Nina may favor drought conditions over the region. They are following: 2- Extreme precipitation events over Saudi Arabia during the wet season and their associated teleconnections; Atif et al. (2020); Atmospheric research; https://doi.org/10.1016/j.atmosres.2019.104655 3- Abid, M.A., Kucharski, F., Almazroui, M., Kang, I.S., 2016. Interannual rainfall variability and ECMWF-Sys4-based predictability over the Arabian Peninsula winter monsoon region. Q. J. R. Meteorol. Soc. 142, 233–242. https://doi.org/10.1002/qj.2648 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable: This paragraph has been removed from the final document
76857	76	13	76	15	Worth mentioning about the record dry spell over Singapore–Malaysia in 2014 that was attributed to the southward contraction of the intertropical convergence zone in the paper: Mcbride, J. l., Sahany, S., Hassim, M. E. E., Nguyen, C. M., Lim, S.-Y., Rahmat, R., et al. (2015). The 2014 Record Dry Spell at Singapore: An Intertropical Convergence Zone (ITCZ) Drought. Bull. Am. Meteorol. Soc. 96, S126–S130. doi:10.1175/BAMS-D-15-00117.1. [Sandeep Sahany, Singapore]	Not applicable: This paragraph has been removed from the final document
23207	76	13	76	18	Chapters 2 and 3 carried out a substantive analysis of this issue which differs from this characterisation. This text should be removed and replaced with a reference to the substantive assessments performed in earlier chapters. [Peter Thorne, Ireland]	Not applicable: This paragraph has been removed from the final document
39315	76	15	76	17	There is "low confidence" in a finding with only one study cited? [Lourdes Tibig, Philippines]	Not applicable: This paragraph has been removed from the final document
45687	76	17	76	18	The precipitation changes expected for the late 21st century are complex due to the large spread ex-hibited by the future projections, ==> The projections for the precipitation changes in the late 21st century exhibit a large spread, [Christophe Deissenberg, Luxembourg]	Not applicable: This paragraph has been removed from the final document
105355	76	17	76	19	I think this needs to be discussed much more carefully and deliberately, because I think this kind of thing is core to what is being discussed. It is, presumably very important to highlight (a) that there is low confidence that a forced signal can be seen in precipitation deficit data in the face of large, circulation dominated, natural variability and (b) there is low confidence that long-term changes in circulation (which is distinct from the internal variability on decadal and shorter scales that produces most of the noise) are responsible for trends in precipitation deficits. This means, that if trends are detected, we have to ask whether they are due to other causes – limitations in the data would presumably be the number one candidate. [Francis Zwiers, Canada]	Not applicable: This paragraph has been removed from the final document
88179	76	18	76	19	It is unclear where this conclusion is made in chapter 2 - section 2.3.1.4.2? [Sharon Smith, Canada]	Not applicable: This paragraph has been removed from the final document
23209	76	18	76	21	This is assessed in chapter 8 and not chapter 2. The assessment in chapter 8 should be reflected here. [Peter Thorne, Ireland]	Not applicable: This paragraph has been removed from the final document
105357	76	22	76	23	Since the sub-subsection is about precipitation deficits, what can be said with confidence, or lack of confidence, about the role of feedbacks in amplifying precipitation deficits (the sentence talks about precipitation …). [Francis Zwiers, Canada]	Not applicable: This paragraph has been removed from the final document
125987	76	24	76	24	Include citations: Santanello, J A., P A Dirmeyer, C Ferguson, and Kirsten L Findell, et al., June 2018: Land-Atmosphere Interactions: The LoCo Perspective. Bulletin of the American Meteorological Society, 99(6), DOI:10.1175/BAMS-D-17-0001.1. Findell, Kirsten L., P Gentine, B R Lintner, and Christopher Kerr, June 2011: Probability of afternoon precipitation in eastern United States and Mexico enhanced by high evaporation. Nature Geoscience, 4(7), DOI:10.1038/ngeo1174. [Trigg Talley, United States of America]	Not applicable: This paragraph has been removed from the final document
2423	76	27	76	28	However, in other regions, models show uncertainties in the sign of the soil moisture-precipitation feedback (e.g., Berg et al. 2017: Berg, A., B. Lintner, K. Findell, A. Giannini (2017), Uncertain soil moisture feedbacks in model projections of Sahel precipitation, Geophysical Research Letters, 44, 6124-6133.) [Alexis Berg, United States of America]	Accepted - A sentence has been included and the suggested reference added.
125989	76	31	77	8	It might be worthwhilie mentioning that, while the dependence of potential evapotranspiration on atmospheric temperature can partly be explained descriptively by changes in VPD under constant humidity, there is now a somewhat more holistic, approximate theory for how PET itself depends on temperature (https://doi.org/10.1002/2017WR021970). [Trigg Talley, United States of America]	Noted - This discussion would be somewhat too detailed given the space limitations.
18377	76	31	77	8	First, suggest to use PET, not Epot, for potential evapotranspiration to follow the convention. Second increased PET under rising temperature is the key driver of increased ardity under global warming over most land areas, as discussed in Dai et al. (2018). Some of the key refs. on future PET changes and how it may affect drought are listed below but see Dai et al. (2018,) for more refs. Feng S, Fu Q (2013) Expansion of global dry lands under warming climate. Atmos Chem Phys 13: 10081–10094. Fu Q and Feng S (2014) Responses of terrestrial aridity to global warming. J. Geophys. Res. Atmos. 119: 7863-7875. Scheff J, Frierson DMW (2014) Scaling potential evapotranspiration with greenhouse warming. J Clim 27: 1539–1558, doi:10.1175/JCLI-D-13-00233.1. Scheff J, Frierson DMW (2015) Terrestrial aridity and its response to greenhouse warming across CMIP5 climate models. J Clim 28: 5583–5600. Zhao T, Dai A (2015) The magnitude and causes of global drought changes in the 21stcentury under a low–low-moderate emissions scenario. J Clim 28: 4490–4512 Zhao T, Dai A (2017) Uncertainties in historical changes and future projections of drought. Part II: model-simulated historical and future drought changes. Clim Change, 144: 535-548. https://doi.org/10.1007/s10584-016-1742-x [Aiguo Dai, United States of America]	Accepted - the description of the conceptual issues related to the atmospheric evaporative demand has been improved.
20259	76	33	76	33	It seems that most people use the acronym PE rather than Epot [philippe waldteufel, France]	Rejected: We have used the term atmospheric evaporative demand across the section
62561	76	33	76	33	Please change the pot in Epot to a subscript, and throughout section [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected: We have used the term atmospheric evaporative demand across the section
113605	76	33	76	34	Potential evaporation is not open water evaporation. Please refer to maes et al. (2019) and revise. It is exactly the same as what you call 'potentia evapotranspiration'. Maes, W. H., Gentine, P., Verhoest, N. E. C. and Miralles, D. G.: Potential evaporation at eddy-covariance sites across the globe, Hydrol. Earth Syst. Sci., 23(2), 925–948, doi:10.5194/hess-23-925-2019, 2019. See this maybe too: https://www.essoar.org/doi/pdf/10.1002/essoar.10503229.1 [Diego Miralles, Belgium]	Accepted - the description of the conceptual issues related to the atmospheric evaporative demand has been improved

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
105359	76	33	76	38	This opening paragraph could do with some careful editing – it's pretty difficult to read. Also, is the discussion about potential evaporation, or potential evapotranspiration? If these are the same, it would be useful to briefly mention that. [Francis Zwiers, Canada]	Accepted - the description of the conceptual issues related to the atmospheric evaporative demand has been improved
29919	76	33	76	44	This part deals more with definitions than with assessment. Given that the chapter lenght is above the estimated limit, I would suggest to check if some of the text can be removed given that some definitions are in the glossary. [Juan Rivera, Argentina]	Rejected - this is a complex issues that does not only refers to terminology and it is necessary to provide a good description. In any case, the description of the atmospheric evaporative demand issues has been improved.
2425	76	38	76	38	For clarity, I would recommend mentioning how Epot or PET is calculated (e.g., the Penman-Monteith equation), so that the issue of the treatment of stomatal conductance in calculations of future Epot (or PET) trends can be more easily introduced later in the section. [Alexis Berg, United States of America]	Accepted - this has been clarified in the revised document
125991	76	41	76	41	Include citation: Milly, P C., and Krista A Dunne, August 2017: A Hydrologic Drying Bias in Water-Resource Impact Analyses of Anthropogenic Climate Change. Journal of the American Water Resources Association, 53(4), DOI:10.1111/1752-1688.12538. [Trigg Talley, United States of America]	Not applicable. The section has been substantially rewritten.
20261	76	41	76	43	This sentence is unnecessarily clumsy. Also, on line 42, change to "different from" [philippe waldteufel, France]	Accepted - the sentence has been rewritten
70365	76	45	76	46	"An increase in Epot does not necessarily lead to increased ET (Milly and Dunne, 2016), since if soil moisture is limited, soil evaporation and/or plant transpiration cannot supply the atmospheric demand (Box 11.1)." This list is incomplete. Increased Epot does not lead to increased ET when plants regulate their stomata under higher CO2 conditions. Plants can transpire less because of water savings from stomatal closure under increased CO2, not just because soil moisture is limited. This stomatal regulation has been shown to be of first order importance causing actual ET to decouple from Epot in ESMs (Swann et al. 2016). We suggest the following wording: "An increase in Epot does not necessarily lead to increased ET (Milly and Dunne, 2016), since if soil moisture is limited or plants reduce stomatal conductance under high CO2, soil evaporation and/or plant transpiration do not supply the atmospheric demand (Box 11.1)." [Abigail Swann, United States of America]	Accepted - reference to AED and ET issues has been improved in sections 11.6.2 and 11.6.5.
37613	76	45	76	49	This statement introduces the potential impact of ED + soil drought, but without the context of why plants die it is hard to understand the important climatic combinations that drive vegetation damage. Such an explanation would show that plant species die when they reach critical tissue water deficits, and that timing of this is determined by the combination of soil deficit, ED and leaf surface porosity (this latter term being sensitive to temperature due to denaturation of leaf surface wax at high temperature). [Timothy Brodribb, Australia]	Rejected: This issue has been discussed in the Rejected. on water-carbon nexus (that includes mention on the effects of heat on wax porosity) and also more discussion is included in section 11.6.5.
20263	76	46	76	49	This sentence has a problem. Should one delete the "if" on line 48? This removes the problem but perhaps it is not the correct solution. [philippe waldteufel, France]	Accepted - the sentence has been rewritten.
62401	76	46	76	49	Perhaps missing statement in the red highlighted part of the sentence? "Nevertheless, under low soil moisture conditions, it strongly contributes to agricultural/ecological drought impacts (Anderegg et al., 2013, 2016; Williams et al., 2013), and if it leads to ET changes, during periods of drought deficits, also to hydrological droughts (Seneviratne et al., 2012a; Teuling et al., 2013)." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - the sentence has been rewritten.
10105	76	46			I'm not quite sure why Box 11.1 is cited here? This topic is mentioned very briefly in Box 11.1, but not as a main topic or purpose. [Jacob Scheff, United States of America]	Accepted - This has been detailed in the revised document given influence on land atmosphere feedbacks
82061	76	52	76	52	I think 'Thermodynamic procedsses also play a fundamental role' rather undersells this. It should have some references. Some would maybe challenge as to whether thermodynamic T plays a more fundamental role on Epot than circulation? [Jamie Hannaford, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - two sentences merged and two references added.
10107	76	52	76	54	This sentence could use a citation, e.g. Scheff and Frierson (2014), J Climate, https://doi.org/10.1175/JCLI-D-13-00233.1 [Note this is not the same paper as Scheff and Frierson (2015) which is already in the References section.] [Jacob Scheff, United States of America]	Not applicable - this section has been substantially revised and rewritten
101339	76	54	76	54	Increasing temperatures have increase the vapour holding capacity of the atmosphere; whether or not it has increased Epot depends on the joint change in humidity. [Lena M Tallaksen, Norway]	Noted. However, the text mentions "in the absence of other influence". If atmospheric moisture content does not change, VPD increases with increasing temperature.
2427	77	1	77	3	The two parts of the sentence don't really fit together and the sentence is a bit backwards, in my view. I think it's fairer to say, VPD increase over land because the saturation pressure water vapor increases faster than the actual vapor pressure. The ocean moisture-advection mechanism is the primary factor explaining that, and should be cited first. Then, land atmosphere feedbacks also play a role. Thus, I suggest editing with something along the lines of " Increased atmospheric CO2 concentrations have warmed the atmosphere and in the absence of other influences, this increases Epot by means of enhanced VPD. Indeed, because of the greater warming over land than oceans, the saturation pressure of water vapor increases more over land than oceans; oceanic air masses advected over land thus contain insufficient water vapour to keep pace with the greater increase in saturation vapour pressure over land (Sherwood and Fu, 2014; Byrne and O'Gorman, 2018). Land-atmosphere feedbacks are also important in affecting near-surface atmospheric moisture content and temperature, thus affecting relative humidity and VPD (Seneviratne et al. 2010; Berg et al. 2016; Haslinger et al. 2019; Zhou et al. 2019; Box 11.1)". [Alexis Berg, United States of America]	Accepted - This modification has been included in the revised document.
125993	77	4	77	4	Include citation: Findell, Kirsten L., P W Keys, R J van der Ent, B R Lintner, Alexis Berg, and John P Krasting, November 2019: Rising Temperatures Increase Importance of Oceanic Evaporation as a Source for Continental Precipitation. Journal of Climate, 32(22), DOI:10.1175/JCLI-D-19-0145.1. [Trigg Talley, United States of America]	Accepted: Proposed reference is cited
70367	77	4	77	8	the effect of stomatal responses to high CO2 is to reduce evapotranspiration, thus the word "increased" should be "decreased". [Abigail Swann, United States of America]	Accepted - this was an error in the text that has been corrected
113607	77	5	77	5	Why is now potential evaporation referred different and even with quotations. In general: try to stick to te langguage in Serrano et al. (2020). That paper gets the terminology as straight as possible, bearing in mind how confusing this topic can get. [Diego Miralles, Belgium]	Accepted - sentence has been rewritten.
101341	77	6	77	6	Is it correct that high CO2 concentration may lead to increased ET, is it not rather the opposite, that the stomata do not need to open so long to get the same amount of CO2, thus there will be less ET? Ref. statement later on the same page, line 33: CO2 effect on plant water savings and line 44 on reduced water needs by plants. [Lena M Tallaksen, Norway]	Accepted - this was an error in the text that has been corrected
2429	77	6	77	8	On the contrary, as the studies cited in this paragraph show, higher atmospheric CO2 may limit the increase in potential evapotranspiration, as increasing stomatal resistance limits plant transpiration (even if total leaf biomass increases, because of CO2 fertilization). The point of the Milly and Dunne 2016 study cited here is, precisely, that because of this CO2 effect (primarily, but not only), future trends in potential evapotranspiration are actually best estimated simply from net radiation. [Alexis Berg, United States of America]	Accepted - this was an error in the text that has been corrected
10111	77	6	77	8	More generally, this discussion should also mention that increased leaf growth under high CO2 could lead to the opposite effects, canceling some of what you discuss here. Relevant studies: Mankin et al. (2019), Nature Geoscience, https://doi.org/10.1038/s41561-019-0480-x and Mankin et al. (2018), GRL, https://doi.org/10.1002/2018GL077051 . [Jacob Scheff, United States of America]	Accepted - Reference to the Rejected. on carbon-water nexus (CC-Box 5.1) has been included in which discussion on the other physiological CO2 effects is provided.

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37601	77	6	77	8	high CO2 tends to decrease (not increase) stomatal conductance in the short and long term, thereby potentially reducing E. However, high temperature directly increases leaf porosity to water vapor and can thus produce an (unpredictable) increse in transpiration above the expected response based on VPD. [Timothy Brodribb, Australia]	Accepted - this is issue has been corrected and reference to the Rejected. on the carbon-water nexus is included, in which the heat effects on wax porosity are mentioned.
4209	77	6			I thought rising CO2 led to a very small reduciton (10%?) in Et, not an increase. Please check for accuracy. [Nate McDowell, United States of America]	Accepted - this was an error in the text that has been corrected
10109	77	6			I assume "increased" on this line was supposed to be "decreased" ? At least, that is the thrust of most or all of the cited references. [Jacob Scheff, United States of America]	Accepted - this was an error in the text that has been corrected
101611	77	7	77	7	Epot depends on atmoshperic resistence (determined among other by surface/vegetation roughness), whereas actual ET depends on surface (stomata) resistence in addition. [Lena M Tallaksen, Norway]	Rejected - this sentence is mostly focused on ET
6883	77	7			Here it is not clear that one can expect increased evapotranspiration under increasing atmosperic CO2 concentrations because of their effect on stomatal coductance. In fact, elevatd CO2 decreases stomatal ocnducatnce and increases water use efficencey, so may be the authors base their statement in the expected increase in plant biomass resulting from the expected fertilitzation effect, but, if this is the case, this needs to be clearly explained and supported [Josep Penuelas, Spain]	Accepted - this was an error in the text that has been corrected
105361	77	9	77	9	In common with many such sub-subsections, an assessment that is supported by the evidence that is reviewed is not given. The authors shouldn't leave the task of making an assessment to the reader, and also, they shouldn't separate the assessment from the presentation of the evidence by providing it only in the summary paragraph at the end of the entire subsection. [Francis Zwiers, Canada]	Rejected - this has been responded in other comment. Assessment is provided at the end of each of the main sections.
113609	77	11	77	11	Do not forget to mention VPD stress induced on stomata. That is going up with global warming. [Diego Miralles, Belgium]	Accepted- This is now mentioned in 11.6.1.2
15159	77	11			The influence of climate change on snowpack is well-known and while there is some reference to snowpack influences on hydrologic drought, I would argue that the impacts on soil moisture drought are much stronger and more direct in many systems. For example, Gergel et al. (2017) show distinct declines in dry season soil moisture in montane regions where snow provides a substantial reservoir. Likewise, Williams et al. (2020) show that the magitude of anthropogenic climate change on summer soil moisture to date is highest in montane snow dependent watersheds of the western US. Gergel, D.R., Nijssen, B., Abatzoglou, J.T., Lettenmaier, D.P. and Stumbaugh, M.R., 2017. Effects of climate change on snowpack and fire potential in the western USA. Climatic Change, 141(2), pp.287-299. Williams A P, Cook E R, Smerdon J E, Cook B I, Abatzoglou J T, Bolles K, Baek S H, Badger A M and Livneh B 2020 Large contribution from anthropogenic warming to an emerging North American megadrought Science 368 314 LP – 318 [John Abatzoglou, United States of America]	Accepted - reference to the snow importance and references have been included.
105741	77	13	77	14	Justin Sheffield; Eric Wood; Nathaniel Chaney; Kaiyu Guan; Sara Sadri; Xing Yuan; Luke Olang; Abou Amani; Abdou Ali; Siegfried Demuth; Laban Ogallo, 2013: A Drought Monitoring and Forecasting System for Sub-Sahara African Water Resources and Food Security, BAMS-D-12-00124 [Abou Amani, France]	Rejected - This is an article focusing on a particular region. The citations included are more general.
107415	77	13	77	29	You have to know that soil caracteristics and depth play a substantial role impact on soil moisture in arid and semi-arid regions. Furthermore, grazing that has a negative effect on soil structure can affect soil moisture. [Rachda Berrached, Algeria]	Rejected - this section has been substantially revised and shortened
107417	77	13	77	29	Wind speed and frequency can also increase soil moisture deficit. [Rachda Berrached, Algeria]	Rejected - Wind affects indirectly given its role on Atmospheric evaporative demand so this is included.
82063	77	13	77	45	This whole section gives a lot of emphasis on vegetation and the CO2 effects. The role of land surface feedbacks in general is given a few lines at 19 - 21. I'm no expert in this area but I'm aware of whole review articles (e.g. by Sonia Seniveratne) on the state of knoweldge on land surface feedbacks on drought, including soil moisture, so I'm surprused at this balance. [Jamie Hannaford, United Kingdom (of Great Britain and Northern Ireland)]	Noted - The mentioned article is cited in this paragraph. However, this paragraph is about soil moisture-related droughts rather than land-atmosphere feedbacks.
10113	77	17			Section 11.1.1.3 does not exist, I have no idea what section you are actually trying to cite here. This must be corrected. [Jacob Scheff, United States of America]	Accepted - this has been replaced by a reference
101343	77	23	77	24	Not sure I understand the sentence "Thus, vegetation cover can be ... [Lena M Tallaksen, Norway]	Accepted - sentence has been rewritten
18373	77	23	77	29	The criticisms on the PDSI in these lines are unfair because all the other drought indices (including SPEI) listed in your Table 11.3 are even worse in these areas than the PDSI. At the least, the Palmer model attempts to account for the water supply and demand near the surface using a two-layer bucket model, while all the other indices do not even make such attempts at all! ET/PET or P based indices only look at the demand or supply side, not even considering the surface water balance. Yet, they are not criticized for not considering the water balance, but the PDSI is criticized for trying to do that in a simple way. In contrast, the SPEI has all the problems of the PDSI (e.g, they both do not expliictly consider land cover types such snow, forest, grassland, and cropland, and it is not based on any water balance model, etc), yet there are no criticisms at all on the SPEI. These are very biased statements! The PDSI, although not perfect, is a better metric than the others listed in Table 11.3 for quantifying long-term aridity or drought changes in response to GHG forcing (the main purpose of this chapter and the all the IPCC reports), as it accounts for both the impacts from PET and P changes. The time scale is not an issue for the long-term change; it would be a deficiency for quantifying rapidly changing drought such as flash drought, but that is not the main impact of GHG-induced climate change. The statement "Its main limitation is that it is not based on a robust water balance model as it oversimplifies soil surface hydrological processes (SREX Chapter 3)" applies to all drought indices (including SPEI), and it misses the key point that as an index by design it must be calculated using simplified equations and limited input data! Otherwise, you will need a comprehensive climate model or land surface model to have a robust water balance model, and it becomes impractical for any index calculations. People who made such criticisms lack the basic understanding of a drought index. Thus, I strongly recommend to delete this paragraph on the deficiencies of the PDSI, which have already been discussed extensively in previous studies (e.g., Dai 2011, JGR, doi:10.1029/2010JD015541). [Aiguo Dai, United States of America]	Taken into account: The assessment of the PDSI, observational trends and future projections based on this index has been revised and suggested points by the reviewer have been taken into account in the revised document.
10115	77	25	77	26	Should add "under climate change" to the phrase about the Lian et al. (2020) study, otherwise the reader would not realize that Lian et al. are talking about anthropogenic climate change effect here (as opposed to just a general geographical statement like the rest of this paragraph.) [Jacob Scheff, United States of America]	Accepted - the sentence has been included
2431	77	27	77	27	I believe this statement about boundary layer feedback should be unpacked: what feedbacks, with what role? [Alexis Berg, United States of America]	Accepted - further explanation is provided for the feedbacks.
2433	77	27	77	27	"limitation" (no s). [Alexis Berg, United States of America]	Accepted - replaced in the revised document.
11713	77	27	77	27	should be "limitations are" [Amy East, United States of America]	Rejected - corrected according to comment 2433.
70369	77	31	77	32	section 11.1.1.2 does not appear to exist, and we aren't sure which section the authors are intending to refer to. [Abigail Swann, United States of America]	Accepted - It should be 11.6.1.2
105363	77	31	77	45	While this material is interesting, is it material to the scope of this chapter? The objective here is presumably to assess our knowledge of processes involved in producing soil-moisture deficits. Maybe the point is to say that the ability of vegetation to mitigate soil-moisture deficits in an enhanced CO2 climate isn't what we might imagine given the generally accepted notion that water use efficiency is increased with higher CO2 – if that's the idea, start in that way. [Francis Zwiers, Canada]	Accepted - This paragraph has been substantially simplified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70371	77	31	77	45	This paragraph discusses the limitations of plant responses to CO2 in eliminating drought stress, summarizing that CO2 effects would reduce water needs under non-extreme drought conditions. This assessment of the role of CO2 ignores how plant water savings during non-drought, or non-extreme drought conditions actually alters the onset and occurrence of drought by reducing evapotranspiration before extreme drought occurs (Lemordant et al. 2016), and this point should be included. [Abigail Swann, United States of America]	Taken into account: There is a new Rejected. (5.1) related to the carbon-water nexus providing more background on this topic.
10117	77	31			"affect" should be "decrease" for maximum clarity. [Jacob Scheff, United States of America]	Rejected - this section has been substantially revised and rewritten
10119	77	32			Same as line 17, I cannot tell what "11.1.1.2" was supposed to refer to. The copy editor will not know either... [Jacob Scheff, United States of America]	Accepted - It should be 11.6.1.2
4211	77	33			There is far more evidence, from the field and from most biomes now, that CO2 does not benefit most ecosystems. Would you like me to provide those references? [Nate McDowell, United States of America]	Accepted - there is a new Rejected. related to the carbon-water nexus that records this issue.
4213	77	33			I see a parrallelism issue. The section on Epot above did not cover impacts on ecosystems, but this section here does cover impacts on plants. I would suggest a short paragraph on the very negative impacts that rising VPD has on plants (Grossiord et al. 2020 and many other citations such as Williams et al. 2014 NCC and McDowell et al. 2016 NCC). If you want I could draft this short paragraph very easily and share with you for consideration. [Nate McDowell, United States of America]	Accepted - Nevertheless, this is mentioned in the previous section on the atmospheric evaporative demand.
79123	77	34	77	34	reference needed. On line 41, no space gap [Andong Shi, Sweden]	Accepted - relevant references are moved to this sentence.
2435	77	35	77	37	repeated sentence [Alexis Berg, United States of America]	Accepted. Removed in the revised document
62439	77	36	77	36	Xu, Jiang, Jia, & Zhou, 2016). Page 77 line 36 is missing in reference list [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - the reference has been included
62441	77	38	77	38	Duan et al., 2014, 2015), Page 77 line 38 is missing in reference list [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - the references have been included
37603	77	39	77	39	Duan references not listed [Timothy Brodribb, Australia]	Accepted - the references have been included
62443	77	40	77	40	(Nackley et al., 2018) Page 77 line 40 is missing in reference list [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - the references have been included
62563	77	40	77	41	This citation is not in the references: Dikšaitytė, Viršilė, Žaltauskaitė, Januškaitienė, & Juozapaitienė, 2019, (also truncate to Dikšaitytė et al., 2019) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - the references have been included
2437	77	40	77	45	I think the issue at hand should be presented more explicitly, that is: how are soil moisture deficits affected by physiological CO2 effects? I.e., how are CO2 induced changes in ET affecting soil moisture levels at different time scale (e.g., are there water savings from reduced transpiration? See for instance Lemordant et al. 2016 (cited in the report)). A separate issue can then be mentioned, that is, for a given soil moisture anomaly (or, rather, a precipitation anomaly), does CO2's physiological effect limit the impact on plant photosynthesis and ET? [Alexis Berg, United States of America]	Taken into account. An assessment of CO2 impacts has been addressed in 11.6.5
10121	77	41	77	43	Another study providing this type of more nuanced perspective on CO2-driven reductions in crop water requirements, is Deryng et al. (2016), Nature Climate Change, doi.org/10.1038/NCLIMATE2995 . [Jacob Scheff, United States of America]	Accepted - this reference has been included
37607	77	43	77	44	Most would argue that there is low confidence for a meaningful CO2 benefit under any "drought" scenario. Once soils reach any level of significant water deficit, water conservation rather than photosynthesis is prioritized by plants and hence high CO2 has a limited effect under these circumstances (eg Birami et al New Phytologist 226:1607-1621) [Timothy Brodribb, Australia]	Noted. However, in some regions, drought conditions (water deficits) may not necessarily be associated with plant water stress, which is why the text refers to "extreme droughts". The main assessment on this topic is now under the Rejected. 5.1. See text elevated to chapter 11 ES: "There is low confidence that effects of enhanced atmospheric CO2 concentrations on plant water-use efficiency alleviate extreme agricultural and ecological droughts in conditions characterized by limited soil moisture and enhanced AED. There is also low confidence that these effects will substantially reduce global plant transpiration and the severity of hydrological droughts. ".
20749	77	43	77	45	We do not know the difference between "non-extreme" and "very extreme" drought conditions. To be frank, we are not completely sure either about the definition of "drought conditions". All this does nothing to strengthen trust into the present "assessment", whatever it is. [philippe waldteufel, France]	Accepted - the sentence has been reworded in order to provide more clarity.
105365	77	43	77	45	This assessment is a bit complicated because it appears the general link between CO2 concentrations and water use efficiency is being assessed with medium confidence, and then an exception is pointed out (but not assessed). So, two questions come to mind. First, are the authors comfortable making the first assessment? This seems a pretty big question and has probably been addressed multiple times in previous IPCC reports. Second, if you were going to assess the second statement – how would you express that statement, and what would the assessment be? [Francis Zwiers, Canada]	Accepted - the sentence has been removed.
37605	77	44	77	45	The ameliorating effects of CO2 are certainly not evident under soil drought, but more importantly, the associated temperature increase has highly detrimental effects (see Brodribb, Powers, Cochard and Choat, Science 2020). [Timothy Brodribb, Australia]	Accepted - this is recorded in the cross chapter box of the carbon-water nexus
104495	77	48	77	48	Miss commonly used streamflow drought indices based on the threshold level method. It is not sure what type of hydrological drought are referred to in this section, and findings depend to a large degree on the indices chosen. Also whether one uses normalised (or standardised) indices or absolute indices. [Lena M Tallaksen, Norway]	Accepted - we have detailed that hydrological droughts may depend of the hydrological system analysed.
2439	77	48	78	10	This sub-section should mention and discuss the role of snow and glaciers processes in some regions, too - i.e., providing water during low flow periods. For instance, warming affects snowpack levels, as well as the timing of snow melt, thus potentially the seasonality and/or levels of streamflow and low flows. There is a large literature on the topic, for instance, for the Western USA, see Barnhart et al. 2016 and references therein: Barnhart, T. B., Molotch, N. P., Livneh, B., Harpold, A. A., Knowles, J. F., & Schneider, D. (2016). Snowmelt rate dictates streamflow. Geophysical Research Letters, 43(15), 8006-8016. [Alexis Berg, United States of America]	Accepted - mention to snow issues and references added
82065	77	48	78	10	The hydrological drought section is quite weak in my opinion and has quite a lot of emphasis on anthropogenic effects but neglects to cover some of the more basic issues around the complexity of drought propagation and why it is not straightforward to translate from changes in meteorological to hydrological drought, even in more natural settings (many papers from Anne Van Loon on this). I think in general hydro droughts should be elevated in importance. For many people on earth, and for many ecosystems, hydrological drought will be the type of drought that brings substantial impacts in a warming world given the importance of rivers and groundwater to society and the environment. Also, as with Table 1, this section does not do justice to groundwater. It is seen a rather secondary driver of river flow droughts. But groundwater is a crucial part of water supply across vasy areas of the world, directly through aquifer extraction rather than rivers, so arguably groundwater drought should be covered in more detail in and of itself. [Jamie Hannaford, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - mention to complexity of propagation of hydrological droughts as well as hydrogeology have been mentioned and references added
101347	77	48	78	10	Miss commonly used streamflow drought indices based on the threshold level method. It is not sure what type of hydrological drought are referred to in this section, and findings depend to a large degree on the indices chosen. Also whether one uses normalised (or standardised) indices or absolute indices. [Lena M Tallaksen, Norway]	Accepted - we have detailed that hydrological droughts may depend of the hydrological system analysed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45555	77	50	77	53	Caillouet et al. (2017) proposed recently and for the first time a methodology to identify and quantify characteristics (duration, severity) of low-flow events through space and time. Caillouet, L., Vidal, J.-P., Sauquet, E., Devers, A., Graff, B. (2017) Ensemble reconstruction of spatio-temporal extreme low-flow events in France since 1871. Hydrology and Earth System Sciences, 21, 2923–2951, https://doi.org/10.5194/hess-21-2923-2017 [Jean-Philippe Vidal, France]	Accepted - the reference has been included
104493	77	54	77	54	Do not understand 'On the other hand' in this context. [Lena M Tallaksen, Norway]	Accepted - the sentence has been rewritten.
101345	77	54			Do not understand 'On the other hand' in this context. [Lena M Tallaksen, Norway]	Accepted - the sentence has been rewritten.
82067	78	1	78	10	This coverage on human drivers is all good. It would perhaps be worth a mention of how challenging it is given the very imperfect state of our knowledge and data on human impacts on river flow, and hyd droughts. These are nearly all modelling studies using coarse scale datsets of extractions/impoundments which are very uncartain (no doubt why different studies give different results [Jamie Hannaford, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - this issue has been included in the document.
43391	78	1		3	Read "(water management and demand, damming and land use changes (Van Loon et al., 2016; He et al., 2017; Veldkamp et al., 2017; Wu et al., 2018a; Xu et al., 2019b)). " rather than "(water management and demand, damming and land use changes (Van Loon et al., 2016; He et al., 2017; Veldkamp et al., 2017; Wu et al., 2018a; Xu et al., 2019b). " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted - sentence reworded
82797	78	4	78	4	How is the magnitude of hydrological droughts defined in this context? [Blair Trewin, Australia]	Not applicable- this section has been substantially revised and rewritten
74555	78	5	78	5	to correcte suggesded by suggested [Moulay Driss HASNAOUI, Morocco]	Accepted - corrected in the revised document
11715	78	5	78	5	fix spelling of “suggested” [Amy East, United States of America]	Accepted - corrected in the revised document
62445	78	7	78	7	Vicente-Serrano et al., 2017, 2019c both are missing line 7 page 78 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - references are included
62447	78	7	78	7	Otto et al 2015 missing in reference list page 78 line 7 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - references are included
74557	78	8	78	8	Gudmundsson et al., submitted To check if it isn't published [Moulay Driss HASNAOUI, Morocco]	taken into account - The article was accepted for publication before the cutoff date and is included.
71523	78	8	78	8	I do not think this statement may be supported by the results of the study. I suggest to rewrite by: "Gudmundsson et al. (submitted) based on the latest version of the ISIMIP multi-model experiment suggests that the contribution of human water use is smaller than that of anthropogenic climate change to explain spatial differences in the trends of low flows". At least in the Mediterranean this cannot be supported: Vicente-Serrano, S.M., et al. (2019) Climate, irrigation and land-cover change explain streamflow trends in Western Europe. Geophysical Research Letters, 46, 10,821–10,833. [Sergio Vicente-Serrano, Spain]	Taken into account - the text has been revised.
103879	78	10	78	11	Miss groundwater drought indices, ref. comment [9] [Lena M Tallaksen, Norway]	Accepted - Groundwater indices included in table (now Table 11.A.1) and mentioned in the first sentence of the hydrological drought section.
105367	78	11	78	11	In common with many such sub-subsections, an assessment that is supported by the evidence that is reviewed is not given. The authors shouldn’t leave the task of making an assessment to the reader, and also, they shouldn’t separate the assessment from the presentation of the evidence by providing it only in the summary paragraph at the end of the entire subsection. (I won’t repeat this comment again – I repeated here just to reiterate that this comment applies to many sub-subsections). [Francis Zwiers, Canada]	Rejected - as responded above, the assessment related to confidence levels is provided at the end of each section. Nevertheless, a summary paragraph is also included in each section
104499	78	13	78	13	The section on comined indices is focused on the SPEI index; however also other combined indices exists, e.g. the Combined Drought Indicator developed by the EDO (European Drought Observatory) and a similar combined index used by the US Drought Monitor. [Lena M Tallaksen, Norway]	Rejected - The Table (now Table 11.A.1) includes the most widely used drought indices, including those based on different variables. Because of space limitations the considered indices cannot be expanded given the large number of existing drought indices in the literature. In addition the EDO metric is based in part on subjective measures.
113611	78	13	78	13	Adding SEDI [Diego Miralles, Belgium]	Rejected - The table (now Table 11.A.1) includes the most widely used drought indices, including those based on different variables. Because of space limitations the considered indices cannot be expanded given the large number of existing drought indices in the literature. In addition, this is still a very new drought index with very few studies.
113613	78	13	78	13	No time for meto do more; last comment:In the temperature sections there is a physical mechanism section. I am not sure where that fits in Drought. I would appreciate a sentencewhere it fits stating that 'drought can propagate downwind due to land-atmosphere feedbacks and their impct on moisture advection' Miralles, D. G., Gentine, P., Seneviratne, S. I. and Teuling, A. J.: Land-atmospheric feedbacks during droughts and heatwaves: state of the science and current challenges, Ann. N.Y. Acad. Sci., 2019. Merci! [Diego Miralles, Belgium]	Rejected - this is indirectly mentioned and cited in 11.6.1.3
44037	78	13	78	54	The sub-section 11.6.1.5 Combined synthetic measures of drought is clearly focused on precipitation and evaporation (or ET) only. No doubt, SPEI and PDSI are one of the most widely used drought indicators. However, SPEI is mostly regarded as a meteorological drought indicator. However, there are numerous multivariate indices developed which are very useful in characterizing the droughts i.e. combining different hydrological variables will help in comprehensively addressing the drought characteristics. Some of these combined drought indices are, 1. Multivariate Standardized Drought Index (MSDI) by Hao & AghaKouchak (2013, 2014), which includes the standardized deficits of precipitation and soil moisture; 2. Aggregate Drought Index by Booras et al. (2018), which incorporated standardized precipitation index, days of storage remaining (DSR) index, Palmer drought severity index and streamflow forecasts; 3. Integrated Drought Index by Shah and Mishra (2020), which combined standardized deficits of precipitation, runoff, groundwater and soil moisture; etc. These should also be included in the combined synthetic measures of drought. References: 1. Hao, Z., & AghaKouchak, A. (2013). Multivariate standardized drought index: a parametric multi-index model. Advances in Water Resources, 57, 12-18. 2. Hao, Z., & AghaKouchak, A. (2014). A nonparametric multivariate multi-index drought monitoring framework. Journal of Hydrometeorology, 15(1), 89-101. 3. Booras, K., McIntyre, A. R., Joshua Weiss, W., Howells, C., & Palmer, R. N. (2018). Incorporating streamflow forecasts with aggregate drought indices for the management of water supply. Journal of Water Resources Planning and Management, 144(1), 04017078. 4. Shah, D., & Mishra, V. (2020). Integrated Drought Index (IDI) for drought monitoring and assessment in India. Water Resources Research, 56(2), e2019WR026284. [SABYASACHI SWAIN, India]	Rejected - The table (now Table 11.A.1) includes the most widely used drought indices, including those based on different variables. Because of space limitations the considered indices cannot be expanded given the large number of existing drought indices in the literature
44039	78	13	78	54	Slette et al. (2019) carried out a study over several regions using SPEI. They found that over half of the droughts were actually within normal climatic variability. He also emphasized on how a drought can be characterized better, especially from the point of view of ecology. This should be included. Reference: Slette, I. J., Post, A. K., Awad, M., Even, T., Punzalan, A., Williams, S., Smith, M. D., & Knapp, A. K. (2019). How ecologists define drought, and why we should do better. Global change biology, 25(10), 3193-3200. [SABYASACHI SWAIN, India]	Rejected - Note that the paper has some comments and a reply that support the use of the SPEI

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
101351	78	13	79	54	The section on comined indices is focused on the SPEI index; however also other combined indices exists, e.g. the Combined Drought Indicator developed by the EDO (European Drought Observatory) and a similar combined index used by the US Drought Monitor. [Lena M Tallaksen, Norway]	Rejected - The table (now Table 11.A.1) includes the most widely used drought indices, including those based on different variables. Because of space limitations the considered indices cannot be expanded given the large number of existing drought indices in the literature. In addition the EDO and US drought monitor metrics are based in part on subjective measures.
23221	78	13			Section feels like it falls between two stools in introducing only two indicies whereas the subsequent sections use additional indicies such as SPI. [Peter Thorne, Ireland]	Rejected - But SPI is considered in the precipitation assessment (see table 11.A.1)
45553	78	15	78	21	Van Lanen et al. (2016) emphasized that droughts should be understood through the propagation of water deficits through the hydrological cycle and cannot be apprehended only through a meteorological perspective and purely meteorological indices. Van Lanen, H., Laaha, G., Kingston, D. G., Gauster, T., Ionita, M., Vidal, J.-P., Vlnas, R., Tallaksen, L. M., Stahl, K., Hannaford, J., Delus, C., Fendekova, M., Mediero, L., Prudhomme, C., Rets, E., Romanowicz, R. J., Gailliez, S., Wong, W. K., Adler, M.-J., Blauhut, V., Caillouet, L., Chelcea, S., Frolova, N., Gudmundsson, L., Hanel, M., Haslinger, K., Kireeva, M., Osuch, M., Sauquet, E., Stagge, J. H., Van Loon, A. F. (2016) Hydrology needed to manage droughts: the 2015 European case. Hydrological Processes, 30, 3097–3104, https://doi.org/10.1002/hyp.10838 [Jean-Philippe Vidal, France]	Rejected - drought propagation is stressed in 11.6.1.4. In any case, the reference to Van Lanen et al. Has been included there.
79125	78	16	78	16	reconsider the use of word 'environmental' [Andong Shi, Sweden]	Rejected - environmental droughts is a growing concept in the literature (e.g. https://www.sciencedirect.com/science/article/pii/S0012825218306421) but there is agreement in WGI to use ecological droughts.
104497	78	17	78	17	Not only agricultural impacts [Lena M Tallaksen, Norway]	Rejected - this section has been substantially shortened and rewritten
101349	78	17			Not only agricultural impacts [Lena M Tallaksen, Norway]	Rejected - this section has been substantially shortened and rewritten
105369	78	20	78	20	This is probably correct, but it needs to be better nuanced, exceptions should be pointed out unless you feel you can say this is always true, and references need to be provided). [Francis Zwiers, Canada]	Taken into account - the sentence has been removed.
71525	78	20	78	21	I do not think the word "poor" is suitable here. Drought indices are highly correlated with soil moisture measurements. This is normal given the high correlation between climate variability and soil moisture- There is a number of studies demonstrating these high correlations: e.g. Vicente-Serrano, S.M., et al. (2012) Performance of drought indices for ecological, agricultural and hydrological applications. Earth Interactions 16, 1–27., Scaini, Anna, et al., (2015) SMOS-derived soil moisture anomalies and drought indices: a comparative analysis using in situ measurements. Hydrological Processes. 29, 373–383. [Sergio Vicente-Serrano, Spain]	Taken into account - the sentence has been removed.
55421	78	23	78	33	This paragraph goes to great length to motivate why one of the earliest and most popular drought indices should not be discussed in the IPCC report. The motivation is weak and much of the issues noted apply to the other standardized drought metrics used in the SOD as well. My opinion is that this omission should be rectified as pursuing this direction simply ignores a huge body of evidence of documented changes in drought and projected changes in drought from across the world. Below is why the arguments not to use the PDSI are weak. *) argument 1: ("(PDSI) is not based on a robust water balance model as it oversimplifies soil surface hydrological processes". This is an argument recycled from the work of Alley (1984) who pointed to the limitations of the PDSI. Indeed, the PDSI has a simple water balance at its core, but the use of monthly precipitation sums as (one of the) drivers of this drought index precludes the use of much more sophisticated approaches. When the waterbalance is calculated on monthly timescales rather than the (sub)daily timescale as would be realistic, the use of a complex soil moisture model is simply overdoing it. Instead, the argument given in this SOD should be reversed: the PDSI is the only index which has a rudimentary waterbalance where other indices used in the SOD, like SPI or SPEI, fail to acknowledge that it is the waterbalance that actually matters in drought. *) argument 2: ("its calibration is targeted on present climate and can perform poorly under warmer climates"). This is true - the calibration is a sensitive point. But exactly the same argument may be given for the other standardized drought indices (SPI and SPEI) used in the SOD. All these indices use the historical period to calibrate the drought index. This means that for all standardized indices, an unprecedented drought in a future climate will bring the index values completely off the scale. *) argument 3: ("Even calculated using a self-calibration approach, the PDSI has strong problems of spatial comparability since it is an index that represents different drought frequencies among its sites"). It is no wonder that this statement lacks a reference as this is the first time that I see the statement as strong and lacking nuance as this. Perhaps the authors mean that the PDSI drought metric shows spatial variations in the spectrum of frequencies	Accepted: The criticisms related to the used of the PDSI have been removed and the section has been mostly rewritten in the revised document.
81075	78	23	78	33	Trends and variability in PDSI actually compare quite well with soil moisture trends and variability from more complex land surface models, including those used in GCMs. Some example citations that directly compare PDSI and soil moisture from more process based models: https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-15-0590.1, https://advances.sciencemag.org/content/1/1/e1400082, https://www.nature.com/articles/s41586-019-1149-8, https://science.sciencemag.org/content/368/6488/314.abstract. PDSI is not perfect by any means, but this whole paragraph is really quite biased and does not accurately reflect the actual value of PDSI as a drought index, especially when more detailed information is unavailable. Additionally, PDSI has formed the basis for quite a few tree-ring based drought reconstructions, which again provides some confirmation that it is, on some level, representing real world drought processes relevant for vegetation processes. Further, most of the fundamental critiques of PDSI ALSO apply to SPEI, in many cases moreso (e.g., use of PET). So it is a bit strange that SPEI is elevated to such a vaunted position here, when by all account PDSI (despite its legitimate faults) appears to be a much better emulator of real world drought processes. [Benjamin Cook, United States of America]	Accepted: The criticisms related to the used of the PDSI have been removed and the section has been mostly rewritten in the revised document.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2441	78	23	78	53	one of the main problems of the PDSI is that, in applications for ongoing trends or for future projections, one would need to include the effect of increasing atmospheric CO2 over time in the calculation of potential evapotranspiration (PET, which is used into the PDSI). However, because of the lack of precise knowledge on how stomatal conductance will change in the future, this is rarely done, i.e., many studies assume constant stomatal conductance in the calculation of PET. This leads to an overestimation of future PET, and thus of future droughts from the PDSI. This issue should be discussed here I believe. To the extent that SPEI also relies on PET, in my view it suffers from a similar issue as PDSI regarding the computation of future PET under higher atmospheric CO2. These should be mentioned explicitly as well; it seems to me the authors are aware of this issue, and try to circumvent it in the way they interpret the SPEI (i.e., saying that it doesn't reflect soil moisture necessarily, but some form of vegetation stress), but the issue with future PET trends in this type of indices needs to be clarified. [Alexis Berg, United States of America]	Taken into account: The possible role of the future atmospheric CO2 concentrations on the assessment based on climatic drought indices has been mentioned but discussed in more depth in the section 11.6.5. Now we are making reference to the Rejected. on the carbon-water nexus. This issue has been clarified in more depth.
82069	78	23	78	53	I like the idea of having a separate section on these synthetic measures which are not directly about soilmoisture, flow, ot otherwise. I thiknk they are being judged here though on the basis of how they represent something else like soil moisture (L20) or vegetation water stress (38). I imagine for many people though these are not limitations as such as they don't see these indices in the same way. I personally try not to overthink exactly what PDSI or SPEI are trying to represent 'out there' in the world as they are abstractions. Both seem to me to be aridity indices that try and approximate the climatic water balance in some way, and in that regard they are valuable in that they bring together the precipitation and temperature/AED aspects of drought in a single index. Which i think is valuable in itself. It cannot yield soil moisture or runoff, exactly, but provides a useful approximation or analogue [Jamie Hannaford, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - this section has been substantially shortened and rewritten
82071	78	23	78	53	Similalrly I'm not sure whether you are losing something in this 'contest' between PDSI and SPEI, given the decision not to show PDSI further in the chapter - when for all it's faults it's still the basis of a lot of drought science, so there is a lot of highly relevant evidence on drought changes that may not be covered. I'm also not sure it's that simple, The SPEI does have some advantages in flexiblity over the PDSI. But the SPEI is itself not without it's problems. There is a very large literature on issues with standardized indicators in general - not least the choice of probability distribution, the standard period used. These can all bring significant uncertainties. (even the choice of fitting technique, L-mom or MLE can have a big effect). Let alone the much bigger issue with SPEI of how PE is estimated (e.g. use of temperature based methods can be very problematic for climate changer appliucations). I do not have all the papers to hand but there have been discussions e.g. Vicente-Serrano et al. and Stagge et al. 2017. For these reasons, are the authors really confident that it is the go-to index at the expensive of sc-PDSI, say? I do appreciate you do not have the space to go into all these issues at great length but I'd like to see some strengthening of the case to use one rather than the other (at teh very least an acknoweldment of issues inherent in the SPEI too, perhaps in a Box/sub-section somewhere near table 1 that discussed drought indicators and their merits, that way could cover all standardized indices in general [Jamie Hannaford, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The criticisms related to the used of the PDSI have been removed and the section has been mostly rewritten in the revised document.
70373	78	27	78	27	This statement glosses over a critical weakness of the PDSI that is not explicitly mentioned in this paragraph - namely that PDSI deviates farther and farther from the actual water budget as atmospheric CO2 increases because it does not account for changes in the stomatal conductance of plants. We suggest that the following be added to this paragraph "Further, Epot is used as part of the calculation for PDSI, and even if Epot is calculated using physically-based approaches (Sheffield et al. 2012), PDSI fails to account for plant stomatal responses to increasing CO2 leading to larger estimates of water deficit and drought severity at high CO2 in ESMs compared to other drought metrics calculated form the same simulations (Swann et al. 2016, Berg and Sheffield 2018)." [Abigail Swann, United States of America]	Taken into account: The possible role of the future atmospheric CO2 concentrations on the assessment based on climatic drought indices has been mentioned and discussed in more depth in the section of drought projections (11.6.5). Now we are making reference to the Rejected. on the carbon-water nexus (Rejected. 5.1). It is clarified better what drought indices as the PDSI are representing and also the uncertainties of the assessment of the role of the enhanced atmospheric CO2 concentrations on drought severity.
2897	78	31	78	33	I agree with the authors' assessment about the issues regarding the PDSI - although, as I mentioned in another comment, I believe they should also mention that one major issue is that most PDSI-based studies do not consider the CO2-induced change in stomatal conductance when calculating future PET and thus future PDSI values, leading to an overestimation of future drought risk. However, the point of my comment here is that chapter 8 of the report (on the water cycle) also includes results on droughts, for instance on present trends and future projections, which are based on PDSI results -see figure 8.21. This introduces some potential inconsistency at the level of the whole report. [Alexis Berg, United States of America]	Taken into account: The possible role of the future atmospheric CO2 concentrations on the assessment based on climatic drought indices has been mentioned and discussed in more depth in the section of drought projections (11.6.5). Now we are making reference to the Rejected. on the carbon-water nexus. It is clarified better what drought indices as the PDSI are representing and also the uncertainties of the assessment of the role of the enhanced atmospheric CO2 concentrations on drought severity.
71527	78	32	78	33	This is not coherent with the section of drought projections in which assessment of drought projections include PDSI. Independently of the PDSI limitations (any drought metric has limitations and no one is perfect), I would include studies based on the PDSI in the assessment, since there is a number of studies using this tool and some of them may provide coverage in regions in which there are not studies based in other metrics. Ch. 8 is using PDSI to make an assessment of drought trends based on some recent studies. Some of the assessment in CH.12 is also based on PDSI studies. [Sergio Vicente-Serrano, Spain]	Taken into account: The criticisms related to the used of the PDSI have been removed and the section has been mostly rewritten in the revised document.
66397	78	35	78	53	One of the major limitation for the SPEI is the way in which Epot is calculated in the models. This should probably be mentioned somewhere. [Erika Coppola, Italy]	Accepted: Statement on the use of robust combination-based methods to calculate the PDSI in the analysis of long term trends.
55429	78	35	78	53	The discussion of the SPEI fails to mention one aspect. This is a bit hidden in the text (e.g. p91, line 9 "the decoupling of ET en Epot when ET is reduced to soil moisture limitation") but a balanced discussion of the SPEI needs to highlight this issue. In a situation of moisture stress, the potential and actual values of evapotranspiration are indeed decoupled, and (depending on the parameterization of Epot) the Epot increases as the ET decreases. This inverse relation is known since the 1960 from the work of Bouchet and Morton. See Brutsaert and Parlange (1998) Hydrologic cycle explains the evaporation paradox, Nature 396:30. This means that the drier the situation, the larger Epot becomes as well as its difference with ET. The SPEI is based on the difference between precipitation and the reference evapotranspiration rather than the actual evapotranspiration. The combination of these two aspects makes that the effects of changes in the reference evapotranspiration on drought are amplified. A metric like the Palmer Drought Severity Index, which solves a primitive water balance, is less affected by this since it used the estimated actual evapotranspiration. However, the standardization used in the SPEI limits the adverse effects of the amplification of Epot and the SOD bears testimony that the SPEI is a useable index and it is a fine addition to the toolbox of the climatologist. [Gerard van der Schrier, Netherlands]	Taken into account. The criticisms related to the used of the PDSI have been removed and the section has been mostly rewritten in the revised document.
81077	78	35	78	53	SPEI is better characterized as an aridity index, similar to P/PET because it equally weights both P and PET, both of which are (largely) independent of any sort of vegetation constraint. How would one expect it to be any better of an indicator of vegetation stress compared to P/PET? [Benjamin Cook, United States of America]	Accepted: The criticisms related to the used of the PDSI have been removed and the section has been mostly rewritten in the revised document.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
103875	78	40	78	44	It may be useful to separate between water limited regions versus energy limited regions when it comes to ET. [Lena M Tallaksen, Norway]	Noted - We already stress the differences between dry regions and humid areas during dry periods. Because the IPCC reports focus on assessments, space is limited to provided more detailed (textbook-like) information
15155	78	48			I might suggest calling this 'vegetation productivity' rather than 'vegetation activity' [John Abatzoglou, United States of America]	Not applicable - this section has been substantially shortened and rewritten
71529	78	49	78	50	Remove "under these conditions". There are also several studies that show high correlation of SPEI with streamflow in different regions worldwide. [Sergio Vicente-Serrano, Spain]	Taken into account - removed
71531	78	52	78	53	It is necessary to rewrite this sentence. The SPEI is not perfect (as any drought metric) but in practical terms the AED role on the SPEI in humid areas is irrelevant. There is not expected an overestimation of the SPEI on drought severity since the role of the AED on the SPEI only emerges in dry periods, exactly when the AED is relevant. There is a paper under review in JGR that supports this issue: Global characterization of the varying responses of the Standardized Evapotranspiration Index (SPEI) to atmospheric evaporative demand (AED). Tomas-Burguera, M. et al. [Sergio Vicente-Serrano, Spain]	Accepted - replaced the sentence according to the results by Tomas-Burguera et al. 2020
101353	78		78		It may be useful to separate between water limited regions versus energy limited regions when it comes to ET. [Lena M Tallaksen, Norway]	Rejected - not defined the specific text of the comment.
101355	78		78		Miss groundwater drought indices, ref. comment [9] [Lena M Tallaksen, Norway]	Accepted - Groundwater indices included in table (now Table 11.A.1) and mentioned in the first sentence of the hydrological drought section.
105371	79	1	79	5	This summary doesn't seem to say much. There were a few assessments in the preceding sub-subsections that aren't revisited – they should be – and presumably the summary to deal briefly with each sub-subsection before making some final statements. [Francis Zwiers, Canada]	Rejected - an interesting observation but given space limitations it is not possible to include a summary of each subsection.
105373	79	1	79	5	The first sentence of the summary that is offered is not particularly clear, but it seems to be saying that there are nuances in the various drought definitions that affect how useful they are in assessing the impacts of GHG forcing on drought. That statement doesn't need an assessment – it's simply a fact. You might go further, and say that some commonly used (maybe less commonly now) drought indices cannot be used to assess the impact of GHG forcing on drought (e.g., the PDSI) in an informative manner. [Francis Zwiers, Canada]	Accepted - the summary has been rewritten in order to gain clarity
23215	79	1	79	5	The first sentence makes no sense to me and would not be actionable by a policy maker. It lacks necessary details. Then the last sentence also makes no logical sense as written. [Peter Thorne, Ireland]	Accepted - the summary has been rewritten in order to gain clarity
37615	79	1	79	5	Specific consideration of AED + soil deficit + high temperature is badly needed [Timothy Brodribb, Australia]	Accepted - the summary has been rewritten in order to gain clarity
1487	79	2	79	4	"It is important to distinguish precipitation deficits from soil moisture deficits, streamflow deficits, increased atmospheric evaporative demand, and other measures of drought conditions and land water deficits." - perhaps explain why it is important? [Rasmus Benestad, Norway]	Accepted - the summary has been rewritten in order to gain clarity
13755	79	10	79	10	What is AR Chapter 2 ?, not understood [Maria Amparo Martinez Arroyo, Mexico]	Rejected - This is the previous IPCC report.
81079	79	10	79	11	There is no such thing as a "global drought": drought is an inherently regional phenomenon, and the imprecision in this language can really muddy up the communication. What the AR5 actually said was "Confidence is low for a global-scale observed trend in drought or dryness (lack of rainfall) since the middle of the 20th century,owing to lack of direct observations, methodological uncertainties and geographical inconsistencies in the trends". In other words they could not, at the GLOBAL-SCALE detect changes in drought. This is an important distinction. [Benjamin Cook, United States of America]	Accepted - The sentence has been rewritten
23217	79	14	79	16	I would disagree with this characterisation which is reliant upon a handful of papers - some quoted somewhat out of context in my view. There are numerous papers highlighting advances in surface humidity and winds understanding and the substantive assessment findings reached in chapter 2 risk being fatally underminded by this statement. Chapter 11 should refer the reader to these assessments rather than make sucha. statement based upon a single study. [Peter Thorne, Ireland]	Accepted - the sentence has been modified.
6793	79	15	79	15	Please see comment 229 concerning the number of humidity observations, in relation to the analysis by Willett et al.(2014). [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - comment indicated by the reviewer not found among the comments received in the chapter.
23219	79	16	79	22	Chapter 8 had a substantive assessment of soil moisture including observations. Why repeat this assessment here and risk a reader playing a game of spot the difference. Text should be moved to chapter 8 and merged and the chapter 8 finding should be articulated here with the section cross-referenced instead. [Peter Thorne, Ireland]	Accepted - this text has been removed.
81081	79	22	79	22	What is meant by "low temporal agreement"? Why would we expect year to year variability in observed soil moisture to match up with climate models using a free running ocean? [Benjamin Cook, United States of America]	Accepted - the sentence has been reworded.
105375	79	22	79	23	Where has this been assessed? There might be fewer problems, but that doesn't mean that they are fit for detecting change, either in extreme low flows or in extreme high flows. The majority of gauged sites are affected by flow regulation of some kind, which means that there will be very substantial inhomogeneities in the gauge data that would affect the assessment of trends (particularly in the extremes). Annual discharge might not be largely affected, but virtually every other aspect of hydrologic behavior likely is affected. [Francis Zwiers, Canada]	Rejected - of course that gauging stations are affected by human disturbances and management (damming, irrigation, urban supply) but this is the real world and if changes are driven by human factors this must also be assessed. This, in the previous section it has been stressed that hydrological droughts have usually a human component. Moreover, there are also studies based on networks of natural catchments (usually in the headwaters. So, there is an important diversity.
45557	79	22	79	23	The availability of high-quality streamflow information is still problematic even in Europe given some national restrictions on up-to-date time series, as demonstrated by Laaha et al. (2017) for analysing the spatio-temporal footprint of the 2015 European drought. Laaha, G., Gauster, T., Tallaksen, L. M., Vidal, J.-P., Stahl, K., Prudhomme, C., Heudorfer, B., Vlnas, R., Ionita, M., Van Lanen, H. A. J., Adler, M.-J., Caillouet, L., Delus, C., Fendekova, M., Gailliez, S., Hannaford, J., Kingston, D., Van Loon, A. F., Mediero, L., Osuch, M., Romanowicz, R., Sauquet, E., Stagge, J. H., Wong, W. K. (2017) The European 2015 drought from a hydrological perspective, Hydrology and Earth System Sciences, 21, 3001–3024, https://doi.org/10.5194/hess-21-3001-2017 [Jean-Philippe Vidal, France]	Rejected - this section has been substantially shortened and rewritten

