Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15619	0	0	0	0	This is a very interesting chapter, with a clear methodological focus. Therefore, I think it should not be interspersed with assessment of climate change observations and future projections, which would better fit to Chapter 12. For example, Cross Chapter Box 10.3 would better fit in Chapter 12, and I think that statements from this Box should not be used in Chapter 10 ES. I think this chapter could be exclusively methodological, without riskng to duplicate assessments meant to be performed in other Chapters such as Chapter 12. [Samuel Morin, France]	Taken into account. The chapter is methodological, and to exemplify the different methodologies assessed we use illustrative examples. Whenever statements are brought from the illustrative examples to the ES, we have not made clearer that the statement are to illustrate methodologies, not to assess the climate change of the example region.
54365	0	0	0	0	What about machine learning methods (neural networks, deep learning)? They are brand-new, but should be at least mentioned, that they excist. In the whole chapter there are two citations referring to the outcomes from these methods. [Gabriel Stachura, Poland]	Noted. We acknowledge the emerging use of these methods, but note that their likely caveats have not yet been properly assessed in a regional climate change context. We have added some more references but at the same time believe it would be premature to bring them more into focus.
68975	0	0	0	0	Use serial (oxford) commas throughout entire document. [Seth McGinnis, United States of America]	Editorial – copyedit to be completed prior to publication
77115	0	0	1	70	UN standard norms and references for countries and regions should be used throughout. [Emer Griffin, Ireland]	Editorial – copyedit to be completed prior to publication
41907	0	0	236	55	There are too many mentions of "message" in the whole chapter, including section headings (as many as 165 in the whole chapter). This tends to reflect an over-emphasis of communication effort as opposed to scientific assessment of the current status of knowledge in the area. [Rupa Kumar Kolli, India]	Accepted. The balance between the reference to climate information and climate message has changed, with no references to climate message in the FGD to avoid using a term that has been considered loaded and leading to confusion. The main reason for the change is that, as the reviewer points out, the chapter focuses on the assessment of climate information. The concept of climate message is important though because it allows to introduce the phases that climate information goes through, in a collaborative process between the climate information producer and the user, whenever possible and necessary. For this reason, we have kept the discussion about the importance of involving the climate information producer in a dialogue with the users to co-produce what is needed for a better decision making.
66299	0		0		Being CH10 the first of the regional chapter it should serve a little bit more as an introduction to the following chapters. The choice of the several regions for attributions is not really justified or needed since these regions are not the same as in the other regional chapters, but they only serve as an example. [Erika Coppola, Italy]	Accepted. Chapter 10 now contains an introduction to the other regional chapters in section 10.1, with figure 10.4 now illustrating the links between them. The examples for past regional trend attribution have been reduced in number and now serve the purpose of illustration that was originally considered.
132377	0				It would be useful if Chapter 10 could introduce the concept of "regional climate sensitivity" proposed in Seneviratne and Hauser, 2020, Earth's Future. It was found that inter-model uncertainty in regional climate sensitivity is generally contributing more to the uncertainty of projected changes in extremes than uncertainty in global climate sensitivity in the CMIP6 ensemble. This shows how critical the representation of regional processes in climate models is for regional projections of extremes and impacts. Reference: https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2019EF001474 [Sonia Seneviratne, Switzerland]	Accepted. The concept has been introduced in section 10.1 and used in the examples of section 10.6.4 making reference to Ch 11.
67887	0				This chapter refers to previous IPCC documents (AR5, SR 1.5, SROCC and SRLCC) as well as Chapters 4, 5 and 9 on a global scale to be brought to the regional scale. This chapter is to bridge the information gap from the global scale to the regional scale (regional climate change effects in each location that need to be adapted by the community). [Ruandha Agung Sugardiman, Indonesia]	Noted.
67889	0				The deciding factors at the global scale need to be adjusted to the regional scale through the development of methodology and modeling. Regional climate change is caused not only by anthropogenic but also by natural causes. [Ruandha Agung Sugardiman, Indonesia]	Noted, thank you that is correctly understood.
67891	0				This section needs to be enriched with information about problems, guidelines or identification of aspects related to downscaling of global climate modelling to regonal and even to local level. This is because models at the global may not be applicable at regional and local levels. Besides, governments at regional and local levels need to take decisions related to climate change. [Ruandha Agung Sugardiman, Indonesia]	Noted. We don't know which section the reviewer refers to. Section 10.3 address the issue referred to. The mandate of IPCC is not to provide guidelines but to assess literature.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
114741	0				stating the obvious, but this chapters has many interfaces with earlier and later chapters. I encourage teh authors to pay attention to consistency and collaboration across chapters on the variois relevant sections [Jan Fuglestvedt, Norway]	Accepted. Section 10.1 includes a description of the flow of information through the regional chapters and their links to previous chapters. A new figure has been included to offer a simple map to identify the relevant chapters for regional climate and their links. Links to previous and later chapters has been introduced when appropriate.
114745	0				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 strenthne the report if these (to the extent possible) are also considered in ch10. That will also support a better integration across chapters in the TS, SPM and also finally in SyR [Jan Fuglestvedt, Norway]	Taken into account. In section 6, we added outcomes of other scenarios where they added to the discussion and did not foster confusion with the varied sources of information (GCMs, RCMs, large ensembles). This was feasible for the Mediterranean case.
96085	0				The chapter is somewhat confusing using sub-sections up to the 4th order (10.3.1.4.1) which makes it hard to follow sometimes. [Nicole Wilke, Germany]	Taken into account. We have reduced the fourth order as much as possible, and in the FGD it is only employed extraordinarily in Section 3.
115285	0				Chapter 10 struggles with the use of AMV. The recent literature has moved to adopt (largely) the use of AMV to describe secular SST variability, arising from natural and/or externally factors (see DOI: 10.1126/science.aaa4840 for some discussion of this). The chapter uses this in it's correct secular usage when referring to the impacts of AMV variability on regional climate impacts (e.g page 80, in discussion on SST variations on S. American climate) -- though it does still sometimes forget that these AMV changes may be themselves forced (e.g. it often contrasts a paragraph discussing AMV impacts with a paragraph describing forced drivers -- when they may in fact be driven by the same changes). But the chapter currently fails when discussing the causes of AMV changes -- e.g. page 116, line 55 incorrectly states that AMV is an internal mode of variability. This is at odds with the line taken elsewhere (for example, page 75, lines 2-6, clearly states that there is "medium confidence" that the patterns of SST are themselves driven by anthropogenic emissions -- AMV is one area where this evidence is strongest). There is clearly still some debate about the relative roles of forced and anthropogenic forcings (e.g. https://www.nature.com/articles/nature23538) -- but I don't think it is on for a IPCC chapter to attribute AMV as an internal only variability process when the current literature suggests that the external forcing driving the larger fraction of AMV changes remains credible as a hypothesis. The list of attributions of the AMV as a natural only mode of variability are extensive within the chapter. Each of these needs to be looked at and rewritten where these refer to AMV as a natural mode of variability. I can provide a wider more uptodate literature review of natural and forced drivers of the AMV, if this would be considered useful by the chapter authors? [booth ben, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. In Section 4, the relevant text has been modified to acknowledge that the AMV variations can be due to both external forcings and internal variability. References to the assessment performed in chapter 3 about attribution of AMV changes have also been added. The brief statements in 10.6.3 referring to AMV has been modified with the caveat that as aspect of the variability may be anthropogenically forced. The FGD version of the chapter refers to the more authoritative discussion of the AMV in the Annex.
22883	0				Mis-use of the term error is pervasive. Error implies that the truth is not just knowable but known. This is never attainable in practice so it is better to talk about uncertainties and random and systematic effects but avoid the use of the term error. [Peter Thorne, Ireland]	Rejected. We need to use error (on top of uncertainty) because we know for sure that some things are wrong in the simulations as a consequence of the errors in the models (otherwise, models would be perfect, when we know they are not).
22887	0				Chapter should likely change GCM to ESM for consistency with remaining chapters throughout the report thus far. [Peter Thorne, Ireland]	Taken into account. It would not have been correct to change to ESM since many of the global models we cite are not ESMs. We have chosen to use "Global Models" for both AOGCMs and ESMs (See introduction to Section 3).
22939	0				The regional storylines presented in 10.4.1.2 are really interesting and packed with detail but tend very strongly to review over real synthesis and assessment and many lack a coherent narrative arc. It would be critical in revisions to try to really synthesise the evidence (the job of the assessment) instead of write a literature review, and to try to tell the story for each region in somewhat the same order to help the reader to really draw out more clearly the similarities and differences across the case studies. [Peter Thorne, Ireland]	Accepted. The number of examples has been reduced to three and the narrative for each example has been reduced and changed to reflect an assessment rather than a review

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
34717	0				"There are some uncertainties associated with rainfall projection over the East Mediterranean (EM) region. (Alpert et al., 2008) predicted an increasing trend in precipitation over south and central Israel. The trend in A2 and B2 scenarios indicated extreme events as well as drier and wetter conditions in the upcoming years. Using a regional model, (Hochman et al., 2018) have predicted an increasing trend in winter and spring precipitations (~40% under Representative Concentration Pathway RCP4.5 scenario) in south Israel. (Ajjur and Riffi, 2020) examined the trends in 11 extreme precipitation indices in Gaza Strip (Palestine). Most indices increased during 1974-2016. Total precipitation, for example, has risen over two periods 1985-2004 and 2009-2016. References Ajjur, S., Riffi, M., 2020. Analysis of the observed trends in daily extreme Precipitation indices in Gaza Strip during 1974–2016. International Journal of Climatology. https://doi.org/10.1002/joc.6576. Alpert, P., Krichak, S.O., Shafir, H., Haim, D., Osetinsky, I., 2008. Climatic trends to extremes employing regional modeling and statistical interpretation over the E. Mediterranean. Global and Planetary Change 63, 163-170. https://doi.org/10.1016/j.gloplacha.2008.03.003. Hochman, A., Mercogliano, P., Alpert, P., Saaroni, H., Bucchignani, E., 2018. High-resolution projection of climate change and extremity over Israel using COSMO-CLM. International Journal of Climatology 38, 5095-5106. https://doi.org/10.1002/joc.5714." [Salah Ajjur, Qatar]	Noted. Does not seem to apply to our chapter.
22949	0				Please call Berkeley Earth Berkeley Earth and cease and desist with the use of the value-laden acronym BEST which has no place in an IPCC assessment. [Peter Thorne, Ireland]	Accepted. "Berkeley Earth" has been used for the FGD.
110551	0				General comment for Chapter 10 - I am surprised by the lack of discussion about how to evaluate the qualitiy of future projections. It seems like there is an assumption that if the models capture the observatoins well, they will be credible for use in future climate change studies. While this is often an assumption made, I would argue many are trying to push back against this for more process level assessments and checking that the future changes in processes make sense. [Rachel McCrary, United States of America]	Noted. Chapter 10 has a dedicated Section on fitness for projections (Section 10.3.3.10 in the SOD), which goes far beyond what previous reports have assessed, and actually also beyond what the corresponding global Chapter assesses. In addition we provide a discussion of trend evaluation and a discussion of uncertainty assessment.
22757	0				There are a lot of likelihood / confidence language uses that have not been italicised. Either italisice them or replace with non-likelihood / confidence terms [Peter Thorne, Ireland]	Editorial – copyedit to be completed prior to publication
22773	0				In general the chapter does not back cross-reference to the 9 preceding chapters sufficiently. It should reference these chapters and where possible start from their assessment findings going on to add necessary regional detail. There are several cases where a quasi-redundant assessment is performed. This is problematic because it is generally less in-depth and invites readers wishing to discredit the report to play spot the difference. I would strongly urge better attempts to link back to the 9 substantive assessment chapters that precede it in the FGD. [Peter Thorne, Ireland]	Accepted. Substantial effort has been made to start our assessment with a starting point in the previous 9 chapters.
23029	0				As is the case for many other chapters the figures require work to be more self-describing. Several of the present figures lack necessary titles and other aspects which would enable their use in public outreach and in educational lectures. With minor additional effort the figures could in many cases be made much more accessible. I have called out a few cases specifically but this is a generic issue cross-cutting most figures in the chapter. [Peter Thorne, Ireland]	Accepted. All figures have been redrawn taking this recommendation into account. Some figures are complex given the main objective of the chapter of illustrating how lines of evidence are integrating into information and it is difficult to make them self-describing, but an effort has been made according to the reviewer's recommendations.
79443	1	1	1	1	I would try to avoid as much as possible the use of the fifth-level in the numbering of sections. For example, different regions inside a section can be separated by starting a new paragraph, without the need of a new, explicit, subtitle. This would reduce the currently VERY long outline. [Alejandro Di Luca, Australia]	Taken into account. We have reduced the fourth order as much as possible, and in the FGD it is only employed extraordinarily in Section 3.
79445	1	1	1	1	I don’t understand very well the framing of the chapter around the concept of "message" instead of "information". I am sure you have good reasons for this but I don’t think they are clear when reading the chapter, even after reading the subsection 10.1.3. Most of the chapter assess tools that provide climate information, not messages. I understand that there should be a section discussing how to go from information to messages but again, in my opinion, most of the chapter is about information. [Alejandro Di Luca, Australia]	Accepted. The balance between the reference to climate information and climate message has changed, with no references to climate message in the FGD to avoid using a term that has been considered loaded and leading to confusion. The main reason for the change is that, as the reviewer points out, the chapter focuses on the assessment of climate information. The concept of climate message is important though because it allows to introduce the phases that climate information goes through, in a collaborative process between the climate information producer and the user, whenever possible and necessary. For this reason, we have kept the discussion about the importance of involving the climate information producer in a dialogue with the users to co-produce what is needed for a better decision making.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79447	1	1	1	1	I didn't find much discussion about why do we care about regional scales. I think this is important because answers to this question will differ across the various tools described to produce regional infromation. GCM developers might want to run at higher resolution because they want to simulate better large-scale variables (e.g., improve teleconnections). Someone developing a statistical downscaling technique might be interested in producing a time series of precipitation with local statistical properties. The different aim of each technique will probably influence what each of them is good at. [Alejandro Di Luca, Australia]	Noted. While it is clear that there are different tools to address the production of climate information, the regional scale is defined by the context, which is provided by the user. The chapter addresses the relevance of the different methodologies for the generation of climate information given the context, which could be the question from either a user or from a community. Different methodologies (GCMs, statistical downscaling, process understanding, etc.) are adequate to address different questions. As the chapter explains, the interest on the regional scales is not given by the tools, but by the question formulated.
4005	1	1	90	2	This chapter needs to be revised substantially. It should focus on observations. The observation should more emphases the atmospheric observations especially the long-term surface climate records for major continents and countries. It should inform the readers of key facts and possible causes of regional climate change and variability, including mean climate and extreme climate trends over the past decades to a century, which are the basis of modeling and projection as impact assessment in the following chapters. In this regard, the publications on regional climate change observed in major countries, regions and continents should be carefully collected and read. A lot of such publications, including those for big countries like USA, Canada, China, Europe, Indian and Australia, key regions like Arctic, the Tibetan Plateau, East Asia Monsoon area, the Mediterranean Sea, The Caribbean Sea, and major continents like Asia, North America, Africa and Europe, have mostly been missing. The papers published in some important special issues of international journals (e.g. Climate Change, Climate Research, and Advance in Climate Change Research) on the regional climate change and extreme climate change have not been assessed and cited. The authors should spend much more time to search and summary these publications. [Guoyu Ren, China]	Rejected. Please see the scope of the report: https://wg1.ipcc.ch/ar6.
1587	1	1	132	1	I have only looked at part of this Chapter, as well as looking at Ch 2 as well. Chapter 10 reads much more like a scholarly review than Ch 2 which synthesises information down to an Assessment. This chapter seems far too long, and parts of it read like a textbook on how to do Regional Climate Studies. I'll point out where these are more obvious in the parts I've read. Also this chapter makes comments and discussion about data sources. Surely much of this should be in Chapter 2. Some continents may have fewer records and less digitised than others, but Ch 2 showed these are good enough to produce large-scale averages. I know there are differences in timescales, but Ch 2 looked at changes in the hydrological cycle and in atmospheric circulation changes. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Chapter 10 is a methodological chapter with no precedent in IPCC WG1. This makes some "textbook"-like passages inevitable, but we have followed the reviewers recommendation and reduced these to a minimum. As to the section 10.2 on regional observations and the comparison the reviewer makes with Ch2 statement that some data sources "are good enough to produce large-scale averages", there is no real contradiction that a data source can be used for large-scale averages but not for the finer scales of interest of the regional chapters (e.g. the city scale).
1421	1	1	132	51	The chapter overlaps (e.g. data/observations, aerosols, cryosphere) with the other chapters I have read (2, 8, 10, 11 & 12), and I suggest that all chapters coordinate to avoid too much repetitive text/subjects. Having said that, I found this chapter much clearer and better written than the other chapters, so my suggestion is to move the repeated segments to other chapters and merge them. Many of the figures/illustrations are a bit complicated and challenging. It may be helpful to emphasise what message they are supposed to convey. The chapter seems to dedicate uneven space to different topics; some less important topics get more space than others that may be more important. [Rasmus Benestad, Norway]	Taken into account. Overlaps have been reduced for the FGD to a level that allows the Chapters to be stand alone pieces. The figures of the Chapter has been for the FGD reviewed to be more self-describing.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1423	1	1	132	51	I represent an active downscaling group who has taken a different path to ESD to the rest of the community. We have pioneered the use of common EOFs (Benestad (2001); DOI: DOI 10.1002/joc.703) as a way to deal with uncertainties connected to imperfect GCM representation of the predictors, among the first to apply ESD to multi-model ensembles (Benestad (2002); DOI: 10.1175/1520-0442(2002)015<3008:EDMETA>2.0.CO;2), and to downscale parameters for a cumulative probability distribution (cdf) to quantify the probability of heavy precipitation (Benestad, 2007; DOI: 10.3354/cr00693). We have chosen a different path to the mainstream downscaling after close collaboration with statisticians. It is common to apply downscaling on a day-to-day 'weather' basis rather than taking a 'climate' approach where parameters of probability curve are subject to downscaling. Using a common EOF framework and 'climate' approach makes it difficult to use the criteria decided in VALUE and CORDEX-ESD which assume the 'weather' approach, but our work nevertheless demonstrates good results. We have made further progress in downscaling by applying it to e.g. storm track density (Parding et al., 2019; DOI: 10.1175/JAMC-D-17-0348.1), using the combination RCM and ESD to assess the effect of ensemble size on the downscaled results (Mezghani et al., 2019; DOI: 10.1175/JAMC-D-18-0179.1) and evaluating the ensemble behaviour of the downscaled multi-model ensemble (Benestad et al., 2016; DOI 10.1088/1748-9326/11/5/054017). I'm aware of differences in opinion about downscaling and contented points of view with some of the chapter authors, but I'm also a bit surprised and disappointed that none of the work from our research group has been mentioned here. This report would not be objective without a fair representation of the work carried out. [Rasmus Benestad, Norway]	Taken into account. The so-called "climate approach" is not very different from the "weather approach" as the latter one downscales weather statistics (aka climate) and then draws weather time series (which are actually often required by impact modellers). Also the evaluation framework used in initiatives such as VALUE are not interpreting individual weather series but derived climate statistics. The use of common EOFs is a nice technical feature, but does not provide any fundamentally new approach. References to both have been added though at an appropriate level of detail. Note also that already several papers by the reviewer had been cited in the SOD and that we have to provide a broad overview of research conducted by many active and equally important statistical downscaling groups all across the world in basically two paragraphs (one introducing methods, one providing a performance assessment).
69795	1	1	132	51	In many places of the chapter where aerosols are referred to could potentially use the term SLCF in line with AR6-Chapter 6. Sections 10.1.4.1.4; Section 10.3.1.3.1 and Section 10.3.3.2; section 10.3.3.7.1; [Bhupesh Adhikary, Nepal]	Taken into account. We have tried to distinguish between the different SLCFs, as they have different impacts for different regions. SLCF is now used once in the chapter in Cross-Chapter Box 10.3.
83381	1	1	236	6	Again and from a sea ice perspective, why does this chapter focus so heav ily on Arctic sea ice - with virtually no mention of Antarctic sea ice? [Robert Massom, Australia]	Taken into account. We have now made clear that this cross-chapter box is an example and that the comprehensive assessment is performed in other chapters as described in section 10.1
109765	1	1	236	70	The terms "GE" and "LE", both meaning large ensembles of climate model simulations, are used inconsistently throughout the chapter and figure labeling. I suggest to call it "LE" everywhere, it's the established term in ther literature. "GE" refers specifically to an ensemble with 100 members, which basically only applies to the MPI-GE. It's confusing to suddenly call the CESM1-LE (n=40) a "GE". [Flavio Lehner, Switzerland]	Taken into account. In the FGD we use "SMILE" (Single Model Initial-condition Large Ensemble).
1959	1	19	1	19	Goosse' is with 2 s (instead of Goose) [Hugues Goosse, Belgium]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
45135	1	23	1	23	Typo. Replace "Ravaghavan" by "Raghavan" [Krishnan Raghavan, India]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
17631	2		6		as for the FOD, I feel that this chapter 10 is strongly oriented towards regional climate over land with very little attention to regional climate information over the sea. From the chapter content, I feel that regional ocean models are not assessed, nor the model performance over the sea (air-sea fluxes, winds over the sea, ocean currents) or the ocean physical phenomena (ocean deep convection, eddies, …). We may give the same comments for the regional sea ice component. It is likely too late to modify it but this may underline an author selection bias [SAMUEL SOMOT, France]	Noted. For space reasons, a decision was drawn already in the beginning of the writing process that the Chapter focusses on climate over land and addresses the performance of ocean models only when it is relevant for climate over land.
17633	2		6		in general I like the chapter structure [SAMUEL SOMOT, France]	Noted with thanks
17627	3	3			the list of sub-component models seems strange at a first look. No mention of « land-surface models » nor « regional ocean/sea models » that are likely as important as lake models to determine regional climate information [SAMUEL SOMOT, France]	Not applicable. Section has been removed.
17629	3				in the list of approaches, I miss the approaches mixing dynamical and statistical approaches (statistico-dynamical approaches or statistical-dynamical downscaling approaches), such as for example in Najac et al. 2011, https://doi.org/10.1002/joc.2075 and Beaulant e t al. 2011, doi: 10.1002/qj.796 [SAMUEL SOMOT, France]	Taken into account. A paragraph on this has been included.
13571	4	5	4	5	Change 10.3.3.10Fitness by 10.3.3.10 Fitness [Maria Amparo Martinez Arroyo, Mexico]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
130553	5	21	5	22	Format issue! Please consider to drop "of India". [Panmao Zhai, China]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
117275	7	1	10	5	For the whole E.S. reference to section should be given at the end of each paragraph and just the section number. [Maisa Rojas, Chile]	Accepted. Has been changed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
125567	7	1	10	5	Why do some statements have non-bold sentences with additional explanation and others not. Additional explanation should be attached for all of the main, bold statements. [Trigg Talley, United States of America]	Accepted. All headline statements now have supporting statements.
85989	7	1	132	50	A large portion of this chapter focused on how models for the different systems are computed, enhanced and deployed with limited focus on assessing the performance of their performance. This makes the chapter comes across as a methods chapter in climate modelling. Without providing evidence of assessment, the description of models, how they are caliberated, tuned and enhanced could simply be moved to supplementary information. [Debra Roberts and the Durban WGII TSU, South Africa]	Noted. The chapter has two brief subsections on which types of models and model experiments exist to generate regional climate information. Such an overview is needed, as this is the first time the IPCC WG1 focuses on regional climate information, and these aspects might be unknown to many non-technical readers, but are relevant nevertheless. Much of the Chapter, in particular much of section 10.3, is an assessment of model performance for generating regional climate information, which goes far beyond anything that has been presented in earlier IPCC assessment reports on performance of models in representing regional climate (change). Note that a region-by-region assessment is NOT the scope of this Chapter but rather provided by the Atlas.
53533	7	1			There is no mention of seasonality in the ES. Yet, seasonality is an important feature of most regional climates, which may raise methodological issues (which metrics to define seasonality depending on variables and regions?) and may deserve a more thorough assessment in terms of both recent and projected changes. [Hervé Douville, France]	Noted. The other regional chapters address the seasonality of different variables. This is typically done using periods of the year that are relevant for the specific region.
59185	7	1			Section Executive Summary: The authors have made a great effort to synthetize accurately all the contents provided in chapter 10. The text and the messages are well written. However, there is a need to shorten the sentence on each hypothesis proposed in order to align easy comprehension in the main body of the texts. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The ES has been worked over entirely and a science writer has been involved to make sure the writing is understandable
20579	7	3	7	4	In order to spot check this statement, a control of the SROCC SPM was carried out: nowhere was found a passage underlining, as written here, the "urgent need for regional climate information". Please give a reference. [philippe waldteufel, France]	Not applicable. Text has been removed.
114739	7	3	7	22	I think this first para is quite heavy and could need some further improvements in clarity. The first sentence may also need reformualtion; not totally clear what "decison scale" [Jan Fuglestvedt, Norway]	Taken into account. The introduction paragraphs to the ES have been completely re written, considering many review comments. "Decision scale" is no longer used.
130619	7	3	7	22	Some redundant information in this paragraph for the Excutive Summery. [Panmao Zhai, China]	Taken into account. The ES preamble has been completely re written.
111553	7	3	7	22	This part is not ES, but rather Introduction or Preable. Could be moved the title just after this part [Volodymyr Osadchy, Ukraine]	Not applicable. The introduction to the ES has been completely re written.
20205	7	3	7	22	Please keep in mind that the purpose of a summary is to summarize the content of a body of text (here chapter 10). [philippe waldteufel, France]	Noted. However, the lines referred to is the preamble to the ES and does not have the purpose that the reviewer claims, but has the following purpose "Preamble: Introduction to the chapter content and lines of evidence, as well as hints to other related chapters where relevant (intended to guide readers)."
91003	7	4	7	4	Replace "decission scale" with "decission making" [Francois Engelbrecht, South Africa]	Not applicable. Text has been removed.
31415	7	5	7	7	The sentence "The AR6 thus… regional messages of change." is redundant, given the preceding statements and the statement that follows. Suggest omitting. [Markku Rummukainen, Sweden]	Not applicable. Text has been removed.
66303	7	7	7	9	Are we sure that CH10 assesses key foundation for regional information generation with methodology employed in CH12? [Erika Coppola, Italy]	Taken into account. The text has been revised and now reads "The region-by-region assessment of past and future changes in extremes (Chapter 11), climatic impact-drivers (Chapter 12) and mean climate (Atlas) relies on the sources and methodologies used for constructing regional climate change information assessed in Chapter 10."
39205	7	12	7	13	Are you saying in this chapter, only the methodologies of attribution of regional climate change are assessed, whereas Chapter 3 assesses the human influence on the climate system on a global scale? Does this make sense? [Lourdes Tibig, Philippines]	Noted. Yes, this makes sense.
20581	7	13	7	16	The meaning of this sentence is problematic since we do not know what you mean here by "region". Furthermore, while there is talk of regional climate change, regional climates are not mentioned [philippe waldteufel, France]	Taken into account. A definition of "region" has been included in the first sentence of the ES. "Regional climate information" is now mentioned before "regional climate change" is defined. More detail on these concept are given in Section 10.1.
27525	7	13	7	17	Using different definitions of "climate change" at the global and regional scales in the report could be misleading. And it does not seem that pertinent, since global climate change can also be due to some extent to natural internal processes, contrary to what is suggested here. [Eric Brun, France]	Accepted. In the FGD version, Chapter 10 exclusively mention and use the AR6 glossary definition of climate change.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31417	7	13	7	19	This is a curious statement. Climate change at any scale is due to different factors. Attribution in turn is not solely to human influence, but to factors behind observed change. There is no such difference between global and regional climate change as the sentence describes as it is presently formulated. The relative contribution of some factor to some observed behaviour, be it variability or change, can of course differ between regions, or between some regional scale and the global scale. This could be omitted or at least clarified. [Markku Rummukainen, Sweden]	Taken into account. The ES preamble has been completely re written and does no longer refer to differences between regional and global climate change.
41905	7	14	7	14	Replace "ultimately is" with "ultimately leads to". [Rupa Kumar Kolli, India]	Not applicable. Text has been removed.
125569	7	14	7	19	[RISK] These statements are not correct as written. Global-mean climate variations can also be related to internal variability (ENSO/PDO affects global-mean surface temperature), to volcanic eruptions, etc. Natural variability impact should be part of the future projection. [Trigg Talley, United States of America]	Accepted. The ES paragraph has been completely re written and this part has been removed.
15621	7	15	7	16	This part certainly needs some editing, because it gives the impression that global climate change is fully governed by anthropogenic influence (although there is natural variability at the global scale) and regional climate change is fully governed by natural variability, although it is of course also partly/strongly due to anthropogenic influence. [Samuel Morin, France]	Taken into account. The ES preamble has been completely re written and this sentence has been removed.
125571	7	15	7	16	This text seems to imply that natural variability is not an important consideration at the global scale. This is not true, natural variability just has a greater influence at the regional scale than at the global scale. [Trigg Talley, United States of America]	Accepted. The ES paragraph has been completely re written and this part has been removed.
117273	7	16	7	17	Is it really just atmospheric internal variability. What about ocean's internal variability? [Maisa Rojas, Chile]	Taken into account. We now have reformulated to "Regional climate change is the result of the interplay between regional responses to both natural forcings and human influence (considered in Chapters 2, 5, 6 and 7), responses to large-scale climate phenomena characterizing internal variability (considered in Chapters 1–9), and processes and feedbacks of a regional nature." which includes the internal variability of the ocean.
106537	7	16	7	17	Atmospheric internal variability does not lead to change but is an integral expression of the climate so this needs to be removed from the sentence. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The chapter 10 definition of (regional) climate change is exactly the one adopted by AR6 (following AR5-WG1 and the three special reports): Climate change refers to a change in the state of the climate that can be identified (e.g., by using statistical tests) by changes in the mean and/or the variability of its properties, and that persists for an extended period, typically decades or longer. Climate change may be due to natural internal processes or external forcings such as modulations of the solar cycles, volcanic eruptions and persistent anthropogenic changes in the composition of the atmosphere or in land use.
20583	7	16	7	17	Can regional climate change be due to atmospheric internal variability? This is questionable, because internal variability should be considered as a characteristic of the climate itself. Many paragraphs of this AR6/WG1 report, accordingly, discuss the way anthropogenic global change may modify the variability of various climate components. [philippe waldteufel, France]	Rejected. The chapter 10 definition of (regional) climate change is exactly the one adopted by AR6 (following AR5-WG1 and the three special reports): Climate change refers to a change in the state of the climate that can be identified (e.g., by using statistical tests) by changes in the mean and/or the variability of its properties, and that persists for an extended period, typically decades or longer. Climate change may be due to natural internal processes or external forcings such as modulations of the solar cycles, volcanic eruptions and persistent anthropogenic changes in the composition of the atmosphere or in land use.
91005	7	17	7	17	It is probably not correct to state that "atmospheric internal variability" can result in "regional climate change". It may cause variability that can be confused with climate change. Please consider removing this component of the statement. [Francois Engelbrecht, South Africa]	Rejected. The chapter 10 definition of (regional) climate change is exactly the one adopted by AR6 (following AR5-WG1 and the three special reports): Climate change refers to a change in the state of the climate that can be identified (e.g., by using statistical tests) by changes in the mean and/or the variability of its properties, and that persists for an extended period, typically decades or longer. Climate change may be due to natural internal processes or external forcings such as modulations of the solar cycles, volcanic eruptions and persistent anthropogenic changes in the composition of the atmosphere or in land use.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15623	7	19	7	22	Having read Chapter 12 before, reading this immediately raises the flag of the intersection between Chapter 10 and Chapter 12, in partiicular in terms of comunication and engagement with local and regional stakeholders. [Samuel Morin, France]	Taken into account. Chapter 10 and 12 has worked on minimizing the overlap, and the connection between the two chapters is now explicit in the ES: "The assessment of climate services in Chapter 12 builds on the assessment of distillation of regional climate information from multiple lines of evidence in Chapter 10."
41909	7	25	8	44	It is surprising that reanalysis has no mention in this section, despite many studies dealing with reanalysis data included in the chapter for assessment. It is important to recognize that reanalysis data are extensively used in evaluating the model performance, at both global and regional scales. A clear assessment of this practice is important to be included in the Executive Summary.. [Rupa Kumar Kolli, India]	Accepted. A statement on the use of reanalysis at regional scale has been added.
40489	7	27	7	28	lack of confidence level [TSU WGI, France]	Noted. In the SOD, confidence level is present in the sentence referred to: "To increase confidence in future projections of regional climate, there is high confidence that multiple sources of observations and tailored diagnostics are needed to evaluate climate model performance."
111369	7	27	7	33	Reference to Middle East and North Africa missing [Neeshad Shafi, Qatar]	Noted. No particular region is referred to in these lines so it is not clear what revision is requested.
100037	7	27	7	33	Improving observations through planning and positioning new high-resolution, multispectral, hyperspectral, and microwave satellite sensors in Earth's orbit for atmorpsheric and surface remote sensing can also be included here. Sentinel missions of Copernicus Program are good examples of such holistic satellite programs. [Lydia Sam, Sweden]	Taken into account. In new text of Section2.1.1
20585	7	27	10	5	According to Box 1.1, which deals with calibrated uncertainty language used in IPCC report, "Confidence is a qualitative measure of the validity of a finding, based on the type, amount, quality and consistency of evidence (e.g. data, mechanistic understanding, theory, models, expert judgment and the degree of agreement". Keeping this statement in mind leads to question the validity of using confidence statements in the present summary and in many sections of this chapter, inasmuch as the conditions stipulated above are not met [philippe waldteufel, France]	Noted. Not clear what revision is suggested. Since the SOD the chapter has refined its use of the confidence language in collaboration with the bureau.
41911	7	29	7	29	Replace "multiple observational" with "multiple high-quality observational" [Rupa Kumar Kolli, India]	Rejected. High-quality is a term that is difficult to define.
41913	7	31	7	31	Complexity need not necessarily be a fundamental requirement; even simple diagnostics can be quite effective. Instead, the diagnostics must be region-specific or "tailored", as already highlighted in the lead statement. [Rupa Kumar Kolli, India]	Agreed. Deleted "complex".
41915	7	32	7	32	Replace "present-day" with "observed", to be inclusive of historical and paleo climate features. [Rupa Kumar Kolli, India]	Rejected. We refer to processes or feedbacks or interaction that you know they are right because follow a physical laws but for which you can't have direct observations. So "present-day" does not only refer to observations but is a wider concept in this context.
51535	7	35	7	35	The statement in bold includes cities, but these are not mentioned in the underlying statement - please could you clarify in the paragraph below why observations for cities cause difficulties. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text no longer included in the ES statement.
41917	7	35	7	35	Replace "Observational..." with "Inadequate observational...". [Rupa Kumar Kolli, India]	Taken into account. Text has been revised
40491	7	35	7	36	lack of confidence level [TSU WGI, France]	Noted. Wherever a confidence level is not given it is because it is a statement of fact. This has been consulted with TSU/Bureau.
85991	7	35	7	36	It is difficult to understand what is being said here. Is the point here about how limited observational data makes it difficult to assess regional climate change in these contexts? [Debra Roberts and the Durban WGII TSU, South Africa]	Taken into account. Text has been modified to avoid ambiguity
79597	7	35	7	36	The headlines statement does not include confidence measure which is inconsistent with the practice in the IPCC 2018: Global Warming of 1.5°C. An IPCC Special Report on the impacts of global warming of 1.5°C above pre-industrial levels and related global greenhouse gas emission pathways, in the context of strengthening the global response to the threat of climate change, sustainable development, and efforts to eradicate poverty [Wilfran MOUFOUMA OKIA, Switzerland]	Noted. Wherever a confidence level is not given it is because it is a statement of fact. This has been consulted with TSU/Bureau.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
78307	7	35	7	45	Suggest to consider impact on data sparse regions in Southeast Asia in addition to the Mediterranean. Also, is it meant to refer to “data-sparse cities”, as it is not fully clear from the examples cited. [Leonie Lee, Singapore]	Not applicable. Text no longer included in the ES statement.
31419	7	36	7	36	"Observational records" do not "cause difficulties", but issues related to such records may do so. Also, the "pose limits" is rather vague and does not inform how restrictive such limits are. The text that follows provides some examples, but not to the significance of such limits. [Markku Rummukainen, Sweden]	Taken into account. Text has been modified
15625	7	38	7	39	This statement "There is high confidence on elevation-dependant warming in most of the mountain ranges but field measurements are extremely limited at high elevations" is an example of what I think shouldn't appear in Chapter 10. Indeed, here is a statement about observed climate change, and not a focus on methodology. Furthermore, the statement seems to generalize observation in the Hindu Kush Himalaya (Cross-Chapter Box 10.3) to all mountain regions on Earth. Overall, Elevation Dependent Warming is referred to in several chapters of this SOD (see e.g. Chapter 12), always in a quite cursory/simplified manner compared to the puzzling state of the evidence (see SROCC Chapter 2, which features a dedicaed Box). This requires some homogenization between Chapters, and I think assessments of past/future climate trends simply do not belong in Chapter 10. [Samuel Morin, France]	Not Applicable. Text no longer included in the chapter
51533	7	40	7	40	The statement '…decline of observations' is quite vague - please could you clarify if this refers to a decline in obervations in the Mediterranean, India and Africa or if this statement is specific to one region/country/continent. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text has been modified
18767	7	40	7	40	"decline" may be changed to "lack" to be precise. [Govindasamy Bala, India]	Accepted. Text has been revised.
41919	7	40	7	41	I don't think this is a correct representation of the actual situation in India, either in terms of scarcity or decline of observations, notwithstanding the uncertainties brought out in Section 10.6.3.3, mainly in terms of constructing the datasets in different ways for India. It is well-known that the climate data for India are among the best and the longest in the world, and naming it along with the situation in Africa is unwarranted. [Rupa Kumar Kolli, India]	Taken into account. Text has been modified
45217	7	40	7	42	The claim about the scarcity and decline of long-term precipitation and temperature observations over India is incorrect and may be removed from the Executive Summary. [Krishnan Raghavan, India]	Taken into account. The ES statement has been modified
31421	7	44	7	45	The "It is virtually… methods." is a response to problems with data issues, and as it carries a different message than most of the paragraph, it might be useful to already in the bold text at the start of the paragraph to refer to methods to ameliorate difficulties/limits. [Markku Rummukainen, Sweden]	Taken into account. Text has been modified
91007	7	47	7	47	It is not clear what the authors mean with "errors in model formulation", since they also refer to sustematic errors, which is the result of more factors, or other factors, than model formulation. Please consider replacing "Reducing errors in the model formulations" with "Reducing biases". [Francois Engelbrecht, South Africa]	Taken into account. The paragraph has been completely rewritten, the phrase no longer appears.
40493	7	47	7	48	lack of confidence level [TSU WGI, France]	Noted. Wherever a confidence level is not given it is because it is a statement of fact. This has been consulted with TSU/Bureau.
79599	7	47	7	49	The headlines statement does not include confidence measure which is inconsistent with the practice in the IPCC 2018: Global Warming of 1.5°C. An IPCC Special Report on the impacts of global warming of 1.5°C above pre-industrial levels and related global greenhouse gas emission pathways, in the context of strengthening the global response to the threat of climate change, sustainable development, and efforts to eradicate poverty [Wilfran MOUFOUMA OKIA, Switzerland]	Noted. Wherever a confidence level is not given it is because it is a statement of fact. This has been consulted with TSU/Bureau.
34685	7	47	7	50	The first sentence in this key message doesn't make a lot of sense and should be deleted. The second sentence (minus the phrase "in spite of these errors" is a better key message. [Russell Vose, United States of America]	Noted. The paragraph has been completely rephrased.
31423	7	47	7	52	It is left unclear what these errors are, in global models. Key information here would be about the utility of global climate models for climate information on regional scale. The present writing would seem to emphasise need for model improvement, which may not be the key message here. [Markku Rummukainen, Sweden]	Noted. The text has been reorganised and rephrased, the term error is no longer used in this paragraph.
17635	7	50	7	51	this is a very controversial statement as you may know. I would have said « reducing SOME systematic errors » as it has also been proven that some systematic errors are not improved by higher resolution. [SAMUEL SOMOT, France]	Taken into account. The two related paragraphs have been completely revised, the statement is now much more specific.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
20207	7	50	7	52	This issue has been addressed at least twice in previous chapters, with similar conclusions [philippe waldteufel, France]	Noted. And here it is specified for regional scales. The whole text has anyway been rephrased
13573	7	52	7	52	Change the position of the point [Maria Amparo Martinez Arroyo, Mexico]	Rejected. The position of the point indicate that all sentences in this paragraph refers to the section number within brackets.
90973	7	54	8	1	It would be helpful to have some text explaining this key finding, especially since the claim seems controversial. Depending on the accuracy and detail needed for a purpose, results could be "good enough" even if, say, one relevant forcing had not been included / represented. [Wendy Parker, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The point made by the reviewer is captured in the word "relevant". If the projection would be good enough without the forcing, the forcing would not be relevant.
41061	7	54	8	2	There seems only headllines without supporting sentences in this bullet. [TSU WGI, France]	Taken into account. The text has been rephrased and integrated into another paragraph.
79601	7	54	8	2	The sentence is convoluted and need to be clarified. For instance, are aerosols land-use change and ozone concentrations the main requisite for regional climate models to reproduce historical trends and to ensure fitness for purpose? What is meant by “Including all relevant forcings in regional climate models”? In addition, this headlines statement omits the confidence statement. [Wilfran MOUFOUMA OKIA, Switzerland]	Noted. This paragraph has been substantially revised and extended to capture GCMs as well. Note that we cannot list all relevant forcings as they depend on region and variable of interest. Detail can be found in the referenced main text.
109419	7	55	7	55	The chapter assesses the effect of ozone depletion on regional climate. The mention to ozone in the executive summary should clarify that it deals with "stratospheric" ozone. [Sophie Szopa, France]	Accepted. "Stratospheric" has been added.
31425	7	55	8	1	The "and to ensure fitness for… in certain regions" is very vague. What does "fitness" imply? Which regions? Are such forcings included or not (in many/some/relevant cases)? [Markku Rummukainen, Sweden]	Noted. This paragraph has been substantially revised and extended to capture GCMs as well. Note that we cannot list all relevant forcings as they depend on region and variable of interest. Detail can be found in the referenced main text.
81263	7		7		Some headlines does not include confidence statements [Fatima Driouech, Morocco]	Noted. Wherever a confidence level is not given it is because it is a statement of fact. This has been consulted with TSU/Bureau.
116921	7		7		Please reconsider the preamble and stress developments and innovation taking place involving climate scientists, decision makers, practicioners for the co design of actionable climate information to support decision making, with a growing body of related scientific literature and new methodologies and approaches. This motivates the assessment of the state of this new knowledge oerformed in the chapter.Please also refer to the links with ch 5, 8 and 9. [Valerie Masson-Delmotte, France]	Accepted. The ES preamble has been completely re written. Links to Chapters 1-9 and 11-Atlas have been made.
116923	7		7		For elevatio dependent warming, you can refer to the assessment done in SROCC, ch 2, and update it. [Valerie Masson-Delmotte, France]	Taken into account. We have removed the statement that this comment refers to. However we were not able to update the SROCC statement on EDW.
116925	7		7		Many terms in the ES are not clearly defined and could be added to the glossary : "actionable" "climate information" "model formulations" "reducing errors". The wording here is diffferent from the one in chapter 3 and other model evaluation aspects (eg in ch 8 or 9) who talk about biases, caveats, etc. [Valerie Masson-Delmotte, France]	Taken into account. We have harmonised the terms across relevant chapters.
18775	7		10		The Executive summary: There is no mention of "pattern scaling" (discussed in chapter 4) which allows inference of regional changes directly from global mean change. A discussion of pattern scaling and hence connection to chapter 4 should be made in this chapter [Govindasamy Bala, India]	Accepted. A reference to pattern scaling is now available in section 10.4 and links to Chapter 4 been introduced.
132369	8	4	8	9	This summary needs to be toned down. Dynamical downscaling does provide added information in some cases, in particular when topographical information is highly relevant (e.g. mountainous areas) but many simulations based on dynamical downscaling have issues. In particular, many RCMs are used in simplified set-ups which can question their relevance for some projections, e.g.: 1) lack of representation of CO2 effects on evapotranspiration in most CORDEX RCMs possibly leading to an underestimation of warming of extremes compared to GCMs (Schwingshackl et al. 2019, ERL, see also Section 11.2.3); 2) inconsistent aerosol forcing in CORDEX projections in Europe, with impacts on temperature projections, including extremes: Bartok et al. 2017 (see also Section 11.2.3) [Sonia Seneviratne, Switzerland]	Noted. The fact that relevant regional drivers need to be included is already part of the ES statements. We have added the specific case of CO2 in the main text.
117277	8	4	8	9	what exaclty is meant by "at kilometre-scale" ? 1 km? Few km? Less than 10 km? Seems a bit to narrow to mention only that very fine scale. At which scale do you start seeing improvements for precipitation? [Maisa Rojas, Chile]	Taken into account. The sentence has been rewritten. Details and a definition of "kilometre-scale" can be found in the main text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17637	8	4	8	9	I feel that a reference to « extreme precipitation events » is missing in this paragraph as it is likely one of the best example of added-value of high-resolution climate models (RCMs) [SAMUEL SOMOT, France]	Accepted. Text has been revised (as part of a major revision of two related paragraphs).
17639	8	4	8	9	I wish to see a sentence (here or later in the summary) that explicitely states that high-resolution climate models (RCMs) can strongly modify regional climate change given by low-resolution climate models (GCMs) for specific regions and variables. See for example Giorgi et al. 2016, doi:10.1038/ngeo2761, Torma et al. 2020 DOI: 10.1002/asl.967 . This is one of the key reasons to push people to simulate climate change at higher-resolution. [SAMUEL SOMOT, France]	Accepted. The statement here is now included, albeit not with a direct reference to resolution, but precisely stating that relevant processes have to be resolved to simulate the sign of changes in a trustworthy manner.
34687	8	5	8	6	I'd drop the phrase "in spite of errors in model formulation that affects performance" from the end of this sentence. [Russell Vose, United States of America]	Accepted. Sentence has been dropped and paragraph has been rewritten.
31427	8	5	8	6	The "in spite of errors in model formulation that affects performance" is cryptic. If these errors are significant enough to highlight in the Executive Summary, there should be a clearer statement to allow the reader to put it in context. [Markku Rummukainen, Sweden]	Taken into account. The text has been rephrased and positive examples have been given (where, e.g., increasing model resolution helps to reduce errors/add value). Note that we cannot give specific detail in the ES. More details can be found in the main text.
111555	8	5	8	6	The last statement about errors better eather delete or put as a separate statement/sentence [Volodymyr Osadchy, Ukraine]	Taken into account. The text has been rephrased and separated into two paragraphs.
91009	8	6	8	6	"Errors in formulation" is probably not the correct phrase to use. There are probably no "errors" in the formulation of the model's cores (dynamic equations) or in the numerical solution procdures. The authors risk to generate with readers the impression that blatant mistakes have been made with the basic "formulation" of models. More likely, the authors are referring to spatial discretization errors, inadequate physical parameterisations and so on. This needs to be stated far more clearly. [Francois Engelbrecht, South Africa]	Taken into account. The paragraph has been completely rewritten, the phrase no longer appears.
18769	8	6	8	9	Very high resolution such as 1 km also helps to resolve the terrain topography and related circulations better. This may be mentioned. [Govindasamy Bala, India]	Noted. This point is included in a general statement about dynamical downscaling.
91011	8	11	8	11	Here the fundamental problem of "non-stationarity", a fundamental limitation to statistical downscaling methods, should also be mentioned. [Francois Engelbrecht, South Africa]	Noted. "non-stationarity" is a vague term (which actually also applies to dynamical models) covered by the much more specific statements we make.
1293	8	11	8	16	I think it is important to add a point about the fact that empirical-statistical downscaling (ESD - the term used by WCRP-CORDEX, rather than just statistical downscaling) can provide different types of answers to regional climate models (RCMs). E.g. ESD can be used to estimate statistical properties directly for local variables, such as the parameters describing the probability density functions (pdfs). The statistical properties are usually much more predictable than individual outcomes. Often the question is how the pdf changes (such as the probability of rainfall amount greater than 50 mm/day), and in this respect, ESD is well-suited to describe the aggregated characteristics of e.g. hourly or daily precipitation. For ESD to describe the aggregated properties of a variable, it needs samples, such as batches of seasonal data on e.g a daily or hourly time scale. As long as there is a connection between the large-scale ambient conditions and the statistical characteristics of some variable, be it mean temperature, precipitation totals, number of storms, or frequency of heat waves, ESD can in principle make use of statistical theory to estimate such numbers. It could also be noted that ESD has lots in common with artifitial intelligence (AI). There seems to be a widespread misconception that ESD and RCMs should be used to provide the same type of information (e.g. daily rainfall maps). [Rasmus Benestad, Norway]	Noted. These points are discussed in the main text.
18771	8	11	8	16	I hope the discussion is not about predicting the weather in the form of daily temperature and precipitation. Instead, the discussion is about the statistics (mean, std, range, etc) of daily temperature and precipitation. The sentences should be revised to indicate that the discussion is about the statistics. [Govindasamy Bala, India]	Noted. The text makes no reference to predictions. Depending on the context, statistical downscaling may be used to provide statistics or, drawn from these statistics, time series (often at the daily scale) to provide input for impact models. We have replaced characteristics by statistical aspects to clarify this point.
31429	8	11	8	16	Compared to the preceding paragraphs on global climate models and regional climate models, there is no similar emphasis of issues/errors/caveats, which also exist for statistical downscaling. This creates an impression that statistical downscaling would in some way be superior. Messaging first about utility and then about caveats may be a good order. In that case, the previous paragraphs should follow the same presentation, for clarity. [Markku Rummukainen, Sweden]	Noted. We have reorganised the text to distinguish better between strengths and weaknesses for all model types. The new structure accommodates for the reviewer comment.
31431	8	18	8	19	"fundamentally misrepresented" is a cryptic formulation and creates a mental image of serious flaws. It would be useful to find a more unambiguous formulation for what the aim of the statement is. [Markku Rummukainen, Sweden]	Noted. The mental image is about right, but indeed a bit too strong. We therefore replaced "fundamental" by "strongly"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42969	8	18	8	20	application of bais adjusment beneficial ...(high confidence), cannot correct ...(certain). It is not clear for the reader that the "high confidelity" is meant for the application and not for the built-in limitation. [Bodo Ahrens, Germany]	Taken into account. The text has been split to clarify.
1295	8	18	8	25	The crucial difference between downscaling and bias correction is that the former makes use of dependencies between large and small scales whereas the latter does not involve different spatial scales. The motivation for downscaling is the realisation that the models have a minimum skillful scale (Takayabu et al., 2015, DOI: 10.2151/jmsj.2015-042) and the point of downscaling is to take the large-scale information, that the climate models are able to simulate skillfully, and the cross-scale-dependency (which includes information about systematic local geographical effects) to quantify the local climate change. [Rasmus Benestad, Norway]	Noted. This issue is discussed in the main chapter text and too technical for the ES.
68935	8	19	8	19	Change "yet" to "but". [Seth McGinnis, United States of America]	Not applicable. The text is now spread across two paragraphs.
111557	8	19	8	24	The second part of the first sentence should be deleted, since after there is some discussion about difficulties with confidence level. The last sentence is not for ES and better reformulate or delete [Volodymyr Osadchy, Ukraine]	Noted. The second half of the first sentence states something different (cannot correct) than the following sentence (can get worse). The last sentence conveys an important message (that GCM bias adjustment to local scales is often not wise). Anyway, the text has been adjusted.
15627	8	21	8	21	What is the definition for "severe problems" ? "Severe" compared to what ? As such, the statement provides little information and added-value. [Samuel Morin, France]	Noted. The sentence has been rephrased. Note also that ES statements are concise and refer to the main text for further detail, here in the Cross Chapter box on bias adjustment.
125573	8	21	8	22	"Using bias adjustment as statistical downscaling, …" should read "… using bias adjustment prior to …" or "… in addition to …" [Trigg Talley, United States of America]	Rejected. The statement is correct.
117279	8	27	8	28	chapter refers to low-likelyhood high-ompact events. Terms need to be harmonised. [Maisa Rojas, Chile]	Accepted. Low-likelihood high-impact / warming is now used in the entire chapter.
44439	8	27	8	28	in Ch1 and other chapters (incl. SPM) the wording "low likelihood high impact" is used instead of "low probability high risk". Make sure terminology is made consitent across chapters (and within chapter 10). [Jana Sillmann, Norway]	Accepted. Low-likelihood high-impact / warming is now used in the entire chapter.
41149	8	27	8	28	What "low-probablity high-impact changes" refers to is unclear. [TSU WGI, France]	Taken into account. This should be clearer in the FGD since the same terminology is used cross-chapter (Low-likelihood high-impact / warming). The term is introduced in Cross-Chapter Box 1.3.
17641	8	27	8	29	Not sure the 2nd part of the statement is understandable. Rephrase ? « At the regional scale, multi-model mean and ensemble spread are not sufficient to characterise … situations where different models simulate substancially different or even opposite changes ». Not sure what it means [SAMUEL SOMOT, France]	Taken into account. The paragraph has been completely rephrased.
1297	8	27	8	37	The computational cost can also be cut by combining the smaller GCM/RCM ensembles with large multi-model ensemble involving ESD. It is also important to combine the RCM and ESD results for the reason that they make use of information from different and independent sources: in the RCMs the information comes from the computer code that represents the physical laws, whereas in ESD the information comes from information embedded in the observational data. [Rasmus Benestad, Norway]	Taken into account. This point has been added in the main text, 10.3.3.4.
112035	8	27	8	37	The last sentence refers to ensemble reduction which seems to be misplaced in this ES statement. General (spanning climate response) and fit-for-purpose model selection could be a topic for an additional ES. [jose manuel gutierrez, Spain]	Noted. Point has been removed from the ES.
4265	8	27	8	37	Emergent constraints are mentioned a few times in the chapter, but they aren't mentioned in the executive summary. I think that could be an appropriate aspect to mention here along with storylines when discussing how to deal with inter-model spread [Isla Simpson, United States of America]	Noted. However, the ES was too condensed to explicitly mention an individual technique such as emergent constraints.
17643	8	27	8	37	I feel important to state that « we are not sure that current GCM/RCM multi-model ensembles covers the true future at regioal/local scale » that is to say that the true future may lead outside the spread of current ensembles even for time-averaged values. I have no clear proposition to phrase it. I feel that this paragraph is supporting this idea but does not state it clearly enough. [SAMUEL SOMOT, France]	Taken into account. This is considered in a rephrased paragraph, stating that ensemble spread is not a full measure of uncertainty, in particular at the regional scale.
84721	8	27	8	37	Overall all this point needs to be adjusted. Not clear what reported in normal characters explain/justify the first sentences in bold [Annalisa Cherchi, Italy]	Taken into account. The paragraph has been completely rewritten.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
55149	8	30	8	33	This text about model selection/weighting for assessment of regional climate response uncertainty does not seem entirely consistent wiht the similar conclusion on page 10-64 lines 35-37. In the main text, the statement is given with high confidence (vs very high confidence in the ExSumm) and the exclusion of models that unrealistically simulate important processes is not stated as a requirement in the main text as it is in the ExSumm. Overall, the ExSumm conveys a stronger message about the value of this approach (exclusion of some models) than in the main text. [Nancy Hamzawi, Canada]	Taken into account. The ES has been adjusted accordingly.
84715	8	30	8	33	meaning not clear: how do multi-model ensembles exclude models? Doing what? [Annalisa Cherchi, Italy]	Taken into account. Text has been rephrased.
15629	8	31	8	31	While I agree on the "are required", this is probably too prescriptive. It may be possible to turn the sentence around, by stating that studies using ensembles provide more relevant/fit-for-purpose results, and, in contrast, that studies based on single model output are irrelevant to provide fair and relevant regional climate information. The policy implication is then obvious, without being prescriptive. [Samuel Morin, France]	Taken into account. The text has been rephrased.
84717	8	33	8	34	how is this specific to regional climate information? Also, internal variability is repeated twice (sentence should be rephrased) [Annalisa Cherchi, Italy]	Noted. It applies to large scales, but IV is in particular relevant at the regional scale. The text has been revised substantially.
79603	8	33	8	34	Not clear what is meant by “Grand ensembles” in this context, and this concept needs to be defined in the Glossary. For instance, is “Grand ensembles” equating “Large ensemble" [Wilfran MOUFOUMA OKIA, Switzerland]	Taken into account. We now explicitly mention SMILES.
125575	8	34	8	37	The suggestions as to how to select a global/regional model ensemble to cope with computational constraints does not belong in the Executive Summary. [Trigg Talley, United States of America]	Noted. The statement has been removed.
84719	8	34	8	37	not clear how this may happen … [Annalisa Cherchi, Italy]	Not applicable. This statement has been deleted from the ES.
18773	8	35	8	35	Selection of a few models: I believe this selection of the models will be based on the agreement between observations & model simulations at the regional scale. This may be mentioned here. [Govindasamy Bala, India]	Noted. The point about model exclusion covers that issue. Here it is about capturing uncertainties as comprehensively as possible.
31433	8	36	8	37	the "as comprehensively as possible" is unclear. What does this mean relative to "grand ensembles" - will the results be representative or not (or to what degree) of the full ensembles? The selection assumedly aims at some such correspondence. [Markku Rummukainen, Sweden]	Not applicable. The text has been completely rephrased.
44225	8	39	8	40	Urban parameterizations also have strong biases when estimating turbulent sensible heat flux, which is either directly underestimated by satellite-based approaches (Chrysoulakis, N., et al. 2018: Urban energy exchanges monitoring from space. Scientific Reports, 8, 11498), or affected by the biased turbulent latent heat flux, when calculated as a residual of the Urban Energy Balance (Ward, H.C., et al., 2016: Surface Urban Energy and Water Balance Scheme (SUEWS): development and evaluation at two UK sites. Urban Climate, 18, 1 - 32). [Nektarios Chrysoulakis, Greece]	Taken into account. Sentence has been modified
42971	8	39	8	40	Sensible heat flux density is missing here, which is of most importance in urban environment. [Bodo Ahrens, Germany]	Taken into account. Sentence has been modified
125577	8	39	8	44	[CONFIDENCE] Confusing. How can "all types" of parameterization simulate in a "realistic" way with high confidence? Would "many types" be better? Then, it says a simple single-layer parameterization is sufficient only with "low confidence". Seems inconsistent with previous high-confidence statement. [Trigg Talley, United States of America]	Accepted. Text has been modified to avoid confusion and increase clarity
31435	8	39	8	44	It is not evident that this is needed in the Executive Summary. As this is about methodologies and a low confidence statement, it could be omitted. [Markku Rummukainen, Sweden]	Rejected. With new papers out the confidence level has been elevated from the low to medium confidence.