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
82073	79	23	69	26	This whole section makes it sound like streamflow data is much better positioned to answer questions about trends than other variables. This is perhaps a bit of an optimistic view. See many previous commentaries on the challenges of global streamflow data (Hannah et al. 2011 https://onlinelibrary.wiley.com/doi/abs/10.1002/hyp.7794; Viglione et al. 2013; 10.1016/j.jhydrol.2010.03.023; Dixon and Rodda.) While large databases like Gundmundsson are welcome, in general assemblig global data for trends is a real challenge (the points made about gaps and short records are very welcome). Would be worth saying that most efforts to gather data for global trend detection are also hampered by the prevalence of human impacts which make it difficult - in some areas impossible - to identidfy climate driven trends. The datasets like GSIM etc based on GRDC include all sorts of impacts which render interpretation to climate forcing very difficult. Maybe also worth mentioning efforts to define 'Reference Hydrometric Networks' to counter this (e.g. Burn at al. 2012 https://doi.org/10.1080/02626667.2012.728705 and Whitfield et al. 2012 https://doi.org/10.1080/02626667.2012.728706). Also as mentioned elsewhere, groundwater gets no mention. GW is even more challenging as there are few efforts to integrate - the European Groundwater Drought Initiative being a recent one, but there have been very few efforts. [Jamie Hannaford, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - The text has been modified and the points of the reviewer included.
11717	79	23	79	23	“fewer limitations” (rather than “less”) [Amy East, United States of America]	Rejected - This sentence has been entirely rewritten.
10123	79	24	79	26	Another important global streamflow database is described in Dai A. Historical and future changes in streamflow and continental runoff: a review. In: Tang Q, Oki T, editors. Terrestrial water cycle and climate change: natural and human-induced impacts, Geophysical Monograph 221. 1st ed. Wiley; 2016. p. 17–37 Available at http://www.cgd.ucar.edu/cas/adai/papers/Dai-2016_ch02-GeophysMonograph221.pdf This study has much more global coverage. [Jacob Scheff, United States of America]	Accepted - the reference has been included in the revised document.
81083	79	24	79	26	A major limitation in streamflow databases for the detection/attribution of hydrologic drought is that many of the largest bases are highly managed via dams, reservoirs, etc. So their usefulness for climate change studies may be severely limited. This is discussed a bit in: https://www.earth-syst-sci-data.net/11/1655/2019/ [Benjamin Cook, United States of America]	Accepted - reference to these difficulties has been expanded.
103895	79	25	79	26	Observed trends; miss a distinction between annual and seasonal trends; e.g. summer and winter low flows are caused by different processes [Lena M Tallaksen, Norway]	Rejected - this observation is interesting but there is limited space to include detailed seasonal assessment.
103883	79	26	79	27	One could add "there is limited knowledge on groundwater droughts" [Lena M Tallaksen, Norway]	Rejected - this section has been removed in the final document
101357	79	26			One could add "there is limited knowledge on groundwater droughts" [Lena M Tallaksen, Norway]	Rejected - this section has been removed in the final document
62403	79	31	79	35	The Greater Horn of Africa is missing here even though there are a number of papers that have looked at the rainfall deficit in the region for example Rowell et al., 2015 (cited in the section), Lyon ad Dewit 2012, Funk et al., …. Is there a quantification of “strong deficits”? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - included in the list, with reference to the drought of 2011 and reference added
62409	79	31	80	5	The authors also give the impression that the cited literature utilized CMIP models, however in some cases cited literature was using CORDEX. The authors do not mention which RCP is being utilized for the results discussed. This section needs to be expanded and informaion stated as clearly as possible. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - this section is only covering observations
125995	79	31	80	6	Would be good to summarize where annual observed precipitation trends are found to be detectable (outside 5th to 95th percentile range of natural variability), especially over land regions with coverage (1901-2010), as the regions witih detectable decreases are likely the most susceptible regions to increasing drought problems. These are shown in Figure 3 of Knutson and Zeng (2018). While more regions were found to have detectable increasing precipitation trends than detectable decreasing precipitation trends, the detectable decreasing trend regions include some regions not mentioned here: much of the region surrounding the Mediterranean (especially the eastern parts from Egypt through Syria and southeast Europe), parts of northern tropical Africa including the Sudan, extreme southwest Australia and Tasmania, and some island regions including parts of the Caribbean/Bahamas, parts of Japan and Indonesia, and parts of Chile. [Trigg Talley, United States of America]	Rejected - The assessment of precipitation trends is included in chapter 8, here we assess possible changes in drought characteristics. In the regional tables it is included further information on drought trends.
107419	79	31	80	6	Precipitation deficit is highly recorded in the Mediterranean region, especially in Algeria. There are a lot of studies talking on the SPI index ant the precipitation variability among seasons and years. [Rachda Berrached, Algeria]	Noted - there are recent studies that also suggest clear stationary behaviour in the countries of North Africa, including Algeria, Tunisia and Morocco over at least the last four decades (https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/joc.6734, https://www.researchgate.net/publication/276173899_Trends_in_indices_of_daily_temperature_and_precipitations_extremes_in_Morocco, https://link.springer.com/article/10.1007/s11269-017-1856-6, https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/joc.3683). The final assessment indicates the different lines of evidence which lead to an assessment of low confidence regarding meteorological drought changes in the Mediterranean region.
81085	79	31	81	6	There is strong evidence for observed precipitation drying trends in Mediterranean climate regions outside of California (https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-18-0472.1) [Benjamin Cook, United States of America]	Noted. This assessment is focused on the Mediterranean region rather than Mediterranean climate per se. The assessment of changes in precipitation-based Mediterranean droughts has been expanded.
9161	79	32	79	32	The so-called Millennium drought in Australia occurred from 1997-2009, not 2001-2009. The 1997–2009 Millennium drought had substantial economic, environmental and social impacts (Chiew and Prosser, 2011; Leblanc et al., 2012; van Dijk et al., 2013; Chiew et al., 2014). [Kevin Hennessy, Australia]	Accepted - 1997 included
37617	79	38	79	38	Define CDD- consevutive dry days? [Timothy Brodribb, Australia]	Rejected - this is in the glossary.
62449	79	38	79	38	Donat et al. 2014 missing perhaps missing (a) or (b) line 38 page 79 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - reference has been included.
39317	79	43	79	44	an additional paper is being recommended-Supari et al, 2020 (Multi-model projection of precipitation extremes in SouthestAsia based on CORDEX-SEA simulations). [Lourdes Tibig, Philippines]	Rejected - this section is only covering observations
131425	79	45	79	47	The original article (Gallant et al., 2013) draws a much more heterogenous picture of the drought development in Australia between 1911 and 2009. The article also concludes: "Across much of the continent, droughts became less frequent, shorter and less intense from 1911 to 2009 and 1960 to 2009." but adds "However, there were several exceptions including far southwest Western Australia, which has had statistically significant increases in drought intensity. The average length of droughts in parts of southeast Australia statistically significantly increased since 1911. During the second half of the 20th Century droughts in these areas were between 10 and 69% longer than droughts during the first half of the 20th Century." [Hans Poertner and WGII TSU, Germany]	Rejected: this entire section has been substantially changed and reduced a lot in the final document.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9163	79	45	79	47	This sentence is misleading. Reword as "Drought frequency and duration have increased in southwest Western Australia and southeast Australia from 1950-2009, but decreases have occurred in northern Australia (Gallant et al., 2013)". Note that Gallant et al (2013) state that there have been "increases in seasonal-scale drought frequency, duration and intensity in areas of southwest and southeast Australia. In parts of the west and southeast of the Murray–Darling Basin, the average duration of seasonal-scale droughts, defined as successive seasons in drought, statistically significantly increased by between 10 and 69% during the second half of the 20th Century" [Kevin Hennessy, Australia]	Rejected: this entire section has been substantially changed and reduced a lot in the final document.
82799	79	46	79	46	The region here would be better described as "southwest and southeast Australia" (as per the Gallant et al 2013 paper). [Blair Trewin, Australia]	Accepted -reference to AR6 regions has been included
62451	79	48	79	48	Ogunrinde et al., 2019 missing in reference list page 79 line 48 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - the reference has been included
82075	79	52	79	53	Surprised to see Kay et al. 2018 in this section because this paper is primarily about low river flows and is more about future projections than past trends (though it has some). More generally the no trends in N Europe seems very broad brush and I do not think is supported. I thought some studies showed decreasing precip drought severity (i.e.increasing SPI) in N europe, like Stagge et al. (2017, Nat. Sci. Reoprts), in ref list. This is just an example and I am sure there are others (certainly there are many showing increases in runoff see below, but I think some showing trends in drought indicators too. Worth reviewing this. [Jamie Hannaford, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the reference has been removed
62835	79		82		General Comment for section 11.6.2, related to Tables 11-4 to 11-9. The section 11.6.2 regarding observed trends in Droughts is hard to read compared to the similar sections for temperature and precipitation extremes. I suggest to list the references according a continental order or Land Regions. Please consider section 11.4.2 for reference [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - The availability of studies strongly depends on the variable of interest so it varies a lot among drought metrics.
101363	79				Observed trends; miss a distinction between annual and seasonal trends; e.g. summer and winter low flows are caused by different processes [Lena M Tallaksen, Norway]	Rejected - this observation is interesting but there is limited space to include detailed seasonal assessment.
101359	80	1	6		The reference by Stagge et a. 2017 (already in the report) should be added here (trends in SPI and SPEI over Europe). [Lena M Tallaksen, Norway]	Accepted - the reference has been included
103887	80	5	80	6	The reference by Stagge et a. 2017 (already in the report) should be added here (trends in SPI and SPEI over Europe). [Lena M Tallaksen, Norway]	Accepted - the reference has been included
10125	80	9	80	33	This section 11.6.2.2 should include the strong caveat that plant physiological (i.e. stomatal) changes may tend to strongly reduce plant water demand or ETpot (Yang et al. 2019) and that the other cited studies did not try to account for this. Ideally, it should also include the counter-caveat that LAI increases may cancel some of this effect. [Jacob Scheff, United States of America]	Rejected: This section includes an assessment of observations. It does not discuss possible drivers of the observed trends in atmospheric demand. The mention on the effects of plant cover and LAI increase are included in a new cross chapter box on the carbon-water nexus (CC-Box 5.1), and referenced in the chapter when necessary.
39319	80	11	80	14	Please spell out "Epot". [Lourdes Tibig, Philippines]	Accepted - replaced by "atmospheric evaporative demand"
81087	80	11	80	33	More important references demonstrating Epot inensifying recent drought events: https://science.sciencemag.org/content/368/6488/314.abstract. However, isn't this discussion just about observed trends, and not attribution of said trends? So shouldn't the causes, like increases in Epot, of recent trends be merged into the attribution sections? [Benjamin Cook, United States of America]	Taken into account - The reference is included but there is a specific section related to drought attribution and we have tried to maintain the same structure in the different sections so it is not possible to merge sections.
23225	80	14	80	15	This sentence makes no sense as written. Please redraft. [Peter Thorne, Ireland]	Accepted - the sentence has been rewritten
10127	80	14			Again, need to use specific physical quantity(s) rather than "atmospheric dryness" which is vague. [Jacob Scheff, United States of America]	Accepted - this is replaced by the atmospheric evaporative demand, which is well explained in the section 11.6.1.2.
7195	80	15	80	15	another references should be cited as they were kind of pioneer for this: (1) Behrangi, A., E. J. Fetzer, and S. L. Granger (2016), Early detection of drought onset using near surface temperature and humidity observed from space, International Journal of Remote Sensing, 37(16), 3911-3923, doi: 10.1080/01431161.2016.1204478. (2) Behrangi, A., P. Loikith, E. Fetzer, H. Nguyen, and S. Granger (2015), Utilizing Humidity and Temperature Data to Advance Monitoring and Prediction of Meteorological Drought, Climate, 3(4), 999-1017, doi: 10.3390/cli3040999. [Ali Behrangi, United States of America]	Accepted - the references have been included
62565	80	15	80	16	Not sure what this sentence is trying to say...please revise for clarity [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - sentence reworded
125997	80	16	80	18	Why refer to Penman-Monteith analyses when speaking of AED/Epot, which has been defined in the document as free-water evaporation rate? [Trigg Talley, United States of America]	Rejected - the FAO-56 Penman-Montheit is a metric of the atmospheric evaporative demand as it includes constant surface resistance parameter based on reference crop. Therefore, spatial and temporal variations of the reference Eto are driven exclusively by meteorological variables.
8003	80	19	80	19	decreases in relative humidity are restricted to land area (see Chapter 8) [Bart van den Hurk, Netherlands]	Accepted - land areas has been detailed
103891	80	20	80	28	When discussing trends in Pan evaoporation it is important to recognise its heteorogeneous distribution globally, e.g. limited Pan data for Europe. [Lena M Tallaksen, Norway]	Accepted - this has been included in the revised document
105377	80	30	80	30	Need to be careful about this. Global stilling has been noted, but is it clear that this is a climatic phenomenon, and it is clear that this affects regions large enough to affect atmospheric evaporative demand? We notice the problem at meteorological stations, which are situated in areas where meteorological observations are required for other uses (to support civil aviation, agriculture, etc). So, it could well be that the stilling phenomenon reflects changes in surface roughness associated with land use and development near the stations, but that might not be representative of what is happening to winds over the land mass in general. In short, this either needs careful assessment here or there need to be crosslinks to the places in the report that wind changes are assessed authoritatively. Whatever Zhang et al 2019h said needs to be assessed in the context of those assessments of wind changes – the chapter has to do more than just repeat something like this. [Francis Zwiers, Canada]	Accepted - solar brightening/dimming have been included as possible drivers of AED.
81089	80	36	81	11	As noted above, PDSI is a reasonable proxy of soil moisture availability that allows extension of soil moisture records back much further in time (indeed, this is why PDSI is often targeted in tree-ring reconstructions of drought). This would be a good place to reference and discuss documented PDSI trends. [Benjamin Cook, United States of America]	Rejected - The PDSI is not a proxy of soil moisture. Although it uses a water budget model, this is used to obtain CAFEC ET, precipitation runoff, etc. But the soil moisture outputs of the water budget model are not used. W. Palmer stressed in 1965 that the PDSI is mostly a climate drought index, in which Epot and precipitation play the preponderant role. Of course, soil moisture is related to climate variability and the PDSI is usually very well correlated to the soil moisture (as also other drought indices) but the PDSI cannot be consider a metric of soil moisture.
4215	80	38			You might consider examining Solander KC, et al. The pan-tropical response of soil moisture to El Niño. Journal of Geophysical Research, in press. I can send you a pdf if you like. [Nate McDowell, United States of America]	Rejected - this article is on influence of atmospheric circulation on soil moisture anomalies but this section is addressing possible trends.
4217	80	38			An overall critique of this chapter on drought: you briefly highlight that there are negative impacts on drought and rising Epot on plants, but don't do this material sufficient justice I believe. There is overwhelming evidence now that rising VPD, when combined with precip deficit, is leading to, and will lead to far more, tree mortality around the earth. I would suggest a paragraph on this somewhere in this chapter. [Nate McDowell, United States of America]	Accepted partially - This has been stressed in the Rejected. of the nexus between carbon and water (CC-Box 5.1) and in section 11.6.1.2. We cannot enter in detail on drought impacts since this is mostly the role of the WGII report.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9165	80	45	80	45	In New Zealand, since 1972/73, soils at 7 of 30 sites became drier. The 2012–13 drought was one of the most extreme in the previous 41 years (NZ MfE, 2017: http://www.mfe.govt.nz/sites/default/files/media/Environmental%20reporting/our-atmosphere-and-climate-2017.pdf) [Kevin Hennessy, Australia]	Taken into account. This reference is now included in the regional table 11.12. However the final assessment is of low confidence because of the limited available data.
10129	80	45	80	46	This sentence should also again mention that the satellite only senses surface soil moisture, whose trends are not representative of trends in root-zone, plant-relevant soil moisture (Berg et al. 2017.) I know it was mentioned earlier, but is again relevant here. [Jacob Scheff, United States of America]	Accepted - included in the revised document.
23227	80	45	80	47	You have already said this. Do you really need to state it again? [Peter Thorne, Ireland]	Taken into account. The previous text in Table 11.3 (now Table 11.A.1) has been shortened. However, it is necessary to mention this point in the main text in order to provide better assessment of the soil moisture trends.
125999	80	45			Another problem to mention with satellite-based trends is the relatively short record length for distinguishing trends from natural variability. [Trigg Talley, United States of America]	Accepted - this has been stressed in the revised document.
62567	80	51	80	51	please change “South-Eastern North America” to “southeastern North America” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - replaced in the revised document
10131	80	54	81	11	This discussion should again include the strong caveat that many or most of these soil moisture modeling studies did not account for the strong negative effect of plant physiology on ET under increasing CO2 (e.g. Yang et al. 2019), so these long-term modeled soil moisture trends cited here may have a drying bias. [Jacob Scheff, United States of America]	Rejected: see Rejected. on carbon-water nexus (CC-Box 5.1) and Chapter 8 on the uncertainties of the CO2 fertilizing issues. In addition, this section is on observations. Co2 effects is suggested to have an impact on future scenarios under high atmospheric CO2 concentrations, but in the last decades and even considering that the CO2 effects on the atmospheric demand are robust, its effect on the atmospheric demand trends would be small (see Vicente-Serrano, et al., 2020 WIRES climate change 11).
101361	80				When discussing trends in Pan evaoporation it is important to recognise its heteorogeneous distribution globally, e.g. limited Pan data for Europe. [Lena M Tallaksen, Norway]	Accepted - this has been included in the revised document
62569	81	2	81	2	please change “North China” to “northern China” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - replaced in the revised document
62571	81	9	81	9	please change “deplete” to “depleting” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - replaced in the revised document
103899	81	14	81	14	I disagree that there are few studies on trend in hydrological drought; low flow is a measure of drought and there are several low flow trend studies globally, e.g. Stahl et al (2012) discusses trends in high and low flows for Europe; https://www.hydrol-earth-syst-sci.net/16/2035/2012/ [Lena M Tallaksen, Norway]	Accepted - we have rewritten this sentence.
29921	81	14	81	33	The following paper analyze trends in hydrological droughts over Southern South America, an area that is not covered in the session: https://link.springer.com/article/10.1007/s00704-017-2243-1 [Juan Rivera, Argentina]	Accepted - Reference to the trend in hydrological droughts in southern South America has been included.
101365	81	14			I disagree that there are few studies on trend in hydrological drought; low flow is a measure of drought and there are several low flow trend studies globally, e.g. Stahl et al (2012) discusses trends in high and low flows for Europe; https://www.hydrol-earth-syst-sci.net/16/2035/2012/ [Lena M Tallaksen, Norway]	Accepted - we have rewritten this sentence.
101369	81	14			Groundwater drought is missing - and important to state what type of hydrological drought indices the findings refer to. [Lena M Tallaksen, Norway]	Accepted - we have included a paragraph related to the assessment of groundwater droughts
82077	81	16	81	33	My general comments on the Hydrological deficits section mirrors others related to hydrology - quite short, no mention of groundwater (which is a major ommision) and quite a strong emphasis on human drivers but less coverage of the huge literature on climate driven trends in the absence of human impacts. I think the first line 'few studies analysing trends in hyd droughts' is wrong, as there is a large literature, especially if considering the wider sey of variables (Table 1) such as low flows. [Jamie Hannaford, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - we have included a paragraph related to the assessment of groundwater droughts and stressed the role of climate on possible trends.
45549	81	18	81	20	Caillouet et al. (2017) found that larger areas have been affected by low-flows in France after 1940 in an ensemble hydrological reconstruction starting in 1871. Caillouet, L., Vidal, J.-P., Sauquet, E., Devers, A., Graff, B. (2017) Ensemble reconstruction of spatio-temporal extreme low-flow events in France since 1871. Hydrology and Earth System Sciences, 21, 2923-2951, https://doi.org/10.5194/hess-21-2923-2017 Caillouet, L.; Vidal, J.-P.; Sauquet, E.; Devers, A. & Graff, B. Ensemble reconstruction of spatio-temporal extreme low-flow events in France since 1871 Hydrology and Earth System Sciences, 2017, [Jean-Philippe Vidal, France]	Accepted - we have included a the reference.
82079	81	19	81	20	the Barker et al. 2019 supports this but it's just the UK. There are many other papers that look at hydr drought from a wider European perspective (see examples below), albeit with shorter records. Worth noting also that Barker was based on model reconstructed flows and should be some consideration of observations from other studies. Also note that for Groundwater another observation based study suggests increased drought associated with warming in the UK (https://www.hydrol-earth-syst-sci.net/23/1393/2019/) [Jamie Hannaford, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - assessment of groundwater droughts included, more references added related to North Europe and assessment related to climate drivers.
90857	81	19			Refer reference "Regional hydrological drought in north-western Europe: Linking a new Regional Drought Area Index with weather types" [Vivien How, Malaysia]	Rejected. The reference is of 2011 and only very few and well justified references before 2012 are being included.
103903	81	20	81	20	The reference by Barker et al (2019) is limited to the UK. [Lena M Tallaksen, Norway]	Accepted - More references referring to North Europe have been included
101367	81	20			The reference by Barker et al (2019) is limited to the UK. [Lena M Tallaksen, Norway]	Accepted - More references referring to North Europe have been included
62405	81	24	81	26	This statement is confusing on the citations, it seems like at the beginning this is a statement from Udall and Overpeck, however it ends with another citation. “.....Udall and Overpeck (2017) estimated that between 1/6 and 1⁄2 of the flow reduction in the Colorado river between 2000-2014 was related to the unprecedented high temperatures (Xiao et al., 2018)”. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - rewritten in the revised document.
13757	81	25	81	25	standardize fraction size 1/2 [Maria Amparo Martinez Arroyo, Mexico]	Accepted - replaced in the revised document.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
82081	81	27	81	33	This section on drivers is good in pointing out hyd drought changes impacted by both climate and human factors. However it would be worth also highlighting the considerable efforts to index climate driven changes by focusing on reference networks of near-natural catchments. The last IPCC2013 had considerable coverage of such studies, e.g. the European Scale studies of Stahl et al. (2010, 2012, see refs in last ICC report). They are perhaps a little old now but there have been other efforts in Europe at the country-regional scale (Bard et al. 2015: https://www.sciencedirect.com/science/article/pii/S0022169415005582) and in the US and Canada at the regional to contiental scale (Dudley et al. 2020: https://linkinghub.elsevier.com/retrieve/pii/S0022169419309898). There are many more I think in North America but do not have time to dig them out. Also in Australia, ZHang et al. 2016. https://www.hydrol-earth-syst-sci.net/20/3947/2016/. Arguably many of these are less focused on drought per se and more on general streamflw - but they normally focus on low flows or seasonal minima which are a very drought relevant metric, and often make specfic reference to drought changes. Also they are observational and high quality datasets, whereas some of the assembled literature is very model-focused. I think the IPCC chapters would do well to accomodate some of this literature which, by it's very design, seeks to provide the robust, unambiguous evidence on climate driven changes that the IPCC requitres. [Jamie Hannaford, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Section fully rewritten and suggested modifications added.
71533	81	28	81	29	There are not substantial precipitation trends in the Mediterranean, at least in the region in which strongest streamflow decrease has been recorded (Spain and South France) so I do not know how this can be supported by precipitation trends. [Sergio Vicente-Serrano, Spain]	Accepted - Section fully rewritten and suggested modifications added.
126001	81	28			Add Knutson and Zeng (2018) to the list of references showing precipitation decreasing trends in the Mediterranean region. [Trigg Talley, United States of America]	Rejected - this section is related to hydrological droughts.
103907	81	33	81	33	Groundwater drought is missing - and important to state what type of hydrological drought indices the findings refer to. [Lena M Tallaksen, Norway]	Accepted - we have included a paragraph related to the assessment of groundwater droughts
55425	81	38	81	41	Here the SOD should be more balanced by including studies that analyse drought variations based on the PDSI [Gerard van der Schrier, Netherlands]	Accepted: global and regional studies based on the PDSI have been included
10133	81	43	82	3	This discussion should again include the strong caveat that these SPEI studies did not account for the strong negative effect of plant physiology on ETpot under increasing CO2 (e.g. Yang et al. 2019), so the trends may be much too negative. [Jacob Scheff, United States of America]	Taken into account: See Rejected. 5.1 on carbon-water nexus and Chapter 8 on the uncertainties of the CO2 fertilizing issues. In addition, this section is on observations. CO2 effects is suggested to have an impact on future scenarios under high atmospheric CO2 concentrations, but in the last decades and even considering that the CO2 effects on the atmospheric demand are robust, its effect on the atmospheric demand trends would be small (see Vicente-Serrano, et al., 2020 WIRES climate change 11).
10135	81	43	82	3	Even more importantly, this discussion also needs to explicitly mention that these widespread negative SPEI trends are totally inconsistent with the observed global trends in real eco-hydrological quantities like vegetation health and streamflow, which SPEI was developed in order to better predict (Scheff 2018.) [Jacob Scheff, United States of America]	Rejected: there is a substantial scientific literature showing stronger and more frequent episodes of forest dieback and mortality associated to stronger discrepancy between the available water and the atmospheric demand. This is addressed in more detail in the section 11.6.1.2. It is also indicated in the text that SPEI or the PDSI are not primarily intended to be metrics of streamflow or soil moisture.
62455	81	46	81	46	Marengo and Espinoza, 2016b; Fu et al., 2013), both are missing, I guess the (b) was supposed to be for Fu et al 2013 46 page 81 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - included in the revised document.
62457	81	48	81	48	Mathbout et al., 2018 missing in the reference perhaps a missing letter line 48 page 81 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - included in the revised document.
81091	81	53	82	1	I don't understand how SPEI can overestimate Epot effects (in humid areas) if, as you say, it is not sensitive to Epot variability. Are you trying to make a distinction between trends and variability? [Benjamin Cook, United States of America]	Accepted - sentence has been rewritten.
71535	81	54	81	54	Remove "very". The SPEI is not sensitive to the AED in humid regions and under periods of high precipitation. [Sergio Vicente-Serrano, Spain]	Taken into account - sentence has been rewritten.
71537	82	1	82	1	"in water-limited regions, BUT ALSO IN HUMID AREAS DURING PERIODS OF LOW-PRECIPITATION/LOW-SOIL MOISTURE,... (Vicente-Serrano et al., 2020). For example, in the Mediterranean region, the SPEI has decreased over the last decades as a consequence of the enhanced AED (Stagge et al., 2017), which has caused stronger drought severity during periods of low precipitation (García-Herrera et al., 2019). [Sergio Vicente-Serrano, Spain]	Taken into account - sentence has been rewritten.
105379	82	4	82	4	I had promised not to repeat this comment again, but … it applies to every sub-subsection on observed trends …. In common with many such sub-subsections, an assessment that is supported by the evidence that is reviewed is not given. The authors shouldn't leave the task of making an assessment to the reader, and also, they shouldn't separate the assessment from the presentation of the evidence by providing it only in the summary paragraph at the end of the entire subsection. [Francis Zwiers, Canada]	Rejected - as mentioned in other comment, there is small space to provide independent assessment for each subsection. Assessment with confidence levels is provided for the general sections.
55423	82	9	82	9	It is the "Climatic Research Unit" (not Climate) [Gerard van der Schrier, Netherlands]	Not applicable - figure caption was changed.
43393	82	11			Read " 20% of probability (1 event in 5 years), based on Spinoni et al. (2019). " rather than " 20% of probability (1 event in 5 years).Based on (Spinoni et al., 2019). " [Cyriaque Rufin Nguimalet, Central African Republic]	Not applicable - figure caption was changed.
43395	82	19			Read " Units: days/decade (from Dunn et al., submitted)." rather than " Units: days/decade. (from Dunn et al., submitted)" [Cyriaque Rufin Nguimalet, Central African Republic]	Not applicable - figure was merged, caption was changed
10137	82	24	82	25	I would change this to "medium confidence" given the above issues with physiological effects on ETpot. [Jacob Scheff, United States of America]	Rejected: this is based on observations. We can not assess medium confidence because models suggest a possible impact of CO2 fertilization issues. VPD is clearly increasing and there is a reversal in the global stilling so increase in the atmospheric demand is very reasonable. Also note the caveats and uncertainties related to the CO2 fertilization effects on soil and plant hydrology in the carbon-water nexus Rejected. (CC-Box 5.1) and in Ch. 8.

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126003	82	24	82	34	[CONFIDENCE] Should high confidence that AED has increased on average over continents be stated without noting also with high confidence an opposite effect: that CO2 fertilization has surely contributed a widespread decreasing tendency to (the total tendency of) potential evapotranspiration? Seemingly not. (Enhanced LAI might arguably provide a negative feedback to this effect, but shouldn't be able to reverse it altogether if the LAI increase is due to increased WUE.) It is reasonable to expect that increasing VPD (a relatively clear trend) will tend to enhance evapotranspiration; it is also reasonable to expect that increasing atmospheric CO2 concentration (an undisputed trend) will suppress evapotranspiration. Land model computations of soil moisture derived from meteorological observations, if they do not account for the effect of CO2 fertilization, have the same limitations as inferences from AED. The final sentence in this passage is also undermined for the same reason. [Trigg Talley, United States of America]	Rejected: see Rejected. on carbon-water nexus (CC-Box 5.1) and Chapter 8 on the uncertainties of the CO2 fertilization issues. In addition, this section is on observations. CO2 effects is suggested to have an impact on future scenarios under high atmospheric CO2
105381	82	24	82	34	Most of these assessments seem reasonable, but as noted in previous comments, each sub-subsection should present the arguments that lead to the assessments that emerge out of those subsections. Without that, it becomes very hard if not impossible for the reader to trace the assessments given here back to the reasoning and evidence that supports the assessment. I'm not sure that the "high confidence" assessment in the second assessment merits inclusion, at least as stated. What would it take for precipitation deficit to show a trend at the global scale? Presumably a systematic redistribution of precipitation would have had to have occurred, with general reductions over land and general increases over oceans. It then seems not very informative to have "high confidence" in something that, I think only would have surprised us if it had not occurred. [Francis Zwiers, Canada]	Accepted - second assessment based on global precipitation has been removed.
107421	82	27	82	28	Precipitation deficit has also increased in North Africa. [Rachda Berrached, Algeria]	Rejected - there are uncertainties related to data availability and differences between studies. There are also studies that support no changes.
9167	82	27	82	28	Add "southern Australia" based on the findings of Gallant et al (2013) [Kevin Hennessy, Australia]	Accepted - included in the revised document.
71539	82	33	82	34	Replace by: "There is medium confidence that trends in potential evapotranspiration have increased the severity of droughts in some regions and seasons." [Sergio Vicente-Serrano, Spain]	Accepted - included in the revised document.
72113	82	37	83	8	Some model evaluation studies of drought conditions have also been conducted in Africa. For examples, we have the same Gibba et al. 2019 above who also evaluated Consecutive Dry Days (CDD), there is also Ajayi, V.O., Ilori, O.W. Projected Drought Events over West Africa Using RCA4 Regional Climate Model. Earth Syst Environ (2020). https://doi.org/10.1007/s41748-020-00153-x who evaluated SPEI. [Mouhamadou Sylla, Rwanda]	Considered. While these two papers discuss some aspects of droughts, they are not very relevant to the specific discussion of the paragraph.
72115	82	37	83	8	This section only considers model evaluations from CMIP5. It needs to include CMIP6 and CORDEX. [Mouhamadou Sylla, Rwanda]	Accepted - This information is included in the final chapter 11 regional tables.
35081	82	37			It should be acknowledged somewhere in this section that the lack of interactive vegetation in CMIP5 and CMIP6 global models represents a fundamental limitation on the confidence we can place on projections of drought and land surface moisture. [David Gutzler, United States of America]	Accepted - Limitations of models to reproduce these physiological mechanisms by vegetation have been included in the Rejected. 5.1.
71469	82	37			In Chapter 10 we discuss aspects which are relevant here, e.g., the representation of large-scale circulation features such as blocking by different types of models (mainly section 10.3.3.4). There should be a link somewhere in this section. [Douglas Maraun, Austria]	Accepted - mentioned in the revised chapter.
23229	82	39	82	42	This makes very little sense to me as written and anyway adds very little value. I would remove. [Peter Thorne, Ireland]	Accepted - this piece of text has been removed.
102569	82	41	82	41	With regards to dought metrics: This is related to variables, extent of data spatially as well as duration of time series. I suggest adding these points. [Philippe Tulkens, Belgium]	Not applicable - This paragraph has been removed
29923	82	45	83	8	A model evaluation of long-term precipitation deficits in southern South America was performed in the following paper using CMIP6 historical runs: https://doi.org/10.1016/j.atmosres.2020.104953. [Juan Rivera, Argentina]	Accepted - the reference has been included
71449	82	45			In this paragraph, a discussion of shortcomings in the simulation of persistent weather events in the mid-latitudes (dry spells) should be added. This is a key feature of drought. [Douglas Maraun, Austria]	Accepted - mentioned in the revised chapter.
105383	82	47	82	47	I think we need to be careful of assessments of models against observations. If, as the chapter seems to support, most of the variation in precipitation deficits is due to internal variability – then there is not reason to expect any model to reproduce the specific details of what happened, since each model simulation should contain a realization of internal variability that is independent of, and uncorrelated with, the single realization of internal variability that affects the observations. [Francis Zwiers, Canada]	Taken into account - While discrepancies can come from effects of natural variability in observations, some aspects of the general behaviour of models in capturing droughts events (e.g. in terms of frequency) and in reproducing very long term trends are valid to assess possible issues in climate models. In the final text, the sentence has been nuanced by including "generally" before "show limited performance".
98869	82	47	82	54	Bibliography of dry spells model evaluation over Europe: doi:10.1007/s00382-016-3453-4 [Enrique Sanchez, Spain]	Accepted - the reference has been included
72117	83	1	83	8	The part already talks about projections while the section title refers to model evaluation. This paragraph or some elements of it should be moved to the projections section. [Mouhamadou Sylla, Rwanda]	Rejected - It is assessed the spread among models, which is relevant to determine possible robustness in the identification of droughts.
66353	83	5	83	5	conflicting message on model spread with CH12 page 74 line 28-30 [Erika Coppola, Italy]	Accepted - this issue has been coordinated with chapter 12
62573	83	5	83	5	please change "East North America" to "eastern North America" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - Replaced in the revised document
66945	83	8			A regional study has shown that there is a wide range in the ability of CMIP6 models to capture some of the large-scale teleconnections imnportant to drought (Barlow et al., in review). Barlow, M., A. Hoell, and L. Agel, 2020: An evaluation of CMIP6 historical simulations of the teleconnection between tropical Indo-Pacific sea surface temperatures and precipitation in Southwest Asia and the coastal Middle East. In review. [Mathew Barlow, United States of America]	Rejected - The study was not published by the IPCC AR6 WG1 cutoff deadline.
105385	83	12	83	17	There is a term that has crept in here (Epan) that hasn't previously been defined. I assume that the reference is to pan evaporation. I think if you are going to talk about pan evaporation trends then you need to assess the observational data, address the controversies associated with the observed negative trends, and assess the methods used to simulate pan evaporation in climate models. Again, here, it is evident that more than a review is required from the chapter. [Francis Zwiers, Canada]	Accepted: terminology related to the atmospheric evaporative demand has been better described in section 11.6.1.2
126005	83	13	83	15	This appears to be incorrect. Scheff and Frierson analyzed potential evapotranspiration, not Epot/AED. [Trigg Talley, United States of America]	Rejected: the paper uses a metric based on surface resistance with no temporal/spatial variations so it can be considered better a metric of atmospheric evaporative demand and not as potential evapotranspiration, which implies the use of different surface resistance associated to different vegetation types as a function of the albedo, morphology, etc. This is not applied in the cited paper so really they used a metric of potential evaporation.

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10139	83	13	83	15	I am the lead author of the cited study - and I would say the spatial pattern of the model outputs actually did not "resemble" the observations much, especially in the tropics (e.g. South America and Africa) where many models simulated, e.g. a strong ETpot local maximum in the Amazon where in reality there is a local minimum. But at mid/high latitudes I suppose this sentence is more accurate. [Jacob Scheff, United States of America]	Taken into account. In the revised paragraph, it is now first indicated that there is still limited evidence on the evaluation of AED in models ("There is only limited evidence on the evaluation of AED in state-of-the-art ESMs, which is performed on externally computed AED based on model output (Scheff and Frierson, 2015; Liu and Sun, 2016, 2017)."). Furthermore, the following sentence emphasizes the regional discrepancies found in Scheff and Frierson: "the magnitude of potential evaporation displays strong divergence among models globally and regionally". However, it seems justified to highlight that the models' spatial patterns "resemble" the observations in a first approximation.
62575	83	18	83	18	please change “captured well seasonal cycles” to “captured seasonal cycles well” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - replaced in the revised document
23233	83	29			I naively expected this section to include some consideration of behaviour as a function of the realism / complexity of the ESM soil process modelling. Is this missing because there are no studies of this or is this instead an oversight that requires addressing? [Peter Thorne, Ireland]	Accepted - included comments on this issue in the revised document.
101371	83	29			The modelling of soil moisture depend to a large degree on how the soil type (and depth) is represented in the model. If the soil thickness is too deep / shallow the soil moisture content and drying will be influenced accordingly. [Lena M Tallaksen, Norway]	Accepted - included comments on this issue in the revised document.
105387	83	33	83	33	This points to spatial resolution as an issue – but presumably a much bigger issue is the sophistication (or lack thereof) of the land surface models themselves, which remain highly simplified, and the paucity of land surface property data to properly parameterize the land surface models. [Francis Zwiers, Canada]	Accepted - included comments on this issue in the revised document.
79127	83	34	83	34	typo [Andong Shi, Sweden]	Accepted - typo corrected
2443	83	34	83	34	"requires' [Alexis Berg, United States of America]	Accepted - typo corrected
11719	83	34	83	34	delete the “al” at the end of “topographic” [Amy East, United States of America]	Accepted - corrected in the revised document
105389	83	37	83	42	Except for the note on biases, this paints a relatively rosy picture overall, and thus implicitly expresses confidence in available soil moisture data. I’m assuming that performance is mostly being measured against remote sensing products that reflect soil moisture in the upper few centimeters, but is this what is most relevant for drought? Whatever is said here presumably has to be said in the context of what is known about the observations. [Francis Zwiers, Canada]	Accepted - better assessment of the comparison between model's outputs and observations is included
105391	83	47	83	48	Why would I expect there to be any shared variance (my comment concerning page 82, line 47, applies here as well)? Presumably I would only expect a very small fraction to be common in free running models where the only external influence is from anthropogenic, volcanic and solar forcing. I might expect larger fractions of variance to be common in atmospheric models with observed SSTs at the lower boundary, but I would still have to think very carefully about what the common fraction of variance should be, for example, in the interior of a large continent. In short, I wonder if the implied criticism is warranted. Again, an assessment is required rather than a review. [Francis Zwiers, Canada]	Taken into account - This paragraph has been substantially revised and more articles have been assessed.
103909	83	51	83	54	The modelling of soil moisture depend to a large degree on how the soil type (and depth) is represented in the model. If the soil thickness is too deep / shallow the soil moisture content and drying will be influenced accordingly. [Lena M Tallaksen, Norway]	Accepted - included comments on this issue in the revised document.
105393	84	2	84	2	A small editorial comment here is that the word “important” seems to be used relatively often in this section in the French sense of the word (meaning large or substantial). [Francis Zwiers, Canada]	Accepted - wording has been revised.
105395	84	12	84	13	It’s nice to see that there is an assessment at the end of this sub-subsection, but I think one has to say more than just what the assessment is. Instead, I think an argument justifying the assessment should be constructed, drawing on the information presented in the sub-subsection. [Francis Zwiers, Canada]	Taken into account. This section has been expanded. It is now clarified that the assessment of medium confidence is derived as a summary of the presented evidence (which includes some limitations of the models, but also aspects supporting their validity).
62459	84	14	84	14	(e.g., Philip et al., 2017) missing 14 page 84 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - we do not find this references in 84: 14
2445	84	16	84	16	in the interest of clarity, this sub-section should probably explain briefly that streamflows are not directly simulated by climate models (which only simulate runoff) or even by all land surface models, but most often by hydrological models (which are typically driven in a stand-lone manner by observed or simulated climate forcing). [Alexis Berg, United States of America]	Accepted - this has been included in the revised document
105397	84	18	84	28	I think this is a widely experienced problem with hydrologic models (i.e., under estimated low flows) that is of broad concern, so I think more discussion and a wider look at the literature would be warranted. This should, in my view, include an assessment of what we currently know about why this happens. It could, for example, be a problem with representing base flows (and the storage in the deep soil) correctly, or it could be a problem associated with model calibration, which I think tends to weight absolute deviations from observed stream flows (as it must, to respect water balances), at the expense of neglecting what could be large relative errors in the smallest flows of the year. If we are going to use hydrologic models to say something about historical or future changes in low flows, then I think their needs to be enough depth in the discussion and assessment to have some sense of whether there is useful information in the simulated low flow changes. [Francis Zwiers, Canada]	Rejected - the limitations of the models to assess low flows and baseflows is addressed in the text: The simulation of hydrological deficits is much more problematic than simulating mean streamflow or peak flows (Fundel et al., 2013; Staudinger et al., 2015; Stoelzle et al., 2013; Velázquez et al., 2013) since models tend to be too responsive to the climate forcing and they do not satisfactory capture low flows (Tallaksen and Stahl, 2014). We agree that this issue is relevant but the available space is limited so only the main issues are addressed here.
45551	84	21	84	24	At the catchment scale, the hydrological model uncertainty is higher than both GCM and downscaling uncertainty (Vidal et al., 2016) Vidal, J. P., Hingray, B., Magand, C., Sauquet, E., Ducharne, A. (2016) Hierarchy of climate and hydrological uncertainties in transient low-flow projections. Hydrology and Earth System Sciences, 20, 3651-3672, https://doi.org/10.5194/hess-20-3651-2016 [Jean-Philippe Vidal, France]	Accepted - This has been included in the revised document
20265	84	24	84	24	"comprehensive"? [philippe waldteufel, France]	Accepted - removed in the revised document
23235	84	24	84	24	It is not the job of IPCC to decide whether something is comprehensible or not. I suspect you meant comprehensive but even then in what way was it comprehensive such as to deserve being called out in such a manner? This feels a very value-laden judgement. [Peter Thorne, Ireland]	Accepted - removed in the revised document
23237	84	27	84	27	important spread is again value laden - do you mean substantial spread? Importance is dependent upon an individual's frame of reference after all. [Peter Thorne, Ireland]	Accepted - reworded in the revised document
2447	84	38	84	38	A "general increase in the PDSI" would mean a decrease in drought (since droughts correspond to low PDSI values). This should be stated explicitly, as it might be confusing. [Alexis Berg, United States of America]	Accepted - this is an error corrected in the revised document.
62577	84	40	84	40	please change “Northwest US” to “the northwestern US” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - replaced in the revised document.
62453	84	41	84	41	(Cook et al., 2014) missing l reference list Line 41 page 84 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - the reference has been included
62579	84	42	84	42	Pleas ehcange “South North America” to “southern North America” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - replaced in the revised document.

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2449	84	47	84	47	“streamflow deficits”: Climate models do not simulate streamflow directly, so this part of the sentence should be removed here. The next sentence addresses the issue of streamflow changes anyway. [Alexis Berg, United States of America]	Accepted - removed in the revised document
105399	84	48	84	48	The assessments, and how they are obtained from the information that is reviewed, don’t seem to form part of the sub-subsections, so it is difficult to trace the basis for the medium confidence that is assessed here. See, for example, my comment concerning page 84, lines 18-28. Do we really have a basis for assessing medium confidence in stream flow deficits, even at large scales? [Francis Zwiers, Canada]	Rejected: we understand the reviewer's point of view. Models show limitations to simulate soil moisture. There are several uncertainties related to forcing data and the processes involved in the models. Nevertheless, in terms of determining temporal anomalies, basically driven by climate variability and trends, there are studies that show reasonable agreement with soil moisture observations. Moreover, the outputs also show agreement with climatic drought indices, both considering historical observations and model projections. For this reason, we consider that a medium confidence assessment is correct.
126007	84	48	84	50	[CONFIDENCE] The confidence expressed here is perhaps excessive. Ability of a model to reproduce temporal variability of soil moisture and runoff comes mainly from the strong variability of precipitation and its modulation of hydrologic processes. This does not support confidence in the ability of the model to capture correctly the slow, monotonic changes in evapotranspiration driven by the several factors other than precipitation. [Trigg Talley, United States of America]	Rejected: we understand the reviewers' point of view. Models show limitations to simulate soil moisture. There are several uncertainties related to forcing data and the processes involved in the models. Nevertheless, in terms of determining temporal anomalies, basically driven by climate variability and trends, there are studies that show reasonable agreement with soil moisture observations. Moreover, the outputs also show agreement with climatic drought indices, both considering historical observations and model projections. For this reason, we consider that a medium confidence assessment is correct.
117113	84		84		"ability of climate models" : which ones? Check consistency with the assessment done in ch 8 [Valerie Masson-Delmotte, France]	Accepted - GCMs detailed
103913	85	3	85	3	Water scarcity is here mentioned for the first time (if I did not overlook it); should be clearly defined (relate to human water use). [Lena M Tallaksen, Norway]	Accepted - water scarcity has been removed
105401	85	3	85	18	The very first requirement for a detection and attribution study is observations (the question asked in these studies is about observations. And thus while the typology of studies is useful, I would have thought that the starting point would be the observations. At the moment the paragraph only briefly mentions that observations might pose a limitation, and that in the case of drought, there might not be data. [Francis Zwiers, Canada]	Rejected - Assessment based on observations is in 11.6.2
23239	85	3	85	18	This really doesn't feel like it is adding anything useful beyond the sections that follow. I would suggest to remove. [Peter Thorne, Ireland]	Accepted - the paragraph has been removed.
10141	85	3	87	53	There is another long evaluation of this exact same question (attribution of recent drought trends) in Chapter 8 (Section 8.3.1.8), that is framed quite differently and, unfortunately comes to rather different conclusions than this section. Ideally, these two sections should be harmonized and written together so the report is not self-contradictory. I prefer the framing here in Section 11.6.4, that carefully classifies the reported drought effects into precipitation, soil moisture, runoff, etc. However some content/studies from Section 8.3.1.8 may have to added here. [Jacob Scheff, United States of America]	Accepted - Redundancies and inconsistencies between Ch. 8 and 12 have been limited.
101373	85	3			Water scarcity is here mentioned for the first time (if I did not overlook it); should be clearly defined (relate to human water use). [Lena M Tallaksen, Norway]	Accepted - water scarcity has been removed
62581	85	8	85	9	please change “Another type studies focus on long-term trend, most often focusing on soil moisture” to “Another type of study focuses on long-term trends, most often focusing on soil moisture.” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - replaced in the revised document
79129	85	9	85	9	consider rephrasing 'most often...' [Andong Shi, Sweden]	Rejected - this paragraph has been removed
74559	85	10	85	10	to verify if no mistake and to define the acronyme se if it is not defined before some where [Moulay Driss HASNAOUI, Morocco]	Rejected - there is not an acronym in 85:10
71541	85	25	85	25	Remove (García-Herrera, 2019). This reference is not suporting the statement. [Sergio Vicente-Serrano, Spain]	Taken into account - the reference has been removed
71543	85	27	85	29	Remove: "In Europe, human influence was found to have enhanced the magnitude of the 2011-2012 winter drought over the Iberian Peninsula where winter precipitation decreased between the 1960s and 2000s (Trigo et al., 2013; Angélil et al., 2017)." by "In Europe, human influence was found to have enhanced the magnitude of the 2017 drought (García-herrera et al., 2019)". [Sergio Vicente-Serrano, Spain]	Taken into account - the text has been replaced
34723	85	30	85	34	This statement needs more clarification. Please, also refer to my previous general comments (chapters 4 and 10). [Salah Ajjur, Qatar]	Rejected - the comment is unclear
107423	85	31	85	35	No, there is a very clear trend in precipitation deficit records in the Mediterranean region. In Algeria, for example, many researchers have studied precipitation and temperature trends. [Rachda Berrached, Algeria]	Taken into account - The reviewer is not providing the studies to support his rationale, which can thus not be integrated. However, this assessment has been now expanded and highlights that some studies indicate evidence of a trend while others do not support this conclusion. The final assessment is of low confidence in changes in meteorological droughts in Mediterranean region ("The evidence thus leads to an assessment of low confidence in the attribution of observed short-term changes in meteorological droughts in the region (Section 11.9).")
81093	85	31	85	35	There is strong, established evidence that anthropogenic warming is driving increases in meteorological drought over the Mediterranean: https://www.pnas.org/content/112/11/3241.short, https://iopscience.iop.org/article/10.1088/1748-9326/11/4/044005/pdf, https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-11-00296.1, https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-18-0472.1 [Benjamin Cook, United States of America]	Taken into account - Whether there is a precipitation decline in the Mediterranean is not clear, and there are also studies that suggest a main role of the natural variability, which would mask possible anthropogenic influence. This has been clarified and expanded in the text. The final assessment is of low confidence in changes in meteorological droughts in Mediterranean region ("The evidence thus leads to an assessment of low confidence in the attribution of observed short-term changes in meteorological droughts in the region (Section 11.9).")

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
126009	85	32	85	35	An issue here is that it is stated that Vicente-Serrano et al. (submitted) find no clear trend in precipitation over the Mediterranean region during 1850-2018, yet later it is stated that Knutson and Zeng (2018) and Hoerling et al. (2012) find evidence of drying in the region. The drying was stated to be mostly a consequence of enhanced Epot (Vicente-Serrano and others). Actually the drying in Knutson and Zeng (2018) and Hoerling et al. (2012) was based on negative precipitation trends, not enhanced Epot. So why do Vicentee-Serrano et al. (V-S) and Knutson and Zeng (K-Z) differ about the Mediterranean precipitation trends? First, V-S only look at Europe, not at northern Africa and the eastern Mediterranean (Egypt to Syria) where K-Z find the strongest signal. Both studies find reduced precipitation in southeast Europe, so there is agreement there. There is also agreement on only limited evidence for trends in most of southern Europe (Iberian Penisula for example). Since models project (or simulate in historical runs) a reduced precipitation in a large oval-shaped region surrounding the entire Mediterranean, it is important to analyze northern Africa and eastern Mediterranean as well as southern Europe to assess the full "Mediterranean decreasing precipitation" issue. [Trigg Talley, United States of America]	Taken into account - Whether there is a precipitation decline in the Mediterranean is not clear, and there are also studies that suggest a main role of the natural variability, which would mask possible anthropogenic influence. This has been clarified and expanded in the text. The final assessment is of low confidence in changes in meteorological droughts in Mediterranean region ("The evidence thus leads to an assessment of low confidence in the attribution of observed short-term changes in meteorological droughts in the region (Section 11.9).")
10143	85	33	85	35	If the "evidence of drying" is truly "mostly a consequence of the enhanced Epot", then these studies do not belong in this "Precipitation deficits" section - they must go into a different section. [For those studies in which the "evidence of drying" is indeed precipitation-based, they can stay in this section of course.] [Jacob Scheff, United States of America]	Accepted - these studies have been moved to the last section of climatic indices.
105403	85	37	85	37	Suggest being clear that the all of the studies discussed in this paragraph deal specifically with drought in terms of precipitation deficits (so that it is clear that the discussion fits here). [Francis Zwiers, Canada]	Accepted - these studies have been moved to the last section of climatic indices.
11721	85	37	85	37	"greenhouse-gas forcing" [Amy East, United States of America]	Accepted - replaced in the revised document
23241	85	42	85	44	This was the subject of a substantive case study in chapter 10. What is the value of covering redundantly here rather than instead simply cross-referencing? [Peter Thorne, Ireland]	Rejected - we only mention a simple statement on this issue. There is not much redundancy.
62389	85	43	85	43	"… in the western cape region …"-> in the western capetown region [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - replaced in the revised document.
62407	85	44	85	45	Author mentions that there are several studies over East Africa however there is no citation. Examples can include Lyon ad Dewit, 2012, Mwangi et al., 2015, Lott 2013, It will be helpful if this statement is cited "Several studies have focused on recent droughts [illegible] in East Africa." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - new references have been included
108329	85	47	85	48	no evidence for human influence East Africa drought inconsistent with ATLAS page 29 line 48-55 [Nana Klutse, Ghana]	Rejected - The text makes reference to the event attribution and it is not inconsistent with possible trends.
43397	85	53			Read "the 2014 southern Levant drought, Bergaoui et al. (2015) found an anthropogenic " rather than "the 2014 southern Levant drought (Bergaoui et al., 2015) found an anthropogenic " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted - replaced in the revised document
117115	85		85		check the statement that "climate change likey increased the intensitiy of El Nino…" (likely in the sense of the IPCC calibrated language? Confirence in this statement and underlying evidence? (see careful chapters 2-3 on this) [Valerie Masson-Delmotte, France]	Accepted - rewritten in the revised document
81095	86	1	86	7	This whole paragraph needs these (and probably other) citations: https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-14-00616.1, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018GL078312, https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-14-00860.1 [Benjamin Cook, United States of America]	Accepted - two of these references were included.
105405	86	6	86	6	I like the use here of evidence-agreement language (which is the foundation for the IPCC's calibrated uncertainty language). It flags that a further confidence or likelihood assessment can't be made given the evidence available. The evidence-agreement terms could probably be used to advantage more widely in the chapter. [Francis Zwiers, Canada]	Accepted - IPCC confidence language has been carefully checked over the entire chapter.
90675	86	9	86	16	Based on event attribution ensembles of the MIROC5 atmospheric global climate model, Shiogama et al. (2020) suggested that historical anthropogenic warming increased the chances of meteorological droughts exceeding the 2015 observations in equatrial Asia. Shiogama, H., Hirata, R., Hasegawa, T., Fujimori, S., Ishizaki, N. N., Chatani, S., Watanabe, M., Mitchell, D., and Lo, Y. T. E.: Historical and future anthropogenic warming effects on droughts, fires and fire emissions of CO2 and PM2.5 in equatorial Asia when 2015-like El Niño events occur, Earth Syst. Dynam., 11, 435–445, https://doi.org/10.5194/esd-11-435-2020, 2020. [HIDEO SHIOGAMA, Japan]	Accepted - assessment based on the reference has been included.
81097	86	9	86	16	There is now evidence climate change has contributed to the recent Chilean megadrought: https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/joc.6219 [Benjamin Cook, United States of America]	Accepted - assessment based on the reference has been included.
29925	86	12	86	16	I would add two more examples for South America: Boisier, J. P., R. Rondanelli, R. D. Garreaud, and F. Muñoz (2016), Anthropogenic and natural contributions to the Southeast Pacific precipitation decline and recent megadrought in central Chile, Geophys. Res. Lett., 43, 413–421, doi:10.1002/2015GL067265. Boisier, JP, et al. 2018. Anthropogenic drying in central-southern Chile evidenced by long-term observations and climate model simulations. Elem Sci Anth, 6: 74. DOI: https://doi.org/10.1525/elementa.328 [Juan Rivera, Argentina]	Accepted - reference to the central Chile megadrought and two references have been included.
84053	86	13	86	13	The setence "northeast Brazil in 2014 (Otto, et al. 2015)" is wrong. The study of Otto et al. (2015) was in southeast Brazil, instead. We also suggest to include Coelho et al. (2015, DOI: 10.1007/s00382-015-2800-1) [Marco Tulio Cabral, Brazil]	Accepted partially - the reference is not included since it is not related to attribution.
62583	86	13	86	15	please truncate the first citation to " Matrins et al., 2017" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - the reference has been modified
13759	86	13	86	15	Change the format of the Quote to Martins et al., 2017 [Maria Amparo Martinez Arroyo, Mexico]	Accepted - the reference has been modified
15157	86	16			The wording here of "dominant influence" is a bit off. We expect circulation and associated moisture flux anomalies to be the dominant influence of precipitation droughts; Do these studies say anything about the magnitude of climate change influence though? Attribution need not be an issue of what is dominant, but rather how much more severe/likely event X was. [John Abatzoglou, United States of America]	Accepted - the sentence has been reworded.
105407	86	18	86	19	This contradicts page 79, lines 45-47. [Francis Zwiers, Canada]	Not applicable. This paragraph has been revised and condensed and this text is not included anymore. The assessment of changes in meteorological droughts in Australia is provided in the regional table 11.12
9169	86	20	86	20	consider citing Harrington, L. J. et al., 2016: Investigating event-specific drought attribution using self-organizing maps. Journal of Geophysical Research: Atmospheres, 121 (21), 12,766-12,780, doi:10.1002/2016jd025602 [Kevin Hennessy, Australia]	Accepted - the reference has been included
105409	86	25	86	26	It's risky to base a likelihood assessment (implying high confidence and multiple lines of evidence) based on one study. [Francis Zwiers, Canada]	Accepted - assessment has been replaced.
23243	86	31	86	34	This is unclear as written. Do you mean that claims of attribution appear to be dependent upon methodological choices and the models used? If so can you please say so in a manner that is a little clearer to the reader than the present formulation? [Peter Thorne, Ireland]	Accepted - this sentence has been reworded

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
81099	86	39	87	7	There are several new studies that should be cited that have detected and anthropogenic influence on soil moisture trends and drought: https://science.sciencemag.org/content/368/6488/314.abstract, https://www.nature.com/articles/s41586-019-1149-8, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018GL080768, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2015GL064924, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2014GL062433. The following paper also includes a review of drought detection attribution studies since the AR5 that should be included here and elsewhere: https://link.springer.com/article/10.1007/s40641-018-0093-2. [Benjamin Cook, United States of America]	Accepted - all of these relevant studies have been reviewed and included in the assessment
105411	86	40	86	44	It's not obvious that this discussion here is entirely consistent with the assessment of soil moisture deficits in 11.6.3.3 – it seems a bit rosy. [Francis Zwiers, Canada]	Taken into account. We understand the concerns of the reviewer but the assessment also coincides with those provided by drought indices and it is consistent with evidence of enhanced atmospheric evaporative demand that would tend to enhance soil drying and to cause more plant stress. It is also consistent with further assessments based on changes in precipitation minus evaporation in the dry season.
74561	86	45	86	45	conclude that (to separate conclude from that) [Moulay Driss HASNAOUI, Morocco]	Accepted - typo has been corrected.
13761	86	45	86	45	Change concludedthat by concluded that [Maria Amparo Martinez Arroyo, Mexico]	Accepted - typo has been corrected.
43399	86	45			Read " they concluded that observed global soil moisture " rather than " they concludedthat observed global soil moisture " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted - typo has been corrected.
105413	86	54	86	54	We have read much to this point about the reconstructed datasets (I assume that this means things like GLDAS), but clearly they are in a different class with large limitations that should be assessed. [Francis Zwiers, Canada]	Accepted - this discussion has been removed. We understand the concerns by the reviewer but the assessment also coincides with those provided by drought indices and it is consistent with enhanced atmospheric evaporative demand that obviously would dry more the soil and would cause more plant stress.
117117	86		86		recent studies for Australian moisture deficit of years 2019 are missing here [Valerie Masson-Delmotte, France]	Accepted - two references on the attribution of the 2019 Australian mega-fires.
105415	87	3	87	6	I think it would be very hard to have high confidence given all of the concerns. Even the use of the phrase "balance of evidence suggests" alludes to something weaker, meaning that you feel that almost half of the evidence points to a lower level of confidence. I think it's a mistake to try to find a way to make an assessment that is somewhere between medium and high confidence. In effect, this suggests an ability to distinguish as many as 9 levels of confidence – the 5 that are recognized in the uncertainty language guidance and 4 others that are intermediate between those 5 levels. [Francis Zwiers, Canada]	Accepted - The assessment has been substantially revised. The final assessment is for agricultural and ecological droughts, and is based on several lines of evidence, including soil moisture based studies, but also e.g. studies based on changes in precipitation minus evaporation, or indices combining changes in precipitation and atmospheric evaporative demand. The final assessment is provided in Section 11.6.4.5 and is at "medium confidence": "There is medium confidence that human influence has contributed to increases in agricultural and ecological droughts in the dry season in some regions and has led to an increase in the overall affected land area".
74563	87	5	87	5	conclude that … in place of conclude that that … [Moulay Driss HASNAOUI, Morocco]	Accepted - sentence has been modified
62391	87	5	87	5	" .. us to conclude that that is …. " ---> us to conclude that there is a [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - sentence has been modified
81101	87	19	87	29	The Marvel et al studies includes comparisons between PDSI and soil moisture in the models, and demonstrates quite clearly that the spatiotemporal trends in the two compare quite well. Once again, the discussion of PDSI, and the use of PDSI based attribution studies here and in the entire chapter, need to be revised to more accurately reflect the state of the science. [Benjamin Cook, United States of America]	Accepted - this section has been completely revised with a new evaluation of this study.
23245	87	21	87	25	These statements don't make logical sense taken together. One implies a non-detectable signal only in recent decades whereas the other suggests formal detection in the early 20th Century. Both can't simultaneously be true. Is there something I am not getting here? And if so I would probably not be alone in this. [Peter Thorne, Ireland]	Accepted - this section has been completely revised with a new evaluation of this study.
55427	87	21	87	29	In only a few lines, the SOD dismisses the Marvel et al. (2019) study which appeared in one of the worlds top science journals. The arguments used are thin, suggesting that the PDSI is unreliable in monitoring drought response to warming and the remark that drought reconstructions based on proxies are uncertain. There is no evidence given to support the claim that the Marvel et al. study is flawed or that there results are too uncertain to warrant a balanced discussion. Furthermore, the remark that the PDSI has limitations in drought monitoring under warming conditions (note - this applies to observed climates, not projected changes as was discussed earlier in this review) is unsubstantiated. There are no grounds why studies using the PDSI drought metric should be ignored, as is the case with the Marvel et al. study. [Gerard van der Schrier, Netherlands]	Accepted - this section has been completely revised with a new evaluation of this study.
11723	87	23	87	23	"greenhouse-gas forcing" [Amy East, United States of America]	Accepted - replaced in the revised document.
8053	87	28	87	28	first half of the 20th century? [jouni Räisänen, Finland]	Taken into account - yes this is the main finding of the paper.
11725	87	28	87	28	presumably "21st century" here should be "20th century", since you're talking about paleo reconstructions [Amy East, United States of America]	Rejected - the paper refers to the 20th century
126011	87	33	87	53	[CONFIDENCE] The high-confidence statements ignore the opposing force of CO2 fertilization. [Trigg Talley, United States of America]	Rejected - This issue has been widely discussed throughout the chapter. See also chapter 8 and Rejected. on the carbon-water nexus
109959	87	33	87	53	This is a very long and convoluted assessment summary as presently written. The observations portion is quasi-redundant with a prior summary and so arguably not required here. The remainder with some editing could be trimmed without losing meaning. It is questionable whether some of the statements are adequately justified by the preceding assessment text. [Peter Thorne, Ireland]	Accepted - the final assessment summary has been completely rewritten.
11727	87	35	87	35	delete "al" at the end of "dynamic" [Amy East, United States of America]	Accepted - replaced in the revised document.
71545	87	39	87	39	Net radiation? [Sergio Vicente-Serrano, Spain]	Accepted - replaced in the revised document

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
105417	87	40	87	42	I think it would be very hard to have high confidence given all of the concerns. Even the use of the phrase “balance of evidence suggests” alludes to something weaker, meaning that you feel that almost half of the evidence points to a lower level of confidence. I think it’s a mistake to try to find a way to make an assessment that is somewhere between medium and high confidence. In effect, this suggests an ability to distinguish as many as 9 levels of confidence – the 5 that are recognized in the uncertainty language guidance and 4 others that are intermediate between those 5 levels. [Francis Zwiers, Canada]	Accepted - The assessment has been substantially revised. The final assessment is for agricultural and ecological droughts, and is based on several lines of evidence, including soil moisture based studies, but also e.g. studies based on changes in precipitation minus evaporation, or indices combining changes in precipitation and atmospheric evaporative demand. The final assessment is provided in Section 11.6.4.5 and is at "medium confidence": "There is medium confidence that human influence has contributed to increases in agricultural and ecological droughts in the dry season in some regions and has led to an increase in the overall affected land area".
126013	87	43	87	44	The human contribution to decreasing trends in precipitation is more certain in some regions (surrounding the Mediterranean, southwest Australia, probably the Sudan) than is stated here. In these regions it is probably more confident (based on Knutson and Zeng and Hoerling et al, for the Mediterranean; Delworth and Zeng for southwest Australia) than the evaporative demand signal. But in any case, the evidence is pretty strong for human influence on negative precipitation trends in the above regions. [Trigg Talley, United States of America]	Rejected - there are studies that suggest the opposite: a dominant role of the internal climate variability on this issue. This has been addressed in detail in the section 11.6.4.1.
39795	87	44	87	44	"more uncertain" -> Not IPCC confidence language [TSU WGI, France]	Accepted - low confidence has been stated
105419	87	46	87	47	Unfortunately, I don’t have the impression that this section supports an assessment of high confidence. It is possible that there is enough information to support such a finding concerning the direction of change at the global scale, but the way the section has been structured (as a matrix of topics – see my comment concerning page 75, line 4), and the lack of a solid synthesis subsection that draws all of the information together to provide a basis for this assessment, means that it has been left to the reader to find the arguments that support this assessment. [Francis Zwiers, Canada]	Accepted - The assessment has been substantially revised. We have followed the suggestion of the reviewer to include a synthesis section (11.6.4.5). The final assessment for agricultural and ecological droughts is based on several lines of evidence, including soil moisture based studies, but also e.g. studies based on changes in precipitation minus evaporation, or indices combining changes in precipitation and atmospheric evaporative demand. The final assessment is provided in Section 11.6.4.5 and is at "medium confidence": "There is medium confidence that human influence has contributed to increases in agricultural and ecological droughts in the dry season in some regions and has led to an increase in the overall affected land area".
10145	87	47			"drought conditions" must be changed to "soil moisture drought conditions" or "low soil moisture conditions" here, otherwise the public will take this sentence out of context to imply high confidence in human influence on e.g. precipitation drought or streamflow drought, which you do not intend. Similarly, "drying" must be changed to "soil moisture drying". [Jacob Scheff, United States of America]	Accepted - The text has been substantially be revised. The drought assessment is now subdivided between changes in agricultural and ecological droughts (mostly related to soil moisture drought, but also changes in precipitation minus evaporation and indices combining precipitation and potential evaporation), and changes in meteorological droughts, and hydrological droughts.
105421	87	51	87	53	I didn’t have the impression that the section dealt with this topic in any depth – and I’m wondering if it wouldn’t be better simply to be silent and defer to other parts of the AR6 report that deal with wind and circulation change. [Francis Zwiers, Canada]	Accepted - the final assessment summary has been completely rewritten.
15247	88	0	91	0	Drought seems to have become a frequent phenomenon in most regions around the world. Although, the analysis does reflect the increase in drought severity in the future, it would however be good to analyze the projected geographical shift of drought based on the future scenarios (RCP4.5 and RCP8.5) and by how much percent such shift may likely to occur beyond 2050. This will provide a strategic direction to countries towards preparedness and response as well as policy influence. A separate section to this regard would give immense value to the overall chapter in the report. [RISHIRAJ DUTTA, Thailand]	Taken into account - this is exactly what the section includes: changes in drought severity and maps with future projections.
89165	88	1	91	38	Two new studies looking at drought projections in CMIP6 models are Cook et al (2020) and Ukkola et al (2020). Ukkola, A. M., Kauwe, M. G. D., Roderick, M. L., Abramowitz, G., & Pitman, A. J. (n.d.). Robust future changes in meteorological drought in CMIP6 projections despite uncertainty in precipitation. Geophysical Research Letters, n/a(n/a), e2020GL087820. https://doi.org/10.1029/2020GL087820 Cook, B. I., Mankin, J. S., Marvel, K., Williams, A. P., Smerdon, J. E., & Anchukaitis, K. J. (2020). Twenty-First Century Drought Projections in the CMIP6 Forcing Scenarios. Earth’s Future, 8(6), e2019EF001461. https://doi.org/10.1029/2019EF001461 [Angeline Pendergrass, United States of America]	Accepted - these two references have been used for the assessment and cited.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18379	88	1	91	38	While there are uncertainties related to climate models' deficiencies in simulating plants' response to increased CO2 levels and increased water stress, there exists overwhelming evidence that most land areas will become drier in the 21st century due to 1) increased PET as T rises and 2) decreased precipitation over many subtropical land areas. The increased PET is a robust feature in all model projections resulting from increased vapor deficit near the surface (Zhao and Dai 2017). The decerase in subtropical precipitation is also a robust feature in CMIP3 and CMIP5 (Dai et al. 2018) and likely also in CMIP6 models. Thus, there are very good reasons to believe that most land areas will become drier in the coming decades. This conclusion is supported not only by offline calculated aridity or drough indices in many studies (see Dai et al. 2018 for relevant refs. on this), but also supported by top-layer soil moisture changes directly from CMIP3, CMIP5 (and possibly CMIP6) models that included the CO2-induced water use efficiency changes (Zhao and Dai 2015, 2017; Dai et al. 2018). The arguments that deep soil moisture does not dry up that much is irrelevant for most droughts (such as meteorological and agricutural droughts), as drought measures are based on near-surface aridity or dryness, not subsurface conditions. The slower response in subsurface soil moisture is expected because of its longer response time to surface drying. As the time increases in the 22nd century, I would expect the drying to pentrate deeper into the soil layers, but that may take centuries. The other argument that plants's physiological response (including changes in water use efficiency) may compensate the warming-induced drying, leading to no drying or weak drying in the 21st century is also supported by CMIP5 models. For example, Fig. 4 of Dai et al. (2018) clearly shows that CO2's radiative effects domimnate over its physiological effects and the combination of the two would still lead to general drying over most land areas, which is supported by the decreases in top-soil moisture content in these CMIP5 models that already included the plants' physiological response (Zhao and Dai 2015, 2017). One could argue that the CMIP models have major deficiencies in simulating plants' physiological response to CO2, but I can aruge that the CMIP models have all kinds of deficiencies, yet they are still the best tool we have for predicting future climate change! So we have to accept the CMIP model projections with the understanding that they contain large uncertainties. I think the write-up in this section needs to reflect these well documented points regarding model-projected drought changes, rather than vaguely stating that there exist many uncertanties about this and that. Instead, it should focus what we already know, such as the basic change patterns for PET and P and their impacts on drought, as discussed in Dai et al. (2018). Papers cited: Dai, A., T. Zhao, and J. Chen, 2018: Climate change and drought: A precipitation and evaporation perspective. Current Climate Change Reports, 4, 301-312. DOI: 10.1007/s40641-018-0101-6. (http://link.springer.com/article/10.1007/s40641-018-0101-6). Zhao T, Dai A (2015) The magnitude and causes of global drought changes in the 21stcentury under a low–low-moderate emissions scenario. J Clim 28: 4490–4512. Zhao T, Dai A (2017) Uncertainties in historical changes and future projections of drought. Part II: model-simulated historical and future drought changes. Clim Change, 144: 535-548. https://doi.org/10.1007/s10584-016-1742-x [Aiguo Dai, United States of America]	Accepted - The assessment of the CO2 physiological effects has been reorganised and coordinated with Chapters 8 and 5 (new Rejected. on the carbon-water nexus, CC-Box 5.1). The final assessment of these CO2 physiological effects provides a synthesis on the different points of view on this subject.
81103	88	1	91	54	New analyses of drought in the CMIP6 projections have now been published, and results from these studies should be folded into this section: https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2020GL087820, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019EF001461 [Benjamin Cook, United States of America]	Accepted - these two references have been used for the assessment and cited.
70375	88	7	88	9	The wording of this sentence is confusing. The second half of this sentence uses the word “both” but lists only one item. What is different between “reliable model simulations” and “climate model dependency”? Both of these statements seem to be based on the finding that climate models vary widely in their projections of drought, but it’s not at all clear what is meant to be different in these two ways of describing the range of simulated droughts. [Abigail Swann, United States of America]	Accepted - the sentence has been rewritten.
70377	88	9	88	12	"but also vegetation-CO2 feedbacks” is both awkward and less correct than saying something like “as well as plant physiological responses to increasing CO2 [Abigail Swann, United States of America]	Accepted - text has been modified as suggested.
105423	88	12	88	14	Is it within the remit of this chapter, and the WG1 report, to assess risks (which requires an understanding of the consequence drying) or do the authors actually mean probability or likelihood of drying? If so, clarification of language would be in order, I think. [Francis Zwiers, Canada]	Accepted - the sentence has been rewritten
44397	88	13	88	13	replace "risk" with "probability". See IPCC guidance on risk for appropriate use of the term "risk" throughoput the IPCC report. [Jana Sillmann, Norway]	Accepted - the sentence has been rewritten
96143	88	15	88	15	"see Appendix": Which part of the Appendix? [Nicole Wilke, Germany]	Taken into account - reference to the appendix has been removed.
2451	88	19	88	19	Section 11.6.5.1: It would be interesting if this section could indicate whether changes in SPI or CDD simply reflect changes in mean precipitation, or if drought may increase in some regions even thought mean precipitation doesn’t change (simply by changes in the temporal distribution of precipitation). [Alexis Berg, United States of America]	Accepted - this has been included based on recent study by Ukkola et al (2020) using CMIP6 models.
9175	88	19	89	11	No mention of projected precipitation deficits for Australasia. See section 7.2.3 of CSIRO and BoM (2015) Climate change in Australia Technical Report at www.climatechangeinaustralia.gov.au/publications which says "the time indrought is projected to increase over southern Australia (high confidence) and other parts of Australia (low to medium confidence), with a greater frequency of extreme droughts and fewer moderate to severe droughts (medium confidence)". See NZ MfE (2018) Climate Change Projections for New Zealand at http://www.mfe.govt.nz/publications/climate-change/climate-change-projections-new-zealand which projects "Increased drought severity, except for Taranaki-Manawatu, West Coast and Southland. Greatest increases over the northern and eastern North Island and in the lee of the main divide over the South Island" [Kevin Hennessy, Australia]	Accepted - assessment in Australia is provided based on a recent study.
126015	88	21	89	11	[CONFIDENCE] Confidence in the regional precipitation deficit projections are being based too much on model agreement of projections, and not enough on whether the models used for projections are consistent with observed trends over the historical period, and whether there are already detectable negative trends in precipitation in the regions where it is being projected. The regions where there is already a detectable drying over 1901-2010 include: much of the region surrounding the Mediterranean (especially the eastern parts from Egypt through Syria and southeast Europe), parts of northern tropical Africa including the Sudan, extreme southwest Australia and Tasmania, and some island regions including parts of the Caribbean/Bahamas, parts of Japan and Indonesia, and parts of Chile (Knutson and Zeng 2018, Figure 3). Those would be the "hot spot" regions where future precipitation-driven drought increases are most probable. On the other hand, regions like the south-central U.S., where CMIP5 historical runs show a precipitation decrease but observed trends show a precipitation increase, are "user beware" regions, because the models are already off in their projections and have made things too dry over the historical trend period. Southern Africa is another problem area: Projections and historical runs show precipitation decreases, but it is hard to identify large-scale regions with detectable decreases in observations (1901-2010 or 1951-2010; Knutson and Zeng 2018, Figures 3 and 4), meaning that the observed trends are not large relative to natural variability, so lower confidence in projections is called for there. [Trigg Talley, United States of America]	Rejected - The existence of an observed trend is not a necessary condition for confidence in projections given the role of internal variability.
109961	88	21	89	11	This paragraph is far too long to be accessible to the reader. The content needs to be reordered and split into several smaller paragraphs for reader accessability. Presently it feels like a very long proverbial laundry list. [Peter Thorne, Ireland]	Accepted - The paragraph has been reorganised and split between different paragraphs.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
72119	88	32	88	34	This is a misleading information. There is NO lack of agreement in the sign of the changes of CDD in West Africa especially during its main rainy season JJA under RCP85. This should be highlighted. The lengthening of CDD in future climate is of high confidence in West Africa. The existing litterature based in CMIP5 and CORDEX supports it, this literature just needs to be acknowledged. Amongt them they are (in addition of those cited here): 1. Diallo et al. 2016: Projected changes of summer monsoon extremes and hydroclimatic regimes over West Africa for the twenty-first century. Climate Dynamics, DOI: 10.1007/s00382-016-3052-4; 2. Todzo et al. (2020): Intensification of the hydrological cycle expected in West Africa over the 21st century, Earth Syst. Dynam., 11, 319–328, 2020, DOI: https://doi.org/10.5194/esd-11-319-2020; 3. Sylla et al. (2016b): Climate Change in West Africa: Recent Trends and Future Projections. In Joseph A. Yaro and Jan Hesselberg (Eds): Adaptation to Climate Change and Variability in Rural West Africa. Springer, ISBN: 978-3-319-31497-6. DOI: 10.1007/978-3-319-31499-0_3; 4. [Mouhamadou Sylla, Rwanda]	Accepted - sentence has been rewritten and two of the suggested references added.
76691	88	35			you might add that the value of CDD is already larger in the South than in the North Mediterranean and it will increase more in the former than in the latter (rates are about 8days/K and 5days/K, respectively). Lionello, P. and Scarascia L. (2020) The relation of climate extremes with global warming in the Mediterranean region and its North versus South contrast Reg Environ Change 20, doi: 10.1007/s10113-020-01610-z.. The paper shows contrasting trends within the Mediterranean region. [Piero Lionello, Italy]	Accepted - The reference has been included
62585	88	38	88	38	please change “south China...north China” to “southern China...northern China” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - replaced in the revised document
23977	88	40			Replace "area extents" with "areal extents" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - replaced in the revised document
108985	88	42	88	44	Time period for this statement is not clear. [Gemma Teresa Narisma, Philippines]	Accepted - the sentence has been removed.
68521	88	44	88	45	The following paper may be cited here: Nakaegawa, T., A. Kitoh, H. Murakami, and S. Kusunoki. Maximum 5-day Rainfall Total and the Maximum Number of Consecutive Dry Days over Central America in the future climate projected by an atmospheric general circulation model with three different horizontal resolutions. Theoretical and Applied Climatology, 116, Issue 1-2, 155-168: Nakaegawa, T., A. Kitoh, S. Kusunoki, H. Murakami, and O. Arakawa. Hydroclimate change over Central America and the Caribbean in a global warming climate projected with 20-km and 60-km mesh MRI atmospheric general circulation models Papers in Meteorology and Geophysics. 65, 15-33. Kusunoki, S., T. Nakaegawa, R. Pinzón, J. S. Galan and J. R. Fábrega, 29: Future precipitation changes over Panama projected with the atmospheric global model MRI-AGCM3.2. Climate Dynamics, [Tosiyuki Nakaegawa, Japan]	Accepted - the reference has been included
62587	88	46	88	46	please drop the word however [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - the word has been removed
13763	88	50	88	50	Change projected(Chou by projected (Chou [Maria Amparo Martinez Arroyo, Mexico]	Accepted - space added
23979	88	50			Missing space between "projected" and "(Chou" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - space added
62589	88	51	88	51	please change “South Eastern South America and Southern South America” to “southeastern South America and southern South America” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - replaced in the revised document
74565	88	54	88	54	To separate (2015) from identified [Moulay Driss HASNAOUI, Morocco]	Accepted - replaced in the revised document
23981	88	54			Missing space between "(2015)" and "identified" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - replaced in the revised document
66401	88		88		This paper could be revised and use in section 11.6.5.1 Coppola, E., Raffaele, F., Giorgi, F., Giuliani, G., Xuejie, G., Ciarlo, J., et al. (submittedc). Climate hazard indices projections based on CORDEX-CORE, CMIP5 and CMIP6 ensemble. Clim. Dyn. (submitted). [Erika Coppola, Italy]	Accepted. This article is cited in the chapter.
74567	89	1	89	1	to separate drought from in [Moulay Driss HASNAOUI, Morocco]	Accepted - replaced in the revised document.
44041	89	4	89	7	This misses out an important work of Russo (2014), who carried out a multimodel ensemble based projection of the droughts over Europe using non-stationary precipitation index. Results showed that under global warming, climate in Europe will significantly change from its current state with the probability of the occurrence of extreme dry years and seasons increasing over southern dry regions. Therefore, the extreme dry regions of Europe will be more drier in future. Reference: Russo, S., Dosio, A., Sterl, A., Barbosa, P., & Vogt, J. (2013). Projection of occurrence of extreme dry-wet years and seasons in Europe with stationary and nonstationary Standardized Precipitation Indices. Journal of Geophysical Research: Atmospheres, 118(14), 7628-7639. [SABYASACHI SWAIN, India]	Accepted - reference included
66399	89	4	89	9	This paper could be revised and added as a reference Coppola, E., Nogherotto, R., Ciarlo, J. M., Giorgi, F., van Meijgaardm, E., Iles, C., et al. (submitted, a). Assessment of the European climate projections as simulated by the large EURO-CORDEX regional climate model ensemble. J. Geophys. Res. - Atmos. (submitted) [Erika Coppola, Italy]	Rejected - the article is not still accepted and deadline to present the document for review is very close.
98871	89	5	89	5	Bibliograpy of dry spells projections over Europe: https://doi.org/10.1007/s10584-011-0114-9; [Enrique Sanchez, Spain]	Rejected - it is very regional study
98875	89	5	89	5	Bibliograpy of dry spells projections over Europe: https://doi.org/10.1007/s00382-014-2280-8; [Enrique Sanchez, Spain]	Rejected - it is very regional study
13765	89	11	89	11	Change variability(Orth by variability (Orth [Maria Amparo Martinez Arroyo, Mexico]	Accepted - replaced in the revised document
2453	89	11	89	26	lines 19-26: I believe this could benefit from being reorganized and clarified a bit, for instance along those lines: CMIP5 models project an increase in Epot. This increase is primarily driven by the increase in air temperature and radiation over land (see section on observed trends) from greenhouse warming. However, there are at least two problems when inferring the impact of that increase on droughts. First, increases in Epot (defined here as open-water potential evaporation) do not account for the change in plant stomatal conductance (which decreases with increasing atmospheric CO2). Thus, increases in Epot will overestimate the “true” increase in potential evapotranspiration (PET, which is the potential ET of a vegetated surface), which is limited by decreased stomatal conductance. This is also the case for studies that calculate future PET trends while assuming a constant stomatal conductance, as is done is almost all studies on future droughts because of the lack of precise, quantitative knowledge on how stomatal conductance will change under greenhouse warming. Yang et al. (2019) recently proposed a way to account for CO2-induced changes in stomatal conductance when calculating future PET trends. Secondly, another issue is that even if projections of Epot or PET accounted correctly for changes in stomatal conductance, part of the decrease in relative humidity over land that leads to increased Epot is due to feedbacks from limited increase in actual ET from both soil moisture limitation in dry areas (Berg et al. 2016) and from CO2’s negative impact on stomatal conductance in vegetated areas (e.g., (Berg et al. 2016, Swann et al. 2016 and other papers cited here). The increased Epot is thus both a driver and a feedback with respect to changes in ET, complicating the interpretation of it role on drought changes. Consistent with these issues regarding the calculation of future PET, as well as its actual relevance for future trends in actual hydrological variables (ET, runoff, soil moisture), recent studies suggest using that the best estimator of future potential ET may actually simply be net radiation (Milly and Dunne 2016, cited). [Alexis Berg, United States of America]	Taken into account - The assessment has been substantially revised.
81105	89	14	89	37	There is also evidence that, in some areas, that vegetation responses to climate and CO2 will actually increase total vegetation water use and contribute to declines in soil moisture and runoff: https://www.nature.com/articles/nclimate2831, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017GL072759, https://www.nature.com/articles/nclimate2614, https://www.nature.com/articles/s41561-019-0480-x [Benjamin Cook, United States of America]	Accepted - but not in this section. This section only focusses on the projections of atmospheric evaporative demand. The mention of stronger water demand by more dense vegetation has been included in the Rejected. of the carbon-water nexus.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70379	89	18	89	20	This statement obscures the counteracting roles of CO2 fertilization and water use efficiency. The eventual impact of Epot under higher CO2 conditions emerges as the resulting balance between the response of stomatal closure (which leads to increased water use efficiency and less ET) and leaf area growth (which leads to more ET). The references listed already cover this concept. We suggest the following wording "The role of the Epot on drought severity in future projections may vary considering physical and plant physiological processes, including the possible role of CO2 fertilization increasing leaf area and stomatal closure leading to increases in vegetation water use efficiency. [Abigail Swann, United States of America]	Noted. Physiological effects are included and discussed in the assessment, and a new Rejected. (cc-box 5.1) between chapters 5, 8, and 11 provide a detailed assessment on this question.
9171	89	23	89	25	Add "There is high confidence in increasing potential evaporation over Australia" based on section 7.6 in CSIRO and BoM (2015) Climate change in Australia Technical Report at www.climatechangeinaustralia.gov.au/publications [Kevin Hennessy, Australia]	Rejected - the proposed web site does not work.
70381	89	23	89	25	The combination of the first two sentences here does not make sense, especially the word "nonetheless" which implies that the second sentence is at odds with the first and this is not the case. We suggest the following wording: "Soil moisture also contributes to these trends through effects on ET and land-atmosphere feedbacks (Berg et al., 2016; Teuling, 2018) with increases in ET limited compared to the increased Epot due to soil moisture limitation (Berg et al., 2016), with implications for hydrological drought projections. [Abigail Swann, United States of America]	Accepted - replaced in the revised document
70411	89	26	89	26	This statement conflates multiple plant responses to increasing CO2 as "CO2 fertilization," which does not reflect the standard terminology in the literature. Plant responses to CO2 can influence land evapotranspiration through two mechanisms, which have opposite effects on evapotranspiration: (1) increased rates of photosynthesis and increases in leaf area, which increase transpiration, and (2) stomatal closure, which decreases transpiration. The term "CO2 fertilization" generally only refers to the first process. The term "plant physiological responses to increasing CO2" can encompass both CO2 fertilization and stomatal closure. In order for this section to remain clear to the diverse scientific communities interested in projected changes in drought, we recommend outlining each of these two opposing processes and explicitly defining which processes are included in different terms. [Abigail Swann, United States of America]	Accepted - the text now clarifies that the physiological CO2 effects refer to the control on stomatal conductance and water use efficiency. See also CC Box 5.1
70383	89	26	89	26	This statement is incorrect because it suggests that plant physiological responses to CO2 reduce Epot (i.e. the atmospheric demand for water), rather than ET (i.e. the actual flux of water from the land to the atmosphere). Based on our knowledge of the subject and the subsequent text, we believe that the authors mean that plant responses to CO2 could reduce actual ET in vegetated areas (i.e. partially counteract increases in Epot). If so, we recommend rephrasing this sentence to clarify this point. [Abigail Swann, United States of America]	Accepted - the sentence was incorrect and it is removed in the revised document.
70385	89	26	89	31	The statement that evapotranspiration is projected to strongly increase is not supported by results from ESMs - evapotranspiration does not strongly increase and in fact decreases slightly under higher CO2 in ESM simulations in the tropics and subtropics, especially in the Amazon. This can be seen clearly in Figure 8.18 from this report, as well as in Swann et al. 2016 Figure 2 and Figure S2, and Zarakas et al. (in review, preprint doi:10.31223/osf.io/emgxb) Figure S7. It is also unclear why maximum evapotranspiration is the relevant metric, how it is defined, or where it is analyzed in the literature. The reference cited - Vicente-Serrano et al. 2020 - does not appear to discuss trends in evapotranspiration or maximum evapotranspiration. [Abigail Swann, United States of America]	Accepted - the revised text in this section does not refer to possible changes in evaporation and only discusses projections of atmospheric evaporative demand.
70387	89	26	89	31	This wording diminishes the importance of plant responses (dubbed by the authors "fertilizing CO2 effects") by suggesting that because they do not *entirely* compensate for increased atmospheric demand they are somehow unimportant. A better framing would demonstrate that because there is uncertainty in the individual plant responses and thus the exact balance between competing aspects of plant control over water flux they remain a critical uncertainty for determining projections of drought in the future. [Abigail Swann, United States of America]	Taken into account. The text has been substantially revised, and a new Rejected. (CC-Box 5.1), prepared by authors from chapters 5, 8 and 11, provides a detailed assessment on the question of water-carbon relationships in plants. The conclusions of this Rejected. are referred to in Section 11.6.
70389	89	26	89	31	This term "fertilizing CO2 effects" conflates multiple plant responses to increasing CO2, and does not reflect the standard terminology in the literature. Plant responses to CO2 can influence land evapotranspiration through two mechanisms, which have opposite effects on evapotranspiration: (1) increased rates of photosynthesis and increases in leaf area, which increase transpiration, and (2) stomatal closure, which decreases transpiration. The term "CO2 fertilization" generally only refers to the first process. The term "plant physiological responses to increasing CO2" can encompass both CO2 fertilization and stomatal closure. In order for this section to remain clear to the diverse scientific communities interested in projected changes in drought, we recommend outlining each of these two opposing processes and explicitly defining which processes are included in different terms. [Abigail Swann, United States of America]	Accepted - the text now clarifies that the physiological CO2 effects refer to the control on stomatal conductance and water use efficiency. See also CC Box 5.1
62591	89	28	89	28	please change "South North America" to "southern North America" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - replaced in the revised document
11729	89	34	89	35	awkward, fix subject-verb agreement, e.g., to "Moreover, a number of ecophysiological and anatomical processes may reduce the role of CO2…" [Amy East, United States of America]	Accepted - replaced in the revised document
37619	89	34	89	37	there ARE a number of ……. [Timothy Brodribb, Australia]	Accepted - replaced in the revised document
70391	89	34	89	37	This statement conflates multiple plant responses to increasing CO2 as "CO2 fertilization," which does not reflect the standard terminology in the literature. Plant responses to CO2 can influence land evapotranspiration towards two mechanisms, which have opposite effects on evapotranspiration: (1) increased rates of photosynthesis and increases in leaf area, which increase transpiration, and (2) stomatal closure, which decreases transpiration. The term "CO2 fertilization" generally only refers to the first process. The term "plant physiological responses to increasing CO2" can encompass both CO2 fertilization and stomatal closure. In order for this section to remain clear to the diverse scientific communities interested in projected changes in drought, we recommend outlining each of these two opposing processes and explicitly defining which processes are included in different terms. [Abigail Swann, United States of America]	Accepted - the text now clarifies that the physiological CO2 effects refer to the control on stomatal conductance and water use efficiency.
62363	89	42	90	3	The level of confidence is not included. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. Some confidence statement is included in a new summary paragraph in FGD.
62593	89	43	89	43	did you mean to say "although there is substantial consistency in the respective patterns"? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - replaced in the revised document
43401	89	43			Read "although there is substantial consistency in the respective patterns" rather than "although there substantial consistency in the respective patterns" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted - replaced in the revised document
96145	89	44	89	44	"10 cm": Presumably this is a more or less conceptual figure. Can be skipped. [Nicole Wilke, Germany]	Accepted - removed in the revised document
13767	89	44	89	44	Change 10 cm., by 10 cm, [Maria Amparo Martinez Arroyo, Mexico]	Rejected - text removed in the revised document
109963	89	44	89	51	Why is one region given a timescale and others not? The reader is left to guess when the effect may kick in in other regions as a result of calling out a specific period when south N. America experiences this. [Peter Thorne, Ireland]	Accepted - reference to 2050 is removed in the document
9173	89	44	89	51	Add "southern Australia" based on section 7.7.1 in CSIRO and BoM (2015) Climate change in Australia Technical Report at www.climatechangeinaustralia.gov.au/publications which says "there is high confidence in decreasing soil moisture in southern Australia (particularly in winter and spring) driven by the projected decrease in rainfall and higher evaporative demand. There is medium confidence in decreasing soil moisture elsewhere in Australia where evaporative demand is projected to increase but the direction of rainfall change is uncertain" [Kevin Hennessy, Australia]	Accepted - included southern Australia and reference included

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
62393	89	47	89	47	" …. South North America by 2050 ..." is this correct or it is only North America? Please check, thanks. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - reference to 2050 has been removed.
62595	89	47	89	57	please change “South North America” to “southern North America” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - replaced in the revised document
62365	90	1	90	1	Can you specify in which areas the signal to noise ratio among models is low? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - thus the sentence contained an error corrected in the revised manuscript.
2455	90	6	90	6	I believe this sub-section section should mention the fact that many of the hydrological studies in this sub-section are based on offline hydrological models forced by GCM outputs, and thus suffer from the same issue as PET-based metrics calculated directly from GCM outputs (PDSI, SPEI), in that they are forced by PET calculations (from GCM outputs) that don’t account for CO2-induced changes in stomatal conductance. Thus, they might overestimate increases in ET and corresponding decreases in runoff. See Milly and Dunne 2016 (cited in the report) and 2017. Milly, P. C. D., & Dunne, K. A. (2017). A hydrologic drying bias in water-resource impact analyses of anthropogenic climate change. JAWRA Journal of the American Water Resources Association, 53(4), 822-838. [Alexis Berg, United States of America]	Taken into account. The CO2 physiological effects are addressed in the Rejected. 5.1 which is referred to in the revised version of Section 11.6. The assessment is of low confidence of regarding CO2 physiological effects for drought responses and land water budgets under enhanced CO2 concentrations: "Increased CO2 concentrations alleviate the effects of water deficits on plant productivity (medium confidence) but there is low confidence on its role under extreme drought conditions." and "There is low confidence that increased WUE by vegetation will substantially reduce global plant transpiration and diminish the frequency and severity of soil moisture and streamflow deficits associated with the radiative effect of higher CO2 concentrations"
109967	90	8	90	41	It feels like this paragraph could be better ordered and also may benefit from being split into two or more shorter paragraphs for readability. The mountain snow pack piece feels like a separate subject and hence paragraph? [Peter Thorne, Ireland]	Accepted - the original paragraph has been restructured.
45561	90	11	90	15	The frequency of low flows is projected to increase over most of the continents, with regionally and seasonally differentiated effects. Hot-spots include the Mediterranean and Southern America with a high signal-to-noise ratio (Giuntoli et al., 2015). Giuntoli, I., Vidal, J.-P., Prudhomme, C., and Hannah, D. M. (2015) Future hydrological extremes: the uncertainty from multiple global climate and global hydrological models, Earth System Dynamics, 6, 267–285, https://doi.org/10.5194/esd-6-267-2015 [Jean-Philippe Vidal, France]	Accepted - this has been mentioned in the revised document and the reference included.
82801	90	14	90	14	Does this refer to southern Australia in general or the state of South Australia? Also affects P90 L54 and P91 L31. [Blair Trewin, Australia]	Accepted - Correct word is southern Australia
9177	90	14	90	14	replace "South Australia" with "southern Australia" because the former is a State within southern Australia, and the projected increases in drought affect the latter. Include eastern and northern New Zealand based on NZ MfE (2018) Climate Change Projections for New Zealand at http://www.mfe.govt.nz/publications/climate-change/climate-change-projections-new-zealand. [Kevin Hennessy, Australia]	Accepted - Correct word is southern Australia
62597	90	17	90	17	please change “Nothern China” to either “North China” or “northern China” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - replaced in the revised document
62599	90	20	90	20	please change “north and northeast Europe” to “northern and northeastern Europe” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - replaced in the revised document
105425	90	30	90	30	I wonder if medium confidence is actually too high given all of the caveats and the difficulties in well simulating low flows, even in the present climate. [Francis Zwiers, Canada]	Taken into account. The assessment has been substantially revised. In most regions, there is low confidence in projected changes in hydrological droughts. However, in some regions, several studies are available for the assessment, including in some cases observed evidence of hydrological drought changes, allowing higher confidence (e.g. Mediterranean)
45559	90	34	90	40	In the Southern European Alps, both winter and summer low flows are projected to be more severe, with a 25 % decrease in the 2050s (Vidal et al., 2016). Vidal, J.-P., Hingray, B., Magand, C., Sauquet, E., Ducharne, A. (2016) Hierarchy of climate and hydrological uncertainties in transient low-flow projections. Hydrology and Earth System Sciences, 20, 3651-3672, https://doi.org/10.5194/hess-20-3651-2016 [Jean-Philippe Vidal, France]	Accepted - A sentence has been included and the suggested reference added.
44399	90	35	90	35	replace "risk" with "probability". See IPCC guidance on risk for appropriate use of the term "risk" throughoput the IPCC report. [Jana Sillmann, Norway]	Accepted - this sentence has been reworded
62461	90	38	90	38	Rhoades et al 2018 lie 38 page 90 missing in reference list [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - reference has been included
62411	90	40	90	40	Im concerned with the fact that the authors seem to be switching South Africa ad Southern Africa for example in this sentence South Africa was used however the paper focuses on Southern Africa (Abiodun et al., 2019). It will definitely be helpful if the authors are able to differetiate betwee Southern Africa and South Africa as the former is a region ad the later is a country. In the executive summary it is clearly stated Souther Africa however in the main document there seems to be a confusion on the two. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - Replaced in the revised document
106757	90	44	91	5	Drought conditions also strenghten over other parts of Africa: - East Africa (Nguvava et al 2019: https://doi.org/10.1016/j.atmosres.2019.05.008) and also in - - Western africa (Ayaji et al, 2020: https://doi.org/10.1007/s41748-020-00153-x) [Moustapha Tall, Rwanda]	Accepted - references included in the revised document
70393	90	46	90	49	The paragraph starts with several sentences describing results from drought indices that are dominated by the response of atmospheric demand to a changing climate before mentioning caveats. The limitations of this approach need to be mentioned in the first sentence before discussing results which have been demonstrated to be incomplete (i.e. Berg and Sheffield 2018). [Abigail Swann, United States of America]	Rejected - although this section has been mostly rewritten, and clarified better. Further assessment on the CO2 effects in Chapter 8 and the Rejected. 5.1.
72121	90	46	91	1	There is an reinforcement of drought in West Africa too: Ajayi, V.O., Ilori, O.W. Projected Drought Events over West Africa Using RCA4 Regional Climate Model. Earth Syst Environ (2020). https://doi.org/10.1007/s41748-020-00153-x [Mouhamadou Sylla, Rwanda]	Accepted - reference included in the revised document
130563	90	47	90	47	Epot has not been used many times, please consider to spell out. [Panmao Zhai, China]	Accepted - replaced by atmospheric evaporative demand.
9179	90	54	90	54	replace "South Australia" with "southern Australia" because the former is a State within southern Australia, and the projected increases in drought affect the latter. [Kevin Hennessy, Australia]	Accepted - replaced in the revised document
62601	91	1	91	1	please change “South North America” to “southern North America” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - replaced in the revised document
62395	91	1	91	1	"….South North America…" again here means south of north america. Just need little clarification, thanks. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - replaced in the revised document

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70413	91	1	91	16	This section is dismissive of the fact that plant physiological responses to increasing CO2 alter water needs of plants, water availability on land, and therefore the onset and occurrence of drought. This paragraph (and the chapter more broadly) conflates multiple factors into the term “fertilizing CO2 effects” or “CO2 fertilizing effects. This is highly misleading because there are multiple processes which lead to the eventual plant physiological response to CO2 which work in opposition to one another. The science in this chapter would be more correct, and more clear to readers, by explicitly discussing both aspects of plant physiological responses: stomatal closure and leaf area growth. Stomatal closure can occur without CO2 fertilization and works to reduce water loss from plants regardless of soil moisture stress. Leaf area growth, driven by increased plant photosynthesis (i.e. CO2 fertilization), leads to more water loss. It is the balance of these two opposing factors that determines plant controls over ET, and thus the decoupling of ET from PET as CO2 increases. These effects *are* very uncertain, but that does not mean that they can be ignored as they dramatically change the projection of expected water stress on land. In fact this further compounds the challenges of projecting drought under future climate conditions. The point of view of this paragraph does not reflect this aspect of the literature (e.g. Roderick et al. 2015, Milly and Dunne 2016, Swann et al. 2016, Berg and Sheffield 2018, Lemordant et al. 2018, Swann 2018, Sheff 2018) instead favoring one viewpoint that it’s fine to use metrics of drought based on PET. [Abigail Swann, United States of America]	Taken into account. This section has been fully modified according to different reviewers' comments. We note that the topic highlighted by the reviewer is now addressed in detail in the Rejected. 5.1, prepared by chapters 5, 8 and 11 and cited in the revised Section 11.6. The conclusion of this Rejected. is that there is low confidence that CO2 physiological effects affect drought responses: "Increased CO2 concentrations alleviate the effects of water deficits on plant productivity (medium confidence) but there is low confidence on its role under extreme drought conditions. There is low confidence that increased WUE by vegetation will substantially reduce global plant transpiration and diminish the frequency and severity of soil moisture and streamflow deficits associated with the radiative effect of higher CO2 concentrations."
70415	91	1	91	16	This term “fertilizing CO2 effects” conflates multiple plant responses to increasing CO2, and does not reflect the standard terminology in the literature. Plant responses to CO2 can influence land evapotranspiration through two mechanisms, which have opposite effects on evapotranspiration: (1) increased rates of photosynthesis and increases in leaf area, which increase transpiration, and (2) stomatal closure, which decreases transpiration. The term “CO2 fertilization” generally only refers to the first process. The term “plant physiological responses to increasing CO2” can encompass both CO2 fertilization and stomatal closure. In order for this section to remain clear to the diverse scientific communities interested in projected changes in drought, we recommend outlining each of these two opposing processes and explicitly defining which processes are included in different terms. [Abigail Swann, United States of America]	Taken into account: In general, the report has tried to better clarify this issue. First with the Rejected. on the carbon-water nexus (CC-box 5.1). Secondly in the new Figure in Ch 8 the two physiological CO2 effects are considered separately.
10147	91	1	91	16	This discussion needs to explicitly acknowledge that these model-simulated SPEI and PDSI trends are not at all consistent with the model-simulated water availability, runoff, and vegetation trends, likely due in part to neglect of CO2 physiological effect (e.g. Swann et al. 2016, Scheff et al. 2017, Scheff 2018.) This is clear in section 11.6.5.2 but is almost completely omitted here in section 11.6.5.5, except a brief and tangential reference on line 10 here. [Jacob Scheff, United States of America]	Taken into account: We have noticeably improved this section, trying to reconcile the points of view by different reviewers on this issue. We have clarified what the climatic drought indices are representing and also that it is necessary to consider the uncertainties related to the physiological CO2 effects (See also Rejected. on carbon-water nexus CC-Box 5.1 and Chapter 8 section 8.2.3.1). Section 11.6.1.5 also clearly states that "These demand/supply indices are not intended to be metrics of soil moisture, streamflow or vegetation water stress"
13769	91	7	91	7	Change .Milly by . Milly [Maria Amparo Martinez Arroyo, Mexico]	Accepted - replaced in the revised document
43403	91	15			Read " In addition, the fertilizing CO2 effects on vegetation " rather than " In addition, the the fertilizing CO2 effects on vegetation " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted - replaced in the revised document
103917	91	18	91	38	Is an effort made to compare observed trends with projections? [Lena M Tallaksen, Norway]	Rejected - Trends in climatic drought indices are included in 11.6.2.
105427	91	20	91	21	On the one hand, it seems that this has to be true and that an assessment of “high confidence” might actually be an understatement. Global mean precipitation increases with warming, and so this must be balanced by evaporation (ie., global atmospheric demand must go up). On the other hand, the statement is presumably about atmospheric demand over land (i.e., on ~30% of the globe). So, I think the argument to support this assessment does need to be discussed. This also points to an issue that is implicit throughout this section – it presumably should be made clear that the assessments of the water balance components through which drought is reflected concern land areas only, i.e., that they are not truly global. [Francis Zwiers, Canada]	Accepted - land areas have been included to explain where the AED is projected to increase.
109969	91	20	91	38	This is a very long assessment summary again and could be simplified. I am not sure that the preceding assessment text necessarily provides a clear and direct line of sight to some of these conclusions being drawn here. This may be because the sections themselves tend to consist of very long paragraphs making it hard to see the woods for the trees. Use of shorter paragraphs may improve traceability to support this bottom-line assessment. [Peter Thorne, Ireland]	Accepted - we have shortened a bit this paragraph
84913	91	20	92	7	The capsule summary of previous reports is not consistently present in other sections. Although it's useful, it would be better to be consistent across sections. [Turner Jessica, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - This is the summary for the entire projection section.
66355	91	21	91	24	confidence in drought projection show conflicting message compared to CH12. [Erika Coppola, Italy]	Accepted - this issue has been coordinated with Ch 12
126017	91	23	91	24	[CONFIDENCE] Confidence in the regional precipitation deficit projections are being based too much on model agreement of projections, and not enough on whether the models used for projections are consistent with observed trends over the historical period, and whether there are already detectable negative trends in precipitation in the regions where it is being projected. The regions where there is already a detectable drying over 1901-2010 include: much of the region surrounding the Mediterranean (especially the eastern parts from Egypt through Syria and southeast Europe), parts of northern tropical Africa including the Sudan, extreme southwest Australia and Tasmania, and some island regions including parts of the Caribbean/Bahamas, parts of Japan and Indonesia, and parts of Chile (Knutson and Zeng 2018, Figure 3). Those would be the "hot spot" regions where future precipitation-driven drought increases are most probable. On the other hand, regions like the south-central U.S., where CMIP5 historical runs show a precipitation decrease but observed trends show a precipitation increase, are "user beware" regions, because the models are already off in their projections and have made things too dry over the historical trend period. Southern Africa is another problem area: projections and historical runs show precipitation decreases, but it is hard to identify large-scale regions with detectable decreases in observations (1901-2010 or 1951-2010; Knutson and Zeng 2018, Figures 3 and 4), meaning that the observed trends are not large relative to natural variability, so lower confidence in projections is called for there. The evidence is also not very compelling for projecting greater precipitation deficits over Central America and northeast Brazil, again because there is not clear evidence for detectable human-caused decreases in precipitation in those regions. The confidence is therefore much lower there than for the precipitation decrease "hot spots" mentioned above. [Trigg Talley, United States of America]	Taken into account. Uncertainties related to disagreements between long-term observations and models are accounted in the assessment, if they are larger than what can be expected from natural variability.
105429	91	24	91	24	This section would benefit from much more of this kind of justification for the assessments that are made. [Francis Zwiers, Canada]	Rejected - we think the place to describe this is the final summary
62367	91	26	91	28	In page 91, form line 15 to line 16, you say that "the fertilizing CO2 effects on vegetation processes under limited soil moisture are very uncertain". Wouldn't it be more appropriate to consider that there is low confidence given that the effects are uncertain? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - this section has been rewritten.
70395	91	28	91	28	“physiological CO2 effects on plants’ transpiration” is very awkward wording to describe this process. We suggest the wording “plant physiological responses and resulting reductions in transpiration” [Abigail Swann, United States of America]	Accepted - rewritten in the revised manuscript.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
126019	91	28	91	30	Is this statement for near-surface soil moisture or for root-zone soil moisture? Their behaviors differ, as shown by Berg et al. (2017). [Trigg Talley, United States of America]	Rejected - in general both show dryness in comparison to precipitation trends.
9181	91	31	91	31	replace "South Australia" with "southern Australia" because the former is a State within southern Australia, and the projected increases in drought affect the latter. [Kevin Hennessy, Australia]	Accepted - replaced in the revised document
126021	91	32	91	34	These drought indices are undermined by absence of the effect of CO2 fertilization. [Trigg Talley, United States of America]	Rejected - this issue is widely discussed in different parts of the drought section. I would also refer the reviewer to Chapter 8 and the Rejected. on the carbon-water nexus.
70397	91	33	91	34	There is a large uncertainty in drought projections using metrics based on Epot because plant physiological responses to CO2 lead to a decoupling of ET from PET which is not mentioned as a caveat here (Milly and Dunne 2016, Swann et al. 2016, Berg and Sheffield 2018, Swann 2018, Lemordant et al. 2018). [Abigail Swann, United States of America]	Rejected: There is large uncertainty in the possible physiological CO2 effects in future scenarios given the overlap of these physiological effects with the possible CO2 radiative effects, and also as consequence of the important limitations of the GCMs. See also Rejected. 5.1 and Chapter 8 for an assessment on these effects.
39829	91	37	91	37	"some regions" - > Which regions? [TSU WGI, France]	Accepted - detailed in the revised document
107425	91	41	92	2	I think that figures 11.20 and 11.21 should be just after the paragraph where they were being citing (at page 90, line 3). [Rachda Berrached, Algeria]	Editorial - the final position of the figure will be decided during the producation of the report
2457	91	52	92	52	Figure 11.21: This shows the mean change in soil moisture. I believe this is more relevant to the chapters on climatic projections (e.g., chapter 4 on global climate projections, or chapter 8 on water cycle changes): this being the drought section, perhaps it would be better to show a metric of changes in low values or low quantiles of SM, or changes in soil moisture anomalies, or something more characteristic of dry events (or perhaps the change in the mean along with changes in quantiles). [Alexis Berg, United States of America]	Accepted. Changes in the frequency of droughts have been included in Fig. 11.19
101375	91				Is an effort made to compare observed trends with projections? [Lena M Tallaksen, Norway]	Rejected - Trends in climatic drought indices are included in 11.6.2.
41059	92	5	109	40	There is only one figure in the entire section on extreme storms, which is a qualitative one. Consider adding more figures. [TSU WGI, France]	Noted. Considering the balance we do not need more figures in this section.
77701	92	5	109	40	Feser et al., (2015) DOI:10.1002/qj.2364 provide a very comprehensive review of trends in storminess across regions and across models and support the low confidence in past changes in storminess. May I suggest that it be referenced? [Emer Griffin, Ireland]	Taken into account. The reference is a added to the section on ETC (11.7.2).
109971	92	7	92	8	While ARs are associated with storms they really are extreme rainfall events so is there not a substantive risk of double counted assessment here? Also, ARs were assessed in depth in chapter 8 where their assessment likely best belongs given remit and that not all ARs may be associated with a climate extreme impact? [Peter Thorne, Ireland]	Noted. We agree that ARs pose a challenge for the reasons the reviewer has stated. It's not clear how best to categorize them. Our solution was to include them focusing on extreme aspects (of rain) without too much detail, and refer to Section 8.3.2.8.2.
126023	92	7	92	14	"rare", "short-lived," and "local" should be better characterized or explained. These will have different connotations depending upon one's frame of reference. [Trigg Talley, United States of America]	Noted. This is a valid point. In this case though, we feel that the reader would have some familiarity with what these events are, and would have an understanding of these terms in this context.
102571	92	8	92	8	Suggest to add "can" -> "can often have" [Philippe Tulkens, Belgium]	Rejected. We feel that this change doesn't improve clarity or readability, and we prefer to not add words whenever possible.
62467	92	8	92	10	"...because extreme storms are rare". The word rare is not self-explanatory. It is better to give a typical return period or range of extreme storms that exceed a certain threshold of a given parameter such as precipitation. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. This is a valid point. In this case though, we feel that the reader would have some familiarity with what these events are, and would have an understanding of these terms in this context.
114971	92	8	92	10	please, replace this long sentence "Quantifying the relationship between climate change and extreme storms is challenging, partly because extreme storms are rare, short-lived, and local, and individual events are largely influenced by stochastic variability." with a shorter and more clear sentense, FOR EXAMPLE: "Climate Change affects the intensity, location and frequence of storms via a combination of factors driving storm formation" [Elena Maksimovich, France]	Noted We feel that the sentence, while perhaps longish, is clear and useful for context.
114973	92	8	92	10	please, replace RELATIONSHIP with EFFECT OF the Climate Change . Otherwise the phrase "Quantifying the RELATIONSHIP between climate change and extreme storms" could be understood such that storms affect climate change. The term "relashionship" is two a way mechanism. Since stroms do not effect Climate Change, the term "relationship" is not correct here [Elena Maksimovich, France]	Taken into account. We changed the text as suggested.
114975	92	8	92	10	Please, remove "are largely influenced by stochastic variability" part of the phrase. It is a heavy detail, unnecessary for a reader. It doesn't help the reader to understand better the idea. [Elena Maksimovich, France]	Rejected. We disagree. It has a meaning here and we prefer to leave it in.
1489	92	10	92	11	How is it difficult to miss the really big violent storms? Widespread destructions, storm surges, and waves are usually felt by many. There are many ways to represent storms, in addition to the maximum wind speed and the central pressure. The number of storms ought to follow a Poisson distribution over a fixed interval (e.g. a year or a decade) in a stable climate, albeit with some degree of clustering (over- and underdispersion). The interval can be extended in order to get some decent samples in order to reduce the random sampling fluctuations. [Rasmus Benestad, Norway]	Noted. Our purview here is to assess the literature. This comment, while valid, is beyond this purview.
114977	92	10	92	12	Please, remove the first part of this sentence : "The high degree of random variability makes detection and attribution of extreme storm trends more uncertain than .... " This first part is WRONG. There exist methods to estimate the cumulative storm (cyclone, hurricane) power! Counting of storms works well also, especially for tropical storms (with just one core). You could replace this phraze with the following, for example : "While detection and attribution of tendencies in extreme storm occurrence and intensity depend on method choice, more solid conclusions come from analysis of the environmental background in which the storms evolve, e.g., larger-scale temperature trends. [Elena Maksimovich, France]	Rejected. We respectfully disagree. It is not wrong to state that the signal-to-noise ratio in extreme storm metrics is greater than larger-scale environmental phenomena such as GMST changes. This has little to do with the metric itself, whether its storm frequency or accumulated cyclone energy, etc.
42539	92	15	92	15	Please check: good progress has been since the AR5 -> good progress has been made since the AR5 ? [Joan Bech, Spain]	Accepted. We have corrected this typo.
11731	92	15	92	15	sounds like a word is missing: “progress has been made since the AR5.” [Amy East, United States of America]	Accepted. We have corrected this typo.
70167	92	15			A word is missing, i.e. progress has been "made" [Huanping Huang, United States of America]	Accepted. We have corrected this typo.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
109973	92	17	93	7	Almost a page is hardly a capsule. Surely the key points can be distilled into a considerably smaller abstraction than is presently done here? [Peter Thorne, Ireland]	Noted. We're not aware of any formal length restriction to what a capsule is. We are summarizing four complete assessment reports in less than a page, which we feel is appropriately called a capsule. We also feel that this itemized encapsulation will be helpful to the reader, and we prefer to maintain its present form.
1491	92	20	92	52	The previous IPCC reports failed to include a discussion about the dependency of the frequency of TCs to the area of warm sea surface (exceeding 26.5C). Previous studies have looked at the connection between the mean SST and the number of TCs, which, not unsurprisingly, concluded that there was no clear connection. Rather than affecting the frequency, the higher SST has a stronger connection with the storm intensity. But, there have not been many studies investigating the *area* of warm seas and the number of TCs. Benestad (2009; DOI: 10.5194/nhess-9-635-2009) analysed the connection between the area of the warm seas (A) and the number of TCs (n) and found a non-linear relationship: n was proportional to A^5.0+-0.3 for the North-Atlantic. For the North Pacific and the northern Indian Oceans, the exponents were 4.5+-0.4 and and 3.5+-1.5 respectively (rounding to one decimal point). This means that if there are SST biases in the GCM simulation, the frequency of TCs may also be misrepresented. The variations in these dependencies may be due to different ocean basin shapes. The area of the warm pool is another factor affecting TCs besides aerosols, ENSO, wind-shear, etc., and it will be interesting to see how the frequency will change in the future when the area of the warm pool expands. One clue to the sensitivity of the number of TCs was the incredible spike in numbers in 2005. [Rasmus Benestad, Norway]	Noted. These are all good points. As noted, there is not a clear relationship between GHG-warmed SST and increasing TC frequency. This is well documented in the literature going back to early work by Knutson et al. (2008) and earlier. We feel that the key point lives in our statements about TC frequency remaining the same or decreasing under GHG warming. We don't have an a priori expectation, based on theory or models, that TC frequency will increase with increasing GHG concentration.
109975	92	21	92	31	I have not seen a use of enumerated lists anywhere else thus far. Are they consistent with the style guide? Also, the report was published so shouldn't these all be past tense not present tense (this likely applies in many other places in the chapter - I only just picked up on it here). [Peter Thorne, Ireland]	Taken into account. The texts were reformatted without using the enumerated list.
109389	93	10	93	10	There is very little material on RCM evaluation and projections for Tropical cyclones. Suggest consideration of: Redmond, G., K. I. Hodges. C. F. McSweeney, R. G. Jones and D. M. Hein, 2014: Projected changes in tropical cyclones over Vietnam and the South China Sea using a 25km regional climate model perturbed physics ensemble, Climate Dynamics, 10.1007/s00382-014-2450-8; and Gallo, F., J. Daron, I. Macadam, T. Cinco, M. Villafuerte II, E. Buonomo, S. Tucker, D. J. Hein and R.G. Jones, 2018: High-resolution regional climate model projections of future tropical cyclone activity in the Philippines, Int. J. Clim; 1–14. https://doi.org/10.1002/joc.5870 [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. These references are added in Section 11.7.1.3 (Model Evaluation) and Section 11.7.1.5 (Projection of TCs).
80607	93	10	100	33	Just to note that there is a strong focus on the northern hemisphere in this TC text, but little to no mention (that I could see) of the Southern Hemisphere. In particular I think many studies suggest that the Southern Indian Ocean will see a decrease in TC frequency more robustly than elsewhere. [Malcolm J. Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We focused mainly on global trends, which have quantified contributions from the southern hemisphere, and we have cited results specifically from the southern hemisphere (e.g., Callaghan and Power 2011; Haig et al, 2014; Knutson et al 2019).
109979	93	14	93	20	This feels a bit text book and isn't supported by any references. If there are new insights here then supporting references need to be added. Otherwise I do wonder whether it is essential to retain this material? [Peter Thorne, Ireland]	Accepted. We agree that this paragraph can be deleted without significantly compromising anything, and have done so.
1493	93	14	93	20	Is it not a point that the surface temperature of the ocean needs to be above a critical value (26.5C) and that the warm upper ocean layer need to have a certain thickness? Also, it may be obvious, but TCs only form over oceans, follow a seasonal pattern, and the frequency of TCs in the Atlantic and Pacific tends to be affected by ENSO. [Rasmus Benestad, Norway]	Noted. We have deleted the paragraph that this comment refers to.
114979	93	15	93	15	"can be converted to wind, ... " Could you please, break the phrase after wind ? [Elena Maksimovich, France]	Noted. We have deleted the paragraph that this comment refers to.
66153	93	15	93	18	Instead of "For example" - could say "A few examples are". Since it gives more than one example that are separate from each other. It is a little confusing to read. [Marjahn Finlayson, Bahamas]	Noted. We have deleted the paragraph that this comment refers to.
114981	93	18	93	18	"... amounts), and vertical wind shear ..." Could you, please, stop the phrase after amounts). And start new : "Vertical wind shear ..." [Elena Maksimovich, France]	Noted. We have deleted the paragraph that this comment refers to.
114983	93	18	93	19	"Changes in these and other environmental factors, ..." Could you please, remove "and other environmental factors" from this phrase. It doesn't add much value [Elena Maksimovich, France]	Noted. We have deleted the paragraph that this comment refers to.
114985	93	20	93	20	"Please, remove the phrase : This is true for both past and future changes." It doesn't add much value [Elena Maksimovich, France]	Noted. We have deleted the paragraph that this comment refers to.
109981	93	24	93	28	The modes of variability text should make reference to that annex. [Peter Thorne, Ireland]	Accepted. Annex IV is referred to.
100825	93	25	93	27	Acronyms can be used here for the modes of variability already introduced in the Technical Annex VI (e.g IPO/PDV, MJO...). Technical annex should be referred as well here. [Corti Susanna, Italy]	Accepted. Annex IV is referred to, and the acronyms are used to be consistent with Annex IV.
71125	93	27	93	27	Atlantic meridional overturning circulation (AMOC) exists in climatology. "AMOC variability" would be appropriate. But if this corresponds to AMV, the latter would be clearer. [Yu Kosaka, Japan]	Accepted. This is in the parenthesis for the examples of "inter-decadal", AMV is appropriate as suggested.
18059	93	31	93	32	Can add Reed et al. (2019) to citation list (Reed, K. A., Bacmeister, J. T., Huff, J. J. A., Wu, X., Bates, S. C., & Rosenbloom, N. A. (2019). Exploring the impact of dust on North Atlantic hurricanes in a high-resolution climate model. Geophysical Research Letters, 46, 1105– 1112. https://doi.org/10.1029/2018GL080642) [Alyssa Stansfield, United States of America]	Accepted. We have added the reference.
29927	93	32	93	34	I would include a reference to Chapter 2, which assessed the observed changes in the Hadley Circulation [Juan Rivera, Argentina]	Accepted. Section 2.3 is referred to.
15565	93	34	93	36	Instead of just mentioning the Chapters, suggest indicating specifically which Sections are referring to. [SAI MING LEE, China]	Accepted. We have added section information.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
109983	93	39			Section is nice but at odds to other similar sections tends to also address questions of attribution in addition to observations. This then is redundant with two sub-sections hence. If you are going to have an observed section and an attribution section it would be best to keep the contents rigorously seperate? [Peter Thorne, Ireland]	Noted. We weren't considering attribution necessarily, but we are touching on detection here, following IPCC guidance: "An identified change is detected in observations if its likelihood of occurrence by chance due to internal variability alone is determined to be small". Still, we could simply delete the last two sentences of this paragraph to avoid redundancy, and we've done this.
15567	93	41	93	47	For the review of past trends and TC best track data heterogeneous issues, suggest making reference to the following assessment reports/papers mandated by WMO Expert Team (mainly from global perspective) and the UNESCAP/WMO Typhoon Committee Expert Team (for the western North Pacific) : 1. Knutson, T R., Suzana J. Camargo, Johnny C. L. Chan, Kerry Emanuel, Chang-Hoi Ho, James Kossin, Mrutyunjay Mohapatra, Masaki Satoh, Masato Sugi, Kevin Walsh, and Liguang Wu, 2019 : Tropical Cyclones and Climate Change Assessment: Part I. Detection and Attribution, Bulletin of the American Meteorological Society, https://doi.org/10.1175/BAMS-D-18-0189.1 2. Lee, T.C., T.R. Knutson, T. Nakaegawa, M. Ying and E.J. Cha, 2020 : Third Assessment on Impacts of Climate Change on Tropical Cyclones in the Typhoon Committee Region – Part I : Observed Changes, Detection and Attribution, Tropical Cyclone Research and Review, In Press, https://doi.org/10.1016/j.tcrr.2020.03.001. [SAI MING LEE, China]	Accepted. We are heavily citing Knutson et al already. We've added the recently published Lee et al papers.
9183	93	42	93	44	unclear sentence. Low confidence that trend metrics are not affected by changes in technology implies that there's high confidence that trend metrics are affected by changes in technology. Consider rewording as "There is low confidence in TC frequency-based or intensity-based trends due to changes in technology used to collect the best-track data" or "There is high confidence that trend metrics are affected by changes in technology used to collect the best-track data". [Kevin Hennessy, Australia]	Taken into account. We've modified the text following your suggestion.
66155	93	47	93	48	"Further uncertainty is introduced by an incomplete understanding of the mechanisms driving the observed multidecadal variability (Knutson et al., 2019a)." - This sounds confusing in passive voice. Consider using an active tone or changing the subject of the sentence. [Marjahn Finlayson, Bahamas]	Noted. This text was removed.
126025	93	47			Suggested edit: "... particularly in the presence of multidecadal variability, and given the strength of the forced signal ..." [Trigg Talley, United States of America]	Rejected. We're reluctant to add this because it asserts that the forced signal must be substantially weaker than the amplitude of the multidecadal variability, which would require additional support.
15569	93	52	93	53	Suggest also including the following reference paper on the homogenization of best track data for TC intensity related assessment: Kang, N. Y. and J.B. Elsner, 2012 : Consensus on climate trends in western North Pacific tropical cyclones, J. Climate, 25, 7564-7573. [SAI MING LEE, China]	Noted. We do in fact cite Kang and Elsner 2012 in the subsequent sentence.
62469	93	52	94	2	The following new literature also can be cited as efforts to homogenize the best-track data and the studies show the geographical distribution of tropical storms. 1)Squintu AA, van der Schrier G, Brugnara Y, Klein Tank A. Homogenization of daily temperature series in the European Climate Assessment & Dataset. Int J Climatol. Int J Climatol. 2019;39:1243–1261; 2)Wai-kin Wong and Chun-wing hoy., 2018. Progress in Hong Kong's Tropical Cyclone Forecasting and Warning Services in Recent Decades. Tropical Cyclone Research and Review Volume 7, No. 1. 37-50; 3)Hausfather, Z., K. Cowtan, M. J. Menne, and C. N. Williams Jr. (2016), Evaluating the impact of U.S. Historical Climatology Network homogenization using the U.S.Climate Reference Network, Geophys. Res. Lett., 43, 1695–1701, DOI:10.1002/2015GL067640; 4)Choy, C., Chong, S., Kong, D., and Cayanan, E.O. 2015. A Discussion of the Most Intense Tropical Cyclones in the Western North Pacific from 1978 to 2013. Tropical Cyclone Research and Review, Volume 4, No. 1. 1-11. 5)Zhizhong Su, Fumin Ren, Jin Wei, Xiaohong Lin, Shunji Shi, and Xueming Zhou 2015.Changes in Monsoon and Tropical Cyclone Extreme Precipitation in Southeast China from 1960 to 2012. Tropical Cyclone Research and Review, Volume 4, No. 1. 12-17. 6)Williams, C. N., M. J. Menne, and P. W. Thorne, 2012. Benchmarking the performance of pairwise homogenization of surface temperatures in the United States, J. Geophys. Res., 117, D05116, DOI:10.1029/2011JD016761; [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. We are not attempting a literature review though. Some of the suggested references are not TC related and others are in fact not focused on homogenization of best track data but rather data collection efforts. We have added the most relevant citation; Choy et al. (2015).
74569	94	1	94	1	to define so if appropriate if it dosn't mean the word so. [Moulay Driss HASNAOUI, Morocco]	Accepted. We have modified the text by removing "or so".
39749	94	3	94	3	"generally remains positive" -> What is positive? The intensity trend? [TSU WGI, France]	Taken into account. We have modified the text as "intensity trends".
66157	94	8	94	10	"...it is expected that a trend in TC intensity might become detectable over the past 40 years or so, but might also be sensitive to shortening the period of analysis." Unclear here. Maybe try: "An expected trend in TC intensity might become detectable over the past 40 years or so, but this expectation may be sensitive to the shortening of the analysis." [Marjahn Finlayson, Bahamas]	Noted. The text was removed.
62471	94	14	94	18	Include Murakami et al., 2020. [Hiroyuki Murakami, Thomas L. Delworth, William F. Cook, Ming Zhao, Baoqiang Xiang, and Pang-Chi Hsu., 2020. Detected climatic change in the global distribution of tropical cyclones. PNAS, 1-9. https://doi.org/10.1073/pnas.1922500117] [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The text was moved to 11.7.1.4 and we've added the citation Murakami et al. (2020).
15571	94	15	94	17	Suggest also including the following reference paper on the TC intensification rate assessment: Kishtawal, C.M., N. Jaiswal, R. Singh, and D. Niyogi, 2012 : Tropical cyclone intensification trends during satellite era (1986-2010), Geophysical Research Letters, 39, L10810. [SAI MING LEE, China]	Accepted. We have added the reference Kishtawal et al. (2012).
74571	94	18	94	18	Murakamy, submitted To check if it isn't published [Moulay Driss HASNAOUI, Morocco]	Accepted. The text was moved to 11.7.1.4 and we've added the citation Murakami et al. (2020).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
126027	94	18			The Murakami et al. (2020, PNAS submitted) paper is cited here but could be elaborated more. The study provides some support for the idea that reduced forcing from aerosols and volcanic eruption activity contributed to the recent increased TC activity in the N. Atlantic since the 1970s, in qualitative agreement with Dunstone et al. 2013. It should be noted that their study indicates that aerosols had been suppressing TC frequency for many decades, so this increase since the 1970s was more of a ""return toward natural conditions."" The study also projects, again in agreement with Dunstone et al., TC frequency decreases over the coming century in the N. Atlantic due to greenhouse warming. In fact, the Murakami et al. study projects, in response to +1% CO2 forcing, a reduction in TC frequency almost everywhere in the tropics; exceptions include the central N. Pacific (Hawaii region), east of the Phillipines in the N. Pacific, and two relatively small regions in the northern Arabian Sea and Bay of Bengal. This adds to evidence for the notion of a future decrease in global average TC frequency, and for the temporary contribution of aerosol reduction to the recent increase in TC frequency since the 1970s in the N. Atlantic. Citation: Dunstone, N., Smith, D., Booth, B. et al. Anthropogenic aerosol forcing of Atlantic tropical storms. Nature Geosci 6, 534-539 (2013). https://doi.org/10.1038/ngeo1854 [Trigg Talley, United States of America]	Noted. We summarize the results of Murakami et al 2020 in the D&A section, and the text on projections is more appropriate for the projections section. We've added text to the projections section.
15573	94	20	94	31	The discussion for the western North Pacific basin is missing. Suggest including the findings of the UNESCAP/WMO Typhoon Committee Expert Team's Third Assessment on the impacts of climate change on tropical cyclone activity in the western North Pacific, especially the following key findings on TC frequency, intensity and track: (i) The four available best track datasets in the western North Pacific continue to show significant interdecadal variations in basin-wide TC frequency and intensity in the region. While most of the best track datasets depict a decreasing trend in basin-wide TC frequency, the observed trend and its statistical significance are still highly dependent on the best track dataset used, the analysis period chosen, and other analysis details. (ii) There has been encouraging research progress in improving the consensus between best track datasets to investigate intensity trends in the western North Pacific. Increases in the number and intensification rate for intense TCs, such as Cat. 4-5s, in the region since mid-1980s was reported by a number of studies. The intensity of TC landfall over East China and Japan has shown a statistically significant increase while that of south China, the Philippines and Vietnam has not changed significantly. (iii) A statistically significant northwestward shift in TC tracks and a poleward shift in the average latitude where TCs reach their peak intensity in the western North Pacific have also been reported based on data since the 1980s. The prevailing track changes have also resulted in an increase in TC occurrence, including TC landfalls, in some regions, including East China, Japan, and the Korean Peninsula in recent decades. Reference: Lee, T.C., T.R. Knutson, T. Nakaegawa, M. Ying and E.J. Cha, 2020 : Third Assessment on Impacts of Climate Change on Tropical Cyclones in the Typhoon Committee Region – Part I : Observed Changes, Detection and Attribution, Tropical Cyclone Research and Review, In Press, https://doi.org/10.1016/j.tcrr.2020.03.001. [SAI MING LEE, China]	Taken into account. We focused mainly on global trends due to space restrictions. But we do discuss a few key results of the WNP in various parts of the text (e.g. poleward shifts) by adding the reference Lee et al. (2020).
62473	94	23	94	29	Rather than saying "generally unclear", it is better to use the standardized likelihood and confidence classification for the consistent usage of descriptive terminology. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. What we mean here is that we don't know yet what type of trend is expected at this point. Therefore we cannot give a likelihood if we don't even know the sign of the trend.
82805	94	24	94	24	Should specify "eastern Australia" (the Callaghan paper did not consider other parts of Australia) [Blair Trewin, Australia]	Noted. We added a reference to the Chand et al. 2019 paper, which did consider all parts of Australia and also noticed a decreasing trend since 1982. So we modified the text to reflect that.
9185	94	24	94	27	There have been fewer tropical cyclones near Australia since 1982, due to a combination of both climate change and natural variability, together with no clear trend detected in the number of severe tropical cyclones (Chand et al. 2019. Review of tropical cyclones in the Australian region: Climatology, variability, predictability, and trends. Wiley Interdisciplinary Reviews: Climate Change, https://doi.org/10.1002/wcc.602) [Kevin Hennessy, Australia]	Taken into account. We added the Chand et al. 2019 reference in the text.
114987	94	30	94	30	"... broad spectrum of impacts ... " could be replaced by "major", "multiple", "extensive","large" [Elena Maksimovich, France]	Noted. We feel that the text makes the intended point.
114989	94	30	94	30	" ..... large-scale changes in ... " could be replaced with "shift/evolution in TC behavior" : otherwise many "changes" in the same phrase [Elena Maksimovich, France]	Taken into account. Instead of "large-scale changes" we used "large-scale modifications".
9967	94	33	94	33	The phrase Subsequent to may be wordy. Consider changing the wording.(After) [ayman badawy, Egypt]	Noted. We think that the text is fine as it is.
114991	94	34	94	34	"... analysed, and ..." Could you please, break the sentense after "analysed" ? [Elena Maksimovich, France]	Accepted. The sentence is separated.
114999	94	35	94	54	20 lines dedicated to "poleward migration of TC tracks", BUT by how much ? How many degrees latitude ? [Elena Maksimovich, France]	Noted. The amount of poleward migration is dependent on the hemisphere and basin. Therefore is too much information to add to the text.
114993	94	38	94	38	" ... expansion of the tropics .." maybe better to say "of the tropical climate" ? [Elena Maksimovich, France]	Rejected. Tropics and tropical climate are not exactly the same and are not interchangeable here.
114995	94	41	94	42	" .. Part of the northern hemisphere poleward migration is due to interbasin changes in TC frequency " : I wonder if anybody knows what it means [Elena Maksimovich, France]	Taken into account. Modified to "basin wide changes in TC frequency".
114997	94	41	94	44	"Part of the northern hemisphere ..." : Please, remove this sentence, it brings no meaning, but only confusion [Elena Maksimovich, France]	Noted. Modified to "basin wide changes in TC frequency".
13771	94	53	94	53	Change Representative Concentration Pathway8.5 by RCP8.5 [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Done. RCP8.5 had been defined before.
62475	94	54	95	1	It is useful to identify the types of possible anthropogenic contributions to the observed trends. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. GHG specified as the anthropogenic contribution.
126029	95	2	95	2	Term HighResMIP is not defined anywhere in preceding sections. [Trigg Talley, United States of America]	Noted. Referred to the source of HighResMIP.
20267	95	6	95	12	Figure 11.22 is indeed painfully schematic in this SOD version. Let us hope that it will be enriched in the final version [philippe waldteufel, France]	Taken into account. Figure 11.20 (previously Fig. 11.22) was reproduced.
115001	95	26	95	26	"coterminous" could be changed by "mainland" ? "continental" ? [Elena Maksimovich, France]	Accepted. Changed to "contiguous United States".

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9969	95	30	59	30	aircrafts [ayman badawy, Egypt]	Rejected. Here we mean data issues, or artefacts. The word is correct. The sentence was moved to Section 11.7.1.4.
126031	95	30			Zhang et al. (2020) find that for the models they studied, the simulated historical changes in TC propagation speed do not provide supporting evidence for an anthropogenic influence on observed TC propagation speed either globally since 1951 or over the continental U.S. since 1901. Citation: Zhang, G., H. Murakami, T. R. Knutson. R. Mizuta, and K. Yoshida, 2020: Tropical cyclone motion in a changing climate. Sci. Adv., in press. [Trigg Talley, United States of America]	Accepted. Added sentence on Zhang et al. 2020 in Section 11.7.1.4.
70969	95	36	95	43	This discussion, which underpins some key chapter conclusions (p.96, lines 1-2 and p.9, lines 4-5), seems very speculative and hand-wavy. The "circulation slowdown" predicted by Held and Soden is in the divergent circulation, whereas for TC movement, it is the horizontal wind that is important and this is dominated, even in the tropics, by the rotational circulation. It's also hard to see how Arctic amplification could directly affect the lower latitudes that are relevant for TC movement, and anyway the alleged mechanisms discussed here are highly controversial. The authors seem to be push the evidence a bit too far here. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We modified this paragraph making clear that this mechanism has not been shown yet to be connected to the slowdown. The sentences were moved to Section 11.7.1.4.
11733	95	37	95	38	another explanatory sentence would help here. What physical linkage? You have just said it's not clear what the cause of the observed slow down is, but some potential linkage has apparently been identified by Held and Soden, so a brief explanation is warranted. [Amy East, United States of America]	Noted. We modified this paragraph making clear that this mechanism has not been shown yet to be connected to the slowdown. The sentences were moved to Section 11.7.1.4.
28963	95	37			"Sections 8.2.1.3 and 8.2.2.1.2" should be "Chapter 8, Section 8.2.3.2" as the text was rearranged in the SOD [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We site the correct section 8.2.2.2
1495	95	38	95	38	The Arctic amplification is as far as I know more of a surface phenomenon, so there is a need for more information on how it affects TC speeds. Perhaps provide some numbers for the latitudinal expansion and the typical translational speed of TCs. [Rasmus Benestad, Norway]	Noted. We modified this paragraph making clear that this mechanism has not been shown yet to be connected to the slowdown. The sentences were moved to Section 11.7.1.4.
109985	95	39	95	39	Should this make cross-reference to cross-chapter box 10.1? [Peter Thorne, Ireland]	Taken into account. We refer to Rejected. 10.1.
109987	95	40	95	43	Substantive assessment of these aspects were undertaken in chapters 4 and 7 and perhaps it would be better to reference these at the end of this sentence rather than a handfuul of papers as they assessed this and its implications as well as potential unusualness of the recent behaviour of, in particular, the walker circulation and its impacts. [Peter Thorne, Ireland]	Taken into account. The texts are modified to refer to Chapters 4 and 8.
109989	95	45	95	46	This opening sentence contains no actionable information. The summary would read better diving straight in with the next sentence. [Peter Thorne, Ireland]	Noted. The point of the opening sentence is to be clear that there has been changes in TC characteristics . It's an important point to drive home, so we kept as it is.
9187	95	45	95	48	the insurance and disaster risk management sectors need a statement about the total number of TCs and the total number of Cat 4-5, not the proportion of Cat 4-5. Page 99 lines 3-5 state "For a 2oC global warming, the median proportion of Category 4–5 TCs increases by 13%, while the median global TC frequency decreases by 14%, which infers that the median of the global Category 4–5 TC frequency is slightly reduced by 1% or almost unchanged (Knutson et al., 2019b)". This is very policy-relevant and should be considered for inclusion in the Summary. [Kevin Hennessy, Australia]	Noted. The statements on page 99 are about projections, while here we are talking about trends. These are separate problems. The text was not modified.
26181	95	45	96	2	The summary should be itemized with numbers: 1) to 4). [Masato Sugi, Japan]	Noted. As the other summaries are not itemized, we think we should leave it this way.
115003	95	46	95	46	"proportion" please, replace with "number" or "percantage" [Elena Maksimovich, France]	Rejected. The correct term here is indeed proportion.
26183	95	46	95	46	"It is likely" should be "There is medium confidence" to be consistent with the other part of this section, unless there is a specific reason to use "liklihodd level" instead of "confidence level" here. [Masato Sugi, Japan]	Noted. In this paragraph both likelihood level and confidence level are given in the summary. We think it's important to explicitly give both.
51637	95	46	95	47	This sentence seems to be arguing that consistency of observations with simulations and theoretical understanding leads to greater confidence in detectability. I don't think this logic is quite right - surely the consistency means that we can have greater confidence in our theoretical understanding and modelling, or greater confidence in the *trend* but not whether the increase can be detected? Could you clarify the wording to make this clearer to the reader please? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence was modified accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
126033	95	46	95	48	[CONFIDENCE] Confidence level for detectable (unusual compared to natural variability) increase in proportion of stronger TCs should be low confidence. If IPCC allows split confidence levels, the authors could consider between low confidence and low-to-medium confidence. One can say medium confidence it has increased, but don't really know why and don't have medium confidence that the increase is unusual compared to natural variability. Rationale: The conclusion that there is medium confidence that an increase in proportion of stronger TCs globally has become detectable is based on the likely increase in the proportion of stronger TCs globally over the past 40 years, and the statement that this is consistent with theoretical understanding and numerical simulations (citing Knutson et al. 2015, 2019b, and Walsh et al. 2015, 2016, Bender et al 2010 and Kossin et al. 2013). Also cited is the new Kossin et al. (2020) manuscript reporting the observed trend. None of these studies provide convincing evidence that the change reported by Kossin (2020) is outside the range of behavior expected from natural variability -- which is what must be demonstrated for detection. This is different from finding that an observed change over some time period is similar to a modeled signal: Oone must still show that the change is highly unusual compared to natural variability, otherwise such agreement with a projection could be coincidental and not indicating detection. One way detection could be done is to compare the observed trend in the metric to a distribution of trends in climate model long control runs or large ensembles of natural forcing only runs. The modeled signals cited in the above papers are not comparable to the observed change in Cat 3-5 proportion in any case. Further, Bender et al. and Knutson et al. 2015 are based on future climate change scenarios, not historical simulations and so are not really comparable to observed changes over the historical period. The 10% per decade increase in Cat 4-5 numbers in Bender et al. (2010) was reduced somewhat in their expanded simulation study for the Atlantic (Knutson et al. 2013) and their global study, such that they no longer found a statistically significant increase in Cat 4-5 frequency in the Atlantic. Even with this diminishment of signal to noise in the updated study, the Bender et al. study still estimated it would take about six decades along an IPCC A1B scenario for a detectable signal to emerge in Atlantic Cat 4-5 frequency. Sobel et al. (2016) -- see their Figures 3 and 4 -- show that aerosols may have offset much of the impact of historical GHG warming on TC intensity, with an expected signal only beginning to emerge near the end of the 20th century. Their Figure 4 looks at NH Power Dissipation (not proportion of Cat 3-5 storms) and find that multidecadal variability in the observed NH mean TC power dissipation index may dwarf the expected climate change signal in that metric due to anthropogenic forcing since 1950. As these metrics are related this could indicate how natural variability could confound detection over multiple decades. What Kossin et al. (2020) shows is a strong rising trend in proportion of Cat 3-5 hurricanes for 1979-2017. No formal detection was claimed, and anthropogenic influence was not quantified. The signal in Kossin et al. (2020) is strongest in the N. Atl. and South Indian. The signal is weak in NW Pac and slighly negative in N. Ind., with moderate rise in the NE Pacific and South Pacific. In the N. Atlantic, the increase could be due to aerosol reduction or natural variability playing big roles (e.g., Murakami et al., Bhatia et al.), so its difficult to claim that is a GHG forced rising signal (in the N. Atlantic). So the statistical significance of this change at present may come down to trying to understand why the South Indian Ocean has such a strong rising signal. This will require further study. Unfortunately, it is not yet known what the natural (internal) climate variability of this metric on multidecadal time scales looks like. [Trigg Talley, United States of America]	Noted. The summary statement was reformatted without mentioning confidence level.
26185	95	48	95	48	"It is very likely" should be "There is high confidence" to be consistent with the other part of this section, unless there is a specific reason to use "liklihodd level" instead of "confidence level" here. [Masato Sugi, Japan]	Noted. The summary statement was reformatted without mentioning confidence level.
126035	95	50	95	51	[CONFIDENCE] Confidence level for detectable (unusual compared to natural variability) increase in latitude of maximum intensity in NW Pacific should be low-to-medium confidence (if IPCC allows split confidence levels), otherwise low confidence. One can say with medium confidence it has increased, but don't really know why the increase has occurred and don't have medium confidence that the increase is unusual compared to natural variability. Rationale: The WMO TC/climate assessment (Knutson et al., 2019a) assessed this finding. The author team for that report expressed the following opinion on confidence levels (Table 1): low to medium confidence, 8 authors; medium confidence, 1 author; medium to high confidence, 2 authors. IPCC does not report distribution of opinion, but a single confidence level. This case study was discussed in detail in Knutson et al. (2019a), and there are no new published findings on it since that assessment. The methodology of assessing how unusual the observed change is compared to natural variability consists of regressing out ENSO, PDO (or IPO) and the AMO and examining trend of the residuals. This assumes that natural multidecadal variability in the metric is linearly related and well described by some combination of the predictors with little influence of any other process (e.g., atmospheric internal variability, coupled variability unrelated to the predictors) not included in the predictor list. The ability of these predictor variables to statistically describe the variability of the TC metric could be more thoroughly explored using climate model control runs which would give more confidence, though not complete confidence in this methodology. [Trigg Talley, United States of America]	Noted. The summary statement was reformatted without mentioning confidence level.
126037	95	50			"Emanuel concluded that …" (not "showed that"). [Trigg Talley, United States of America]	Accepted. This is the comment on p.97 L50 (Section 11.7.1.4). We modified the sentence as suggested.
84915	95	51	95	51	Medium confidence in a decrease in TC translational speed in the US seems like a strong statement given it's based on a single study. In general the summary paragraph gives a dissproportiatly long discussion of potential slowdowns relative to the brevity of the summary paragraphs elsewhere in the chapter. [Turner Jessica, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The summary statement was reformatted without mentioning confidence level.

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126039	95	51	95	53	[CONFIDENCE] "TC translation speed has slowed detectably over the US since 1900" should be low confidence. This would actually be a good candidate for a balance of evidence statement in the case of trying to avoid Type II errors as discussed by Knutson et al. (2019a), if IPCC were using that approach. In Knutson et al. (2019a), there was a balance of evidence/Type II error avoidance statement for detection of a global reduction in TC propagation speed, but that should now be dropped altogether for the global reduction, owing to the Comment and Reply in Nature on this topic (which was published too late for Knutson et al. to consider). Instead, recommend applying a similar balance of evidence/Type II error avoidance statement to the continental US TC slowdown result since 1900 (detectable vs. not detectable). However, since IPCC is not using this Type II error/ balance of evidence approach, the recommendation that fits with their handling of confidence levels is just low confidence for the continental US propagation speed decrease since 1901. A few related comments/questions on the continental U.S. slowdown finding follow, as this was not discussed in Knutson et al. (2019a). One of the difficulties with this type of analysis concerns how long the propagation speed of an individual tropical cyclone is tracked over U.S. land. At some point, the tropical cyclones may transition to extratropical cyclones: Is that when the propagation speed tracking is discontinued for that storm? In other words, what specific criterion was used to decide when to stop computing a propagation speed for a storm that would contribute to the annual mean value for a given year? Are we assured that the process of determining when a tropical cyclone is no longer a tropical cyclone is something which is homogeneous over time since 1900? It would be much harder to "make the call" on extratropical transition in 1900 than during the satellite era, even over land. Another issue is the small sample size (going from global to just U.S. land) and the presumably non-normal nature of the propagation speed data (zero bounded below and with likely a skewed distribution at higher speeds), as storms pick up speed in the higher latitude westerlies, leading at times to large values. How robust is the trend analysis to this non-normality? Is the trend influenced by a small number of large values (tail of distribution) and are there enough independent samples in each year to estimate a robust mean value for that year? Should the annual median value be used instead to construct the time series? [Trigg Talley, United States of America]	Noted. The summary statement was reformatted without mentioning confidence level.
11735	96	1	96	2	again, see last comment – what theory? Another sentence explaining the possible physical cause of slowdown would be appropriate. [Amy East, United States of America]	Taken into account. Given the multiple comments about this issue, we deleted this sentence.
20751	96	5	97	5	While the previous subsection insists on the importance (including possible consequences on flood) of the TC translation speed, there is no indication here of the ability of models to simulate realistic translation speeds and eventually to reproduce the observed slowing trend. According to the following section, there are some encouraging findings but no consensus. [philippe waldteufel, France]	Taken into account. TC translation speeds is a characteristics of TC statistics, its evaluation is conducted by existing studies. We add a reference of Yamaguchi et al. (2020,NatComm). Yamaguchi et al. (2020,NatComm) show no decrease in TC translation speeds between 1951-2019, while an increase trend emerges for future global mean translation speed. 1) Yamaguchi, Munehiko, Chan, Johnny C. L., Moon, Il-Ju, Yoshida, Kohei, Mizuta, Ryo. Global warming changes tropical cyclone translation speed. Nature Communications. 2020, vol. 11, no. 1, p. 47. https://doi.org/10.1038/s41467-019-13902-y.
29277	96	5	97	5	Should less computationally-expensive downscaling approaches that allow larger ensembles and long-term studies be discussed here? (e.g., Emanuel et al., 2006; Emanuel et al., 2008; Lee et al., 2018) [Andra Garner, United States of America]	Taken into account. These approaches are referred to in the projection sections, so we also add the downscaling approach by Emanuel (2006,2008) and Lee et al. (2018) in this subsection. 1) Emanuel, Kerry, Ravela, Sai, Vivant, Emmanuel, Risi, Camille. A statistical deterministic approach to hurricane risk assessment. Bulletin of the American Meteorological Society. 2006, vol. 87, no. 3, p. 299–314. 2) Emanuel, Kerry, Sundararajan, Ragoth, Williams, John. Hurricanes and global warming: Results from downscaling IPCC AR4 simulations. Bulletin of the American Meteorological Society. 2008, vol. 89, no. 3, p. 347–367. 1) Lee, Chia Ying, Tippett, Michael K., Sobel, Adam H., Camargo, Suzana J. An environmentally forced tropical cyclone hazard model. Journal of Advances in Modelling Earth Systems. 2018, vol. 10, no. 1, p. 223–241.
62477	96	7	96	9	The following citation can be given to the example (i.e. SST). According to Bacmeister et al., 2018, more detailed basin-scale projections of future TC activity are subject to large uncertainties due to uncertainties in future SSTs. In most cases, these uncertainties are larger than the effects of mitigating from RCP8.5 to RCP4.5. [Julio T. Bacmeister, Kevin A. Reed, Cecile Hannay, Peter Lawrence, Susan Bates, John E. Truesdale, Nan Rosenbloom, Michael Levy, 2018. Projected changes in tropical cyclone activity under future warming scenarios using a high-resolution climate model. Climatic Change, 146: 547–560]. Furthermore, Yoshida et al., (2017) suggest that the regional TC activity changes have large uncertainty corresponding to sea surface temperature warming patterns. [Yoshida, K., Sugi, M., Mizuta, R., Murakami, H., & Ishii, M. (2017). Future changes in tropical cyclone activity in high-resolution large-ensemble simulations. Geophysical Research Letters, 44,9910–9917. https://doi.org/10.1002/2017GL075058.] [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Added this sentence a bit farther down in the text in the projection section (11.7.1.5): "Even within a single model, uncertainty in the pattern of future SST changes leads to large uncertainties (including the sign) in the projected change in TC statistics in individual ocean basins although global TC would appear to be less sensitive (Yoshida et al., 2017; Bacmeister et al., 2018)."
1497	96	7	96	19	One test of the models could be to evaluate their ability to reproduce the dependency of the number of TCs on the area of the warm seas seen in the real world. [Rasmus Benestad, Norway]	Taken into account. Added this sentence a bit farther down in the text: "One test of the models is to evaluate their ability to reproduce the dependency of the TC statistics in the different basins in the real world, in addition to their capability of reproducing atmospheric and ocean environmental conditions."

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107713	96	21	96	27	How does TC frequency in HighResMIP compare to observations and how does this affect confidence levels compared with previous projections (e.g., Camargo, S: Global and Regional Aspects of Tropical Cyclone Activity in the CMIP5 Models, J. Clim, 26, 9880-9902, DOI: 10.1175/JCLI-D-12-00549.1, 2020)? [Emily Collier, Germany]	Accepted. Added Camargo et al. (2013) and changed lines 22-24 to "They do simulate storms of relatively high vorticity that are at best described as “TC-like”, but metrics like storm counts are highly dependent on tracking algorithms but are typically underestimated (Camargo, 2013; Wehner et al., 2015; Zarzycki and Ullrich, 2017; Roberts et al., 2020b)." and added this sentence to the end of the paragraph: "Confidence in the projection of TC statistics and properties is increased by the higher resolution models with more realistic simulations."
18061	96	21	96	39	Can mention potential of variable-resolution (VR) models for studying TCs in specific basins. VR models reduce lateral boundary condition errors associated with regional models and are much cheaper to run than high-resolution global models, which means ensembles can be run with the computational savings. Citations: Zarzycki et al., 2014 (https://doi.org/10.1175/MWR-D-13-00179.1); Zarzycki and Jablonowski, 2014 (https://doi.org/10.1002/2014MS000352); Hashimoto et al, 2016 (https://doi.org/10.1007/s00382-015-2852-2), Stansfield et al., 2020 (https://doi.org/10.1175/JHM-D-19-0240.1) [Alyssa Stansfield, United States of America]	Accepted. Variable resolution global models offer an alternative to regional models for individual TC or basin wide simulations (Yanase et al. 2012; Zarzycki et al., 2014; Harris et al. 2016; Reed et al., 2020; Stansfield et al., 2020) Computationally less intense than equivalent uniform resolution global models, they also do not require lateral boundary conditions thus reducing this source of error (Hashimoto et al., 2016). 1) Harris, Lucas M., Lin, Shian Jiann, Tu, Chia Ying. High-resolution climate simulations using GFDL HiRAM with a stretched global grid. Journal of Climate. 2016, vol. 29, no. 11, p. 4293–4314. 2) Yanase, Wataru, Satoh, Masaki, Taniguchi, Hiroshi, Fujinami, Hatsuki. Seasonal and intraseasonal modulation of tropical cyclogenesis environment over the bay of bengal during the extended summer monsoon. Journal of Climate. 2012, vol. 25, no. 8, p. 2914–2930.
100873	96	24	96	27	There are some recent indications from HighResMIP that increasing model resolution in the ocean and atmospheric component could greatly alleviate the bias in tropical cyclones frequency (Roberts et al., 2020). However, preliminary results tend to indicates that increasing resolution is not the unique way to address the biases in the Tropical Cyclones frequency, for instance, Stochastic Physics has a nearly equivalent effect on the mean number and distribution of TCs (Vidale et al. 2020, submitted to J. Clim). Roberts, M. J., Camp, J., Seddon, J., Vidale, P. L., Hodges, K., Vanniere, B., Mecking, J., Haarsma, R., Bellucci, A., Scoccimarro, E., Caron, L.-P., Chauvin, F., Terray, L., Valcke, S., Moine, M.-P., Putrasahan, D., Roberts, C., Senan, R., Zarzycki, C. and Ullrich, P. (2020) Impact of model resolution on tropical cyclone simulation using the HighResMIP-PRIMAVERA multi-model ensemble. Journal of Climate, 33 (7). pp. 2557-2583. ISSN 1520-0442 doi: https://doi.org/10.1175/jcli-d-19-0639.1 [Corti Susanna, Italy]	Noted. It is already described that the resolution is not unique method to improve TC statistics. We additionally refer to (Vidale et al. 2021, J. Clim).
62479	96	24	96	27	Yoshida et al., 2017 also shows results of 60km global atmospheric model used to simulate TCs of 4-5 category. According to the authors, the global number of TCs decreases by 33% in future projection. Although geographical TC occurrences decrease generally, they increase in the central and eastern parts of the extratropical North Pacific. Meanwhile, very intense (category 4 and 5) TC occurrences increase over a broader area including the south of Japan and south of Madagascar. [Yoshida, K., Sugi, M., Mizuta, R., Murakami, H., & Ishii, M. (2017). Future changes in tropical cyclone activity in high-resolution large-ensemble simulations. Geophysical Research Letters, 44,9910–9917. https://doi.org/10.1002/2017GL075058.] [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. See response to comment #62477. Basin scale uncertainty is very large and the new text notes that.
20753	96	41	96	44	This sounds promising. It is frustrating that according to the title of Nakano et al (2017) they report preliminary results. However, no paper relating further steps can be found among the 2050 Masuo Nakano references listed by Google Scholar… [philippe waldteufel, France]	Noted. It is desired to include follow up papers to Nakano et al. (2017) if they are published.
18063	96	41	96	44	Also, there are a few studies showing the viability of climate models to act as operational forecast models for TCs, when initialized with observed initial conditions and run for short periods of time. Citations: Zarzycki and Jablonowski, 2015 (Zarzycki, C.M. and C. Jablonowski, 2015: Experimental Tropical Cyclone Forecasts Using a Variable-Resolution Global Model. Mon. Wea. Rev., 143, 4012–4037, https://doi.org/10.1175/MWR-D-15-0159.1); Reed et al., 2020 (https://doi.org/10.1126/sciadv.aaw9253) [Alyssa Stansfield, United States of America]	Accepted. Added this sentence after line 44: "Likewise, high-resolution climate models show promise as TC forecast tools (Zarzycki and Jablonowski, 2015; Reed et al., 2020), further narrowing the continuum of weather and climate models and increasing confidence in projections of future TC behaviour."
109991	96	41	96	44	It feels to me like more of a play should be made of this and it should be explicitly integrated in the prior paragraph as these models are part of the continuum of capabilities rather than some kind of special case. That NWP models can when appropriately initialised capture realistic structures, tracks and evolution is important in building confidence that future generations of climate models stand a good chance of doing so. This should perhaps be made more explicit? [Peter Thorne, Ireland]	Taken into account. See response to comment #18063. We did not merge the paragraphs but note that this convergence of weather and climate modelling increases confidence.
6795	96	42	96	42	From my experience at ECMWF I would not say that our operational model is "highly tuned" for forecasting purposes. Model changes are tested at forecast ranges from days to seasons (down to hours in data assimilation) and in longer simulations. Some tuning is inevitable insofar as a model change will not be accepted without exceptional justification if it gives poorer verification results for a wide range of metrics. But I would drop the word "highly". Though I am not in a position to comment objectively. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We modified this sentence as "However, there is limited application for future projections as they are specifically developed for operational purposes and TC climatology is not necessarily well evaluated".
126041	96	49			Add another sentence: "… Roberts et al., 2019). Coarse resolution atmospheric models may degrade coupled model performance as well. For example, in a case study of …" [Trigg Talley, United States of America]	Accepted. We added this sentence "Coarse resolution atmospheric models may degrade coupled model performance as well." to line 49.
26187	96	50	96	50	Move "Ogata et al., 2015,2016" to Line 48, after "Murakami et al., 2015b;". Ogata et al. 2015, 2016 show atmosphere-ocean coupling effect on TC intnsity bias, not resolution effect on SST bias. [Masato Sugi, Japan]	Accepted. These references are moved.
41117	96	50	96	50	Unclear what is meant by 'hurricane activity' -> Total number of hurricanes? Applies elsewhere where 'activity' is referred to [TSU WGI, France]	Taken into account. We changed "activity" to "frequency and intensity".
9897	97	3	97	3	, an, may be redundant when used with the uncountable noun analysis in your sentence. Consider removing it. [ayman badawy, Egypt]	Accepted. Changed to "incomplete analyses".