44223	8	40	8	40	Please change "latent heat fluxes" to "turbulent latent heat fluxes". [Nektarios Chrysoulakis, Greece]	Noted. Sentence has been modified
79309	8	42	8	44	The use of word "sufficient" together with low confidence leads to a rather contradicting message. [Prodromos Zanis, Greece]	Accepted. Sentence has been modified accordingly
17645	8	42	8	44	do we need to put this statement in the summary if it has a low confidence. It is rare to find « low confidence » in the summaries [SAMUEL SOMOT, France]	Accepted. With new papers out the low has been elevated to medium and then kept in the ES
106539	8	47	9	11	These findings are not relevant in a chapter focusing on methods for generating information/messages and should be moved to the Atlas (along with the supporting assessment material) which deals with regional mean observed, attributed and projected changes. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. These statements correspond to the following bullet point in the approved outline of Ch10: "Interplay between internal variability and forced change at the regional scale, including attribution"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
84723	8	47	9	17	not clear how all these sentences are relevant from the perspective of methodology to extract information at regional levels [Annalisa Cherchi, Italy]	Noted. These statements correspond to the following bullet point in the approved outline of Ch10: "Interplay between internal variability and forced change at the regional scale, including attribution"
17659	8	47			this sub-section title is missleading as the section includes statements not related to attribution but also to future projections. Perhaps revise all those subtitles of the summary. [SAMUEL SOMOT, France]	Accepted. The title has been changed to reflect that the role of internal variability is important for both the past, present and future.
40569	8	49	8	50	No supporting sentences for this headline. [TSU WGI, France]	Taken into account. The statements have been strongly modified and supporting sentences have been added.
34689	8	49	8	50	You could probably go into a bit more detail here. [Russell Vose, United States of America]	Accepted. Supporting sentences have been added
91013	8	49	8	50	The confidence statement should probably be "virtually certain", not "high confidence". Remember that you are referring here to many regions collectively, not to a single region. Please also cross-check with Chapters 4 and 11 in terms of consistency of this confidence assessment. [Francois Engelbrecht, South Africa]	Accepted. The confidence statement has been modified to follow the reviewer suggestion
79605	8	49	8	50	Is this finding warrant to be an headline statements? It seems pretty close to the statement made in the WG1 contribution to AR5, SPM, Figure SPM 6, Section D which stated: “Over every continental region except Antarctica, anthropogenic forcings have likely made a substantial contribution to surface temperature increases since the mid-20th century (see Figure SPM.6). For Antarctica, large observational uncertainties result in low confidence that anthropogenic forcings have contributed to the observed warming averaged over available stations. It is likely that there has been an anthropogenic contribution to the very substantial Arctic warming since the mid-20th century. {2.4, 10.3}” [Wilfran MOUFOUMA OKIA, Switzerland]	Rejected. The scale is different. We are talking here about sub-continental regions, not continental ones. We have also increased our likelihood statement. In addition, we state that the contribution of anthropogenic forcing is major or dominant (greater than 50%) and not just substantial.
13575	8	49	50	8	Specific observed changes at the regional level could be included to reinforce the idea of the paragraph. [Maria Amparo Martinez Arroyo, Mexico]	Taken into account. While we did not find it appropiate for our chapter to provide regional examples, we have added a supporting sentence on the methodologies and time scales referred to.
51537	8	52	8	52	This statement references multi-decadal precipitation changes in several regions, would it be possible to briefly include which regions these changes correspond to? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The ES statement has been completely reformulated. Specific regions have not been included but can be found in the relevant sub-sections supporting the ES statement.
34691	8	52	8	52	Why not mention the actual regions? [Russell Vose, United States of America]	Taken into account. The ES statement has been completely reformulated. Specific regions have not been included but can be found in the relevant sub-sections supporting the ES statement.
79311	8	52	8	53	There is a lack of consistency in the use of confidence language. It is initially assigned a medium confidence for attributing regional precipitation changes ro anthorpogenic forcing but later on it is assigned a low confidence for the total anthropogenic contribution to precipitation changes. [Prodromos Zanis, Greece]	Taken into account. The ES statement has been strongly rephrased to clarify the associated confidence statements
45111	8	52	8	54	The assignment of medium confidence in line 53 and low confidence in line 54 is confusing. The text may be rephrased suitably [Krishnan Raghavan, India]	Taken into account. The ES statement has been strongly rephrased to clarify the associated confidence statements
91015	8	52	9	1	The statement in question is assigned "medium Confidence", but in lines 53 and onwards no support is provided for this, rather statements are made that suggest "low confudence" for the statement. Some of the evidence leading to "medium confidence" should rather be mentioned. [Francois Engelbrecht, South Africa]	Taken into account. The ES statement has been strongly rephrased to clarify the associated confidence statements
125579	8	52	9	1	This point is an excellent key message; however, the supporting sentence only discusses the reasons why attribution of precipitation changes are challenging. Recommend adding a supporting sentence expressing why there is medium confidence that there has been a human influence. [Trigg Talley, United States of America]	Taken into account. The statement has been strongly revised and the focus has shifted on the emergence of the anthropogenic signal.
1299	8	53	8	53	Perhaps rephrase since the uncertainty associated with observations varies from place to place. It is perhaps greater on a global scale (where the climate models give the best repersentation) and smallest in regions with good network of rain gauges, thermometers and radars (where the models are limited due to their minimum skillful scales). E.g. ' Variable observational uncertainty and internal variability as well as limitations due to model minimum skillfull scale...' [Rasmus Benestad, Norway]	Noted. These points have been put forward in the FGD executive statements related to "sources of information".

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51539	8	53	8	53	This statement refers to model errors - please could you clarify briefly what they correspond to, i.e. representation of small scale phenomena (line of sight, pg 69, line 55). [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The statement has been strongly revised and the word error is no longer used.
88825	8	53	8	53	The ordering here of the different sources of difficulty is inconsistent in section 10.4.1.3. See comment relating to line 13, and 26-27 on page 90. [Krishna AchutaRao, India]	Accepted. The statement has been revised and the ordering has been changed to be consistent between section 4 and the ES.
31437	8	54	8	54	The "low confidence with regard to a well-constrained quantification" is a very complex expression. Perhaps just "low confidence in quantification" [Markku Rummukainen, Sweden]	Taken into account. The new and relevant ES statement has been completely rephrased.
116927	8		8		There is a need to check the complementarity of statements on detected trends, attribution to forcings (or modes of variability), emergence, projected trends, at regional scales, for key variables, building on ch 3, 8, this chapter (example of precipitation here). One suggestion is to have a common Annex, which would develop detailed tables to support shorter synthesis versions for the TS. [Valerie Masson-Delmotte, France]	Noted. This has been discussed with chapter 1 and 3 in particular. Chapter 3 has developed such a table showing the influence of modes of variability on precipitation and temperature trends for all AR6 regions (albeit only for a recent 40-year period).
17657	8		9		On the whole I see no strong message in this summary to state if high-resolution models or downscaling techniques are or are not useful to provide regional climate change information. I would expect such a statement before the start of the section dedicated to co-production and distillation [SAMUEL SOMOT, France]	Noted. We have included rather specific statements, as the usefulness of different model types depend strongly on the context.
88827	9	1	9	1	Should this be "... greenhouse gases, stratospheric ozone depletion, and different aerosol species"? See additional comments on page 81, line 9 and page 90, line 29. [Krishna AchutaRao, India]	Accepted. "Stratospheric" has been added.
55151	9	3	9	6	Two comments: 1. does this conclusion about anthropogenic forcing being the dominant factor influencing future regional multi-decadal temperature trends (under high emission scenarios) require a time period to support the assessment of high confidence? Page 10-92 concludes that it is extremely likely that this signal will have emerged under high emission scenarios by 2050. It is unclear (to some readers) if signal emergence from anthropogenic forcing is the same as anthropogenic forcing becoming the dominant factor but regardless, it seems a time period is required for this conclusion in the ExSumm. 2. Can anything be stated here about lower emission scenarios? As a stand-alone statement, it is surprising that all we can say about the contribution of anthrogenic forcing to future land-based regional temperature trends is that it will be dominant under high emission scenarios. [Nancy Hamzawi, Canada]	Not Applicable. The statement has been removed from the ES.
79607	9	3	9	6	Inconsistent format of the headline statement where there is no bullet point to back up the statemen [Wilfran MOUFOUMA OKIA, Switzerland]	Not Applicable. The statement has been removed from the ES.
40567	9	3	9	11	No supporting sentences for these two bullets. [TSU WGI, France]	Taken into account. The statements have been strongly modified and supporting sentences have been added.
15633	9	3	9	17	To me, much of the material provided here belongs to Chapter 12 (regional changes) and not Chapter 10, which provides a much-needed methodological assessment relevant to regional climate change. [Samuel Morin, France]	Rejected. The approved outline of the AR6 WGI report states that Chapter 10 should include "Interplay between internal variability and forced change at the regional scale.", as well as "Scale specific methodologies e.g. urban, ..."
17647	9	3			why « initial-condition » [SAMUEL SOMOT, France]	Not Applicable. This statement has been completely removed from the ES.
34693	9	4	9	4	Delete the phrase "temperature change due to." [Russell Vose, United States of America]	Not Applicable. The statement has been removed from the ES.
88829	9	4	9	5	Suggest changing to "dominant factor in". [Krishna AchutaRao, India]	Not Applicable. The statement has been removed from the ES.
17649	9	4			« will be » → « is » [SAMUEL SOMOT, France]	Not Applicable. This statement has been completely removed from the ES.
13577	9	5	9	5	You could list which are those regions [Maria Amparo Martinez Arroyo, Mexico]	Not Applicable. This statement has been removed from the ES.
15631	9	5	9	6	I don't understand the focus on "the high-end (SSP5-6 8.5 and RCP8.5) GHG emission scenarios" here. There are many other relevant climate scenarios to be referred to. [Samuel Morin, France]	Not Applicable. The statement has been removed from the ES.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31439	9	5	9	6	It would be interesting, relevant and useful to refer to results from the full set or spread of emission scenarios. A singular focus to the high end does not provide sufficient information for many contexts. [Markku Rummukainen, Sweden]	Not applicable. The statement has been removed from the ES.
34695	9	6	9	6	The key message would be stronger with some mention of one or two additional SSPs. [Russell Vose, United States of America]	Not Applicable. This statement has been removed from the ES.
79609	9	8	9	11	Inconsistent format of the headline statement where there is no bullet point to back up the statemen [Wilfran MOUFOUMA OKIA, Switzerland]	Taken into account. The statement has been strongly revised and supporting sentences have been added.
79313	9	9	9	9	Please clarify in this sentence and make more clear the part "coming from initial-condition large ensembles". [Prodromos Zanis, Greece]	Taken into account. The sentence has been strongly revised.
4267	9	9	9	11	I think this statement is somewhat unclear. It could be taken as stating that internal variability won't be important anymore in the mid-21st century e.g., if you were to look at a 30y trends from 2050, the role for internal variability would be less than it is for a 30 year trend now. I think it would be clearer without the words "will still" and "multi-decadal" e.g., "it is very likely that internal variability plays an important role in projected long-term precipitation trends in many land regions until at least the mid-21st century". Or if the intention is rather to state that the magnitude of the internal variability compared to the other sources of uncertainty is large, then I think there would be clearer ways of stating that.. [Isla Simpson, United States of America]	Accepted. The statement has been completely revised following the reviewer's suggestion and additional section changes for the FGD.
17651	9	9			not sure « in particular those coming from initial-condition large ensembles » is required for this statement that would probably be the same without initial-condition ensembles [SAMUEL SOMOT, France]	Not Applicable. This statement has been removed from the ES.
31441	9	10	9	10	"significantly influence" is unclear, as it leaves it unclear whether (or to what extent) changes will be experiences. Significantly influence could in principle mean anything between that there is a detectable minor effect, or that forced trends will hardly be feasible to detect. [Markku Rummukainen, Sweden]	Taken into account. The statement has been completely revised and significant is no longer used.
106541	9	13	9	17	Consider moving this statement (and relevant assessment material) to the Atlas. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. These statement corresponds to the urban climate box located in chapter10, for traceability we prefer to keep it in chap10.
38531	9	13	9	17	The effect of urbanization on local temperatures is an interesting developmet here, but thos is only one type of land use change and there are many. Where is this assessed in the report? [robert vautard, France]	Noted. Land use is covered in Chapters 5,2,7,8,10 and 11 according to Table 1.1.
34949	9	13	9	17	Detailed Comments by SOD Chapter – Chapter 10: The SOD comments that the GMAS response to urbanization is negligible, though does affect minimum temperatures. This appears to lack any understanding of Urban Heat Island effect, which is very evidently distorting those GMAS datasets. See general comment #1 above. [Jim O'Brien, Ireland]	Taken into account. Text has been modified
34697	9	13	9	17	This key message is not a new finding of AR6. The global-scale result has been understood for a couple of AR cycles now, and the impact of urbanization on local-scale temperature has been known for a lot longer than that. I recommend dropping this key message. [Russell Vose, United States of America]	Taken into account. Text has been modified.
114849	9	13	9	17	need coordination on climate change signal due to urbanisation [Muhammad Amjad, Pakistan]	Noted.
17653	9	13		17	strange to see in bold the sentence dedicated to global annual mean whereas we are in chapter 10 [SAMUEL SOMOT, France]	Accepted. Text has been modified and the regional aspect has been put in bold
17655	9	13		17	no message concerning the way cities influence the future climate change response locally ? At least to say that we don't know yet if there is an influence [SAMUEL SOMOT, France]	Accepted. A new sentence has been added in the ES.
82659	9	15	9	15	Is there any basis for the qualifier "rapidly" here? (also affects the corresponding text in Box 10.2). [Blair Trewin, Australia]	Accepted. Text has been modified
54367	9	15	9	17	In Technical Summary it should be stated clearly and exactly, what kind of affection it is (as some people might don't know, whetver the minimum temperature rises or sinks) [Gabriel Stachura, Poland]	Taken into account. TS text has been modified
22733	9	15	9	17	This is unclear as written. Do you mean the highest and lowest temperatures recorded in a given year or are you rather talking about daily maximum and minimum temperatures? How to interpret this text is ambiguous presently. [Peter Thorne, Ireland]	Accepted. Text has been modified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
88831	9	15	9	17	This would still be at the cities and surroundings scale right? Perhaps this needs to be specifically mentioned as the ES statement begins with global annual mean. Also, is there anything to be said about global annual mean max and min temperatures being affected by urbanization? [Krishna AchutaRao, India]	Accepted. Sentence has been modified. No clear evidences on the impact of urbanization on max and min temperature globally so difficult to make assessment for both separately.
79611	9	20	9	20	inconsistent format of the headline statement where there is no bullet point to back up the statement [Wilfran MOUFOUMA OKIA, Switzerland]	Taken into account. We have corrected the ES so that all headline statements (in bold) are correctly backed up by the following sentences (in non-bold).
39207	9	20	9	26	Absolutely so that assessments of the performance of regional climate change messages are vital. [Lourdes Tibig, Philippines]	Noted.
41921	9	22	9	22	Add ", sectoral experts" after "climate scientists". [Rupa Kumar Kolli, India]	Not applicable. ES statement has been removed and formulated differently.
20587	9	22	9	26	These remarks reflect common sense, independently of any confidence statement, and they of course apply to the global scale as well as to smaller scales. However, they seem at most marginally relevant when considering the terms of reference of IPCC, such as they are summarised on the welcome page of the IPCC internet site. [philippe waldteufel, France]	Noted. Not clear what revision is suggested here. This particular statement has been reworked for the FGD following concrete suggestions from reviewers.
17661	9	22		26	Did you identify in the literature any danger of the co-production ? For example, co-production leading to biased messages or messages influenced by sectorial lobbying activities or conflict-of-interest signature in published science. I have no expertise in this field but I may imagine that social scientists have looked at that as in other scientific fields. I'm aware that answering this comment may not be easy and may lead to controversial discussions in the author team. [SAMUEL SOMOT, France]	Noted. The Executive Summary statements involving co-production have been substantially revised for the FGD and placed in the context of the best approaches to distillation. Section 10.5, however, does present a variety of challenges to co-production and application of its outcomes that need to be recognized and accommodated if co-production is to be successful.
39209	9	24	9	25	What do you mean by "values of those constructing, communicating and receiving the message"? [Lourdes Tibig, Philippines]	Taken into account. Values is meant here as total of ideas and concepts of a person. This further outlined and discussed in section 5 with the relevant references.
31443	9	25	9	25	the meaning of "values" should be explained, as the use of the term is somewhat different in scientific and layperson terms. [Markku Rummukainen, Sweden]	Taken into account. Values is meant here as total of ideas and concepts of a person. This has been further outlined and discussed in section 5, with also references to the relevant literature
31445	9	28	9	29	"potentially contrasting" could be replaced with something like "even when contrasting", if clarifying. [Markku Rummukainen, Sweden]	Not applicable. The ES statement has been removed.
15635	9	28	9	31	This statement is quite strong and I support it. However, I think that the remaining three paragraphs are not needed at ES level, and that, furthermore, they would probably better fit Chapter 12. [Samuel Morin, France]	Taken into account. The three last SOD ES statements have been merged and shortened and it is now clearer that they are supporting statements to the FGD statements on distillation (SOD page 9, lines 28-31).
68937	9	28	9	31	Bad punctuation makes this sentence difficult to parse. Change to: "there is high confidence that distilling climate messages derived from multiple, potentially contrasting lines of evidence such as observed, palaeoclimate proxy, and simulated data; theoretical understanding; diverse analysis methods; and expert judgment increases confidence in regional climate change messages." [Seth McGinnis, United States of America]	Not applicable. ES statement has been removed and content reformulated.
20589	9	28	9	32	Basing a conclusion on multiple, potentially contrasting, lines of evidence such as observed, palaeoclimate proxy and simulated data, theoretical understanding, diverse analysis methods and expert judgment is not a practice limited to the so called distillation or to regional scales. It is suggested to chapter 10 authors to study in chapter 7 section 7.5, pages 84-103, how the conclusion concerning ECS is reached [philippe waldteufel, France]	Noted. This statement does not suggest that the process of combining different lines of evidence is unique to chapter 10. Therefore it is not clear what revision is suggested by the reviewer.
55153	9	28	9	32	Recommend deleting the phrase "potentially contrasting" here. The main message here is that synthesizing/distilling information from multiple lines of evidence increases confidence in results (consistent with results shown in SPM Box.3). If evidence is contrasting, then it would be a reason for lower confidence in results so again, this phrase does not seem to fit. Saying "potentially contrasting" is unclear. The intent of this is better explained in the Indian Monsoon example beginning on line 43 where past trends in precipitation contrast with future projections and the point is made that these results are NOT in fact contrasting/contradictory. [Nancy Hamzawi, Canada]	Not applicable. ES statement has been removed.
34699	9	28	9	32	This is not a key message; rather, it's a description of how the IPCC uses the available evidence base to form confidence and likelihood statements. [Russell Vose, United States of America]	Rejected. This statement is not about how IPCC works, but about regional climate messages that are constructed outside the IPCC environment. Text is revised but with similar content

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39211	9	28	10	5	Indeed, these three examples of the distillation process for regional climate change messages. Please include one on What were the criteria used in selecting these three examples (e.g., drier future in the Cape Town region, the contrast between long-term future increases in Indian monsoon rainfall and decining rainfall in the observational record, and Mediterranean warming exceeding Northern Hemisphere warming),including as an example the How about including the case of the trends of the AMOC or the case of intensifying tropical cyclones in any of the ocean basins? [Lourdes Tibig, Philippines]	Taken into account. The three separate examples have been removed as separate ES statements, but are now used to serve as examples of an over arching statement. More or other examples could be chosen, but they cover a wide range of possibilities that result from the distillation process and therefore serve as illustrating examples. The motivation for the choice of these examples is discussed in section 10.6.1.
31447	9	31	10	5	The examples may not be necessary to display in the Executive Summary, as they are rather specific and available in the chapter material with additional context. [Markku Rummukainen, Sweden]	Taken into account. The three last SOD ES statements have been merged and shortened as supporting statements to the statements on distillation (SOD page 9 lines 28-31).
31449	9	34	9	34	the "will gain" is a formulation that would seem to be suggest something that has not been shown/experienced yet. Perhaps "gains" would be more suitable, if this is a real case study example. [Markku Rummukainen, Sweden]	Not applicable. The Executive Summary statements for the example regions have been revised and restructured to be presented as supporting examples for overarching statements about the distillation of regional climate information.
79613	9	34	9	37	inconsistent format of the headline statement where there is no confidence measure [Wilfran MOUFOUMA OKIA, Switzerland]	Not applicable. The Executive Summary statements for the example regions have been revised and restructured to be presented as supporting examples for overarching statements about the distillation of regional climate information.
81253	9	34	9	41	Please include a sentence explaning why the choice of this example [Fatima Driouech, Morocco]	Not applicable. The Executive Summary statements for the example regions have been revised and restructured to be presented as supporting examples for overarching statements about the distillation of regional climate information.
106543	9	34	10	5	Having three executive summary statements on specific examples seems strange in a methodology chapter. Suggest condensing this material and adding it in as detail to provide additional context to the previous executive summary statement on distillation (L38-32). [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The Executive Summary statements for the example regions have been revised and restructured to be presented as supporting examples for overarching statements about the distillation of regional climate information.
38533	9	34	10	5	These are the messages for the illustrative case studies. However it must be explained somewhere that these are illustrative case studies, perhaps introduced in the first ES paragraph, otherwise there is an impression that CH10 does full assessment but misses many regions. [robert vautard, France]	Not applicable. The Executive Summary statements for the example regions have been revised and restructured to be presented as supporting examples for overarching statements about the distillation of regional climate information.
34701	9	34	10	5	These are not key messages. They are traceable accounts (sans confidence statements) about projected regional-scale changes. They are nicely done, but I think they belong elsewhere. [Russell Vose, United States of America]	Not applicable. The Executive Summary statements for the example regions have been revised and restructured to be presented as supporting examples for overarching statements about the distillation of regional climate information.
85993	9	34	10	5	Since these are specifics of the previous bullet (lines 28-32) providing a line of sight to these specifics should suffice instead of elevating them to the ES. [Debra Roberts and the Durban WGII TSU, South Africa]	Not applicable. The Executive Summary statements for the example regions have been revised and restructured to be presented as supporting examples for overarching statements about the distillation of regional climate information.
17663	9	34		41	the writing style of this paragraph is not the same as in previous paragraph of the summary. To be revised ? [SAMUEL SOMOT, France]	Not applicable. The Executive Summary statements for the example regions have been revised and restructured to be presented as supporting examples for overarching statements about the distillation of regional climate information.
79615	9	43	9	44	inconsistent format of the headline statement where there is no confidence measure [Wilfran MOUFOUMA OKIA, Switzerland]	Not applicable. The Executive Summary statements for the example regions have been revised and restructured to be presented as supporting examples for overarching statements about the distillation of regional climate information.
17665	9	43		50	the writing style of this paragraph is not the same as in previous paragraph of the summary. To be revised ? [SAMUEL SOMOT, France]	Not applicable. The Executive Summary statements for the example regions have been revised and restructured to be presented as supporting examples for overarching statements about the distillation of regional climate information.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79617	9	52	9	55	inconsistent format of the headline statement where there is no confidence measure [Wilfran MOUFOUMA OKIA, Switzerland]	Not applicable. The Executive Summary statements for the example regions have been revised and restructured to be presented as supporting examples for overarching statements about the distillation of regional climate information.
17669	9	52	10	5	Despite the uncertainty, is it possible to give a plausible range of enhanced warming ratio for the Summer Mediterranean Warming with respect to the global annual mean warming, something like [1.2 ; 1.8] (fictive numbers) [SAMUEL SOMOT, France]	Not applicable. The Executive Summary statements for the example regions have been revised and restructured to be presented as supporting examples for overarching statements about the distillation of regional climate information.
17667	9	52		53	it seems important to precise if we are speaking about the Mediterranean region as a whole or only the land part of it. Do we speak about the past trends or the future projections or both. Also, do we speak about summer warming or all-year warming here ? The phrasing seems to indicate all-year warming whereas the title of section 10.6.4 is « Mediterranean summer warming ». [SAMUEL SOMOT, France]	Not applicable. The Executive Summary statements for the example regions have been revised and restructured to be presented as supporting examples for overarching statements about the distillation of regional climate information.
17671	9	53			I would situate the Mediterranean warming wrt the global mean warming that is a well known concept in the report and not with respect to the Northern Hemisphere mean warming that is less often used in the literature and in the report. [SAMUEL SOMOT, France]	Not applicable. The Executive Summary statements for the example regions have been revised and restructured to be presented as supporting examples for overarching statements about the distillation of regional climate information.
81251	9		9		Conclusions about the importance of co-design and production is also given in the ES chapter 11, there is need for more coordination to avoid duplication [Fatima Driouech, Morocco]	Taken into account. There has been extensive coordination between the regional chapters has been to ensure coherence and avoid duplication also in the ES. Duplications have been removed.
116929	9		9		Why focus only on RCP85 here, to discuss carefully x chapters. Choices need to be explained. The reference to Antarctica is cryptic, it could be better to expand. [Valerie Masson-Delmotte, France]	Not Applicable. The statement has been removed from the ES.
116931	9		9		Statements on cities could be developed to be integrated with insights from ch 8 and from ch 6. [Valerie Masson-Delmotte, France]	Accepted. Merged assessment has been put in both the TS and the SPM.
116933	9		9		Please define what is meant by "increasing / gain confidence in regional climate messages". Who measures this confidence, how? [Valerie Masson-Delmotte, France]	Not applicable. Confidence has been removed. Instead "trust" is used, which is explained and discussed in the main text.
116935	9		9		Implicitely, the chapter addresses issues about the persistence of past trends into the future, without being explicit; it could be more explicit about this. [Valerie Masson-Delmotte, France]	Taken into account. A more clear distinction has been made about past trends and future projections. This applies in particular to section 10.4 and 10.6
116937	9		9		I suggest not to refer to the mid Holocene as an analogue (it is not an analogue due to different forcing, different spatial and seasonal characteristics compared to the response to anthropogenic forcing). But please explain what mechanism can be tested using past "natural experiments" on the Earth's climate that have relevance for the same mechanism operating in the future. This aspect ("analogies and their limits") is worth coordinating across chapters (with the paleoclimate group). [Valerie Masson-Delmotte, France]	Accepted. Reference to mid Holocene has been removed.
4269	10	3	10	3	Is this discrepancy between CMIP5 and CMIP6 referring to global warming? Or is this still referring to Mediterranean warming as the in the previous sentence. It's unclear. [Isla Simpson, United States of America]	Not applicable. The Executive Summary statements for the example regions have been revised and restructured to be presented as supporting examples for overarching statements about the distillation of regional climate information.
79317	10	3	10	5	This has to be further clarified. Is it meant the comparison in climate sensitivity between CMIP6 versus CMIP5? This should be cross-checked and linked with chapter 7. Recent work shows that the effective climate sensitivity has increased in CMIP6 models which is primarily due to stronger positive cloud feedbacks from decreasing extratropical low cloud coverage and albedo (Zelinka et al, 2020, https://doi.org/10.1029/2019GL085782). [Prodromos Zanis, Greece]	Not applicable. The Executive Summary statements for the example regions have been revised and restructured to be presented as supporting examples for overarching statements about the distillation of regional climate information.
95909	10	4	10	5	Ch10: Pg10, Lines 4-5: On the Statement "... which highlights the need for .....". Is it not better for this assessment to be complete as per sources assessed rather than indication that something is still pending [Joseph Mutemi, Kenya]	Not applicable. The Executive Summary statements for the example regions have been revised and restructured to be presented as supporting examples for overarching statements about the distillation of regional climate information.
95911	10	4	10	5	On the Statement "... which highlights the need for .....". Is it not better for this assessment to be complete as per sources assessed rather than indication that something is still pending?. [Joseph Mutemi, Kenya]	Not applicable. The Executive Summary statements have been replaced by a single statement that has been substantially revised and shortened.
31651	10	5	10	6	In the context of this sentence, high end is used as a synomym of SSP5-8,5. In the sea-level litterature, high end is used to describe "unlikely high impacts scenarios" discussed in section 10.3.4.2 (e.g., Stammer et al 2019 and references therein https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019EF001163). It could be useful to clarify the terminology or how different terms are used in different communities. [Gonéri Le Cozannet, France]	Not applicable. The Executive Summary statements for the example regions have been revised and restructured to be presented as supporting examples for overarching statements about the distillation of regional climate information.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68939	10	5	10	50	There are also problems with comma usage throughout the preamble. I strongly recommend usage of serial or Oxford comma in all lists to reduce ambiguity and confusion. There are also some inappropriate uses of pseudo-parenthetical commas (e.g., lines 19-20) that should be removed. [Seth McGinnis, United States of America]	Editorial – copyedit to be completed prior to publication
6829	10	23	10	26	It would be more appropriate to compare the resolutions (temporal and spatial) of satellites offering the same product. For example it may be better to compare the spatial/temporal resolutions of Landsat to the respective ones of Sentinel-2 as far as land cover is concerned. [Constantinos Cartalis, Greece]	Accepted. Text has been reformulated.
45113	10	42	10	50	This key message in the Executive Summary is very good. The following minor modification may be considered in lines 45 and 49. Replace "mechanisms" by "forcing mechanisms" [Krishnan Raghavan, India]	Accepted. The statement has been strongly modified, but "mechanisms" has been replaced by "forcings".