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9971	97	3	97	3	The indefinite article, an, may be redundant when used with the uncountable noun analysis in your sentence. Consider removing it. [ayman badawy, Egypt]	Accepted. Changed to "incomplete analyses".
13773	97	11	97	11	Change chapter by Chapter [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Changed to "Chapter".
62481	97	14	97	16	"However, there is still no consensus on the relative magnitude of human and natural influences on past changes in Atlantic hurricane activity, and particularly which factor has dominated the observed increase." This type of statements must follow a standard likelihood and confidence level classifications corresponding to factors identified to show impacts on the observed increase in Atlantic hurricane activity since the 1970s. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Following this sentence, we added "and it remains uncertain whether past changes in Atlantic tropical cyclone activity are outside the range of natural variability. "
62483	97	16	97	18	Include Murakami et al., 2020. Hiroyuki Murakami, Thomas L. Delworth, William F. Cook, Ming Zhao, Baoqiang Xiang, and Pang-Chi Hsu., 2020. Detected climatic change in the global distribution of tropical cyclones. PNAS, 1-9. https://doi.org/10.1073/pnas.1922500117 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Murakami et al. (2020) is correctly cited. [updated from (Murakami, submitted).]
126043	97	16			Add: "… dominated the observed increase, and it remains uncertain whether past changes in Atlantic tropical cyclone activity are outside the range of natural variability. A recent result …" [Trigg Talley, United States of America]	Accepted. We added "and it remains uncertain whether past changes in Atlantic tropical cyclone activity are outside the range of natural variability. "
126045	97	17			"showed that" should be "suggested that". [Trigg Talley, United States of America]	Accepted.
74573	97	18	97	18	Murakamy, submitted To check if it isn't published [Moulay Driss HASNAOUI, Morocco]	Accepted. It is now published and correctly cited here.
126047	97	18			Elaborate: "… variability, and external forcing (anthropogenic aerosols and volcanic eruptions) also played an important role, particularly in the North Atlantic basin (Murakami et al. submitted)." [Trigg Talley, United States of America]	Accepted. It is now published and correctly cited here.
1499	97	20	97	20	Where? Globally or in one ocean basin? And what was the number? (2005 was also crazy TC year in the North-Atlantic) [Rasmus Benestad, Norway]	Noted. The remainder of the paragraph details specific basins. The anthropogenic influence on the 2005 Atlantic season has not been studied.
39321	97	20	98	7	It is being suggested that these paragraphs on the attribution studies of tropical cyclone activity be synthesized and that main findings be qualified with uncertainty language. The literature review is not what is expected of assessment reports. [Lourdes Tibig, Philippines]	Taken into account. We added a summary paragraph with uncertainty language. [See also #39323.]
43405	97	21			Read " Murakami et al. (2017) explored the unusually " rather than " Murakami et al., 2017 explored the unusually " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. Citation is corrected.
80703	97	25	97	25	It is El Niño, not El Nino [Helene Jacot Des Combes, Marshall Islands]	Accepted. Corrected.
13775	97	25	97	25	Change El Nino by El Niño [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Corrected.
105969	97	32	97	34	Trenberth, et al. (2015) identify a notable climate impact on Hurricane Sandy. They find that when ocean heat content was reduced by an amount commensurate with observed warming to 2012 in an ECMWF model, Sandy's sustained wind speed decreased by 3.6 m/s the storm depth decreased by 7.6 hPa, and its precipitation was reduced by 35%. They also suggest that the storm surge was influenced by roughly 19 cm from sea level rise contribution. Trenberth, Kevin E., John T. Fasullo, and Theodore G. Shepherd. "Attribution of Climate Extreme Events." Nature Climate Change 5, no. 8 (August 2015): 725–30. https://doi.org/10.1038/nclimate2657. [Sohum Pawar, United States of America]	Noted. Trenberth et al. is simply repeating the original findings of Magnusson et al. (2012)and we elect to use that citation. As this section does not discuss stearic sea level rise, we do not include that part of the comment. This sentence was added at line 34: "On the other hand, Magnusson et al., (2014) found that in ECMWF simulations, that the simulated cyclone depth and intensity as well as precipitation, were larger when their model was driven by the warmer actual SST than the climatological average SST."
15575	97	34	97	34	Please replace "typhoon Haiyan" by "Super Typhoon Haiyan". [SAI MING LEE, China]	Accepted. "super" is added.
126049	97	38			Add: "… anthropogenic forcing in their model." [Trigg Talley, United States of America]	Accepted. Added "in their model" after "… anthropogenic forcing".
80705	97	39	97	41	There are 2 different references in this sentence for the same information. [Helene Jacot Des Combes, Marshall Islands]	Taken into account. This sentence is modified to consistently refer to these references.
91053	97	53	98	1	Additional reference on Hurricane Harvey [Richard Smith, United States of America]	Noted.
91055	97	53	98	1	Russell et al. (2020) analyzed the spatial distribution of extreme rainfall over six states adjoining the Gulf of Mexico, finding that high sea surface temperatures in the Gulf of Mexico increased the probability of extreme precipitation events over the whole region, but especially for a Hurricane Harvey type of event in the region of Houston. [Richard Smith, United States of America]	Noted. This study is a statistical approach, leading to a similar result as previous papers. 1) Russell, Brook T., Risser, Mark D., Smith, Richard L., Kunkel, Kenneth E. Investigating the association between late spring Gulf of Mexico sea surface temperatures and U.S. Gulf Coast precipitation extremes with focus on Hurricane Harvey. Environmetrics. 2020, vol. 31, no. 2, p. e2595. https://doi.org/10.1002/env.2595.
91057	97	53	98	1	B. Russell, M. Risser, R.L Smith and K.E. Kunkel (2020), Investigating the association between late spring Gulf of Mexico sea surface temperatures and US Gulf Coast precipitation extremes with focus on Hurricane Harvey Environmetrics, Vol. 31, issue 2, March 2020, paper e2595 (posted online July 23, 2019) . Supplementary Materials [Richard Smith, United States of America]	Noted. See response to #91055.
117119	97		97		extreme rather than super? [Valerie Masson-Delmotte, France]	Accepted. (p.97,L25) "super El Nino" is changed to "extreme El Nino", as consistent with SROCC and Annex IV.
11737	98	1	98	7	have any studies attributed Cyclone Idai, which hit Mozambique in 2019 with major destructive power, to effects of climate change? If possible to cover this example too in this subsection, i.e., if scientific analysis of the event frequency/magnitude for this storm have been published, it would be good to include an example of a devastating extreme event affecting an African region. [Amy East, United States of America]	Noted. Unfortunately, we are not aware of any attribution studies for Cyclone Idai.

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126051	98	2			Delete "during Harvey" here as the van Oldenbourgh study focused on extreme precipitation in general in climate models, not hurricane precipitation, or Hurricane Harvey's precipitation. Also, the Wang et al. (2018b) reference could be deleted, as they looked at the impact of environmental changes on Harvey's precipitation, but those changes were not attributed to anthropogenic forcing so no idea what part of them are natural vs. anthropogenically forced, so they have little relevance to the topic of detection attribution here. [Trigg Talley, United States of America]	Noted. van Oldenbourgh surely argues Harvey's precipitation. Wang et al. also their sensitivity experiments are motivated by anthropogenic warming.
72205	98	4	98	5	The reference provided for the risk of urbanization driving flooding during Hurricane Harvey (Zhang et al 2018d) is contradicted by the United States Geological Service (USGS) publication The 100-year Flood (URL below) that reports: "The effects of development (conversion of land from forested or agricultural uses to commercial, residential, or industrial uses) on peak flows is generally much greater for low-recurrence interval floods than for high-recurrence interval floods, such as 25- 50- or 100-year floods. During these larger floods, the soil is saturated and does not have the capacity to absorb additional rainfall. Under these conditions, essentially all of the rain that falls, whether on paved surfaces or on saturated soil, runs off and becomes streamflow." https://www.usgs.gov/special-topic/water-science-school/science/100-year-flood?qt-science_center_objects=0#qt-science_center_objects. Further Zhang et al 2018 failed to account for the increase in resilience that accompanies development, e.g. flood control, a major factor in reducing the risk of flooding. See USGS publication Flow Modification in the Nations's Streams and Rivers 2019 (https://doi.org/10.3133/cir1461) which reports that due to the "modification of natural flows in streams and rivers... high flows have been reduced in magnitude, are of shorter duration, are less frequent, and vary less from one year to the next than they would naturally." See also Formeta and Feyen, 2019: Empirical evidence of declining global vulnerability to climate-related hazards (https://doi.org/10.1016/j.gloenvcha.2019.05.004) which reports increasing resilience to extreme weather events with increasing development. As such there the findings of Zhang et al 2018d is not consistent with other evidence. [Hunter Cutting, United States of America]	Considered. Storyline analyses are by their very nature incomplete assessments of total risk. The Zhang et al study looks at surface roughness change from urbanization to draw their conclusion about a 21-fold increase in risk, all else being equal. Things such as flood control are of course, not part of this storyline Furthermore, the return time of Harvey precipitation was estimated to be at least a 1000 years and the flood depth accordingly larger. As the USGS report deals with 100 year floods (and it is unlikely that flood controls consider millennial floods). There is no contradiction
126053	98	5	98	7	Not sure why this study is highlighted. The key thing is not whether a forecast model can be run with altered boundary conditions and model some possible anthropogenic contribution to rainfall in advance of an event. The important issue is whether researchers can convincingly demonstrate, through models, observations, theory, and analysis, that anthropogenic forcing likely had some contribution to an event, with estimated uncertainty bounds on various findings. Scientific understanding is key, not speed of information going to public/media. Peer review of findings is also important as a quality control on information going from the scientific community to the broader world. [Trigg Talley, United States of America]	Considered. We realize that the wording in the original writing can be misinterpreted as highlighting Zhang et al. (2018d). The text is modified.
18065	98	5	98	7	Can update this sentence now that the full study has come out (Reed et al. (2020), https://doi.org/10.1126/sciadv.aaw9253). Reed et al. (2020) used the conditional hindcast attribution technique (e.g. - Patricola and Wehner, 2018) with a variable-resolution global climate model to conclude that Hurricane Florence's mean total overland rainfall was increased by about 5% due to anthropogenic climate change. [Alyssa Stansfield, United States of America]	Taken into account. The reference is updated as Reed et al. (2020).
24101	98	7	98	7	Why is there no summary statement on attribution of TCs? Looks like a pretty important omission. [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We added a summary paragraph.
29279	98	7	98	7	I believe that 2008 should be changed to 2018. [Andra Garner, United States of America]	Accepted. Corrected.
6797	98	7	98	7	There was a hurricane called Florence in 2018, so is the "2008" stated here a typo? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected.
15577	98	12	98	16	The assessment TC projections conducted by the Expert Team of the UNESCAP/WMO Typhoon Committee on the impacts of climate change on tropical cyclone activity in the western North Pacific should also be mentioned in this Section. Reference : Cha, E.J., T.R. Knutson, T.C. Lee, M. Ying and T. Nakaegawa, 2020 : Third Assessment on Impacts of Climate Change on Tropical Cyclones in the Typhoon Committee Region – Part II : Future Projections, Tropical Cyclone Research and Review, In Press. [SAI MING LEE, China]	Accepted. Cha et al. (2020) is added as a reference.
15579	98	12	100	33	A figure showing the projected changes of various TC metrics in different basins as depicted in the assessments conducted by WMO and UNESCAP/WMO Typhoon Committee expert teams should be included in the Chapter. Reference: 1. Knutson, T. R., S. J. Camargo, J. C. L. Chan, K. Emanuel, C. H. Ho, J. Kossin, M. Mohapatra, M. Satoh, M. Sugi, K. Walsh, L. Wu, 2019b : Tropical Cyclones and Climate Change Assessment: Part II. Projected Response to Anthropogenic Warming, Bull. Amer. Meteorol. Soc., https://doi.org/10.1175/BAMS-D-18-0194.1. 2. Cha, E.J., T.R. Knutson, T.C. Lee, M. Ying and T. Nakaegawa, 2020 : Third Assessment on Impacts of Climate Change on Tropical Cyclones in the Typhoon Committee Region – Part II : Future Projections, Tropical Cyclone Research and Review, In Press. [SAI MING LEE, China]	Noted. A figure was dropped at the final stage of FGD.
43407	98	13			Read "by Knutson et al. (2019b), " rather than "by (Knutson et al., 2019b), " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. Corrected.
26189	98	15	98	16	"although there are some differences … IPCC and WMO reports" This may be unnecessary, rather confusing. [Masato Sugi, Japan]	Rejected. This is an important notice. There are several possible reasons for the differences. First there have been some new studies not covered in the WMO assessment which can potentially alter confidence levels. Second, IPCC AR6 does not use split confidence levels like “Low to Medium”, so cases where those were given in the WMO assessment apparently need to be altered for IPCC AR6. Third, the confidence level guidance given by IPCC AR6 corresponds to the “Type I error avoidance” assessment statements in the WMO assessment. In contrast, the “Type II error avoidance” statements in the WMO assessment—these are not used in IPCC so the balance of evidence “Type II error avoidance” statements in the WMO report therefore lead to different confidence in the IPCC AR6 framework. Finally, differences between the reports could arise due to differences in author opinion on the confidence levels. In the WMO report, a distribution of confidence levels across the 11-member author team was provided, and the summary assessment statements were based on confidence levels that were adopted by a majority of the authors. IPCC AR6 presents a single confidence level statement in each case, which can lead to differences between the reports.

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126055	98	15	98	16	It is unclear what differences in confidence calibrations between IPCC and WMO reports (Knutson et al., 2019a,b) are being referred to here. The WMO author team was attempting to follow IPCC guidelines from AR5 on confidence levels; so, for example, "medium confidence" should mean the same thing in the two reports in terms of the aims of the authors. Differences can arise because the authors for AR6 disagree with authors of the WMO report about a confidence level for a particular finding. (The WMO report presented a distribution of confidence level opinions across their author team.) But there is no inherent difference in the guidance for the confidence levels. The WMO report does bring in a different aspect not currently used in the AR6 draft (alternative assessment with a Type II error avoidance focus) but that was for the detection and attribution part or the report (Knutson et al., 2019a), not the projections part (Knutson et al., 2019b). [Trigg Talley, United States of America]	Noted. The confidence level in this section is different from the WMO report. This is an independent assessment of literature by different experts, and also partly because the intermediate confidence levels such as "medium-high confidence" are difficult to determine and are not used here.
41483	98	18	98	21	Knutson et al., 2019b does include a large variety of studies and not only studies based on high-resolution models. I am not sure if the statement is backed up by the reference. The statement also seems to be in conflict with line 44-47 of the same page. [Alexander Nauels, Germany]	Noted. The combined sentences with the sentence based on Knutson et al. (2019b) (L18-21) and the following sentence (L21-22) are consistent with L44-47.
62485	98	18	98	32	The results of Yoshida et al., 2017 also can be cited in this paragraph where authors concluded that the global number of TCs decreases by 33% in the future projection simulated using 60km global atmospheric model. [Yoshida, K., Sugi, M., Mizuta, R., Murakami, H., & Ishii, M. (2017). Future changes in tropical cyclone activity in high-resolution large-ensemble simulations. Geophysical Research Letters, 44,9910–9917. https://doi.org/10.1002/2017GL075058.] [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. This reference is added,
18067	98	18	98	32	Could add a sentence on Fedorov et al. (2019), which used a cloud-resolving model that explicity represents moist convection and investigated the response of TCs to decreases in the meridional SST gradient. They found that the overall number of TCs increases and also an increase in the occurance of "hybrid" storms that share characteristics of TCs and warm-core extratropical cyclones. (Fedorov, A.V., Muir, L., Boos, W.R. et al. Tropical cyclogenesis in warm climates simulated by a cloud-system resolving model. Clim Dyn 52, 107–127 (2019). https://doi.org/10.1007/s00382-018-4134-2) [Alyssa Stansfield, United States of America]	Noted. This is a process study using an idealized framework. Can be referred to if a new mechanism is proposed. This paper describes generally accepted concept in which TC genesis is sensitive on SST distribution.
1501	98	18	99	5	There has been one suggested driver, however (DOI: 10.5194/nhess-9-635-2009): the area of the warm sea surface (SST above the critical threshold 26.5C). An increase in the frequency of the most intense TCs and a decrease in the overall TC frequency implies a change in the pdf for the windspeed. Usually, the pdf for the windspeed is taken to follow a Weibull distribution, and while it's possible to change its shape so that there is an increase in the frequency of the most extreme cases and a reduction in the frequence of all cases above the critical threshold defining a TC, the vast majority of the possibilities for the parameters (shape and scale) suggest that the change in both moderate extreme and the most estreme wind speeds are the same. The shape of a pdf provides a fingerprint of the underlying physical processes. Hence a change in the sense of trend for different intensity TCs implies a change in the shape of the pdf, which would be interesting to explain. [Rasmus Benestad, Norway]	Noted. Ambiguity of projected SST distributions causes uncertainties of future TC distributions. This paper describes generally accepted concept in which TC genesis is sensitive on SST distribution. This is the reviewer's paper: 1) Benestad, R. E. On tropical cyclone frequency and the warm pool area. Natural Hazards and Earth System Sciences. 2009, vol. 9, no. 2, p. 635–645. https://www.nat-hazards-earth-syst-sci.net/9/635/2009/.
13777	98	21	98	22	Change Bhatia et al., 2018 and Vecchi et al., 2019) by Bhatia et al. (2018) and Vecchi et al. (2019) [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Corrected.
43409	98	21		22	Read "coupled model results are noted in Bhatia et al. (2018) and Vecchi et al. (2019)." rather than "coupled model results are noted in Bhatia et al., 2018 and Vecchi et al., 2019)." [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. Corrected.
26191	98	24	98	24	Add "Yamada et al. 2020" after "Sugi 2019". Yamada et al. 2020 (submitted to PEPS) also showed that most HighResMIP models projected a decrease in TC seeds. Yamada, Y., C. Kodama, M. Satoh, M. Sugi, M. J. Roberts, R. Mizuta, A. T. Noda, T. Nasuno, M. Nakano, P. L. Vidale, 2020: Evaluation of the contribution of tropical cyclone seeds to changes in tropical cyclone frequency due to global warming in high-resolution multi-model ensemble simulations. (submitted to PEPS) [Masato Sugi, Japan]	Accepted. The paper is added.
126057	98	29			Reword to clarify: "In a different approach, a statistical-dynamical downscaling framework assuming constant seeding rate with warming (Emanuel, 2013), exhibits ..." [Trigg Talley, United States of America]	Accepted. The text is reworded.
126059	98	30	98	32	Suggested rewording: "This disparity in the sign of the projected change in global TC frequency shown by a small number of studies, and the difficulty in explaining the mechanisms behind the different signed responses, further emphasize the lack of process understanding ..." [Trigg Talley, United States of America]	Accepted. The text is reworded as " "This disparity in the sign of the projected change in global TC frequency and the difficulty in explaining the mechanisms behind the different signed responses further emphasize the lack of process understanding ..."
62487	98	34	98	35	The study of Bacmeister et al., 2018 can be considered as a citation for future projections where the bias correction of the sea-surface temperatures (SSTs) was carried out in the 28 km horizontal resolution global model. It says that the impact of mitigating from RCP8.5 to RCP4.5 is explicitly considered and is compared with uncertainties arising from SST projections. [Julio T. Bacmeister, Kevin A. Reed, Cecile Hannay, Peter Lawrence, Susan Bates, John E. Truesdale, Nan Rosenbloom, Michael Levy, 2018. Projected changes in tropical cyclone activity under future warming scenarios using a high-resolution climate model. Climatic Change, 146: 547–560] [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. This work is cited elsewhere in the section.
26193	98	36	98	36	Add " 2002" after "Sugi et al." Sugi et al. 2002 is the first paper that showed the close relationship between the weakening of tropical circulation (vertical mass flux) and the reduction of TC frequency due to global warmng. Sugi, M., A. Noda and N. Sato, 2002: Influence of Global Warming on Tropical Cyclone Climatology: An Experiment with the JMA Global Model. J. Meteor. Soc. Japan, 80, 249-272. [Masato Sugi, Japan]	Noted. Sugi et al. (2012) is already referred to, and is more relevant and newer. No need to refer to Sugi et al. (2002).
26195	98	37	98	37	Modify "intense TCs" to "TC intensity", and add "associated with each TC" after "vertical mass flux". [Masato Sugi, Japan]	Taken into account. We take the suggestion to clarify this sentence, but use different words. It now reads "...further posits that the robust simulated increase in the number of intense TCs, and hence increased vertical mass flux associated with intense TCs, must lead to a decrease in overall TC frequency because of this association. "
126061	98	39			Revise to: "... decreases of one climate model by altering ..." [Trigg Talley, United States of America]	Noted. The paper is specific for one climate model, but we think that in principle this can be applied to other models.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39323	98	49	100	21	Similarly, these paragraphs on projections of changes inTC characteristics be synthesized and use of uncertainty language is encouraged. The Summary at the end does not capture some of the climate messages found in the literature review-manner in which it is presented. [Lourdes Tibig, Philippines]	Taken into account. We added a summary paragraph with uncertainty language. [See also #39321.]
26197	98	50	98	50	Add "Knutson et al. 2019b" before "Wehner et al., 2018a" [Masato Sugi, Japan]	Taken into account. Knutson et al. (2020,BAMS) is added to this sentence.
82807	98	52	98	52	Presumably this refers to systems of tropical storm intensity? (the term "category 0" is not widely used in the community). [Blair Trewin, Australia]	Taken into account. This sentence is modified. Not necessarily tropical storm but category 1 (hurricanes) are included.
26199	98	53	98	53	Add "Knutson et al. 2019b" before "Wehner et al., 2018a" [Masato Sugi, Japan]	Taken into account. Knutson et al. (2020,BAMS) is added to this sentence.
13779	99	1	99	1	Change Fig. by Figure [Maria Amparo Martinez Arroyo, Mexico]	Noted. This sentence was changed and moved to the end of this paragraph.
43411	99	1		2	Read " of Knutson et al. (2019c), and in Roberts et al. (2019b)." rather than " of Knutson et al., 2019c, and in Roberts et al., 2019b)." [Cyriaque Rufin Nguimalet, Central African Republic]	Noted. This sentence was changed and moved to the end of this paragraph.
41485	99	2	99	2	Knutson et al., 2019c is not listed in the references. [Alexander Nauels, Germany]	Noted. This sentence was changed and moved to the end of this paragraph.
110961	99	2	99	2	No Knutson et al., 2019c in the reference list. [Ning Zhao, Germany]	Accepted. The reference was corrected as Knutson et al. (2020).
9899	99	2	99	2	in may be unnecessary in this context. Consider removing it. [ayman badawy, Egypt]	Noted. This sentence was changed and moved to the end of this paragraph.
9973	99	2	99	2	It appears that the preposition in may be unnecessary in this context. Consider removing it. [ayman badawy, Egypt]	Noted. The text was modified.
110909	99	2	99	3	Regarding the note that hiresmip will be included in the FGD. I recommend the authors also include and cite/summarize some of the results from CORDEX in this section on projections of tropical cyclones for comparison. I don't see that TC in CORDEX have been assessed in other chapters. The resolution is on the order of that from hiresmip, so it would make for an appropriate comparison. For example, Diro et al 2014 examine TC projections in the Central American CORDEX domain, and Rendfrey et al 2020 examine projections from North American CORDEX. (These two papers also use the same tracking algorithm.). The latter examined many of the quantities discussed in this section (size, translations speed, intensity, precipitation, duration), and in terms of projections of spatial distribution of TC, the two studies show general agreement. There are other papers out there too that examine other basins, depending on CORDEX domain (several showed up at the top of my list in a quick google scholar search). Diro, G.T., Giorgi, F., Fuentes-Franco, R., Walsh, K.J.E., Giuliani, G. and Coppola, E., 2014. Tropical cyclones in a regional climate change projection with RegCM4 over the CORDEX Central America domain. Climatic Change, 125, 79-94. Rendfrey T.S., M. S. Bukovsky, R. R. McCrary, R. Fuentes-Franco, 2020. An assessment of tropical cylones in North American CORDEX WRF simulations. Weather and Climate Extremes, submitted September 2019 (revised and resubmitted May 2020). [Melissa Bukovsky, United States of America]	Taken into account. CORDEX results are included.
69249	99	7	99	14	Although the changes in TC maximum surface wind speeds are summarized in this paragraph, it would be invaluable to describe the changes in TC minimum central pressure as well. TC minimum central pressure is frequently used for impact assessment and adaptation. [Kaoru Magosaki, Japan]	Taken into account. TC intensity is defined by using either maximum wind speeds or minimum surface pressure in Knutson et al. (2020), A CRM study by Tshuboki et al. (2015) shows the deepest TC under a warming condition in WNP is 857 hPa.
24455	99	7	99	14	Changes in wind speed is shown in this paragraph. It is better to add description of changes in minimum lowest pressure for WGII. The minimum central pressure uses by impact assessment modelers, frequently. [Nobuhito Mori, Japan]	Taken into account. TC intensity is defined by using either maximum wind speeds or minimum surface pressure in Knutson et al. (2020), A CRM study by Tshuboki et al. (2015) shows the deepest TC under a warming condition in WNP is 857 hPa.
69251	99	7	100	33	The changes in TC maximum wind speeds, tracks and other TC statistics are described in these paragraphs, qualitatively. Although changes in TC rainfall are described quantitatively, TC intensity, tracks and size would also be important to be described quantitatively. The mean and range of uncertainty of projections are invaluable for instance, for adaptation activities. [Kaoru Magosaki, Japan]	Noted. In this report, only the summary of the storm changes is shown by Figure 11.20 because of the limitation of space.
26201	99	12	99	14	Move "These projetions can vary … Yoshida et al., 2017)" to Line 5, after "(Knutson et al., 2019b)." That the projections of CAT4-5 TC frequency vary substantially between the ocean basins (or even within an ocean basin) is the reason why the global Category 4-5 TC frequency is slightly reduced or almost unchanged. [Masato Sugi, Japan]	Taken into account. The text was modified and this sentence was moved to the previous paragraph.
126063	99	14			This reference may be relevant: Villarini, G., and G.A. Vecchi, Projected increases in North Atlantic tropical cyclone intensity from CMIP5 models, Journal of Climate, 26(10), 3231-3240, 2013. [Trigg Talley, United States of America]	Noted. This is a paper specific to NA and does not consider differences between basins.
62489	99	16	99	18	It may not be appropriate say generally agree. Use standard likelihood and confidence level classification consistently. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. Text is revised to avoid "generally agree".
62491	99	16	99	18	Cite examples for existing studies rather than simply quoting from Knutson et al.,18 2019b) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Knutson et al. (2019b) is a review article and a more complete citation.
24515	99	16	99	25	Hatsuzuka et al. (2020) showed increase of tropical cyclone-induced heavy precipitation across Japan even though the intensity of TC remains unchanged. Please consider this paper as a contribution to this paragraph. Hatsuzuka, D., T. Sato, K. Yoshida, M. Ishii, R. Mizuta, 2020: Regional projection of tropical-cyclone-induced extreme precipitation around Japan based on large ensemble simulations, SOLA, 16, 23-29, DOI:10.2151/sola.2020-005 [Tomonori Sato, Japan]	Accepted. The reference was added with an appropriate sentence.
126065	99	17			This reference can be relevant: Villarini, G., D.A. Lavers, E. Scoccimarro, M. Zhao, M.F. Wehner, G.A. Vecchi, T.R. Knutson, and K.A. Reed, Sensitivity of tropical cyclone rainfall to idealized global scale forcings, Journal of Climate, 27(12), 4622-4641, 2014. [Trigg Talley, United States of America]	Noted. It would be relevant, but we are referencing the specific consensus result of "12% for a 2oC global warming "
26203	99	19	99	19	Add "TC" before "intensity". [Masato Sugi, Japan]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9975	99	19	99	19	The phrase has been shown to be may be wordy. Consider changing the wording. (is) [ayman badawy, Egypt]	Accepted.
42541	99	20	99	20	Typo: 2016).Projections -> 2016). Projections [Joan Bech, Spain]	Accepted.
13781	99	20	99	20	Change .Projections by . Projections [Maria Amparo Martinez Arroyo, Mexico]	Accepted.
18073	99	21	99	25	Can add Stansfield et al. (2020) to this discussion, which showed that under two RCP scenarios, precipitation rates within North Atlantic TCs and the amount of precipitation produced per hour of TC impact in the North Atlantic both are projected to increase by the end of the century compared to a historical simulation. However, the annual average TC-related Rx5day (annual maximum 5 day precipitation) over the eastern United States is projected to decrease because of a decrease in landfalling TCs. (Stansfield et al. (2020), Changes in Precipitation from North Atlantic Tropical Cyclones under RCP Scenarios in the Variable-Resolution Community Atmosphere Model, Geophysical Research Letters. doi: 10.1029/2019GL086930) [Alyssa Stansfield, United States of America]	Accepted. Stansfield et al. (2020) is referred to as projected TC precipitation changes in the North Atlantic.
62493	99	27	99	28	The time range of the projections is important to mention in this sentence. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Throughout this section, projection in the late 21st century or 2deg warming level is considered.
24457	99	27	99	37	The description of future changes in TC is too qualitative. It is better to write down numbers (e.g. range of poleward shift in degree, changes in translation speed in m/s). This kind of qualitative or narrative information is difficult to use by impact assessment or policy makers. [Nobuhito Mori, Japan]	Taken into account. The mean migration rate in WNP is referred to based on Kossin (2016,JCLI).
109993	99	27	99	37	It is probably worth being explicit per basin in this paragraph rather than using a catch-all remaining basins. Also, it would be worth considering whether to cover potential for TC genesis in either the S. Atlantic or the mediterannean basin which is, presumably, of high interest to policy makers in those regions? [Peter Thorne, Ireland]	Taken into account. Poleward migration is unclear in the eastern Pacific, South Pacific, and Indian Ocean. TC possibility in South Atlantic and Mediterranean should be considered in other sections.
66159	99	35	99	37	"There is presently no clear consensus in projected changes in TC translation speed (Knutson et al., 2019b), although recent studies suggest a slowdown outside of the tropics (Yamaguchi et al., 2019;Zhang et al., In review) ." - I believe Jim Kossin has a paper that was either recently published (2019 or 2020) or that has been submitted which touches on global TC translational speed. [Marjahn Finlayson, Bahamas]	Taken into account. Kossin (2019,Nature, reply) is added.
18071	99	39	99	48	Perhaps add a sentence about how TCs in nature vary in size substantially (Chavas D. R. and K. A. Emanuel (2010), A QuikSCAT climatology of tropical cyclone size, Geophys. Res. Lett., 37, L18816, doi:10.1029/2010GL044558.) and there is no definite theory on what controls TC size, although this is an area of active research Citations: Chavas D. R. and K. A. Emanuel, 2014 (https://doi.org/10.1175/JAS-D-13-0155.1); Chavas and Reed, 2019 (https://journals.ametsoc.org/doi/pdf/10.1175/JAS-D-19-0001.1)) [Alyssa Stansfield, United States of America]	Taken into account. The observational results and theoretical understanding are added here.
18069	99	40	99	40	Reed et al. (2020) attributed a 1-2% increase in Hurricane Florence's size due to anthropogenic climate change (Reed et al. (2020), https://doi.org/10.1126/sciadv.aaw9253). [Alyssa Stansfield, United States of America]	Noted. This is an attribution study, not projection.
126067	99	42			Suggest change to: "... intensity (Yamada et al., 2017). In contrast, the TC downscaling study of Knutson et al. (2015) simulates a reasonable interbasin distribution of TC size climatology, but projects no statistically significant change in global average TC size." [Trigg Talley, United States of America]	Taken into account. The sentence is reworded.
69253	99	50	100	9	The projections of extreme sea levels (mainly storm surges) are discussed in Chapter 9.6.4.2. Please check again and add the corresponding references from Chapter 9. [Kaoru Magosaki, Japan]	Accepted. The corresponding section in Chapter 9 (9.6.4.2) is referred to.
24459	99	50	100	9	Similar studies have been conducted in relation to the characteristics of typhoons and storm surges by Mori et al. (2019) globally and Yang et al. (2018) around Korea. Mori, N., T. Shimura, K. Yoshida, R. Mizuta, Y. Okada, M. Fujita, T. Temur Khujanazarov, E. Nakakita (2019) Future changes in extreme storm surges based on mega-ensemble projection using 60-km resolution atmospheric global circulation model, Coastal Engineering Journal, Taylor & Francis, 61:3, pp.295-307. doi:10.1080/21664250.2019.1586290 Yang, J.A, S.Y. Kim, N. Mori, H. Mase (2018) Assessment of long-term impact of storm surges around the Korean Peninsula based on a large ensemble of climate projections, Coastal Engineering, Elsevier, Vol.142, pp.1-8. doi: 10.1016/j.coastaleng.2018.09.008 [Nobuhito Mori, Japan]	Taken into account. These two references are referred to as studies in East Asia.
40635	99	50	100	9	Please check that use of the term 'risk' in this paragraph is consistent with IPCC usage. If simply you're referring to the physical hazard, then you shouldn't use the term 'risk'. Risk, as defined by the IPCC, also factors in exposure and vulnerability. [TSU WGI, France]	Taken into account We changed the text without using "risk".
44401	99	52	99	53	remove the word "risk". See IPCC guidance on risk for appropriate use of the term "risk" throughoput the IPCC report. [Jana Sillmann, Norway]	Taken into account We changed the text without using "risk".
15581	100	2	100	9	Suggest including the reference below (Chen et al., 2020) which used the pseudo-global-warming (PGW) technique to investigate the changes in peak intensity and induced storm surge of western North Pacific land-falling tropical cyclones due to warmer climate conditions: Chen, J., Z.Q. Wang, C.Y. Tam, N.C. Lau, D.S. Lau, H.Y. Mok, 2020 : Impacts of climate change on tropical cyclones and induced storm surges in the Pearl River Delta region using pseudo-global-warming method, Sci Rep 10, 1965. https://doi.org/10.1038/s41598-020-58824-8 [SAI MING LEE, China]	Taken into account. This reference is added.
44373	100	5	100	5	the use of "flood risk" is not really justified in this context (see definition of the term risk in IPCC). It should be rephraseed to "increase in the probability of floods" [Jana Sillmann, Norway]	Taken into account We changed the text without using "risk".
13783	100	15	100	15	Change CMIP-5 by CMIP5 [Maria Amparo Martinez Arroyo, Mexico]	Accepted.
66163	100	15	100	17	"Greenhouse gas forcing in CMIP-5 and Community Earth System Model Large Ensemble (CESM-LE; Kay et al., 2015) simulations, however, erodes the pattern and degradesthe natural shear barrier along the U.S. coast." - Is there more information or studies or the rest of the North Atlantic Basin? Also is there any comments on TC track steering or direction, e.g. as described in Colbert & Soden 2012? [Marjahn Finlayson, Bahamas]	Noted.
66161	100	17	100	17	"pattern and degradesthe natural shear" - add space between degrades and the [Marjahn Finlayson, Bahamas]	Accepted.
110963	100	17	100	17	Space is missing between "degrades" and "the". [Ning Zhao, Germany]	Accepted.
43413	100	17			Read " and degrades the natural shear barrier " rather than " and degradesthe natural shear barrier " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
126069	100	23	100	24	[CONFIDENCE] Confidence level should be medium-to-high (as in the WMO TC/climate assessment, Knutson et al., 2019b) for the statement that average peak TC wind speeds and the proportion of Cat 4-5 TCs will increase globally with warming. If this is not available due to IPCC rules, then medium confidence for both. Rationale: There is good model agreement on an increase but evidence is still lacking for a clear detection of an observed increase (i.e., that an observed increase is highly unusual compared to expected changes realizable from natural variability only). This is an essential part of a case for high confidence in a projection (don't just rely on models and theory for confidence, but actually see the change unambiguously in the data and it's clearly distinguishable from natural variability). One of the tricky things about future changes of mean intensity (or of the related change in the shape of the pdf of intensity) is that it depends not just on SST but also on details of the atmospheric profile of temperature change in the tropics. Both models and theory indicate this. For Emanuel potential intensity it's through outflow temperature changes, while in a high resolution hurricane prediction model, it's the amount of upper tropospheric warming relative to surface warming as discussed, for example, in Tuleya et al. (2016: Impact of upper tropospheric temperature anomalies and vertical wind shear on tropical cyclone evolution using an idealized version of the operational GFDL hurricane model. Journal of the Atmospheric Sciences, 73(10), DOI:10.1175/JAS-D-16-0045.1). Unfortunately there is also uncertainty in precisely how tropical upper tropospheric temperature is going to evolve in the future, which contributes to uncertainty in the TC intensity projections, along with changes in subsurface ocean stratification, salinity, etc. [Trigg Talley, United States of America]	Rejected. The high confidence is based on agreement of model simulations and theory and change already detectable is not required. - Theory, Upper tropospheric temperature is one aspect, but not all. The vertical temperature profile is not independent of SST. The temperature profile changes with SST as moist adiabatically.
26205	100	23	100	33	The summary should be itemized with numbers: 1) to 7). [Masato Sugi, Japan]	Rejected. Summary is in general not itemized.
26207	100	25	100	25	Add "over the northern part of North Pacific" after "Category 4-5 TCs". Note that the frequency of Category 4-5 TCs will increase in limitted regions, not globally, as indicated by "slightly reduced or almost unchanged" (Page 99, Line 5) [Masato Sugi, Japan]	Taken into account. This information is added.
126071	100	25	100	26	[CONFIDENCE] "There is high confidence that average tropical cyclone rain-rates will increase with warming." Recommend medium-to-high confidence (as in the WMO TC/climate assessment, Knutson et al., 2019b). If this is not available due to IPCC rules, then medium confidence. This projection has high level of agreement among existing modeling studies (although not as many studies have examined this as TC frequency change), the mechanistic understanding is strong, as is support for anthropogenic increases in total precipitable water, a key ingredient. What remains missing is a clear detection of an observed increase (i.e., that an observed increase is highly unusual compared to expected changes realizable from natural variability only). This is an essential part of a case for high confidence in a projection (don't just rely on models and theory for confidence, but actually see the change unambiguously in the data, and it's clearly distinguishable from natural variability). Recent detection/attribution studies for the Harvey event by Risser and Wehner and van Oldenbourgh et al., while of high quality, analyze observed long-term changes in extreme precipitation in general, not tropical cyclone precipitation. Additionally, the Harvey event was mainly due to the multi-day stall-out of the hurricane in the region. [Trigg Talley, United States of America]	Rejected. Confidence level can be high even if observed trends is not detectable. Evidence based on EA studies.
11739	100	26	100	26	remove hyphen (“rain rates”) [Amy East, United States of America]	Accepted.
109995	100	26	100	29	I did not see sufficient explicit support for this scaling argument in the prior text. Maybe it just wasn't made explicit enough? [Peter Thorne, Ireland]	Noted. This is explicitly stated in the text, p.99, L16-20 in SOD.
26209	100	28	100	28	Add "TC" before "wind-" [Masato Sugi, Japan]	Accepted.
1503	100	31	100	31	I disagree with the 'medium confidence' concerning the global frequency of TCs over all categories because (a) the report does not include all relevant results/papers, (b) the GCMs have a large number of caveats concerning SST biases and representation of cloud processes, and (c) there are no clear explanation for why there should be different trends for TCs of different intensity (which would also mean that the shape of the pdf for windspeeds would change, suggesting a change in the physical system/processes). Empirical studies indicate a dependency of the number of tropical cyclones on the area of the warm oceans, which are expected to expand (hence the migration of TCs to higher latitudes). [Rasmus Benestad, Norway]	Noted. These are extensively assessed in the texts p. 98, L18-47 (SOD).
26211	100	32	100	32	Delete "or remain unchanged". There is no reason to include this phrase only for this item. [Masato Sugi, Japan]	Rejected. This is a specific message on the projection of the frequency of TC.
130561	100	36	100	36	Sub-section title "Midlatitude storms" change to "Extropical storms" is more appropraite. [Panmao Zhai, China]	Accepted. Section name has been changed as suggested.
11741	100	36	100	51	the introductory paragraph of section 11.7.2 makes it sound as though atmospheric rivers will be covered in detail in this section, but then we hear almost exclusively about ETCs and almost nothing about ARs. I realize that another chapter seems to have a separate section on ARs, but the way this intro paragraph is set up, readers are expecting some information about them here too that isn’t given [Amy East, United States of America]	Taken into account. This section now focuses in extratropical cyclones while ARs are assessed in CH. 8.3.2.8.
109123	100	36	104	44	The introduction to section 11.7.2 says it "assesses synoptic scale storms that affect midlatitude regions including extratropical cyclones 39 (ETCs) and atmospheric rivers (ARs)", but then it only talks about ETCs. There's a single paragraph near the end that says, basically, ARs exist and have impacts and are being studied, but that's it. This section needs significantly more discussion of ARs. In addtion to the multitude of references in section 8.2.3.2 that could inform this discussion, I think the manuscript recently submitted to the Journal of Hydrometeorology by Kelly Mahoney, et al, titled "Precipitation Projections for the Western United States in NA-CORDEX models," should be considered. [Seth McGinnis, United States of America]	Taken into account. This section now focuses in extratropical cyclones while ARs are assessed in CH. 8.3.2.8.
42543	100	41	100	41	Typo: the the -> the [Joan Bech, Spain]	Accepted. The text has been revised.
43415	100	41		42	Read "Since the AR5, the high relevance" rather than "Since the the AR5, the high relevance" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. The text has been revised.
9977	100	47	100	47	algorithms [ayman badawy, Egypt]	Accepted. The text has been revised.
1505	101	1	101	14	It might be relevant to point out that the detection of storms also is influenced by the spatial resolution of the reanalyses used. This is mentioned later under 'Model evaluation' on p. 102 L 16-25. The material seems to be split ut in a way that is not so easy for the readers to follow. [Rasmus Benestad, Norway]	Accepted. The discussion about the identification/tracking has been merged in Chapter 3.
9979	101	8	101	8	The phrase In addition may be wordy. Consider changing the wording. (also) [ayman badawy, Egypt]	Not applicable. This paragraph has been integrated in Chapter 2.
9981	101	13	101	13	The phrase are able to may be wordy. Consider changing the wording. (can) [ayman badawy, Egypt]	Not applicable. This paragraph has been integrated in Chapter 2.
29929	101	26	101	26	The assessment of observed trends in extratropical cyclones should include the findings from Chapter 2, section 2.3.1.4.3 [Juan Rivera, Argentina]	Noted. FGD refers to assessments by Chapter 2
110001	101	26			This is a largely redundant assessment with that undertaken in chapter 2. These should be reconciled and chapter 11 should, starting from that undertaken in chapter 2 add any necessary detail. [Peter Thorne, Ireland]	Noted. FGD refers to assessments by Chapter 2
1507	101	28	101	37	It is probably ERA5 better to use for storm detection as it has higher spatial resolution (~30 km grid as opposed to ~125 km). [Rasmus Benestad, Norway]	Noted. What is assessed here are the published literature.
109997	101	32	101	33	This is a very odd call-out particularly when there are also new versions of a whole host of products including NOAA 20CRv3 which is demonstrably superior to this demonstrator effort by ECMWF. Chapter 1 has introduced the reanalyses so rather than referring to one specific product I would suggest replacing this with a reference to the chapter 1 section. [Peter Thorne, Ireland]	Accepted. This discussion has been integrated within Chapter 2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6799	101	33	101	36	Poli et al.(2016) is a better reference for ERA-20C than Poli et al.(2013). It is already in the list of references for the chapter. More importantly, while heterogeneity in the type of data assimilated is an issue for reanalysis in general, it is less of one for ERA-20C, as it assimilated only in situ surface pressure and wind data, and thus (aside from changes in observation numbers) suffered heterogeneity only insofar as the way observations were made for these variables changed over the years. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This paragraph has been removed from this section.
42545	101	34	101	34	Typo: 2018)and -> 2018) and [Joan Bech, Spain]	Editorial. The final draft will undergo professional copy-editing prior to publication.
43417	101	34			Read "(Reboita et al., 2015; Varino et al., 2018) and due to " rather than "(Reboita et al., 2015; Varino et al., 2018)and due to " [Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. The final draft will undergo professional copy-editing prior to publication.
100827	101	36	101	36	Dell'Aquila et al. 2016 https://doi.org/10.1002/2016GL068829 also showed that centennial reanalyses as ERA20C or the XX Century reanalysis cannot be used to study trends in synoptic variability, e.g. ETC etc… [Corti Susanna, Italy]	Not applicable. This paragraph has been removed from this section.
20755	101	39	101	53	Apparently the privileged criterion is the number of ETC for which the depth of the central pressure depression exceeded some threshold. Are they any studies looking for features of the whole distribution of ETC as a function of the central depression? [philippe waldteufel, France]	Rejected. It is unclear what the reviewer is asking for.
82809	101	40	101	40	8 out of how many? [Blair Trewin, Australia]	Accepted. The text has been revised.
6801	101	40	101	40	It would be clearer to change "during the satellite era (since 1979)" to "since 1979" due to there being no clear-cut definition of the "satellite era". In the context of this particular reference, it should be remembered that reanalyses that span years prior to 1979 may use bogussing or pseudo-obs (such as the Australian PAOBs) that were produced making use of imagery provided by satellites that were in orbit prior to 1979, in addition to the VTPR sounding data noted earlier. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text has been revised.
42547	101	50	101	50	Typo: the some -> some (please check) [Joan Bech, Spain]	Accepted. The text has been revised.
6803	101	50	101	50	ERA-Interim and MERRA are no longer in production. They have been replaced by ERA5 and MERRA-2, so ERA-Interim and MERRA should not be referred to as "latest high-resolution reanalyses", even if they were so at the time the referenced article was published in 2013. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text has been revised.
110003	102	5			This is a largely redundant assessment with that undertaken in chapter 3. These should be reconciled and chapter 11 should, starting from that undertaken in chapter 3 add any necessary detail. [Peter Thorne, Ireland]	Noted. This subsection is shortened and starting from assessment available from earlier chapters.
80609	102	30	102	37	Gao et al. (2020) (https://iopscience.iop.org/article/10.1088/1748-9326/ab88fa) show that explosively developing ETCs have higher frequency and intensity at higher resolution in HighResMIP simulations and reanalyses. [Malcolm J. Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This reference has been added.
11743	102	44	102	50	lines 44, 49, and 50: "ETCs", no apostrophe (plural, not possessive) [Amy East, United States of America]	Accepted. The text has been revised.
1509	103	9	103	12	The following sentence is difficult: "In addition, there is substantial evidence that the response of extreme precipitation water vapour increases differs between climate models with parameterized and with explicit convection" - what is the message here? Also, the other sentences could be improved and written in a clearer way. [Rasmus Benestad, Norway]	Not applicable. This paragraph has been removed.
24103	103	11	103	11	Why is there no assessment of attribution of mid-latitude storms when there is attribution of the other phenomena so far? [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have added a new subsection 11.7.2.3 that includes some material on the attribution of changes.
51639	103	11	103	11	I'm unclear if there is a reason for why there is no assessment of attribution of mid-latitude storms when there is attribution of the other phenomena so far? As per an earlier comment on Ch 9 it would be extremely useful to include this and explain how this compares to previous attribution statements for TCs made in AR5 and SROCC. If there is new evidence that suggests different conclusions to previous statements this should be made clear here and in the Executive Summary. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have added a new subsection 11.7.2.3 that includes some material on the attribution of changes.
42327	103	14	103	14	The ETC storm attribution section is missing. Few studies are present, but worthwhile to mention. The section can also benefit from references in CH12, where storms are assessed region-by-region [robert vautard, France]	Accepted. We have added a new subsection 11.7.2.3 that includes some material on the attribution of changes.
62497	103	14	103	37	The description on ETC projections was not sufficiently supported with other RCP scenarios. Only the RCP8.5 scenario was considered for projections. It may need a proper justification. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The section might reflect that there is much more literature assessing changes using the RCP8.5 scenario. We have included other scenarios as much as possible.
110007	103	14			Despite linking to the relevant sections this section still proceeds to redundantly repeat many of the assessments made in chapters 4 and 8. Chapter 11 should focus solely on extreme ETCs per its charge and leave a characterisation of all ETCs to chapters 4 and 8. [Peter Thorne, Ireland]	Noted. This subsection is shortened and starting from assessment available from earlier chapters.
110575	103	14			This section does not touch on the fact that ETCs are also associated with severe snow storms in mid-latitudes which are a major hazard/extreme event. The following paper would be a starting point to include in this section. Zarzycki, C. M. (2018). Projecting changes in societally impactful northeastern U.S. snowstorms. Geophysical Research Letters, 45, 12,067–12,075. https://doi.org/10.1029/2018GL079820. [Rachel McCrary, United States of America]	Accepted. This reference has been added, together with a short discussion about the effects of changes in ETCs on snow.
70973	103	24	103	27	In subsequent research, Zappa and Shepherd (2017, already cited) showed that there was a storyline of wintertime atmospheric circulation change over Europe in which the windstorm risk increased notably over northern Europe [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We have left the discussion in general terms without addressing in detail specific regions.
8055	103	24	103	27	When citing numeric values, please specify the time and the scenario. [jouni Räisänen, Finland]	Accepted. Text has been revised accordingly.
1511	103	27	103	27	Parding et al (2019; DOI: 10.1175/JAMC-D-17-0348.1), however, used empircval-statistical downscaling techniques to project the storm track density over the North-Atlantic based on CMIP5 (RCP 4.5 and 8.5 108 and 81 simulations respectively) and found a large spread within the cyclone density projections based on CMIP5 SLP simulations; however, the multimodel ensemble median indicates an increase in the storm-track activity in winter, and a poleward shift during autumn and spring. [Rasmus Benestad, Norway]	Rejected. In this section we focus on results based on the objective identification of individual storms and the methodology used in Parding does not identify individual cyclones.
11263	103	27	103	34	Here, it should be noted that results from Chang (2018) examining projected changes in NH cyclones with strong near surface winds (both at lowest model level as well as model generated 10-m winds) are consistent with the results of Seiler and Zwiers (2016) that intense cyclones (those with strong winds) in the Pacific are projected to shift northeastward with overall decrease in the Pacific, while in the North Atlantic such cyclones decrease significantly. These results are also consistent with the model projected changes in the frequency of strong near surface winds. Reference: Chang, E.K.M., 2018: CMIP5 projected change in Northern Hemisphere winter cyclones with associated extreme winds. J. Climate, 31, 6527-6542, doi: 10.1175/JCLI-D-17-0899.1 Seiler, C., and F. W. Zwiers, 2016: How will climate change affect explosive cyclones in the extratropics of the Northern Hemisphere? Climate Dyn., 46, 3633–3644 [Edmund Kar-Man Chang, United States of America]	Accepted. This reference was added in the text.
131427	103	28	103	28	ETCs instead of ECTs [Hans Poertner and WGII TSU, Germany]	Accepted. The text has been revised.
80611	103	28	103	28	typo: ECT [Malcolm J. Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text has been revised.
11261	103	28	103	28	ECTs should be ETCs [Edmund Kar-Man Chang, United States of America]	Accepted. The text has been revised.

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62495	103	30	103	33	RCP8.5 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The text has been revised.
82811	103	32	103	32	It would be useful to define "explosive" in this context. [Blair Trewin, Australia]	Rejected. The term "explosive" was defined the page before.
42549	103	36	103	36	Typo: 2018)while -> 2018) while [Joan Bech, Spain]	Editorial. The final draft will undergo professional copy-editing prior to publication.
43419	103	36			Read "a single GCM (Seiler et al., 2018) while maximum wind " rather than "a single GCM (Seiler et al., 2018)while maximum wind " [Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. The final draft will undergo professional copy-editing prior to publication.
42551	103	48	103	48	Typo: (2016)small -> (2016) small [Joan Bech, Spain]	Accepted. The text has been revised.
82813	103	48	103	48	There's a word missing here - should it be "project small changes"? [Blair Trewin, Australia]	Accepted. The text has been revised.
13785	103	48	103	48	Change (2016)small by (2016) small [Maria Amparo Martinez Arroyo, Mexico]	Accepted. The text has been revised.
131429	103	48	103	49	verb missing in the sentence [Hans Poertner and WGII TSU, Germany]	Accepted. The text has been revised.
9189	103	48	103	49	Replace "Pepler et al (2016) small" with "Pepler et al (2016) found small". Add "A decrease in Australian east coast low pressure systems is simulated by 18 CMIP5 climate models during the 21st century (Dowdy et al. 2019)". Reference is Dowdy et al (2019): Review of Australian east coast low pressure systems and associated extremes. Climate Dynamics, https://doi.org/10.1007/s00382-019-04836-8. [Kevin Hennessy, Australia]	Accepted. The text has been revised.
117121	103		103		what does "CMIP-class models" mean? What about highresMIP? [Valerie Masson-Delmotte, France]	Accepted. We have revised the text to clarify about the models' ensembles.
70971	104	12	104	15	This statement is no longer true if one considers the accumulated precipitation in cyclone clusters (a key driver for wintertime flooding). We are submitting a manuscript (E Bevacqua, G Zappa and TG Shepherd: "Shorter cyclone clusters modulate changes in European wintertime precipitation extremes") which shows across the CMIP5 models that the accumulated precipitation in wintertime cyclone clusters generally increases across Europe, although by less than the mean precipitation per cyclone, because of a decrease in the overall number of cyclones within clusters. This dynamical modulation of the accumulated precipitation varies substantially between northern and southern Europe. You may contact e.bevacqua@reading.ac.uk for the submitted version of this paper. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Cyclone clusters are however not explicitly consider in this section.
126073	104	12			Unclear what is being said to increase by 3% per degree warming. The intensity of winds in the average ETC? The intensity of the dynamical convergence in the average ETC? The water vapor content in the average ETC? Or what? [Trigg Talley, United States of America]	Accepted. The text has been revised.
65089	104	22	104	23	Gertler and O'Gorman 2019). However, overall frontal intensity may decrease with decreasing temperature gradients (Catto et al. 2014). In addition, convective precipitation (due to latent heating enhancements) may form a larger proportion of overall ETC precipitation (Gertler and O'Gorman 2019). Papers: "Gertler, C.G. and O'Gorman, P.A., 2019. Changing available energy for extratropical cyclones and associated convection in Northern Hemisphere summer. Proceedings of the National Academy of Sciences, 116(10), pp.4105-4110. https://doi.org/10.1073/pnas.1812312116", "Catto, J. L., Nicholls, N., Jakob, C., and Shelton, K. L. (2014), Atmospheric fronts in current and future climates, Geophys. Res. Lett., 41, 7642– 7650, doi:10.1002/2014GL061943." [Laurie Agel, United States of America]	Rejected. We mostly focus on individual extratropical cyclones that can be considered extremes while the suggested paper it is more about the general theory.
1513	104	32	104	34	What is the difference between the atmospheric rivers and mid-latitude cyclones and what are their similarities? And what is the definition of an artmospheric river? [Rasmus Benestad, Norway]	Taken into account. This section now focuses in extratropical cyclones while ARs are assessed in CH. 8.3.2.8.
126075	104	38			[CONFIDENCE] Has an increase in average and maximum ETC rain-rates been detected in observations? In other words, are observed increases outside the range of changes expected from natural variability alone? If not, recommend that either medium or medium-to-high confidence should be used, rather than high confidence. Admittedly, with more water vapor present in a warmer atmosphere, it makes sense that any storm systems that have moisture convergence as a dominant component of their water vapor budget should see enhanced precipitation rates. The factors that could negate this would include weaker dynamical convergence or reduced relative humidities. While unlikely that such factors can over-ride the large scale increase in water vapor from Clausisu-Clapeyron, it would be wise to require the increase in ETC precipitation rates to be observed and detected (i.e., highly unusual observed trends compared to trends expected from natural variability alone) before high confidence is assessed. [Trigg Talley, United States of America]	Rejected. The detection is not used as a necessary condition to use high confidence although it is used as additional evidence in support to statements made for projections. Following the reasoning provided by the reviewer and a large number of studies looking at mean/extreme rainfall rate changes, we have assessed the projected increases with high confidence.
85039	104	41	104	42	"There is medium confidence that changes in the intensity of ETC" should be something like "There is medium confidence that modelled changes in the intensity of ETC" [Turner Jessica, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text has been revised.
9191	104	44	104	44	Need a summary statement about frequency and intensity. Add "Changes in the frequency and intensity of ETCs will be small, with increases or decreases that follow changes in the storm tracks (medium confidence)" based on text on page 103 lines 16-19. [Kevin Hennessy, Australia]	Taken into account. We have largely reformulated the summary paragraph since SOD.
9883	104	47	104	47	There is another category "high-latitude storm" should be included and discussed after section 11.7.2 and before 11.7.3 because of new studies. Groenemeijer et al. (2017) have documented occasional ground observations of such severe weather in Europe and have shown that environmental conditions supportive of severe convective weather occur from time to time in these high latitudes. A new study using the new satellite data from Global Precipitation Measurement (GPM) which features a space-borne Dual-frequency Precipitation Radar providing near-global coverage (65°S to 65°N), established the occurrence of convective storms at high latitude land regions particularly Canada and Asia (Houze et al., 2019). The occurrences of those high-latitude storms collocated with regions with greatest surface warming. References: Houze, R. A., Jr, J. Wang, J. Fan, S. J. Brodzik, Z. Feng (2019). Extreme convective storms over high-latitude continental areas where maximum warming is occurring. Geophysical Research Letters, 46. https://doi.org/10.1029/2019GL082414. Groenemeijer, P., Púčik, T., Holzer, A. M., Antonescu, B., Riemann-Campe, K., Schultz, D. M., Kühne, T., Feuerstein, B., Brooks, H. E., Doswell, C. A., Koppert, H., & Sausen, R. (2017). Severe convective storms in Europe: Ten years of research and education at the European Severe Storms Laboratory. Bulletin of the American Meteorological Society, 98(12), 2641– 2651. https://doi.org/10.1175/BAMS-D-16-0067.1 [Jiwen Fan, United States of America]	Rejected. "High-latitude storms" have not been considered explicitly in this assessment. Hoeze et al. (2019) is referred to as general aspect of observed convective storm in high latitudes (60-65N/S). Groenemeijer et al. (2017) is referred to as a regional aspect of Europe.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
62499	104	47	105	54	There is very limited literature on severe convective storms. More information can be included in the following citations. 1) Ducrocq, V., & co-authors. (2014). HYMEX-SOPI: The field campaign dedicated to heavy precipitation and flash flooding in the northwest Mediterranean. Bulletin of the American Meteorological Society, 95, 1083–1100.; 2) Gatzen, C. G. (2013). Warm-season severe wind events in Germany. Atmospheric Research, 123, 197–205.; 3) Gospodinov, I., Dimitrova, T., Bocheva, L., Simeonov, P., & Dimitrov, R. (2014). Derecholike event in Bulgaria on 20 July 2011. Atmospheric Research, 158–159, 254–273. 4) Hart, J. A., & Cohen, A. E. (2016). The statistical severe convective risk assessment model. Weather and Forecasting, 31, 1697–1714; 5) Kain, J.S., Dembek, S. R., Weiss, S. J., Case, J. L., Levit, J. J., & Sobash, R. A. (2010). Extracting unique information from high-resolution forecast models: Monitoring selected fields and phenomena every time step. Weather and Forecasting, 125, 1536–1542.; 6) Kumjian, M. R. (2013). Principles and applications of dual-polarization weather radar. Part I: Description of the polarimetric radar variables. Journal of Operational Meteorology, 1(19), 226–242.; 7) Kumjian, M. R. (2013). Principles and applications of dual-polarization weather radar. Part II: Warm-and cold-season applications. Journal of Operational Meteorology, 1(20), 243–264.; 8) Schaumann, J. S., & Przybylinski, R. W. (2012). Operational application of 0–3 km bulk shear vectors in assessing QLCS Mesovortex and Tornado Potential. Preprints, 26th Conference on Severe Local Storms, Nashville, TN: American Meteorological Society, 142p. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. All these references are not for trends or related to climate change, but climatological aspects or case studies in specific regions. Regional aspects of storms are more focused in Chapter 12, so this comment is transferred to Chapter 12. In particular, Reference 2) Gatzen et al(2013) is considered as regional aspects of convective systems, and is related to "severe winds" section 11.7.4. 1) Ducrocq, V., & co-authors. (2014). HYMEX-SOPI: The field campaign dedicated to heavy precipitation and flash flooding in the northwest Mediterranean. Bulletin of the American Meteorological Society, 95, 1083–1100.; This is an observational study in general, and not directly linked to climate change. 2) Gatzen, C. G. (2013). Warm-season severe wind events in Germany. Atmospheric Research, 123, 197–205.; German severe winds observation between 1997 and 2011, "In total, 252 convective wind events contributed to 837 (81%) of the wind reports" 3) Gospodinov, I., Dimitrova, T., Bocheva, L., Simeonov, P., & Dimitrov, R. (2014). Derecholike event in Bulgaria on 20 July 2011. Atmospheric Research, 158–159, 254–273: This is a case study. 4) Hart, J. A., & Cohen, A. E. (2016). The statistical severe convective risk assessment model. Weather and Forecasting, 31, 1697–1714; lightning data set in CONUS between 2006 and 2014. 5) Kain, J.S., Dembek, S. R., Weiss, S. J., Case, J. L., Levit, J. J., & Sobash, R. A. (2010). Extracting unique information from high-resolution forecast models: Monitoring selected fields and phenomena every time step. Weather and Forecasting, 125, 1536–1542.; Model diagnosis methodology 6) Kumjian, M. R. (2013). Principles and applications of dual-polarization weather radar. Part I: Description of the polarimetric radar variables. Journal of Operational Meteorology, 1(19),
13841	104	49	104	50	It's recommended to mention that the definition of severe convective storms such as heavy precipitation, it's depends on the maximum precipitation thresholds of each region. [Maria Amparo Martinez Arroyo, Mexico]	Noted. Extreme precipitation is focused in section 11.4. No clear definitions are possible to severe convective storms.
69545	105	11	106	2	This section is missing recent work on the mechanisms and drivers of changes in environmental conditions associated with severe convection. In particular, two theories for increased CAPE with warming have been constructed: Singh, M. S. & O'Gorman, P. A. Influence of entrainment on the thermal stratification in simulations of radiative-convective equilibrium Geophys. Res. Lett., Wiley Online Library, 2013, 40, 4398-4403 Agard, V. & Emanuel, K. Clausius-Clapeyron Scaling of Peak CAPE in Continental Convective Storm Environments J. Atmos. Sci., 2017, 74, 3043-3054 And a number of follow ups have been published: Seeley, J. T. & Romps, D. M. Why does tropical convective available potential energy (CAPE) increase with warming? Geophys. Res. Lett., Wiley Online Library, 2015, 42, 10429-10437 Romps, D. M. Clausius-Clapeyron Scaling of CAPE from Analytical Solutions to RCE J. Atmos. Sci., 2016, 73, 3719-3737 Furthermore, observational evidence for such mechanisms being present in the atmosphere was presented in Singh, M. S.; Kuang, Z.; Maloney, E. D.; Hannah, W. M. & Wolding, B. O. Increasing potential for intense tropical and subtropical thunderstorms under global warming Proc. Nat. Acad. Sci. USA., 2017, 114, 11657 - 11662 [Martin Singh, Australia]	Accepted. The following references are added. Singh et al. (2017) for future projection of environmental change for convective storms. Singn and O'Gorman (2013) mechanism of CAPE change, followed by Agard and Emanuel (2017), Seeley and Romps (2015), and Romps (2016).
115005	105	13	105	14	"Severe convective storms are sometimes embedded in synoptic-scale weather systems such as tropical and 14 extratropical cyclones ..." convective storms - they are always a component of cyclones. [Elena Maksimovich, France]	Rejected. Convective storms are not always a component of cyclones, but can be isolated storms.
115007	105	13	105	33	The only meaningfull text is in the last 3 sentences. Everything else is NOT new, not worth IPCC report [Elena Maksimovich, France]	Rejected. There is a need to clarify the scope of this section by describing and defining severe convective storms. Literatures on changes on severe convective storms are limited, so this paragraph is intended to help enhancement of more researches on severe convective storms. The later part of the section is related to the recent active researches on structure and cloud microphysics of severe convective storms, and is important for this report.
11745	105	24	105	24	add "the" after "Many of", and also change "line-shaped" to "linear" [Amy East, United States of America]	Taken into account. "line-shaped" is changed to "band-shaped".
11747	105	26	105	26	delete "the" before "Eastern Asia" [Amy East, United States of America]	Accepted.
87399	105	28	105	30	Cloud microphysics characteristics of MCSs are examined and roles of warm rain processes are also stressed recently (Hamada et al. 2015, Sohn et al 2013)' is suggested to be revised as 'Cloud microphysics characteristics and three dimensional space-borne radar measurements of extreme rainfalls in East Asia are examined and roles of warm rain processes are emphasized recently (Hamada et al. 2015, Sohn et al 2013, Hamada and Takayabu, 2018)' Added citation: Hamada, A, and Y. N. Takayabu, 2018: Large-scale environmental conditions related to midsummer extreme rainfall events around Japan in the TRMM region, J. Climate, 31, 6933-6945. https://doi.org/10.1175/JCLI-D-17-0632.1 [Yukari Takayabu, Japan]	Taken into account. Hamada and Takayabu (2018) is added.
9901	105	30	105	30	The singular verb suggests does not appear to agree with the plural subject studies. Consider changing the verb form for subject-verb agreement. [ayman badawy, Egypt]	Accepted.