116939	10		10		The statement about "the yet unresolved discrepancy between warming in CMIP5 and CMIP6" is very mysterious for the reader, please explain what it is, why it matters. [Valerie Masson-Delmotte, France]	Not applicable. The Executive Summary statements for the example regions have been revised and restructured to be presented as supporting examples for overarching statements about the distillation of regional climate information.
15637	11	1	11	1	I suggest replacing "messages" by "information" in the title. This is broader and more neutral, I think. [Samuel Morin, France]	Accepted. Messages and information are different concepts. The former is created in a co-production process with the user of the information taking into account all relevant context and values while the latter is the product of the distillation of all the climate-based physical sources (data, process understanding, etc.). For this reason, messages cannot be replaced by information, but by user-oriented climate information. However, the balance between the use of climate information and climate message has changed, with no references to climate message in the FGD to avoid using a term that has been considered loaded and leading to confusion. The main reason for the change is that, as the reviewer points out, the chapter focuses on the assessment of climate information. The concept of climate message is important though because it allows to introduce the phases that climate information goes through, in a collaborative process between the climate information producer and the user, whenever possible and necessary. For this reason, we have kept the discussion about the importance of involving the climate information producer in a dialogue with the users to co-produce what is needed for a better decision making.
110553	11	1			I found the "introduction" to this chapter (section 10.1) to be very difficult to follow. The big picture focus and goals of this chapter are not stated clearly and sufficiently. The text quickly dives into complex jargon/definitions/nuances and the theme of the chapter is lost (or not really stated directly enough early on). The first paragraph of the executive summary does define the problem a bit more succently - however in the other chapters i have read the Executive Summary is treated independely from the text and I would argue you should not have to read the executive summary first. I believe the focus of this chapter is on trying to outnline the metholdoliges used in the linterature and in practice to create "regional climate messages" that are useful for end-useres. I think the entire chapter could be re-organized to me more succenct and follow clear themes in each section. [Rachel McCrary, United States of America]	Accepted. The first paragraph of the ES and the introduction are now aligned. The introduction is more succinct, and better supported by figure 10.1.
110555	11	1			What is a "regional climate message"? This should be made clear fom the start - but it is used on line 28 as if this was something anyone would understand and not defined until section 10.1.3. [Rachel McCrary, United States of America]	Accepted. Climate information is now defined in the introduction, while the reference to climate message has been removed. In the new version of the chapter the balance between the use of climate information and climate message has changed, with no references to climate message in the FGD to avoid using a term that has been considered loaded and leading to confusion. The main reason for the change is that, as the reviewer points out, the chapter focuses on the assessment of climate information. The concept of climate message is important though because it allows to introduce the phases that climate information goes through, in a collaborative process between the climate information producer and the user, whenever possible and necessary. For this reason, we have kept the discussion about the importance of involving the climate information producer in a dialogue with the users to co-produce what is needed for a better decision making.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
110557	11	3			I suggest making the preamble it's own section (other chaptesr call this "framing" or "Introduction" or "Purpose". Use this to outnline the scope of the problem this chapter is hoping to cover, state things it will not be doing, point to where it will be useful etc. Then The details can be covered in the remaining chapters. [Rachel McCrary, United States of America]	Noted. The preamble, now called "Introduction", is a key piece of the chapter and fits in the introductory section; in fact, it's not long enough to be a self-standing section.
59193	11	5	11	7	What are those forcing driving the regional and global climate change? It is best to include oceanic circulation and intertropical convergent zone as the main forces driving regional and global climate change in these lines. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The forcings and drivers are explained in subsequent subsections. The oceanic circulation and the intertropical convergence zone are expressions of the Earth's climate and contribute to both the internal variability and forced change. The forcings and drivers are what the new 10.1.3 section tries to describe and assess.
45099	11	7	11	9	Is there any distinction between 'regional climate' and 'local climate'? If yes, would it be useful to provide a comment on this? [Dmitry Kovalevsky, Germany]	Noted. The spatial scales of regional climate are discussed in 10.1.2. It is there where more details on this type of distinctions are described. In this chapter regional climate refers to all those climate variations that take place at scales higher than continental areas.
41923	11	7	11	9	Clubbing sub-continental to local scales together may not be appropriate to define a regional domain, and may not be helpful (see also Section 10.1.2.1). While it is difficult to prescribe regional domains in any definitive sense, regional climate processes cannot be considered in the same way as the local climate processes such as those in a city. Regions or sub-regions should be seen as relatively large geographical domains under the influence of certain common regional drivers (e.g., South Asian summer monsoon, West African monsoon, East Asian monsoon, etc.) and in many cases comprise of more than one country. [Rupa Kumar Kolli, India]	Noted. The definition of region used in the chapter aims at considering those sub-continental areas for which a plethora of data sources, physical and chemical processes and methodologies are taken into account at the time of generating climate information. While sources, processes and methodologies can be very different for the urban and the typical IPCC WGI regional scales, the approach to consider different lines of evidence and assess their confidence in the production of regional information shares methodologies, limitations and challenges, which is what is dealt with in this chapter.
13579	11	11	11	11	indicate if it refers to adaptation to climate change [Maria Amparo Martinez Arroyo, Mexico]	Noted. It is not necessarily for climate change, it is often the case that users are interested in climate information to adapt to climate variability.
1301	11	13	11	13	Perhaps mention that mesoscale (5 kilometers to several hundred kilometers) and synopitc scales (orders of 1000 kilometers) are commonplace in meteorology. [Rasmus Benestad, Norway]	Rejected. This risks of making the sentence more complex and confusing, which is unnecessary in an introductory subsection.
106545	11	17	11	22	Suggest reordering the material here to start with observations, then attribution and then the models. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The order was not considered fundamental in this paragraph, although the whole paragraph has been rewritten to include all these sources.
4573	11	19	11	19	"……information formulation process (Li et al., 2020)." There is no reference here, I suggest to mention the following one.This paper is the latest comprehensive discussion on issues related to the detection of climate change at the regional scale. Ref: Li QX, Dong WJ and Jones P, 2020, Continental Scale Surface Air Temperature Variations: Experience Derived from the Chinese Region, Earth-Science Reviews, 200, 102998, DOI : https://doi.org/10.1016/j.earscirev.2019.102998 [Qingxiang LI, China]	Accepted. The reference has been added to the revised text and shared with section 10.2.
66539	11	20	11	20	I suggest to remove the work "inherent" here. For example there are biases in RCM simulations that are not inherent to the RCMs but emanates from erroneous large-scale boundary conditions from the GCM. Also, bias adjustment is only improving the situation as long as the observations are more realitstic than the model. [Kjellström Erik, Sweden]	Accepted. The sentence has been rewritten. The biases are inherent to all the model chain (both global and regional models) and the observations for the adjustment need to be of high quality.
23641	11	20	11	21	High quality observations are also important to assess added value. I would highlight it by adding at the end of the sentence: "...and are the basis for assessing model performance as well as to quantify the added value introduced by the models." [Deniz Bozkurt, Chile]	Accepted. The text has been rewritten.
106547	11	21	11	21	Climate information does not require attributing changes so sentence needs rephrasing. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Climate information does not require attribution, but benefits from it. The paragraph now makes clear that it refers to sources of information, which could all contribute to the production of climate information if available.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106549	11	22	11	24	This sentence does not make sense, please rephrase. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The whole introduction has been rewritten.
23643	11	22	11	24	It is a bit confusing whether "the near-term" and "next 30 years" correspond to decadal climate simulations. [Deniz Bozkurt, Chile]	Noted. The sentence refers to near-term climate predictions. It's now made clearer that 30 years refer to projections and ten years to decadal predictions.
111559	11	24	11	24	The new-future period is usually considered as 2021-2040. Thus, better to put "next 20-30 years' [Volodymyr Osadchy, Ukraine]	Accepted. The reference to the 30 years has been removed. The sentence now focuses on the contributions of climate predictions and projections.
106551	11	26	11	27	Need to add attribution into the sentence and rephrase accordingly. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Attribution is either observation- or model-based.
20591	11	26	11	27	Since the expression " regional climate information " is used here and later without specifying whether it concerns the past or the future, logically it includes the present. Then, as Chapter 10 writers are undoubtedly aware, there are, in national weather services, climatology departments, the missions of which include tasks and methods concerning the production and diffusion of climate information about the actual climate. Please discuss how the tasks and methods described here are situated with respect to these services [philippe waldteufel, France]	Noted. Services are discussed in section 10.5 (where the reader is referred to chapter 12, where they are fully developed). It is implicit in that material that services provide regional climate information for the past, present and future, the present being especially important when climate is in a non-stationary state as it's the case these days.
106553	11	33	11	38	Suggest adding here or as a subsection a figure and maybe text containing the remits of and relationships between the regional (and other WG I) chapters. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. A new figure (figure 10.4) with the role of regional climate in the whole report has been added to the FGD.
59195	11	33	11	38	Author(s) should consider adding more content to the key objective of this chapter. It is too skeletal as stated here. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The sub-section has been almost completely rewritten, although the content that can be added is limited due to space constraints.
106555	11	38	11	38	Include chapter 9 in the list. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. A new figure (figure 10.4) describing the role of regional climate in the whole report has been added to the FGD and the corresponding text describes the role of all the relevant chapters.
22735	11	38	11	38	Why exclude 5, 7 and 9 from here? [Peter Thorne, Ireland]	Accepted. A new figure (figure 10.4) describing the role of regional climate in the whole report has been added to the FGD and the corresponding text describes the role of all the relevant chapters.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
20593	11	40	11	41	Is it wise to go ahead and discuss regional climate messages right away? Would not it be more sensible and logical to begin by attempting to define what a regional climate is? Or at least to recall the present definitions? [philippe waldteufel, France]	Accepted. The balance between the reference to climate information and climate message has changed in the section (and in the whole chapter), with only few references to climate message in the FGD. The reason is that the chapter focuses on climate information. The concept of climate message is important though because it allows to introduce the phases that climate information goes through, in a collaborative process between the climate information producer and the user and this is the case regardless of what the chosen definition for region. Regional climate is described in sub-section 10.1.2.
22737	11	40	11	50	Other chapters have accompanied this text with a figure providing a graphical abstract. Why is that not done here? [Peter Thorne, Ireland]	Noted. Figure 10.1 was Chapter 10's graphical abstract. Now the chapter includes a standard visual abstract in figure 10.2.
110913	11	43	11	44	To a large extent this is an overarching comment, but it enters the discussion here first, so it is where I'm going to insert it. 10.3 may address "the performance of models in simulating relevant climate phenomena to estimate the credibility of future projections", but I don't think the various requirements for establishing future credibility are introduced and tied together concisely in this chapter at any given point. This one introductory bit attempts it, but it could be introduced and summarized more thoroughly somewhere. Perhaps in introducing 10.3.3.10? As right now, I don't think 10.3.3.10 "Fitness of climate models for projecting regional climate" ties it all together cleanly either. Somewhere the point needs to be concisely stated that the reliability/credibility/confidence of future regional projections is a function of the performance of the models used for the projections in simulating historical climate, our understanding of changes in climate processes in historical climate, and the physically plausibility of projected changes. Unfortunately, given the removal of the projections from this Chapter in the SOD, I think establishing all of these points regarding regional projection reliability/credibility/confidence clearly will remain somewhat neglected/unclear, even given 10.3.3.10, as there are no good, explicit examples discussed to tie it all together. However, in my opinion, it is an exceptionally important point to make in this chapter even if the projections don't appear until the Atlas (you could always point at something in the atlas?). Please consider making some changes to take this comment into account. [Melissa Bukovsky, United States of America]	Accepted. This is a very relevant comment that we have tried to address in a holistic way by rewriting the introduction, a large part of section 10.3 and revisiting the case studies in section 10.6 that now try to illustrate what it takes to produce credible regional climate information using all lines of evidence, including model performance. It might still leave open the challenge of how to address some of the problems linked to the credibility of the information when a number of sources are considered, but we could go as far as the literature allowed us.
90981	11	44	11	44	This the first of approximately 20 mentions of credibility / credible models in this chapter. It would be good to say what is meant by credible. Usually I think that "credible" means something like "believable" or "plausible", but I am not sure if that is what is meant here; it could also mean "having the right credentials", e.g. in terms of model construction and/or performance. Once the meaning is specified, it could then be checked whether the term was being used consistently throughout the chapter. [Wendy Parker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Credibility is defined in 10.3.3.9 with respect to the quality and fitness for purpose of climate models. The reviewer is right in noting that the adjective credible is used in a loose way, but this is what we found in the modelling and climate information literature.
116941	11		11		Please refer to chapter 9 too (eg cryosphere aspects, ocean aspects can affect regional changes). [Valerie Masson-Delmotte, France]	Accepted. Chapter 9 is mentioned in the new figure 10.4 that describes the regional chapters in the context of the whole report.
116943	11		14		Many aspects of the first section look more like a textbook than an assessment. For instance, SR15 had a box dedicated to the recent Syrian drought in a longer context, based on an assessment of the literature; please check it carefully so that the example provided is the outcome of an assessment of the state of knowledge (see SR15, chapter 3). [Valerie Masson-Delmotte, France]	Accepted. Section 10.1 is introductory and is used to provide some of the concepts that are then referred to in the chapter and in other regional chapters. However, an effort has been made to reduce the textbook style and address the assessment requirements. The first paragraph of the ES and the introduction are now aligned. The introduction is more succinct, and better supported by figure 10.1.
35171	11				I feel that the framing and phrasing of the « preamble » is very much oriented towards « users » of the regional climate information and that the basic notion of «acquiring new knowledge on regional climate » is missing. The study of the regional climate and regional climate change is not only dedicated to delivering information and messages but its goal is also to bring new knowledges and to improve understanding of the regional climate and small-scale climate phenomena using dedicated observationnal and modelling approaches. I feel that not having the words « knowledge » and/or « understanding » in the preamble is an issue [SAMUEL SOMOT, France]	Accepted. The preamble, now "Introduction", includes the need to gather knowledge as requested by the reviewer. This is also made explicit in figure 10.1

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39213	12	1	12	7	We look forward to seeing this simplified view of the construction of a regional climate message. [Lourdes Tibig, Philippines]	Noted. The frequent use of climate message has changed in the section (and in the whole chapter), with no explicit mention to climate message in the FGD. The reason is that the chapter focuses on climate information and the revised version of Fig 10.1 tries to make this clearer. The concept of climate message is important though because it allows to introduce the phases that climate information goes through to efficiently reach the user, at times in a collaborative process between the climate information producer and the user. Section 10.6 mentions now examples of where messages are often constructed.
20597	12	5	12	28	Welcome in the world of meteorology. This explosive propagation of small scale errors was the reason why L.F. Richardson failed when he carried out around 1925 the first attempt to forecast the weather (with the help of hundreds of human operators since the computers would only come 30 years later). Hence this phenomenon is indeed reasonably well known. Former chapters of the SOD explain why initialized predictions are only conceivably useful provided the numerical model simulates, rather than atmosphere alone, both atmosphere and ocean. [philippe waldteufel, France]	Noted. The upscale propagation of initial errors is probably too specialised to be described in detail in this sub-section.
35433	12	10	15	26	The content is of great importance and I consider it essential to guide research at the regional level, since it points out from the concept of region to the different sources of uncertainty.It constitutes an epigraph that establishes a guide, within the Report itself, to delve into different aspects [Gladys Linares-Fleites, Mexico]	Noted. Although we have reorganized section 10.1, we kept the same spirit as in the SOD.
1303	12	15	12	15	It probably also should be noted that adding relevant information from several independent sources also makes it possible to reduce the uncertainty due to increased constraints. This is explained in Benestad et al. (2017; DOI 10.1038/NCLIMATE3393.) [Rasmus Benestad, Norway]	Taken into account. The ability to constrain the uncertainties requires that the lines of evidence are credible and that a methodology to merge them is available. While having additional sources is always helpful, it is possible that they do not constrain the result but provide a better estimate of the uncertainty, even if the resulting spread is larger.
20595	12	19	12	21	What is meant by "increasing recognition"? The links mentioned here have been agreed upon by scientists for well above a century [philippe waldteufel, France]	Accepted. The term has been removed.
130621	12	20	12	20	Please consider change "Earth System" to "Climate System". [Panmao Zhai, China]	Accepted. The text has been rewritten.
79619	12	21	12	22	The following and more recent articles can indicate the current state of knowledge on the issue of unified and seamless prediction on weather and climate timescales (Ruti et al. 2020; Bellprat et al. 2019) Bellprat, O., V. Guemas, F. Doblas-Reyes, and M. G. Donat, 2019: Towards reliable extreme weather and climate event attribution. Nat. Commun., 10, 1–7, https://doi.org/10.1038/s41467-019-09729-2. Ruti, P. M., and Coauthors, 2020: Advancing Research for Seamless Earth System Prediction. Bull. Am. Meteorol. Soc., 101, E23–E35, https://doi.org/10.1175/BAMS-D-17-0302.1. [Wilfran MOUFOUMA OKIA, Switzerland]	Noted. Thanks for the references, but the number of references in the text has been kept to a minimum in this introductory section.
70909	12	23	12	25	It's not just about predictability, it's also about physical mechanisms. Perhaps that is intended to be included, but it would be good to spell it out. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence has been modified.
38535	12	24	12	25	Statement is unclear [robert vautard, France]	Accepted. The sentence has been modified.
70911	12	25	12	27	It is important to note that this is not true of everything, and in particular seems not to be true of model errors related to atmospheric drag processes (Sandu et al. 2016 doi: 10.1002/2015MS000564), which are much less explored than those related to convection [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence has been modified.
125581	12	27	12	27	These citations do not cover the Cloud Associated Parameterization Testbed. These simulations and the associated publications similarly investigate the fact that long-term model biases show up within just a few days of the simulation. These results include the analysis of precipitation (not only cloud radiative biases). https://pcmdi.llnl.gov/projects/capt [Trigg Talley, United States of America]	Noted. These references will be equally valid in this sentence, but we have decided to keep the introduction as short as possible.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44227	12	28	12	32	The ability of the current modelling schemes to capture the two-way feedbacks between the different scales could also be discussed here. [Nektarios Chrysoulakis, Greece]	Noted. These references will be equally valid in this sentence, but we have decided to keep the introduction as short as possible. This kind of processes are assessed in section 10.3.
20599	12	35	12	43	Figure 10.2 is a nice looking figure, but be aware that the reason why these interplays between space and time scales are relevant for climate change information is that they are relevant for climate information to begin with! The word "thunderstorm" is sometimes used in a broad sense; however, it often happens to designate storms featuring lightnings but no hail, as opposed to "hailstorm". [philippe waldteufel, France]	Rejected. Thunderstorm is composed of an individual cumulonimbus (its horizontal scale is several km).
44229	12	37	12	37	It is better the abbreviations GCMs and RCMs to be explained in the grey box of Figure 10.2. [Nektarios Chrysoulakis, Greece]	Accepted. Revise the figure
106557	12	37	12	41	Suggest using the acronym CPM (for Convection Permitting Model) rather than CRM which could be confused with Cloud Resolving Model and also because these model permit explicit convection to take place but are not, e.g. at 4km, actually resolving convection as observed. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Revise the figure
43287	12	37		38	Read "Schematic diagram derived from the inventive way of displaying relevant interacting space and time scales to regional climate change information (Orlanski, 1975). " rather than "Schematic diagram derived from the inventive way of (Orlanski, 1975) displaying relevant interacting space and time scales to regional climate change information. " [Cyriaque Rufin Nguimalet, Central African Republic]	Noted: the caption has been revised
20601	12	46	13	27	While this subsection supplies information about the range of considered regional scales, the characterisation of regional climates is still missing. One cannot speak of climate change if one has not first defined the climate before change occurs [philippe waldteufel, France]	Accepted. Regional climate has been defined in Section 10.1.1.
111561	12	48	12	49	This first simple but solid sentence worth to be in ES as statement of fact [Volodymyr Osadchy, Ukraine]	Accepted. The sentence has been used in the first paragraph of the ES.
95913	12	50	12	51	Statement ...and processes like the atmospheric general circulation or large- ...". I recommend replace "or" with "and" [Joseph Mutemi, Kenya]	Editorial – copyedit to be completed prior to publication
22739	12	51	12	51	Suggest adding a reference to the modes of variability annex after mention of MoV [Peter Thorne, Ireland]	Not applicable. Text has been removed.
100425	12	56	13	1	This paragraphs should refer to {Atlas.2} section [Lincoln Alves, Brazil]	Accepted. Reference to Atlas has been added.
111563	12	56	13	14	AR6 reference regions used in other regional chapters should be referenced somewhere here [Volodymyr Osadchy, Ukraine]	Accepted. In the FGD we explain that AR6 regions are used in the following regional chapters. We also mention that in Chapter 10 regions are selected for their adequacy to illustrate methodological aspects.
22741	13	2	13	3	It is now cross-chapter box 2.2 [Peter Thorne, Ireland]	Accepted. 2.1 has been changed to 2.2.
125583	13	3	13	3	The working definition for a region used in this chapter is buried in the text. Please either move it to the beginning or end of the paragraph. Another alternative would be to put the definition in bold font. [Trigg Talley, United States of America]	Accepted. Definition has been moved to end of paragraph.
108971	13	3	13	5	Is this implying that continental regions, based on this defintion, are not considered regions in this chapter? [Gemma Teresa Narisma, Philippines]	Noted. It is correct that Chapter 10 does not use the continental regions. This has been clearer explained in the text.
108973	13	3	13	5	Would it be helpful to have a corresponding metric (e.g. km2) and this can better connect with Figure 10.2 that has the space scale in km [Gemma Teresa Narisma, Philippines]	Accepted. We now explicit that the spatial scales is in the range of a few thousand down to a few kilometres and refer to Figure 10.3 (SOD Figure 10.2).
125585	13	5	13	5	Why "megacities" here, but only "cities" on page 11, line 9? [Trigg Talley, United States of America]	Accepted. "megacities" has been changed to "cities".
66305	13	12	13	13	To consider these example as representative of all continents sounds a little too ambitious. [Erika Coppola, Italy]	Not applicable. The number of example studies has been reduced drastically for the FGD and the text has been changed accordingly.
40421	13	17	13	21	In Figure 10.3, Japan and Korea are not included in the region of "East Aisa". Figures regarding regional assessments must be carefully double-checked. [TSU WGI, France]	Not applicable. Figure 10.3 as well as the East Asia case study have been removed from the chapter.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106559	13	19	13	19	Is the term "attribution" being correctly used here. Check with the relevant box in Chapter 1. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Yes, it is consistent with Cross-Chapter Box1.4.
125587	13	19	13	25	In Figure 10.3, the comprehensive case studies of constructing regional climate messages, Cape Town is in a black circle but Mediterranean and South Asian are indicated by black box. Why? It is confusing. [Trigg Talley, United States of America]	Not applicable. Figure 10.3 has been removed from the chapter.
79621	13	32	13	34	The following and more recent articles can indicate the current state of knowledge on the issue of subseasonal-to-seasonal (Min et al. 2019; Robertson et al. 2020) Min, Y.-M., S. Ham, J.-H. Yoo, and S.-H. Han, 2019: Recent Progress and Future Prospects of Sub-Seasonal and Seasonal Climate Predictions. Bull. Am. Meteorol. Soc., https://doi.org/10.1175/bams-d-19-0300.1. Robertson, A. W., F. Vitart, and S. J. Camargo, 2020: Subseasonal to Seasonal Prediction of Weather to Climate with Application to Tropical Cyclones. J. Geophys. Res. Atmos., 125, https://doi.org/10.1029/2018JD029375. [Wilfran MOUFOUMA OKIA, Switzerland]	Taken into account. The 2017 reference in relation to sub-seasonal-to-seasonal considerations is retained. However, the text has been revised to more clearly represent the concept and the context provided by a seamless framework for predictions. Reference in relation to seasonal-to-multiannual is updated.
13631	13	33	13	33	It's suggested to explain what is meant "current initiatives", as the idea is confusing. [Maria Amparo Martinez Arroyo, Mexico]	Noted. Current initiatives speaks to research documented in literature at the time of preparation of the report. No change is made.
13633	13	38	13	38	It's recommended to clarify the idea of this statement, as it is confusing this text: "only one realization". [Maria Amparo Martinez Arroyo, Mexico]	Noted. Recorded measurements of any observed climate variable constitutes "one realization of internal variability". No change is made.
4271	13	40	13	40	The point of the observational large ensemble is that it is derived from observations, not from models, so I wouldn't say that observational ensembles are "produced from models". I believe that models only play a role in estimating what the forced trends are. [Isla Simpson, United States of America]	Accepted. The text has been revised and no longer includes "produced by models".
59187	13	43	13	51	Elaborate more on the relationship between spatial and time scales and consider recent references. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The text is necessarily concise in providing a review of key considerations for temporal and spatial scales and their relationship.
13581	13	48	13	48	Change Munoz by Muñoz [Maria Amparo Martinez Arroyo, Mexico]	Noted. The comment is no longer applicable as the text has been revised.
106561	13	54	13	54	Add "the causes of" after "attribute". [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The change has been made and a sentence now appears in Section 10.1.3.
13583	14	5	14	10	Change is suggested to give better context: The time characteristics of climate variability has implications for regional impacts (Bathiany et al., 2018). This is true not only because a longer event accumulates more impacts, but also because it can have impacts greater than the sum of its parts. For instance, while the compounding effect of the three-year drought experienced in Syria commencing in 2006 has been considered to exacerbate water and agricultural insecurity, failure of agricultural systems and widespread migration (Kelley et al., 2015); a long heat wave can have greater impacts on human mortality than the sum of individual hot days (Gasparrini and Armstrong, 2012). [Maria Amparo Martinez Arroyo, Mexico]	Not applicable. The paragraph has been shortened in the interest of reducing the section length.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1305	14	5	14	10	If the definition of local climate is the local weather statistics, then these statistics can include the pdfs representing the duration of phenomena (e.g. droughts and heatwaves - a geometric-type pdf), number of events (e.g. number of storms - a Poisson-type pdf), persistence and pdfs describing extremes. The question is then whether changes in the large-scale and global conditions alter these pdfs. I suggest including a comment saying that the local climate can be defined as weather statistics and that statistical theory can provide answers in a way in which climate models are unable in a direct fashion. The climate models must calculate the day-by-day weather and then estimate the statistics based on these daily simulations, whereas statistical analysis can analyse the statistical properties (the parameters of the pdfs) directly (e.g. dependency of these on the global state - many of them are also influenced by geographical and seasonal factors - as explained in Benestad et al (2016) DOI: 10.1088/1748-9326/11/5/054017). [Rasmus Benestad, Norway]	Noted. Comment is no longer applicable as section has been revised..
20603	14	5	14	10	All this is true, but remarkably commonplace. For example, by very definition, a heat wave lasts several days. A specific name for this phenomenon has been estimated necessary by climatologists since several decades. [philippe waldteufel, France]	Not applicable. The paragraph has been shortened in the interest of reducing the section length.
112041	14	12	14	32	The importance of using differente baselines is mentioned here. It would be good to include a mention to the Interactive Atlas, which allows to explore different baslines (currently AR5: 1986-2005, AR6: 1995-2014, and also WMO: 1981-2010 -1961, but further baselines will be added for the FGD, include those which could be relevant for the Chapters). [jose manuel gutierrez, Spain]	Accepted. A reference to Interactive Atlas has been included.
54373	14	15	14	15	inconsistency - "near-term" was first defined in this chapter(p.11, line 23-24) as 30 years. [Gabriel Stachura, Poland]	Accepted. Section 10.1.1 does no longer mention "next 30 years" as near term, this was a mistake.
44441	14	17	14	17	Do you mean RCM use different baselines than GCMs? Then it should be written like this as now it reads as if also GCMs use different baselines than those defined and used in other chapters, which would be misleading. [Jana Sillmann, Norway]	Accepted. Statement has been removed.
106563	14	22	14	22	Added "assessment of" before "CMIP6" and replace "exercise" with "simulations". [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Comment is no longer applicable as section has been revised.
106565	14	25	14	29	Include a reference to the fact that this is accounted for and can be explored further in the Interactive Atlas. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. A sentence has been included to indicate that the influence of different baseline periods can be explored using the Interactive Atlas.
22743	14	29	14	32	Indeed, but then when new estimates of historical change arise that substantively change the historical change to date estimates (chapter 2) how does that impact such an approach? [Peter Thorne, Ireland]	Accepted. Text has been added to suggest that the caveats of the approach should always be taken into account and references to Cross-Chapter Box 11.1 made.
20209	14	37	14	37	Please correct "to the way" [philippe waldteufel, France]	Accepted. The sentence has been modified.
106567	14	40	14	41	Isn't the performance assessment just a part of the uncertainyt quantification in which case rephrase. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The performance assessment refers to the accuracy of the simulations when compared to the observations and is complementary to the uncertainty assessment.
4575	14	44	14	44	".....current climate (Section 10.3.3; Eyring et al., 2019; Li et al., 2017; Knutti et al., 2010). The following paper is representative regarding regional (China) studies. Ref: Li QX., Zhang L, Xu WH, Zhou TJ, Wang JF, Zhai PM and Jones P., 2017, Comparisons of time series of annual mean surface air temperature for China since the 1900s: Observation, Model simulation and extended reanalysis. Bull. Amer. Meteor. Soc., 98(4): 699—711, doi: 10.1175/BAMS-D-16-0092.1 [Qingxiang LI, China]	Noted. The introductory section has been kept as short in references as possible.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
105743	14	45	14	47	Guillermo Mendoza, Ad Jeuken, John H. Matthews, Eugene Stakhiv, John Kucharski, Kristin Gilroy, 2018, Climate Risk Informed Decision Analysis (CRIDA), Collaborative Water Resources Planning for an Uncertain Future by UNESCO and ICIWaRM Press [Abou Amani, France]	Rejected. It is not clear if the reviewer wants the reference to be added.