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39867	105	32	105	32	"these types" -> Unclear which types of MCSs you're referring to [TSU WGI, France]	Considered. The sentence is reformulated.
126077	105	40			"convective" not "convection" [Trigg Talley, United States of America]	Accepted.
42553	105	42	105	42	The references quoted (Tochimoto and Niino 2018 & Elsner et al 2019) refer to Japan and USA - to provide a broader view I suggest to complement them with a recent study performed over a mediterranean region: Rodríguez, O., & Bech, J. (2018). Sounding-derived parameters associated with tornadic storms in Catalonia. International Journal of Climatology, 38(5), 2400-2414 [Joan Bech, Spain]	Taken into account. We added an article on European tornadoes (Antonescu et al., 2016). Rodríguez, O., & Bech, J. (2018) is an example of a southern Europe, not a comprehensive review, not appropriate for here,
40017	105	45	105	45	Check use of "likely" is consistent with IPCC uncertainty language, i.e., does it have quantitative meaning? [TSU WGI, France]	Considered. Text is revised and "likely" not used.
87395	105	50	105	50	In early June of the Eastern Asia' should be changed to 'In early June to late July of the Eastern Asia' [Yukari Takayabu, Japan]	Accepted.
109115	105	50	105	51	Change "severe precipitations are frequently caused with MCSs" to "severe precipitation events are frequently caused by MCSs" [Seth McGinnis, United States of America]	Accepted.
9193	105	50	105	51	replace "of the Eastern Asia" with "in eastern Asia". Replace "precipitations" with "precipitation events" [Kevin Hennessy, Australia]	Accepted. We use "East Asia".
109117	105	51	105	51	Change "severe precipitations" to "severe precipitation events" [Seth McGinnis, United States of America]	Accepted.
87397	105	51	105	51	caused with MCSs.' should be replaced with 'caused with MCSs or mesoscale frontal systems, which are not necessarily associated with large atmospheric instability buth with very moist atmospheric conditions (Yokoyama et al. 2020, Tsuji et al. 2020) Citation: Yokoyama, C., H. Tsuji, and Y. N. Takayabu (2020) The effects of an upper-tropospheric trough on the Heavy Rainfall Event in July 2018 over Japan, 98, Special Edition on Extreme Rainfall Events in 2017 and 2018, 235-255 https://doi.org/10.2151/jmsj.2020-013, Tsuji, H., C. Yokoyama, and Y. N. Takayabu (2020) Contrasting features of the July 2018 heavy rainfall event and the 2017 Northern Kyushu rainfall event in Japan. J. Meteor. Soc. Japan, 98, Special Edition on Extreme Rainfall Events in 2017 and 2018, https://doi.org/10.2151/jmsj.2020-045 [Yukari Takayabu, Japan]	Rejected. Here in this section, roles of MCSs are highlighted, and not necessary to mention other systems. These references are case studies, not appropriate to be cited here.
88181	105	54	105	54	Permafrost is a sub-surface phenomenon so it is more correct to refer to areas "underlain by permafrost" rather than covered by permafrost. [Sharon Smith, Canada]	Rejected. The underlying text does not discuss permafrost.
11749	105	54	105	54	delete the “al” at the end of “topographic”. [Amy East, United States of America]	Accepted.
11751	105	54	105	54	Another important reference here is Oakley et al., 2018, which showed the importance of mesoscale meteorological features such as narrow cold-frontal rain bands in causing extreme and deadly rainfall. Their example storm caused post-fire debris flows by raining intensely on recently burned terrain, but even without a recent fire these mesoscale systems can bring extreme rainfall in western North America and other regions: Oakley, N. S., Cannon, F., Munroe, R., Lancaster, J. T., Gomberg, D., & Ralph, F. M. (2018). Brief communication: meteorological and climatological conditions associated with the 9 January 2018 post-fire debris flows in Montecito and Carpinteria, California, USA. Natural Hazards and Earth Systems Science, 18, 3037–3043 [Amy East, United States of America]	Rejected. This reference is a case study, and not directly related the assessment of this section.
42555	106	2	106	2	I suggest adding here a recent study covering (absolute) extreme precipitation events for different time-scales over Spain which indicated the dominant topographic effects on extreme rainfall events: Gonzalez, S., & Bech, J. (2017). Extreme point rainfall temporal scaling: a long term (1805–2014) regional and seasonal analysis in Spain. International Journal of Climatology, 37(15), 5068-5079. [Joan Bech, Spain]	Rejected. This is a reference on rainfall, and not specifically on severe convective storms.
110009	106	5			This section need considerable work for clarity. The evidence also needs to be better synthesised. I expected to see something on global lightning trends which are now an ECV according to GCOS and indicative of global MCS activity. There is relevant literature to assess and I can find a CA from the GCOS task team on lightning observations if useful. There is a recent GCOS report that may be helpful on the matter that can be found via the GCOS website. [Peter Thorne, Ireland]	Taken into account. The reference is not a peer review article, and is not suitable for Section 11.7.
126079	106	7	106	51	Ridler et al. (2018) show increasing trends of lightning, hail, and convective winds in all of Europe except the Iberian Peninsula and Europe, and Taszarek et al. (2018) showed increased thunderstorms and severe thunderstorms in Europe over recent decades. [Trigg Talley, United States of America]	Taken into account. We refer to Taszarek et al. (2019,https://doi.org/10.1175/JCLI-D-18-0372.1) which discuss the trends of thunderstorms in Europe. Ridler et al. is not available.
9195	106	7	106	51	a few typos need to be corrected [Kevin Hennessy, Australia]	Noted. Typos are corrected,
80707	106	15	106	16	The term 'in marine time continent' is unclear. Do you mean 'in maritime continent'? [Helene Jacot Des Combes, Marshall Islands]	Accepted.
9885	106	23	106	24	Add a sentence to reflect the most recent progress on this, i.e., “…increase of convective storms, and hails and severe thunderstorms (Kossin et al., 2017; Kunkel et al., 2013). A significant interannual variability of hailstone occurrences is found in the U.S. Southern Great Plains, and aerosols, sea surface temperature anomalies over the northern Gulf of Mexico, and El Niño–Southern Oscillation (ENSO) are the three factors identified to likely influence the interannual variability (Jeong et al., 2020).” Reference, Jeong, J.-H., J. Fan, C.R. Homeyer, and Z. Hou. 2020. “Understanding hailstone temporal variability and contributing factors over the United States Southern Great Plains,” Journal of Climate, 33: 3947-3966. DOI: 10.1175/JCLI-D-19-0606.1. [Jiwen Fan, United States of America]	Taken into account. A significant interannual variability of hailstone occurrences in the Southern Great Plains of US is mentioned.
9903	106	24	106	24	(It is likely that tornado activity has)This sentence may be considered wordy. Consider changing the wording. (Tornado activity has likely) [ayman badawy, Egypt]	Considered. The sentence is reformulated.
126081	106	24	106	26	"It is likely that tornado activity has increased in the United States particularly over the 2000s, with a decrease in the number of days per year where tornadoes are observed but an increase in the number of tornadoes on days when they occur (Elsner et al., 2015, 2019; Kossin et al., 2017; Allen, 2018)." [Trigg Talley, United States of America]	Noted. Not sure what is the comment.
40019	106	24	106	51	Check use of "likely" is consistent with IPCC uncertainty language, i.e., does it have quantitative meaning? [TSU WGI, France]	Accepted.
13787	106	29	106	29	Change .Feng by . Feng [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Already corrected.
109119	106	32	106	32	Change "out" to "outside" [Seth McGinnis, United States of America]	Accepted.
109851	106	33	106	34	"Westra et al. (2014) found that there is an increase in the intensity of short-duration convective events (minutes to hours) over the whole world." This statement does not seem to reflect what is said in the paper - there are definitely more increases than decreases in short duration precipitation intensities but not across the whole world... [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Added "except eastern China".
13789	106	37	106	37	Change event(Blanchet by event (Blanchet [Maria Amparo Martinez Arroyo, Mexico]	Accepted.
109121	106	48	106	48	"Over the 2000s" is ambiguous; it could be read as meaning since the turn of the century or referring only the first decade. Modify accordingly. [Seth McGinnis, United States of America]	Noted. The text in the summary was modified.
41179	106	48	106	48	What is tornado "activity" specifically refer to? - unclear [TSU WGI, France]	Noted. The text in the summary was modified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
126083	106	48	106	49	There's no support for the statement that tornado activity has increased in the United States. Brooks et al. (2014) showed that the number of (E)F1+ tornadoes has no trend. They showed that variability of tornado occurrence has increased as reflected in the fewer number of days per year with tornadoes, but an increase in the number of days with many tornadoes, as well as increased variability in the timing of the early part of the tornado season. It is also slightly wrong to say that there's been an increase in the number of tornadoes on days when they occur. There has been a shift in the distribution of tornadoes/day as seen in Brooks et al. (2014) and Elsner et al. (2015), but the statement seems to refer to an increase in the mean, which, while true, is somewhat misleading in that it's been a change in distribution, rather than a shift overall. Gensini and Brooks (2018) showed increases in tornado occurrence in the mid-South of the US and decreases over the High Plains in the reporting database from 1979 to 2017. More importantly, they showed these changes were reflected in environmental parameters that are associated with tornadoes. Citation: Brooks, H., G. W. Carbin, and P. T. Marsh, 2014: Increased variability of tornado occurrence in the United States. Science, 346, 349-352, https://doi.org/10.1126/science.1257460. [Trigg Talley, United States of America]	Accepted.
13791	106	48	106	51	Indicate the text in bold letters [Maria Amparo Martinez Arroyo, Mexico]	Noted. All the text in the summary was changed to the normal font.
62501	107	1	108	7	As the processes associated with severe convective storms occur over a wide range of spatial and temporal scales, some of which are poorly understood and/or are inadequately sampled by observational networks, the model calibration and validation aspects are going to be difficult and leading to poorly validated approaches. Therefore, model simulations and their interpretations should be done with much caution. This aspect must be emphasised in this section. In this context, I want to focus on the following sentence. "It is medium confidence that the frequency of severe convective storms increases in the spring, accompanied by a less significant increase in the summer months (Diffenbaugh et al., 2013". I would suggest that such a conclusion or attribution is not rationally or adequately supported, and it should be with low confidence unless more and more through investigations are not synthesised. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The limitation of the future assessment based on the model parameters is added.
20757	107	3	107	18	Is not it true that the occurrence of "compound events" is enhanced by the fact that, although some events are considered separately, they happen to be fed by the same or neighbouring drivers? For example, heavy rainfall is often fed by an extratropical cyclone which also contributes to a storm surge. Similarly, a period with high surface temperature may culminate in a heat wave, while at the same time the heat generates a strong evaporative demand and a drought inevitably follows. The occurrence of wildfires in such conditions is a possible logical consequence rather than a coincidence. In other words, the "compound events" topic appears because events have been broken down according to specific physical parameters; this may of course offer advantages but should not hide frequent cases where events are components of a common phenomenon. [philippe waldteufel, France]	Noted. This comment is not relevant here as this paragraph is not on "compound events". This comment is on a wrong section.
84917	107	3	107	18	This section seems to focus on models that can explicitly resolves severe convective storms but there are a number of studies that look at proxies such as CAPE and wind shear. Would it be of value to include a discussion of how models perform with regards to proxies. [Turner Jessica, United Kingdom (of Great Britain and Northern Ireland)]	Considered. A paragraph on other type of models is added.
13793	107	13	107	13	Change .Cloud by . Cloud [Maria Amparo Martinez Arroyo, Mexico]	Accepted.
1637	107	16	107	16	Similar results have been obtained for Mediterranean heavy rain events (Buzzi et al., 2014). REFERENCE: Buzzi, A., Davolio, S., Malguzzi, P., Drofa, O., and Mastrangelo, D.: Heavy rainfall episodes over Liguria in autumn 2011: numerical forecasting experiments, Nat. Hazards Earth Syst. Sci., 14, 1325–1340, https://doi.org/10.5194/nhess-14-1325-2014, 2014 [Mario Marcello Miglietta, Italy]	Noted. This reference is for a case study and is not appropriate to refer to here.
13795	107	16	107	16	Change .MCSs by . MCSs [Maria Amparo Martinez Arroyo, Mexico]	Accepted.
70169	107	21	107	29	Compared to detection and attribution in previous sections, the discussions here on severe storms are very short. There should be many studies out there, which worth reviewing to extend the content. [Huanping Huang, United States of America]	Noted. We are not aware of the many studies the reviewer was thinking and this reviewer did not give references.
110011	107	21			This section feels like it significantly undercatches the work in this area and is a review not an assessment presently. It needs to be made longer and be more of an assessment. [Peter Thorne, Ireland]	Noted. This reviewer did not give references.
23983	107	26			Change "convection events" to "convective events" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
41155	107	27	107	29	What did these event attribution studies find? [TSU WGI, France]	Taken into account. We added the conclusion of the case studies.
42329	107	32	107	32	Since the projection section currently reads as a series of regional statements, CH12 could take on board material from CH11 on region-by-region convective storms, to avoid an overlap [robert vautard, France]	Comment noted. No action taken

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
69547	107	32	108	25	This section ignores the tropics and subtropics, and is very North America focused. While the tropics are not known for tornadoes, most of the worlds lightning occurs in the tropics, and there are plenty of impacts including winds, extreme precipitation and (in the subtropics) hail. There are a number of papers that find large increases in CAPE in the tropics in projections (and theories for this increase as noted above). E.g.,: Singh, M. S.; Kuang, Z.; Maloney, E. D.; Hannah, W. M. & Wolding, B. O. Increasing potential for intense tropical and subtropical thunderstorms under global warming Proc. Nat. Acad. Sci. USA., 2017, 114, 11657 - 11662. Sobel, A. H. & Camargo, S. J. Projected future seasonal changes in tropical summer climate J. Climate, 2011, 24, 473-487. Whether or not such increases in CAPE will translate to stronger updrafts/more severe convection is less clear. Idealised CRM simulations present some evidence for an intensification of convection with warming, although these studies also find that despite the increase in updraft velocity, there is no dynamical amplification of convective precipitation extremess Romps, D. M. Response of tropical precipitation to global warming J. Atmos. Sci., 2011, 68, 123-138 Muller, C. J.; O'Gorman, P. A. & Back, L. E. Intensification of precipitation extremes with warming in a cloud-resolving model J. Climate, 2011, 24, 2784-2800 Singh, M. S. & O'Gorman, P. A. Increases in moist-convective updraught velocities with warming in radiative-convective equilibrium Q. J. R. Meteorol. Soc., 2015, 141, 2828-2838 Singh, M. S. & O'Gorman, P. A. Influence of microphysics on the scaling of precipitation extremes with temperature Geophys. Res. Lett., 2014, 41, 6037-6044 There have also been some evaluations of sever convective environments outside the USA that are not cited here: Allen, J. T.; Karoly, D. J. & Walsh, K. J. Future Australian severe thunderstorm environments. Part II: The influence of a strongly warming climate on convective environments J. Climate, 2014, 27, 3848-3868 [Martin Singh, Australia]	considered. This section has expanded to include other regions, in particular to assess relevant papers listed here.
18381	107	32	108	25	Please note that Chen et al. (2020a) extended the CONUS analysis of Rasmussen et al. (2017) to the globe and found that while CAPE increases almost everywhere (except some subtropical oceans in the eastern Pacific and Atlantic), CIN also strengthens over most land areas (but changes little over ocean), and these mean CAPE and CIN changes result from a shift in the frequency of weak CAPE and/or weak CIN cases toward stronger CAPE and/or CIN cases. Such CAPE and CIN changes lead to large increases in severe storms producing heavy precipitaiton but decreases in light preciptiation events (Chen et al. 2020b). In addition, Dai et al. (2017) found that rainstorm size would increase while rainstorm number would decrease under RCP8.5 in the 21st century over the CONUS based on 4k WRF simualtions. These findings are highly relevant to this section on these two pages. Papers cited: Chen, J., A. Dai, Y. Zhang, and K. L. Rasmussen, 2020a: Changes in the convective potential available energy and convective inhibition under global warming. J. Climate, 33, 2025–2050, https://doi.org/10.1175/JCLI-D-19-0461.1 Chen, J., A. Dai, and Y. Zhang, 2020b: Linkage between projected precipitation and atmospheric thermodynamic changes. J. Climate, revised on Feb. 27, 2020 (manuscript available from adai@albany.edu). Dai, A., R. M. Rasmussen, C. Liu, K. Ikeda, and A. F. Prein, 2017: A new mechanism for warm-season precipitation response to global warming based on convection-permitting simulations. Climate Dyn., https://doi.org/10.1007/s00382-017-3787-6. Rasmussen, K. L., A. F. Prein, R. M. Rasmussen, K. Ikeda, and C. Liu, 2017: Changes in the convective population and thermodynamic environments in convection-permitting regional climate simulations over the United States. Climate Dyn., https://doi.org/10.1007/s00382-017-4000-7. [Aiguo Dai, United States of America]	Taken into account. Checked: Chen's 2nd paper is available: Chen, Jiao, Dai, Aiguo, Zhang, Yaocun. Linkage Between Projected Precipitation and Atmospheric Thermodynamic Changes. Journal of Climate. 2020. https://doi.org/10.1175/JCLI-D-19-0785.1.
110013	107	32			This section reads more like a literature review than an assessment. Significant efforts to better synthesise and draw out key findings would be very beneficial. [Peter Thorne, Ireland]	Noted.
126085	107	34	108	7	The most significant result of Gensini and Mote (2015) is the increase in variability of springtime severe thunderstorm occurrence in the late 21st century simulations. While Diffenbaugh et al. (2013) show an increase in the mean in summer severe convective storms, they also show a large increase in variability, with approximately a quarter of their model simulations showing a decrease. The result that variability is likely to increase in the future is as strong as that the mean will increase. It also relates to the Brooks et al. (2014) result showing variability increases in the tornado record. [Trigg Talley, United States of America]	Taken into account. Diffenbaugh et al. (2013) does not clearly states an increase in the mean in summer severe convective storms.
2811	107	36	107	36	Sensitivity experiments suggest a stronger intensity of tornado-spawning supercells generated over the sea in case of higher SST (e.g., Miglietta et al., 2017). Reference: Miglietta M. M., Mazon J., Motola V., Pasini A., Effect of a positive Sea Surface Temperature anomaly on a Mediterranean tornadic supercell, Scientific Reports, 7, 12828, 1-8, 2017, DOI:10.1038/s41598-017-13170-0; [Mario Marcello Miglietta, Italy]	Noted. We need to describe more relevant environmental variables, rather than SST.
9905	107	44	107	44	Your article usage with the geographic name Africa may be incorrect. [ayman badawy, Egypt]	Accepted. Corrected.
126087	107	53	108	2	Brooks (2013) is an inappropriate reference for this statement. That paper actually says that there's little support for the idea tornadoes will increase and they may actually decrease, and the strongest inference is that non-tornadic convective winds will increase. [Trigg Talley, United States of America]	Considered. The text is reformulated to reflect the original meaning of Brooks (2013).
66403	107		107		These two submitted papers based on CP model ensembles that are now under revision could be revised and added in section 11.7.3.5 Ban et al, The first multi-model ensemble of regional climate simulations at kilometer-scale resolution Part I: Evaluation of precipitation, Climate Dynamic, submitted; Pichelli et al, The first multi-model ensemble of regional climate simulations at kilometer-scale resolution part 2: future precipitation projections, Climate Dynamic, submitted [Erika Coppola, Italy]	Rejected. These two papers were not available, and it seems that these are published after the literature cut off date.
11753	108	2	108	2	awkward phrasing, “It is medium confidence” [Amy East, United States of America]	Noted. The corresponding sentence is rewritten.
9197	108	2	108	4	the 3 studies cited only apply to the USA: 1 paper says there's an increase in spring and autumn, the 2nd paper says spring, and the 3rd paper says spring and summer. Therefore, I suggest revising the statement to "the frequency of severe convective storms increases in spring (medium confidence), summer and autumn (low confidence) in the USA" [Kevin Hennessy, Australia]	Considered. This is specifically meant to apply to the USA in FGD.
70837	108	6	108	6	"2017)and" should be "2017) and" [Adrean Webb, Japan]	Accepted. Corrected.
13797	108	6	108	6	Change (Púčik et al. 2017)and by (Púčik et al. 2017) and [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Corrected.
110015	108	10	108	25	This summary is overly long and contains introductory material either already in or that should be instead in the preceding assessment text instead. The summary should be much shorter and highlight solely the key assessment findings reached. [Peter Thorne, Ireland]	Considered. Summary is shortened and to the point.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
69549	108	12	108	25	I think this summary does not give a balanced view of the evidence with regards to future increases in severe convective environments. Increases in CAPE are mentioned as a potential reason for extended severe convection seasons (implicitly in the midlatitudes). In actual fact, there is robust evidence from GCMs that CAPE increases by substantial amounts in simulations of global warming, in various rmidlatitude regions (USA, Australia and others -- see the citations in my comments above) and across the tropics and subtropics as a whole (where the majority of the most intense storms occur). These increases are backed up by theory (see my comments above), and there has been some work to find evidence for the relevant mechanisms in observations. Where there is considerable uncertainty is in whether these increases in convective environments will result in actual increases in thunderstorm intensity or the fequency of severe thunderstorms, and the implications of these changes for e.g., convective precipitation extremes. [Martin Singh, Australia]	Considered. The revsied summary is shortened but it is now also more balanced to include different regions.
82815	108	22	108	24	This result is specific to the United States, not global as implied by the text. [Blair Trewin, Australia]	Considered. Assessment that is specific to the USA is now made clear in the summary.
9199	108	22	108	24	As indicated above, revise this statement to "the frequency of severe convective storms increases in spring (medium confidence), summer and autumn (low confidence) in the USA" [Kevin Hennessy, Australia]	Considered. This is specifically meant to apply to the USA in FGD.
126089	108	24	108	25	[CONFIDENCE] Has an increase in average and maximum ETC rain-rates been detected in observations? In other words, are observed increases outside the range of changes expected from natural variability alone? If not, recommend that either medium or medium-to-high confidence should be used, rather than high confidence. Admittedly, with more water vapor present in a warmer atmosphere, it makes sense that any storm systems that have moisture convergence as a dominant component of their water vapor budget should see enhanced precipitation rates. The factors that could negate this would include weaker dynamical convergence or reduced relative humidities. While unlikely that such factors can over-ride the large scale increase in water vapor from Clausisu-Clapeyron, it would be wise to require the increase in ETC precipitation rates to be observed and detected (i.e., highly unusual observed trends compared to trends expected from natural variability alone) before high confidence is assessed. [Trigg Talley, United States of America]	Noted. This comment is regarded for "severe convective storms", not for "ETC". The text in the summary paragraph was modified.
9201	108	25	108	25	The absence of a definitive statement about projected changes in hail, lightning, tornadoes and windstorms for a broad range of regions around the world highlights an important knowledge gap, especially for the disaster risk management and insurance sectors. Consider adding a statement such as "There is significant uncertainty about projected regional changes in hail, lightning, tornadoes and wind-storms due to limited analysis of initial simulations using convective-permitting or storm-resolving models (high confidence)". [Kevin Hennessy, Australia]	Accepted. A sentence is added.
42331	108	28	109	40	This section does not bring much compared to previous, and could be removed in case space is needed. It mixes mean wind changes and extremes. The section is overlapping with CH12 storms CID. Suggestion that CH12 takes material from this section. It is more in the remits of CH12. [robert vautard, France]	Considered, We focus on extreme aspects of winds, and removed the part on mean wind changes. CH12 more focuses on regional aspects, so although there is some overlaps we keep the minimum material here in CH11.
126091	108	30	109	40	Extreme winds should include calm winds, in addition to the extreme high-speed wind discussed in the section. Abhishek et al (2010) published a statistical analysis of NAAQS station data in cities of east and midwest U.S., in which the pertinent trend of increasing calm winds particularly in night time is evident. [Trigg Talley, United States of America]	Rejected. Calm winds are not extreme, and they are not relevant to this section.
126093	108	47	108	48	Where in SROCC was this statement made? Appears to be a misquote and a misleading misquote. Either delete the statement or provide location exactly where that is stated in SROCC so it can be evaluated. Page 67 of SROCC mentions something along these lines, but always with the crucial caveat "low confidence", which is not included here in the AR6 draft, but is an essential part of the SROCC assessment. [Trigg Talley, United States of America]	Taken into account. Omit the reference to SROCC.
82817	108	47	108	50	This would be better merged with the tropical cyclone section. [Blair Trewin, Australia]	Noted. This part is on the aspect of extreme winds of SROCC, and it is reasonable to cite here.
110017	108	52	109	30	This is grossly redundant with the assessment of surface winds carried out in chapter 2. Chapter 11 should solely concern itself with an assessment of extreme winds per its charge and leave mean windspeed changes to chapter 2 per its charge. This text should be replaced with a simple cross-reference to the substantive assessment inchapter 2 and its assessment. Chapter 11 should then proceed to assess changes in extreme winds. If this leaves the section as a whole subcritial then it should be removed. [Peter Thorne, Ireland]	Taken into account. Avoid overlapping with Chapter 2, 2.3.1.4.4, and focus on extreme winds.
126095	109	1	109	3	The conclusions on terrestrial measurements appears to be based only on the Azorin-Molina et al. (2017) paper for Spain and Italy stations, which is very limited coverage. The conclusion should be strengthened by citing studies covering broader geographical regions. [Trigg Talley, United States of America]	Noted No other references are suggested.
43421	109	1		2	Read " (e.g., anemometer heights (Troccoli et al., 2012))" rather than " (e.g., anemometer heights (Troccoli et al., 2012)" [Cyriaque Rufin Nguimalet, Central African Republic]	Noted. The related sentence was removed.
43423	109	2		3	Read " (e.g., the average of four daily measurements or 24-hour wind runs (Azorin-Molina et al., 2017))" rather than " (e.g., the average of four daily measurements or 24-hour wind runs (Azorin-Molina et al., 2017)" [Cyriaque Rufin Nguimalet, Central African Republic]	Noted. The related sentence was removed.
43425	109	5			Read "(e.g., Troccoli et al. 2012)" rather than "(e.g., Troccoli et al. 2012))" [Cyriaque Rufin Nguimalet, Central African Republic]	Noted. The related sentence was removed.
24461	109	9	109	19	Projection of extreme sea surface winds has been analyzed based on large ensemble projections. Mori, N., T. Shimura, K. Yoshida, R. Mizuta, Y. Okada, M. Fujita, T. Temur Khujanazarov, E. Nakakita (2019) Future changes in extreme storm surges based on mega-ensemble projection using 60-km resolution atmospheric global circulation model, Coastal Engineering Journal, Taylor & Francis, 61:3, pp.295-307. doi:10.1080/21664250.2019.1586290 [Nobuhito Mori, Japan]	Taken into account. The reference is added as projection of surface wind speed changes.
13799	109	13	109	14	Change [Vautard et al., 2010] by (Vautard et al., 2010) [Maria Amparo Martinez Arroyo, Mexico]	Accepted.
43427	109	13			Read " (Vautard et 13 al., 2010)" rather than " [Vautard et 13 al., 2010]" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted.
13801	109	16	109	16	Change [Marin et al., 2014] by (Marin et al., 2014) [Maria Amparo Martinez Arroyo, Mexico]	Accepted.
43429	109	16			Read " (Vautard et 13 al., 2010)" rather than " [Vautard et 13 al., 2010]" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted.
13803	109	17	109	17	Change [Lin et al., 2012] by (Lin et al., 2012) [Maria Amparo Martinez Arroyo, Mexico]	Accepted.
43431	109	17			Read " Lin et al. (2012) note" rather than " [Lin et al., 2012] note" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted.
43433	109	18			Read " Hande et al. (2012) using radiosonde " rather than " (Hande et al., 2012) using radiosonde " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted.
43435	109	29			Read "and Zheng et al. (2017) found the positive wind " rather than "and (Zheng et al., 2017) found the positive wind " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted.
110019	109	32	109	40	Given that medicanes are low pressure systems they really should be covered under one of the tropical cuyclones, extra-tropical cyclones or MCS rather than shoe-horned in in this manner. Calling them out in this way doesn't really fit so I would move them into one or more of the prior sections. [Peter Thorne, Ireland]	Taken into account. Medicane are referred to here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1635	109	34	109	34	An official classification of Medicanes is still missing; hence, in order to fulfill this gap, a classification has been proposed based on the prevailing mechanism of development in their mature stage (Miglietta and Rotunno, 2019).Reference: Miglietta M.M., Rotunno R., Development mechanism of Mediterranean tropical-like cyclones (Medicanes), Q. J. Roy. Meteor. Soc., 145, 1444-1460, 2019; doi:10.1002/qj.3503 [Mario Marcello Miglietta, Italy]	Noted. The suggested reference is added, but IPCC report does not need such official classifications.
126097	109	34			The following paper is relevant for medicanes' impact Zhang, W., G. Villarini, E. Scoccimarro, and F. Napolitano, Examining the precipitation associated with medicanes in the high-resolution ERA-5 reanalysis data, submitted to International Journal of Climatology, 2019. [Trigg Talley, United States of America]	Taken into account. The reference is published as 1) Zhang, Wei, Villarini, Gabriele, Scoccimarro, Enrico, Napolitano, Francesco. Examining the precipitation associated with medicanes in the high-resolution ERA-5 reanalysis data. International Journal of Climatology. 2020, vol. n/a, no. n/a. https://doi.org/10.1002/joc.6669.
40021	109	39	109	39	Check use of "likely" is consistent with IPCC uncertainty language, i.e., does it have quantitative meaning? [TSU WGI, France]	Noted. The word "likely" is intended.
9219	109	40	109	40	By 2090 for RPC8.5, mean and extreme windspeeds (highest daily-average windspeed with 1-year or 20-year return periods) are projected to decrease by less than 10% in winter over southern mainland Australia, and decrease by less than 10% in autumn and spring over south-east mainland Australia (CSIRO and BoM, 2015). By 2030, windspeed changes over Australia are projected to be small compared to natural variability (CSIRO and BoM, 2015). [Kevin Hennessy, Australia]	Noted. The reference is not clearly indicated.
1515	109	41	109	41	No summary for extreme winds? [Rasmus Benestad, Norway]	Taken into account. The summary paragraph is added.
40559	109	43	109	43	No mention of the term 'compound events' in the body of the SPM (it's only mentioned in tables). There is expanded coverage of this topic in the report compared to AR5, so should it be highlighted more in the SPM. [TSU WGI, France]	Accepted. The revised version of the SPM now mentions compound events explicitly.
100507	109	43	109	43	Three relevant review papers on compound events have been published/accepted recently. The first study (Aghakouckak et al., 2020) highlights how anthropogenic warming increases the risk of compound and cascading hazards. The second study (Zscheischler et al., accepted) present a new typology of compound events including four categories (preconditioning, multivariate drivers/hazards, temporally compoundig, spatially compounding) and their associated methodological approaches. The third study (Raymond et al., accepted) introduces the concept of connected events to denote the exacerbating role that humans can play with regard to the severity extreme events. REFs: AghaKouchak, A., Chiang, F., Huning, L. S., Love, C. A., Mallakpour, I., Mazdiyasni, O., ... & Sadegh, M. (2020). Climate Extremes and Compound Hazards in a Warming World. Annual Review of Earth and Planetary Sciences, 48.; Zscheischler, J., Martius, O., Westra, S., Bevacqua, E., Raymond, C., Horton, R., van den Hurk, B., AghaKouchak, A., Jézéquel, A., Mahecha, M.D., Maraun, D., Ramos, A.M., Ridder, N., Thiery, W., Vignotto, E., A typology of compound weather and climate events, Nature Reviews Earth and Environment, accepted.; Raymond, C., Horton, R.M., Zscheischler, J., Martius, O., AghaKouchak, A., Balch, J., Bowen, S.G., Camargo, S.J., Hess, J., Kornhuber, K., Oppenheimer, M., Ruane, A.C., Wahl, T., White, K., Understanding and Managing Connected Extreme Events, Nature climate change, accepted. [Wim Thiery, Belgium]	Accepted. The references have been integrated into the introduction paragraphs.
112839	109	43	110	25	Note that compound events are here restricted to compound extremes in the climate system (e.g. heat and drought). Especially in the context of covid-19 (but also more generally) there is also increasing attention for compound events referring to impacts (e.g. the compound impact of covid-19, the economic impact of the covid-19 response measures, plus floods and locusts in East Africa). Might be good to also mention an example of such compound disasters that include compound drivers of risk beyond the climate aspects, and then make it explicit that this section is focused on the the combination of two or more climate or weather events. You could also refer to WGII (especially ch16) which will say a few words about this, also building on this section 11.8 in WGI. [Maarten van Aalst, Netherlands]	Accepted. A sentence has been added not indicate the scope of this section, which is limited to physical aspects of compound events.
88009	109	45	111	30	SROCC section 6.8.2 also discusses WBC regions where warming boundary currents contribute to greater local SLR together with more intensified local storm storms and hence storm surge and rainfall as regions of heightened likelihood of compound events as climate changes [Kathleen McInnes, Australia]	Noted. A reference has been added.
62503	109	48	109	50	The usage of word hazard is not appropriate for the definition. Better to stick to the standard definitions in DPSIR analysis (Drivers, Pressures, State, Impact, Response). I would define a compound event as the combination of multiple drivers and/or pressures that contribute to societal or environmental risk. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. However, the present focus is in the context of the WG1 assessment and thus only focuses on climate-impact drivers (or hazard), not on other aspects contributing to risk (vulnerability and exposure). The definitions have been revised to better highlight this context.
74365	109	52	109	54	Yulizar and Bardossy (2020), Study of changes in the multivariate precipitation series'; extremes have spatial and temporal extents that called as unusual events. An event might not be considered as an extreme at one place or time, but might resulting unusual events due to their interaction in time and space (multivariate) [Yulizar Yulizar, Indonesia]	Noted. Article could not be included because of space constraints.
110021	109	52	109	54	But then what about the successively occuring events? Do they get excluded per this interpretation text here? That would seem unduly limiting. See similar concerns raised on the ES. [Peter Thorne, Ireland]	Accepted. This section has been revised to introduce the typology by Zscheischler et al., 2020, which also covers sequences of hazards
117123	109		109		reference to SROCC needed as some compound events were assessed in ch 6 [Valerie Masson-Delmotte, France]	Noted. But compound events are assessed in 11.8
44417	110	2	110	2	According to the terminology introduced in Ch1, and used in Ch12, the word "hazards" needs to be replaced with "climatic impact drivers". [Jana Sillmann, Norway]	Accepted. The word hazard has been replaced with "climatic-impact drivers", except when text from articles was cited.
44425	110	5	110	16	This paragraph fits better in Ch 12 as it refers to implications of coumpound events for impact and risk assessment. Note also that on page 10 (line 34) in Ch11, it is emphazised "Note that this chapter does not assess impacts" [Jana Sillmann, Norway]	Rejected. This paragraph merely highlights the relevance of certain types of events as it is otherwise what the analyses in the climate space should focus on.
79131	110	8	110	8	Is 'coping capacity' a proper term? [Andong Shi, Sweden]	Noted. The term was retained since it is well established in the literature.
44419	110	11	110	31	The word "hazards" needs to be replaced with "climatic impact drivers". [Jana Sillmann, Norway]	Accepted. The word hazard has been replaced with "climatic-impact drivers", except when text from articles was cited.

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23659	110	12	110	15	A compound event produced a heat wave in southern South America, flooding in the hyperarid region of the Atacama desert, and record melting and warmth in the Antarctic Peninsula during the March 2015. This can be a good example of geological, glaciological, public health, and biological implications of such large scale compund event. Barrett, B. S., Campos, D. A., Veloso, J. V., & Rondanelli, R., 2016. Extreme temperature and precipitation events in March 2015 in central and northern Chile. Journal of Geophysical Research: Atmospheres, 121, 4563–4580. https://doi.org/10.1002/2016JD024835 Bozkurt, D., Rondanelli, R., Garreaud, R., Arriagada, A., 2016. Impact of warmer eastern tropical Pacific SST on the March 2015 Atacama floods. Monthly Weather Review, 144 (11), 4441-4460, https://doi.org/10.1175/MWR-D-16-0041.1. Bozkurt, D., Rondanelli, R., Marín, J., Garreaud, R., 2018. Foehn event triggered by an atmospheric river underlies record-setting temperature along continental Antarctica. Journal of Geophysical Research-Atmospheres, 123(8) 3871-3892, https://doi.org/10.1002/2017JD027796. Rondanelli, R., Hatchett, B., Rutllant, J., Bozkurt, D., Garreaud, R., 2019. Strongest MJO on record triggers extreme Atacama rainfall and warmth in Antarctica. Geophysical Research Letters, 46(6), 3482-3491, https://doi.org/10.1029/2018GL081475. [Deniz Bozkurt, Chile]	Noted. The example is too detailed to include given the space limitations of the chapter (which is already too long).
9887	110	14	110	15	Add an example for a compound extreme case based on a new study, i.e., "…can lead to tree mortality (Allen et al.,15 2015); wildfires increase occurrences of hailstones and lightning (Zhang et al., 2019)." Reference, Zhang, Y., Fan, J., Logan, T., Li, Z., and Homeyer, C. R. (2019). Wildfire impact on environmental thermodynamics and severe convective storms. Geophysical Research Letters, 46, https://doi.org/10.1029/2019GL084534. [Jiwen Fan, United States of America]	Accepted. Reference has been added
79	110	15	110	15	When discussing compund extremes at different locations you can add the following references: (1) concurrent wet-dry hydrlogical extremes (or floods-droughts) at the global scale <De Luca, P., Messori, G., Wilby, R. L., Mazzoleni, M., and Di Baldassarre, G.: Concurrent wet and dry hydrological extremes at the global scale, Earth Syst. Dynam., https://doi.org/10.5194/esd-2019-27, 2020>; and (2) compound cold-wet extremes in Eastern North America and Europe <De Luca, P., Messori, G., Pons, F. M. E., and Faranda, D. Dynamical systems theory sheds new light on compound climate extremes in Europe and Eastern North America Q. J. R. Meteorol. Soc., https://doi.org/10.1002/qj.3757>. [Paolo De Luca, Netherlands]	Accepted. References has been added
88485	110	15	110	15	It might be relevant to note something like "Compound storm types consisting of co-located cyclone, front and thunderstorm systems have a higher change of causing extreme rainfall and extreme winds than individual storm types (Dowdy and Catto 2017)." Reference: Dowdy, A.J. and Catto, J.L., 2017. Extreme weather caused by concurrent cyclone, front and thunderstorm occurrences. Scientific Reports, 7, p.40359, https://doi.org/10.1038/srep40359 [Andrew Dowdy, Australia]	Accepted. Reference has been added
39831	110	16	110	16	"spatially-concurrent' -> Don't you mean just temporally concurrent? [TSU WGI, France]	Accepted. The beginning of the sentence has been rephrased as "Extremes may occur at similar times at different locations" to clarify the meaning
62505	110	18	110	25	I would suggest using the DPSIR framework or the DAPSI(W)R(M) (Elliot et al., 20), which can be adapted as a unifying framework for all environmental management issues. [Elliott, M., Burdon, D., Atkins, J. P., Borja, Á., Cormier, R., De Jonge, V. N., and Turner, R. K. 2017. "And DPSIR begat DAPSI(W)R(M)!" - A unifying framework for marine environmental management. Marine Pollution Bulletin, 118(1–2): 27–40, https://doi.org/10.1016/j.marpolbul.2017.03.049] [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This does not fully match the Risk framework from IPCC, which focuses instead on three dimensions only (hazards/climatic-impact drivers, vulnerability and exposure).
126099	110	18	110	25	Can an opposite effect exist? Is it possble that an event viewed in a univariate framework might overstate its (real) impact when the multivariate viewpoint is adopted? In other words, can nonlinearity reduce risk rather than amplify it? Perhaps there is an analogous question that can be asked for all types of compound events in general. [Trigg Talley, United States of America]	Accepted, this comments is correct. A reference to Hillier et al., 2020 (Nat. Clim. Ch., https://www.nature.com/articles/s41558-020-0832-y) has been added
62507	110	18	111	23	In addition to the suggestions of Moftakhari et al., (2017), compound or concurrent events can occur as sea-level rise due to long term consequence of climate change and extreme events such as storm surges superimposed with the maximum spring high tide. Spring high tide cannot be considered as an extreme event but if it is combined with the other two events, the impact would be very high. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The dependence on tide has been added
40633	110	28	111	23	Please check that use of the term 'risk' in this paragraph is consistent with IPCC usage. If simply you're referring to the physical flood hazard, then you shouldn't use the term 'risk'. Risk, as defined by the IPCC, also factors in exposure and vulnerability. [TSU WGI, France]	Accepted. When referring to "occurrence" or "probability" in WG1 context only, the word "risk" has been replaced.
44403	110	28	111	30	The definition of flood risk only contains physical elements, nothing about exposure and vulnerability. Thus, the use of the word risk in this context (and in general in this chapter when addressing flood risk) needs either to be justified (as it does not reflect the IPCC definition of risk) or replaced by another word that is appropriate in the physical science context (such as probability, occurence, frequency, etc) [Jana Sillmann, Norway]	Accepted. The word "risk" has been changed into "occurrence"
126101	110	30	110	54	[PRECISION] The subject is also covered in Chapter 9 . It would be good to coordinate the coverage and main conclusions between chapters in the report. Also the change in coastal intense precipitations can significantly change design storm properties, important to engineering and planning. [Trigg Talley, United States of America]	Accepted. Text has been cross-checked.
44375	110	31	110	31	the use of "flood risk" is not really justified in this context (see definition of the term risk in IPCC). It should be rephraseed to "the probability of flood occurence" [Jana Sillmann, Norway]	Accepted. Replaced with "flood occurrence".
5603	110	31	110	32	The floods in the coastal regions are influenced by strom surge, extreme rainfall, river flow, but also by sea level rise, wave, tide (and groundwater for estuaries) [Benoit Laignel, France]	Accepted. The sentence has been adapted accordingly
44377	110	35	11	4	the repeated use of "flood risk" in this paragraph is not justified in this context (see definition of the term risk in IPCC). What is refered to in this paragraph is actually the changes in a the climatic impact driver (related to floods, see Ch 12). Elements of vulnearbility or exposure are not considered, thus the use of the term "risk" should therefore be avoided (see also IPCC guidance document on risk). It should be rephraseed to "probability of floods" [Jana Sillmann, Norway]	Accepted. The term risk has been replaced
70977	110	35	110	42	A global analysis of compound flood risk (including the role of dependence) has recently been submitted by Bevacqua et al. (https://eartharxiv.org/4x2u8/) [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The reference has been added
24463	110	35	110	54	Observed compound effects of storm surge and river discharge by Typhoon Jebi was summarized following article. Mori, N., T. Yasuda, T. Arikawa, T. Kataoka, S. Nakajo, K. Suzuki, Y. Yamanaka, A. Webb (2019) 2018 Typhoon Jebi post-event survey of coastal damage in the Kansai region, Japan, Coastal Engineering Journal, Taylor & Francis, 61:3, pp.278-294. [Nobuhito Mori, Japan]	Noted. Reference could not be included because of space limitations.
5605	110	39	110	47	The English Channel is also concerned by teh dependence between storm surge and extreme rainfall (PhD K Graff, 2020) [Benoit Laignel, France]	Noted. Reference could not be included because of space limitations.

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70979	110	42	110	42	A comparison between using river discharge data vs precipitation in the estimation of compound flood risk by Bevacqua et al. has recently been accepted for publication (https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2019-415/) [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The reference has been added
38369	110	49	110	49	Taiwan is a province of China, not an independent country. The current expression is seriously wrong. The term “Taiwan” shall be changed to “Taiwan, province of China”. [Yaming LIU, China]	Rejected. Too detailed.
70975	110	54	111	1	This sentence practically repeats what is said on p.110, line 42 [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence has been removed
9907	111	14	111	14	The phrase a number of may be wordy. Consider changing the wording. (Several) [ayman badawy, Egypt]	Rejected. The reference may be wrong. Cannot find word "may" in indicated location.
71451	111	15	111	18	We have extended the European study to a global study, currently under review (Bevacqua et al. subm. To Comm. Earth Environ., https://eartharxiv.org/4x2u8/). We show that under a high emissions scenario the concurrence probability of extreme meteorological tides and inland precipitation would increase by more than 30% on average along coastlines worldwide by 2100 compared to present. In latitudes above 40 north, compound meteorological extremes would become more than 2.5 times as frequent, while they would happen less frequently in parts of the subtropics. Climate-induced dynamics in precipitation extremes contribute to about 80% of the projected change in concurrence probability, while dynamics in meteorological tides account for 16% and those in the dependence between the two extremes for 4%. [Douglas Maraun, Austria]	Accepted. The text and reference has been added
44379	111	17	111	18	The sentence should read: The increasing probability of compound flooding is mostly driven by an intensification of precipitation extremes and aggravated flooding (intensity and frequency) due to sea level rise. See IPCC guidance on risk for appropriate use of the term "risk" throughoput the IPCC report. [Jana Sillmann, Norway]	Accepted. The sentence has been adapted accordingly
5607	111	17	111	18	In fact, the increasing risk of flooding is driven by precipitation extremes, stronger storm, river flood and sea level rise [Benoit Laignel, France]	Rejected. There is no literature on stronger storms and stronger river floods and their impact on compound flooding trends
126103	111	27	111	30	The summary should be reconsidered. The medium confidence level may be appropriate globally, but the qualifiers "some locations" and "along the US coastline" would seem to justify higher confidence for those particular instances. It may be appropriate to note the different confidence levels for different regions. [Trigg Talley, United States of America]	Rejected. Because of limited literature, "medium confidence" is considered appropriate.
44381	111	28	111	28	replace "risk" with "probability" . See IPCC guidance on risk for appropriate use of the term "risk" throughoput the IPCC report. [Jana Sillmann, Norway]	Accepted. This has been changed
5609	111	28	111	30	Why is there a medium confidence for the increasing risk of flooding. In the coastal zone, there is a high confidence that the sea level rise combined with the storm and heavy rainfall will aggrave the coastal flooding. With the sea level rise, we do not need a strong storm to have a flooding [Benoit Laignel, France]	Accepted. Confidence level has been changed to "high"
44405	111	30	112	32	The section mainly assesses the climatic impact driver "wildfire" see chapter 12 and physical conditions (e.g. ECVs) that increase wildfire occurence. I therefore think that using the wording wildfire risk is not appropriate according to the IPCC guidance document on risk. [Jana Sillmann, Norway]	Accepted. This has been adapted accordingly
126105	111	33	112	32	As noted, dry spells and hot spells have always been correlated. Similarly, damagingly high winds and extreme rainfall are correlated. One wonders why concurrent droughts and heatwaves constitute a compound event, while strong storms (tropical or not), for example, do not. No specific suggestion for revision here, just a suggestion that the authors think about it. [Trigg Talley, United States of America]	Rejected. While dry and hot conditions are correlated, they are not changing at the same rate with global warming. Hot conditions are strongly increasing everywhere. This alone will lead to more frequent dry and hot conditions, even in regions when droughts are not becoming more frequent.
104689	111	33	112	32	Given that the conpound extremes have high impacts on socioty, and are main concerns for future climate change, would it be good to add a subsection about 'climate model evaluation' on reprenting such events (like Section 11.3.3)? Or if there are quite few studies focusing on the model evaluation, would it be suitable to mention such a need in summary of this section? [Peng Zhang, Sweden]	Noted. However, the available literature is limited to provide a detailed assessment on this point.
43437	111	44			Read " (Mueller and Seneviratne, 2012; Whan et al., 2015) " rather than " (Mueller and Seneviratne, 2012, Whan et al., 2015) " [Cyriaque Rufin Nguimalet, Central African Republic]	Not applicable. This part of the sentence was removed.
85073	111	48	111	49	Comment provided by Stacey New: Examples of the UNSEEN method could also be used again here as an example of research looking into compound events and the results of these events e.g. Maize yield shocks (Chance of experiencing unprecedented drought is 5% is China – Kent et al. (2019), Maize Drought Hazard in the Northeast Farming Region of China: Unprecedented Events in the Current Climate.). This would be good examples of potential crop failure events. [Stacey New, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Too detailed given space constraints.
44427	111	48	111	52	These sentences fit better in Ch 12 (section 12.3.7.2) as the text refers to implications of coumpound events for impact and risk assessment. [Jana Sillmann, Norway]	Not applicable. The sentences have been removed
39325	111	48	111	52	I am surprised to see a discussion, albeit, a short one, of impacts [Lourdes Tibig, Philippines]	Not applicable. This text has been removed
11015	111	50	111	51	Additional support evidence. González, ME., S Gómez-González, A Lara, R Garreaud, I Díaz-Hormazábal. 2018. The 2010-2015 Megadrought and its influence on the fire regime in central and south-central Chile. Ecosphere DOI:10.1002/ecs2.2300 [Mauro Gonzalez, Chile]	Not applicable. The section on impacts has been removed
11017	111	50	111	51	Additional support evidence. Bowman D.M., A. Moreira-Muñoz, C.A. Kolden, R.O. Chávez, A.A. Muñoz, F. Salinas, A. González-Reyes, R. Rocco, F. de la Barrera, G.J. Williamson, N. Borchers, L. A. Cifuentes, J.T. Abatzoglou, F.H. Johnston. 2018. Human–environmental drivers and impacts of the globally extreme 2017 Chilean fires. Ambio https://doi.org/10.1007/s13280-018-1084-1 [Mauro Gonzalez, Chile]	Not applicable. The section on impacts has been removed
71453	112	1	111	5	The Manning et al., 2019 paper is on changes in observed compound long-duration hot and dry events. So it should be moved to the sentence before on the observational record. [Douglas Maraun, Austria]	Accepted. This has been changed
126107	112	2			"… will continue to do so …" is a very strong statement, implying 100% certainty, since there is no confidence level given. Is this justified? What if drought occurrence decreases more than expected? [Trigg Talley, United States of America]	Rejected. Temperature will increase everywhere under unabated warming. Hence, even without decreasing precipitation trends, the likelihood of compound drought and heat will increase. This is also stated in the last sentence of this paragraph
9203	112	3	112	7	Add "and southeast Australia (Kirono et al (2017). See Kirono, D., Hennessy, K. and Grose, M. (2017). Increasing risk of months with low rainfall and high temperature in southeast Australia for the past 150 years. Climate Risk Management, 16, 10-21, https://doi.org/10.1016/j.crm.2017.04.001 [Kevin Hennessy, Australia]	Accepted. This has been added
45689	112	7	112	7	higher than the minimum ==> higher than the one in minimum [Christophe Deissenberg, Luxembourg]	Rejected. Could not find referred text.
9205	112	14	112	14	change "Hope et al. 2019" to "Hope et al. 2019; Dowdy 2018)". The new reference is Dowdy, A.J. (2018). Climatological Variability of Fire Weather in Australia. Journal of Applied Meteorology and Climatology 57: 221-234. [Kevin Hennessy, Australia]	Accepted. This has been added

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105971	112	14	112	19	When discussing the California wildfires via the lens of compound extreme events, it may also be valuable to consider the role that shifting atmospheric patterns have played in intensifying wildfire behavior — particularly the recent increase in extreme autumn wildfires. Liu, et al. (2019) note that the frequency of offshore Diablo wind events in Northern California, particularly extreme ones, doubled from 1979-1998 to 1999-2018. Based on this, they suggest that this "impl[ies] a higher likelihood of wildfires in the last 20-year period compared to the previous 20-year period." Goss, et al. (2020) also identify a role for offshore wind events in driving extreme autumn wildfires, noting that they "have coincided with extreme fire weather conditions during periods of strong offshore winds coincident with unusually dry vegetation enabled by anomalously warm conditions and late onset of autumn precipitation." Liu, Y. C., P. Di, S. H. Chen, X. M. Chen, and J. DaMassa. "Diablo Winds in the California Bay Area: Their Climatology, Extremes, and Predictability." AGU Fall Meeting Abstracts 23 (December 1, 2019). https://perma.cc/V7EV-27D5. Goss, Michael, Daniel L. Swain, John T. Abatzoglou, Ali Sarhadi, Crystal Kolden, A. Park Williams, and Noah S. Diffenbaugh. "Climate Change Is Increasing the Risk of Extreme Autumn Wildfire Conditions across California." Environmental Research Letters, 2020. https://doi.org/10.1088/1748-9326/ab83a7. [Sohum Pawar, United States of America]	Rejected. Too detailed given space constraints.
15161	112	14			The wording "occurrence" here is off. Burned area extent here is more appropriate. Also, note that this result is not unique to California, but was shown for western US forests in Abatzoglou and Williams, 2016. [John Abatzoglou, United States of America]	Accepted. This has been changed
9909	112	18	112	18	The noun phrase ecosystem seems to be missing a determiner before it. Consider adding an article. [ayman badawy, Egypt]	Considered. "Vegetation" has been changed into "vegetation type"
88487	112	19	112	19	An example for Australia could be provided, such as "Observations show a long-term trend towards more dangerous weather conditions for bushfires in many regions of Australia which is attributable at least in part to anthropogenic climate change (Dowdy 2018). Reference: Dowdy, A.J., 2018. Climatological variability of fire weather in Australia. Journal of Applied Meteorology and Climatology, 57(2), 221-234, https://doi.org/10.1175/JAMC-D-17-0167.1 [Andrew Dowdy, Australia]	Accepted. This has been added
85075	112	19	112	20	Comment provided by Jennifer Weeks: Paragraph spacing needed. [Stacey New, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The two paragraphs were merged.
11755	112	22	112	24	another important reference worth a sentence right here is: Jolly, W. M., Cochrane, M. A., Freeborn, P. H., Holden, Z. A., Brown, T. J., et al. (2015). Climate-induced variations in global wildfire danger from 1979 to 2013. Nature Communications, 6, 7537. doi:10.1038/ncomms8537. They found that between 1979 and 2013 the global burnable area affected by long fire-weather seasons doubled, and the mean length of fire-weather season increased by 19%. [Amy East, United States of America]	Accepted. This has been added
126109	112	22	117	42	Are compound events receiving special attention because their effects are not simply additive? Or is it just that they are correlated? Is it worth making this distinction? Does Box 11.3 illustrate non-linear impacts of the type suggested in the final sentence of the box, or is that more in the realm of speculation? [Trigg Talley, United States of America]	Noted. Compound events are mentioned because their effects are not simply additive. In addition, a substantial body of literature has emerged on this topic. Finally, the IPCC scoping document for the AR6 explicitly indicated that ch11 should address compound events.
126111	112	29	112	30	[DROUGHT] Does at the global scale here mean "in many regions" or the global mean occurrence of concurrent heatwaves and drought? This language is confusing and the underlying discussion in the text seems to suggest observed increases in many regions. Also the link of the increases to anthropogenic forcing comes up in the summary section but is not even mentioned explicitly in the text above. For high confidence, some discussion/justification is needed. One can imagine there is high confidence in anthropogenic driven increases in heatwaves, but the compound event (co-occurring drought and heatwaves) needs its own explicit discussion and justification on why high confidence that observed increases are due to human influence. [Trigg Talley, United States of America]	Noted. As stated higher up "The dominant signal is related to the increase in heatwave occurrence, which means that even if drought occurrence is unaffected, compound hot and dry events will be more frequent." Since heatwave occurrence has been attributed to anthropogenic forcing, the increase in drought and heat is also linked to anthropogenic forcing. The clause "which has been attributed to anthropogenic forcing has been added after "heatwave occurrence" to make this link clear.
9207	112	29	112	32	replace "over some regions" with "over southern Europe, northern Eurasia, the US and Australia". Is there a specific part of the US which has become more fire prone, e.g. California? The lack of emphasis on fire trends and projections in this summary and the preceding text is surprising, given the high level of policy-relevance and the wide range of literature. Much more could be said about regional fire-weather projections (e.g. Dowdy, A.J., Ye, H., Pepler, A., Thatcher, M., Osbrough, S.L., Evans, J.P., Di Virgilio, G. and McCarthy, N., (2019b). Future changes in extreme weather and pyroconvection risk factors for Australian wildfires. Scientific Reports, 9(1), pp.1-11, and papers cited within). A statement about fire-weather projections should be included here and in the Executive Summary. [Kevin Hennessy, Australia]	Accepted. The text has been adapted accordingly
110023	112	30	112	31	I'm not sure I saw enough of an evidence basis in the underlying text to justify this finding - what is the defensible trace for this? [Peter Thorne, Ireland]	Noted, The text has been slightly reworded.
108153	112	35	113	16	Other examples of extreme precipitation and temperature compound events could be mentioned in this section. Tencer et al (2014) documented these compound events over Canada whereas Tencer et al (2016) and Olmo et al (2020) over Southern South America. Tencer B., Weaver A., Zwiers F., 2014. Joint occurrence of daily temperature and precipitation extreme events over Canada. J. of Applied Meteorology and Climatology 53, 2148:2162. Tencer B., Bettolli ML., Rusticucci M., 2016. Compound temperature and precipitation extreme events in Southern South America: associated atmospheric circulation and simulations by a multi-RCM ensemble. Clim. Res. 68, 183–199. Olmo M, Bettolli ML, Rusticucci M. Atmospheric circulation influence on temperature and precipitation individual and compound daily extreme events: spatial variability and trends over southern South America. Weather and climate extremes. Submitted December 2019. [Maria Bettolli, Argentina]	Rejected, The IPCC here assesses trends in compound events or future projections, the suggested studies do not discuss trends.
5685	112	35	113	19	The influence of climate extremes on pathogens is missing. Extreme events like drought can trigger insect pest outbreaks (e.g., bark beetles in Central European forests in 2018 / 2019). Please include this field of study here. [Joachim Rock, Germany]	Rejected. Too detailed given space constraints and not directly related to WG1 scope.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
81	112	35	113	19	I think it would be worth adding one or two sentences that mention these three other types of compound extremes: (1) concurrent wet-dry extremes (i.e. floods-droughts) in remote regions around the globe <De Luca, P., Messori, G., Wilby, R. L., Mazzoleni, M., and Di Baldassarre, G.: Concurrent wet and dry hydrological extremes at the global scale, Earth Syst. Dynam., https://doi.org/10.5194/esd-2019-27, 2020>; (2) compound wet-windy extremes, or widespread flooding driven by extra-tropical cyclones <De Luca, P., Hillier, J.K., Wilby, R.L., Quinn, N.W., and Harrigan, S. (2017) Extreme multi-basin flooding linked with extra-tropical cyclones Environ. Res. Lett., 12, 114009, https://doi.org/10.1088/1748-9326/aa868e> and <De Luca, P., Messori, G., Pons, F. M. E., and Faranda, D. Dynamical systems theory sheds new light on compound climate extremes in Europe and Eastern North America Q. J. R. Meteorol. Soc., https://doi.org/10.1002/qj.3757>; and (3) compound cold-wet extremes <De Luca, P., Messori, G., Pons, F. M. E., and Faranda, D. Dynamical systems theory sheds new light on compound climate extremes in Europe and Eastern North America Q. J. R. Meteorol. Soc., https://doi.org/10.1002/qj.3757>. [Paolo De Luca, Netherlands]	Rejected, The report here assesses trends in compound events or future projections, the suggested studies do not discuss trends.
100779	112	35			There should be a Section 11.8.4 Compound Air Pollution and Heat Waves, or at a minimum this topic should be discussed under 11.8.3. The combined temperature-pollution would seem to be an important highlight for this chapter. In terms of concurrent extremes that increase risk, the unique work by J Schnell describing how heat waves, ozone pollution and particulate matter pollution based on regional observations should be a key example of overlapping extremes for human health. These results should be in WGI Chapter 11 because they are not examples of local/urban pollution, but rather cases that can and should be simulated with current chemistry-climate models and projected for the future. J.L. Schnell and M.J. Prather (2017), Co-occurrence of extremes in ozone, particulate matter, and temperature, PNAS, 114(11): 2854-2859, doi: 10.1073/pnas.1614453114. [Michael PRATHER, United States of America]	Noted. Temperature and air pollution is discussed in the 2nd paragraph of this section
110027	112	35			I would have expected here to see mention of daisy chained extremes such as e.g. late winter snow and cold followed by drought (NW Europe, 2018) or hurricanes followed by heat waves (Matthews et al., 2019, Nature Climate Change). Where is the discussion of meteorological juxtaposed events which might not be directly coincident or related such as these examples? [Peter Thorne, Ireland]	The IPCC here assesses trends in compound events or future projections not merely lists all kind of possible event types. The Matthews at al study is discussed further down.
6805	112	37	112	37	Humans are also susceptible to cold conditions, which can cause hypothermia. Although the risks of hyperthermia go up as the climate warms, the risks of hypothermia go down. Should this be mentioned? Can it be stated that the risks of increased hyperthermia outweigh the risks of decreased hypothermia? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. All of section 11.8.3 has been removed.
82777	112	37	112	45	The Raymond et al 2020 paper on extreme high wet bulb temperatures (https://advances.sciencemag.org/content/6/19/eaaw1838) is also worth discussing here. [Blair Trewin, Australia]	has been added
10077	112	37	112	45	See also Li et al 2020, /10.1088/1748-9326/ab7d04, using large initial conditions ensembles to look at extremes of WBGT as a function of GSAT [Robert Kopp, United States of America]	Accepted. This has been added
42333	112	37	112	45	This paragraph is really oreineted toward health impacts and is in the remits of CH12, which discusses heat setress by region. Suggestion to move material to CH12 [robert vautard, France]	Accepted. All of section 11.8.3 has been removed.
29281	112	37	112	45	I recommend that Raymond et al., 2020 ("The emergence of heat and humidity too severe for human tolerance" Science Advances) be referenced and discussed here as well. [Andra Garner, United States of America]	Not applicable. All of section 11.8.3 has been removed.
100497	112	37	112	45	It could be useful to assess additional studies reporting future projections in extreme indices that are relevant from a human health perspective. For instance, Mora et al. (2017) report projections of heatwaves identified as deadly based on health impact data. In addition, Pal and Eltahir (2015) and Im et al. (2017) show that late 21st century projections of wet bulb temperature extremes may exceed thesholds of human adaptability under RCP8.5 in South Asia and around the Persian Gulf. In essence, these three studies all investigate combined changes in temperature and near-surface humidity. REFS: Mora, C., Dousset, B., Caldwell, I. R., Powell, F. E., Geronimo, R. C., Bielecki, C. R., ... & Lucas, M. P. (2017). Global risk of deadly heat. Nature Climate Change, 7(7), 501-506.; Im, E. S., Pal, J. S., & Eltahir, E. A. (2017). Deadly heat waves projected in the densely populated agricultural regions of South Asia. Science advances, 3(8), e1603322.; Pal, J. S., & Eltahir, E. A. (2016). Future temperature in southwest Asia projected to exceed a threshold for human adaptability. Nature Climate Change, 6(2), 197. [Wim Thiery, Belgium]	Not applicable. All of section 11.8.3 has been removed.
110025	112	37	112	45	There is a substantive assessment of WBGT undertaken in chapter 4. Why are you repeating it here and why in particular are you not cross-referencing it? [Peter Thorne, Ireland]	Not applicable. All of section 11.8.3 has been removed.
82771	112	38	112	39	This should read "hot and humid", not "hot and dry" [Blair Trewin, Australia]	Not applicable. All of section 11.8.3 has been removed.
33255	112	38	112	39	Typo: It should be "The effect of extremely hot and wet conditions on humans" (I think it might be overlooked by the copy-editing process) [Gonzalez Sergi, Spain]	Not applicable. All of section 11.8.3 has been removed.
20759	112	39	112	39	Hot and dry? In view of the context one would expect "hot and wet" [philippe waldteufel, France]	Not applicable. All of section 11.8.3 has been removed.
126115	112	39	112	39	Is "hot and dry" supposed to read rather as "hot and humid"? [Trigg Talley, United States of America]	Not applicable. All of section 11.8.3 has been removed.
28627	112	39	112	39	Should "dry" be "humid", because the extremely hot and humid conditions are harmful to humans? [Jiacan Yuan, China]	Not applicable. All of section 11.8.3 has been removed.
126113	112	39			The term "dry" should be "humid". [Trigg Talley, United States of America]	Not applicable. All of section 11.8.3 has been removed.
28629	112	42	112	44	Coffel et al. (2018) actually used wet bulb temperature rather than WBGT. In addition, what wet bulb temperatures are considered “extreme” should be clarified. [Jiacan Yuan, China]	Not applicable. All of section 11.8.3 has been removed.
28631	112	42	112	45	There is a newly-published paper (Li et al. 2020) which has found frequency of extreme WBGT events have dramatically increased over tropics and midlatitudes in response to global warming. The world population annually exposed to WBGT exceeding 33°C (harmful for healthy humans at rest) has already increased three-fold to 275 million due to the current 1°C warming, and that is expected to reach 789 million with a 2°C warming. Reference: Li, D., Yuan, J., & Kopp, R. (2020). Escalating global exposure to compound heat-humidity extremes with warming. Environmental Research Letters. https://doi.org/10.1088/1748-9326/ab7d04 [Jiacan Yuan, China]	Not applicable. All of section 11.8.3 has been removed.
109789	112	45	112	45	Just a possible addition: A model study in which future atmospheric circulation was nudged to present-day conditions recovers much of the changes in heat stress characteristics found in fully coupled models, confirming the notion that changes in humidity-heat compound event charcteristics are primarily thermodynamically driven (Rastogi, D., F. Lehner, M. Ashfaq (2020): Revisiting recent United States heatwaves in a warmer and more humid climate. Geophysical Research Letters, DOI: 10.1029/2019GL086736). [Flavio Lehner, Switzerland]	Not applicable. All of section 11.8.3 has been removed.
13843	112	47	112	47	It's recommend briefly to explain the role of the high ozone concentrations on the high temperatures and droughts (compound event). [Maria Amparo Martinez Arroyo, Mexico]	Not applicable. All of section 11.8.3 has been removed.
18275	112	47	112	53	Cross chapter link to Section 6.4.1 is recommended here. Please note that the result from Meehl et al., 2018, that the ozone levels will decrease on heatwave days during 2080-2099 with reduced NOx in US and Europe, could be dependent on the choice of isoprene chemistry mechanism, and thus atmospheric chemistry community may not regard this as conclusive. [Yugo Kanaya, Japan]	Not applicable. All of section 11.8.3 has been removed.
126117	112	48	112	49	Is the effect on ecosystem carbon uptake short-lived or does it last much longer than the time scale of the event? Is a short-lived effect something that matters for impacts? [Trigg Talley, United States of America]	Not applicable. All of section 11.8.3 has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
126119	112	48	112	49	Are effects of ozone on human health perhaps worth mentioning here? [Trigg Talley, United States of America]	Not applicable. All of section 11.8.3 has been removed.
17705	112	49	112	50	"future heat waves become more intense" should be "future heat waves are expected to become more". [Sridhara Nayak, Japan]	Not applicable. All of section 11.8.3 has been removed.
108965	112	49	112	53	In fact the Meehl et al study only examined RCP6. Other scenarios with increasing global methane levels may have globally increased ozone. Thus, locally ozone may go up even though local ozone precursors decrease. In this case it is not altogether clear what the relation is between heat waves and ozone in those locations where local precursors decrease. The very general statement given here should be modified to the specifics of what has been shown. [Peter Hess, United States of America]	Not applicable. All of section 11.8.3 has been removed.
117125	112		112		coordination is needed x chpaters for fire weather. Please build on SRCCL for this aspect. [Valerie Masson-Delmotte, France]	Accepted. Text has been cross-checked with SRCCL and ch12. References have been added.
41121	113	1	113	1	Unclear what 'SES' stands for [TSU WGI, France]	Not applicable. All of section 11.8.3 has been removed.
126121	113	1	113	1	The acronym SES was used as South East Southern America (SES) in line 25, page 11-42. Consider changing it here or the earlier instance. [Trigg Talley, United States of America]	Not applicable. All of section 11.8.3 has been removed.
11757	113	1	113	1	this is confusing because elsewhere in this chapter, the acronym SES is used to refer to Southeastern South America. What is SES being used for here? Based on line 4 it sounds like SES is a type of event, but what does this stand for? [Amy East, United States of America]	Not applicable. All of section 11.8.3 has been removed.
23661	113	1	113	3	Bozkurt et al. (2019) analyzed the extreme 2004 case when anomalous warming of the mountainous eastern Anatolia resulted in unprecedented snowmelt runoff amounts in the Euphrates and Tigris basins together with the accompanying rainfall due to atmospheric river. Eventually, an extreme discharge with a return period of ~50 years caused floods adversely affecting the residential and agricultural areas in the basin. The event also triggered, for the first time, the opening of the spillways of the basin's several dams to release water to accommodate the incoming flux. Therefore, Bozkurt et al. (2019) can be added as an example of heavy rainfall and snowmelt over the mountainous areas. Bozkurt, D., Ezber, Y., Sen, O.L., 2019. Role of the East Asian Trough on the eastern Mediterranean temperature variability in early spring and the extreme case of 2004 warm spell. Climate Dynamics, 53(3-4), 2309–2326, https://doi.org/10.1007/s00382-019-04847-5. [Deniz Bozkurt, Chile]	Not applicable. All of section 11.8.3 has been removed.
23769	113	1	113	11	rain-on-snow impacts are an issue for high latitude as they lead to icings with sever impacts, see IPCC cryosphere special report and e.g. http://dx.doi.org/10.1098/rsbl.2016.0466 and https://esajournals.onlinelibrary.wiley.com/doi/abs/10.1890/09-1927.1 [Annett Bartsch, Austria]	Not applicable. All of section 11.8.3 has been removed.
100509	113	3	113	3	As a possible additional example, intense precipitation preceded by wildfire and/or landslides may lead to mudflows and flash floods in mountainous regions (Jacobs et al., 2016). REF: Jacobs, L., Maes, J., Mertens, K., Sekajugo, J., ... & Dewitte, O. (2016). Reconstruction of a flash flood event through a multi-hazard approach: focus on the Rwenzori Mountains, Uganda. Natural hazards, 84(2), 851-876. [Wim Thiery, Belgium]	Not applicable. All of section 11.8.3 has been removed.
110577	113	3	113	3	Musselman et al. (2018) could be included here.Musselman, K. N., F. Lehner, K. Ikeda, M. P. Clark, A. F. Prein, C. Liu, M. Barlage, and R. Rasmussen, 2018: Projected increases and shifts in rain-on-snow flood risk over western North America. Nature Climate Change, 8, 808–812, https://doi.org/10.1038/s41558-018-0236-4. [Rachel McCrary, United States of America]	Not applicable. All of section 11.8.3 has been removed.
74575	113	5	113	5	Doschlod et al. To check if it isn't published [Moulay Driss HASNAOUI, Morocco]	Not applicable. All of section 11.8.3 has been removed.
44421	113	13	113	13	The word "hazards" needs to be replaced with "climatic impact drivers". [Jana Sillmann, Norway]	Not applicable. All of section 11.8.3 has been removed.
82549	113	13	113	14	The attribution statement doesn't necessarily follow from the observational one - there are some lines of evidence (see Figure 2.34) suggesting that ENSO variability in the late 19th/early 20th century was comparable with post-1950 (hence the comparison of post-1950 with pre-1850 in Chapter 2, with no assessment of the comparison between 1850-1950 and post-1950. [Blair Trewin, Australia]	Not applicable. All of section 11.8.3 has been removed.
83	113	16	113	19	I would remove the text and reference <Matthews, et al. 2019>. The study, although published in a high-impact journal and surely provocative, i) does not provide a strong observational basis with respect to such compound hazards; ii) it is based on a storyline approach and hence with this method any catastrophe can be ideally generated; and iii) from the abstract I have the impression that the authors are afraid that 'only an estimated 1,000 people have been impacted', highlighting the speculative message of the paper. [Paolo De Luca, Netherlands]	Not applicable. All of section 11.8.3 has been removed.
126123	113	16	113	19	The paper by Matthews et al. (2019) contends that the TC-heat combination is a rare case, which is questionable. Further their analysis for the increases under future climate conditions is based on an analogue stational relationship. This should be reexamined. [Trigg Talley, United States of America]	Not applicable. All of section 11.8.3 has been removed.
45691	113	20	113	20	"Overall temperature across the mountainous HKH will increase by " Under RCP8.5? [Christophe Deissenberg, Luxembourg]	Not applicable. All of section 11.8.3 has been removed.
43439	113	37			Read "several regions across the world. The first focuses " rather than "several regions across the world The first focuses " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. It was corrected by inserting a period.
96147	113	47	113	48	It should please be explained why the corrected normalization is the more appropriate estimate. And how was the corrected done? [Nicole Wilke, Germany]	Noted. Only one version of standardization is shown now. Also, the reference cited in the caption provides more details about normalization.
43443	113	49		50	Read "since 1990 onward (from Sippel et al., 2015). " or "since 1990 onward. From Sippel et al. (2015). " rather than "since 1990 onward. From Sippel et al. 2015. " [Cyriaque Rufin Nguimalet, Central African Republic]	Taken into account. The citation is corrected.
43441	113	49			Read "in the overall land area " rather than "in the overal land area " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. It was corrected to "overall".
82551	114	1	115	40	I think it would be useful to carry out more comparisons between 2015-16 and 1997-98 (the 1982-83 signal is complicated by the El Chichon eruption), something only touched on briefly in the current text - some questions of interest here would include how the overall footprint of impacts compares between the two events, and whether there is evidence that the additional 18 years of global warming has exacerbated impacts (especially temperature-sensitive impacts such as marine heatwaves). It may also be worth commenting on typical ENSO teleconnections which were weak or absent in 2015-16, e.g. high rainfall in California and western coastal South America. [Blair Trewin, Australia]	Noted. This BOX shows case studies of concurrent climate anomalies, and the 2015/2016 El Nino is one of the example. Its comparison is not a purpose of this section. The comparison between the two El Nino is also touched depending on literature.
82819	114	3	115	26	One clear signal of the 2015-16 event which has been missed here is its effect on global-scale temperatures, in particular its signal in very high monthly GMST values in late 2015 and early 2016 (contributing to record high annual GMST in 2016). [Blair Trewin, Australia]	Taken into account. The information of the highest GMST was inserted by refereeing to Chap 2 (Section 2.3.1.1).
6807	114	6	114	8	Is the absolute temperature of the Niño 3.4 region a sensible measure of the strength of an El Niño, considering the general rise in SST that is occuring as part of climate change. Should the strength of an El Niño not instead be characterised by the anomaly relative to a sliding climatology? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. SROCC defined extreme El Niño events based on precipitation anomalies over the NINO3 region (El Niño) and SST over the NINO4 region (La Niña) respectively, as proposed by Cai et al. (2014, 2015). (See Annex VI.2.3)
13805	114	8	114	8	Change Niño 3.4 by Niño3.4 [Maria Amparo Martinez Arroyo, Mexico]	Noted. This part is deleted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
110029	114	11	114	14	This appears to directly contradict the substantive assessments of chapters 3 and 4 on the matter and also the chapter 2 finding is selectively quoted. The assessment from all three chapters was effectivvely that no change was detected or robustly projected. This box cannot be phrased in such a way that it undermines the substantive assessment findings of the three chapters charged with the primary assessment on the matter. [Peter Thorne, Ireland]	Accepted. The assessment is deleted from here.
110033	114	25	115	11	This is a very long and very dense paragraph. For readability I would separate into several smaller paragraphs if at all possible. [Peter Thorne, Ireland]	Taken into account. The paragraph was divided.
68497	114	39			Unnecessary space before "2016" [Yukiko Imada, Japan]	Noted. This part is deleted.
110031	114	44	114	44	Is precision to 6 s.f. really warranted here? This seems like a easy target for those wishing to discredit the report when figures are given with such undue precisions. [Peter Thorne, Ireland]	Noted. This part is deleted.
82553	115	13	115	26	It was also the least active Australian region TC season since satellite records began in 1969-70 (a typical El Nino signal). The 2016 BAMS State of the Climate can be used as a citation for this. [Blair Trewin, Australia]	Taken into account. This information was added.
41151	115	13	115	26	What about hurricanes in the north Atlantic? Were there fewer? [TSU WGI, France]	Noted. ACE of hurricanes in the North Atlantic in 2015 was below normal (Blunden and Arndt 2016 BAMS), and we do not need to refer to it.
86309	115	30	115	32	Suggest seeking graphic support to make the figure look sleeker [TSU WGI, France]	Noted. This figure is deleted.
11759	116	2	116	2	to reflect the content of this paragraph more accurately, add “and precipitation” in the section subheading [Amy East, United States of America]	Accepted. The subsection title was modified.
110035	116	4	116	34	This paragraph would be easier to follow if it were split in two - one on heat impacts and one on precipitation. [Peter Thorne, Ireland]	Taken into account. The paragraph was divided.
10995	116	4	116	34	This section on spring/summer 2018 could note that many of the anomalies over Europe had a large contribution from a highly anomalous Summer NAO state, which does not appear related to anthropogenic forcing (Drouard et al, already cited). [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. NAO was mentioned here and Drouard et al. (2019) was referred to.
13807	116	9	116	9	Change 90’000 by 90, 000 [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Corrected.
11761	116	9	116	9	fix notation, “90,000 students” [Amy East, United States of America]	Accepted. Corrected.
13809	116	11	116	11	Close the parentheses in the following sentence: (leading to yield reductions of up to 50% for the main crops, (Toreti et al., 2019), [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Corrected.
8057	116	22	116	25	Imada et al. (2019) state that " the warm event in July 2018 would never have happened without human-induced climate change" (p. 12) but also that "the double-High condition appears to be natural variability and not affected by the human-induced climate change at this stage" (P. 12, see also their Fig. 2). Thus, the high temperatures can be attributed to anthropogenic forcing, but this is not the case for the anomalous atmospheric circulation. [jouni Räisänen, Finland]	Taken into account. The text was modified.
68499	116	22	116	25	"the anomalous North Pacific Subtropical High could not be simulated without greenhouse gas forcing in an ESM": This part is not true (at least, is not the result of Imada et al. 2019). Imada et al. (2019) showed that the "high temperature" could not be simulated without GHG forcing in an "AGCM" (not ESM). The paper also showed that the North Pacific Subtropical High in 2018 was extremely strong but within the range of natural variability. [Yukiko Imada, Japan]	Taken into account. The text was modified.
87401	116	28	116	28	Tsuji et al., 2019; YOKOYAMA et al. 2020' should be changed to 'Tsuji et al. 2020; Yokoyama et al. 2020' [Yukari Takayabu, Japan]	Accepted. The reference was updated.
68501	116	28			YOKOYAMA -> Yokoyama [Yukiko Imada, Japan]	Accepted.
68503	116	30	116	32	In addition to Kawase et al. (2019) which employed storyline EA, Imada et al. (under revision (minor revision) in npj Climate and Atmospheric Science) employed risk-based EA using large-ensemble RCM (20km grid spacings) simulations and showed that the probability of "the Heavy Rain Event of July 2018" in Japan was increased from 0.22% to 2.00% due to anthropogenic warming. - Imada, Y., H. Kawase, M. Watanabe, M. Arai, H. Shiogama, and I. Takayabu: Advanced risk-based event attribution for heavy regional rainfall events. npj Climate and Atmospheric Science, in revision. [Yukiko Imada, Japan]	Accepted. Imada et al. (2020) and the related sentence is added.
86311	116	38	116	42	Suggest seeking graphic support to make the lower panel the figure look sleeker [TSU WGI, France]	Taken into account. Box 11.4, Figure 2 is reproduced.
11119	117	1	142	4	In Table 11.4 - 11.9, some attributions of extrem pricipitation and drought are weak or not informative. For precipitation extremes, the shift from light to heavy precipitation over eastern China (Table 11.5 for east Asia), or the increase in extreme precipitation with warming (Table 11.7 for America) are not really attribution. The attribution of drought refers to climate change (e.g., Table 11.4 for north Africa and west africa, Table 11.6 for central Europe and Mediterrean) or drought conditions (Table 11.5, for southease Asia), or Decrease of dry years (Table 11.6, for north Europe). Those attributions are not really meaningful. In addition, when there is no evidence, I think there is no need to say that there is insufficient evidence to attribute observed trends and events for extreme precipitation (Table 11.4), or there is no or little evidence is found (Table 11.5 and Table 11.9). [Wen Wang, China]	Not applicable. All of section 11.8.3 has been removed.
11123	117	1	142	4	In Table 11.4 - 11.9, there is a general issue that hydrological droughts are almost not mentioned in any region in Table 11.4 to Table 11.9 althought hydrological drouhts are briefly described in section 11.6.3.4. Maybe the cause of this issue is the lack of literature. [Wen Wang, China]	Not applicable. All of section 11.8.3 has been removed.
110037	117	7	117	8	This needs updating to reflect the WG1 assessed change which is 1.1 (and will likely be higher still in FGD) [Peter Thorne, Ireland]	Noted. The text was modified to be consistent with SR1.5.
13811	117	8	117	8	Change SR15 by SR1.5 [Maria Amparo Martinez Arroyo, Mexico]	Accepted.
62721	117	9	117	11	This study also found that events similar to the 2018 May-July temperature extremes would approximately occur 2 out of 3 years under +1.5°C global warming, and every year under +2°C of global warming (Box 11.3, Figure 4). This line should be written as This study also found that events similar to the temperature extremes of May-July 2018 would approximately occur 2 out of 3 years under +1.5°C global warming, and every year under +2°C of global warming (Box 11.3, Figure 4). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted.
62723	117	12	117	12	extremely hot days in Japan. Please replace extremely hot days with extreme hot days [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45693	117	24	117	26	"the implications of that science for their decision-making." Arguably the message in this fragment is too prescriptive. Would you consider changing it to, e.g., "the real implications of any decision they might take" or "the real implications of their decisions"? Alternatively, one might think of "the real fundaments of their decision-making"? In the following sentences, you might want to speak of a variety of stakeholders. [Christophe Deissenberg, Luxembourg]	Rejected. It seems that this comment is not for this section.
44407	117	40	117	40	please replace the word "risk" with "concern". See IPCC guidance on risk for appropriate use of the term "risk" throughoput the IPCC report. [Jana Sillmann, Norway]	Accepted.
43445	117	41			Read " (Zampieri et al., 2017; Kornhuber et al., 2020)" rather than " (Zampieri et al., 2017;(Kornhuber et al., 2020)" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted.
14633	117	47	142	2	None of the projections un Table 11.4-11.9 indicate the scenario or time horizon that is relevant for the stated projections. [Roshanka Ranasinghe, Netherlands]	Considered: The projection tables are restructured to provide future projections corresponding to three global warming levels: 1.5, 2, and 4°C above the pre-industrial.
39327	117	47	142	2	Please consider improving how the findings are presented in each of the regional tables. For example, present the findings, followed by the uncertainty language. What was the rationale in the way the table is structured?. [Lourdes Tibig, Philippines]	Considered. The tables have been significantly revised such that the evidence and rationales are provided to support the assessment (the use of particular uncertainty language).
110039	117	47			While a useful reference resource I am unconvinced that these tables belong in the main text. It is hard to envisage how these would be laid out and it is presumably the case that mnmost people will want to electronically search through them. I therefore wonder whether they should instead be in an annex. [Peter Thorne, Ireland]	Considered. Both a very short form and longer form of tables are produced.
102573	117	49	117	54	As also stated in the general comments document: What about arctic/cold/polar regions? [Philippe Tulkens, Belgium]	Taken into account but we just have Russian Artic (RAR) in Asia Table and Greenland/Iceland (GIC) in Europe Table.
112837	117	49	142	2	Great table that will be very useful -- but note some potential overlaps with ch12 -- important to guide the reader of the whole report well in what to find where. [Maarten van Aalst, Netherlands]	Considered. This issue is resolved with Chapter 12 such that Chapter 12 starts from the assessment of these tables.
109391	117	50	117	52	Suggest including a Small Islands table to be consistent with Ch12/Atlas/WG II (and maybe a Polar table) or an explanation of why not. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Considered. Assessment for small islands are not consolidated in a Rejected. in Atlas.
42335	117		142		The tables overlap with CH12 tables which describe CIDs by region. These tables often have less references than in CH12, so the proposed process is to move narratives and references from CH12 to these tables. This will be up to LA groups in the CH11-12 harmonization process. [robert vautard, France]	Not applicable. All of section 11.8.3 has been removed.
71547	118	1	118	1	Inconsistent with assessment of trends in CDD in 11.6 in West Africa. Again in page 119 [Sergio Vicente-Serrano, Spain]	Accepted. Assessment has been updated
72123	118	1	121	43	Table 11.4 needs to be re-worked in collaboration with Chap12 lead authors. There are discrepancies in some African regions. [Mouhamadou Sylla, Rwanda]	Accepted. Regional assessments have been harmonized with regional chapters
126125	118	1	142	1	[CONFIDENCE] For Tables 11.4-11.9, general principles need to be observed to establish high confidence in a projection: (1) robust agreement of projections among models, (2) good physical understanding of the processes involved in the change, (3) at least medium confidence that a detectable anthropogenic signal has already been observed, and (4) consistency between model historical run trends and observed trends (or if inconsistent, that the observed trend and historical run trends are in the same direction and the observed trend is larger than the historical run trend (not smaller)). What is needed for medium confidence in detection and attribution of anthropogenic influence? Detection cannot be a linear trend alone, but established through demonstration that a trend is highly unusual compared to expected trends, as simulated in long control runs. [Trigg Talley, United States of America]	Take into account. The tables are restructured with introductory texts to explain the rational of the assessments.
130567	118	1	142	2	Table 11.4-11.9 are bit too long. Can we consider to reduce a bit? [Panmao Zhai, China]	Accepted: The tables were completely restructured.
51641	118	1	142	2	These are very useful and clear tables, thank you. Is it posisble to (robustly) draw out differences in outcomes for difference scenarios for some of these types of events and/or understand differences in timing for the projections? That would be very useful and policy-relevant information. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Considered: The projection tables are restructured to provide future projections corresponding to three global warming levels: 1.5, 2, and 4°C above the pre-industrial.
126127	118	1			Table 11.4. North Africa (S.MED) Temperature Extremes, Detection and Attribution; event attribution box. Can add here that Knutson et al. (2013) find detectable anthropogenic warming (mean temperature over parts of this region that have coverage; their Figure 11, 1951-2010 period). Although this is not for extreme temperatures, in the absence of analysis of extremes, the mean temperature D&A analysis is of some use. This can help support the high confidence rating in the projection. Citation: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709-8743, https://doi.org/10.1175/JCLI-D-12-00567.1 [Trigg Talley, United States of America]	Noted. But it is difficult to assess Knustson's results in the context of these regional tables as the paper is about attribution of changes in mean temperature and the regions are also very different.
126129	118	1			Table 11.4. North Africa (S.MED) Droughts, dryness, and aridity; Detection and attribution; event attribution box. This is the general region where GPCC data vs. CMIP5 models indicate detectable anthropogenic decreases in annual precipitation (Knutson and Zeng, 2018), which supports Bergaoui et al. (2015). [Trigg Talley, United States of America]	Accepted
107427	118	3	118	4	There are many studies assesing temperature trends, precipitation deficits ans drought in North Africa (Mediterranean region), especially, in arid and semi-arid regions. Forcez contributing to these trends are also determined by many authors. [Rachda Berrached, Algeria]	Accepted. New papers have been added
107429	118	3	118	4	Climate changes are not the only factors affecting dryines and aridity. [Rachda Berrached, Algeria]	Not applicable. Table has been changed
11121	118	3	121	1	In regional assessments for Africa, the assessment of drought too heavily relies on CDD. [Wen Wang, China]	Taken into account. Drought assessment is now based in meteorological, hydrological, agricultural and ecological drought indices
62379	118	4	118	4	In the main heading of the table it is better to write Abbreviations with details "Precipitation extremes and flooding (including effects of TC, ETC and atmospheric rivers) " [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. The tables are reorganized with more information in the table caption.
55177	118	4	142	1	This set of Tables represents an extraordinary compilation. In contrast to Tables 11.1 and 11.2 (which we commented on earlier), this set of Tables contains citations to the underlying literature that provide a certain level of traceability and support (albeit limited to citations with no discussion or analysis, which would be impossible in such a tabular synthesis). While the level of ambition is impressive, as is the comprehensive nature of this 25 page(!) long table, it is very difficulty for a reviewer (even a technical expert) to comprehensively review all of these entries. It is also unclear how the confidence assessment for all these hundreds of entries was made and what the mechanism for insuring consistencey/calibration across the assessments. While we are not entirely convinced of the value of such an attempt at comprhensiveness, we would strongly urge that at the very least a few sentences be added on pg. 118 to explain how the table entries were created, what the systematic approach to confidence assessment was, etc. [Nancy Hamzawi, Canada]	Accepted: the approach/procedures are now described in the opening subsections of 11.9 for extreme temperature, extreme precipitation and drought