20605	14	45	14	47	The reference is irrelevant. Smith and Matthews (2015) do not deal with any distillation process. [philippe waldteufel, France]	Accepted. The reference has been removed.
110559	14	49	14	54	At this point in the chapter - it appears that you are primarly focusing on uncertainties and confidence in regional climate estimations in "general" and not for specific types of methods. However I would argue your list here really only covers the uncertainties in GCM and RCM models - there is no discussion of the additional uncertaties that develop with statistical downscaling. It either needs to be more clear about what methods these types of uncertaintes relate to - or include additioal uncertaties that can exist in regional climate assessmets. [Rachel McCrary, United States of America]	Accepted. A paragraph to deal with these aspects has been added in the sub-section.
35173	14	49		55	Concerning the partitioning of the uncertainty at regional/local scale, the references used so far rely only on GCM (if I'm not wrong). You may want to assess articles dealing with RCM ensembles such as old articles (Déqué et al. 2007 DOI 10.1007/s10584-006-9228-x, Déqué et al. 2012 doi: 10.1007/s00382-011-1053-x) or the most recent articles using Euro-CORDEX ensembles (Evin et al. (2019, DOI: 10.1175/JCLI-D-18-0606.1, Christensen and Kjellstrom 2020, https://doi.org/10.1007/s00382-020-05229-y) [SAMUEL SOMOT, France]	Noted.
10069	14	53	14	55	Parametric uncertainty is incorrectly lumped into "structual uncertainty" here [Robert Kopp, United States of America]	Accepted. The sentence now mentions model uncertainty, which for some authors includes structural and parametric uncertainty.
106569	14	54	14	54	Replace "structural" with "model" for consistency with later text and because "structural" is only one aspect of model uncertainty (as explained later). [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence now mentions model uncertainty, which for some authors includes structural and parametric uncertainty.
22745	14	54	14	54	Structural uncertainty is the inter-model span so not equivalent to imperfections in climate models as is claimed here. This characterisation is inconsistent with earlier chapters. [Peter Thorne, Ireland]	Rejected. The inter-model span is known as spread. This is an ad-hoc measure of uncertainty that does not usually reflect the true structural uncertainty because the observations are not interchangeable with the members of the multi-model. The structural uncertainty is responsible for the large number of systematic errors identified, including the inability to properly represent the teleconnections.
125589	14	55	15	1	Varies *among* variables and *among* regions. [Trigg Talley, United States of America]	Noted. This is left for the copy editor.
1307	14	57	14	57	Also, there is the evaluation of individual model simulations as opposed to an ensemble of models (e.g. as in seasonal forecasting). It is expected that ensembles produce trend statistics which span the observed trend for the same period (e.g. using a student's T-test) and the range of values spanning the ensemble members on an annual basis corresponds to the observed interannual variability (e.g. use a binomial distribution with 10% of observed values falling outside the 90% confidence interval estimated from the ensemble members as explained in Benestad et al (2016) DOI: 10.1088/1748-9326/11/5/054017). [Rasmus Benestad, Norway]	Noted. While we agree with the reviewer, it's not clear which part of the text the comment refers to because there is no line 57 in this page. We assume that the reviewer refers to the concept of forecast reliability, which is not considered in this chapter because the literature on this issue in the climate change context does not exist.
4273	15	1	15	2	"among the largest" seems like it really depends on what you're looking at and when e.g., in the short term, internal-variability is more important. In the long term, it depends on what variability you're looking at and, in fact, Lehner et al seems to indicate it depends whether you're considering CMIP5 or CMIP6. "Among the largest" is also rather vague when there are three possibilities to choose from. Suggest instead stating "The model uncertainty can be a particularly important contributor to the uncertainty cascade at the regional scale". [Isla Simpson, United States of America]	Not applicable. The sentence has been removed to reduce the length of the section.
125591	15	2	15	9	"uncertainty cascade" not defined. [Trigg Talley, United States of America]	Not applicable. The uncertainty cascade is not referred to any more.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
110639	15	8	15	10	The cited reference Soerland et al. 2018 tackles the issue of RCM vs GCM uncertainty, nothing about observational uncertainty and bias adjustment which is also mentioned in the sentence. For this, I recommend to add the citation: Casanueva, A, Herrera, S, Iturbide, M, et al. Testing bias adjustment methods for regional climate change applications under observational uncertainty and resolution mismatch. Atmos Sci Lett. 2020;e978. https://doi.org/10.1002/asl.978 [Ana Casanueva, Spain]	Accepted. The discussion about the inconsistency between GCMs and RCMs has been moved to section 10.3 where the reference has been added.
35175	15	8		10	Not sure Sorland et al. 2018 is a good reference for « inconsistency between the GCM and RCM physics .. ». I don't have a good one to propose unfortunatly. [SAMUEL SOMOT, France]	Not applicable. The reference has been removed.
20607	15	9	15	9	The reader of this report ought to be supposed unaware of what a "bias adjustment" is. He/she had not heard about bias adjustment methods until opening chapter 10. [philippe waldteufel, France]	Accepted. The reader is now referred to the corresponding sub-sections where the concepts are further developed and assessed.
106571	15	13	15	13	Replace "structural" with "model" for consistency with earlier text. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence now mentions model uncertainty, which for some authors includes structural and parametric uncertainty.
20609	15	13	15	26	The superiority of multi-model approaches has been studied and known for a couple of decades. To quote only references subsequent to 2016 is misleading [philippe waldteufel, France]	Noted. As this is an assessment and not a review the assessment starts from any new material since the most recent IPCC report. This is why the references for something as well established as a multi-model are recent.
125593	15	15	15	18	[ENSEMBLES] If the key message in the Executive Summary on page 8, line 27-37 is included, then the weaknesses associated with using a multi-model ensemble for uncertain quantification and projections should be noted as well. In particular, that potential for models could suffer from similar deficiencies should be noted in the guidance for selecting a multi-model ensemble. [Trigg Talley, United States of America]	Noted. The paragraph already mentions the importance of model independence. Besides, the ideas mentioned in the comment are assessed at length in section 10.3.
110561	15	18	15	21	Steinschneider et al (2015) may be a good reference for a regional impacts studie that tired to take into account model families. S Steinschneider, R McCrary, LO Mearns, C Brown - Geophysical Research Letters, 2015 - The effects of climate model similarity on probabilistic climate projections and the implications for local, risk-based adaptation planning [Rachel McCrary, United States of America]	Noted. The number of references has been kept to a minimum in the introductory section.
20611	15	18	15	23	Chapter 10 authors are encouraged to read box 4.1 [philippe waldteufel, France]	Noted. Uncertainty and confidence are dealt with in almost all chapters. The reader has been referred to checking their treatment in all the previous chapters.
45101	15	23	15	26	Phrase on lines 23-26 is not completely clear; apparently, 'are' on line 25 should be deleted. [Dmitry Kovalevsky, Germany]	Not applicable. The sentence needed rewording but the text has now been removed.
54375	15	23	15	26	I think something is grammarly wrong in this sentence, or at least it's too long [Gabriel Stachura, Poland]	Not applicable. The sentence needed rewording but the text has now been removed.
125595	15	23	15	26	The sentence "The complex scene …" needs re-wording. It's grammatically incorrect. It may have missing words. Maybe remove "are" to make a proper sentence? [Trigg Talley, United States of America]	Not applicable. The sentence needed rewording but the text has now been removed.
1309	15	26	15	26	The problem of model uncertainty can partly be analysed using common EOFs together with reanalyses. This can be done for large multi-model ensembles in epirical-statistical downscaling (ESD) where downscaled results over the historical period can be evaluated against historical observations (trend statistics and interannual variability). Additionally, the statistics associated with the principal components representing the reanalysis and the GCM can reveal whether the climate models simulates a different spatio-temporal covariance structure than that of the best source of information we have for the real world. Common EOFs were introduced about 40 yeears ago, but surprisingly few climate researchers make use of them. [Rasmus Benestad, Norway]	Noted. This is an introductory sub-section. The usefulness of the common EOFs as a diagnostic tool is considered in section 10.3.
1311	15	31	15	37	This is the ideal situation and the message is extremely important. I'm not sure that it works this way in the real life. My impression is that there still is little real coproduction of knowledge. One important quiestion I like to ask decision-makers is exactly how they intend to use the information that they want me to provide them. What options do they have? And what would sway them to pick another option? What if the information happend to be incorrect? (the models may be wrong or something unexpected could happen, such as a pandemy outbreak or eruption of a super volcano). [Rasmus Benestad, Norway]	Noted. The comment is very relevant and raises important questions, and section 10.5 now assesses part of the user interaction issues and challenges.
45103	15	32	15	32	'vulnerable to climate' or 'vulnerable to climate change'? [Dmitry Kovalevsky, Germany]	Accepted. It is now specified that it refers to both variability and change.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41925	15	32	15	32	Replace "vulnerable" with "sensitive". [Rupa Kumar Kolli, India]	Noted. Vulnerable was used to make it compatible with the concept of vulnerability in the risk framework, although it gives a negative connotation.
106573	15	33	15	37	This sentence is not clear or incorrect (policy decisions do not take place through distillation of climate information). [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence has been rewritten.
125597	15	33	15	37	Sentence is too long. Break after "climate sources" for clarity. Then, start new sentence with "The connection plays an important role ..." [Trigg Talley, United States of America]	Accepted. The sentence has been completely rewritten.
1313	15	39	16	2	I'd add that it's important to include proper statisticians in this work (if a reference should be needed for that, then DOI: 10.1038/NCLIMATE3393 could be one) [Rasmus Benestad, Norway]	Noted. The expertise of the actors involved goes well beyond the traditional climate scientist and encompasses many profiles, including statisticians.
112861	15	39	16	2	Nissan et al. (2019) discuss the potential pitfalls of an inappropriate use of climate data for regional climate projections and presents guidelines for developments agencies and practitioners, as well as for climate scientist. It might be relevant in other sections of the chapter, but it seems very relevant here. Reference: Nissan, H., Goddard, L., de Perez, E.C., Furlow, J., Baethgen, W., Thomson, M.C. and Mason, S.J., 2019. On the use and misuse of climate change projections in international development. Wiley Interdisciplinary Reviews: Climate Change, 10(3), p.e579. [Paula Gonzalez, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The reference is used now in section 10.3.
79455	15	42	15	44	Either I disagree or I don't understand this statement. In my opinion, the time series from a GCM grid cell can be perfectly useful to create messages even to characterise climate or climate changes at the local scale. [Alejandro Di Luca, Australia]	Rejected. The key aspect in the sentence is "equivalent to an observational estimate of a point within the cell". The point estimate is of a different nature to the value of a grid cell in the model, which represents the conditions within the cell in an average sense.
9987	15	48	15	52	The sentence "The regional climate message generation approaches first distil...." is unclear. What is meant by "specific climate message distillation approach"? Not clear. [Renard Siew, Malaysia]	Accepted. The sentence has been rewritten.
20613	15	53	16	2	These instructions and recommendations will appear more clearly when they are hopefully, later in this chapter, illustrated by examples [philippe waldteufel, France]	Accepted. That is correct, the links to the appropriate sub-sections of section 10.5 are now available in the rewritten text.
20615	16	4	16	4	The word "linear" should be replaced by "continuous" [philippe waldteufel, France]	Rejected. The references use the word "linear".
110563	16	5	16	6	the statement "using often climate simulations only that are transformed into maps or derived data products" is unclear. Is this referencing the creation of user friendly data archives (e.g. Nature Conservancy CMIP3 archive)? This may not be obvious to an audience less familiar with these products. [Rachel McCrary, United States of America]	Accepted. The sentence has been rewritten.
57469	16	11	16	11	I suggest adding two references after Buontempo et al., 2018 that are highly relevant to user-oriented climate services: Hewitt, C. D., R. C. Stone and A. B. Tait, 2017: Improving the use of climate information in decision-making, Nature Climate Change, 7, 614-616. Golding, N., C. D. Hewitt, A. Taylor, J. Strachan, R. Parfitt and L. Vilarkin : The Rules of Engagement: Refining approaches to effective engagement for climate services, Climate Services (Submitted) [Chris Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The more updated reference Hewitt et al. 2020 has been added. We could not find the second reference published before the deadline for papers.
84725	16	15	16	15	section 10.1.4 is too redundant. This section could be largely reduced just focusing on those aspects specifically related to regional assessment [Annalisa Cherchi, Italy]	Accepted. The section has been shortened and rewritten.
125599	16	15	18	54	In the Section 10.1.4.1, which discusses forcings for regional climate, there's a subsection on WMGHG and a subsection on stratospheric ozone, but no discussion of how regional ozone gradients might influence climate at regional levels. The chapter would benefit from a discussion of radiative forcing due to tropospheric ozone. [Trigg Talley, United States of America]	Taken into account. While the section has been shortened and rewritten, tropospheric ozone has been left to Chapter 6.
125601	16	15	18	54	Overall, this section on forcings controlling regional climate does not provide the information that is necessary, which is specifically how these factors influence regional climate. In the places where useful information is provided, the section reads just as individual statements of fact. The section is missing high-level messages of what is important, with too little focus on regional implications of these forcing factors and too much "extra" information. This section needs significant work to be useful. [Trigg Talley, United States of America]	Accepted. The section has been shortened and rewritten.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73811	16	15	19	39	Should there be a summary statement for each source of regional climate variability discussed in this section? Only 10.1.4.1.7 has one (page 18, line 47). Having a summary statement for each source would make this section more consistent. [Rondrotiana Barimalala, South Africa]	Taken into account. This is a good idea, but such an assessment would require much more space, while our effort has been to reduce the size of the sub-section. We tried to include those assessments in the examples of climate information that other sections in the chapter present.
1315	16	20	16	20	Perhaps add some words after 'them' and before 'This' saying: 'The processes and the character of such local variability is often systematically influenced by given geographical factors (e.g. elevation, latitude, mountains, distance to the coast).' This is useful information and means that we already know a fair bit about the local climate (hence the presence of e.g. vinyards in e.g. France), which is implicit but could be made more explicit. [Rasmus Benestad, Norway]	Accepted. The section has been shortened and rewritten.
95915	16	20	16	21	Avoid referring reader to other chapters. It is sufficient to indicate that this section is well aligned sections in Chapters 3 and 7. This way, reader will continue reading and benefiting from this the subsection and the chapter in general. [Joseph Mutemi, Kenya]	Rejected. Links to other chapters where most of these forcings and drivers are assessed in detail are required.
22747	16	21	16	21	Why only chapters 3 and 7? Feels a very odd call out? [Peter Thorne, Ireland]	Accepted. Links to all other relevant chapters have now been added.
22749	16	23	16	24	I think chapter 8 likely changed their section ordering because section 8.2 was IIRC about the theoretical expectations of the changing hydrological cycle which is doubt the analogy you intend here? [Peter Thorne, Ireland]	Accepted. Pointers to other chapters have all been double checked.
59197	16	27	16	27	Bush burning, vegetation respiration, decay of dead biota (plant & animals), etc. are part of the natural forcing controlling regional climate. Author(s) should consider rephrasing and incoporating this to sentences in these lines. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. These are relevant forcings, but including them all in this sub-section would undermine our effort to shorten it (and the alignment with other chapters, like chapter 6). They have been included in the examples (e.g., 10.6.3.5 for evapotranspiration) where they are relevant to explain change and variability.
114743	16	27	18	54	As far as I can see, this needs close coordination with ch2, ch6 and ch7. Please involved authors to read and comment and as CAs. [Jan Fuglestvedt, Norway]	Accepted. Authors from these chapters have been contacted before rewriting the sub-section.
108975	16	27			Why are topography and SST not considered as forcings controlling regional climate? [Gemma Teresa Narisma, Philippines]	Noted. Topography is constant and not a source of change (although it can modulate the impact), while SSTs are a climate variable and not a forcing of climate (because they can be forced by the forcings or be part of a driver like the ocean modes).
22759	16	27			Most of these, including in many cases their regional expression, have been covered - often in some depth - in the preceding 9 chapters and yet cross-referencing is scant in many cases. Often the text here is so simplified that it risks being used by readers to undermine the substantive assessments elsewhere. The section would be better explicitly starting from the relevant key findings per forcing agent arising from the prior 9 chapters and then adding anything necessary to this. At the moment it feels like it does not sufficiently acknowledge and build upon the assessments arising from the prior chapters. [Peter Thorne, Ireland]	Accepted. The section has been shortened and rewritten. Links to the relevant sections in the previous chapters have now been included.
125603	16	30	16	39	This area of text has no clear message and needs to be rewritten. It is just a collection of facts. It does not convey a high-level message about how GHGs influence regional climate. The first sentence of this paragraph essentially means everywhere and does not add value. The fact that the GHG forcing has a hemispheric constrast, a latitudinal constrast (e.g., poles vs. tropics), and can influence regional warming and precipitation patterns should be the key message. The response of LH and SW over ocean vs. land would be true for any radiative perturbation and has nothing specific about the GHG forcing. [Trigg Talley, United States of America]	Accepted. The whole sub-section has been shortened and rewritten.
22751	16	30	16	39	This feels grossly oversimplified and like it might cut across multiple aspects of chapters 3, 4,7, 8 and 9 and potentially could be used by vested interests to play spot the difference to the much more substantive assessments undertaken therein. It has just two supporting references. [Peter Thorne, Ireland]	Accepted. The whole sub-section has been shortened and rewritten.
27527	16	32	16	33	About 'Over the ocean, the increased radiative forcing leads to an increase in latent heat flux [...]' : The latent heat flux does not increase everywhere over the ocean: see evapotranspiration changes in Figure 8.18 [Eric Brun, France]	Accepted. The whole sub-section has been shortened and rewritten.
42973	16	33	16	33	Why decrease of sensible heat flux over ocean? Absolutely or relatively? [Bodo Ahrens, Germany]	Noted. Relatively.
35177	16	37			You may be interested to assess 2 recent articles on the effects of GHG in RCMs have been evaluated with contrasted (opposite) results in Jerez et al. 2018 DOI: 10.1038/s41467-018-03527-y and Kroner et al. 2017 DOI 10.1007/s00382-016-3276-3 (section 4.3) [SAMUEL SOMOT, France]	Noted. The first reference is now included in section 10.3. The second reference is discussed in Jerez et al. 2018.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
87375	16	43	16	47	This sentence is not exactly an assessment, but rather like a review paper. You say the NAO might be sensitive to solar forcing (mainly fro model) and finsih by saying thay this is not true, nor significant in observations (Ortega et al. 2015, Chiodo et al. 2019). So what is your assessment. Science is usually based on data, and those one do not prove any link according to the last two authors. I suggest to be far more cautious, and say that this link is far from clear in the data, so it remains mainly theoritical at the moment, and therefore of poor use for e.g. climate predictions. [Didier Swingedouw, France]	Accepted. The sentence has been rewritten to convey the uncertainty in the regional impact of this forcing.
4275	16	43	16	50	I think it should be acknowledged that this is challenging to parse from the observational record given the large internal variability and the limited degrees of freedom when considering the 11-year solar cycle. A caveat is presented like this for the volcanic influence below and I think it is also warranted here for the solar influence. [Isla Simpson, United States of America]	Accepted. The sentence has been rewritten to convey the uncertainty in the regional impact of this forcing.
80043	16	49	16	49	I'd recommend adding "Possible" before "Impacts on the winter..." as all these solar connections are subject to big observational and modeling uncertainties [Gabriel Chiodo, Switzerland]	Accepted. The sentence has been rewritten to convey the uncertainty in the regional impact of this forcing.
91017	16	54	16	55	The most prominent effect of Antarctic ozone depletion of recent decades is the poleward displacement of the Southern Hemisphere westerlies. This should prominently be mentioned here, with references to Chapter 4 (which deals extensively with these aspects). [Francois Engelbrecht, South Africa]	Accepted. The poleward displacement of the westerlies is now mentioned. However, reference to Chapter 4 has not been made since we do not find an explicit mention of this relation in the FGD (although 4.3.3.1 discuss the relation between ozone depletion and the SAM).
4277	16	54	17	3	I think it would be worth mentioning here that ozone depletion is also thought to have shifted the westerlies poleward. [Isla Simpson, United States of America]	Accepted. The poleward displacement of the westerlies is now mentioned.
80319	16	54	17	3	Ozone depletion not only affects the Hadley cell. It is also related with the predominance of the SAM positive phase which induces a poleward migration of storm tracks and associated precipitation, affecting regions like Australia or central Chile [Paola Arias, Colombia]	Accepted. The relation to the SAM has been included.
1317	16	55	16	55	In some chapters the term 'Hadley circulation' is used while here it is referred to as the 'Hadley cell'. I think it would be useful to keep to one, and the term 'Hadley cell' is better since it's also the term used in e.g. Wikipedia. [Rasmus Benestad, Norway]	Noted. Editorial.
116945	16		18		I suggest to use an approach consistent with chapter 3 on forcings. Volcanic forcing is missing for external, natural forcing (with solar forcing). For aerosol forcing, please ensure an approach consistent with chapters 3, 6 and 8 and build on their assessment. Part of the assessment related to solar forcing is not consistent with the assessment in chapter 3, 8 (to check very carefully, building also on the corresponding AR5 assessment). I suggest to have volcanic forcing close to solar forcing (especially as it has been shown to play a key role for variations of the past centuries in ch 1-3 at the global scale). I suggest to separate human forcing (GHG, aerosols) from other factors related to land use and land cover change (including dust). For dust, please build on SRCCL and link to other chapters of AR6 WGI. In theory, dust is not a forcing, but a feedback (see also chapter 7). Please check coherency with ch 3 - 8 on assessements of the role of volcanic forcing on modes. [Valerie Masson-Delmotte, France]	Accepted. The coordination with the other relevant chapters has been increased. Besides, this sub-section has been shortened to make use of the information available in previous chapters.
125605	17	6	17	14	There is no need to have a separate 10.1.4.1.4 Aerosol section since authors separate out natural and anthropogenci aerosols separately anyway. [Trigg Talley, United States of America]	Accepted. The whole sub-section has been shortened and rewritten.
73813	17	6	18	29	Could 10.1.4.1.4, 10.1.4.1.5 and 10.1.4.1.6 be combined in one subsection? It seems the first one introduces the other two but they are separated in different subsections. A summary statement should be added. [Rondrotiana Barimalala, South Africa]	Accepted. The whole sub-section has been shortened and rewritten, adding appropriate links to chapters 6 and 7, as well as others (4).
30637	17	6	18	29	Subsections of "Natural aerosols" and "Anthropogenic aerosols" should be put under "10.1.4.1.4 Aerosols". In addition, aerosols from wild fires are important for regional climate. [Hong Liao, China]	Accepted. The whole sub-section has been shortened and rewritten, adding appropriate links to chapters 6 and 7, as well as others (4).
41189	17	6	18	29	Why 10.1.4.1.4 (Aerosols) is put here even with the following two sections (10.1.4.1.5 Natural aerosols and 10.1.4.1.6 Anthropogenic areosols)? Could these three sections be combined into one section? [TSU WGI, France]	Accepted. The whole sub-section has been shortened and rewritten, adding appropriate links to chapters 6 and 7, as well as others (4).
22753	17	7	17	20	Chapters 6 and 7 spent a huge amount of space addressing this issue and again the risk here is that the present text oversimplifies and risks undermining this more substantive assessment undertaken in preceding chapters. [Peter Thorne, Ireland]	Accepted. The whole sub-section has been shortened and rewritten, adding appropriate links to chapters 6 and 7, as well as others (4).
95837	17	8	17	8	Volcanic plumes can last for a longer time. Writing "from a few hours to several weeks" is thus more adequate. [Christine Bingen, Belgium]	Not applicable. The sentence has been removed.
81547	17	8	17	8	10.1.4.1.4 Aerosols: references to life time need to be changed to be consistent with table 6.1 listening these for various aerosol components. [Cathrine Lund Myhre, Norway]	Accepted. The whole sub-section has been shortened and rewritten, adding appropriate links to chapters 6 and 7, as well as others (4).
79319	17	9	17	10	Check also the links with Section 6.3 of Chapter 6. [Prodromos Zanis, Greece]	Accepted. The whole sub-section has been shortened and rewritten, adding appropriate links to chapters 6 and 7, as well as others (4).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
110751	17	10	17	10	'majority of aerosols scatter radiation'' this is absolutely true for natural aerosols not anthropogenic aerosols [Bruno Korgo, Burkina Faso]	Noted. Both anthropogenic and natural aerosols scatter and absorb. Scattering has been the effect with the largest impact in the past, although absorption in both types is also taken into account, with the large uncertainties associated with it.
5491	17	14	17	14	Add after the last sentence about the BC effects on snowpack and surface water cycle: "Black carbon deposited in snow pack also affect regional water cycle by altering snowmelt timing (Waliser et al. 2011)." [Jinwon Kim, United States of America]	Taken into account. This effect now appears in Cross-Chapter Box 10.4.
93733	17	16	17	20	Aerosol not only affect temperature and precipitation at both local and remote, but as feedback, temperature also can affects aerosols, related research have been carried out and published (Chen Y , Zhao C , Ming Y . Potential impacts of Arctic warming on Northern Hemisphere mid-latitude aerosol optical depth[J]. Climate dynamics, 2019, 53(3-4):1637-1651.). I think the above references should be added here. [Yikun Yang, China]	Taken into account. Feedbacks of climate on atmospheric composition are dealt with by Chapter 6.
125607	17	16	17	20	It's also worth adding here that aerosol radiative forcing has played a significant role in modulating North Atlantic tropical cyclone activity (Ting et al., Natural and forced North Atlantic hurricane potential intensity change in CMIP5 models, J. Clim, 28, 3926; 2015; Sobel et al., Human influence on tropical cyclone intensity, Science, 353, 242, 2016). [Trigg Talley, United States of America]	Taken into account. Tropical cyclone activity is dealt with by Chapter 11.
40087	17	23	18	17	Do these paragraphes regarding the feedback processes overlap with section 6.4? [TSU WGI, France]	Noted. No, they do not, especially after the major shortening and rewrite done for the FGD.
71601	17	24	17	25	In order to be coherent with the rest of the document I suppose that it should be Section 10.6.4 instead Section 6.4. [Sixto Herrera, Spain]	Not applicable. The sentence has been removed.
2513	17	24	18	17	I think that section 10.1.4.1.5 should include a brief description of other types of natural aerosols that dominate aerosol particle number in pristine areas and are thought to be a fundamental part of the aerosol-cloud-precipitation-climate interactions in several environments. For example biogenic secondary organic aerosols comprise an important fraction of aerosol number and even mass over Boreal (Tunved et al., 2006), temperate (Merikanto et al., 2009) and tropical forests (Wang et al., 2016). Similarly, biogenic marine emissions in remote regions are thought to affect CCN during biological activity (Sanchez et al., 2018). Additionally, natural fires inject vast amounts of carbonaceous aerosols into the atmosphere affecting climate via direct and indirect aerosol climate effects (Santin et al., 2016). The uncertainty in aerosol-cloud interactions perhaps limits a clearer understanding of how changes in these emissions will feedback on climate regionally and globally, but substantial research has been carried out to improve understanding, and in my opinion deserves to be mentioned in this chapter. Literature: 1. Tunved, P., et al. "High natural aerosol loading over boreal forests." Science 312.5771 (2006): 261-263. 2. Merikanto, J., et al. " Impact of nucleation on global CCN", Atmos. Chem. Phys., 9, 8601–8616 (2009). 3. Wang, Jian, et al. "Amazon boundary layer aerosol concentration sustained by vertical transport during rainfall." Nature 539.7629 (2016): 416-419. 4. Sanchez, K. J., et al. "Substantial seasonal contribution of observed biogenic sulfate particles to cloud condensation nuclei." Scientific reports 8.1 (2018): 1-14. 5. Santin, C, et al. "Towards a global assessment of pyrogenic carbon from vegetation fires." Global Change Biology 22.1 (2016): 76-91. [Juan Camilo Acosta Navarro, Spain]	Noted. This is indeed an important piece of information. However, for reasons of space and consistency with Chapter 6 (which should cover all types of short-term forcings) we selected only the main aerosol sources. Besides, these are the ones considered in most climate simulations, which are the main source of information about future climate.
93735	17	24			In addition to mineral dust, volcanic aerosol, and sea salt, natural aerosols also indlude many other types, such as pollen, forest exudates, and geyser steam [Yikun Yang, China]	Noted. However, for reasons of space and consistency with Chapter 6 these are the selected main aerosol sources.
79321	17	25	17	25	It is actually Section 6.3.6 of Chapter 6 (instead of 6.4). [Prodromos Zanis, Greece]	Noted. All links to other chapters have been checked in the FGD.
35179	17	27	17	50	concerning the dust-climate coupling at regional scale, you may be interested in assessing articles for the Mediterranean region dealing with this coupling at various temporal scales : Nabat et al. 2012 doi:10.5194/acp-12-10545-2012 , Nabat et al. 2015a doi:10.1007/s00382-014-2205-6, Nabat et al. 2015b – already cited in the chapter, Nabat et al. 2020 https://www.atmos-chem-phys-discuss.net/acp-2019-1183/ [SAMUEL SOMOT, France]	Accepted. Nabat et al. (2020) is now included in section 10.3.3.6, which collects the assessment of the relationship between aerosols and regional climate.
33081	17	29	17	29	central asia and and west asia suluid also be considered in other climate factors and its relation with other asia sub- regions as well as global climate change. [Sahar Tajbakhsh Mosalman, Iran]	Accepted. All these sources are now mentioned.
110753	17	29	17	29	northern africa (the sahara desert is the most important source of the world) [Bruno Korgo, Burkina Faso]	Noted. All main sources are considered.
32751	17	29	17	29	central asia and and west asia suluid also be considered in other climate factors and its relation with other asia sub- regions as well as global climate change. [sadegh zeyaeyan, Iran]	Accepted. All these sources are now mentioned.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19453	17	29	17	29	Central Asia and West Asia should also be considered in other climate factors and its relation with other Asia sub-regions as well as global climate change. [Mostafa Jafari, Iran]	Accepted. All these sources are now mentioned.
93737	17	29	17	30	I think there is a certain overlap between these regions. Such as how to divide Southwest Asia and Indian subcontinent? [Yikun Yang, China]	Noted. Such a subdivision is not relevant in an introductory paragraph like this one where we have to condense a lot of information.
110755	17	43	17	44	'The surface direct radiative effect is likely negative over land and ocean, especially when the assumed solar absorption by dust is large" this sentence needs to be rephrased or explained, giving that absorption has a heating effect and dust is not typically absorbent, it sounds like a contradiction and can be confusing [Bruno Korgo, Burkina Faso]	Accepted. The whole sub-section has been shortened and rewritten.
22755	17	48	17	48	Surely it's the actual shortwave absorption not a prescribed forcing? The forcing is not something decided by humans? [Peter Thorne, Ireland]	Not applicable. The sentence has been removed.
22761	17	52	18	7	Contains nothing overtly regional and cross-cuts with analyses in chapters 2 and 4. Also does not mention efforts via VolMIP and Bethke et al., 2017 to explore 21st Century potential responses to volcanic eruptions which were covered in some depth in chapter 4 and may be cross-referenced. [Peter Thorne, Ireland]	Accepted. The paragraph has been rewritten, linking the sentence to Chapter 4 (Cross-Chapter Box 4.1). Bethke et al. (2017) is now included in section 10.3.3.6, where the modelling aspects of volcanic aerosol with regional implications are assessed.
95839	17	55	17	55	In the case of the Pinatubo eruption in June 1991, the complete volcanic decay lasted not less than 5 year. [Christine Bingen, Belgium]	Noted. The estimates are given to illustrate the average behaviour. It is clear that explosive eruptions might lead to longer-lasting impacts.
125609	17	55	17	55	Brewer-Dobson [Trigg Talley, United States of America]	Accepted. The correction has been made.
95841	17	55	17	56	It might be specified that the overall transport is a fast longitudinal transport, and a gradual poleward transport within the hemisphere reached by the volcanic plume. [Christine Bingen, Belgium]	Noted. This type of detail is dealt with by Cross-Chapter Box 4.1.
99411	18	1	18	3	Consider if the results of Toohey et al., 2019 is relevant to point out here. They show that Northern Hemisphere temperature reconstructions suggest that explosive high-latitude eruptions produce more Northern Hemisphere cooling per unit injected stratospheric sulphur than tropical eruptions. Toohey, M., Krüger, K., Schmidt, H., Timmreck, C., Sigl, M., Stoffel, M., & Wilson, R. (2019). Disproportionately strong climate forcing from extratropical explosive volcanic eruptions. Nature Geoscience, 12(2), 100-107. [Herman Fuglestvedt, Norway]	Noted. This is now dealt with by Cross-Chapter Box 4.1.
125611	18	5	18	6	Either say global temperature *decrease* of 0.2°C or say a response of -0.2°C. Also, it's worth adding that the response is transient, peaking about 2 years following eruption. [Trigg Talley, United States of America]	Accepted. The sentence has been rewritten.
54377	18	5	18	7	global mean temperature decreased with 0,2 Celsius degrees, right? Because it is not plain, what was the sign of a response (+ or -). As for precipitation it is clear. [Gabriel Stachura, Poland]	Accepted. The sentence has been rewritten.
45105	18	6	18	6	I would recommend explicitly mentioning here that this is a 0.2C _cooling_ response. [Dmitry Kovalevsky, Germany]	Accepted. The sentence has been rewritten.
79323	18	6	18	6	Please specify that this is -0.2 deg C. [Prodromos Zanis, Greece]	Accepted. The sentence has been rewritten.
13635	18	6	18	7	It's recommended to mention if there is any information on the impact on temperature and precipitation over oceanic regions due to the volcanic eruptions . [Maria Amparo Martinez Arroyo, Mexico]	Noted. This is now dealt with by Cross-Chapter Box 4.1.
107017	18	9	18	17	The Menegoz et al (2018, https://doi.org/10.1007/s00382-017-3986-1) paper emphasing the need for large ensemble to extract the forced volcanic signal on the NAO/Europe dynamics that could even be conditionnal to the AMV phase could be added here. [Christophe CASSOU, France]	Accepted. The need to use large ensembles has been added to the corrigenda process.
64841	18	12	18	14	The dynamical signal occurring in the atmosphere after volcanic eruptions is described here, but not detailed because of low signal-to-noise ratio. However, as stated in Swingedouw et al. (2017), the observations suggest an higher probability for NAO+ signals during several winter after volcanic eruptions that might no project exactly on NAO pattern (Barnes et al., 2016) and is described with caution as a "winter warming over Northern Eurasia" that can occur during several years after volcanic eruptions (Zanchettin et al., 2013). References: Barnes, E.A., Solomon, S. and Polvani, L.M., 2016. Robust wind and precipitation responses to the Mount Pinatubo eruption, as simulated in the CMIP5 models. Journal of Climate, 29(13), pp.4763-4778.; Zanchettin D., C. Timmreck, O. Bothe, S. J. Lorenz, G. Hegerl, H. F. Graf, J. Luterbacher, and J. H. Jungclaus (2013), Delayed winter warming: A robust decadal response to strong tropical volcanic eruptions? Geophys. Res. Lett. 40, 204–209, doi:10.1029/2012GL054403. Same for El-Niño-la Niña signals shown in Swingedouw et al., (2017) that is slightly significant when cnosidering teh large eruptions of the last millenium. [Martin Ménégoz, France]	Noted. Part of this material is now included in Cross-Chapter Box 4.1, and another is part of the assessment in the rewritten (and shortened) sub-section.
17809	18	17	18	17	include Dee et al., 2020 (doi: 10.1126/science.aax2000) reference here. [Raphael Neukom, Switzerland]	Noted. We tried to keep the number of references short.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22763	18	20	18	29	No reference to chapter 6 and chapter 7 which spent considerable time on this issues. Again, a real risk of a reader playing spot the difference here and would be better to recap the findings from these chapters then add extra detail as needed. [Peter Thorne, Ireland]	Accepted. Links to Chapters 6, 7 and 8 have been added following discussions with the authors of those chapters.
108977	18	20			I suggest to include biomass burning and forest fires in this subsection [Gemma Teresa Narisma, Philippines]	Noted. These are aspects that have been dealt with by Chapter 6.
80321	18	21	18	23	As for natural aerosols, this subsection should also briefly mention the impacts of anthropogenic aerosols on regional climate. For instance, anthropogenic aerosols have caused a weakening of the monsoon circulations in South Asia, East Asia and West Africa during the second half of the 20th century, competing with the strengthening effect of GHGs [Paola Arias, Colombia]	Noted. The regional impacts are mentioned in sections 10.4 and 10.6 with specific examples. The Atlas Chapter has included assessments for additional regions.
2813	18	21	18	25	This section (10.1.4.1.6) is a bit short, perhaps a few other examples of how anthropogenic aerosol changes have affected climate regionally could be mentioned? For example: Anthropogenic aerosol changes during the 20th century are likely important in explaining the evolution of Arctic climate (Najafi et al., 2015), North Atlantic climate vaibility (Booth et al., 2012; Haustein et al., 2019), and Asian and African monsoon systems (Section 8.3.2.4.2) during the same period. Literature 1. Najafi, Mohammad Reza, Francis W. Zwiers, and Nathan P. Gillett. "Attribution of Arctic temperature change to greenhouse-gas and aerosol influences." Nature Climate Change 5.3 (2015): 246-249. 2. Booth, Ben BB, et al. "Aerosols implicated as a prime driver of twentieth-century North Atlantic climate variability." Nature 484.7393 (2012): 228-232. 3. Haustein, Karsten, et al. "A limited role for unforced internal variability in twentieth-century warming." Journal of Climate 32.16 (2019): 4893-4917. [Juan Camilo Acosta Navarro, Spain]	Accepted. The whole sub-section has been rewritten. All forcings are now in a single sub-section, the text has been shortened and sharpened and links to other relevant chapters have been expanded.
5495	18	29	18	29	Analyses of the CMIP5 model data shows that the slight cooling trend over East Asia in the period from 1940 to 1970 are induced by increased anthropogenic sulfate aerosols (Shim et al. 2019). [Jinwon Kim, United States of America]	Accepted. This is now included in sections 10.3.3.6 and 10.4.1.1.
99959	18	32	18	45	Population and shifting demography should also find a mention here. [Bhardwaj Anshuman, Sweden]	Rejected. Population and shifting demography is a WG2IIsubject.
22765	18	33	18	36	Why is the substantive land use land cover assessment undertaken across at least chapters 2 and 7 not cross-referenced here and the principal findings therein recapped as a starting point? [Peter Thorne, Ireland]	Taken into account. Reference to previous reports and chapters have now been included.
125613	18	38	18	45	This paragraph contains statements that require references. Add references to support claims -- specifically for the influence of afforestation on borael and tropical areas. [Trigg Talley, United States of America]	Not applicable. Text has been removed due to space limits.
111565	18	38	18	45	This paragraph needs references on afforestation effects in boreal and tropical regions [Volodymyr Osadchy, Ukraine]	Rejected. Due to space limits we were not able to go into this detail.
78771	18	41	18	42	This statement is wrong. Afferostation leads to a higher albedo over snow covered areas and a lower albedo over non-snow covered areas. [Yasemin Aktas, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text has been removed due to space limits.
17037	18	47	18	47	It's important to mentioned why the global mean surface temperature response to urbanization is neglible, ¿Does this depend of the size of the land cover/use which is changing to urban zones or the involved process? [Maria Amparo Martinez Arroyo, Mexico]	Noted. Text has been modified with link to global chapter2 for more information on urbanization effect at global scale.
22767	18	47	18	48	This was assessed in chapter 2 and there is no need to repeat this global assessment again. The releavnt aspect of the chapter 2 assessment should be quoted in its place and the section cross-referenced. [Peter Thorne, Ireland]	Accepted. Text has been modified and reference has been added to the assessment done in chapter2.
39215	18	47	18	54	The first statement is very confusing, especially with the messages in the following sentences that describe the responses of the regional air temperature in terms of minimum temperatures. [Lourdes Tibig, Philippines]	Noted. Text has been modified with link to global chapter2 for more information on urbanization effect at global scale.
4007	18	47	19	8	A lot of publications published about this topic, and the authors should read and make a proper assessment. Some of the papers were published in international journals, which applied more sophisticated methods and much objective classification of the urban and rural stations than those used in the works of 1980s-1990s. I would suggest the authors to make a more objective and balanced assessment. [Guoyu Ren, China]	Taken into account. More references has been added in the urban box 10.3

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16937	18	48	18	52	I recommend to consider the results of the study of Krayenhoff et al. (2018). Specifically, I would also recommend to mention their finding related to the nonlinearity of the interactions between urban expansion and climate change. This aspect is particually interesting and highlight the importance of interactions which can only be captured using regional downscaling approaches. I would suggest to add a sente like the following: "In North America, Krayenhoff et al. (2018) found that urban expansion and climate change (RCP8.5 scenario) would interact nonlinearly, providing a small offset to urban and climate change-induced summer night-time warming over the twenty-first century." Reference: Krayenhoff, E. Scott, et al. "Diurnal interaction between urban expansion, climate change and adaptation in US cities." Nature Climate Change 8.12 (2018): 1097-1103. https://doi.org/10.1038/s41558-018-0320-9 [Gianluca Mussetti, Switzerland]	Taken into account. Suggested reference has been added and text has been modified.
39217	18	48	18	54	Urbanization may amplify regionally the air temperature response to climate change in different climate zones either under present or future conditions with strong impact on minimum temperature.Yet there is an ES statement (lines 16-17, page 9) that there is very high confidence that annual -men maximum temperature is less affected than annual-mean minimum temperature by historical urbanization. I refer you to "may amplify" [Lourdes Tibig, Philippines]	Noted. text has been modified.
116947	18		18		For biophysical effects of land cover and land use, please build on SRCCL, including or urban effects. [Valerie Masson-Delmotte, France]	Accepted. This is now included in the Box 10.3 of urban climate and in the rewritten section 10.1.3.1.
73815	19	1	19	39	I wonder if there is any reason why ENSO is not included in this section given its significant impacts on regional climate (e.g. over India, southern Africa,etc). In fact, ENSO is the only phenomena that's not mentioned here but it is later included in the teleconnection assessment. [Rondrotiana Barimalala, South Africa]	Noted. The section has been worked over and shortened. Detail on individual modes can now be found in Annex VI. Therefore, also no reference to ENSO has been added.
41927	19	1	19	39	It is surprising that the ENSO, which is the most predominant driver of regional climate variability around the world, is completely missing in this section. [Rupa Kumar Kolli, India]	Noted. The section has been worked over and shortened. Detail on individual modes can now be found in Annex VI. Therefore, also no reference to ENSO has been added.
55155	19	2	19	2	It should be noted that multi-variable dependence might also change due to climate change (e.g. intensification of compound events) and therefore preserving such dependence in bias adjustment/correction might not necessarily be optimal for certain variables/cases. [Nancy Hamzawi, Canada]	Noted. The comment is not relevant for this section, the page and line referred to by the reviewer does not mention bias adjustment. The comment seems to refer to the Cross-Chapter Box 10.2. The multivariate dependence is discussed in Section 10.3 in the FGD.
91019	19	3	19	3	"Internal climate variability… is is a strong internal driver of regional climate". This is a rather meaningless statement. [Francois Engelbrecht, South Africa]	Noted. The sentence has been rephrased to avoid that it sounds tautological.
20617	19	3	19	4	According to the present reader, this sentence expresses a misconception, since internal variability is an intrinsic property of the climate, whether global or regional [philippe waldteufel, France]	Noted. Drivers is not to the same as external forcing. A driver of regional variability can be both external forcing as well as internal modes of variability. This section discusses the latter type of driver.
22769	19	3	19	8	This paragraph should mention the annex on modes of variability which is where the definitional aspecst and key figures on modes of variability can be found. [Peter Thorne, Ireland]	Noted. A link to the annex has been added.
82661	19	7	19	7	Modes of variability are defined in Annex VI should also be referenced here. [Blair Trewin, Australia]	Noted. A link to the annex has been added.
100813	19	7	19	8	Here it should be included the reference to Annex VI, where the climate modes of variability are defined and described. [Corti Susanna, Italy]	Noted. A link to the annex has been added.
70913	19	10	19	12	I don't see any of these modes indicated in Fig. 10.2 [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Neither figure nor text are meant to be comprehensive. In fact several examples from the text have been deleted. PDV and AMV are mentioned in both, NAO and SAM only in the main text to avoid overloading the figure.
91021	19	10	19	13	Please refer to the Mov Annex. [Francois Engelbrecht, South Africa]	Noted. A link to the annex has been added.
22771	19	10	19	13	Where is the reference to the very substantive assessments of this topic in chapters 2,3,4 and 8? [Peter Thorne, Ireland]	Noted. The reference is in the introductory paragraph (apart from Chapter 8, which was erroneously referred to as Chapter 9).
100815	19	11	19	13	Here as well should be included the reference to Annex VI for NAO, NAM and SAM (AVI.2 AVI.3). [Corti Susanna, Italy]	Noted. A link to the annex has been added.
44255	19	11	119		Hist-aer models results are presented with blue line and hist-GHG models results are presented with grey line in Figure 10.25 (c), as correctly described here in Figure caption. However, the blue and grey colours are opposite in the legend (Figure 10.25 (c) - upper left part), which should be corrected. [Nektarios Chrysoulakis, Greece]	Accepted. The mistaken line identification has been corrected in the FGD version of this figure.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
95917	19	15	19	15	"...in different seasons..." can rewrite like "...various seasons" i.e. avoid over use of "different". [Joseph Mutemi, Kenya]	Noted. But various implies a high number of seasons and is thus the wrong term. To be precise we keep the wording.
91023	19	15	19	20	SAM and its variability is discussed in considerable more detail in Chapters 3, 4 and the MoV Annex. Please check for consistency and refer to these Chapters and Annex. [Francois Engelbrecht, South Africa]	Noted. References to Chapters 3 and 4 have been in the SOD, the link to Annex VI has been added.
70915	19	17	19	18	It's very confusing to say that the SAM has variability that can be attributed to natural processes. Isn't this true of all modes of variability? As written, the implication is that other modes do not. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	The SAM-case has been clarified to be an example.
50047	19	19	19	20	Regarding the non-stationarity of teleconnection patterns more papers, using different techniques to study them, could be added, see for instance Zubiate et al. (2017) https://doi.org/10.1002/qj.2943 and Rousi et al. (2020) https://doi.org/10.3390/cli8010013 [Eftychia (Efi) Rousi, Germany]	Rejected. The purpose of this paragraph is not to discuss techniques but rather the issue of non-stationarity.
5611	19	19	19	20	Add the ref Rossi et al., 2011 Global Planetray Change for teh teleconnections [Benoit Laignel, France]	Rejected. This is a pre-AR5 reference.
100817	19	22	19	24	Reference to Annex VI needed for AMV (AVI.8), IOD (AVI.5) and PDV (AVI.7). IPO should also be referred in relation to PDV (or as PDV). Annex VI.7 discusses also the different flavours of Pacific Decadal Variability eg IPO, PDO, SPDO, TPDV… [Corti Susanna, Italy]	Noted. A link to the annex has been added.
102499	19	23	19	23	In Chapter 2, Pacific Decadal Variability (PDV) Is called Pacific Decadal Oscillation (PDO) [Philippe Tulkens, Belgium]	Noted. PDV and PDO are not the same. We now only refer to PDV as a general example.
42723	19	23			In other chapters the term PDV is used to encompass both the PDO and the IPO - arguably the IPO therefore does not need to be mentioned separately here. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The IPO has been deleted.
115283	19	24	19	24	Buckley and Marshall, 2016 is used as a citation for AMV, where as the paper actually reviews AMOC (which is a important component that can contribute to AMV, but not the only one). I suggest that this is either not an appropriate reference for AMV or the authors wanted to refer to AMOC here instead. [booth ben, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The reference has been deleted.
82663	19	24	19	24	IOD is (normally) an annual mode so should not be included in a list of multiannual modes (if IOD is there, then ENSO should be too). ENSO is currently absent from the text altogether - suggest a paragraph before this one which covers ENSO and IOD as major large-scale modes of interannual variability. [Blair Trewin, Australia]	Noted. The text has been shortened, and now neither IOD nor ENSO are listed. They can be found in Annex VI, which we refer to.
1319	19	34	19	34	Please explain what 'IPV' is. [Rasmus Benestad, Norway]	Not applicable. The example has been deleted.
45107	19	34	19	34	Has the abbreviation 'IPV' been explained in the text above? [Dmitry Kovalevsky, Germany]	Not applicable. The example has been deleted.
73817	19	42	21	51	There is a bit of inconsistencies how different parts of the box is written. The AR5 reads more like a review whereas the SRCCL/SROCC/SR1.5 are more assessments. [Rondrotiana Barimalala, South Africa]	Noted. The reviewer is right. There are so many assessment statements in AR5 that we decided to summarize the main content.
22775	19	42			I am not sure of the wisdom of this box given that many of the findings have been nuanced and / or superceded with new assessment findings in the 9 preceding chapters. It risks giving the impression that these findings are 'current' when in fact prior chapters have instead provided a new basis to start from. This also is at odds with earlier chapters (except for 8) who have embedded AR5 and SR along with preceding chapters findings (where applicable) at the start of each substantive assessment section rather than collecting these as is done here. For a whole of report consistency it would make sense to adopt a similar approach in teh current chapter and dispense with the current box in favour of such an approach. [Peter Thorne, Ireland]	Noted. We thought of this alternative way of collecting the knowledge from the previous reports because our chapter is methodological and fairly new. It helps setting the scene for many of the new aspects the chapter assesses.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35181	19	51			before AR5 I would have mentioned AR4 in which we can find a strong regional chapter in WGI (Christensen, J.H., B. Hewitson, A. Busuioc, A. Chen, X. Gao, I. Held, R. Jones, R.K. Kolli, W.-T. Kwon, R. Laprise, V. Magaña Rueda, L. Mearns, C.G. Menéndez, J. Räisänen, A. Rinke, A. Sarr and P. Whetton (2007): Regional Climate Projections. In: Climate Change 2007: The Physical Science Basis. Contribution of Working Group I to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change [Solomon, S., D. Qin, M. Manning, Z. Chen, M. Marquis, K.B. Averyt, M. Tignor and H.L. Miller (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA.) [SAMUEL SOMOT, France]	Noted. This is a good idea, but we were specifically requested to start the assessment from AR5.
66307	19		19		Box 10.1 the section on AR5 fits the box, it is less clear how the other 3 sections are fitting the purpose of the box since they do not highlight the regional information. [Erika Coppola, Italy]	Accepted. The box has been largely rewritten to focus on the relevant material from previous reports.
116949	19		19		Please refer to the Annex on modes in 10.1.4.2. I suggest to add a table in that Annex on in this chapter on the relative strength of effects of modes for regions and key variables like temperature and precipitation. It is currently missing and could be built of the approach implemented in the Annex. [Valerie Masson-Delmotte, France]	Noted. A table has been added to the Annex VI, as well as a reference to both Annex and table.
79325	20	3	20	3	"arise" instead of "arises" [Prodromos Zanis, Greece]	Rejected. The verb refers to "added value", which is singular.
20619	20	7	20	10	The quotation of AR5/WGII/Ch21 is correct. The confidence evaluation, however, does not belong to it; hence it is your own evaluation. How was this evaluation reached? Where is the evidence? Ch21 makes an authoritative statement, which clearly cannot be assimilated to evidence. [philippe waldteufel, France]	Accepted. The confidence statement has been removed.
110911	20	18	20	21	There was an additional point made in AR5 that isn't captured clearly by this comment, that I think is exceptionally important to this chapter and should be worked in here. Christensen et al. 2013, p 1255: "[c]redibility in regional climate change projections is increased if it is possible to find key drivers of the change that are known to be well-simulated and well-projected by climate models." [Melissa Bukovsky, United States of America]	Accepted. The sentence has been added.
20621	20	18	20	21	Hardly anybody will disagree with these statements. Note however that, in a scientist approach, even high quality observations and performing simulation models are not enough to ensure reliability: in addition it is necessary to interpret and understand what one observes, and what the simulation produces. [philippe waldteufel, France]	Noted. This is one of the reasons why Chapter 10 focuses on the additional steps required to produce climate information, which include process understanding among many other things.
15639	20	23	20	23	I don't understand what "The chapter noted" refers to. If this is still referring to WG2 AR5 Chapter 21, then I suggest avoiding paragraph breaks (or make explicit what "The chapter" refers to) and use past tense for AR5 material. [Samuel Morin, France]	Accepted. The sentences referring to that report have been reduced to shorten the box and the sentence has been corrected according to what the reviewer mentions.
1321	20	25	20	27	Some important reasons why the uncertainy has not appeared to reduce with downscaling is (a) because downscaling has been limited to a small number of GCMs inspite of the fact that each GCM simulations produce pronounced regional multi-decadal internal variability (Deser et al., 2012; Nature Climate change - also se Mezghani et al. (2019) DOI: 10.1175/JAMC-D-18-0179.1), (b) because most reported downscaling has involved RCMs which do not constrain the solutions the same way as ESD does by incoprorating additional and more independent information from observations. The mainstream downscaling community may have been somewhat selective and biased when citing colleagues resulting in the impression that downscaling does not improve the picture on uncertainty. It's absolutely vital to consider climate change in terms of probabilities and use large multi-model ensembles that are evaluated against historical data to quantify the range of possible future outcomes, given an emission scenario. On a regional scale, much of the uncertainty reange is associated with stochastic internal variability, but it's possible to use common EOFs to get information about model shortcomings. [Rasmus Benestad, Norway]	Taken into account. The points mentioned by the reviewer are assessed in section 10.3, precisely because previous report did not go in detail through them.
35183	20	34		41	Not sure this paragrph is understandable. To be revisited ? [SAMUEL SOMOT, France]	Noted. The understandability of the paragraph has been checked in internal reviews.
20623	20	39	39	31	According to this presentation, it looks like the top down perspective leads to adaptation, while the bottom up leads to mitigation? It would be of high interest to hear what the users have to say [philippe waldteufel, France]	Rejected. We do not see how the reviewer reaches such a conclusion. It would help if he had provided a reference to follow the argument.
20625	20	54	21	3	On the other hand, irrigation may contribute to raise relative humidity in such a way that the overall result is detrimental. See Kang et al., Nature Communications, DOI: 10.1038/s41467-018-05252-y [philippe waldteufel, France]	Noted. Irrigation is one of the regional drivers assessed in 10.3.3.6 to cover this gap.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116951	20		20		the box on previous reports is a good synthesis but it would help a reader if this could be used in the corresponding sections as a starting point, and with summary statements being explict on whether this assessment strengthens these points, or leads to changes, or addresses new aspects not previously covered. [Valerie Masson-Delmotte, France]	Noted. The box sets the scene in the rewritten introduction section.
71603	21	5	21	11	Although most of the affirmations reflected in this paragraph include the corresponding level of certainty, the latest ones referring to the urban areas and urbanization effects are not properly referenced. [Sixto Herrera, Spain]	Accepted. Confidence levels from SRCCL have been inserted for the last two sentences.
111567	21	9	21	9	Statement in parentcess should be eather very likely or high confidence [Volodymyr Osadchy, Ukraine]	Noted. The removal of "very likely" has been requested in the corrigenda of the FGD.
54369	21	9	21	10	what is the confidence of the fact that "urban areas stimulate storm occurrence and heavy precipitations in part due to the presence of aerosols"? Because it is not that obvious, I think.Whatever the confidence is, it should be defined also here since previous sentences have such. [Gabriel Stachura, Poland]	Noted. The reference is the report itself. However, additional evidence is provided in Box 10.2 on urban climate in AR6 Chapter 10.
14831	21	13	21	29	SROCC also put the recent changes in ocean and the cryosphere into a longer context. This should appear as well in this summary. For example recent reconstruction of Antarctic climate could be highlighted. [Marie-France Loutre, Switzerland]	Noted. These aspects are dealt with in Chapter 9.
35185	21	13		29	I feel relevant to write that SROCC report did not address specifically climate change for the regional seas. As climate change for those specific areas (not well represented in GCM) is not assessed in AR6 too, it means that information for regional seas are not assessed anywhere by IPCC. [SAMUEL SOMOT, France]	Noted. We let Chapter 9 know about this request.
15641	21	14	21	29	Maybe I missed the point of this Box, but I thought its purpose was to assess how regional climate information was addressed in previous IPCC reports, on a methodological standpoint. This is how the Box starts, explicitly for AR5 (WG1 and WG2). For SROCC, I read here a quasi-random (sorry) compilation of statements extracted from SROCC, without clear regional focus on a methodological standpoint. There are specific statements on SROCC, for example on strengths and limitations of methods used e.g. for mountain regions in SROCC Chapter 2, see e.g. section 2.2.1, Box 2.1 on limits of observational and model-based evidence for climate change in mountain regions. [Samuel Morin, France]	Accepted. This is a limitation of the box, mainly driven by its short length. The selection of sentences has been made to align them as much as possible with the examples mentioned in Chapter 10.
16085	21	20	21	25	"it is virtually certain that…": It would be good to introduce the sentences containing SROCC assessments with "The SROCC assessment is that…" or something similar - otherwise these SROCC assessments can be misunderstood as being also the AR6 assessments. There is a risk that this leads to inconsistencies due to possibly different assessments otherwhere in AR6 (notably in Ch 9). [Gerhard Krinner, France]	Accepted. The sentence has been changed.
125615	21	27	21	29	The degree of sea-level rise, and therefore its hazard on islands and coastal regions, is certainly sensitive to emission scenario. But the sentence seems to indicate that the sensitivity itself depends on emission scenario? Is that the intent? If so, then further explanation is needed. [Trigg Talley, United States of America]	Accepted. The sentence has been changed.
91025	21	31	21	50	The authors should also point our the SR1.5 analysis of "regional tipping points" and "regional hotspots". It is also important to point out the SR1.5 analysis for regions included both changes in the physical climate system (WG1) and impacts on natural and human systems (WGII). It is this combined analysis that resulted in the identification of climate change hotspots. There is also no reason to in this brief summary refer specifically to the findings for the Mediterranean. [Francois Engelbrecht, South Africa]	Noted. We had to choose some elements of the SR1.5 to keep the box length under control.
14833	21	31	21	51	SR1.5 also put the recent climate changes into a longer context. This should appear as well in this summary [Marie-France Loutre, Switzerland]	Accepted. A substantial number of additional references to SR1.5 have now been introduced in the chapter.
34715	21	40	21	41	The following statement needs a reference (there is substantially lower risk for human systems and ecosystems in the Mediterranean region at 1.5°C compared to 2°C). [Salah Ajjur, Qatar]	Rejected. The reference is the report itself.