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45695	118	16	118	31	This paragraph is very important from a public awareness/policy-making viewpoint. However, it is formulated in a somewhat hermetic and arguably unnecessarily convoluted way, and it is difficult to recognize the exact message he wants to convey. It appears to conflate two distinct issues, the extrapolation of an existing trend or cycle and the attribution of phenomena to to human-induced climate change. The links between short- and long-term are confusing. Here a suggestion to make it more accessible to non-climatologist stakeholders, trying to follow closely my understanding of the existing text – although a deeper reformulation might be appropriate. "The observations at a global, local, and regional scale provide empirical evidence of a changing climate. Issues of quality, representativity, and consistency of these observations with other lines of evidence are illustrated at different places in this assessment report (see for example the comparison of various observational datasets in the regional climate change assessments in Section Atlas.5). These issues significantly constrain their use for attributing trends to human activity (see Section Atlas.5). Whenever such an attribution can be made, however, it can be extremely useful for policymakers, since one often can reasonably expect that the trends will continue if the human drivers of climate change remain unchanged. Thus, teasing out the role of human-induced global warming in weather trends helps understand which risks are likely to be increasing in the longer term, without being distracted by short-term natural fluctuations.//Trend extrapolation, which assumes that recent and historical trends will continue, produces large forecast errors if discontinuities occur within the projected time period. Thus, the extrapolation of trends attributed to human activity is mostly relevant when information is needed only for the near future. This is the case for example with sea level rise, where locally observed trends are extrapolated to the near future to assist planning of beach nourishment programs and other short-term adaptation interventions. (Daron, 2015; Baart et al., 2018). However, even a short-term projection of future conditions based on currently observed trends is not always justified, as a large internal variability at decadal time scales can easily be mistaken for a systematic human impact on the likelihood of extreme events. In that case the extrapolation of trends cannot be expected to be a reliable estimator for the future (Schiermeier, 25 2018)." [Christophe Deissenberg, Luxembourg]	The approach/procedures are now described in the opening subsections of 11.9 for extreme temperature, extreme precipitation and drought.
45697	118	36	118	36	"are a basic repository of many" or "provide a data base for many"? [Christophe Deissenberg, Luxembourg]	Not applicable. it's not referring to this section
45699	118	43	118	43	Please check the use of "uses". ==> Some work on? [Christophe Deissenberg, Luxembourg]	Not applicable. it's not referring to this section
45701	118	43	118	51	The logical link between the first two and the remaining sentences is obscure. [Christophe Deissenberg, Luxembourg]	Not applicable. it's not referring to this section
62799	118		118		Table 11.4. Specific comment for Precipitation extremes and Flooding, Observed trends (S.MED). Please check the Low Confidence attribution, which might be "Medium Confidence" instead, according page 11-54, lines 37-38. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. The confidence level is recalibrated accoridng to evidence in the FGD.
62797	118		121		Table 11.4. General comment. Please consider the use of the Low Confidence assessment if "Insufficient evidence to assess/to attribute observed trends and events" is stated. According to Tables 11-5 to 11-9, no confidence assessment was include if the evidence was insufficient. Instead, the cells were left in blank. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	This is taken into account when reformatting the tables.
13815	118		121		Change events. by events [Maria Amparo Martinez Arroyo, Mexico]	Not applicable. Tables have been updated
40557	118		142		No mention of temperature levels in the projection columns of these tables or historical time periods in the observation columns or attribution columns. If this is intentional, it warrants explanation in the text. [TSU WGI, France]	Considered: The projection tables are restructured to provide future projections corresponding to three global warming levels: 1.5, 2, and 4°C above the pre-industrial.
62831	118		142		General Comment for Tables 11-4 to 11-9 regarding the observed trends (column) in Droughts, dryness and aridity. Observed trends on Droughts, dryness and aridity only includes the Precipitation deficits section (11.6.2.1). Please include data from sections 11.6.2.2 (Atmospheric and evaporative demand), 11.6.2.3 (Soil moisture deficits) and 11.6.2.4 (hydrological deficits), as well. Moreover, data from section 11.6.2.5 (Combined synthetic measures of droughts) are described only for North Africa (Table 11.4, S.MED). Please complete this data in the respective tables 11-4 to 11-9 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: The final tables contain three types of drought: Meteorological Droughts (MET); Agricultural and Ecological Droughts (AGR/ECOL) and Hydrological Droughts (HYDR)
62833	118		142		General Comment for Tables 11-4 to 11-9 regarding Detection and attribution; event atributtion (column) for Droughts, dryness and aridity. Please define in the whole 11-4 to 11-9 tables if only the positive attributions to anthropogenic signal are going to be listed for. Or else, if all the attributive results (positive, negative and inconclusive results) are going to be listed, instead. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considered. The attribution column now provides one assessment taking evidence from detection and attribution studies and event attribution studies.
126131	119	1			Table 11.4. Sahara (SAH) Temperature Extremes, Detection and Attribution; event attribution box. Knutson et al. (2013) find detectable anthropogenic increases in mean temperature over the part of this region with adequate coverage for their trend analysis (Figure 11, 1951-2010 period). Although this is not for extreme temperatures, in the absence of analysis of extremes, the mean temperature D&A analysis is of some use. This can help support the high confidence rating in the projection. Citation: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709-8743, https://doi.org/10.1175/JCLI-D-12-00567.1 [Trigg Talley, United States of America]	Noted. But it is difficult to assess Knustson's results in the context of these regional tables as the paper is about attribution of changes in mean temperature and the regions are also very different.
126133	119	1			Table 11.4. West Africa (WAF) Temperature Extremes, Detection and Attribution; event attribution box. Knutson et al. (2013) find detectable anthropogenic increases in mean temperature over the part of this region with adequate coverage for their trend analysis (Figure 11, 1951-2010 period). Although this is not for extreme temperatures, in the absence of analysis of extremes, the mean temperature D&A analysis is of some use. This can help support the high confidence rating in the projection. Citation: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709-8743, https://doi.org/10.1175/JCLI-D-12-00567.1 [Trigg Talley, United States of America]	Noted. But it is difficult to assess Knustson's results in the context of these regional tables as the paper is about attribution of changes in mean temperature and the regions are also very different.
126135	119	1			Table 11.4. West Africa (WAF) Droughts, dryness, and aridity; Projections. Knutson and Zeng (2018) conclude there is detectable anthropogenic decrease in precipitation near the region where Klutse et al. project increases in CDDs (Figure 3), so some support from observations vs. historical runs. [Trigg Talley, United States of America]	Accepted. The suggested paper has been added
45703	119	15	119	15	It would be useful to provide some information of the kind of bottom-up approaches that could be envisaged [Christophe Deissenberg, Luxembourg]	Not applicable. it's not referring to this section
45705	119	46	119	51	The paragraph's last two sentences arguably belong at the end of the previous paragraph. Please consider deleting "it may now be argued that" [Christophe Deissenberg, Luxembourg]	Not applicable. it's not referred to this section
40319	119		119		For WAF: "late onset of the rainy season" -> "later onset of the rainy season"? [TSU WGI, France]	Not applicable. Table has been updated and expression has been removed
13813	119		119		Change 1.5C and 2C by 1.5°C and 2°C [Maria Amparo Martinez Arroyo, Mexico]	Accepted
23985	119				For this table (and analogous tables for other regions): How have multiple lines of evidence been assessed (e.g. as suggested in Ch10), and how is that assessment demonstrated? For example, which of the studies listed in these tables are GCMs and which are RCMs? Do any use large ensembles? Do GCM and RCM findings support each other or not? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Considered. The approach/procedures are now described in the opening subsections of 11.9 for extreme temperature, extreme precipitation and drought

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
126137	120	1			Table 11.4. Central Africa (CAF) Temperature Extremes, Detection and Attribution; event attribution box. Knutson et al. (2013) find detectable anthropogenic increases in mean temperature in at least part of this region with adequate coverage for their trend analysis. Part of the region has no detection, part has not enough data for their trend analysis and part has detectable and attributable anthropogenic warming (Figure 11, 1951-2010 period). Although this is not for extreme temperatures, in the absence of analysis of extremes, the mean temperature D&A analysis is of some use. This helps support the high confidence rating in the projection. Citation: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709-8743, https://doi.org/10.1175/JCLI-D-12-00567.1 [Trigg Talley, United States of America]	Noted. But it is difficult to assess Knustson's results in the context of these regional tables as the paper is about attribution of changes in mean temperature and the regions are also very different.
126139	120	1			Table 11.4. Central Africa (CAF) Droughts, dryness, and aridity; Detection and attribution; event attribution box. In this region Knutson and Zeng (2018) trend analysis (1901-2010, GPCC data vs. CMIP5 models) finds detectable anthropogenic decreases in precipitation in some grid boxes suggesting possible emergence of a signal, though coverage is relatively limited. Little signal was found in the dry season, consistent with Otto et al. for the Congo Basin. Still, annual precipitation seems to have some tentative drying signal worth including here. [Trigg Talley, United States of America]	Accepted. Reference has been considered
126141	120	1			Table 11.4. NEAF and CEAF. Temperature Extremes, Detection and Attribution; event attribution box. Knutson et al. (2013) find detectable anthropogenic increases in mean temperature in at least part of this region with adequate coverage for their trend analysis. Part of the region has no detection, part has not enough data for their trend analysis and part has detectable and attributable anthropogenic warming (Figure 11, 1951-2010 period). Although this is not for extreme temperatures, in the absence of analysis of extremes, the mean temperature D&A analysis is of some use. This helps support the high confidence rating in the projection. Citation: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709-8743, https://doi.org/10.1175/JCLI-D-12-00567.1 [Trigg Talley, United States of America]	Noted. But it is difficult to assess Knustson's results in the context of these regional tables as the paper is about attribution of changes in mean temperature and the regions are also very different.
126143	120	1			Table 11.4. NEAF and CEAF. Droughts, dryness, and aridity; Detection and attribution; event attribution box. While the cited studies find that observed drying is not attributable to anthropogenic warming, these studies are for limited subregions within these two region. Within this overall region, and especially in parts of Sudan, Knutson and Zeng (2018) trend analysis (Figure 3, 1901-2010, GPCC data vs. CMIP5 models) finds detectable anthropogenic decreases in precipitation in some grid boxes suggesting possible emergence of a signal at least in that subregion. The observed drying trends here are much stronger than in the CMIP5 historical runs. Note this is not the same subregion as Rowell's East African Climate Paradox or Otto et al., Somalian analysis. Phillip et al. was for Ethiopia. Uhe et al. was for Kenya. So different subregions can explain some of the "differences" in findings. [Trigg Talley, United States of America]	Accepted. Assessment has been updated
126145	120	1			Table 11.4. SWAF. Temperature Extremes, Detection and Attribution; event attribution box. Knutson et al. (2013) find detectable anthropogenic increases in mean temperature over most of this region. See their Figure 11, 1951-2010 period. Although this is not for extreme temperatures, in the absence of analysis of extremes, the mean temperature D&A analysis is of some use. This helps support the high confidence rating in the projection. Citation: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709-8743, https://doi.org/10.1175/JCLI-D-12-00567.1 [Trigg Talley, United States of America]	Noted. But it is difficult to assess Knustson's results in the context of these regional tables as the paper is about attribution of changes in mean temperature and the regions are also very different.
126147	120	1			Table 11.4. SEAF. Temperature Extremes, Detection and Attribution; event attribution box. Knutson et al. (2013) find detectable anthropogenic increases in mean temperature over most of this region. See their Figure 11, 1951-2010 period. Although this is not for extreme temperatures, in the absence of analysis of extremes, the mean temperature D&A analysis is of some use. This helps support the high confidence rating in the projection. Citation: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709-8743, https://doi.org/10.1175/JCLI-D-12-00567.1 [Trigg Talley, United States of America]	Noted. But it is difficult to assess Knustson's results in the context of these regional tables as the paper is about attribution of changes in mean temperature and the regions are also very different.
126149	120	1			Table 11.4. SWAF. Droughts, dryness, and aridity; Detection and attribution; event attribution box and Projections box. As to the Otto et al. study, this only applies to a very small subregion within this region. As shown by trend analysis in the AR5 and by Knutson and Zeng (2018), most of this region has only non-detectable or nonsignificant trends. Parts of it have positive precipitation trends. Similarly, the high confidence in likely projected increases in dryness should be re-examined. Confidence in drying here is overstated based on the very limited evidence for significant large-scale decreasing precipitation trends in the region (with some nominal historical increasing trends, though not significant). Historical runs show a strong drying trend since 1901, but observed trend results are more mixed. The model-obs comparisons are better for the 1951-2010 period but the 1901-2010 period comparisons suggest caution with accepting model projections (especially with high confidence ... that is probably too confident). [Trigg Talley, United States of America]	Accepted for Attribution box and not applicable for projection. Projection boxes have been changed
126151	120	1			Table 11.4. SEAF. Droughts, dryness, and aridity; Detection and attribution; event attribution box and Projections box. As to the Bellprat et al. study, this study does not present evidence for a statistically significant decreasing precipitation trend in the region. They just show a "loose consistency" with model historical runs, though that is not done in detail. As shown by regional precipitation trend maps in AR5 and by Knutson and Zeng (2018), most of this region has only non-detectable or nonsignificant trends since 1901. Historical runs show a strong drying trend since 1901, but observed trend results are more mixed. The model-obs comparisons are better for the 1951-2010 period but the 1901-2010 period comparisons suggest caution with accepting model projections (especially with high confidence ... that is probably too confident). [Trigg Talley, United States of America]	Accepted. Assessments have been updated
45707	120	26	120	26	Arguably socio-economic pathways are not a SOURCE of uncertainty. Socio-economic factors are. [Christophe Deissenberg, Luxembourg]	Not applicable. It's not referring to this section
62801	120		120		Table 11.4. Specific comment for observed trend in temperature extremes (NEAF and CEAF): Please provide the references for the following statement "Medium Confidence: Increases in frequency of warm days (TX90P). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
62803	120		120		Table 11.4. Specific comment for projections in temperature extremes (NEAF and CEAF): Please provide the references for the following statement "High confidence: Likely increases in frequency of warm days (TX90P) and decreases in frequency of cold days (TX10P)" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Boxes for projections have been changed
62805	120		120		Table 11.4. Specific comment for Droughts, dryness and aridity; detection and attribution (NEAF and CEAF): Please consider the "Low Confidence" assesment for the high evidence and medium agreement regarding the no attribution of drying to anthropogenic climate. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Assessments of observed drought and attribution have been changed
40293	120		120		For NEAF and CEAF: "high evidence that drying..." -> High evidence is not IPCC uncertainty language (could say robust evidence) [TSU WGI, France]	Accepted
52659	121	1	121	1	Include observed trends from Kruger and Nxumalo 2017 and Mackellar et al 2014 for Southern Africa [Mary-Jane Bopape, South Africa]	Taken into account. We included only Kruger and Nxumalo 2017 and other relevant literature
66357	121		121		Table 11.4 conflicting confidence on projection for N. Africa, WAF, CAF drought, and NEAF, CEAF, SWAF, SEAF extreme precipitation compared to CH12 [Erika Coppola, Italy]	Accepted. Regional assessments have been harmonized across regional chapters

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
62807	121		121		Table 11.4. Specific comment for projections in precipitation extremes (SWAF): Please, review the following phrase: "High confidence: increases in precipitation intensity ...", in the concordance with the statement from 11-64, Line 5: "However, over western South Africa, heavy rainfalls amount are projected to decrease." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Declined. The high confidence in projected changes is for the western part of the country of South Africa not for the region
62809	121		121		Table 11.4. Specific comment for Droughts, dryness and aridity; detection and attribution (SWAF): Please complete the phrase "Recent meteorological drought" with the corresponding years (2015-2017) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
62811	121		121		Table 11.4. Specific comment for Droughts, dryness and aridity; detection and attribution (SWAF): Please improve the chapter citation for "Ch. 17-18: (Herring et al., 2018)" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. References have been changed
62813	121		121		Table 11.4. Specific comment for Droughts, dryness and aridity; detection and attribution (SEAF): Please provide the references for the following statement "Medium Confidence: increase in dryness (CDD)" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. References have been provided
40309	121		121		For SWAF and SEAF: "Increases in heavy precipitation..." -> Do you mean for frequency or intensity? [TSU WGI, France]	Not applicable. We use "intensification of heavy precipitation " for the whole tables
13817	121		121		South West Africa results are the same as South East Africa, is this possible? [Maria Amparo Martinez Arroyo, Mexico]	not applicable. Tables have been changed
45709	122	1	122	3	illustrates the GCM-RCM inconsistency for European surface temperature projections at the end of the 21st century in summer under the RCP8.5 scenario between the CMIP5 GCM and CORDEX RCM ensembles. illustrates the inconsistency between the CMIP5 GCM and CORDEX RCM ensembles for European summer surface temperature projections at the end of the 21st century un-der the RCP8.5 scenario. [Christophe Deissenberg, Luxembourg]	0
33089	122	1	122	55	Add “Iran plateau” in column 1 of table11.5 with the following information Temperature: Observed trends: High confidence: Increase in frequency and magnitude of warm extremes, increase in frequency and severity of cold extremes Detection and attribution; event attribution: Projections: High confidence: Increase in the frequency and magnitude of warm extremes and increase in frequency and severity of cold extremes Precipitation extremes and flooding: Observed trends: High confidence: Increase in frequency and magnitude of extreme precipitation Detection and attribution; event attribution: High confidence: Intensified precipitation events in 2019 Khozestan, Lorestan, SistanBalochestan Projections: High confidence: Changes in extreme precipitation are for much of the region, though in decrease in precipitation is projected for most of regions especially on Zagros and North West of Iran plateau. Droughts, dryness and aridity: Observed trends: High confidence: Increase in drought conditions due to decreased rainfall and increased dry days Detection and attribution; event attribution: Increase in drought conditions, in most of Iran Plateau Projections: High confidence: Increase in drought conditions Climatology Research Institute reports https://cri.ac.ir/index.php/fa/, National Drought Warning and Monitoring Center (NDWMC) reports http://ndc.irimo.ir/eng/index.php , Zeyaeyan et al 2017 https://doi.org/10.3390/cli5020033, Mansouri Daneshvar 2019 https://doi.org/10.1186/s40068-019-0135-3 and a lot of papers exist for this issue. [Sahar Tajbakhsh Mosalman, Iran]	Rejected: The text and the structure of the tables were completely changed
32759	122	1	122	55	Add “Iran plateau” in column 1 of table11.5 with the following information Temperature: Observed trends: High confidence: Increase in frequency and magnitude of warm extremes, increase in frequency and severity of cold extremes Detection and attribution; event attribution: Projections: High confidence: Increase in the frequency and magnitude of warm extremes and increase in frequency and severity of cold extremes Precipitation extremes and flooding: Observed trends: High confidence: Increase in frequency and magnitude of extreme precipitation Detection and attribution; event attribution: High confidence: Intensified precipitation events in 2019 Khozestan, Lorestan, SistanBalochestan Projections: High confidence: Changes in extreme precipitation are for much of the region, though in decrease in precipitation is projected for most of regions especially on Zagros and North West of Iran plateau. Droughts, dryness and aridity: Observed trends: High confidence: Increase in drought conditions due to decreased rainfall and increased dry days Detection and attribution; event attribution: Increase in drought conditions, in most of Iran Plateau Projections: High confidence: Increase in drought conditions Climatology Research Institute reports https://cri.ac.ir/index.php/fa/, National Drought Warning and Monitoring Center (NDWMC) reports http://ndc.irimo.ir/eng/index.php , Zeyaeyan et al 2017 https://doi.org/10.3390/cli5020033, Mansouri Daneshvar 2019 https://doi.org/10.1186/s40068-019-0135-3 and a lot of papers exist for this issue. [sadegh zeyaeyan, Iran]	Rejected. The table considers Arabian Peninsula as one region and does not assess Iran plateau as a separate region.
126153	122	1			Table 11.5. ARP region. Temperature Extremes, Detection and Attribution; event attribution box. Knutson et al. (2013, Figures 10, 11) show very mixed D&A results here even for mean temperature: some nondetection and a few gridpoints with detectable increases, no cooling trends, and some of the region without sufficient data for trend analysis. Although this is not for extreme temperatures, in the absence of analysis of extremes, the mean temperature D&A analysis is of some use. This helps support the high confidence rating in the projection. Citation: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709-8743, https://doi.org/10.1175/JCLI-D-12-00567.1 [Trigg Talley, United States of America]	Noted. But it is difficult to assess Knustson's results in the context of these regional tables as the paper is about attribution of changes in mean temperature and the regions are also very different.
126155	122	1			Table 11.5. WCA region. Temperature Extremes, Detection and Attribution; event attribution box. Knutson et al. (2013, Figures 10, 11) show mostly detectable and attributable waming here for mean temperature, with a few gridboxes without detectable trends. Although this is not for extreme temperatures, in the absence of analysis of extremes, the mean temperature D&A analysis is of some use. This helps support the high confidence rating in the projection. Citation: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709-8743, https://doi.org/10.1175/JCLI-D-12-00567.1 [Trigg Talley, United States of America]	Noted. But it is difficult to assess Knustson's results in the context of these regional tables as the paper is about attribution of changes in mean temperature and the regions are also very different.
11125	122	3	122	3	Table 11.5: Regional assessments for Asia, for East Asia, there are many more research results about droughts in China. When talking about "Since the 1950s some regions of China have experienced a trend to more intense and longer droughts", the "some regions" here should be more specific, mostly aglong a strip extending from southwest China to the western part of northeast China (see Chapter 12, section 12.4.2.2). Maybe the explanation by Wang et al. (Theoretical and Applied Climatology, 2020, 139:1–15) for drought attribution in southwest China is better than Qin et al. (2015a) who attribute the drought to less precipitation defict and high temperature. Wang et al. showed that Atlantic Oscillation (AO) and Sunspots (SS) are the two most important influencing factors for the variations in dryness/wetness over SW China. [Wen Wang, China]	Accepted: Wang et al. (2020) was included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19457	122	4	271	9	Extreme events in West Asia and Central Asia, such as recent flood (in Iran) or dust and storm and frequent drought should be considered and highlighted in the report. [Mostafa Jafari, Iran]	Accepted: suggestion has been considered
33091	122	4	271	9	extreme events in west asia and central asia , such as recent flood,(in IRAN) and dust and storm and frequent drought should be considered and highlighted in the report. [Sahar Tajbakhsh Mosalman, Iran]	Accepted: suggestion has been considered
32761	122	4	271	9	extreme events in west asia and central asia , such as recent flood,(in IRAN) and dust and storm and frequent drought should be considered and highlighted in the report. [sadegh zeyaeyan, Iran]	Accepted: suggestion has been considered
66359	122		122		Table 11.5 conflicting confidence on projection for ARP, WCA, RFE, RAR (not following AR6 region naming), WSB, ESB, TIB, SAS, EAS, SEA drought and WCA and RFE for extreme precipitation compared to CH12 [Erika Coppola, Italy]	Accepted: The text and the structure of the tables were completely changed
23249	122		122		table11.5: Regional Assessments for Asia, column 1 from left, after Arabian peninsula add:+ IRAN platue, [Hamideh Dalaei, Iran]	Declined: the addition wouldl be inconsistent with AR6 region naming
23251	122		122		table11.5: Regional Assessments for Asia, column 2 from left,Observed Trends Temperature, and column4,Projection,and colum 5,6,7 precipitation extreme and flooding and colum 8,10 add" http://ndc.irimo.ir/ at refrense. And add " Dalaei,et al,2017" [Hamideh Dalaei, Iran]	Rejected: Dalaei et al. (2017) was not inserted because it was unavailable
62815	122		126		Table 11.5. In the Projections for Temperature extremes column, check the spelling of "(...) frequency and severity of cold EXREMES" (ARP, WCA, RFE, ESB, WSB, RAR, TIB, SAS and SEA) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	0
86305	122		134		Please fill in the blank entries in the tables [TSU WGI, France]	Accepted: The tables were completely restructured. They are showing evidences and the uncertainty.
40641	122		134		Please fill in the blank entries in the tables or explain why they are blank [TSU WGI, France]	Accepted: The tables were completely restructured. They are showing evidences and the uncertainty.
126157	123	1			Table 11.5. RFE region (note this is assumed to be the NEA region on the maps in the draft report). Temperature Extremes, Detection and Attribution; event attribution box. Knutson et al. (2013, Figures 10, 11) show mostly detectable and attributable warming here for mean temperature. Although this is not for extreme temperatures, in the absence of analysis of extremes, the mean temperature D&A analysis is of some use. This helps support the high confidence rating in the projection. Citation: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709-8743, https://doi.org/10.1175/JCLI-D-12-00567.1 [Trigg Talley, United States of America]	Noted. But it is difficult to assess Knustson's results in the context of these regional tables as the paper is about attribution of changes in mean temperature and the regions are also very different.
126159	123	1			Table 11.5. RFE region (note this is assumed to be the NEA region on the maps in the draft report). Droughts, dryness, and aridity; Detection and attribution; event attribution box. Knutson and Zeng (2018, Figure 3) infer detectable anthropogenic increases in precipitation in scattered subparts of the region with sufficient data to attempt a trend analysis over 1901-2010. The signal is at least consistent across the scattered high latitude regions. [Trigg Talley, United States of America]	Accepted: Knutson and Zeng (2018) was included in the table
126161	123	1			Table 11.5. ESB region. Temperature Extremes, Detection and Attribution; event attribution box. Knutson et al. (2013, Figure 11) show mostly detectable and attributable warming here for mean temperature (1951-2010). Although this is not for extreme temperatures, in the absence of analysis of extremes, the mean temperature D&A analysis is of some use. This helps support the high confidence rating in the projection. Citation: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709-8743, https://doi.org/10.1175/JCLI-D-12-00567.1 [Trigg Talley, United States of America]	Rejected: Knutson et al. (2013) was not included in the table
126163	123	1			Table 11.5. ESB region. Droughts, dryness, and aridity; Detection and attribution; event attribution box. Knutson and Zeng (2018, Figure 3) infer detectable anthropogenic increases in precipitation in scattered subparts of the region with sufficient data to attempt a trend analysis over 1901-2010. The signal is at least consistent across the scattered high latitude regions. [Trigg Talley, United States of America]	Accepted: Knutson and Zeng (2018) was included in the table
126165	123	1			Table 11.5. WSB region. Droughts, dryness, and aridity; Detection and attribution; event attribution box. Knutson and Zeng (2018, Figure 4 for 1951-2010 period) infer detectable anthropogenic increases in precipitation in this region. [Trigg Talley, United States of America]	Accepted: Knutson and Zeng (2018) was included in the table
45711	123	9	123	11	The sentence is ambiguous. Is the impossibility to assess due only to the too large number products or to the nature of the products? [Christophe Deissenberg, Luxembourg]	0
45713	123	17	123	20	The sentence is very hard to understand precisely [Christophe Deissenberg, Luxembourg]	0
126167	124	1			Table 11.5. RAR region. Temperature Extremes, Detection and Attribution; event attribution box. Knutson et al. (2013, Figure 11) show some detectable and attributable warming here for mean temperature (1951-2010), but some non-detection in the western part of the region. Although this is not for extreme temperatures, in the absence of analysis of extremes, the mean temperature D&A analysis is of some use. This helps support the high confidence rating in the projection. Citation: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709-8743, https://doi.org/10.1175/JCLI-D-12-00567.1 [Trigg Talley, United States of America]	Noted. But it is difficult to assess Knustson's results in the context of these regional tables as the paper is about attribution of changes in mean temperature and the regions are also very different.
126169	124	1			Table 11.5. RAR region. Droughts, dryness, and aridity; Detection and attribution; event attribution box. Knutson and Zeng (2018, Figures 3 and 4 for 1901-2010 and 1951-2010, respectively) infer detectable anthropogenic increases in precipitation at scattered points across this region, especially since 1901. In a few cases there were drying trends, inconsistent with model historical runs. [Trigg Talley, United States of America]	Considered. But it is hard to point to scattered points as a basis for the assessment. The paper is not cited in this context.
126171	124	1			Table 11.5. TIB region. Droughts, dryness, and aridity; Detection and attribution; event attribution box. Knutson and Zeng (2018, Figure 4 for 1951-2010 period) find limited data over most of the region, except the northern and eastern extremes, where there have been detectable and attributable precipitation increases since 1951, consistent with observed reduced drought occurrence. [Trigg Talley, United States of America]	Considered. But it is hard to point to scattered points as a basis for the assessment. The paper is not cited in this context.
9239	124	4	124	4	Table 11.5: Regional assessment for Asia Russian-Arctic (RAR), Precipitation extremes and flooding, Projections Sillmann et al., 2013b; --> Sillmann et al., 2013b; Kusunoki et al. 2015 [Shoji Kusunoki, Japan]	Noted. But the comment is difficult to understand
40317	124		124		For TIB: "Decrease in drought occurrence and severity based on other metrics ... but drought frequency" -> Does 'drought occurrence' mean the same thing as 'drought frequency'? Suggest to stick to one term to avoid confusion. [TSU WGI, France]	Accepted
24497	125	0	126	0	Please refer case studies of annual and daily precipitation in Japan and Korea to cover the East Asia. [Nobuhito Mori, Japan]	0
126173	125	1			Table 11.5. SAS Region. Temperature Extremes, Detection and Attribution; event attribution box. Knutson et al. (2013, Figures 10, 11) and Knutson and Ploshay (2016, Figure 5) show some detectable and attributable warming here for mean temperature. Although these studies are not for extreme temperatures, in the absence of analysis of extremes, these mean temperature and wet bulb globe temperature D&A analyses are of some use. This helps support the high confidence rating in the projection. Citations: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709-8743, https://doi.org/10.1175/JCLI-D-12-00567.1. Knutson, Thomas R., and Jeff J Ploshay, 2016: Detection of anthropogenic influence on a summertime heat stress index. Climatic Change, 138(1-2), DOI:10.1007/s10584-016-1708-z. [Trigg Talley, United States of America]	Noted. But it is difficult to assess Knustson's results in the context of these regional tables as the paper is about attribution of changes in mean temperature and the regions are also very different.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
126175	125	1			Table 11.5. SAS Region. Droughts, dryness, and aridity; Detection and attribution; event attribution box. Knutson and Zeng (2018, Figures 3 and 4 for 1901-2010 and 1951-2010, respectively) infer only limited evidence for detectable anthropogenic decreases in precipitation in this region. Those areas with detectable anthropogenic decrease include Sri Lanka and parts of NE India. Pakistan shows some drying trends, but these are not simulated in the historical runs and so are not attributable to anthropogenic influence based on the Knutson and Zeng analysis. [Trigg Talley, United States of America]	0
126177	125	1			Table 11.5. EAS Region. Droughts, dryness, and aridity; Detection and attribution; event attribution box. Knutson and Zeng (2018, Figures 3 and 4 for 1901-2010 and 1951-2010 respectively) infer only limited evidence for detectable anthropogenic decreases in precipitation in this region since 1901. These support the other mentioned studies. Trends since 1951 are mostly nondetectable in the region. [Trigg Talley, United States of America]	0
62381	125	2	125	5	[Table 11.5: Regional assessments for Asia] There are some recent publications need to cite, which can help to emulate recent climatic modelling (Future Temperature and Precipitation Projections/scenarios) research and projections for all the table including Table "South Asia (SAS)". Here are the titles: 1) Assessing the potentials of digitalization as a tool for climate change adaptation and sustainable development in urban centres, Sustainable Cities and Society, Vol. 53, 101888 2) Vulnerability Assessment of Urban Expansion and Modelling Green Spaces to Build Heat Waves Risk Resiliency in Karachi. International Journal of Disaster Risk Reduction, Vol. 46, 101468 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	0
40285	125		125		For EAS: "There is evidence that the droughts have changed..." -> Increased or decreased? [TSU WGI, France]	Taken into account: Texts inside the tables were changed
62817	125		125		Table 11.5. In the Projections for Temperature extremes column (SAS region), please include the "Medium Confidence" assessment for the sentence "more intense heatwaves of longer duration…". Assessment statement is referred in page 11-49, lines 44-46. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
40299	125		125		For SAS: "...though increases are weaker at the end of the century (Mishra et al., 2014b)" -> Under what scenario? [TSU WGI, France]	Considered. The relevant parts are revised.
38157	125		126		Relevant studies for Korea are not included in the East Asia (EAS) sections. Adding below studies and others would help strengthen the conclusions: [Temperature extremes] Kim Y.-H., S.-K. Min, D. A. Stone, H. Shiogama, and P. Wolski, 2018: Multi-model event attribution of the summer 2013 heat wave in Korea. Weather Clim. Extrem., 20, 33-44, doi: 10.1016/j.wace.2018.03.004 Min, S.-K., Y.-H. Kim, I.-H. Park, D. Lee, S. Sparrow, D. Wallom, and D. Stone, 2019: Anthropogenic contribution to the 2017 earliest summer onset in South Korea. Bull. Amer. Meteor. Soc., 100, S73-S77, doi: 10.1175/BAMS-D-18-0096.1. [Precipitation extremes] Ahn, J.-B., S. Jo, M.-S. Suh, D.-H. Cha, D.-K. Lee, S.-Y. Hong, S.-K. Min, S.-C. Park, H.-S. Kang, K.-M. Shim, 2016: Changes of precipitation extremes over South Korea projected by the 5 RCMs under RCP scenarios. Asia-Pacific J. Atmos. Sci., 52, 223-236. Kim G., D.-H. Cha, C. Park, G. Lee, C.-S. Jin, D.-K. Lee, M.-S. Suh, J.-B. Ahn, S.-K. Min, S.-Y. Hong, and H.-S. Kang, 2018: Future changes in extreme precipitation indices over Korea. Int. J. Climatol., S1, e862-e874, doi: 10.1002/joc.5414. [Droughts] Choi, Y.-W., J.-B. Ahn, M.-S. Suh, D.-H. Cha, D.-K. Lee, S.-Y. Hong, S.-K. Min, S.-C. Park, H.-S. Kang, 2016: Future changes in drought characteristics over South Korea using multi regional climate models with the standardized precipitation index. Asia-Pacific J. Atmos. Sci., 52, 209-222. [Junhee Lee, Republic of Korea]	Accepted: Kim et al. (2018) was included in the table
68505	125				"East Asia" - "Temperature extremes" - "Detection and attribution; event attribution": Imada et al. (2014) also showed anthropogenic influences on extreme temperature over Japan with a high confidence. - Imada, Y., H. Shiogama, M. Watanabe, M. Mori, M. Kimoto, and M. Ishii, 2014: The Contribution of anthropogenic forcing to the Japanese heat waves of 2013. Bull. Amer. Meteor. Soc., 95, S52-S54. [Yukiko Imada, Japan]	Accepted: Imada et al. (2014) was included in the table
68507	125				"East Asia" - "Temperature extremes" - "Projections": Imada et al. (2019, SOLA, already in References) also estimated increase by 1.4 times and 1.8 times in the frequency of extremely hot days over Japan under 1.5 and 2.0 warming, respectively. [Yukiko Imada, Japan]	Accepted: Imada et al. (2019) was included in the table
68509	125				"East Asia" - "Precipitation extremes and flooding" - "Detection and attribution; event attribution": Kawase et al. (2019a, b) and Imada et al. (in revision) also showed anthropogenic influences on extreme rainfall events in Japan. - Kawase, H., Imada, Y., Tsuguti, H., Nakaegawa, T., Seinino, N., Murata, A., et al. (2019). The heavy rain event of July 2019 in Japan enhanced by historical warming. Bull Am Meteorol Soc. doi:10.1175/BAMS-D-19-0173.1. - Kawase, H., Y. Imada, H. Sasaki, T. Nakaegawa, A. Murata, M. Nosaka, and I. Takayabu, 2019: Contribution of historical global warming to local-scale heavy precipitation in western Japan estimated by large ensemble high-resolution simulations. J. Geophys. Res., 124, 6093-6103. - Imada, Y., H. Kawase, M. Watanabe, M. Arai, H. Shiogama, and I. Takayabu: Advanced risk-based event attribution for heavy regional rainfall events. npj Climate and Atmospheric Science, in revision. [Yukiko Imada, Japan]	Accepted: Kawase et al. (2019) was included in the table
23987	125				For this and other similar tables, there must be a re-design to ensure that column headings appear on all pages. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: Tables were completely restructured
23993	125				For the South Asia assessment, Ch11 may wish to assess the work of Hunt et al. (2019, in review): The impacts of climate change on the winter water cycle of the western Himalaya. K. M. R. Hunt, A. G. Turner and L. C. Shaffrey, Climate Dynamics, submitted, in which pseudo-global warming approaches have been used to analyse current and future behaviour of western disturbances in the Himalayas in 40 case studies. The finding is of much greater precipitation associated with such events in future. There are also related changes from snowfall to rainfall in these events, with likely conequences for the seasonality of water resource availability. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: Hunt el al. (2019) was not included
76859	126	0	126	0	Worth noting the findings from the 2nd National Climate Change Study - number of days with heavy rainfall will increase in most parts of SEA and indications of increase in intensity. Reference: Marzin, C., R. Rahmat, D. Berni, L. Bricheno, E. Buonomo, D. Calvert, H. Cannaby, S. Chan, M. Chattopadhyay, W. K. Cheong, et al. (2015) Singapore's Second National Climate Change Study – Phase 1 [Sandeep Sahany, Singapore]	Accepted: Marzin et al. (2015) is included in the text

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
197	126	1	126	1	Table 11.5: Regional assessments for Asia East Asia (EAS)-Precipitation extremes and flooding-projections Ohba and Sugimoto (2020, CD) also show the increase in extreme snowfall over central Japan and northern Japan in future climate projections. Please consider my proposal to add the following reference. Ohba, M., and S. Sugimoto 2020: Impacts of climate change on heavy wet snowfall in Japan, Climate Dynamics, 54, 3151–3164. doi:10.1007/s00382-020-05163-z. https://link.springer.com/article/10.1007/s00382-020-05163-z [Masamichi Ohba, Japan]	Accepted: Ohba and Sugimoto (2020) is included in the text
126179	126	1			Table 11.5. SEA Region. Temperature Extremes, Detection and Attribution; event attribution box. Knutson et al. (2013, Figures 10, 11) and Knutson and Ploshay (2016, Figure 5) show some detectable and attributable warming here for mean temperature. Although these studies are not for extreme temperatures, in the absence of analysis of extremes, these mean temperature and wet bulb globe temperature D&A analyses are of some use. This helps support the high confidence rating in the projection. Citation: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709-8743, https://doi.org/10.1175/JCLI-D-12-00567.1. Knutson, Thomas R., and Jeff J Ploshay, 2016: Detection of anthropogenic influence on a summertime heat stress index. Climatic Change, 138(1-2), DOI:10.1007/s10584-016-1708-z. [Trigg Talley, United States of America]	Noted. But it is difficult to assess Knustson's results in the context of these regional tables as the paper is about attribution of changes in mean temperature and the regions are also very different.
126181	126	1			Table 11.5. SEA Region. Droughts, dryness, and aridity; Detection and attribution; event attribution box. Knutson and Zeng (2018, Figure 3 for 1901-2010 period) infer a mixture of detectable anthropogenic increases and decreases across the region. In the Phillippines are increases in some regions; in western Indonesia and near the Indian Ocean are decreases. [Trigg Talley, United States of America]	Noted.
62819	126		126		Table 11.5. Specific comment for Droughts, dryness and aridity; detection and attribution (SEA): Please complete the phrase "Low confidence: Increase in drought conditions attributable…". Maybe, attributable to anthropogenic signal? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
24517	126		126		Hatsuzuka et al. (2020) could be a piece to be fit here as a study showing future increase in tropical cyclone induced rainfall across Japan. Hatsuzuka, D., T. Sato, K. Yoshida, M. Ishii, R. Mizuta, 2020: Regional projection of tropical-cyclone-induced extreme precipitation around Japan based on large ensemble simulations, SOLA, 16, 23-29, DOI:10.2151/sola.2020-005 [Tomonori Sato, Japan]	Accepted: Hatsuzuka et al. (2020) is included in the text
89609	126				I suggest this additional reference on projection of precipitation extremes in Southeast Asia: Supari et al. 2020. Multi-model projections of precipitation extremes in Southeast Asia based on CORDEX-Southeast Asia simulations. Env. Res., 184. doi: 10.1016/j.envres.2020.109350 [Faye Abigail Cruz, Philippines]	Accepted: Supari et al. (2020) is included in the text
89611	126				I suggest this additional reference on projection of precipitation extremes in Vietnam: Trinh-Tuan, L. et al. 2019. Application of Quantile Mapping Bias Correction for Mid-Future Precipitation Projections over Vietnam. SOLA, 15, 1-6. doi: 10.2151/sola.2019-001 [Faye Abigail Cruz, Philippines]	Accepted: Trinh-Tuan et al. (2019) is included in the text
126183	127	1			Table 11.6. NAU region. Droughts, dryness, and aridity; Detection and attribution; event attribution box. Knutson and Zeng (2018, Figures 3 and 4 for 1901-2010 and 1951-2010, respectively) infer detectable anthropogenic increases in precipitation in this region, especially since 1951. These results are qualitatively consistent with the observed decrease in drought over NW Australia (Gallant et al., 2013). The observed increases are larger than simulated in the CMIP5 historical runs. [Trigg Talley, United States of America]	Rejected. After considering all the evidence, including the papers by Gallant et al. (2013) and by Knutson and Zeng (2018), we opted for a medium confidence on a decrease in meteorological drought but low confidence on the attribution of this decrease partly due to strong differences across the region.
66361	127		127		Table 11.6 conflicting confidence on projection for SAU drought and NAU, SAU, for extreme precipitation. Eastern Australia (EAU) not an official AR6 region compared to CH12. [Erika Coppola, Italy]	Noted. Calibrated language has been agreed with Chapter 12 lead authors.
62821	127		127		Table 11.6. Specific comment for Droughts, dryness and aridity; detection and attribution (NAU). If no evidence has been found to sustain the Low Confidence statement, I would suggest to left the space in blank. See also comment number 5 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. See Section 9.1: "Low confidence is assessed when there is limited evidence, either because of a lack of available data in the region and/or a lack of relevant studies."
9209	128	1	128	1	CAU: replace "the central Australia trends in extreme precipitation" with "central Australia, trends in extreme precipitation". Regarding projections for extreme precipitation, I suggest changing "low confidence" to "high confidence", deleting "but agreement among models is low" and deleting "Evans et al (2017) which only applies to south-east Australia. As indicated above, section 7.2.2 of CSIRO and BoM (2015) Climate change in Australia Technical Report at www.climatechangeinaustralia.gov.au/publications says "extreme rainfall events are projected to increase in intensity (high confidence)". Projected changes in RX1-day and RX1-day-RV20 are provided for RCP4.5 and RCP8.5 for 2081-2099 relative to 1985-2005 for 4 regions (including central Australia, covering Australia, based on 21-24 CMIP5 GCMs. Alexander and Arblaster analysed 22 CMIP5 GCMs and found “most intense precipitation extremes increase substantially, with a separation becoming clear between emissions scenarios … Projected changes in precipitation extremes show increases in the most intense rainfall events across most of the country, though with very few regions of significant and consistent change across the models… Future changes in precipitation extremes are less consistent across the models and most regions show little significant and robust change over the 21stC”. However, Fig 16 and Table 7 in A&A (2017) show robust increases in RX5-day. Increases are also robust in RX1-day-RV20 (CSIRO and BoM, 2015). The key message is that extreme precipitation increases while heavy precipitation (e.g. RX1-day) shows mixed results. [Kevin Hennessy, Australia]	Taken into account. We have included these comments in the assessment.
82821	128	1	129	1	It would be useful to give timespans for the trends quoted in observed SAU temperature extremes to reconcile what would otherwise be apparently contradictory findings on extreme cold events. [Blair Trewin, Australia]	Noted. All trends refer to the period since 1950 to the present.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9211	128	2	128	2	EAU: regarding observed trends in extreme precipitation, add "mixed trends in various heavy rainfall indices (Alexander and Arblaster 2017)", based on AWAP trend maps in their Figure 9. Regarding projections for extreme precipitation, I suggest changing "low confidence" to "high confidence", and deleting "but agreement among models is low". As indicated above, section 7.2.2 of CSIRO and BoM (2015) Climate change in Australia Technical Report at www.climatechangeinaustralia.gov.au/publications says "extreme rainfall events are projected to increase in intensity (high confidence)". Projected changes in RX1-day and RX1-day-RV20 are provided for RCP4.5 and RCP8.5 for 2081-2099 relative to 1985-2005 for 4 regions (including central Australia, covering Australia, based on 21-24 CMIP5 GCMs. Alexander and Arblaster analysed 22 CMIP5 GCMs and found “most intense precipitation extremes increase substantially, with a separation becoming clear between emissions scenarios … Projected changes in precipitation extremes show increases in the most intense rainfall events across most of the country, though with very few regions of significant and consistent change across the models... Future changes in precipitation extremes are less consistent across the models and most regions show little significant and robust change over the 21stC”. However, Fig 16 and Table 7 in A&A (2017) show robust increases in RX5-day. Evans et al (2017) state "Across a range of metrics, robust increases in the magnitude of precipitation extreme indices are found", based on downscaling 4 GCMs over southeast Australia. Increases are also robust in RX1-day-RV20 (CSIRO and BoM, 2015). The key message is that extreme precipitation increases while heavy precipitation (e.g. RX1-day) shows mixed results. [Kevin Hennessy, Australia]	Taken into account. We have included these comments in the assessment.
9213	128	3	128	3	SAU: regarding observed trends in extreme precipitation, add "mixed trends in various heavy rainfall indices (Alexander and Arblaster 2017)", based on AWAP trend maps in their Figure 9. Regarding projections for extreme precipitation, I suggest changing "low confidence" to "high confidence", deleting "but agreement among models is low" and deleting "Robust decrease in ETCs in winter in the Australian east coast based on GCMs and RCMs (Dowdy et al., 2013b, 2013a; Ji et al., 2015; Pepler et al., 2016)" because this applies to the east coast, not southern Australia. As indicated above, section 7.2.2 of CSIRO and BoM (2015) Climate change in Australia Technical Report at www.climatechangeinaustralia.gov.au/publications says "extreme rainfall events are projected to increase in intensity (high confidence)". Projected changes in RX1-day and RX1-day-RV20 are provided for RCP4.5 and RCP8.5 for 2081-2099 relative to 1985-2005 for 4 regions (including central Australia, covering Australia, based on 21-24 CMIP5 GCMs. Alexander and Arblaster analysed 22 CMIP5 GCMs and found “most intense precipitation extremes increase substantially, with a separation becoming clear between emissions scenarios … Projected changes in precipitation extremes show increases in the most intense rainfall events across most of the country, though with very few regions of significant and consistent change across the models... Future changes in precipitation extremes are less consistent across the models and most regions show little significant and robust change over the 21stC”. However, Fig 16 and Table 7 in A&A (2017) show robust increases in RX5-day. Evans et al (2017) state "Across a range of metrics, robust increases in the magnitude of precipitation extreme indices are found", based on downscaling 4 GCMs over southeast Australia. Increases are also robust in RX1-day-RV20 (CSIRO and BoM, 2015). The key message is that extreme precipitation increases while heavy precipitation (e.g. RX1-day) shows mixed results. Regarding drought projections, change "medium confidence" to "high confidence", consistent with CSIRO and BoM (2015) section 7.2.3 which states "the time in drought is projected to increase over southern Australia with high confidence". [Kevin Hennessy, Australia]	Taken into account. We have included these comments in the assessment.
39765	128		128		"Increase in trends…" -> Unclear what this means. Can it be rephrased [TSU WGI, France]	Accepted. Text has been rephrased.
40287	128		128		For EAU: "Anthropogenic greenhouse gas influence on extreme rainfall events in eastern Australia is highly uncertain" -> "highly uncertain" isn't IPCC uncertainty language [TSU WGI, France]	Accepted. Text has been rephrased.
62823	128		128		Table 11.6. Specific comment for Eastern Australia (EAU) and Cross Comentary to Figure 1.15 (Chapter 1, 1-180) AR6 Reference Land and Ocean Regions. Eastern Australia (EAU) is not defined or showed in the referred figure and elsewhere [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. EAU is now a new AR6 region.
98189	129	1	129	1	Table 11.6, SAU region. Temperature extremes, event attribution. The following reference can also be cited here: Knutson et al. (2014b). [Note that Knutson et al. 2014a and 2014b are the same paper and listed twice in the references.] [Thomas Knutson, United States of America]	Accepted. This paper is included in the assessment and cited in the Table.
98193	129	1	129	1	Table 11.6, SAU region. Droughts, dryness and aridity section, Detection and attribution; event attribution column. Add here: Medium confidence: Two studies finding detectable decreases in precipitation with some contribution from anthropogenic influences in southwest Australia (Knutson and Zeng, 2018; Delworth and Zeng 2014), along with a single study finding the same for coastal southeast Australia and Tasmania based on 1951-2010 and 1901-2010 trends (Knutson and Zeng 2018) (low confidence). [Thomas Knutson, United States of America]	Taken into account. These papers has been considered in the assessment.
98195	129	1	129	1	Table 1.6, NZE region. Temperature extremes, event attribution. One study showing detectable anthropogenic influence on mean temperatures in New Zealand is Knutson et al. 2013 for 1901-2010 trends (Fig. 10), though not detectable for 1951-2010 trends (Fig. 11). Ref: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709–8743, https://doi.org/10.1175/JCLI-D-12-00567.1. [Thomas Knutson, United States of America]	Rejected. The paper assesses surface mean temperatures and does not provide information on extreme temperatures.
98197	129	1	129	1	Table 1.6, NZE region. Droughts, dryness and aridity section, Detection and attribution; event attribution column. An additional study showing modest evidence for detectable anthropogenic decreases in at least parts of New Zealand is Knutson and Zeng (2018) who found one gridbox for the South Island in GPCC 5x5 deg data with detectable anthropogenic decreases over 1901-2010, and two gridboxes on the North Island with detectable anthropogenic decreases over 1951-2010, the latter qualitatively supporting Harrington et al. conclusion. [Thomas Knutson, United States of America]	Accepted. This paper was included in the assessment. We assessed the attribution of droughts in NZ with low confidence due to important differences between the north and the south.
66711	129	1	129	1	Table 11.6. Additional New Zealand references. Extreme rainfall: Rosier S, Dean S, Stuart S, Carey-Smith T, Black MT, Massey N (2015) Extreme rainfall in early July 2014 in Northland, New Zealand—was there an anthropogenic influence? Bull Am Meteorol Soc 96(12):S136–S140. Droughts: Harrington LJ, Gibson PB, Dean SM, Mitchell D, Rosier SM, Frame DJ (2016) Investigating event-specific drought attribution using self-organizing maps. J Geophys Res Atmos 121(21):12,766–712,780. Droughts and extreme rainfall: D. Frame, S. Rosier, I. Noy, L. Harrington, T. Carey-Smith, S. Sparrow, D. Stone, S. Dean, 2020, Climate change attribution and the economic costs of extreme weather events: a study on damages from extreme rainfall and drought, Climatic Change. https://doi.org/10.1007/s10584-020-02729-y. And on marine heatwaves in the Tasman/nearby, Salinger et al., 2020, Unparalleled coupled ocean-atmosphere summer heatwaves in the New Zealand region: drivers, mechanisms and impacts, Climatic Change, https://link.springer.com/article/10.1007/s10584-020-02730-5 [Dave Frame, New Zealand]	Taken into account. Most of these studies were included in the assessment.
9215	129	32	130	10	NZ: Observed trends in extreme temperature - add "number of frost days (below 0 degrees Celsius) decreased and the number of warm days (over 25 degrees Celsius) increased at around one-third of measured sites over the period 1972–2016 (NZ MfE 2017: http://www.mfe.govt.nz/sites/default/files/media/Environmental%20reporting/our-atmosphere-and-climate-2017.pdf)". Extreme temperature projections - add "Annual frequency of days over 25C increases 40% (RCP2.6) to 100% (RCP8.5) by 2040 and 40% (RCP2.6) to 300% (RCP8.5) by 2090. Annual frost frequency decreases 30% (RCP2.6) to 50% (RCP8.5) by 2040 and 30% (RCP2.6) to 90% (RCP8.5) by 2090 (NZ MfE 2018: http://www.mfe.govt.nz/publications/climate-change/climate-change-projections-new-zealand)". Observed trends in extreme precip - add "No clear evidence that intense rainfall events have changed from 1960-2016 (NZ MfE 2017)". Extreme precip attribution - add "Anthropgenic influence was detected for extreme rainfall in 2014 in Northland (Rosier et al., 2015)". Observed drought trends - add "Since 1972/73, soils at 7 of 30 sites became drier. The 2012–13 drought was one of the most extreme in the previous 41 years (NZ MfE 2017). [Kevin Hennessy, Australia]	Taken into account. We have integrated these comments in the assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
62825	129		129		Table 11.6. Specific comment for Droughts, dryness and aridity; detection and attribution (NZE). It is stated that the dry conditions are more favourable as a result of anthropogenic climatic change (Harrington et al., 2014). However, according to page 11-86, Lines 21-22 not all the studies agreed with that statement. Why including only one of the contrasting statements here? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text has been rephrased.
40301	129		129		For SAU: "Southwest Australia identified as a hot spot for drought risks in the future" -> Please check that use of the term 'risk' is consistent with IPCC usage. If simply you're referring to the physical hazard, then you shouldn't use the term 'risk'. Risk, as defined by the IPCC, also factors in exposure and vulnerability. [TSU WGI, France]	Accepted. We have removed the word "risk".
98199	130	1	130	1	Table 11.6, Western Pacific Islands region. Temperature extremes, D&A section. Knutson et al. (2013) present some evidence for detectable anthropogenic mean warming in most of this region, either for 1901-2010 trends (Fig. 10) or 1951-2010 trends (Fig. 11). Knutson and Ploshay (2016, Fig. 5) find some evidence that wet bulb globe temperatures have detectable anthropogenic increases in some WP island regions (1973-2012 trends). Ref: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709–8743, https://doi.org/10.1175/JCLI-D-12-00567.1. [Thomas Knutson, United States of America]	Not Applicable: This region is not assess here.
98201	130	1	130	1	Table 11.6 CAR Region, Temperature extremes, D&A section. Knutson et al. (2013) present some evidence for detectable anthropogenic mean warming across most of the Caribbean region for 1901-2010 trends (Fig. 10), though some are not detectable for 1951-2010 trends (Fig. 11). Knutson and Ploshay (2016, Fig. 5) conclude that mean summertime wet bulb globe temperatures have detectable anthropogenic increases across most of the Caribbean region (1973-2012 trends). Ref: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709–8743, https://doi.org/10.1175/JCLI-D-12-00567.1. [Thomas Knutson, United States of America]	Rejected: The papers are excellent but in that table we need D&A studies related to extremes.
98203	130	1	130	1	Table 11.6, CAR region, Droughts, dryness, and aridity, D&A section. Knutson and Zeng (2018, Fig. 3,4) report detectable precipitation declines (1901-2010 trends) with some anthropogenic contribution for some regions of the Caribbean, though these are not detectable for the 1951-2010 trend period. [Thomas Knutson, United States of America]	Rejected: The paper is excellent but in that table we need D&A studies related to extremes.
9241	131	4	131	4	Table 11.7: Rregional assessment for Central and South America Caribbean (CAR), Temperature extremes, Projections High confidence: --> High confidence: Increase in the frequency of hot days (Tmax>= 35C) and hot nights (Tmin>=25C) over the Caribbean (Hall et al., 2012). [Shoji Kusunoki, Japan]	Accepted: The paper was included.
9243	131	4	131	4	Table 11.7: Rregional assessment for Central and South America Caribbean (CAR), Precipitation extremes and flooding, Projections Low confidence: --> Low confidence: Increase in SDII over the Caribbean (Hall et al., 2012). [Shoji Kusunoki, Japan]	Accepted: The paper was included, but I think it is the opposite: Based on (Hall et al. 2013): SDII (5-10% decrease). MRI high-resolution AGCM, A1B, (2073-2099)-(1979-2003): SDII: 5-10% decrease
9245	131	4	131	4	Table 11.7: Rregional assessment for Central and South America Caribbean (CAR), Precipitation extremes and flooding, Projections Yang et al., 2018a). --> Yang et al., 2018a). Increae in RX1day, RX5day over Panama (Kusunoki et al., 2018). [Shoji Kusunoki, Japan]	Accepted: The paper was included in SCA. MRI high-resolution AGCM, A1B, (2073-2099)-(1979-2003): SDII: 5-10% decrease
9247	131	4	131	4	Table 11.7: Rregional assessment for Central and South America Caribbean (CAR), Drought, dryness and aridity Yang et al., 2018a). --> Yang et al., 2018a). Increae in CDD over Panama (Kusunoki et al., 2018). [Shoji Kusunoki, Japan]	Accepted: The paper was included in SCA. MRI high-resolution AGCM, A1B, (2073-2099)-(1979-2003): SDII: 5-10% decrease
66363	131		131		Table 11.7 conflicting confidence on projection for CSA, NSA, NES, SAM, SES, SSA , CAR, drought and NSA, NES, NWS, SWS, SAM, SES, CAR for extreme precipitation compared to CH12 [Erika Coppola, Italy]	Taken into account: We checked all the inconsistencies with Chapter 12.
40279	131		131		For CAR: "Increases in CDD over most stations..." -> Are the projections for station locations? [TSU WGI, France]	Yes, in Stennet-Brown et al. (2017) they use an Statistical Downscaling Model (SDSM) to project the change in extremes
40281	131		131		For CAR: "Warmer conditions over the north and cooler conditions over the eastern Caribbean" -> Are you referring to extremes here? [TSU WGI, France]	Yes, Stennet-Brown et al. (2017) use extremes indicators
68523	131		131		The following paper may be cited here: Nakaegawa, T., A. Kitoh, H. Murakami, and S. Kusunoki. Maximum 5-day Rainfall Total and the Maximum Number of Consecutive Dry Days over Central America in the future climate projected by an atmospheric general circulation model with three different horizontal resolutions. Theoretical and Applied Climatology, 116, Issue 1-2, 155-168: Nakaegawa, T., A. Kitoh, S. Kusunoki, H. Murakami, and O. Arakawa. Hydroclimate change over Central America and the Caribbean in a global warming climate projected with 20-km and 60-km mesh MRI atmospheric general circulation models Papers in Meteorology and Geophysics. 65, 15-33. Kusunoki, S., T. Nakaegawa, R. Pinzón, J. S. Galan and J. R. Fábrega, 29: Future precipitation changes over Panama projected with the atmospheric global model MRI-AGCM3.3. Climate Dynamics, [Tosiyuki Nakaegawa, Japan]	Accepted: The paper was included.
40303	131		132		For SCA and CAR: "Massive heat waves projected..." -> Please use more precise and less provocative language. Do you mean large-scale? [TSU WGI, France]	Accepted: The text inside the tables was completely changed.
41115	131		135		Unclear statements in the detection and attribution column. "Increase in extreme precipitation with warming". Do you mean that the increase in extreme precipitation has been attributed to anthropogenic forcing? [TSU WGI, France]	Not Applicable: All columns were modified. This text was erased.
44409	131		142		check the use of the word "risk" in table 11.7. In several places it should be replace with "probability" (or similar), e.g. Anthropogenic forcing has increased the risk of many hot events [Jana Sillmann, Norway]	Accepted: The text inside the tables was completely changed.
3209	132	0	132	0	For the Amazon basin, please include updated information regarding observed rainfall and runoff trend (e.g. Marengo and Espinoza 2016 doi:10.1002/joc.4420.; Espinoza et al., 2019 doi: 10.1007/s00382-018-4462-2; Barichivich et al., 2018, doi:10.1126/sciadv.aat8785.) [Jhan Carlo Espinoza, France]	Accepted: Based on Espinoza et al. (2019), we extracted information about DDF over SAM. Although in Marengo and Espinoza (2016) and Barichivich et al. (2018) it was not possible to quantify the change in extremes over NSA and SAM.
3211	132	0	132	0	For the Andes Cordillera, please include updated information reviewed in Pabón-Caicedo et al (2020 doi: 10.3389/feart.2020.00061) [Jhan Carlo Espinoza, France]	Rejected: The paper is excellent but in that table we need D&A studies related to extremes.
39763	132		134		"Increase in extreme precipitation..." -> Unclear exactly what you mean. Frequency, intensity, total? [TSU WGI, France]	Accepted: The text was changed.