125617	21	40	21	41	Why call out the Meditarranean as a specific region here? It just makes it feel like the report is European-centric. [Trigg Talley, United States of America]	Noted. There is a risk for the chapter to look Euro-centric, but one of the threads of the chapter is how to build a case study of the production of climate information, and the Mediterranean is one of them (there are three in total). The Mediterranean is mentioned in several places of the chapter to close the chapter with one of the case studies. Something similar is done for South Asia and the Cape Town area.
96087	22	1	24	34	Cross-Chapter Box 10.1: Well written and informative summary of research regarding the influence of the Arctic on mid-latitude climate. [Nicole Wilke, Germany]	Noted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11637	22	3	24	32	The text in this box accurately portrays the fact that there are many possible mechanisms that may connect Arctic and mid-latitudinal climate, and that there is uncertainty in these mechanisms due to conflicting interpretations. However, much of the information presented in this box discusses the eastern Arctic (Kara/Barents Sea) and feedbacks in the Atlantic Ocean, such as with the NAO, and does not represent several important contributions that address linkages in the western Arctic and the western mid-latitudes, including feedbacks with the Aleutian Low. For example, Cvijanovic et al. (2017, doi:10.1038/s41467-017-01907-4) propose a relation between sea ice loss and decreased rainfall in California; Cox et al. (2019, doi:10.1029/2019GL083306) suggest that early sea ice melt is correlated with the Beaufort Sea anticyclone and the Aleutian Low; and Kennel and Yulaeva (2020, doi:10.1073/pnas.1717707117) propose that sea ice loss can trigger central Pacific El Niño events, and suggest potential feedbacks that result in a weaker late winter Aleutian Low. Yamamoto et al. (2017, doi:10.5194/cp-13-1111-2017) also make an important contribution from the paleoclimate record, proposing that there has been a teleconnection between the Aleutian Low and Arctic sea ice via the Bering Strait inflow throughout the Holocene. Furthermore, it seems appropriate to mention that these feedbacks are not universally driven by changes in Arctic sea ice extent, but also that Arctic sea ice may vary due to changes in North Pacific heat flux (e.g. Praetorius et al., 2018, doi:10.1038/s41467-018-05337-8; Zhang et al., 2020, doi:10.1029/2019JC015308). In summary, the text in this box would be strengthened by incorporating additional literature that addresses: 1) feedbacks between Arctic sea ice and mid-latitudinal climate in the western hemisphere, and 2) the potential for Arctic sea ice to be modulated by North Pacific heat flux. [Ellie Broadman, United States of America]	Taken into account. Some of the suggested references have been included to address better the feedbacks between the Arctic sea-ice and the mid-latitudes in the western hemisphere and the Arctic sea-ice to be influenced by atmospheric dynamic process, including North Pacific heat flux.
65185	22	3	24	32	Would be good to mention that recent studies have pointed to potential impacts even further afield than the mid-latitudes (namely the tropics). In particular the tropical Pacific. This includes both observational (Kennel et al 2020 [doi: 10.1073/pnas.1717707117]) and modelling studies (Deser et al 2015 [doi: 10.1175/JCLI-D-14-00325.1], Wang et al 2018 [doi: 10.1029/2018GL077325], England et al 2020 [doi: 10.1038/s41561-020-0546-9]). [Mark England, United States of America]	Taken into account. Text has been revised. The potential influence on tropics is mentioned and references have been added.
70317	22	9	22	12	In a study (Johannessen O.M., Kuzmina S.I., Bobylev L.P., Miles M.W. (2016), Surface air temperature variability and trends in the Arctic: new amplification assessment and regionalisation, Tellus A: Dynamic Meteorology and Oceanography, 68:1, DOI: 10.3402/tellusa.v68.28234), for evaluation of Arctic amplification, an Arctic Amplification Index (AAI) is defined as the ratio between absolute values of the Arctic and Northern Hemisphere trends, calculated in successive 30-yr periods with moving 1-yr intervals. The ongoing winter warming is characterised by values of AAI <~4; spring and autumn amplification correspond to AAI ~1.5-3. A tendency for the recent strengthening of Arctic amplification is revealed for all seasons, especially for winter. [Dmitry Kovalevsky, Germany]	Rejected. The suggested reference discusses Arctic warming. Cross-Chapter Box10.1 focuses on impact of Arctic warming on mid-latitudes. Arctic warming itself is discussed else in the report to which Cross-Chapter Box10.1 refers in the beginning.
38537	22	9	22	18	It should be mentioned somewhere, maybe right from the start in the box that despite theories, winter cold spells are observed to decline anywhere in the Northern hemisphere (van Oldenborgh et al 2019). It is a basic observational fact which is not presented here. van Oldenborgh, G. J., E. Mitchell-Larson, G. Vecchi, H. de Vries, R. Vautard, and F. E. L. Otto, 2019: Cold waves are getting milder in the northern midlatitudes. Environmental Research Letters, https://doi.org/10.1088/1748-9326/ab4867 [robert vautard, France]	Accepted. Text has been revised and reference has been added at the start of the box.
1323	22	13	22	14	Perhaps make a note that the albedo feedback cannot explain the warming during the Arctic winter, when the polar night embeds the whole region in darkness. The reasons for warming in the Arctic (e.g. the Barents region) is explained in the report Adaptiva Actions in a Changing Arctic (AACA, 2017; ISBN –13 978-82-7971-102-5 - as with the IPCC reports, this report too has been through multiple peer reviews) by the Arctic Monitoring and Assessment Programme (AMAP) under the Arctic Council: Seesection 4.2.3.1 in AACA (2017) for a discussion of various types. [Rasmus Benestad, Norway]	Rejected. Cross-Chapter Box10.1 discusses the impact of Arctic warming on midlatitudes. Artic warming is discussed in other sections. Beginning of the box refers to those sections.
16087	22	27	22	27	"and the impact" - maybe add "diagnosed", "proposed", or "projected" impacts [Gerhard Krinner, France]	Not applicable. Phrase has been removed.
6777	22	43	22	47	This discussion needs to recognise that the greater warming of high latitudes is most marked at low levels in the atmosphere. And warming is larger in the tropical upper troposphere than at low levels. Please see the paragraph at the foot of page 32 of Chapter 4 of the SOD, where it is stated that "... increases the meridional temperature gradient" in the upper troposphere. So "all other things" may not be "equal". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The increase in meridional temperature gradient in the upper troposphere and the impact on storm tracks is discussed later in Cross-Chapter Box10.1 in the assessment of the future impact of Arctic warming.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4181	22	43	22	49	This section elaborated the atmospheric circulation response to Arctic amplification and mentioned the associated with temperature anomaly over mid-latitude continent. Recently, a dynamical mechanism of the Arctic amplification affecting Greenland blocking maintenance and movement was presented by Zhang and Luo (2020, JAS, A Nonlinear Theory of Atmospheric Blocking: An Application to Greenland Blocking Changes Linked to Winter Arctic Sea Ice Loss). They pointed out the BDL(Baffin bay, Davis strait and Labrador sea) sea ice loss can weaken the zonal wind and meridional gradient of potential vorticity, further prolong the blocking lifespan and accelerate the blocking westward movement. This result is identical with Francis et al.. A systematic mathematical derivation supports for this research and I think it is significant for scientists and society to understand how climate change affect the atmospheric circulation. [Wenqi Zhang, China]	Taken into account. Reference added.
79169	22	43	23	9	The mechanism proposed in Mori et al. (2014, 2019a), Kug et al. (2015) etc., summarized in Section 10.4.1.2.5, is different from the zonally uniform variability (i.e. NAM) described here. [Yu Kosaka, Japan]	Noted.
20627	22	47	22	49	"may increase", "they frequently occur", "tends to occur"; the chapter 10 authors seem a bit hesitant [philippe waldteufel, France]	Noted. We want to stress the hypothetical character of these statements. This is in agreement with the "low confidence" statement at the and of the box.
10977	22	54	22	55	I think the NAM and the WACC are often thought to be more distinct than this text suggests. This section could also refer to section 10.4.1.2.5 where the Eurasian cooling is discussed in a lot more depth. [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Noted. In the text therefore the term "sometimes" is used to denote that the connection between NAM and WACC is often thought to be more distinct. The Eurasian section 10.4.1.2.5 has been removed due to shortening.
64839	23	5	23	6	Following Peings (2019) and Gastineau et al. (2017), the relationship between sea-ice and snow cover anomalies during the autumn and before NAO signals would be related to the Ural-Siberian anticyclone, trough anomalies of moisture and heat fluxes: Santolaria Otin, M., Garcia-Serrano, J. Ménégoz, M. and Bech, J.: "On the observed connection between Arctic sea ice and Eurasian snow in relation to the winter North Atlantic Oscillation", in review for Environmental research Letter. [Martin Ménégoz, France]	Accepted. Text revised and references included
20211	23	33	23	33	Change "criticised by" to "criticised for". [philippe waldteufel, France]	Taken into account. Text has been revised
10979	23	33	23	36	Overall, this box is very nice, and rightfully cautious about the effects of recent Arctic warming. You could also add a new Blackport and Screen paper to this list, suggesting any trends in waviness have not continued into recent years (DOI: 10.1126/sciadv.aay2880). [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Reference has been included.
125619	23	36	23	38	It is not accurate to state that Kretschmer et al. found no evidence that Eurasian snow cover impacts mid-latitude weather. They found a robust causal relationship between snow cover and Ural blocking, which is a key driver of polar vortex variability: "we even find some evidence that EA-snow can influence AO directly" (page 4078). What they do conclude is that the evidence for sea ice is stronger. [Trigg Talley, United States of America]	Not applicable. Phrase has been removed.
82665	23	38	23	38	Check cross-reference - there is no section 9.5.4.6. Potentially also relevant to this discussion is uncertainty in observed autumn snow cover, as discussed in Chapter 9 (P79 L9-18). [Blair Trewin, Australia]	Taken into account. Reference to that section is removed. Uncertainty in observed autumn snow cover is not discussed in the box as it focusses on Artic-Midlatitude circulation.
84769	23	49	23	52	to consider also a recent published paper (Liang et al 2020 GRL https://doi.org/10.1029/2019L085397) that quantify the Arctic sea-ice driven variability for the Arctic and portions of mid-high latitudes depending on the number of members in AGCM multi-model ensembles [Annalisa Cherchi, Italy]	Taken into account. Reference added and mentioned that a large multi-model ensemble is needed for a statistical significant response.
42975	23	54	23	55	The statement is not wrong, but perhaps a bit misleading. There are/will be changes in the dynamical and therefore potential for more cold extremes in the midlatitudes. [Bodo Ahrens, Germany]	Noted. The inclusion of the phrase "without any additional changes in atmospheric dynamics", ensures that the statement applies to situations without changes in atmospheric dynamics..
59415	24	1	29	1	It would be helpful to include more 'confidence' / 'uncertainty' statements in IPCC style, beyond the 'virtually certain'. To be more in line with other sections that actively include those statements (e.g. 10.2.2.6.) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Section 1.4.2 has been removed. In the Cross Chapter box confidence statements have been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59417	24	1	29	1	The section is well written and clear. Though I wonder what the 'novel' / pressing elements to convey are compared to AR5 and/or what has been known for longer. Possibly this could be further highlighted, or the section could be shorthened. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Section 1.4.2 has been removed. The novel elements have been mode more clear in the Cross Chapter box.
4279	24	5	24	5	I think it should be “between Barents-Kara Sea ice loss” i.e., “with” → “between”. [Isla Simpson, United States of America]	not applicable text has been removed.
10981	24	5	24	6	Maybe just a confusing sentence - it suggests B-K sea ice loss in winter is linked to weakened storm tracks in summer, but I'm not sure that's meant? If these are related it's likely in the same season. [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Sentence has been removed.
87383	24	9	24	11	It might be useful to sum up the potential impact before to provide an assesment of confidence. [Didier Swingedouw, France]	Noted. It is not completely clear what is meant here. The structure of the Cross Chapter Box is to discuss the proposed impacts of the Arctic warming and next provide an assessment of confidence. Text has been revised to make this structure more clear.
20629	24	9	24	11	Typical example of a case where a confidence statement appears meaningless [philippe waldteufel, France]	Rejected. The confidence statement is about the exact role and quantitative effect. This does not rule out impact of Arctic warming.
24507	24	13	24	28	Using a large ensemble GCM experiment, Sato and Nakamura (2019) decomposed Eurasian summer temperature patterns into two components, namely the one associated with global climate change and the other associated with internal variability due to land-atmosphere interaction. They also pointed out summer hot extremes are linked with snow cover. It is suggested to mention that land surface plays an important role to amplify extreme events and its anomaly could persist over seasons and could bridge winter and summer climates. 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 Nakamura, T., K. Yamazaki, T. Sato, and J. Ukita, 2019: Memory effects of Eurasian land processes cause enhanced cooling in response to sea ice loss. Nature communications, 10, 5111 (2019), DOI: 10.1038/s41467-019-13124-2 [Tomonori Sato, Japan]	Taken into account. References have been added impact of snow cover has been mentioned.
87385	24	13	24	32	Here a discussion on the potential impact of AMOC changes on the Arctic, on meridional gradient and therefore on future behavior of mid-latitude might be useful, e.g. SROCC report chapter 6.7 and Haarsma et al. (2015). Link with other chapter might do the job as well (e.g. chapter 4, 8) [Didier Swingedouw, France]	Rejected. The focus on this box is on the impact of the Arctic on the mid-latitude. How the AMOC affects the Arctic and the mid-latitude is relevant, but is outside the scope of Cross-Chapter BoxOC10.1,taken into account the limited length of the box.
102501	24	15	24	15	PDV = PDO? [Philippe Tulkens, Belgium]	Noted. PDV (Pacific Decadal Variability) is indeed the now accepted name in the AR6 report, instead of PDO (Pacific Decadal Oscillation)
125621	24	16	24	16	"and" is missing before "land-surface processes" [Trigg Talley, United States of America]	editorial. Text changed
1325	24	23	24	23	There is also a study that suggests stronger winter-time cyclonic activity in a warmer world both over the North Atlantic and the Arctic oceans (Parding et al., 2019; DOI: 10.1175/JAMC-D-17-0348.1). [Rasmus Benestad, Norway]	Taken into account. Reference has been included.
14823	24	24	39	39	This section completely ignore a third type of observation, i.e. the indirect observation. Climate can be reconstruction from various indicator (tree rings, sediments, ice, historical documents, …) over much longer time interval than direct observation or satellite information, somtimes for place where there is no direct measurement. [Marie-France Loutre, Switzerland]	Noted. This is recognized as a relevant source in sections 10.1.3.1, 10.2.3.2, 10.4.2.1, and 10.4.2.3.
4281	24	26	24	26	“equator-pole temperature gradient” → “equator-pole temperature gradient decrease” [Isla Simpson, United States of America]	accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
125623	24	30	24	32	Not sure what the statement that "there is low confidence in the dominant contribution of Arctic warming compared to other drivers" is based on but it should be clarified. This might be true based on modeling studies but is not true based on observational studies. The magnitude of influence of Arctic drivers is of comparable magnitude if not larger than tropical drivers. [Trigg Talley, United States of America]	Taken into account. Text has been revised to "there is low confidence in the relative contribution of Arctic warming to mid-latitude atmospheric changes compared to other drivers".
22777	24	32	24	32	Should this not be relative rather than dominant? [Peter Thorne, Ireland]	Taken into account. Text has been revised. Dominant has been removed and relative has been used instead.
79449	24	37	24	37	Section 10.2: to me one key issue here that is not made very explicit is to show how observational uncertainties change as we go from global to regional/local scales, not only because of the spatial inhomogeneity of observations but mainly because of natural variability. Is this discussion somewhere and I missed it? Also, I don't find very relevant to the chapter all the technical discussion about observations (types, challenges, modele evaluation, etc). Why not moving this technical description into the Annex I on observations? [Alejandro Di Luca, Australia]	Taken into account. The discussion on natural variability in given as much as possible under the different use case study in Chapter10. In section2 there is a subsection on observational uncertainties where example of uncertainty propagation from global to regional is given. We think it is too late to change the whole structure of section2 and we prefer to keep it as it is after the FOD and SOD reviews.
108979	24	37			There are similarities in this observation section with the observations sections of Ch11 (11.2.1) and the Atlas (Atlas3.1). There can be clarity if the Ch11 and Atlas observation sections and the aspect of observations that is covered by Ch10 are mentioned. [Gemma Teresa Narisma, Philippines]	Accepted. Text has been revised
22791	24	39			There is a very similar section in chapter 1. This should be cross-referenced and form the starting point for the present section. Redundancies should be removed. It is key that the section here and that in chapter 1 be consistent and I would suggest these be much more tightly co-developed in the FGD stage. [Peter Thorne, Ireland]	Taken into account. Link to global chapters is given as general introduction of section2.
125625	24	41	26	9	[ACCESSIBILITY] Much of Section 10.2.1.1 is basic statements about types of observations, coverage, and calibration. It's likely covered elsewhere, and could be dropped here for brevity. [Trigg Talley, United States of America]	Rejected: Observation issues has been described in Ch1 (1.5.1) and Ch2 (2.2.3.3, 2.3.1.2.1) but it focused only global behaviour of the aerosols, O3, or radiation. In Ch10, we focused on local resolution observation instruments development.
36329	24	43	24	45	Section 10.2.1.1. under "observational data" indigenous and local knowledge is missing. Possibility for inconsistency with Chapter 1. At the same time this knowledge or data sources can support gaps identified under line 49 same section in relation to scarcity of data from the Southern Hemisphere. This data/knowledge has an advantage as being locality and context dependent thus could be considered as part of local/community infomatation or used in a focused manner on communicating climate information to local/regional users. [PENDO MARO, Belgium]	Accepted: To pay more respect to "observation data" is agreeable, this kind of discussion has been done in Section 10.2.4, thus we add those information there.
111569	24	43	24	48	It will be good to mention here WMO role in establishing and coordinating the observing programs [Volodymyr Osadchy, Ukraine]	Taken into account: WMO's roll is very important. Their activity has been described in Ch-1 (1.5.1), introducing GCOS.
20631	24	43	25	11	While all this seems true, it applies to every spatial scale, both global and regional [philippe waldteufel, France]	Accepted. The introduction to Section 2 has been reformulated and is now focussed on regional and local climate.
7359	24	45	24	45	Does occultation refers to radio occultation or all occultation measurements? If it does refer to radio occultation, suggest to add radio here. [Axel von Engeln, Germany]	Taken into account: However, this paragraph has been removed
5499	24	45	24	46	Separate platforms from sensors in this statement like: "(e.g., space-borne, radar, reflectometry, occultation, and lidar observations)" --> based on various sensors (radar, reflectmetory, lidar, etc.) on various platforms (airborne, spaceborne, surface). [Jinwon Kim, United States of America]	Taken into account: However, this paragraph has been removed
78153	24	49	24	51	The information is unclear since the sentence is not well formulate [Houria Abahous, Morocco]	Taken into account: This sentence disappears
66301	24		24		Section 10.2 overlaps with Atlas section on observations [Erika Coppola, Italy]	Taken into account. Coherence between and reference to Atlas and to other chapters dealing with observations has been checked.
22779	25	3	25	5	The backbone of the observing system is synoptic weather sites and airport sites yet neither of these get a mention. Also, the assessment gives no view to the recent progress in database management see https://journals.ametsoc.org/doi/10.1175/BAMS-D-16-0165.1 [Peter Thorne, Ireland]	Accepted: Thorne et al. (2018) has been included as a reference
13637	25	5	25	5	¿Where supersite observatories are located? (Briefly explain please) [Maria Amparo Martinez Arroyo, Mexico]	Taken into account: "ideally located in rural areas"
22781	25	5	25	8	You are discussing reference network sites as defined by GCOS and articulated in the GCOS Reference Upper Air Network and proposed in the Global Surface Reference Network. See https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/joc.5458 (and yes I am uncomfortable with a second self cite suggestion!) [Peter Thorne, Ireland]	Taken into account: GCOS is an important system to be described. However, its been introduced in Ch1 (1.5.1).
78155	25	7	25	8	The affirmation is not always verifed. Adequate calibration , quality control and homogenization must be applied when observational data are recorded. [Houria Abahous, Morocco]	Accepted: P23L27-28: According to the comment, this sentence has been changed as 'Adequate calibration of instruments, quality control and homogenization are essential in these sites.'

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
82667	25	10	25	14	This is, of course, a very limited selection of such data sets - the text should make it clear that these are gridded (at least in the examples given) and that these are only a few examples ("several" could be replaced by "many"). It may also be better to restrict the examples to national/regional data sets (which most, although not all, of the stated examples are), and say this explicitly, to establish a clear separation from well-known global data sets. [Blair Trewin, Australia]	Taken into account. "Several" is changed to "Many" in order to express that these are some examples out of many possible.
22783	25	11	25	14	What does this really add? There is no information content as to what these are products of or where or for what they are applied. Furthermore they seem a somewhat eclectic mix and miss out major products I would expect to see in such a citation set. My feeling is that the paragraph could be cut prior to the present sentence with no negative implications. [Peter Thorne, Ireland]	Taken into account: Here we refer "impact study" examples which used these various temporal data
22789	25	16	25	50	This paragraph is too long but also doesn't cover things I would expect such as that products are from polar orbiter and geostationary orbits. It should be split into several shorter paragraphs and revised for completeness. [Peter Thorne, Ireland]	Taken into account: Fully modified and shorten.
125627	25	16	25	50	[ACCESSIBILITY] This section is well-written, however it contains a lot of background information and specific information on precipitation and other satellite data products that does not really need to be in an assessment report. Listing the collection of precipitation data sets does not provide useful information here. [Trigg Talley, United States of America]	Taken into account: We reconstructed this subsection to make it more compact.
44231	25	16	25	50	High spatial resolution global surface products should also be referred in this paragraph. For example: urban footprints (Marconcini, M., et al., 2018: Mapping the global settlement growth from 1985 to 2015-the world settlement footprint evolution dataset. In Proceedings of the AGU Fall Meeting Abstracts, Washington, DC, USA, 10–14 December; and Florczyk A.J., et al. 2019: GHSL Data Package 2019, EUR 29788 EN, Publications Office of the European Union, Luxembourg, 2019, ISBN 978-92-76-13186-1); land cover (Chen, J. and Chen, J. 2018: GlobeLand30: Operational global land cover mapping and big-data analysis. Sci. China Earth Sci. 61, 1533 - 1534); surface temperature (Parastatidis, D., et al., 2017. Online Global Land Surface Temperature Estimation from Landsat. Remote Sensing, 9, 1208); and surface albedo (Chrysoulakis, N., et al., 2019: Exploiting satellite observations for global surface albedo trends monitoring. Theoretical and Applied Climatology, 137, 1171 - 1179). [Nektarios Chrysoulakis, Greece]	Accepted: referred to the four items, urban footprints, LULC, Land surface temperature, and surface albedos. However, we re-construct this sub-sub-section.
20635	25	16	26	1	Is one given to understand that the "climate science community" might not belong to the scientific community? For the present reader of WG1, this is a major worry [philippe waldteufel, France]	Accepted: This sub-sub section has been reconstructed and that phrase has been removed.
20633	25	16	26	9	All this is not specific of sub global scales [philippe waldteufel, France]	Taken into account: However, this sub-sub section has been totally reformed.
125629	25	17	25	17	"none or sparse" --> "few or no" [Trigg Talley, United States of America]	Accepted: However, this sentence has been removed
125631	25	17	25	17	Many satellite products do not extend to polar regions, so it's not be fair to say "most" have global coverage. In fact, this is indirectly noted on page 26, line 7: "In order to fill the gap around the Polar Regions …" [Trigg Talley, United States of America]	Taken into account: Subsection 10.2.2.1 has been rewritten considering the comment (P23L38-P24L27)
22785	25	18	25	21	What is this doing here and what value does it add? If you are going to introduce ECVs do it at outset of the section and, more fundamentally IPCC should not be an infomercial for a given funding program. [Peter Thorne, Ireland]	Taken into account: We also added examples other than Copernicus, e.g. NASA's Decadal Survey and ESA-CCI, not to be infomercial for a certain programme.
82669	25	18	25	21	The reference to 54 ECVs seems out of place here and would be better in the introductory paragraph of the section, as ECVs are relevant to in situ observations too. [Blair Trewin, Australia]	Take into account. This sub-section has been reconstructed.
106575	25	20	25	20	Include a reference to the ESA Climate Change Initiative before the reference to C3S (as a lot of the C3S ECV products are generated in pre-operational form within CCI). [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference to ESA-CCI has been included.
108127	25	21	25	31	In regional climate assessments it is also important to bear in mind that the degree of discrepancy in satellite estimates of surface variables varies from one region to another (even in neighbouring regions), particularly for precipitation estimates derived from infrared brightness temperature and/or the reflectivity values. This could be in part associated with shortcomings of the algorithms used to estimate precipitation. A single algorithm is not always applicable to different regions of the world, therefore algorithms should be subject to a region-dependent calibration. Also, calibration is related to the data scarcity mentioned in section “10.2.2.3 Data scarcity” [Maria Bettolli, Argentina]	Take into account: We discussed on "uncertainty of observation data" in Sub-sub section 10.2.2.6, thus we write such issues there.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
78157	25	26	25	31	These two sentences might be more valuable when added to section 10.2.2.2 that describes causes of inhomogeneities [Houria Abahous, Morocco]	Take into account: These sentences remain in this subsection, but a note is added to Section 10.2.2.2 that this section 10.2.2.2 focuses on the homogenization of land stations.
125633	25	29	25	31	In addition to cross-calibration and recalibration, climate data records with absolute climate accuracy can be another approach to generating a long-term climate data record (Wielicki et al., 2013; doi:10.1175/BAMS-D-12-00149.1.) [Trigg Talley, United States of America]	Taken into account: SOD section 10.2.3.1 has been partially merged into FGD section 10.2.1.1(P24L34-54), and removed.
22787	25	31	25	31	This is not an example that follows from the text preceding it. [Peter Thorne, Ireland]	Taken into account: The "uncertainty" has been discussed in 2.2.6, thus information is input to that sub-sub section
13639	25	32	25	50	However, it is important to mention the weaknesses of these remote sensing datasets according to each region, in order to show the reader a broader perspective of the use of these products. [Maria Amparo Martinez Arroyo, Mexico]	Taken into account: "observed data uncertainty" is described in Sub-sub section 10.2.2.6, thus we write such information there.
1327	25	34	25	34	Check the latitude range of TRMM. Some versions of it at least covers 50S-50N (https://climatedataguide.ucar.edu/climate-data/trmm-tropical-rainfall-measuring-mission). [Rasmus Benestad, Norway]	Rejected: Orbit inclination of the TRMM satellite was 35degrees. Therefore, observation range of sensors onboard the TRMM satellite was 36.5N-36.5S at best, including the orbit swath. However, some of the 'TRMM products', as 3B42, 3B43 are produced from the TRMM constellation, which consists of 8-10 microwave sensors onboard other polar orbital satellites, X-calibrated with TRMM Microwave Imager (TMI), which crosses other polar orbits. Thus, they have data in 50N-50S range.
96089	25	38	25	38	What exactly are "extreme precipitation systems" in this context? Monsoon? Hurricanes? Please clarify. [Nicole Wilke, Germany]	Taken into account: P24L31-33:Rephrased to express that these data are used to "characterize" the regional extremes
1329	25	40	25	40	These satellite products are also important to study the semi-global rainfall area (Benestad, 2018; DOI: 10.1088/1748-9326/aab375) which by the way ought to be defined as an additional global essential climate variable because it gives an important description of the global hydrological cycle and the global rainfall statistics. [Rasmus Benestad, Norway]	Accepted : Benestad (2018) has been cited.
82671	25	52	26	1	The new WMO Stewardship Maturity Matrix for Climate Data is worth mentioning in this context too. http://www.wmo.int/pages/prog/wcp/wcdmp/hq-gdmfc/documents/AttachmentC.GuidanceBookletonWMOStewardshipMaturityMatrixforClimateData_06022019.pdf). [Blair Trewin, Australia]	Taken into account. This paragraph has been removed.
82673	26	1	26	9	This doesn't really relate to the preceding sentence so may be better in a separate paragraph? [Blair Trewin, Australia]	Not applicable. The section has been completely re written.
20637	26	7	26	8	Stating that sun synchronous orbits are chosen in order to fill the polar regions gap is not serious [philippe waldteufel, France]	Accepted: this sub-sub-section has been reconstructed.
106577	26	12	26	12	Need to add something in this section on the merged obs/reanalysis datasets, e.g. WFDEI (Weedon et al) and its updated version, WFDE5. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The section 10.2.1 is now very dense and it was decided to keep a reduced level of detail. However, the comment inspired the inclusion of the WFDE5 dataset in figure 10.6.
112043	26	12	26	41	Products combining renalysis and observations are also widely used and it would be good to describe both pros and cons. They are for instance the reference datasets for large collaborations such as ISIMIP which require a number of variables (WFDEI, EWEMBI, WFDE5, AgMERRA would be worth to mention). EWEMBI is used in Figure 10.5. [jose manuel gutierrez, Spain]	Not applicable. The suggested products are available in the Interactive Atlas
125635	26	14	26	16	Combine the first two sentences as "Derived products are created from raw datasets ..." [Trigg Talley, United States of America]	Accepted- Text revised.
68941	26	14	26	17	This paragraph is awkward and should be revised. Perhaps something like "Derived observational products are created from raw datasets (which may be collected from surface stations, remote-sensing tools, or research vessels) using either statistical interpolation techniques or numerical atmospheric and land-surface models." [Seth McGinnis, United States of America]	Taken into account. Text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
20639	26	14	26	17	This paragraph omits a major step, in which the raw data (often a voltage or even a numerical count) is converted into a meaningful physical quantity, through applying adequate measurement theory [philippe waldteufel, France]	Taken into account. Text revised.