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109913	133	1	133	1	Table 11.7, SWS region, Temperature extremes; D&A section. Detectable mean warming (1901-2010; 1951-2010) with some anthropogenic contribution along most of Chile inferred (Knutson et al. 2013, Fig. 10, 11). Ref: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709–8743, https://doi.org/10.1175/JCLI-D-12-00567.1. [Thomas Knutson, United States of America]	.Not Applicable: The articles are excellent but we need here D&A related to extremes
109915	133	1	133	1	Table 11.7, SWS region, Droughts, dryness, and aridity, D&A section. Knutson and Zeng's results (2018, Fig. 3, 4) suggest there is a detectable decrease in precipitation at least partly due to anthropogenic forcing in parts of Chile. [Thomas Knutson, United States of America]	Not Applicable: The articles are excellent but we need here D&A related to extremes.
108155	133		133		Table 11.7: Regional assessments for Central and South America, Region : South Western South America (SWS) Section, Column: Temperature Extremes, Observed Trends. I suggest adding the following citation: Olmo M, Bettolli ML, Rusticucci M. Atmospheric circulation influence on temperature and precipitation individual and compound daily extreme events: spatial variability and trends over southern South America. Weather and climate extremes. Submitted December 2019. Olmo et al 2019 found significant positive trends for TX90p over SWS region based on station data and CPC gridded data in the period 1979-2015. [Maria Bettolli, Argentina]	Accepted: The paper was included.
29931	133				Why "medium confidence" in the robust drying trend? Most of the available literature shows a declining trend in precipitation (rainfall/snowfall in the Andes) and streamflow [Juan Rivera, Argentina]	Rejected: In Meteorological Drought column we are analysing "Consecutive Dry Days"(CDD). Over SWS we have few stations and mostly showing positive change in CDD but non signif.
84055	134	0	134	0	In the first row (SAM) column 8, we suggest to add Borges at al. 2018 (DOI: 10.1002/joc.5686, who found strong evidence of increase in CDD. [Marco Tulio Cabral, Brazil]	The article is excellent, but we need to include more comprehensive studies, not just one city.
109907	134	1	134	1	Table 11.7, SES region, Temperature extremes; D&A section. Detectable mean warming (1901-2010; 1951-2010) with some anthropogenic contribution inferred (Knutson et al. 2013, Fig. 10, 11); detectable increase in summertime wet bulb globe temperature (1973-2012) with some anthropogenic contribution inferred (Knutson and Ploshay 2016, Fig. 5). Ref: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709–8743, https://doi.org/10.1175/JCLI-D-12-00567.1. [Thomas Knutson, United States of America]	Noted. But it is difficult to assess Knustson's results in the context of these regional tables as the paper is about attribution of changes in mean temperature and the regions are also very different.
109909	134	1	134	1	Table 11.7, SES region, Droughts, dryness, and aridity, D&A section. Knutson and Zeng's (2018, Fig. 3, 4) results suggest there is a detectable increase in precipitation at least partly due to anthropogenic forcing in this region, which spans all seasons except winter (JJA). [Thomas Knutson, United States of America]	Not Applicable: The articles are excellent but we need here D&A related to extremes.
109911	134	1	134	1	Table 11.7, SSA region, Temperature extremes; D&A section. Detectable mean warming (1901-2010; 1951-2010) with some anthropogenic contribution inferred (Knutson et al. 2013, Fig. 10, 11); detectable increase in summertime wet bulb globe temperature (1973-2012) with some anthropogenic contribution inferred (Knutson and Ploshay 2016). Ref: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709–8743, https://doi.org/10.1175/JCLI-D-12-00567.1. [Thomas Knutson, United States of America]	Not Applicable: The articles are excellent but we need here D&A related to extremes.
40305	134		134		For SES: "Anthropogenic climate change has increased the risk of the April-May 2017 extreme rainfall in the Uruguay River basin" Please check that use of the term 'risk' in this is consistent with IPCC usage. If simply you're referring to the physical hazard, then you shouldn't use the term 'risk'. Risk, as defined by the IPCC, also factors in exposure and vulnerability. [TSU WGI, France]	Accepted: All columns were modified.
108157	134		134		Table 11.7: Regional assessments for Central and South America Region : Southeastern South America (SES) Section, Column: Temperature Extremes, Observed Trends. I suggest adding the following citation: Olmo M, Bettolli ML, Rusticucci M. Atmospheric circulation influence on temperature and precipitation individual and compound daily extreme events: spatial variability and trends over southern South America. Weather and climate extremes. Submitted December 2019. Olmo et al 2019 found significant positive trends for TX90p and TN90p and significant negative trends for TX10p over SES region based on station data and CPC gridded data in the period 1979-2015. [Maria Bettolli, Argentina]	Accepted: The paper was included.
108159	134		134		Table 11.7: Regional assessments for Central and South America Region : Southeastern South America (SES) Section, Column: Precipitation extremes and flooding (including effects of TC, ETC and atmospheric rivers), Observed Trends. I suggest adding the following citation: Olmo M, Bettolli ML, Rusticucci M. Atmospheric circulation influence on temperature and precipitation individual and compound daily extreme events: spatial variability and trends over southern South America. Weather and climate extremes. Submitted December 2019. Olmo et al 2019 found significant positive trends in the frequency of days with high precipitation (over the 75th percentile) over SES region based on station data and CPC gridded data in the period 1979-2015. [Maria Bettolli, Argentina]	Accepted: The paper was included.
108161	134		135		Table 11.7: Regional assessments for Central and South America Region : Southern South America (SSA), Column: Temperature Extremes, Observed Trends. I suggest adding the following citation: Olmo M, Bettolli ML, Rusticucci M. Atmospheric circulation influence on temperature and precipitation individual and compound daily extreme events: spatial variability and trends over southern South America. Weather and climate extremes. Submitted December 2019. Olmo et al 2019 found significant positive trends for TX90p and TN90p over SSA region based on station data and CPC gridded data in the period 1979-2015. [Maria Bettolli, Argentina]	Accepted: The paper was included.
108163	134		135		Table 11.7: Regional assessments for Central and South America Region : Southern South America (SSA), Column: Droughts, dryness and aridity, Observed Trends. I suggest adding the following citation: Olmo M, Bettolli ML, Rusticucci M. Atmospheric circulation influence on temperature and precipitation individual and compound daily extreme events: spatial variability and trends over southern South America. Weather and climate extremes. Submitted December 2019. Olmo et al 2019 found significant negative trends in the frequency of days with high precipitation (over the 75th percentile) during the cold season (April to September) over SSA region based on station data and CPC gridded data in the period 1979-2015. [Maria Bettolli, Argentina]	Accepted: The paper was included.
109917	135	1	135	1	Table 11.8, GIC region. Droughts, dryness, and aridity, D&A section. Knutson and Zeng (2018, Fig. 3, 4) results suggest there is a detectable increase in precipitation at least partly due to anthropogenic forcing for parts of Iceland. [Thomas Knutson, United States of America]	Rejected: Drought tables have been entirely reformatted according to different drought types.

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109919	135	1	135	1	Table 11.8, NEU region, Temperature extremes; D&A section. Detectable and partly attributable anthropogenic warming inferred, entire region except Scandinavia, where there is a mix of detectable increases and nondetectable trends (Knutson et al. 2013). Region has inferred detectable and partly attributable anthropogenic increase in summertime mean wet bulb globe temperatures (1973-2012 trend). Ref: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709–8743, https://doi.org/10.1175/JCLI-D-12-00567.1. [Thomas Knutson, United States of America]	Noted. But it is difficult to assess Knustson's results in the context of these regional tables as the paper is about attribution of changes in mean temperature and the regions are also very different.
109921	135	1	135	1	Table 11.8, NEU region. Droughts, dryness, and aridity, D&A section. Knutson and Zeng's (2018, Fig. 3, 4) results suggest there is a detectable increase in precipitation (1901-2010; 1951-2010) at least partly due to anthropogenic forcing for most of the region. [Thomas Knutson, United States of America]	Accepted: Drought tables have been entirely reformatted according to different drought types.
44437	135	1	138	1	Why is in Table 11.8 for Europe no mentioning of "effects of TC, ETC and atmospheric rivers" on precipitation extremes and flooding? The other regional tables contain this in the table heading but not Table 11.8. There is sufficient literature for AR effects on European precipitation and flodding to base an assessment on and I am missing a discussion about it in section 11.7 , e.g. Azad, R. and A. Sorteberg (2017). Extreme daily precipitation in coastal western Norway and the link to atmospheric rivers, J. Geophys. Res. Atmos. 122(4), 2016JD025615. and Benedict, I., K. demark, T. Nipen, and R. Moore (2019). Large-Scale Flow Patterns Associated with Extreme Precipitation and Atmospheric Rivers over Norway, Mon. Wea. Rev. 147(4), 1415–1428. and Whan, K., J. Sillmann, N. Schaller, and R. Haarsma, 2020: Future changes in atmospheric rivers and extreme precipitation in Norway, Climate Dynamics, 54:2071–2084, doi: 10.1007/s00382-019-05099-z [Jana Sillmann, Norway]	rejected: regional tables are not showing mechanisms
9249	135	8	135	8	Table 11.8: Regional assessment for Europe Greenland/Iceland (GIC), Precipitation extremes and flooding, Projections Sillmann et al., 2013b; --> Sillmann et al., 2013b; Kusunoki et al. 2015 [Shoji Kusunoki, Japan]	Accepted: Greenland extremes have been rewritten and new references added.
76693	135	8			Table11.8: Concerning the PROJECTIONS of extreme for the Mediterranean you could add Lionello and Scarascia 2020 (doi: 10.1007/s10113-020-01610-z) which supports the increase of extreme warm temepratures, shows a contrast in future trends of precipitation extremes (increasing in the north and decreasing in the south Med) and increase ad increase of CDD (stronger in the South than in the North) [Piero Lionello, Italy]	Accepted: reference added
66365	135		135		Table 11.8 conflicting confidence on projection for NEU, EEU drought and NEU and MED for extreme precipitatio compared to CH12 [Erika Coppola, Italy]	Rejected: Drought tables have been entirely reformatted according to different drought types.
40295	135		135		For NEU: "No important changes... -> Not the right wording. Could say no significant or no substantive differences. [TSU WGI, France]	Rejected: Drought tables have been entirely reformatted according to different drought types.
109923	136	1	136	1	Table 11.8, CEU region, Temperature extremes; D&A section. Detectable and partly attributable anthropogenic annual mean warming inferred for 1901-2010 and 1951-2010 (Knutson et al. 2013) and similarly for summertime mean wet bulb globe temperatures inferred for 1973-2012 (Knutson and Ploshay 2016. Ref: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709–8743, https://doi.org/10.1175/JCLI-D-12-00567.1. [Thomas Knutson, United States of America]	Accepted: Table completely replaced after SOD, including new assessment on detection and attribution.
109925	136	1	136	1	Table 11.8, CEU region. Droughts, dryness, and aridity, D&A section. Non-detectable trends in mean precipitation for this region generally (Knutson and Zeng 2018). [Thomas Knutson, United States of America]	Accepted: Drought tables have been entirely reformatted according to different drought types.
62827	136		136		Table 11.8. Specific comment for Droughts, dryness and aridity; detection and attribution (CEU). Please, consider the "Medium Confidence" assessment for attribution of the 2017 drought event to climate change. According to section 11.6.4.1, Lines 29-31 for the 2015 drought: "no conclusive evidence whether human-induced climate change was a driver of the rainfall deficit..." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: Drought tables have been entirely reformatted according to different drought types.
40283	136		137		For CEU: "drought projections based on soil moisture and drought indices" -> Doesn't say what the projected changes are. [TSU WGI, France]	Rejected: Drought tables have been entirely reformatted according to different drought types.
109927	137	1	137	1	Table 11.8, MED region, Temperature extremes; D&A section. Detectable and partly attributable anthropogenic annual mean warming inferred for 1901-2010 and 1951-2010 (Knutson et al. 2013, Fig. 10, 11) and similarly for summertime mean wet bulb globe temperatures for 1973-2012 (Knutson and Ploshay 2016, Fig. 5). Ref: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709–8743, https://doi.org/10.1175/JCLI-D-12-00567.1. [Thomas Knutson, United States of America]	Accepted: Table completely replaced after SOD, including new assessment on detection and attribution.
109929	137	1	137	1	Table 11.8 MED region. Droughts, dryness, and aridity, D&A section. Hoerling et al. (2012) conclude there is a detectable anthropogenic decrease in wintertime precipitation for the Mediterranean region. Knutson and Zeng (2018, Fig. 3, 4) results suggest there is a detectable decrease in mean precipitation (1901-2010; 1951-2010) at least partly due to anthropogenic forcing for most of the region; within the Europe sector the decreases are most detectable in this region for south-central and southeastern Europe. Ref: Hoerling, M., J. Eischeid, J. Perlwitz, X. Quan, T. Zhang, and P. Pegion, 2012: On the Increased Frequency of Mediterranean Drought. J. Climate, 25, 2146–2161, https://doi.org/10.1175/JCLI-D-11-00296.1 [Thomas Knutson, United States of America]	Rejected: Drought in the Mediterranean has been revised and attribution has not been detected based on several observation studies and also modelling approaches. This was generated in consensus with different WGI chapters.
40291	137		137		For MED: "Increase of climatic and hydrological droughts based on". By climatic droughts, do you mean meteorological droughts? [TSU WGI, France]	Accepted: Drought tables have been entirely reformatted according to different drought types.
23767	137		137		use 'rain-on-snow' for consistency (instead of rain on snow) [Annett Bartsch, Austria]	Accepted: Replaced
109931	138	1	138	1	Table 11.8, EEU region, Temperature extremes; D&A section. Detectable and partly attributable anthropogenic annual mean warming was inferred for 1901-2010 and 1951-2010 (Knutson et al. 2013, Fig. 10, 11) and similarly for an increase in summertime mean wet bulb globe temperatures over 1973-2012 (Knutson and Ploshay 2016, Fig. 5). Ref: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709–8743, https://doi.org/10.1175/JCLI-D-12-00567.1. [Thomas Knutson, United States of America]	Rejected: Mean warming is not considered in the tables, which focus on extremes.
109933	138	1	138	1	Table 11.8 EEU region. Droughts, dryness, and aridity, D&A section. Knutson and Zeng's (2018, Fig. 3, 4) results suggest there is a detectable increase in mean precipitation (1901-2010) at least partly due to anthropogenic forcing for most of this region. [Thomas Knutson, United States of America]	Accepted: This is included in the reformatted tables.
110655	138	1			Consider to mention in Table 11.8 this paper about heat stress (WBGT) projections in Europe, showing especially critical situations for southern Europe: Casanueva, A., Kotlarski, S., Fischer, A.M. et al. Escalating environmental summer heat exposure—a future threat for the European workforce. Reg Environ Change 20, 40 (2020). https://doi.org/10.1007/s10113-020-01625-6 [Ana Casanueva, Spain]	Rejected: several papers on heat extremes have been included.
40289	138		138		For EEU: "Increase in extreme precipitation". and "Increase in precipitation extremes". -> Vague. What specific aspects change? [TSU WGI, France]	Rejected: Replaced

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
109935	139	1	139	1	Table 11.9, NCA Region, Temperature extremes, D&A section. Detectable and partly attributable anthropogenic annual mean warming was inferred for 1901-2010 (Knutson et al. 2013, Fig. 10) and similarly for an increase in summertime mean wet bulb globe temperatures over 1973-2012 (Knutson and Ploshay 2016, Fig. 5). Ref: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709–8743, https://doi.org/10.1175/JCLI-D-12-00567.1. [Thomas Knutson, United States of America]	Considered. The paper was cited in the SOD and is cited in the FGD of this chapter.
109937	139	1	139	1	Table 11.9, NCA Region. Droughts, dryness, and aridity, D&A section. Knutson and Zeng's (2018, Fig. 3, 4) results suggest that precipitation trends in this region are mostly non-detectable (1901-2010, 1951-2010) except for parts of the region during 1981-2010 where detectable and partly attributable anthropogenic decreases were inferred for some gridpoints in the region. [Thomas Knutson, United States of America]	Considered. This paper is cited.
109939	139	1	139	1	Table 11.9, NWN Region, Temperature extremes, D&A section. Detectable and partly attributable anthropogenic annual mean warming was for most of this region, particularly for 1951-2010 (Knutson et al. 2013, Fig. 11) and similarly for an increase in summertime mean wet bulb globe temperatures over 1973-2012 (Knutson and Ploshay 2016, Fig. 5). Ref: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709–8743, https://doi.org/10.1175/JCLI-D-12-00567.1. [Thomas Knutson, United States of America]	Considered. The paper was cited in the SOD and is cited in the FGD of this chapter.
109941	139	1	139	1	Table 11.9, NWN Region. Droughts, dryness, and aridity, D&A section. Knutson and Zeng's (2018, Fig. 3, 4) results suggest that precipitation trends in this region are mostly non-detectable (1901-2010, 1951-2010) except for parts of the region near the coast during 1951-2010 where detectable and partly attributable anthropogenic increases were inferred for some gridpoints. Substantial negative trends in southwest Canada for 1951-2010 and 1981-2010 were also assessed as non-detectable. [Thomas Knutson, United States of America]	Considered. This paper is cited.
110579	139	4	142	1	It surprises me - given the work that had been done on changes in snow and extremes - that snow is not mentoined in the North America summary table. [Rachel McCrary, United States of America]	Considered but the chapter has to be selective on what to assess to keep the size manageable.
66367	139		139		Table 11.9 conflicting confidence on projection for NCA, CAN, ENA, WNA drought and WNA for extreme precipitation compared to CH12 [Erika Coppola, Italy]	0
40297	139		139		For NWN: "Increased drought risk during summer..." -> Please check that use of the term 'risk' is consistent with IPCC usage. If simply you're referring to the physical hazard, then you shouldn't use the term 'risk'. Risk, as defined by the IPCC, also factors in exposure and vulnerability. [TSU WGI, France]	Considered. The word "risk" is not used in the FGD version
68525	139		139		The following paper may be cited here: Nakaegawa, T., A. Kitoh, H. Murakami, and S. Kusunoki. Maximum 5-day Rainfall Total and the Maximum Number of Consecutive Dry Days over Central America in the future climate projected by an atmospheric general circulation model with three different horizontal resolutions. Theoretical and Applied Climatology, 116, Issue 1-2, 155-168: Nakaegawa, T., A. Kitoh, S. Kusunoki, H. Murakami, and O. Arakawa. Hydroclimate change over Central America and the Caribbean in a global warming climate projected with 20-km and 60-km mesh MRI atmospheric general circulation models Papers in Meteorology and Geophysics. 65, 15-33. Kusunoki, S., T. Nakaegawa, R. Pinzón, J. S. Galan and J. R. Fábrega, 29: Future precipitation changes over Panama projected with the atmospheric global model MRI-AGCM3.4. Climate Dynamics, [Tosiyuki Nakaegawa, Japan]	Considered. This paper is cited.
109943	140	1	140	1	Table 11.9, NEC Region, Temperature extremes, D&A section. Detectable and partly attributable anthropogenic annual mean warming was for some of this region, particularly for 1951-2010 (Knutson et al. 2013, Fig. 11) and for most of the region with available data for summertime mean wet bulb globe temperatures over 1973-2012 (Knutson and Ploshay 2016, Fig. 5). Ref: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709–8743, https://doi.org/10.1175/JCLI-D-12-00567.1. [Thomas Knutson, United States of America]	Considered. The paper was cited in the SOD and is cited in the FGD of this chapter.
109945	140	1	140	1	Table 11.9, NEC Region, Droughts, dryness, and aridity, D&A section. Knutson and Zeng's (2018, Fig. 3, 4) results suggest that precipitation trends for some gridpoints in this region with available data have detectable increases with some contribution from anthropogenic forcing for 1901-2010 trends but not for 1951-2010 trends. [Thomas Knutson, United States of America]	Considered. This paper is cited.
109947	140	1	140	1	Table 11.9, CNA Region. Droughts, dryness, and aridity, D&A section. Knutson and Zeng's (2018, Fig. 3, 4) results suggest that precipitation trends for much of this region have pronounced detectable increases (stronger than modeled) with some contribution from anthropogenic forcing for 1901-2010 trends. Fewer points within the region have detectable increases for 1951-2010 and almost none have detectable increases for 1981-2010. [Thomas Knutson, United States of America]	Considered. This paper is cited.
91059	140	1	140	1	After Wang et al. (2018b), add: Russell et al. (2020) [Richard Smith, United States of America]	Noted. But we were not able to locate Russell et al. (2020) as details about the paper was not provided.
40277	140		142		For CAN, ENA and WNA: "Increase in precipitation extremes". -> Vague. What specific aspects change? [TSU WGI, France]	Considered. Specifics are given now.
109949	141	1	141	1	Table 11.9. ENA Region, Temperature extremes, D&A section. In addition to the Vose et al. (2017) results for the northern region, the warming is not detectable over 1901-2015 for the Southeast U.S. (Vose et al. 2017). and partly attributable anthropogenic annual mean warming was for some of this region, particularly for 1951-2010 (Knutson et al. 2013, Fig. 11) and for most of the region with available data for summertime mean wet bulb globe temperatures over 1973-2012 (Knutson and Ploshay 2016, Fig. 5). Ref: Knutson, T.R., F. Zeng, and A.T. Wittenberg, 2013: Multimodel Assessment of Regional Surface Temperature Trends: CMIP3 and CMIP5 Twentieth-Century Simulations. J. Climate, 26, 8709–8743, https://doi.org/10.1175/JCLI-D-12-00567.1. [Thomas Knutson, United States of America]	Considered. The paper is cited.
109951	141	1	141	1	Table 11.9, ENA Region. Droughts, dryness, and aridity, D&A section. Knutson and Zeng's (2018, Fig. 3, 4) results suggest that precipitation trends for much of this region have pronounced detectable increases (stronger than modeled) with some contribution from anthropogenic forcing for 1901-2010 trends. Some (but fewer) points within the region have detectable increases for 1951-2010 and none have detectable increases for 1981-2010. [Thomas Knutson, United States of America]	Considered. The paper is cited.
109953	141	1	141	1	Table 11.9, ENA Region, Drought, dryness, and aridity, Projections section. The projections of drier conditions (reduced SPI) referred to for spring and summer in this region are not apparent as detectable changes in the observed 20th century trends in this region (though there are nondetectable decreasing trends over 1981-2010 in the southeast and Great Lakes region). Given the lack of detectable long-term decreasing trends in observed precipitation over the region, high confidence in the projection of reduced SPI does not seem justified. [Thomas Knutson, United States of America]	Considered, no action is taken. High confidence in the projection does not require a detection/attribution of past changes.
91061	141	1	141	1	After Wang et al. (2018b) both times, add: Russell et al. (2020) [Richard Smith, United States of America]	Noted. But we were not able to locate Russell et al. (2020) as details about the paper was not provided.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
105975	142	0	142	0	Williams, et al. (2020) also offers strong support for this high-confidence assessment of the potential for chronic, long-duration hydroilogical drought in Western North America over a century timescale. Williams, A. Park, Edward R. Cook, Jason E. Smerdon, Benjamin I. Cook, John T. Abatzoglou, Kasey Bolles, Seung H. Baek, Andrew M. Badger, and Ben Livneh. "Large Contribution from Anthropogenic Warming to an Emerging North American Megadrought." Science 368, no. 6488 (April 17, 2020): 314–18. https://doi.org/10.1126/science.aaz9600. [Sohum Pawar, United States of America]	Considered. The paper is cited.
109955	142	1	142	1	Table 11.9, WNA Region. Droughts, dryness, and aridity, D&A section. Knutson and Zeng's (2018, Fig. 3, 4) results also suggest that there is little evidence for detectable annual mean precipitation trends for this region. For the spring, summer and fall seasons, there are a few gridpoints in the region with detectable decreases with some contribution from anthropogenic forcing inferred, particularly for 1981-2010 trends, but less so for 1901-2010 or 1951-2010 trends. [Thomas Knutson, United States of America]	Considered. Past changes in drought are now carefully assessed considering various literatures for three different drought types.
109957	142	1	142	1	Table 11.9, WNA Region, Drought, dryness, and aridity, Projections section. The projections of drier conditions (reduced SPI) referred to for spring and summer in this region are not apparent as detectable changes in the observed 20th century trends in this region (though there are detectable decreasing trends for a minority of gridpoints over 1981-2010 in this region). Given the lack of detectable long-term decreasing trends in observed precipitation over the region, high confidence in the projection of reduced SPI may not be justified. [Thomas Knutson, United States of America]	Considered. Assessment has lower confidence in FGD.
40321	142		142		For WNA: "Little evidence is found for..." -> Not IPCC uncertainty language [TSU WGI, France]	Considered and reworded.
42337	143	1	145	7	I do not think this box is appropriately framed, as it contains a lot of material that will be updated by WGII in much deeper details and the material largely overlaps with CH12 handshake remits and section 12.3 and 12.5. The box 11.4 Table 1 is interesting, but material before should be cut, or at least extremely simplified and refer to CH12 whereever impacts are mentioned. [robert vautard, France]	Agreed. The material on REF2 is now part of the Cross-Chapter Box in chapter 12.
66507	143	1	145	7	One possibility is that the WGI material is handed to CH12 (Section 12.5), with authors being CAs of CH12 [robert vautard, France]	Agreed. The material on REF2 is now part of the Cross-Chapter Box in chapter 12.
112843	143	1	147	37	really like these boxes -- don't drop them in case of pressure on space! [Maarten van Aalst, Netherlands]	Accepted: Thank you very much
106517	143	1			WGII ch2 "Terrestial and freshwater ecosystems" CCB EXTREMES is highly relevant to Box 11.4. Care should be taken to ensure consistency between WGs in messages and uncertainty assessments of those messages. [camille parmesan, France]	noted
114813	143	1			Rather than making many individual comments on this box I will limit it to one over-arching comment. As I said in my review of the FOD, this box can in principle be useful to the assessment of climate risks, and to the RFCs, by characterizing how the nature of extreme events changes as a function of global warming level. However currently the box primarily assesses impact literature and carries out risk assessment, work that should be (and is being) done in WG2. Also, it does not contain much new assessment of extremes itself, beyond pointing to other places in the chapter or report, and what it does have could be (or maybe already is) covered by Ch 12 (eg, the table contains useful information, but I imagine could easily go elsewhere, or be included in Chapter 12). So my recommendation would be to eliminate the box entirely. Regarding the WG2 overlap, the box carries out its own assessment of literature on impacts or adaptation, including the role of governance, current preparedness for extremes, adaptation defecits, barriers to adaptation, ability to adapt under different SSPs, and risks to health, food security, and ecological systems. On that basis, it draws its own conclusions about risk, including that "changes in extremes lead to high risks for a large number of people, even at low levels of global warming." In this way it pre-empts the WG2 assessment, and indeed will be in conflict with it. In addition, the box seems to be partly aimed at sending a message to WG2 about how it "should" assess risks in the RFCs, asking them to pay attention to D&A of extremes, compound events, the results of SR15 on risk, etc. I don't think we need this reminder, and a WG1 box does not seem like an appropriate place for such communication. [Brian O'Neill, United States of America]	Agreed. The material on REF2 is now part of the Cross-Chapter Box in chapter 12.
13819	143	6	143	6	Change chapter by Chapter [Maria Amparo Martinez Arroyo, Mexico]	noted
11127	143	6	144	46	As the "Reasons for concerns" (RFCs) were included in the AR5 WG2 report, similar staff must be also included in AR6 WG2 report. Should Chapter 11 avoid talking too much about impacts, risks and adaptation as much as possible to avoid overlapping? [Wen Wang, China]	Agreed. The material on REF2 is now part of the Cross-Chapter Box in chapter 12.
1517	143	7	143	7	When "Reasons for Concern" is replaced by RFCs the text removes the concern through using an unfamilar and non-communicating abbreviation. I doubt that is a very smart thing to do. Another aspect is that climate extremes can coincide with non-climatic events that makes the situation more difficult and complicated (e.g. a pandemic, economic collapse or social unrest). [Rasmus Benestad, Norway]	Agreed. The material on REF2 is now part of the Cross-Chapter Box in chapter 12.
44423	143	27	143	27	The word "hazard" needs to be replaced with "climatic impact driver". [Jana Sillmann, Norway]	Agreed. The material on REF2 is now part of the Cross-Chapter Box in chapter 12.
62725	143	36	143	36	Please replace this line "While still limited, there is now new literature available to better estimate the reasons for concerns arising" with "While still limited, there is now new literature available to better estimation of the reasons for concerns arising [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	This material has been removed from the chapter following other review comments.
39699	143	38	143	38	"...have influence on weather pattern and climate of mid-latitudes." -> No mention of the effects of Arctic amplification on mid-latitude weather (Cross-Chapter Box 10.1) in the TS, which has been much discussed in the media since AR5. As noted in Ch11 "There is also low confidence in possible effects of the Arctic warming on mid-latitude temperature extremes". Although this is a low confidence statement, it's important to include in the TS as it opposes the 'high confidence' statements coming out in the media. [TSU WGI, France]	noted
44451	143	48	143	51	This text is WGII material and is not supported by any reference here, thus should be deleted. [Jana Sillmann, Norway]	Agreed. The material on REF2 is now part of the Cross-Chapter Box in chapter 12.
44455	143	53	144	8	If you are dicussing impacts in this RCP-SSP context (which is actually WGII material), you also need to mention that socioeconomic development (e.g. differences between SSP1 and SSP4) would be able to compensate some of the heat-related impacts particularly for lower warming levels, such as 1.5 to 2 degrees as discussed in Russo et al. 2019 https://doi.org/10.1038/s41467-018-08070-4, which should be cited in this paragraph. [Jana Sillmann, Norway]	Agreed. The material on REF2 is now part of the Cross-Chapter Box in chapter 12.
62727	143	54	143	54	"many countries would in 2050 live under a government struggling to provide disaster preparedness and thus" this sentence should be revised as many countries in 2050 will live under a government struggling to provide disaster preparedness and thus [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The material on REF2 is now part of the Cross-Chapter Box in chapter 12.
117127	143		143		I do not think that the box 11.4 is very helpful. Sharpening chapter 1 would be a better option. [Valerie Masson-Delmotte, France]	Agreed. The material on REF2 is now part of the Cross-Chapter Box in chapter 12.
20761	144	3	144	3	Should one read "only" or "even"? [philippe waldteufel, France]	Noted. The material on REF2 is now part of the Cross-Chapter Box in chapter 12.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44453	144	4	144	6	This number (75%) is only supported by one reference. Recently more papers on this topic have been published and should be cited and used to confirm or adjust this number. See for instance: https://iopscience.iop.org/article/10.1088/1748-9326/ab7d04 and https://doi.org/10.1073/pnas.1910114117 [Jana Sillmann, Norway]	Noted. The material on REF2 is now part of the Cross-Chapter Box in chapter 12.
43447	144	12		13	Read "(Mora et al., 2018; Gaupp et al., 2019; Vogel et al. 2019). Box 11.3 shows " rather than "(Mora et al., 2018, Gaupp et al., 2019; Vogel et al. 2019)Box 11.3 shows " [Cyriaque Rufin Nguimalet, Central African Republic]	Noted. The material on REF2 is now part of the Cross-Chapter Box in chapter 12.
13821	144	13	144	13	Change Vogel et al. 2019)Box 11.3 by Vogel et al. 2019). Box 11.3 [Maria Amparo Martinez Arroyo, Mexico]	Noted. The material on REF2 is now part of the Cross-Chapter Box in chapter 12.
62383	144	19	144	24	This paragraph need revision it may cause confusion to the non-technical/policy personals readers " Therefore, in order to estimate whether and at what level of global warming very high risks arising from extremes would occur that could challenges limits to adaptation, the spatial extent of extremes and the potential of compounding extremes need to be assessed. Sections 11.3, 11.7 and 11.8 highlight increasing evidence that temperature extremes, higher intensity precipitation accompanying tropical cyclones, and compound events such as dry/hot conditions conducive to fire or storm surges resulting from sea level rise and heavy precipitation events, pose widespread threats to societies already at relatively low warming levels." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The material on REF2 is now part of the Cross-Chapter Box in chapter 12.
80709	144	25	144	25	'such that' is repeated twice [Helene Jacot Des Combes, Marshall Islands]	Noted. The material on REF2 is now part of the Cross-Chapter Box in chapter 12.
43449	144	25			Read "in the undisturbed world such that these event may not " rather than "in the undisturbed world such that such that these event may not " [Cyriaque Rufin Nguimalet, Central African Republic]	Noted. The material on REF2 is now part of the Cross-Chapter Box in chapter 12.
44457	144	26	144	28	I wonder how meaningful this sentence is, or in particular the numbers given in the sentence. According to Cattiaux and Ribes 2018, the changes in return levels really depend on the definiton of the heat event. [Jana Sillmann, Norway]	Agreed. The material on REF2 is now part of the Cross-Chapter Box in chapter 12. Without specific numbers.
39493	144	32	144	35	Consider to remove the words 'several breadbaskets' and replace it by 'the agriculture sector', since in line 32 'sectors' are mentioned and food security impacts all agricultural subsectors, thus on crops, livestock, forestry, fisheries and aquaculture. The impact of extreme events will be among and within countries and will affect all four dimensions of food security (availability, access, utilization and stability). [Tamara van 't Wout, Qatar]	This material has been removed from the chapter following other review comments.
20763	144	41	144	45	Rather than intended for readers, this sentence seem a message to WG2 authors urging them to read the WG1 report. [philippe waldteufel, France]	Agreed. The material on REF2 is now part of the Cross-Chapter Box in chapter 12.
13823	144	43	144	43	Change 2 by II [Maria Amparo Martinez Arroyo, Mexico]	noted
39329	145	1	145	8	Theseexamples came from single studies? [Lourdes Tibig, Philippines]	Yes. The material on REF2 is now part of the Cross-Chapter Box in chapter 12. Without specific numbers and referring to additional studies.
10075	145	5	145	6	See also Li et al 2020, /10.1088/1748-9326/ab7d04, using large initial conditions ensembles to look at extremes of WBGT as a function of GSAT [Robert Kopp, United States of America]	This material has been removed from the chapter following other review comments and is now in ch12.
11763	145	14	145	14	“small island territories” [Amy East, United States of America]	Not applicable. Box 11.5 was removed after the SOD review. The important information it contained has been incorporated in Cross-Chapter Box Atlas.2 on small islands.
62385	145	14	146	34	In the 11.5: Climate extremes in small islands territories. The authors are suggested to add one case study of flood from South Asia (India, Pakistan) highly vulnerable to seasonal floods during the Monsoon. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Box 11.5 was removed after the SOD review. The important information it contained has been incorporated in Cross-Chapter Box Atlas.2 on small islands.
40545	145	14	147	24	No confidence language in this box. [TSU WGI, France]	Not applicable. Box 11.5 was removed after the SOD review. The important information it contained has been incorporated in Cross-Chapter Box Atlas.2 on small islands.
66889	145	17	146	5	The opening paragraph suggests that floods and droughts are the predominant major climatic impacts, and while these are serious and ongoing, other extreme events—like tropical cyclones, which are only covered in the last paragraph—are equally significant and more difficult to prepare for and should be considered as part of the opening paragraph. The TCs paragraph could also be moved up in the discussion because of the potential impact that they can have on small island states, especially with stronger storms. [Kristin Campbell, United States of America]	Not applicable. Box 11.5 was removed after the SOD review. The important information it contained has been incorporated in Cross-Chapter Box Atlas.2 on small islands.
68463	145	17	146	5	The opening paragraph suggests that floods and droughts are the predominant major climatic impacts, and while these are serious and ongoing, other extreme events—like tropical cyclones, which are only covered in the last paragraph—are equally significant and more difficult to prepare for and should be considered as part of the opening paragraph. The TCs paragraph could also be moved up in the discussion because of the potential impact that they can have on small island states, especially with stronger storms. [Durwood Zaelke, United States of America]	Not applicable. Box 11.5 was removed after the SOD review. The important information it contained has been incorporated in Cross-Chapter Box Atlas.2 on small islands.
66891	146	7	146	20	At least one country of the Pacific (Tuvalu) has no remaining groundwater and depends exclusively on rainwater. Further, many islands have limited access to desalination plants, but increasing numbers of outer-lying islands depend completely on rainwater. Additionally, “plays an additional role” suggests that this contributes to the reduction of freshwater, and could be rephrased to emphasize that pumping increases groundwater salinization, which in some areas is compounded by sea-level rise. [Kristin Campbell, United States of America]	Not applicable. Box 11.5 was removed after the SOD review. The important information it contained has been incorporated in Cross-Chapter Box Atlas.2 on small islands.
68465	146	7	146	20	At least one country of the Pacific (Tuvalu) has no remaining groundwater and depends exclusively on rainwater. Further, many islands have limited access to desalination plants, but increasing numbers of outer-lying islands depend completely on rainwater. Additionally, “plays an additional role” suggests that this contributes to the reduction of freshwater, and could be rephrased to emphasize that pumping increases groundwater salinization, which in some areas is compounded by sea-level rise. [Durwood Zaelke, United States of America]	Not applicable. Box 11.5 was removed after the SOD review. The important information it contained has been incorporated in Cross-Chapter Box Atlas.2 on small islands.
11765	146	11	146	11	a word is missing. Should this be “a 20% decrease”? [Amy East, United States of America]	Not applicable. Box 11.5 was removed after the SOD review. The important information it contained has been incorporated in Cross-Chapter Box Atlas.2 on small islands.
62729	146	11	146	14	Bailey et al. (2016) projected a 20% in 12 groundwater availability by 2050 in Coral Atoll islands of the Federal States of Micronesia, but stressed that 13 under high sea level rise the decrease can be higher than 50% because the intrusion of marine water in the 14 aquifer, as well as drought events, increases the salinity of the freshwater sources. (Please replace this line with Bailey et al. (2016) projected that 20% in groundwater availability will decrease by 2050 in Coral Atoll islands of the Federal States of Micronesia, but stressed that under high sea level rise the decrease can be higher than 50% because the intrusion of marine water in the aquifer, as well as drought events, increases the salinity of the freshwater sources.) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Box 11.5 was removed after the SOD review. The important information it contained has been incorporated in Cross-Chapter Box Atlas.2 on small islands.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39707	146	20	146	20	"...to one event every 6.3 years at the end of the twenty-first century" -> Under what emission scenario or temperature level? [TSU WGI, France]	Not applicable. Box 11.5 was removed after the SOD review. The important information it contained has been incorporated in Cross-Chapter Box Atlas.2 on small islands.
20269	146	30	146	30	Spurious "the" before 90% [philippe waldteufel, France]	Not applicable. Box 11.5 was removed after the SOD review. The important information it contained has been incorporated in Cross-Chapter Box Atlas.2 on small islands.
31645	146	46	146	54	This paragraph refers to different processes: erosion (ref to Albert et al) and flooding during extreme events (Wadley et al, Merrifield et al), saltwater intrusions (Gingerish et al), which all can be gathered under coastal risks. However, there is also the question of chronic (and permanent flooding, probably not relevant to a report on extreme events). These could deserve a word. Additional comment: a review of erosion observations in atoll islands is available in Duvat 2019; Duvat, V. K. (2019). A global assessment of atoll island planform changes over the past decades. Wiley Interdisciplinary Reviews: Climate Change, 10(1), e557 and this topic is also assessed in the SROCC Ch4. [Gonéri Le Cozannet, France]	Not applicable. Box 11.5 was removed after the SOD review. The important information it contained has been incorporated in Cross-Chapter Box Atlas.2 on small islands.
80711	146	51	146	52	For this section I would suggest to include the paper by Storlazzi et al., 2018 on coastal flooding, overwash and island habitability in the Roi-Namur island of the Kwajalein atoll on the Republic of the Marshall Islands. It is a good example of future projections of coastal flooding from a combination of waves and sea level rise [Helene Jacot Des Combes, Marshall Islands]	Not applicable. Box 11.5 was removed after the SOD review. The important information it contained has been incorporated in Cross-Chapter Box Atlas.2 on small islands.
39871	146	54	146	54	"twentieth century" -> Should be 21st century [TSU WGI, France]	Not applicable. Box 11.5 was removed after the SOD review. The important information it contained has been incorporated in Cross-Chapter Box Atlas.2 on small islands.
11767	147	3	147	3	"122-year flood depths" is confusing and awkward terminology: what does this mean? Changes in flood stage (elevation) over a 122-year record? Surely not the flood stage/depth for a "122-year flood" referring to recurrence interval for a certain magnitude event? [Amy East, United States of America]	Not applicable. Box 11.5 was removed after the SOD review. The important information it contained has been incorporated in Cross-Chapter Box Atlas.2 on small islands.
8059	147	7	147	8	Something missing from the sentence(s)? [jouni Räisänen, Finland]	Not applicable. Box 11.5 was removed after the SOD review. The important information it contained has been incorporated in Cross-Chapter Box Atlas.2 on small islands.
39761	147	15	147	23	"In general, TCs are expected to…". -> Can you provide a confidence language for these sentences? [TSU WGI, France]	Not applicable. Box 11.5 was removed after the SOD review. The important information it contained has been incorporated in Cross-Chapter Box Atlas.2 on small islands.
43451	147	19		20	Read " (Vecchi and Soden, 2007; Ting et al., 2019)" rather than " (Vecchi and Soden 2007; Ting et al. 2019)" [Cyriaque Rufin Nguimalet, Central African Republic]	Not applicable. Box 11.5 was removed after the SOD review. The important information it contained has been incorporated in Cross-Chapter Box Atlas.2 on small islands.
43453	147	22			Read "(Kossin et al., 2014; section 11.7.1)" rather than "(Kossin et al. 2014; section 11.7.1)" [Cyriaque Rufin Nguimalet, Central African Republic]	Not applicable. Box 11.5 was removed after the SOD review. The important information it contained has been incorporated in Cross-Chapter Box Atlas.2 on small islands.
69255	147	31	147	37	The section 11.10 "Limits to the assessment" should be elaborated further. As there are many remaining difficulties of projections on extreme events, and it would seem necessary to describe in detail the knowledge gaps to be filled in the AR7. [Kaoru Magosaki, Japan]	Noted. "Limits to the assessment" is not included in FGD.
112845	147	31	147	37	This needs expansion -- expecially being clear about extremes that we still have trouble with, and possibly some indication of whether we're expecting progress in the coming years on these? Maybe also add a few more examples of the sort of tipping points we have limited knowledge about but could be important. [Maarten van Aalst, Netherlands]	Noted. "Limits to the assessment" is not included in FGD.
126185	147	31	147	37	The paragraph on limitations is surprisingly brief. Many areas of uncertainty for the assessment have been given throughout the chapter. Would it be appropriate to summarize them here? [Trigg Talley, United States of America]	Noted. "Limits to the assessment" is not included in FGD.
110043	147	31			Section is clearly grossly incomplete. Several pieces of text in preceding sections may well be more germane here and should be moved accordingly. [Peter Thorne, Ireland]	Considered.
9217	147	33	147	37	Needs much more detail to capture knowledge gaps mentioned throughout the chapter and to be consistent with the Executive Summary on page 9 lines 42-50. There are general uncertainties for all extreme events related to the limited extent and quality of historical data, process understanding, climate model capabilities, attribution studies and agreement/evidence for projections. There are also specific uncertainties for individual types of extreme events, e.g. hail, lightning, tornadoes, tropical cyclones, floods, droughts and fires. [Kevin Hennessy, Australia]	Noted. "Limits to the assessment" is not included in FGD.
20767	147	33	147	37	On page 31 lines 35-38 the insufficient involvement of statisticians was mentioned. Time for a reminder? [philippe waldteufel, France]	Noted. "Limits to the assessment" is not included in FGD.
86307	147	33	147	37	Very short section. Needs expanding. [TSU WGI, France]	Noted. "Limits to the assessment" is not included in FGD.
20765	147	34	147	34	In addition to hail, one might mention lightning [philippe waldteufel, France]	Noted. "Limits to the assessment" is not included in FGD.
39331	147	34	147	37	Limits to the assessments is a very important highlight of the assessments, especially with regards to observational gaps in the most vulnerable regions. Please include Asia inasmuch as one finds the less dense observing networks in some sub-regions. [Lourdes Tibig, Philippines]	Noted. "Limits to the assessment" is not included in FGD.
107431	147	36	147	37	There are many studies. I can send you some of them if you want. [Rachda Berrached, Algeria]	Noted. "Limits to the assessment" is not included in FGD.
80713	147	37	147	37	Based on the limited literature from this region, the Pacific SIDS is also a region with observational gaps but also with projection gaps [Helene Jacot Des Combes, Marshall Islands]	Noted. "Limits to the assessment" is not included in FGD.
11769	147	37	147	37	"several world's regions" is awkward, suggest instead "several regions of the world" [Amy East, United States of America]	Noted. "Limits to the assessment" is not included in FGD.
13825	147	54	147	54	Change RCO 8.5 by RCP8.5 [Maria Amparo Martinez Arroyo, Mexico]	Accepted. This has been corrected.
39333	148	3	149	4	Frequently Asked Questions are meant to convey one, if not the most important discussion in the chapter, and arguably, the one important message about the chapter. I find FAQ as very difficult to summarize. I suggest that the chapter finds an easier to understand answer to the question. [Lourdes Tibig, Philippines]	Considered. The text is largely rewritten focusing on simple comparison between changes in mean and extremes in temperature and precipitation. Discussion on statistical distributions is removed.
62647	148	5	148	8	Sentences are irrelevent and not adding much value in answering the question, and should be removed. For a common reader, the answer should be very specific without introduction. Othwerwise, it may be confusing to a common reader. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Unfortunately, during the edition process a full paragraph went missing.
100499	148	7	148	8	Incomplete sentence. Please change to: 'For near-surface temperature, increases in extreme heat events are expected to be larger in magnitude than the global mean warming' [Wim Thiery, Belgium]	Accepted. Unfortunately, during the edition process a full paragraph went missing.
96149	148	7	148	8	Sentence should be revised, it seems something is missing here. [Nicole Wilke, Germany]	Accepted. Unfortunately, during the edition process a full paragraph went missing.
62895	148	7	148	8	Consider splitting this sentence in 2: " For near-surface temperature, increases in extreme heat events are expected to be larger in magnitude [period] Extremes will occur on the backdrop of global mean warming." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Unfortunately, during the edition process a full paragraph went missing.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
110045	148	7	148	8	As written this sentence makes no logical sense. It feels like some words have been removed which were necessary for it to be a coherent and internally consistent statement. [Peter Thorne, Ireland]	Accepted. Unfortunately, during the edition process a full paragraph went missing.
126187	148	7	148	8	Incomplete or confusing sentence. [Trigg Talley, United States of America]	Accepted. Unfortunately, during the edition process a full paragraph went missing.
62837	148	8	148	8	FAQ 11.1. Please, review the following sentence: "For near-surface temperature, increases in extreme heat events are expected to be larger in magnitude extremes will occur on the backdrop of global mean warming". Maybe split it into two separated sentences? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Unfortunately, during the edition process a full paragraph went missing.
126189	148	8	148	8	Typo? A colon (or rewording) is needed between the words "magnitude" and "extremes". [Trigg Talley, United States of America]	Accepted. Unfortunately, during the edition process a full paragraph went missing.
11771	148	8	148	8	not sure what this was meant to say, one or more words seems to be missing [Amy East, United States of America]	Accepted. Unfortunately, during the edition process a full paragraph went missing.
40869	148	10	148	19	Suggest to expend more words explaining what the PDF is showing in real terms. Also, there's a need to simplify some of the language. Here's an attempt at this: "For a given weather or climate variable (e.g. temperature), we can represent the range of values experienced in a given location using a plot called a probability density function, or PDF. The vertical axis of this plot indicates the likelihood of a given value on the x-axis (e.g., an air temperature) occurring. Surface air temperatures tends to follows a bell shaped curve, with the extreme warm and cold temperatures (at the left and right of the range, respectively), occurring less frequently than the temperatures in the middle of the range (near the mean). As the climate warms, the entire PDF shifts to the right, moving both the mean, as well as extremes, to higher temperatures. Thus, cold extremes occur at milder temperatures while warm extremes reach higher temperatures. In addition to a shift in the distribution, its shape can also change.... etc.". Also, instead of saying "As a result, changes in local mean temperatures can vary greatly across regions and throughout the year, though most land regions warm more than the global average.", you could say "Some regions are warming faster than others, resulting in greater shifts in the PDF. The rate of warming (and thus the shift) can also differ between seasons in a given region." [TSU WGI, France]	Not applicable. This section has been heavily revised.
41207	148	12	148	13	You mention 'daily maximum temperatures', which is more complex example than the complete range of air temperature. [TSU WGI, France]	Not applicable. This section has been heavily revised.
110047	148	13	148	13	By definition they will have so may seems the wrong qualifier here? [Peter Thorne, Ireland]	Considered. The text is largely rewritten focusing on simple comparison between changes in mean and extremes in temperature and precipitation. Discussion on statistical distributions is removed.
13827	148	26	148	26	change ways by ways. [Maria Amparo Martinez Arroyo, Mexico]	Considered. The text is largely rewritten focusing on simple comparison between changes in mean and extremes in temperature and precipitation. Discussion on statistical distributions is removed.
40095	148	28	148	30	Don't observations also show these shifts in PDFs? [TSU WGI, France]	Considered. The text is largely rewritten focusing on simple comparison between changes in mean and extremes in temperature and precipitation. Discussion on statistical distributions is removed.
17707	148	28	148	40	What about the anthropogenic forcing due to land use changes? [Sridhara Nayak, Japan]	Rejected. This discussion is out of the scope of the FAQ.
1519	148	48	148	49	For daily rainfall, we have two types of pdfs: one for dry days (a 'null' pdf with zeros) and one for the rainy days (a gamma or exponential distribution). This reflects the fact that different processes/conditions are present when it rains/doesn't rain. A combined function for the probability can be expressed as the product of the wet-day frequency (f_w) and the cumulative probability function (cpf) for the wet days. A change in both the mean and extreme precipitation anounts may be a result of more rainy days or a change in the pdf/cpf (the cdf is the integal of the pdf). The wet-day mean is closely linked to the atmospheric dynamics whereas the pdf itself is more strongly dependent on the thermodynamic properies of the local atmosphere. [Rasmus Benestad, Norway]	Rejected. This discussion is out of the scope of the FAQ that aims at the general public using a non-technical, simple, language.
110049	148	52	148	54	This is an over-simplification. The water vapour holdinfg capacity increases by 7% but as shown in chapters 2,3,4, and in particular 8 it doesn't follow that the actual content increases by that amount, particularly over land regions. [Peter Thorne, Ireland]	Considered. The text is modified.
40325	149	0			General figure FAQ11.x : the captions should be shortened (e.g. by putting more information on the figure itself) [TSU WGI, France]	Not applicable. The figure is replaced with maps of future changes in mean and extreme temperature and precipitation.
40329	149	0			General FAQ11.x : you need to be careful to avoid overlap between the FAQs [TSU WGI, France]	Noted. All FAQs are heavily edited to avoid overlap.
40097	149	0			due to missing paragraphs, it's hard to follow this FAQs but I would try to get rid of the acronyms and simplify the language, to make the text more accessible [TSU WGI, France]	Accepted.
40119	149	0			FAQ11.1 is sometimes a bit too abstract: for instance it's not clear what determines when you just have a shift or a shift+var [TSU WGI, France]	Not applicable. The figure and caption has changed.
40893	149	0			that's a pity not to have something more explicit on the return period on extreme events (e.g. cf p152 L38-45.) [TSU WGI, France]	Considered. The text is largely rewritten focusing on simple comparison between changes in mean and extremes in temperature and precipitation. Discussion on statistical distributions is removed.
40905	149	0			the classical key message : less cold extreme more hot extreme not that clear in here, is it on purpose? [TSU WGI, France]	Considered. The text is largely rewritten focusing on simple comparison between changes in mean and extremes in temperature and precipitation. Discussion on statistical distributions is removed.
39649	149	0			can you please check the length of FAQ11.1, it should be 650-750 words long [TSU WGI, France]	Accepted. We have revised the length of the FAQ and is now well within the limits.
24495	149	2	149	5	The description of "Annual averaged precipitation will be increased 1-3% per degree Celsius" s something strange because the extreme precipitation will be increased in 6-7% per degree Celsius in page 62. It is generally opposite. [Nobuhito Mori, Japan]	Noted. But it is unclear what the comment is about.
126191	149	23	149	26	High-frequency precipation also changes in distribution toward higher intensity, as well as changing in the overall distribution. Suggest using the LP3 or Gumbel distribution to describe the curve changes as commonly practiced in hydrology. Only shifting of the distribution curve to the higher intensity is overly simplistic, inconsistent with rainfall hydrology. [Trigg Talley, United States of America]	Not applicable. The figure and caption has changed.
40941	149		149		The figure caption is too technically worded and very hard to follow. Also, why does a shift in the mean affect the variability? [TSU WGI, France]	Not applicable. The figure and caption has changed.
40475	150	0			it would be good to have a short conclusion [TSU WGI, France]	Accepted. Conclusion added.
41053	150	0			there is a slight mismatch between the message of the summary and the content of the text. [TSU WGI, France]	Taken into account. In revising the text, the summary has been rewritten.
40049	150	0			Could you be more concrete with the part on events not anticipated? It seems very theoretical at the moment and it's hard to grasp. But this is only important if the it is in the last paragraph, as it would be a pity to finish off on a very vague note. [TSU WGI, France]	Accepted. We have clarified the meaning of unprecedented events.
40121	150	0			FAQ11.2 is very interesting but you need to treat "impacts" with care here, as this topic belongs to WG2 [TSU WGI, France]	Accepted. The discussion of impacts has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40945	150	0			The flow is sometimes a bit hard to follow. I think a clearer structure could improve the flow and help the reader. For instance this: 1) summary 2) introduction - the impact of climate change on extreme event (current L18-24) 3) new extreme event locally +example (e.g. wildfire in Greenland) 4) compound event 5) unprecedented extremes : never seen at all + not anticipated. 6) conclusion and word about increased impact due to exposure and vulnerability [TSU WGI, France]	Accepted. We have rearranged the FAQ text to generally follow this structure.
112847	150	1	150	1	The current FAQ is "did climate change cause that recent extreme". More often, the question is actually: "did climate change cause that recent disaster". It might be good to change it to that, and then start by saying that the disaster is the result of hazard, exposure and vulnerability. The latter two also have an effect on the disaster. But for the hazard part, here is what we can say: ... (and then basically what you have in the current FAQ). [Maarten van Aalst, Netherlands]	Taken into account. For its location in WGI, this FAQ focuses on the extreme event (hazard). However, we have added text to clarify that other factors (exposure, vulnerability) are important for determining the level of disaster.
29283	150	3	150	6	Something about the way the second sentence is worded here seems misleading . . . "Future extreme events will be similar to those experienced in the past . . ." but then the paragraph goes on to explain how future extreme events will, in fact, differ from historical norms. I suggest either removing the phrase above, or clarify. [Andra Garner, United States of America]	Taken into account. The FAQ has been revised with a clearer explanation of how extreme events of the future may be different, even though the event types will generally be the same.
39495	150	11	150	12	Adapated' to be 'adapted' [Tamara van 't Wout, Qatar]	Edited
20271	150	11	150	13	adapated (twice); also on lines 51-53 suprising (twice) [philippe waldteufel, France]	Edited
39497	150	12	150	13	Adapated' to be 'adapted' [Tamara van 't Wout, Qatar]	Edited
39499	150	14	150	16	Consider to add the word 'and' to 'changes in the frequency and/or severity', since it is likely that some extreme events will become more frequent and be more severe, which will have significant impacts. For instance under heavy precipitation section on 7, line 19 it mentions 'frequency and/or in intentsity of heavy rainfall' [Tamara van 't Wout, Qatar]	Considered. Text is edited.
1521	150	18	150	19	Would it be useful to say that energy conversion plays a big role for many types of extremes, e.g. connected to latent heat conversion to kinetic energy (through clouds). The more water vapour, the more energy in the system, and the more potential for violent storms. [Rasmus Benestad, Norway]	Noted but no action is taken. As an FAQ, it needs to be simple and not technical.
28965	150	18		20	Nice FAQ! The sentence "In a warmer climate, extreme events may occur with differing characteristics to what we have experienced in the past. Characteristics of the same events types (e.g., heatwaves, floods or droughts) may change: future extremes may be more severe, may occur more frequently or may occur for differing durations." does not seem to add much above "extremes may change" so could be removed or condensed. The following lines are good. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The FAQ has been rewritten and this text revised
126193	150	22	150	24	This China example was not described before. Note also the typo "is projected to occur, of be exceeded" should be "or be exceeded". [Trigg Talley, United States of America]	Not applicable. This example has been removed.
40615	150	23	150	24	Over what time range? [TSU WGI, France]	Not applicable. This example has been removed.
62765	150	23			of should be replaced with or. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial – copyedit to be completed prior to publication
62897	150	28	150	31	Oceanic extreme events are covered in chapter 9. Consider pairing the oceanic exaxmple of bleeching corals with another terrestrial example as permanent changes of forest ecosystems to steppes in response to drying or permanent ecosystem alterations by wildfires that seems relevant for the terrestrial extreme events covered in chapter 11. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The ocean example has been removed and new examples included.
39501	150	38	150	39	Consider to add the word 'damage' and rephrase into ' increase the risk of wildfires and damage and losses on the agriculture sector' [Tamara van 't Wout, Qatar]	Accept. "Damages" has been added.
40871	150	40	150	40	Suggest to explain why a drought prior to extreme rainfall compounds flooding, as you might think the opposite (extreme rainfall beforehand would). [TSU WGI, France]	Accepted. We have added a short note to explain.
39503	150	40	150	41	This sentence and the link between drought followed by extreme rainfall and exacerbating runoff depends on various factors and consider to rephrase into 'a drought followed by extreme rainfall, may exacerbate the runoff due to reduced soil infiltration capacity'. This can be particially the result of natural phenomena (e.g. relief, slope, rainfall intensity, type of soil; compaction of the soil) and also caused by inadequate land management practices (burning of crop residues, excessive tillage, eliminating hedges, etc) that destroy soil structure, reduce organic matter levels, eliminate beneficial soil fauna and therefore reduce water infiltration (FAO, 2005) http://www.fao.org/3/a0072e/a0072e00.htm#Contents [Tamara van 't Wout, Qatar]	Considered. The FAQs simplified in the revised version and this is not discussed anymore.
39505	150	40	150	41	The words of 'introducing multiple impacts' is a bit vague and would need to have may additional words that mention impacts on what, such as human and natural systems. [Tamara van 't Wout, Qatar]	Accepted.
20769	150	46	150	50	Assuming models become increasingly able to reproduce correctly internal variability of climate and its evolution with climate change, should they not offer samples of extreme events non observed so far? Does the recentr literature explore this logical possibility? [philippe waldteufel, France]	Considered. We simplified the FAQ in the revised version and now have specific definition of unprecedented event. This is not applicable anymore.
126195	150	46	150	53	The discussion is correct regarding the 50-60 year data record and the possibility that extremes, even in a stable climate, could exceed instances in that record. The key point here, though, is what is intended regarding "unprecedented." There are methods for estimating unrecorded extremes, but the key concerns are generally with regard to the historical record and efforts to prepare for historical extremes. In that case, the discussion would benefit by focusing on historical precedent and responses. [Trigg Talley, United States of America]	Taken into account. This paragraph has been removed and the meaning of unprecedented, in the case of this FAQ, has been clarified.
96151	151	9	151	10	FAQ 11.2, Figure 1: The coloured points could be described in the figure caption: For example, it is not apparent what exactly "Storylines" means in this context. Some may only look at the figures and won't read the whole text. Please improve. [Nicole Wilke, Germany]	Taken into account. This figure has been replaced.
41007	152	0			the structure is not very clear to me and I think the flow could be improved [TSU WGI, France]	Accepted. The text has been streamlined and reorganized.
41045	152	0			there are significant overlaps with FAQ11.1 (L38-45) and FAQ11.2 (p153 L4-9) [TSU WGI, France]	Taken into account. The focus of the three chapter FAQs has been revised to tell a clear story that flows without significant overlap.
40671	152	0			reading the title of FAQ11.3, one expects to have a FAQ on detection and attribution and a clear explanation on how we can tell that human are probably responsible for that specific extreme events. It is quickly mentioned here and there but it doesn't seem to be the focus of this FAQ. [TSU WGI, France]	Accepted. The FAQ has been simplified with a more clear focus on event attribution.
39679	152	0			the text could be streamlined (e.g. L12-17 has strong similarities with L47-55) [TSU WGI, France]	Accepted. These two paragraphs have been combined.
1523	152	1	152	15	One way to tell is to analyse the number of record-breaking events (e.g. DOI: 10.1029/2008EO410002). [Rasmus Benestad, Norway]	Noted. But this is not applicable here as we focused on events that are unprecedented in the observations (that is, never observed) rather than record-breaking events.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40539	152	1	153	9	Nice figure for this FAQ. I would have expected the FAQ text to talk more about what causes extreme events and the influence of climate change on these factors, e.g., atmospheric circulation patters, dynamic vs. thermodynamic effects. Could also mention some examples of attributions that have been made... e.g., Hurricane Harvey. Also, you mention 'extreme climate events', you could put some examples in brackets, as I think there could be confusion between what's an extreme weather event vs. an extreme climate event. [TSU WGI, France]	Noted. While these are good suggestions, they are too complex for an FAQ.
62649	152	3	152	3	The sentence is confusing and will mislead a common reader, and also creat questions on AR6 substance. Climate is a long-term term (at least 30 years) state, it does not vary in day-to-day scale. The sentence must be modified for better clarity. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The opening paragraph has been revised and this sentence removed.
29471	152	3	153	2	we can say climate change is the responsible for this recent extremes. Human activities have increase over the past decades. For instance in West Africa(Southern part of Nigeria,Ondo State), residents have been experiencing storms for the past 2 years. stormy winds that are very destructive and strong. May 5, 2018 came with a very stong wind which have never been expeienced before now. it occured in 2019 and since the beginning of raining season in this area, there has been more wind than precipitation. though no research have been carried out to ascertain the cause, but it is becoming evident that this is a result of climate change(low confidence). before this, there has been increase in daily temperature for a while in this region. so we can't but relate this changes and extremes to climate change. [Babatunde Oyekan, Nigeria]	Taken into account. The FAQ has been revised with a better focus on extreme event attribution.
39507	152	7	152	9	Consider to add the word 'and' to 'the frequency and/or severity', since it is likely that some extreme events will become more frequent and be more severe [Tamara van 't Wout, Qatar]	Accepted.
100495	152	47	152	47	The terms 'heatwaves' and 'heat waves' seem to be used interchangeably throughout the chapter. Using one term consistently would be helpful. [Wim Thiery, Belgium]	Accepted.
28967	152	49			Also a good FAQ - I wonder if the mention of event attribution can be more clear. I think the public can appreciate the technique of running lots of computer simulations, one set with the present climate conditions and another set with the global warming signal removed. Stating this may make it clear how the scientists estimate the human contribution and make the FAQ stronger and more useful. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The FAQ has been simplified with a more clear focus on event attribution.
110051	153	4	153	9	This seems at odds with some of the main text that assesses some recent heat events in Australia may well have been all but impossible without human interference in the climate system. A check is required for consistency here I think. [Peter Thorne, Ireland]	Taken into account. The text of this FAQ was simplified and this paragraph was removed.
39509	153	5	153	8	Consider to remove the word 'place' [Tamara van 't Wout, Qatar]	Taken into account. The text of this FAQ was simplified and this paragraph was removed.
39709	153	6	153	7	"...we do not yet have convincing evidence that any of these events would have actually been impossible in the absence of climate change." -> This seems inconsistent with the ES, which states that "The available evidence suggests that some recent extreme events could not have occurred without human influence" [TSU WGI, France]	Taken into account. The text of this FAQ was simplified and this paragraph was removed.
62785	154	1	230	1	These references should be included in the present chapter because: 1) These authors clearly defined what can be considered as marine heatwave (Hobday et al., Progr. Oceanogr. 2006); and 2) These authors quantified for first time the increasingly frequency, intensity and duration of marine heatwaves on a global-scale, and the potential consequences that they will have on biodiversity (Smale et al. Nat. Clim. Change 2019). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Marine heatwaves do not belong to the scope of chapter 11 and are addressed in chapter 9. This scope is indicated in the ES and introduction to chapter 11.
69955	160	13	160	14	Please check typo. [Young-Hwa BYUN, Republic of Korea]	Corrected
5687	171	15	171	16	The study from Evaristo & McDonnell was retracted and should not be cited. [Joachim Rock, Germany]	Accepted. This reference was removed.
9251	176	47	176	47	Insert the following citation after line 47. Hall, T. C., Sealy, A. M., Stephenson, T. S., Kusunoki,S., Taylor, M. A., Chen, A. A., and Kitoh, A. (2012) Future climate of the Caribbean from a super-high-resolution atmospheric general circulation model. Theor. Appl. Climatol. doi:10.1007/s00704-012-0779-7. [Shoji Kusunoki, Japan]	Accepted: the reference was included.
23989	181	11	181	12	Note that this paper has already been published for some time and is no longer under review: Hunt, Turner and Shaffrey, (2019), see https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-18-0601.1 [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Corrected
98191	185	49	185	55	Knutson et al. (2014a and b) are referencing the same paper. Delete one of them and adjust the citation in the text. [Thomas Knutson, United States of America]	The reference has been removed in FGD
9253	187	41	187	41	Insert the following citation after line 41. Kusunoki, S., Mizuta, R., and Hosaka, M. (2015) Future changes in precipitation intensity over the Arctic projected by a global atmospheric model with a 60-km grid size. Polar Science 9, 277-292, doi:10.1016/j.polar.2015.08.001. [Shoji Kusunoki, Japan]	Rejected. Too detailed for the assessment.
9255	187	41	187	41	Insert the following citation after line 41. Kusunoki, S., Nakaegawa,T., Pinzon, R., Sanchez-Galan, J. E., and Fabrega, J. R. (2019) Future precipitation changes over Panama projected with the atmospheric global model MRI-AGCM3.2. Climate Dyn. doi:10.1007/s00382-019-04842-w. [Shoji Kusunoki, Japan]	Accepted: the reference was be included.
9259	189	48	189	48	typo: First author should be "Li, X.", not "Li, X. X.". [Xing Li, China]	The reference has been removed in FGD
87965	190	13	190	13	add "A. Libertino, D. Ganora and P. Claps. (2019) Evidence for increasing rainfall extremes remains elusive at large spatial scales: the case of Italy. Geophysical Research Letters, Volume 46, Issue 13, 7437-7446, DOI:10.1029/2019GL083371" [Pierluigi Claps, Italy]	Accepted
62437	194				Mcvicar et al. 2012 page 194 mentioned twice in citation however there is no (a) or (b) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Considred. This is resolved in FGD.
62511	196	57	196	58	Reference: Correction of reference and full details. Hiroyuki Murakami, Thomas L. Delworth, William F. Cook, Ming Zhao, Baoqiang Xiang, and Pang-Chi Hsu., 2020. Detected climatic change in the global distribution of tropical cyclones. PNAS, 1-9. https://doi.org/10.1073/pnas.1922500117 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Corrected
20273	213	5	231	5	Please spell what the acronyms in column 1 of the table mean, or give an adequate reference [philippe waldteufel, France]	Not applicable - table removed from chapter
87403	218	30	218	31	The reference for Tsuji et al. should be corrected to ' Tsuji, H., C. Yokoyama, and Y. N. Takayabu (2020) Contrasting features of the July 2018 heavy rainfall event and the 2017 Northern Kyushu rainfall event in Japan. J. Meteor. Soc. Japan, 98, Special Edition on Extreme Rainfall Events in 2017 and 2018, Available at: https://doi.org/10.2151/jmsj.2020-045. [Yukari Takayabu, Japan]	Corrected
17149	224	1	224	1	Please equip the square with patterns or symbols, instead of color gradient only. It will help the graph to be more understanable by colour blind people. Thanks. [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. It is unclear what this question is referring to. The question seems to be misplaced or referring to the wrong chapter page.
68511	226	55			YOKOYAMA, C., TSUJI, H., and TAKAYABU, Y. N. -> Yokoyama, C., H. Tsuji, and Y. N. Takayabu [Yukiko Imada, Japan]	Corrected

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
5697	231	1	231	1	Table 11.A.1, row "ENA": Please check and correct: why do you consider "snowfall" to be an extreme event? Either delete this row or substantiate the "extremeliness" of this event. [Joachim Rock, Germany]	Not applicable - table removed from chapter
100421	231	1	237	1	Some references are missing at Table 11.A.1 (eg.: Otto et al. 2015; de Abreu et al 2019) [Lincoln Alves, Brazil]	Not applicable - table removed from chapter
55179	231	1	237	1	This is another large and ambitious table that we view as problematic. We would note that there is no consistency in style or content across the large tables that appear in Chapter 11. In this table, there are a multitude of confidence statements with no description of how they were derived. The IPCC Guidance document on uncertainty makes very clear that assessment statements using calibrated languge must be accompanied by a 'traceable account' that lays out the evidence, its quality and agreement, the reason for assiging a certain level of confidence, etc. Simply having the words 'low', 'medium' or 'high' in a box provides no information at all to the reader as to how this was arrived at. Although there is a box in each row that contains literature references, we would note that many of these are references to brief overview papers (e.g. Herring et al.), which do not actually contain the information that is relied on. This makes it virtually impossible for a reader to trace back the confidence assessment to the underlying literature (citations within citations, pointing to further citations). In some of our attempts to trace these statements back to the primary source, it became apparent that the references in the table were incorrect. An additional problem is the column labled "quality of evidence". There seems to be no correlation between quality of evidence and confidence, and it is hard to imagine how one can have 'medium' confidence in a finding for whith there is 'low' quality evidence (and often only a single paper cited). We would note that the IPCC Uncertainty Guidance makes clear that authors can use a 'confidence' qualifier if the evidence permits, but that authors have to choose between giving an 'evidence and agreement' statement or a 'confidence' statement -- not both. [Nancy Hamzawi, Canada]	Not applicable - table removed from chapter
126197	231	1			Table 11.A.1. ARO sea ice extent. The reference is not specific enough. Cite the actual article: Zhang, R. and T. R. Knutson, 2013: The role of global climate change in the extreme low summer Arctic sea ice extent in 2012. In: Explaining Extreme Events of 2012 from a Climate Perspective. Bulletin of the American Meteorological Society, 94(9), S23-S26. [Trigg Talley, United States of America]	Not applicable - table removed from chapter
109393	231	5	231	11	Suggest grouping the table entries by phenomena/region as in the rest of Ch 11 or region/phenomena as in Ch12/TS/SPM. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - table removed from chapter
82169	231	5	231	11	Table 11.A.1: the table needs some structure, e.g. based on regions or the type of the extreme event. The year of occurence of the individual extreme event could be included. [Borbála Gálos, Hungary]	Not applicable - table removed from chapter
11773	231	5	231	11	the caption for Table 11.A.1. should tell readers where to find the list of the acronyms in the table [Amy East, United States of America]	Not applicable - table removed from chapter
5691	231	5	237	1	Table 11.A.1, row "CNA": Please check and correct: why do you cite only one study and assign "medium confidence" to the assessment due to other studies? Why don't you evaluate and cite these other studies, too? [Joachim Rock, Germany]	Not applicable - table removed from chapter
5693	231	5	237	1	Table 11.A.1.: please bring some order in this table. Order by region and type of event (any of these may be the first criterion to order by). [Joachim Rock, Germany]	Not applicable - table removed from chapter
79165	231	5	237	2	If this is meant to be a complete list of event attribution studies since 2013, then this one should be added: Undorf et al., ERL, 2020 (https://iopscience.iop.org/article/10.1088/1748-9326/ab6999/pdf) because of the event attribution part in that study. There are also findings on projections of heat extremes in nw europe [Sabine Undorf, Sweden]	Not applicable - table removed from chapter
17145	231	5	237	2	Please give table header on the next every pages of Table 11.A.1. Repeat Header Row. Thanks. [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - table removed from chapter
39809	231	12	231	12	"Quality of evidence" should perhaps be called "Level of evidence" as other aspects besides quality are considered (amount, type). This phrase appears in the Mastrandrea guidance. [TSU WGI, France]	Not applicable - table removed from chapter
40451	231	12	237	1	Is each row in the table for a specific extreme event? If so, I suggest adding an extra column that gives a name or more details for each event (e.g., 'European summer heatwave 2018'). That way, users interested in a specific event can readily look it up in the table. [TSU WGI, France]	Not applicable - table removed from chapter
80715	231	12	237	1	Table 11.A.1 is a very long table and it does not seem very organized. It would be better to organize it by continent (no need to add a column jut an extra line with the continent name) and within each continent, either organize by region or by type of event. [Helene Jacot Des Combes, Marshall Islands]	Not applicable - table removed from chapter
62899	231		237		Appendix table 11.A.1: consider spelling out regions names rather than using abbreviations as the table has enough space in column 1. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - table removed from chapter
5695	234	1	234	1	Table 11.A.1, row "NEU": Please check and correct: why do you consider "sunshine hours" to be an extreme event? Either delete this row or substantiate the "extremeliness" of the meteorological standard metric. [Joachim Rock, Germany]	Not applicable - table removed from chapter
80717	234	1	234	1	what type of extreme event does that correspond to? [Helene Jacot Des Combes, Marshall Islands]	Not applicable - table removed from chapter
80719	238	1	238	1	In figure 11.1, the orange color selected for Tland is too close from the red color selected for TXxland so it is difficult to separate them [Helene Jacot Des Combes, Marshall Islands]	Taken into account - figure is updated (is now Figure 11.3 in the FGD)
96153	238	1	238	1	This is a very useful graph even though the forcing is not fully comparable. Suggest to provide this for other RCPs/SSP-RCPs, too. Most important including for practitioners is of course RCP8.5, because it has been used in many post-AR5 studies. In addition, we wonder, if the "raw" CMIP6 results are shown or those merged with multiple lines of evidence. Please clarify, and please be coherent across the report. [Nicole Wilke, Germany]	Noted. The figure now only considers observed changes.
110053	238	1	238	2	The figure would benefit from an overall title. As the idea is comparability of panels it is extremely unhelpful for the third panel to have a stretched y-axis relative to the first two. Its axis should similarly extend from -2 to 4. Otherwise at a minimum you need a yardstick of 1c height next to every panel to stress the difference in scale. [Peter Thorne, Ireland]	Taken into account - overall title added. Now only shows one panel so other comments are not applicable. Is Figure 11.3 in FGD.
108899	238	1	238	13	Fig.1 Check consistency with the figure shown in chapter 4 and refer to GSAT. I think it is misleading to show observations with annual resolution next to a strongly smoothed multi-model means. This seems to suggest a mismatch between the multi-model mean and observations. I recommend showing a range across CMIP realizations that demonstrates that the reanalysis fall within the range of simulations. If you use annual resolution for reanalyses also use annual resolution to produce the band around the CMIP5 multi-model mean. Thereby it becomes obvious that there is no mismatch here. [Erich Fischer, Switzerland]	Considered. The figure now only considers observed changes.
102575	238	1	238	16	What is the blue shaded area (entire model span or 95% conf.)? Please add. [Philippe Tulkens, Belgium]	Not applicable - figure no longer shows model estimates (is now Figure 11.3 in FGD)
102577	238	1	238	16	The big difference between CMIP5/6 (considering the global scale) has not been commented. [Philippe Tulkens, Belgium]	Noted. The figure now only considers observed changes.
49949	238	2	238	2	The title on the 3rd panel down is either distorted in some way or cutoff. [Daniel Gilford, United States of America]	Not applicable - figure was redone, no longer has the 3rd panel. Is Figure 11.3 in the FGD
126199	238	3	238	13	Perhaps the third panel should use the same scale as the first two. Or, even better, replace the third panel with three side-by-side zooms of the observed/reanalyzed time period. [Trigg Talley, United States of America]	Not applicable - figure was redone, no longer has the 3rd panel. Is Figure 11.3 in the FGD
113511	238		238		I would strongly suggest to use also TXx for the reanalyses and make it consistent and comparable. [Diego Miralles, Belgium]	Considered. The figure now only considers observed changes.
117129	238		238		Is the key message that one cannot see the differences between the red and orange curves? [Valerie Masson-Delmotte, France]	Noted - figure is updated (is now Figure 11.3 in the FGD)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
62859	238		238		Figure 11.1: Panel 3 Observations/Reanalysis: In the current version the lines are too thick and display the data poorly, consider enlarging this panel as the majority of it is empty space. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - figure is updated (is now Figure 11.3 in the FGD)
73875	238		238		Figure 11.1 - The thrid plot showing obserations/reanalysis should ideally be super-imposed on the top 2. Rather than plotted on a separate plot. Also should consider adding ERA5 in addition or instead of ERA-I? [Jatin Kala, Australia]	Taken into account - figure is updated (is now Figure 11.3 in the FGD). No longer shows any reanalysis datasets.
53547	238		271		All maps shown in CH11 are global maps. Is it what we expect from a so-called "regional chapter"? What about the influence of orography (only two instances of this word in the whole chapter) and other regional features on projected extremes for instance? [Hervé Douville, France]	Considered. Some figures (e.g. projected changes in extreme precipitation and temperature) provide regional projections.
17147	239	1	239	1	Figure 11.2 is brilliant. However, please split it into smaller region figures as the symbols become too small. Probably A. North and South America, B. Europe and Africa, C. Asia and Australia. Thanks [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Considered. This figure is replaced with a synthesis fig based regional assessment tables in 11.9
108901	239	1	239	9	Does this figure only reflect the papers of the BAMS special issues or also papers that have been published elsewhere? [Erich Fischer, Switzerland]	Considered. This figure is replaced with a synthesis fig based regional assessment tables in 11.9
71301	240	0	240	0	Quality of the figure should be improved. [Kenji Taniguchi, Japan]	Accepted - the figure has been reproduced.
110055	240	1	240	1	These maps are not in the standard projection for WG AR1. Colour scale applies to all three panels so only needed once (and can be larger for legibility). Panel titles need to be larger. Figure would greatly benefit from an over-arching title that is self describing. [Peter Thorne, Ireland]	Accepted - now in Robinson projection. Only one colour bar shown, titles enlarged, overall title added.
68761	240	2	240	3	Change 'annual maximum precipitation' to 'annual maximum one-day precipitation' [Bodeker Greg, New Zealand]	Editorial
113513	240		240		I would suggest not to change the projection from figure to figure. [Diego Miralles, Belgium]	Accepted - now in Robinson projection.
42557	241	0	241	0	Typo in the Figure 11.3 y-axis title: cchanges -> changes [Joan Bech, Spain]	Noted. Typo is corrected.
71303	241	0	241	0	Vertical label of the figure. "cchanges" -> "changes" [Kenji Taniguchi, Japan]	Noted. Typo is corrected.
108903	241	1	241	3	This figure requires a better caption. Why is limited to only 3 types of extremes. I prefer this assessment over the NAS report but given that there is an obvious disagreement in the ranking of drought and heavy precipitation the lines of evidence need to be thoroughly documented [Erich Fischer, Switzerland]	Noted. The figure is finalized.
2905	241	1	241	4	More extremes should be added in Fig.11.3. [Zong Ci Zhao, China]	Noted. More are added in the final version.
82163	241	3	241	3	Fig.11.3: global and regional should be distinguished. All extremes should be included. [Borbála Gálos, Hungary]	Noted. More extremes are included.
62841	241		241		Figure 11.3. Figure seems to be an unfinished version or sketch. Please, do not forget to replace it for the final version. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The figure is finalized.
117131	241		241		could the same pictograms as in fig 11.2 be used? [Valerie Masson-Delmotte, France]	Noted. Fig 11.2 is now redrawn.
73877	241		241		Figure 11.3 - what do the different shades of grey mean? [Jatin Kala, Australia]	Noted. Light shades mean higher confidence.
62757	241				Figure 11.3: Spelling error in Y axis. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Typo is corrected.
108905	242	1	242	8	I like this figure but it seems to introduce a complementary approach to the IPCC Uncertainty Guidance Note. It is not clear whether this is a meaningful alternative approach or an application of the existing Uncertainty Guidance to the field of event attribution. [Erich Fischer, Switzerland]	Noted. Fig 11.4 is not included in FGD.
113515	242		242		I like this framework, but is it the one used through the entire report? otherwise it may just make it confusing. [Diego Miralles, Belgium]	Noted. Fig 11.4 is not included in FGD.
110059	243	1	243	1	Panels are not plotted in the WG1 standard projection. Lacking also a self-describing figure title and the panel titles are also not intuitive. Colour scale is likely not colour blind friendly and does not map to the WG1 suggested schema. [Peter Thorne, Ireland]	Not applicable - figure removed
73879	243		243		Figure 11.5 - I find this hard to digest. Why the 10-year period specifically? [Jatin Kala, Australia]	Noted. This figure is removed.
80721	244	1	244	1	Not all regions are represented in Figure 11.6. What about the other regions, for example NTP, ETP and STP for CMIP5 and NPO, EPO and SPO for CMIP6? Obviously the missing regions are mostly oceanic but the small island states located in these regions would benefit greatly from this analysis. [Helene Jacot Des Combes, Marshall Islands]	Noted. Only land regions are included.
108907	244	1	244	6	The caption to this figure is insufficient and does not provide enough details to interpret the figure. It seems very surprising that changes in Rx1day would already emerge at a level of warming of 0.25°C. [Erich Fischer, Switzerland]	Considered. Caption is expanded with supporting reference.
102579	244	1	244	9	Link to regions figure missing. If it does not exist please add. [Philippe Tulkens, Belgium]	Accepted - now pointing to the Atlas for the region definitions. (Note is Figure 11.8 in the FGD)
62893	244	2	244	2	Figure 11.6: consider spelling out the different regions as it is inconvenient to look them up in a different publication and could be implemented e.g. by a list of abbreviation below the figure or as part of the figure caption. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - now pointing to the Atlas for the region definitions. (Note is Figure 11.8 in the FGD)
110061	245	1	245	1	Font sizes need to be much larger to stand a hope of being legible. Figure lacks an overall self-describing title. [Peter Thorne, Ireland]	Accepted - font size increased, overall title added. Is Figure 11.9 in FGD.
108911	245	1	245	9	Is there really a 7°C trend in TXx over the Andes or is there missing data in some subperiods? This seems incredibly high. The fact that there is a substantial fraction of areas of no trend in TXx is not surprising given the high variability but in my opinion this is insufficiently documented and explained in the text. [Erich Fischer, Switzerland]	Taken into account - The subplot in the middle shows TX90p and its units are days/ decade and not Celsius. We updated the plot to make the title and units better legible.
102581	245	1	245	11	Add the grey area to legend -> not just in caption [Philippe Tulkens, Belgium]	Accepted - added a legend for the stippling and grey area. Is Figure 11.9 in the FGD
62701	245	1	245	11	All three figures are labeled with (a) and in the caption they are only referred to as “top”, “middle” and “bottom”. I would advise either to use different labels (a), (b) and (c) and also refer to them in the caption in that way, or alternatively remove the labels and stick with “top” “middle” and “bottom” in the figure. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - now using (a), (b), and (c). Is Figure 11.9 in FGD
113517	245		245		Please could you use the same range for the Max and Min maps (top and bottom)? It seems that for most land regions, the Min has increased much more than the Max, so that the base of the temperature histogram hasn't widened but narrowed in recent years. This disagrees with previous IPCC reports, if I recall right. Is this discussed somewhere? [Diego Miralles, Belgium]	Taken into account - TXx and TNn now use the same range. Is Figure 11.9 in FGD.
62843	245		245		Figure 11.7. Please consider to increase the font size of the figure legends. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - font size increased. Is Figure 11.9 in FGD.
110063	246	1	246	1	The lower panels are not ins tandard projection and lack self-describing titles. The overall figure could be made more self-describing with some additional efforts. [Peter Thorne, Ireland]	Accepted - figure now in standard projection and re-designed.
17151	246	1	246	1	Figure 11.8 Above Section, the one that comparing ERA-5 and HadEx3, is some how too dense and may lead to confusion at glance. It is better to be splitted. If it is possible, please use same interval scale grids (more contrast colours), which will make it more comparable. [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - panel is now removed. Is Figure 11.10 in FGD.
108913	246	1	246	9	How do you compare the percentile based values. Shouldn't they be equal by construction during the reference period? [Erich Fischer, Switzerland]	Not applicable - panel removed
5701	246	1	247	1	Figure 11.8: Please consider adding a sentence clarifying the meaning of the colours in the map as in the text to figure 11.13. For example, does orange colour indicate that he models underestimate the temperatures? [Joachim Rock, Germany]	Accepted - colorbar now indicates "too cold" and "too warm". Is Figure 11.10 in the FGD.
113519	246		246		Negative RMSEs? [Diego Miralles, Belgium]	Not applicable - panel removed
73881	246		246		Figure 11.8 - Good that you used ERA5. Then you should only use ERA5 througout, e.g., Figure 11.1 [Jatin Kala, Australia]	Taken into account - Figure 11.1 (Figure 11.2 in the FGD) no longer shows reanalysis data.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33249	246		246		Caption in Figure 11.8 indicates top,medium and bottom panel. Top panel is clear and it is the portrait diagram. However, middle and lower panels are joint and someone can think it is the same panel with 4 subpanels. I suggest either separate middle and bottom panels or refer to the 4 panels as bottom panels with top and bottom rows or (a) (b) (c) and (d) [Gonzalez Sergi, Spain]	Taken into account - top panel has been removed. Other panels have been redesigned. Is figure 11.10 in the FGD.
102583	247	1	248	12	Stippling barely visible, the grey lines used in the atlas works well (suggestion) [Philippe Tulkens, Belgium]	Taken into account - hatching added. Is Figure 11.11 in the FGD.
108915	247	2	247	10	Please adapt stippling and use a standard AR6 color bar [Erich Fischer, Switzerland]	Taken into account - hatching added. Changed the colormap. Is Figure 11.11 in the FGD.
110065	247	3	247	4	Panels are not in standard projection and lack titles. The overall figure lacks a self-describing title. [Peter Thorne, Ireland]	Accepted - projection changed, panel titles and overarching title added. Is Figure 11.11 in FGD.
82167	247	5	247	5	Fig.11.9: colorbar should be finer, in order to show more clearly, whether projected changes (°C) in annual maximum daily maximum temperature (TXx) exceeds the 1.5°C, 2°C, 3°C, and 4°C of global warming levels. [Borbála Gálos, Hungary]	Accepted - colorbar now has more levels. Is Figure 11.11 in FGD.
113521	247		247		Stippling missing. [Diego Miralles, Belgium]	Taken into account - hatching added. Is Figure 11.11 in the FGD.
73883	247		247		Figure 11.9 - Unless I am missing something, I do not see any stippling? Is it the white dots? [Jatin Kala, Australia]	Taken into account - hatching added. Is Figure 11.11 in the FGD.
110067	248	2	248	3	Same comment applies as prior figure [Peter Thorne, Ireland]	Accepted - projection changed, panel titles and overarching title added. Is Figure 11.11 in FGD.
62767	248	2			Stippling is difficult to see, perhaps a darker color would be better, if able to be differentiated from coastlines. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - hatching now in black. Note that no region is actually hatched (as the models agree everywhere). Is now Figure 11.11.
117133	248		248		I need to be convinced that there is no difference between CMIP5 and CMIP6 [Valerie Masson-Delmotte, France]	Taken into account - Now only shows CMIP6 data. See e.g. Wehner 2021 and Seneviratne and Hauser, 2020
73885	248		248		Figure 11.10 - Again I do not see stippling anywhere? Is it the white dots? But you have white in the color bar. Why not use black crosses for stippling? [Jatin Kala, Australia]	Accepted - hatching now in black. Note that no region is actually hatched (as the models agree everywhere). Is now Figure 11.11.
80723	249	1	249	1	Because the small island states in the Pacific are located in an ocean region, they do not appear in figure 11.11. It would be useful to add a panel with their region to this figure. They would benefit from this analysis. [Helene Jacot Des Combes, Marshall Islands]	Rejected - CMIP6 models do not have sufficient resolution to assess small islands.
102585	249	1	249	7	Link to regions figure missing. If it does not exist please add. [Philippe Tulkens, Belgium]	Taken into account - now links to the region definitions in the Atlas.
108917	249	1	249	10	This plot seems to be inconsistent with the claim that TXx warm 50%-200% than GSAT. In some areas the warming seems to be no higher than GSAT despite land-sea warming contrast. [Erich Fischer, Switzerland]	Considered. The text is revised. In particular, the statement in the SOD ES about 50-200% is removed.
108919	249	1	249	10	I strongly suggest to show thin lines for individual models to convince the numerous people who argue that the linear scaling only holds for the multi-model mean and not for the individual models [Erich Fischer, Switzerland]	Accepted - this is now done in Panel (a). Is now Figure 11.3 in FGD.
73887	249		249		Figure 11.11 - Good plot, can hardly see the regions though, suggest to remove the mini-plots in top left corner. [Jatin Kala, Australia]	Taken into account - removed the mini-maps. Figure was redesigned. Is now Figure 11.3.
110069	250	1	250	2	Top panel is not in standard projection and key does not match map (all symbols filled in map contrary to implicationfrom key. Figure lacks title. How do you explain juxtaposed significant increases and decreases in very short space in this figure? Is this because of differing station duration rather than a real physical signal? What similarity criteria have been applied to minimise teh chance of spatial gradients arising because of differing station series length rather than real geophysical effects? Some of the sharp gradients seem implausible. [Peter Thorne, Ireland]	Taken into account. Maps are now in standard projection. Figure has been redesigned to better differentiate between the categories. Yes, the trends over the space are very noisy with widely scattered increasing and decreasing trends. We are not yet able to confidently assess local changes in extreme precipitation events. The complexity of the processes and the large natural variability of precipitation relative to the size of change that is expected from the warming climate mean lower signal-to-noise ratio since the signal from climate change is often hidden amongst a large amount of natural noise. Thus, the chance of seeing an intensification of extreme precipitation at an individual meteorological station is still slim, and the trends over the space are very noisy. However, we can see significant intensification at ~1-in-10 stations at global land and some continents (see boxplot figures), that's still much more than we expect in an unchanging climate, where we would expect the false detection of intensification to occur at ~1-in-40 stations.
108921	250	1	250	10	The colors are hard to see and I don't see any open circles [Erich Fischer, Switzerland]	Accepted - the figure has been redesigned. Is Figure 11.13 in the FGD.
5699	250	1	250	14	Figure 11.12: Please clarify and revise the text and - if needed - the figure. It is not clear what is shown in panel b. Is the blue dot in the left panel ("global land") indicating that 9.5% of the stations show a significant positive trend? And why are all box-and-whisker-plots showing means deviating from the null line if they are constructed from a "no trend"-null hypothesis? Should this indicate that even under a "no trend"-hypothesis 2% of the stations (globally) would show significant positive trends? [Joachim Rock, Germany]	Taken into account - the figure has been redesigned. Caption has been updated. Is Figure 11.13 in the FGD.
102587	250	1	250	16	Please add text/legend to the figure (not just caption) on the difference between the two columns/bars for each region. The difference is difficult to immediately comprehend [Philippe Tulkens, Belgium]	Accepted - colours now explained in a figure legend. Is Figure 11.13 in the FGD.
84901	250	4	250	16	I can't understand what 11b represents from the figure description. Please could the description be made more clear. [Turner Jessica, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - the caption has been updated. Figure 11.13 in the FGD.
17153	251	1	251	1	Figure 11.13, Is it just exactly the same with Figure 11.8? I suggest to remove it and refer the related discussion to Figure 11.8. [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Noted - One of the figures shows temperature extremes, the other shows precipitation extremes. Figure updated.
110071	251	1	251	1	Maps are not in standard projection. The colour bar scale font is far too small. Figure lacks a self describing title. [Peter Thorne, Ireland]	Taken into account - Figure redesigned. Maps are now in standard projection. Font size increased, title added. Is Figure 11.14 in FGD.
108923	251	1	251	12	Evaluation of precipitation variable in top panel would make more sense against HadEX and REGEN. ERA-5 is basically a model product in terms of precipitation [Erich Fischer, Switzerland]	Not applicable - panel removed.
102589	251	1	251	13	Very small upper figure -> enlarge. Also font. [Philippe Tulkens, Belgium]	Not applicable - panel removed.
113523	251		251		Not sure again why RMSEs are negative. [Diego Miralles, Belgium]	Not applicable - panel removed.
110073	252	1	252	1	Panels are not in standard projection and lack titles. The overall figure lacks a self-describing title. [Peter Thorne, Ireland]	Accepted - Figure redesigned. Maps are now in standard projection. Title added. Is Figure 11.16 in FGD.
73889	252		252		Figure 11.14 - do not use white for stippling if white is used in the color bar. [Jatin Kala, Australia]	Accepted - Figure redesigned. Hatching now in black. Is Figure 11.16 in FGD.
5703	253	1	253	13	Please revise figure. The grey shading is barely noticable and in line 10 it should probably read "… climate models (B and D).", not "B and C". [Joachim Rock, Germany]	Not applicable - figure removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
113711	253	10	253	10	"(B and D)." instead of "(B and C)." [Agnieszka Kowalczyk, Poland]	Not applicable - figure removed
17155	254	1	254	1	Figure 11.16:, Please attribute, give name, or give index explanation for each pointed region. [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - figure removed
110075	254	1	254	2	Figure is not in standard projection. Lacks overall title. Key font text is too small to be readily legible. Reason for boundng boxes could be labelled within the figure? [Peter Thorne, Ireland]	Not applicable - figure removed
62787	254	1	254	5	This figure could be enlarged? At the present state is difficult to distinguish the different trends in the areas. Maybe it could be presented in landscape instead portrait form?. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - figure removed
102591	254	1	254	7	Larger dots needed. [Philippe Tulkens, Belgium]	Not applicable - figure removed
62351	254				Figure 11.16: The figure caption does not indicate what "sufficient data" means. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - figure removed
110077	255	1	255	2	These definitional aspects are the domain of chapter 8. This figure should be there and not here. [Peter Thorne, Ireland]	Accepted: The figure 11.17 has been removed from chapter 11 and now a combined and more simplified figure with Ch. 8 is included
108929	255	1	255	10	I like the idea of such a schematic but there seems to be a lack of feedback to the atmospheric drivers.Also, do we need the term "critical" everywhere? [Erich Fischer, Switzerland]	Taken into account - A simplified figure on mechanisms leading to drought, not including the terms "critical", is now included in chapter 8 (Figure 8.6). The previous figure in chapter 11 also included the feedback of evapotranspiration on air humidity, which is also the case in Fig. 8.6.
89133	255	1		2	Flash drought could be added here - this is a rapid intensification of drought. Otkin, J. A., Svoboda, M., Hunt, E. D., Ford, T. W., Anderson, M. C., Hain, C., et al. (2018). Flash Droughts: A Review and Assessment of the Challenges Imposed by Rapid-Onset Droughts in the United States. Bulletin of the American Meteorological Society, 99(5), 911–919. https://doi.org/10.1175/BAMS-D-17-0149.1 Pendergrass, A. G., Meehl, G. A., Pulwarty, R., Hobbins, M., Hoell, A., AghaKouchak, A., et al. (2020). Flash droughts present a new challenge for subseasonal-to-seasonal prediction. Nature Climate Change, 10(3), 191–199. https://doi.org/10.1038/s41558-020-0709-0 [Angeline Pendergrass, United States of America]	Rejected - we do not think this is the best place to mention flash droughts. Flash droughts are mentioned and the references included in 11.6.1.
126201	255	1			Figure caption should mention the meaning of the black and red lines. [Trigg Talley, United States of America]	Not applicable: The figure 11.17 has been removed from chapter 11 and now a combined and more simplified figure with Ch. 8 is included
126203	255	1			The flow of arrows on the left-hand side of Figure 11.17 might not be the most "hydrologically correct" representation of the order of causation. A better schematic might have precipitation deficits driving soil moisture deficits, which drive groundwater deficits, which drive streamflow deficits, which drive surface-water storage deficits, but with precipitation deficits also driving each of those elements directly. [Trigg Talley, United States of America]	Taken into account: The figure 11.17 has been removed from chapter 11 and now a combined and more simplified figure with Ch. 8 is included
126205	255	1			A very important element that is missing from Figure 11.17 is snow-water-equivalent (SWE) deficit, which is driven by precipitation deficit, which drives soil moisture and groundwater deficit, and which affects the surface energy budget, thereby influencing evapotranspiration. [Trigg Talley, United States of America]	Accepted: The figure 11.17 has been removed from chapter 11 and now a combined and more simplified figure with Ch. 8 is included. The new figure includes snow.
126207	255	1			[DROUGHT] Figure 11.17 places precipitation deficits and AED deficits at the top, establishing them as the drivers of drought in general. The visual elevation of AED to this position might not be optimal. An alternative would be (instead of AED) to place atmospheric CO2 concentration at the top. This drives increased longwave radiation, which drives near-surface warming. If this is considered too much external to the issue of drought, then one could start with the near-surface warming and the enhanced surface radiation as the drivers at the top. Even in this case, atmospheric CO2 concentration increase ought to appear at the top as another important factor for drought (rather than place inside the diagram as "physiological CO2 effects"). The CO2 fertilization suppresses evapotranspiration. The warming, of course, acting through C-C, enhances evapotranspiration by increasing the VPD. The radiation enhancement, by supplying more energy, also potentially increases evapotranspiration. [Trigg Talley, United States of America]	Taken into account: The figure 11.17 has been removed from chapter 11 and now a combined and more simplified figure with Ch. 8 is included
113525	255		255		Some editing could make this diagram look a lot better… I miss two things: (a) VPD-stress, (b) 'ecological drought'. The latter term is used in some papers and I find it useful to give a name to the drought in your green box. [Diego Miralles, Belgium]	Taken into account: The figure 11.17 has been removed from chapter 11 and now a combined and more simplified figure with Ch. 8 is included
10149	255				On Fig 11.17, the red upward arrow connecting "Physiological CO2 effects: enhanced LAI and WUE" to "Evapotranspiration" should perhaps be labeled positive in addition to negative. That's because enhanced LAI = more ET, even as enhanced WUE = less ET. Right now the arrow is drawn/labeled as if it is only for the effect of WUE on ET. Relevant studies: Mankin et al. (2019), Nature Geoscience, https://doi.org/10.1038/s41561-019-0480-x and Mankin et al. (2018), GRL, https://doi.org/10.1002/2018GL077051 . [Jacob Scheff, United States of America]	Taken into account: The figure 11.17 has been removed from chapter 11 and now a combined and more simplified figure with Ch. 8 is included
70399	255				Figure 11.17: There is a box labeled “Physiological CO2 effects” which lists as sub items “enhanced LAI” and “WUE”, along with an arrow indicating that these processes reduce evapotranspiration. This depiction is not correct as labeled or is confusing at best. The overall effect of all plant responses combined is a decrease in evapotranspiration (Swann et al. 2016, Skinner et al. 2017), but “enhanced LAI” increases evapotranspiration (Skinner et al. 2017). Plant responses to CO2 can influence land evapotranspiration through two mechanisms, which have opposite effects on evapotranspiration: (1) increased rates of photosynthesis and increases in leaf area, which increase transpiration, and (2) stomatal closure, which decreases transpiration. It would be more clear if the influence of these two factors (LAI and WUE) on ET were labeled separately, or if the language indicated that the change in WUE is larger than the increase in LAI such that ET fluxes decrease due to the combination of plant responses. Another option would be to use stomatal closure and LAI increase as the two factors since they more directly control resulting changes in ET. [Abigail Swann, United States of America]	Taken into account: The figure 11.17 has been removed from chapter 11 and now a combined and more simplified figure is included in Chapter 8 (fig, 8.6), including a separation of both physiological CO2 effects in the figure
102593	256	1	256	10	Larger dots needed. Unclear. [Philippe Tulkens, Belgium]	Rejected - This figure does not contain dots.
126209	256	1			[DROUGHT] Figure 11.18 expresses drought in terms of SPEI. It is stated that the SPEI is computed from the CRU "Epot". However, in the terminology of this chapter, Epot refers to AED, while the CRU dataset contains the FAO-56 reference evapotranspiration, which is one form of potential evapotranspiration, for a particular vegetated surface with fixed albedo, roughness, and stomatal resistance. The CRU variable is not appropriate for use with the SPEI as it has been defined in this document. [Trigg Talley, United States of America]	Accepted - replaced in the revised document.
113527	256		256		What about adding SEDI? [Diego Miralles, Belgium]	Rejected - There is not yet literature to assess trends in SEDI
10151	256				Fig. 11.18 caption says 3-month, but the figure panel titles say 12. Which one is correct?? This must be made consistent. [Jacob Scheff, United States of America]	Accepted - it is 12-month. Caption was not correct.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10153	256				Fig. 11.18 caption needs to make clear that the CRU Penman-Monteith Epot didn't account for physiological effects (Yang et al. 2019), and thus the SPEI trends pictured may be much too negative. [Jacob Scheff, United States of America]	Rejected - The figure is on observations. Independently of the important uncertainties of the possible CO2 effects on the surface hydrology (see cross-chapter carbon-water nexus), an effect of the observed CO2 trends would not be very relevant. See e.g. Figure 6 in Vicente-Serrano et al. (2020) WIres Climate Change 11, e632.
110079	257	3	257	4	Font size is far too small. Panel title could be expanded to Annual consecutive dry days decadal trends 1950-2018 [Peter Thorne, Ireland]	Taken into account - font size increased and title expanded.
82803	258	1	258	6	I have significant reservations over the use of CDD as a primary global drought metric because of its limited utility in climates which are seasonally dry (it doesn't tell you anything about what is happening during the normal rainy season, which is usually of more societal importance). It would be better to use a different drought metric here if possible. [Blair Trewin, Australia]	Rejected - We agree with the point by the reviewer but this metric may be also be useful in humid climates. Chapter 12 contains a figure with trends in the SPI, which refers to the severity of the precipitation deficits.
108927	258	1	258	10	Can you provide evidence that CDD and soil moisture deficit is forcing independent and scales with GSAT independent of the SSPs? [Erich Fischer, Switzerland]	Noted. This level of detail could not be provided because of space limitations, but the new Rejected. 11.1 on "Translating between regional information at global warming levels vs scenarios for end users" provides a detailed assessment on the literature evidence showing that changes in extremes as function of the global warming levels are generally independent of the scenario choice.
126211	259	1	259	5	Caption and figure itself disagree as to whether total or surface soil moisture (either one would need a definition) is what is shown. Which is it? [Trigg Talley, United States of America]	Editorial
10155	259	2			In Fig. 11.21 caption, "surface soil moisture" needs to be corrected to "total soil moisture" (a typo, but an important one!!) [Jacob Scheff, United States of America]	Editorial
70401	259				Figure 11.21: Projected changes in surface soil moisture for projections at 1.5°C, 2°C, 3°C and 4°C of global warming. Surface soil moisture is not the most relevant variable for drought and may differ from deeper soil moisture (Berg et al. 2017). Would be more appropriate to use a depth range that contains the rooting zone, could be either total column or integrated to 3m. [Abigail Swann, United States of America]	Accepted: A new figure on total soil moisture has been included
5705	260	1	260	7	Figure 11.22: Is this a preliminary figure? To give the statements, using a map of the world as background is not necessary, so in this case the figure can be deleted. [Joachim Rock, Germany]	Considered. The fig is re-done and convey a lot more information than a few statements.
117135	260		260		consider a representation of confidence (possible with dots, 1 for low, 2 for medium, 3 for high, as used in a panel in the SROCC SPM) [Valerie Masson-Delmotte, France]	Considered. But it was later decided to be explicit in the caption about confidence.
8061	261	4	261	5	The difference between standard normalization and corrected normalization will be obscure to many readers and is irrelevant for the main message of the figure. Please redraw the figure using just the corrected normalization. [jouni Räisänen, Finland]	Accepted - now only showing the corrected normalization.
110081	262	1	262	2	This looks like a piece of abstract art. It should be in standrad projection. The background map should be white with continental outlines. Overlapping colours should be able to be made out e.g. is the green splodge over western europe also covered by the larger red splodge? Title is required for a chance for this figure to work as a standalone piece. [Peter Thorne, Ireland]	Noted. This figure is deleted.
38371	263	1	263	6	The East Section of China-India Border is wrongly drawn and the Dotted Line of South China Sea, Nanhai Zhudao, Diaoyu Dao and its affiliated islands of China are missing in Box 11.3 Figure3. In order to avoid unnecessary disputes, it is suggested to delete the boundary lines from the Figure. [Yaming LIU, China]	Noted. The figure is reproduced.
111525	263	1	263	7	For more homogeneity betwen maps and figures, we suggest do not show boundaries of countries in some cases (Box 11.3, Figure3) unless we need to focus in specific countries. [Moulay Driss HASNAOUI, Morocco]	Noted. The figure is reproduced.
5707	264	1	264	8	Box 11.3, Figure 4: Please use the same reference periods in both panels of this figure. The later period has experienced some warming compared to the earlier reference period, so the comparison between both panels is exaggerated. [Joachim Rock, Germany]	Noted. This figure is deleted.
5709	266	1	266	19	FAQ 11.1, Figure 1: There is no part "c" given in the figure. [Joachim Rock, Germany]	Not applicable. This figure has been replaced.
17709	266	17	266	18	The bracketed sentence is not clear. What is “Parts a-c modified”? [Sridhara Nayak, Japan]	Not applicable. This figure has been replaced.
113529	266		266		These histograms are inconsistent with the disproportionate increase in T for minimum T respect to maxiimum T (see comment above). [Diego Miralles, Belgium]	Taken into account. This figure has zoomed in on the distributions and the larger change in cold extremes is now apparent.
62839	267		267		FAQ 11.2, Figure 1. Figure comment. Present and futre climate percentile lines are very similar. Maybe, the dashed lines representing the 95th percentile could be thicker for a better representation of the extreme's envelope. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This figure has been replaced.
108925	268	1	268	10	Why do the red and blue line overlap at the very left end of the panels. Is this really what we expect them to do? [Erich Fischer, Switzerland]	Accepted. The figure was showing the entire distribution and has been adjusted to show only the relevant parts, removing the mentioned overlap.
11775	269	1	269	70	On Figure 11.A.1, in these multi-panel figures, the world map insets are too small for readers to decipher the colors on the maps. [Amy East, United States of America]	Taken into account - figure redesigned. Maps now removed. Is Figure 11.SM1.
62901	269		269		Figure 11.A.1: consider making insert maps larger or remove them. It is not visible what the authors want to show in these maps in the current version when studying the figure panels in a standard paper size format. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - figure redesigned. Is Figure 11.SM.1 in FGD.
11777	271	1	27	70	On Figure 11.A.2, in these multi-panel figures, the world map insets are too small for readers to decipher the colors on the maps. [Amy East, United States of America]	Not applicable - figure removed
62789	271	1	271	4	The quality of this figure should be increased to can read the text inside bars at 100% zoom [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - figure removed
102595	271	1	271	9	Difficult to see regions on small maps. Please improve. [Philippe Tulkens, Belgium]	Not applicable - figure removed
102597	271	1	271	9	Link to regions figure missing. If it does not exist please add. [Philippe Tulkens, Belgium]	Not applicable - figure removed
102599	271	1	271	9	Not the same regions as in figure 11.2 [Philippe Tulkens, Belgium]	Not applicable - figure removed
62903	271		271		Figure 11.A.2: consider making insert maps larger or remove them. It is not visible what the authors want to show in these maps in the current version when studying the figure panels in a standard paper size format. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - figure removed
29497	Pag. 76	Line 5.	Pag. 76	Line 5.	Sec. 11.6.1.1 Precipitation deficits. Line 5. The ENSO has a role the main climate driver in the tropical regions at NAM (Méndez et al. 2010, Méndez & Magaña et al. 2010, Andrade-Velázquez 2017, Andrade-Velázquez & Medrano-Pérez in press). [Mercedes Andrade, Mexico]	Rejected - Too detailed.
29499	Pag. 76	Line 5.	Pag. 76	Line 5.	Méndez M and Magaña V. 2010. Regional aspects of prolonged meteorological droughts over Mexico and Central America. Journal of Climate, 23 (5), 11751188. https://doi.org/10.1175/2009JCLI3080.1 [Mercedes Andrade, Mexico]	Rejected - Too detailed and older study (2010)

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29501	Pag. 76	Line 5.	Pag. 76	Line 5.	Méndez-González J, Ramírez-Leyva A, Cornejo-Oviedo E, Zárate-Lupercio A, and Cavazos T. 2010. Teleconexiones de la Oscilación Decadal del Pacífico (PDO) a la precipitación y temperatura en México. Investigaciones geográficas, 57-70. http://dx.doi.org/10.14350/rig.23862 [Mercedes Andrade, Mexico]	Rejected - Too detailed and older study (2010)
29503	Pag. 76	Line 5.	Pag. 76	Line 5.	Andrade-Velázquez M, 2017. Visión climática de la precipitación en la cuenca del Río Usumacinta. La cuenca del Río Usumacinta desde la perspectiva del cambio climático., IMTA, 5775. Available at: https://www.imta.gob.mx/biblioteca/libros_html/la-cuenca-rio-usumacinta/files/assets/common/downloads/publication.pdf [Mercedes Andrade, Mexico]	Rejected - Too detailed and grey literature.
29505	Pag. 76	Line 5.	Pag. 76	Line 5.	Andrade-Velázquez, M. & Medrano-Pérez, O. In press. "Precipitation patterns in Usumacinta and Grijalva Basins (southern Mexico) under a changing climate" [Mercedes Andrade, Mexico]	Rejected - Too detailed and older study (2010)
29507	Pag. 85.	Line 15.	Pag. 85.	Line 15.	Sec.11.6.4.1 Precipitation deficits. Line 15. ..,at Mexico south the precipitation changes are ligh (Montero-Martínez et al. 2018, Andrade-Velázquez & Medrano-Pérez in press), anthropogenic climate change was not a dominant influence. [Mercedes Andrade, Mexico]	Rejected - The text is general related to the reliability of attribution studies
29509	Pag. 85.	Line 15.	Pag. 85.	Line 15.	Andrade-Velázquez, M. & Medrano-Pérez, O. In press. "Precipitation patterns in Usumacinta and Grijalva Basins (southern Mexico) under a changing climate" [Mercedes Andrade, Mexico]	Rejected - The text is general related to the reliability of attribution studies
29511	Pag. 85.	Line 15.	Pag. 85.	Line 15.	Montero-Martínez MJ, Santana-Sepúlveda JS, Pérez-Ortiz NI, Pita-Díaz O and Castillo-Liñan S. 2018. Comparing climate change indices between a northern (arid) and a southern (humid) basin in Mexico during the last decades. Advances in Science and Research, 15, 231237. https://doi.org/10.5194/asr-15-231-2018 [Mercedes Andrade, Mexico]	Rejected - The text is general related to the reliability of attribution studies
51643	Table 11.1				Under "drought events" at the top of page 11-21 (there are no line number sorry), I don't understand what "has increased the potential for worsening of drought conditions" means. Do you just mean, "has increased the potential for drought" or "has led to worse drought conditions"?+B54B53:H59B52:H59B51:H59B50:H59B49:H59B48:H59B47:H59B46:H59C45:H59B44:H59B43:H59H26B55:H59 [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Considered, text reworded.
126213					[CONFIDENCE] A general comment about the tropical cyclone assessment parts of Chapter 11, based on participation in the recent WMO Expert Team on TCs and Climate Change assessment (Knutson et al., 2019a,b). The TC sections reference much of the material covered in that assessment. However, the confidence levels for TC climate change detection (several metrics) and for TC climate change projections are typically higher in this SOD than in the WMO assessment. There are several possible reasons for the differences. First, there have been some new studies not covered in the WMO assessment, which can potentially alter confidence levels. IPCC AR6 does not use split confidence levels like "Low to Medium" so cases where those were given in the WMO assessment apparently need to be altered for IPCC AR6. In practice, the authors seem in general to pick the higher confidence level (sometimes not be justified, as highlighted in the line-by-line comments). Another possible reason for discrepancy could be a difference in what is meant by confidence levels and other assessment language between the two reports. That said, the confidence level guidance given by IPCC AR6 (page 1-27) corresponds to the "Type I error avoidance" assessment statements in the WMO assessment. In contrast, the "Type II error avoidance" statements in the WMO assessment -- which were introduced so that the WMO authors could have a "low bar" option for assessment when even "low to medium confidence" was not justified -- are apparently not used in IPCC, so the balance of evidence "Type II error avoidance" statements in the WMO report therefore correspond to "Low confidence" in the IPCC AR6 framework. The assumption here is that IPCC does not want to have an explicit consideration of avoidance of Type II error, and using a "low bar" option like balance of evidence for assessing detectable change. If that is so, then to be consistent with the WMO TC/climate assessment (Knutson et al., 2019a), any statements in that report are simply low confidence statements in the IPCC framework. Finally, differences between the reports could arise due to differences in author opinion on the confidence levels. In the WMO report, a distribution of confidence levels across the 11-member author team was provided, and the summary assessment statements were based on confidence levels that were adopted by a majority of the authors. IPCC AR6 presents a single confidence level statement in each case, which can lead to differences between the reports. In terms of difference of opinion, the two areas of disagreement are as follows: (1) for climate change detection, what type of evidence can lead to medium confidence vs. low confidence in a detectable change (i.e., where the observed change in highly unusual compared to expected changes from natural variability only); and (2) for projections, what type if evidence is needed for high confidence vs. medium confidence, for example. These points are addressed in the line-by-line comments on individual assessment statements in the WGI AR6 SOD, particularly for those that differ from the WMO assessment. Finally, in comparison to the recent WMO assessment (Knutson et al., 2019a), one peculiar aspect of the draft is that greater confidence is expressed in the AR6 draft of detection of positive (more potential damage) trends, but there are cases of decreasing TC activity. For example, the statistically significant decrease in extreme landfalling TC frequency in NE Australia and the nearly (not quite) significant decrease in U.S. landfalling hurricane frequency get no mention at all in the report, let alone an elevation to greater confidence. In a thought experiment, if these were pointing toward increasing frequency would the authors have treated them the same way? Any implications of this? [Trigg Talley, United States of America]	Noted. This comment along with other relevant specific comments on the chapter 11's TC assessment have been carefully considered. We have compared the recent WMO assessment and the assessment made in the SOD. We found a number of differences, mostly in how and what type of information is communicated and under what specific guidelines and practices. The difference is not as simple as "type I error avoidance" or "type II error avoidance" that should have rendered AR6 assessment to conclude "low confidence" when WMO assessment is "median confidence", as suggested in this comment. For example, the WMO assessment required a detection (and perhaps also attribution) of certain change for a high confidence in future projection. This treatment was also suggested a number of times in specific comments. But this is not required by the IPCC guidance. In fact, high confidence is given for projection in IPCC AR5 and AR6 assessments even if a similar change may not have been detected in historical climate. To address this and the other related comments, we have restructured the TC assessment section such that past changes, attribution, and projection are clearly separated. This makes it clear that assessments in Chapter 11 are constructed differently than the WMO assessment. More importantly, this makes the TC assessment internally consistent with the rest of Chapter 11, and the whole AR6 WGI report in which past changes, attribution and projections are in three separate chapters.
126215					There are many passages in this chapter where a statement is made about some kind of change in extremes happening "on global [or regional or continental or local] scale". The exact meaning of such a statement is not clear. Presumably it does not mean everywhere around the global land area. Does it mean over more than half the global area? Or over more than half of the area with significant change? Or...? [Trigg Talley, United States of America]	Noted. When it is stated in the chapter that a change (e.g. an increase) has occurred on global scale, it means that the change occurred over number of sites (or areas represented by the sites) is larger than would be expected by chance, usually far larger than over 50% of the region. For example, in terms of extreme precipitation, there are about 70% of sites showing increase. Among them, there are close to 10% of sites showing significant increase, less than 2.5% of sites showing significant decrease (Fig 11.13). As the number of sites with significant increase is far greater than expected by chance (slightly larger than 2.5%) and the number of sites with significant decrease not larger than expected by chance, extreme precipitation is assessed to have intensified on global scale.
126217					What is the significance of boldface in the Executive Summary? Does it mean "this is the first sentence of the paragraph," in which case it is superfluous, or does it mean "this is a statement that is about to be further explained?" It doesn't seem to be the latter, e.g., in lines 44-49 (and elsewhere), where the statement about seasonal timing is bolded, but what follows is about peak values. Nor does it appear to correlate with confidence level. [Trigg Talley, United States of America]	Noted. The bold face statements are overall statements that introduce the material of the respective paragraphs, either as summary of the following text or as salient statements related to the material of the overall paragraph. The ES text has been substantially revised and the bolded statements have been revised as well.
126219					The word "whereby" is used many times throughout the chapter, seemingly incorrectly. The intended meaning appears to be "whereas." [Trigg Talley, United States of America]	Noted. Incorrect uses of "whereby" have been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
126221					[DROUGHT] How is the reader to interpret terms like "dry," "drier," "drying trends," etc.? Just as with "drought," "dry" has many possible meanings (precipitation, atmospheric humidity, measures/components of PET, runoff/blue water availability). Ambiguity undermines the usefulness of many statements in the chapter and could cause misunderstanding. [Trigg Talley, United States of America]	Partially taken into account. The text has been revised and is now organized in different "drought" types. The terms "atmospheric dryness" has been dropped. The word "dry" is maintained in self-explanatory contexts.
126223					[DROUGHT] Chapter 11 text on the subject of drought is organized within a framework having two main drivers [precipitation deficits and increases of "atmospheric evaporative demand" (AED, defined in the text as the free-water evaporation rate)] and two main manifestations [soil moisture deficits and hydrological deficits]. Figure 11.17 partially reflects this framework, but with some departures: the figure adds plant water deficits, and it splits hydrological deficits (termed hydrological drought in the figure) into two components -- streamflow and storage deficits. One simple point is that it would be desirable to harmonize better the text organization and the figure. A related point is that terminology is not always internally consistent in the text. For example, occasionally the phrase "evaporative demand" or the notation "Epot" or "AED' is used instead of "atmospheric evaporative demand." This inconsistency impedes understanding of the text, especially in light of the facts that there exist other closely related quantities (specifically, potential evapotranspiration and equilibrium evaporation) and that each of these quantities has its own variants. [Trigg Talley, United States of America]	Accepted. The terminology has been harmonized. The term "atmospheric evaporative demand" (AED) is now used as unifying concept (also instead of Epot and other related measures). Note that the previous figure 11.17 has been now simplified and merged with a figure found in Chapter 8, and is no longer included in chapter 11 (see Section 11.6).
126225					[DROUGHT] The highlighting of AED as the only driver of evapotranspiration in the table of contents constitutes an implicit statement that the CO2 fertilization effect on plant water use (in fact, any characteristics or processess related to plants) is not a factor for the issue of drought. If this is the judgment of the authors, then it ought to be made more explicit and prominent in the document, at the very outset, justifying the singling out of precipitation and AED as the only drivers. An alternative to using AED as the highest-level organizer of text would be to replace it with something like "drivers of evapotranspiration." [Trigg Talley, United States of America]	Taken into account. AED is not highlighted as only driver of evapotranspiration (ET). The chapter team has made this point clearer in the text, also in the ES. The ES text now includes both "ET" and "AED" as acronyms to highlight their respective - and different - roles. The ES text also explicitly notes "Precipitation deficits and changes in evapotranspiration (ET) govern net water availability." Effects of other drivers beyond AED for ET are clearly addressed in the text in Section 11.6 (both CO2 but also feedbacks from soil moisture). The subdivision of atmospheric conditions between precipitation and AED is justified by the fact that increased AED is a demonstrated factor in amplifying drought in observations and projections, although it is not the only driver for ET and ET's response to AED is modulated by several other factors, including CO2.
126227					The assessment acknowledges the weakness of SPEI as an index of soil moisture, but gives it prominence as a measure of vegetation stress. This emphasis on SPEI should perhaps be reconsidered in light of other review comments suggesting the assessment overstates the role of AED and understates that of plant physiology. [Trigg Talley, United States of America]	Taken into account. The revised assessments is now organized by drought types, and both soil moisture changes and metrics based on the combination of precipitation and AED (such as the SPEI, but also PDSI) are considered in the assessment for "agricultural and ecological droughts". However, the assessment now also clarifies that metrics based on the combination of P and AED, such as SPEI, can overestimate drying in projections, and are thus quantifying a potentially amplifying driver (due to vegetation stress) but are not a sufficient driver for these types of droughts. This is summarized in the ES: "A lack of sufficient soil moisture, sometimes amplified by increased atmospheric evaporative demand (AED), results in agricultural and ecological drought"
126229					The focus in the chapter is on soil-moisture deficits and hydrological deficits as the main manifestations of drought, and the table of contents reflects this. The second of these clearly relates to impacts on "blue water" availability. The first, however, is not so much an end impact as a mediating factor between the root drivers and the impact on (natural or managed) ecosystem health/productivity. For this reason, and to the extent possible, it would be desirable also to elevate those ecosystem impacts ("greening," etc.) within the chapter framework. [Trigg Talley, United States of America]	Rejected. The terms "green" and "blue" water are not very established in the general public. In addition, this chapter focuses on droughts rather than general measures of water availability. For this reason, we have restructured the assessment around different drought types: "meteorological droughts" (precipitation deficits), "agricultural and ecological droughts" (soil moisture deficits, sometimes amplified by AED effects on plant stress), and "hydrological droughts" (streamflow deficits).
126231					[DROUGHT] Given the structure of the chapter (summary material up front, with details to follow), which is presumably imposed by IPCC, terms like AED are not defined until far into the document. Given that AED has various potential interpretations, this is problematic. If less ambiguous terms (radiation, VPD, stomatal resistance/conductance) were used, this problem could be alleviated. Rather than organize text around precipitation and AED as drivers of drought, it might be better to use water supply (i.e., precipitation), energy supply (radiation, VPD-wind/advected energy), and biophysics (particularly bulk stomatal resistance). If this gets into too much detail for an organizing framework, then perhaps "drivers of evapotranspiration" could replace AED. [Trigg Talley, United States of America]	Taken into account. The text has been simplified. The role of evapotranspiration (ET) is now prominently highlighted in the beginning of the ES text on droughts: "Precipitation deficits and changes in evapotranspiration (ET) govern net water availability". The role of AED as a potentially amplifying factor - rather than unique and sufficient driver - for agricultural and ecological droughts is clarified in another ES sentence: "A lack of sufficient soil moisture, sometimes amplified by increased atmospheric evaporative demand (AED), results in agricultural and ecological drought.". More details are also provided in the ES on the single drivers of AED: "Increases in evapotranspiration have been driven by AED increases induced by increased temperature, decreased relative humidity and increased net radiation (high confidence).". Finally, the potential effects of CO2 effects on plant transpiration have been mentioned, including the fact that they remain very uncertain in the context of very extensive droughts (assessment also prepared with other chapters in the Rejected. 5.1): "There is low confidence that effects of enhanced atmospheric CO2 concentrations on plant water-use efficiency alleviate extreme agricultural and ecological droughts in conditions characterized by limited soil moisture and enhanced AED. There is also low confidence that these effects will substantially reduce global plant transpiration and the severity of hydrological droughts."

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
126233					The chapter doesn't say much about snow, although snow is the source of water for a large fraction of the global human population. One papeer (DOI: 10.1126/science.aay9187) shows that warming-induced loss of snowpack, acting through the radiation budget to enhance ET, can have a strong impact on total amount (not just timing) of streamflow. Half of the hydrological drought (runoff reduction) in the Upper Colorado River of North America over the last 20 years can be attributed to regional warming, as this and other recent papers have shown. [Trigg Talley, United States of America]	Taken into account. Snow effects on droughts are now integrated in Section 11.6 (see answer to comment #2155). However, mean changes in snow do not belong in this chapter but rather in Chapter 9 (as part of the cryosphere) and Chapter 8 (water cycle).
126235					A case can be made that climate models, collectively, underestimate the extent to which evapotranspiration is suppressed by stomatal closure. This underestimate would be a consequence of an excessive negative feedback. The negative feedback is the enhancement of direct evaporation from soil that results when the reduced transpiration induces an increased VPD. This negative feedback depends on the connection of the surface soil moisture to the atmosphere. Evidence suggests that in many climate models the direct soil-atmosphere connection is too strong. Citations: DOI: 10.1175/JCLI-D-18-0583.1 doi:10.1002/2016ms000832 doi:10.1175/JHM-D-13-0162.1 [Trigg Talley, United States of America]	Noted. This cannot be generalized, there is indeed a tendency for models to underestimate stomatal control in normal conditions, but at a result of drying this control can then be overestimated. This is summarized in the Section 11.6 assessment: "Regarding the performance of regional and global climate models, an evaluation of an ensemble of RCM simulations for Europe (Stegehuis et al., 2013) shows that these models display too strong drying in early summer, resulting in an excessive decrease of latent heat fluxes, with potential implications for more severe droughts in dry environments (Teuling, 2018; Van Der Linden et al., 2019). Compared with a range of observational ET estimates, CMIP5 models show an overestimation of ET on annual scale, but an ET underestimation in boreal summer in many North-Hemisphere mid-latitude regions, also suggesting a tendency towards excessive soil drying (Mueller and Seneviratne, 2014), consistent with identified biases in soil moisture-temperature coupling (Donat et al., 2018; Vogel et al., 2018; Selten et al., 2020)."
126237					[DROUGHT] Chapter 11 text makes the case in a couple locations "that under critical soil moisture deficits CO2 effects on plant water savings are limited" and makes the summary assessment "that CO2 effects would reduce water needs by plants under non-extreme droughts but not under very extreme soil moisture drought conditions." These statements do not contradict, but overlook, the relevant fact that CO2-induced conservation of water prior to a potential soil-moisture drought would preempt the water shortage in the first place. It would not be necessary for the plants to reduce their water needs under very extreme soil moisture drought conditions if such conditions were prevented from arising, thanks to antecedent water conservation. Furthermore, these statements are located inside sections on AED, when in fact they are talking aboiut something external to AED. [Trigg Talley, United States of America]	Taken into account. The assessment is maintained and is based on the existing literature. But it is now addressed in more depth as part of the Rejected. 5.1 (written together with chapter 5 and other chapters).
126239					Have the authors considered/addressed somewhere in the text the possibility that increased atmospheric dryness (VPD) and the resulting enhancement of AED could be more a response to increased water-use efficiency (need for less ET) than a driver of increased ET? [Trigg Talley, United States of America]	Noted. This aspect is covered in the Rejected. 5.1 that is referred to in Section 11.6 and addresses water-co2 relationships (see sentence "the effects of stomatal closure on near-surface atmosphere that leads to increased air temperature and vapor-pressure deficits (Berg et al., 2016; Vogel et al., 2018; Zhou et al., 2019; Grossiord et al., 2020).")
117035					Chapter 11 is way overlength (by around 20%) and it must be shortened. Please make the best use of concise approaches, to shorten and sharpen the assessment. [Valerie Masson-Delmotte, France]	Accepted. The chapter has been shortened.
117041					Congratulations for the state of maturation of the chapter. Work is needed to reduce the amount of self-citation (to avoid the perception that chapter authors are assessing mostly their own work); better integration with other chapters of WGI on a number of issues; reducing length [Valerie Masson-Delmotte, France]	Noted. Thank you for the positive feedback. More literature has been cited, reducing the level of self-citation, there has been a lot of effort invested in the integration with other chapters and cross-chapter references. Length has been reduced.
117043					The introduction could be revised to strengthen the interactions of this chapter within the AR6 WGI report and integration with WGII (linking to related chapters). Is a coordination in place? [Valerie Masson-Delmotte, France]	Accepted. In this round of revisions, chapter 11 has had intensive coordination with other chapters, e.g. Chapters 8, 12 and Atlas on drought; chapter 5 and 8 on CO2-droughts-plant interactions; chapter 12 on fires; and chapter 3 on attribution of extremes. Links to other chapters have also been made more explicit in the text of the introduction.
117045					I am concerned about the thin line between WGI and WGII, and I think that some aspects of the chapter address what is the core part of WGII assessment. Please consider carefully in interaction with the corresponding WGII chapters how to best address the issue of floods and find an accurate terminology (streamflow?). [Valerie Masson-Delmotte, France]	Accepted. Coordination with WGII authors has been increased. The distinction of different types of floods is better clarified. This chapter addresses pluvial floods and river floods, coastal floods are addressed in chapter 9.
117047					The chapter has a number of specific choices such as using Tglob instead of GSAT anomaly, or identifying levels of temperature for emergence. Finally, it is mostly reliant on the RCP - CMIP5 framework while there has been a strong effort in chapters 1-9 to focus on SSPs. This needs to be carefully considered to avoid the perception of a chapter disconnected from the rest of the report. [Valerie Masson-Delmotte, France]	Accepted. "GSAT" is used where relevant. The use of global warming levels (GWLs) as dimension of integration is fully consistent with the introduction of different dimensions of integrations in chapter 1. The mapping between scenario-based projections and GWL-based projections is now addressed in the new Rejected. 11.1.
112699					interesting and engaging chapter. [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thank you for the positive feedback!
117051					In the ES, it is hard to find what is novel since AR5 - SR15. [Valerie Masson-Delmotte, France]	Accepted. New elements have been better highlighted.
117053					As the chapter is focusing on the period since 1950, it would make sense to report the current level of GSAT for a reader to understand how to use levels of warming. [Valerie Masson-Delmotte, France]	Accepted. This information is now indicated in the ES.
117055					Coordination is needed with chapters 5 and 8 on issues related to transpiration and CO2 effects on plants in a warming climate. These aspects are discussed in all 3 chapters but without a clear common message. [Valerie Masson-Delmotte, France]	Accepted. This text has been coordinated, in particular thanks to the new Rejected. 5.1 in Chapter 5. The chapter text refers to this Rejected. in several instances (ES and Section 11.6)
117057					Using SRCCL as a starting point could save space in several places. [Valerie Masson-Delmotte, France]	Accepted. The SRCCL has been used as starting point in several locations, in particular in Sections 11.1.6 and 11.8.3.
100419					General comment on the whole Chapter. More details for the South America regions are missing. Parts of the text just mention South America, without specifying which regions [Lincoln Alves, Brazil]	Accepted. More regional details have been included, also for South America. Note in particular the new regional tables in Section 11.9.
117059					I suggest to coordinate with other chapters on fire weather and make sure that a consistent picture is emerging, also building on SRCCL assessment. [Valerie Masson-Delmotte, France]	Accepted. The text on fire weather is now referring to the SRCCL in Section 11.8. References to Chapter 12 have also been included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
117063					Check the use of the terms "impact" and "risk", sometimes not in the right sense. [Valerie Masson-Delmotte, France]	Accepted. "Risk" is only used in a few well-justified instances in the revised chapter.
62791					General comment for Confident Assesment language: Please consider to replace the confidence assesment category "Extremely Likely", used in the previous AR4 and AR5, for either "Virtually Certain" or "Very Likely" categories suggested for the AR6. Examples: Page 6, L43; Table 11.1 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. "Extremely likely" is an official category of the IPCC uncertainty language.
117071					Coordination with other chapters on ENSO is needed. Please refer to extreme El Nino as introduced in SROCC where relevant . [Valerie Masson-Delmotte, France]	Accepted. Box 11.4 which addresses extreme El Nino is now referring to the SROCC.
117073					Coordination with other chapters is needed on global dimming, aerosol forcing and related effects. I would expect that different levels of warming but with different types of forcing (more or less aerosols) would lead to different changes in extreme precipitation due to aerosol effects. Could this be explored with the diversity of SSPs? [Valerie Masson-Delmotte, France]	Taken into account. Some of these aspects are addressed in the cc-box 3.1. Some literature on aerosol effects on precipitation extremes indicate effect of aerosols to be different from that of GHG in terms of temperature scaling, but what considered to be extreme precipitation in the papers is quite moderate. Sillmann et al. (2019) analysed simulations of Precipitation Driver and Response Model Intercomparison Project and found that for extreme precipitation that occurs once a year or less frequently, the magnitudes of the rates of change per 1°C change in global mean temperature are similar regardless of whether the temperature change is caused by increases in CO2, CH4, solar forcing, or SO4. However, the literature of effects of aerosol forcing on extremes is generally more limited than for effects on mean climate.
117079					Please refer to SROCC for the overview of attribution associated with some ocean related extreme events (table in SROCC ch 6). [Valerie Masson-Delmotte, France]	Rejected. Ocean extremes are not addressed in this chapter but in Chapter 9. The scope of the respective chapters and the fact that ocean extremes are addressed in Chapter 9 in now better highlighted in the chapter (ES and Introduction).
117081					The chapter refers to megadrought but does not provide insights on megadroughts (to coordinate with ch 8 on use of paleo evidence?). [Valerie Masson-Delmotte, France]	Considered. This is not a main topic for chapter 11, but it is mentioned in Section 11.6 for completeness
117087					pelase correct, the aim of the Paris Agreement is not 1.5°C. [Valerie Masson-Delmotte, France]	Accepted. This is fixed in the final version of the chapter.
117089					for the concept of global temperature of emergence, could an uncertainty range be provided? [Valerie Masson-Delmotte, France]	Noted. It would make the figure very heavy to include all the ranges of uncertainty. Some estimate of uncertainty is provided by having both the CMIP5 and CMIP6 estimates.
81255					Thank you to the authors for the work done [Fatima Driouech, Morocco]	Noted. Thank you for the positive feedback!
117095					A cross cutting assessment related to SLCF - aerosols could be relevant [Valerie Masson-Delmotte, France]	Noted. More details on aerosol effects have been included but literature is limited. This topic rather belong in the scope of Chapter 6.
81257					Howver it is really not very understable why floods are included among climate events. Floods results from climate events! Would be more appropriate to say for example "floods linked events/extremes". Flood assessment is for WGII and not WGI. [Fatima Driouech, Morocco]	Rejected. Several types of floods clearly belong in the WG1 assessments, for instance pluvial floods are solely determined by precipitation. To address the concerns of the reviewer, the distinction of different types of floods is however now better clarified. This chapter addresses pluvial floods and river floods, coastal floods are addressed in chapter 9.
81261					Box 11.3, Figure 3. Please use correct countries borders if there is need tu put borders (the focusof the report is more on global and regional). Ovoiding misundertanding here would increase the use of this interesting map among decision mackers and several users [Fatima Driouech, Morocco]	Not applicable. This figure has been replaced with a single figure not include country borders.
117101					I think that GMST is used several times instead of GSAT. [Valerie Masson-Delmotte, France]	Noted. Use has been harmonized. GSAT has been used when appropriate.
6779					There are references to "future projections" in quite a few places in this chapter. Most can probably be changed simply to "projections". See comment 9 on the entire report. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. A literature search has shown that the term "Future projections" is often used. It also seems more easily understandable for the general public than the word "projections" alone.
71295					In summary, it is better to show regions mentioned (e.g. P.8, L.16-17) [Kenji Taniguchi, Japan]	Accepted. The regions have been made now explicit in the ES (with a mention of the AR6 acronyms).
29329					very good work. [Zangari del Balzo Gianluigi, Italy]	Noted. Thank you for the positive feedback!
112829					Great chapter, very clear ES, great step forward in knowledge, really clearly presented [Maarten van Aalst, Netherlands]	Noted. Thank you for the positive feedback!
62413					Fig 11.7 , Inclusion of the statistical sigificance of the trends will be helpful. The fot size is also too small [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Statistical significance is now included. Figure re-designed with larger font size (Figure 11.9 if the FGD).
62415					Figure 11.14: Caption does not mentio which RCP was utilised in this case [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Caption now lists the used scenarios. (Figure 11.16 in FGD)
62417					Figure 11.18: Title has SPI -12 or SPEI 12 but caption says 3 months Observed trends in drought severity and frequency obtained from 3-month SPEI and SPI [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. Caption and Figure title is now correct. (Figure 11.17c in the FGD).
62419					Figure 11.19: Useful to add stripling were the trend is statistically significant [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Statistical significance is now included. (Figure 11.17a if the FGD)
112085					The interactive Atlas provides flexible scenario/period and warming level projections (fpr CMIP5 and 6) for a number of indices described in Ch11 (TXx, TNn, CDD, Rx1day) and used both in Ch11 and Ch12. Some of the Ch11 figures can be reproduced in the Interactive Atlas (Box 11.1 - Fig 1a, 11.9, 11.10, 11.14, 11.20) and thus they can provide support for extended information by proper cross-referencing. Coordination is needed in order to ensure consistency. The Interactive Atlas includes also CORDEX data so there is an opportunity to collaborate with Ch11 (as it is done in the SOD with Ch12) to extend assessment for regional information, or at least to assess key differences resulting from CORDEX as compared to the global model data. [jose manuel gutierrez, Spain]	Accepted. A reference to the Interactive Atlas is now indicated in the text.

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115925					FAQ11.1 Why is the FAQ focused on projections and not observations. What about the change in intensity of extreme events / mean warming (this was reported in SR15). A reference to amplification in cities could make the link with FAQ10.2. I am not convinced that the paragraph lines 42-46 addresses the FAQ topic. [Valerie Masson-Delmotte, France]	Considered. FAQ11.1 has been heavily edited. It is based on both past changes and future projections to illustrate that changes in local surface temperature extremes follow closely the corresponding changes in local average surface temperatures but changes in precipitation extremes may not follow those in average precipitation. A reference to amplification in cities is not made to simplify the main message.
62421					Figure 11.20: No mention of scenario that is being utilized [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Caption now lists the used scenarios. (Figure 11.19 a-c in FGD)
115927					FAQ11.2 I find the flow of information hard to follow in this FAQ (too many ideas). I am not sure that some aspects are fully traceable to the WGI assessment (eg past adaptation). Aspects linked to recurrence / recovery time are not fully adressed (eg coral reefs). Insights on compound events + simultaneous events are missing. Ex heat wave + air pollution, or extreme sea level + extreme rainfall, or new events linked to high mountains (eg hazards at different places or different seasons), or new events in oceans (loss of oxygen + acidification + heat wave). I would suggest to better reflect insights x chapters incl ch 9 here. The figure is complex and not described in the text (storylines? what is the main message of that figure?). [Valerie Masson-Delmotte, France]	Considered. FAQ11.2 has been heavily edited. The message is simplified to illustrate that extremes that are unprecedented, either in magnitude, frequency, timing or location will occur in the future with warming and the frequency of these unprecedented extreme events will increase with increasing global warming. The figure is replaced with a new figure illustrating the concept.
62423					Figure 11.21: No mention of scenario that is being utilized [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Caption now lists the used scenarios. (Figure 11.19 d-f in FGD)
115929					FAQ11.3 The beginning is too generic and vague. There are repetitions with other FAQs (esp 11.1 and 11.2, also cities and SLCF) and the text does not fully provide a response to the question. Insights from paleoclimate information are missing (very rare events part of natural variability). I suggest to better explain how it is possible to explore how events have been modified in a changing climate (event attribution). I do not understand the link betwen the text and the figure (too much overlap with FAQ11.1). [Valerie Masson-Delmotte, France]	Considered. All FAQs are carefully revised and iterated under TSU's coordination. Texts are heavily edited to reduce overlap among FAQs. FGD FAQs' Figures are very different. Figure in FAQ11.1 is replaced with maps to show difference between changes in mean and extremes while the figure for FAQ11.3 is also redesigned to improve readability.
62425					Ethiopia Droughts text box incorrectly placed [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Not sure what this comment is referring to. There is not Ethiopia drought text box.
33257					In this chapter are assessed the weather and climate extremes of almost all landmasses of the earth except Antarctica. There are not direct attribution studies in Antarctica (except, as far as I know, Massonnet et al. 2015 in BAMS about the sea ice extent, a climate component assessed in chapter 9). However, there are some studies about extreme temperatures in Antarctica driven by Fohen winds, stratospheric circulation or low-level circulation. I do not suggest including Antarctic extreme events in this chapter, but I think that it could be stated somewhere that they won't be assessed in this chapter due to the limited evidences. [Gonzalez Sergi, Spain]	Noted. This chapter is focusing on inhabited regions and is thus not covering extreme events in Antarctica. This scope is now clarified in the ES.