125637	26	29	26	30	The sentence should be revised as "Reanalyses products are designed to merge irregular observations" via "models that encompass many physical". [Trigg Talley, United States of America]	Accepted- Text revised.
68943	26	29	26	35	The first sentence should explain how reanalysis works. Maybe something like "Reanalyses are numerical climate simulations that use data assimilation to incorporate observations." [Seth McGinnis, United States of America]	Accepted- Text revised.
125639	26	29	26	35	Add a reference to Liu et al. (2017). These authors used WRF at 4km resolution spanning North America for a retrospective 2000-2013 simulation, as well as future-climate simulations. Citation: Liu, C., et al, Continental-scale convection-permitting modeling of the current and future climate of North America. Climate Dynamics, 49, 71-95, 2017. [Trigg Talley, United States of America]	Accepted- Text revised.
1331	26	29	26	36	It is important to stress that the reanalyses incorporate a growing volume of observations from a growing number of sources over time, which makes them inaproriate for trend analysis (Bengtsson et al., 2004; DOI: 10.3402/tellusa.v56i5.14466). This is also stated further down, citing a more recent paper, but it may easily get missed if it's not noted together with the presentation of reanalyses. There is extensive use of trend analyses based reanalyses in the chapters which are subject to these caveats. There may also be a portion of the climate research community who are unaware about this limitation (the community includes a wide range of disiplines). [Rasmus Benestad, Norway]	Taken into account. Text revised.
22793	26	29	26	41	The global reanalysis discussion should be pointed to the chapter 1 assessment. Here the focus should be on regional reanalysis products and far greater detail should be given with the range of regional reanalysis products now available fully introduced and their benefits and caveats clearly articulated. It needs considerably more detail than is presently afforded to these products. But the global reanalyses discussion should be deferred to chapter 1 and cross-reference given. [Peter Thorne, Ireland]	Accepted- The regional reanalysis has been discussed in detail now in the subsection with a link to the global reanalysis.
1589	26	44	27	18	QC of records ought to be discussed in Ch 2. QC is important, but this is an assessment of what the literatre says, not what the problems in putting it together are. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text has been changed to a more assessment style and Chapter 2 has been made aware of the importance of the quality control work.
33083	26	44	31	11	accuracy of interpretation of global changes in regional and sub -regional observation should be cheched, https://ijnrr.areeo.ac.ir/ [Sahar Tajbakhsh Mosalman, Iran]	Taken into account. Section 10.2 has been revised entirely, including the coherence with chapter 2.
32753	26	44	31	11	accuracy of interpretation of global changes in regional and sub -regional observation should be cheched, https://ijnrr.areeo.ac.ir/ [sadegh zeyaeyan, Iran]	Taken into account. Section 10.2 has been revised entirely, including the coherence with chapter 2.
19455	26	44	31	11	Accuracy of interpretation of global changes in regional and sub-regional observation should be checked. https://ijnrr.areeo.ac.ir/article_2834_0552ad6ee8c07bc816e2156f9c34f9fe.pdf [Mostafa Jafari, Iran]	Noted.
59199	26	44	31		Author(s) should consider adding conflict of interest to challenges facing climate assessment. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. This is deeply discussed in section5.
106579	26	46	26	46	Need to say something here about estimating errors, good error estimates are essential for appropriate use of observational data. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The error estimation has been added among the list of aspects considered by the quality control.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73819	26	46	27	18	A summary statement is missing [Rondrotiana Barimalala, South Africa]	Noted. We assume the reviewer refers to an assessment statement. Not all sub-sub-sections need to have assessment statements. Some of them, like this one, assess issues identified in the literature that feed into the whole section.
125641	26	46	27	18	[ACCESSIBILITY] In Section 10.2.2.1 (Quality Control), there's nothing here that's unique to regional climate. Is it covered elsewhere? If so, it can be dropped here for brevity. [Trigg Talley, United States of America]	Taken into account. The subsection is very short for reasons of space, but given that there is very little about quality control in other chapters, the first paragraph in the revised version offers an introduction to the basic concepts. It then focuses on issues specific to regional climate.
22797	26	46			I find it very odd that there would be a section on quality control without even passing reference to quality control of station data holdings globally by GHCND (daily) and HadISD (sub-daily) or the range of national quality control programs applied to their databases. [Peter Thorne, Ireland]	Accepted. The WMO reports and representative references for the quality control of global and regional datasets have been included, in spite of the whole sub-sub-section being reduced.
82675	26	48	27	18	The current text intertwines quality control issues (which I would view as being issues which affect observations on timescales of months or less) with homogeneity/bias issues - in particular, the text on reanalyses at P27 L2-7 is a homogeneity issue, not a quality control one (as defined here). QC issues will often (although not always) have limited impacts on assessments on climate timescales. Whilst every data set is different, it may be worth discussing the issues around QC of conventional in situ meteorological data as an example - national meteorological services carry out QC to a greater or lesser extent (and often historical data have not received the same level of QC as recent data), but specific QC processes are still needed in compiling global-scale data sets - the papers describing data sets such as GHCN are a good starting point here. Also possibly worthy of mention are the challenges of maintaining data quality at source, particularly in developing countries, and the problems of instrument outages/breakdowns, data not being communicated etc. - WMO 2017 (p4-5) is a potential reference here (https://library.wmo.int/doc_num.php?explnum_id=4217). [Blair Trewin, Australia]	Accepted. The subsection has been shortened. It focuses more on the observational issues. The reference has been added.
78159	26	50	26	51	The sentence might be more developped. The Qc procedure does not depend only on the specific nature of the dataset. For example it could also depend on the expertise of the user [Houria Abahous, Morocco]	Noted. We do not fully agree with the comment as the user is not selecting the QC procedure, although as many other elements entering the production of regional climate information the QC should also consider the user aspects. The WMO has a set of criteria that should be used in the quality control of a dataset (mainly observational). The application of these criteria should lead to some objective information and the user does not define the criteria (at least not for specific purposes). The user plays a role in how the information is communicated and how it is used.
22795	27	2	27	7	This is a 5 year old paper talking about prior generation reanalysis products. As chapter 1 and chapter 2 both note newer products are much improved. This sentence could be used to discredit their analyses and thus needs to be removed or very substantively updated. The two papers in the second sentence also refer to old reanalyses versions. The conclusions may no longer hold for the newer products assessed in AR6 WG1. [Peter Thorne, Ireland]	Accepted. The sentence has been rewritten. The Kobayashi et al reference has been used as an example of problems with old reanalyses that are still widely used to validate some model aspects.
102503	27	2	27	11	When discussing QC in the context of reanalysis products it should be mentioned that in terms of representing observations obviously the underlying model plays a huge role. For virtually any site in the world, if one compares 3-4 of the most popular reanalysis products with respect to e.g. precipitation, local results are systematically different. Thus in practice QC of reanalysis products is based on past experience - one knows that for this area in Greenland, ERAI is too dry... [Philippe Tulkens, Belgium]	Accepted. The sentence has been modified.
20641	27	4	27	7	Certainly, no product elaborated from observations of the Earth system should be automatically regarded as a climate quality product, whether for global or regional applications. Some failures may escape quality control procedures. Torralba et al (2017), quoted here, report that some discrepancies in the JRA-55 analysis might be attributable to deficiencies in deriving wind speed, according to a personal communication from the Japan Meteorological Agency. This kind of thing happens. [philippe waldteufel, France]	Taken into account. The intention of the sentences about reanalyses is that they should not be taken at face value. Many of the variables are not systematically compared with point or gridded observations at the regional level. Besides, the choice of one single reanalysis for model validation ignores the fact that an ensemble of reanalyses might differ wildly, especially at the regional level.
1333	27	13	27	18	Gridded data are not suitable for studying extremes for variables such as 24-hr precipitation because interpolated between points is a weighted sum of surrounding points. This results in a spatial inhomogeneity when extremes are limited to single stations. This is referred to representation errors (Gervais et la. 2014; DOI: 10.1175/JCLI-D-13-00319.1). There is also some discussion about gridded product on p. 29 L22-34 where this could fit. However, splitting the discussion of different aspects of gridded data may make it more difficult to get the whole picture if they use this chapter as a look-up resource to seak some specific information. [Rasmus Benestad, Norway]	Accepted. Representativeness error is referred to in sub-sub-section 10.2.1.1 and now mentioned in this sub-sub-section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4009	27	21	27	55	In addition to inhomogenity, the systematic bias in the climate data should be assessed here. The bias comes from mainly the urbanization effect. The inhomogenity of data should be regarded as a systematic bias only in case of the temperature data sereis at one station or at a few stations. In a large area with enough observational sites, it is the random errors rather than systematic biases. However, the urbanization effect is the systematic bias in any case, and it is the largest source of uncertainty in monitoring and detection of climate change at present. [Guoyu Ren, China]	Rejected. Not supported by the literature. [Also the effect of urbanization is considered to be an inhomogeneity. Regionally, especially in China, this is an important effect. The literature does not support trend biases being mainly from urbanization globally.]
22803	27	21			This is not a fair reflection of the state of homogenisation at the present time. It does not reflect well the state of the art in particular on the derivation of national products and is very esoteric in its choice of references. I would suggest inviting Matt Menne or Victor Venema to help redraft this. They co-led a recent task team on homogenisation for WMO. The piece should be ordered: 1. Data issues; 2. State of the art techniques; 3. Global regional and national efforts; 4. Benchmarking performance. Each should be a substantive paragraph. The section should also liaise with chapters 1 and 2 who cover similar material (in particular chapter 1) and make appropriate cross-references to these. [Peter Thorne, Ireland]	Accepted. Text revised. Some of the references were somewhat exotic and have been replaced. I wrote the first draft and as far as I can judge this is a fair assessment of the homogenization literature. I have added Hansen et al. as a classical reference for this. Phil Jones is a typical citation, maybe not this paper, but we already cite this paper and the does make the claim in this paper as well.
78161	27	23	27	31	The introduction to this section 10.2.2.2 might be improved. The structure might first give a definition of time series inhomogeneities and then distiguish between envrionmental and technical sources of inhomgeneities. [Houria Abahous, Morocco]	Accepted. Text revised. [Adding a definition is a good idea. One has been added as the introduction of the section. I feel the distinction is already explained, although without using these terms.]
65043	27	23	28	4	A discussion on the correction of artificial trends could be included as well as homogeneity breaks, e.g. “Alexandersson, H. and Moberg, A. 1997: Homogenization of Swedish temperature data. PartI: Homogeneity test for linear trends. Int. J. Climatol. 17, 25-34.” and “Domonkos, Peter. "Efficiency evaluation for detecting inhomogeneities by objective homogenisation methods." Theoretical and applied climatology 105.3-4 (2011): 455-467.” [Magnus Joelsson, Sweden]	Rejected. Beyond the mandate of the report. [there is a limit to the length of the text, in noisy data it tends to be difficult to make the distinction and most homogenization methods correct for both types of inhomogeneities without making an explicit distinction.]
65045	27	23	28	4	It should be discussed what order of magnitude the adjustments done in the homogenisation process are compared to the climate signal (Tuomenvirta, 2000, ‘Cocheo, Claudio, and Dario Camuffo. "Corrections of systematic errors and data homogenisation in the daily temperature Padova series (1725–1998)." Improved Understanding of Past Climatic Variability from Early Daily European Instrumental Sources. Springer, Dordrecht, 2002. 77-100.’), ‘Brunetti, Michele, et al. "Temperature and precipitation variability in Italy in the last two centuries from homogenised instrumental time series." International Journal of Climatology: A Journal of the Royal Meteorological Society 26.3 (2006): 345-381.’). This stresses the importance of homogenisation. [Magnus Joelsson, Sweden]	Accepted. Text revised
1591	27	23	28	4	There is just too much here. Issues for screens in Europe were stated by Ch 2 to be essentially resolved. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. [It is unclear which edits are suggested. No arguments or references are provided for the optimistic claim about the influence of screen changes in Europe. Even if this this claim were true for Europe, I am unaware of any article making such a daring claim for the entire world and this report is about global warming.]
16939	27	25	27	27	The clarity of this sentence can be improved. Here "cooling" is not a reason, but rather a consequence. I would suggest changing to something like "Typical reasons for this are the urbanization of a station's surrounding (...), or its relocation which may lead to a cooling (...)" [Gianluca Mussetti, Switzerland]	Accepted. Text has been revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
82679	27	27	27	34	Somewhere in this section, it should be noted that site- or network-specific inhomogeneities, while important up to regional scale, have limited impact in global-scale land temperature estimates - Jones 2016 is a reference for this (doi: 10.1007/s00376-015-5194-4). [Blair Trewin, Australia]	Accepted. Text has been revised. [Have added an explanation of which inhomogeneities are important for single stations (and their sizes by request of another peer reviewer) and which ones are important for changes in the global temperature anomalies. The Jones paper is not the final word on this topic. It only considers the transition to Stevenson screens in Europe and urbanization. There are many more factors that happened over large parts of the Earth, such as the transition to automatic weather stations, the relocation of stations from cities to airports, spread of irrigation, improvements in station siting due to technology and a shift of focus from weather to climate change. The paper makes a good point that the SST inhomogeneities are likely more important than LST inhomogeneities.]
65041	27	31	27	31	Xu et al. (2013) can be added as reference here as they discuss a warm bias in the Chinese climate records induced by automatisation of weather stations [Magnus Joelsson, Sweden]	Rejected. [Xu et al. Is a good paper, but a general homogenization application paper (for the Chinese network). But it does not study the transition to AWS in much detail, the references included in the cited WMO report give a better overview than this single case study. In case you do would like to cite it next to the WMO report, I have added the reference to the word document.]
78163	27	31	27	33	Here it worth noting that there is two approaches : relatives and absolutes methods to reduce inhomogeneities [Houria Abahous, Morocco]	Rejected. [as absolute homogenization methods are not used much I feel that in this text we should not go into that much detail.]
104565	27	31	27	33	Zhou et al. (2018) recently compared different effects of the statistical homogenization with neighbouring stations as a reference between sparse and dense observation networks in China and further showed various impacts of their homogenized temperatures on assessing reanalysis products. It's better to update the citation '(Trewin, 2010)' as '(Trewin, 2010; Zhou et al., 2018).' Reference: Zhou, C., Y. He, and K. Wang, 2018: On the suitability of current atmospheric reanalyses for regional warming studies over China. Atmos. Chem. Phys., 18, 8113-8136. [Chunlüe Zhou, United States of America]	Rejected. [Trewin (2010) is a review paper cited for this very basic concept. If we would cite any paper using the concept we would have to cite thousands of papers.]
82677	27	31	27	33	Suggest adding "in conjunction with the use of metadata" to this sentence. [Blair Trewin, Australia]	Accepted. Text has been revised. [As requested.]
79645	27	37	27	37	Replace “A recent study suggests...” by “Recent studies suggest...” [Rodrigo Manzanas, Spain]	Accepted. Text revised. [As requested.]
7209	27	39	27	49	I suggest somewhere in this paragraph you refer to Behrangi et al. (2016) where they used estimates of temperature , humidity, and water vapor pressure from AIRS satellite to study drought. 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. [Ali Behrangi, United States of America]	Rejected. [We presume this comment was intended for another section but cannot identify what it refers to]
104577	27	43	27	44	For clarity, this sentence would be revised as: 'The uncertainties related to the homogenization procedure are determined by the ratio of break signal to noise of time series and the performance of the homogenization method used (Zhou et al., 2018; Zhou et al., 2020).' These two papers adopted the neighboring series and the verified reanalysis series as a reference to amplify the ratio of break signal to noise and discussed the uncertainties from the used homogenization. References: Zhou, C., Y. He, and K. Wang, 2018: On the suitability of current atmospheric reanalyses for regional warming studies over China. Atmos. Chem. Phys., 18, 8113-8136. Zhou, C., J. Wang, A. Dai, and P. W. Thorne, 2020: A new approach to homogenize global twice-daily radiosonde temperature data from 1958 to 2018. J. Clim., under review. [Chunlüe Zhou, United States of America]	Rejected. [The first reference does not study the influence of the SNR on homogenization.]

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
20643	27	43	27	44	Understanding this sentence is challenging. It uses "both", which announces two options, and then states three possibilities. In addition, it is recommended not to speak of "noise signal", as by definition noise is not a signal. Noise supplies a contribution, however. [philippe waldteufel, France]	Accepted. Text has been revised. [The word “both” has been deleted. They say: One person’s noise is another person’s signal. So I feel it is not wrong, but I have changed “signal” to “time series” or “series”.
111571	27	47	27	51	The paper could be of use in the assessment https://doi.org/10.1002/joc.6353 Homogenization of a combined hourly air temperature dataset over Romania by Alexandru Dumitrescu Sorin Cheval José A. Guijarro [Volodymyr Osadchy, Ukraine]	Noted. [Such papers are useful. As Chimani et al. Discus it is not enough to know the statistical properties of the corrections made to estimate the (stochastic) properties of the inhomogeneities present.]
15643	27	49	27	49	I suggest replacing the term "validation" everywhere by "evaluation". Indeed, "validation" conveys the impression that observations used as a reference for model simulations are perfect, which is of course not the case, because observations also carry uncertainties. "Evaluation" is more neutral in this respect, and better conveys the fact that models and observations are complementary ways at addressing the "reality", and that exploring differences/deviations between the two are a driver for scientific progress. [Samuel Morin, France]	Rejected. [It is customary in the field of homogenization to call these kind of studies “validation studies”. The study in question starts with simulated climate data, to which simulated inhomogeneities are added, so the truth is actually perfectly known.]
104567	27	51	27	51	Text would be added at the end of this paragraph to clarify the difficulties of daily data homogenization: 'It's currently still difficult to homogenize daily temperature data mainly becase daily data are influenced by not only large-scale synoptic fluctuations but also local processes that are complex and nonlinear, especially over topographically-complex regions (Zhou et al., 2020). Recently, Zhou et al., (2020) developed a new approach with the verified reanalysis data as a reference to homogenize sub-daily near-surface and radiosonde temperature for community use, such as studying regional extremes.' Reference: Zhou, C., J. Wang, A. Dai, and P. W. Thorne, 2020: A new approach to homogenize global twice-daily radiosonde temperature data from 1958 to 2018. J. Clim., under review. [Chunlüe Zhou, United States of America]	Accepted. Reference has been added.
20645	27	53	28	1	Why focus on regional scale? Why focus on warming? What is meant by "mostly also"? [philippe waldteufel, France]	Taken into account. [The paragraph has been edited for clarity. At scales below a region it is possible that the noise error dominates. Have edited the text to make clear that also improvement are expected at scale above regional. We are only this confident for temperature data as they have been studied in most detail. Many other climate elements are not correlated well from station to station, which is a problem as explained in the previous paragraph. If the network density is not sufficient, it is possible that only the trend bias is improved, but the that the trend error increase; see previous paragraph and references therein.]
21157	27	53	28	1	This whole paragraph is a bit unclear to me [Faranak Tootoonchi, Sweden]	Taken into account. [The paragraph has been edited for clarity. Hopefully that makes it clearer.]
22801	27	54	28	1	This text makes no sense as written and bears little methodological resemblance, if any, to modern homogenisation techniques. I would suggest engaging a relevant CA such as Matt Menne or Victor Venema to advise. [Peter Thorne, Ireland]	Taken into account. The sentence has been edited for clarity. Hopefully that clears the problem, which is not very accurately described.
42979	28	1	28	4	Satellite clims have been homogenized too: e.g. Brinkmann et al. (2014), doi:10.3390/rs6010352 [Bodo Ahrens, Germany]	Accepted. Text has been revised. [We have added a paragraph that points the readers to the literature on the homogenization of satellite data, and radiosondes]
22799	28	3	28	3	You could add the daily benchmarking performed by Killick et al., 2019? https://academic.oup.com/climatesystem/article/3/1/dzz001/5523091 [Peter Thorne, Ireland]	Accepted. Text has been revised.
80323	28	7	28	7	This section mentions data availability in Europe, South Asia and West Africa, as well as comment on data scarcity. Condom et al. (2020) reviews the available measurement netwwork for hydrological and climatological variables in the Andes, a region with prominent data scarcity. This paper can be found at: https://www.frontiersin.org/articles/10.3389/feart.2020.00092/full [Paola Arias, Colombia]	Accepted: Included a sentence on how Condom et al., 2020 reviewed the available measurement network for hydrological and climatological variables in the Andes, a region with prominent data scarcity.
39219	28	7	28	53	This discussion on data scarcity, lt alone quality, presents a graphic description of the degrading quality of observational data not just in regions specified in lines 23-24. Observation network densities throughout Asia, nt just in Siberia and the Tibetan Plateau, but also in countries in specific regions of Asia leaves much to be desired. This is also prticularly true for quality control and homogeneization in the previous discussion. [Lourdes Tibig, Philippines]	Accepted: Included Asia in the description, so it reads regions of Africa, Asia, Australia and South America

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102505	28	9	28	10	A particular reason for remote sensing not being able to resolve data scarcity can lie in the fact that, e.g., clouds may hinder relevant scenes to be recorded (e.g. during rainfall), [Philippe Tulkens, Belgium]	Accepted: A sentence has been inserted to reflect "the variation in the performance of the satellite products could for instance be due to clouds hindering relevant scenes to be recorded during rainfall
22805	28	9	28	12	This is a very odd opening to a section that presumably is on in-situ data scarcity. The section would read better were it removed or at the very least placed much later in the piece. [Peter Thorne, Ireland]	Accepted. Text revised.
20647	28	9	28	12	Inasmuch as this whole subsection points out how the scarcity of data is detrimental, it does not make sense to choose as a starting point to assess negatively satellite products, which indeed allow to remedy to this scarcity. Evidently it does not make sense either to prefer satellite only information in case ground truth is available. In weather forecasting numerical simulation centres, every data is assimilated, using now 4D-var schemes which optimize relative weights. Hopefully climate research will follow, at least for initialised projections. [philippe waldteufel, France]	Taken into account. Text has been revised.
125643	28	9	28	12	Be careful to not apply the deficiencies with space-based detection for precipitation with other data types (e.g, energy budget, etc.). The challenges vary significantly across variables. This chapter as a whole need to be written with more than just precipitation data in mind. This statement assumes that the chapter is only discussing precipitation, as precipitation is not even mentioned in the sentence. [Trigg Talley, United States of America]	Taken into account. Text has been revised.
82683	28	9	28	12	This sentence seems out of place as the remainder of the section is about in situ networks. [Blair Trewin, Australia]	Taken into account. Text has been revised.
1593	28	9	28	52	GPCC doesn't release the station data they have. This needs to be pointed out. If climatologists knew what they had, they would be in a much better position to improve daily and monthly in-situ precipitation data. An assessment could then say, should this situation continue. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Inserted a sentence: "GPCC, in addition to specifying the total number of observing stations used in creating their gridded dataset, could give more information about the station data they have."
1595	28	9	28	52	Not all E-OBS station data are freely available. KNMI are at least trying to get more freely available, but GPCC aren't. The different availability of long series across Europe is down to different policies in the different countries of Europe. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: The sentence inserted to address comment #1593 also takes care of this comment
65047	28	9	28	53	There could be a discussion of different gap-filling-techniques [Magnus Joelsson, Sweden]	Taken into account: Included a brief write up on the gap-filling techniques.
82681	28	9	28	53	A significant issue here, which is mentioned in the E-OBS context but is valid more broadly, is that it is common for regional data sets to incorporate information from only a small fraction of the observations that are actually being made - sometimes this will be a deliberate selection (using only the stations with the longest/most complete/highest-quality data), but often it is because the observations are not available to data set compilers, often for data policy reasons. This is a real issue (especially in developing countries) despite various attempts through WMO channels and elsewhere to address it. [Blair Trewin, Australia]	Accepted: Added a sentence to show that the resistance of data owners to share there is valid in other parts of the world, especially in the developing world. But changed the word resistance to reluctance
65049	28	9	132	53	Could crowd sourced data be interesting in solving the problem of data scarcity? [Magnus Joelsson, Sweden]	Taken into account: Included a sentence on the need to investigate the role crowd-sourced data could play in minimizing data scarcity
22807	28	15	28	20	This is not a good example. This is a single timescale, single ECV and single data collection centre. There is no similar drop-off in the nature of rainfall reports archived via GHCND at the daily timescale or by ISD at the sub-daily timescale. So, this highlights isues pertaining to data management and not a paucity of actual data. [Peter Thorne, Ireland]	Accepted: Included sentences to caution about data management issues that could be misinterpreted as coming from the paucity of actual data. "Care must be taken to identify issues pertaining to data management that could be misinterpreted as paucity of actual data. For instance, the explanation for the decline in the GPCC data from 1990 onwards must done appropriately. This is because GPCC is a single timescale, single ECV and single data collection centre. There is no similar drop-off in the nature of rainfall reports archived via GHCND at the daily timescale or by ISD at the sub-daily timescale."
22809	28	25	28	28	Another mislaeding example here. CRUTEM4 is now replaced by CRUTEM5 in chapter 2. CRUTEM is yet to take advantage of the significant international efforts to curate and make available improved global holdings (Rennie et al., 2014, GDJ) which increased the global available station count for monthly mean temperatures from c.7k to c.35k including addition of many African stations. [Peter Thorne, Ireland]	Accepted: Rephrased the sentence to illustrate apparent data scarcity could be due to reasons other than actual data scarcity as stated earlier. Also gave the reference for CRUTEM5 (Osborn et al., 2020 accepted in JGR)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1597	28	26	28	28	This is an Access issue. If countries put out more data on the SYNOP/CLIMAT networks, it would get used. For CRUTEM4, the number of stations have reduced, but the sites are just not being transmitted. SAWS needs to change their policies. This is the reason for the reduction. You know that too, so state it. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: Indicated that the decline in the stations since the 1980s could be due to data access issues. That is, countries not getting their data onto the SYNOP/CLIMAT networks for reasons other than non-operational stations.
1335	28	30	28	42	Should tidal stations and river gauges also be included here (local sea level and hydrological data)? Perhaps also mention problems concerning a represenative sample of extremes when the rain gauges are taken by the floods and anometers are blown away? (Problems capturing the extremes due to 'clipping' (cut-off/off the scale/out of calibrated range) may be indicated through an analysis of the statistics of new record-breaking indices (Benestad, 2008; DOI: 10.1029/2008EO410002)). [Rasmus Benestad, Norway]	Rejected: Tidal stations and river gauges could either be included or be left out of this discussion of data scarcity. However, as the focus is not on the computation of extremes, etc. the mention of the increase in uncertainty of the long-term estimates should suffice the discussion. In a discussion of the computation on the uncertainty for example, the different data sets or recognized parameters could be accounted for or mentioned.
20649	28	30	28	42	One must accept the fact that there will never be a ground network with the wished station density all over the land surfaces. And on this basis one must suggest ways forward. Certainly several scientists work to achieve the best compromises, accounting for the limitations of ground networks. This chapter is expected to report on such contributions. [philippe waldteufel, France]	Taken into account - text revised. The review comment refers to inadequacy of sampling which could be as important or serious as errors of measurements at the points sampled, or even more serious. Given that, this chapter is focused on methodology of constructing regional climate messages (or information), mentioning the need to recognize there are methods being used to handle this, should suffice. Note that some of the methods mentioned earlier regarding reconstructing or estimating missing data, could be used. For example, Sattari et al., 2017; Kanda et al., 2018
22811	28	32	28	33	This fails to acknowledge that a large number of countries have adopted open access data policies since AR5 or shared substantive holdings. It also fails to acknowledge the adoption of WMO resolution 60 which encourages the sharing of historical data holdings. [Peter Thorne, Ireland]	Accepted: A statement has been inserted to acknowledge the increase in the number of countries that have adopted open data access policies or shared holdings since AR5. Appropriate implementation of WMO resolution 60 will improve on the current situation even much better.
40583	28	32	28	34	Not sure if the wording of "resistance of some data owner" is proper here. [TSU WGI, France]	Accepted: Changed the word "resistance" to "reluctance"
110645	28	33			Related to the sparse observations even in Europe, a reference to the even worse situation for variables other than temperature and precipitation could be made. For instance, heat stress applications in Europe where humidity and wind speed is needed. ECA&D lacks data for these variables in large parts of 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]	Accepted: A sentence has been inserted to note the scarcity of other variables even in Europe, a region with high resolution dataset (e.g. Canavuena et al., 2020).
71605	28	35	28	36	Are the references well written? [Sixto Herrera, Spain]	Accepted: Typo with "and O" corrected
87387	28	36	28	36	Typo with "and O" [Didier Swingedouw, France]	Accepted: Typo with "and O" corrected
22813	28	38	28	42	These efforts are good and worthy but calling these out without recognising the substantive imprrovements that have been made to global holdings for a number of variables and timescales is not advisable. Where is the text around improvements in availability of monthly temperature data via the ISTI databank? Where is the discussion of the 130k stations made available via GHCND or the availability of sub-daily quality controlled holdings via HadISD? It is necessary to cover these aspects here for completeness surely? [Peter Thorne, Ireland]	Accepted: The ISTI, GHCND and HadISD initiatives have been mentioned with appropriate references.
1599	28	38	28	42	WCA&D has done nothing for years. SACA&D has gridded products, but little non-Australian data available. The South America network (LCA&D) has some good products. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: The LCA&D dataset mentioned.
42977	28	40	28	40	I think the APHRODITE data could be mentioned too: http://aphrodite.st.hirosaki-u.ac.jp/products.html [Bodo Ahrens, Germany]	Accepted: The APHRODITE's dataset has been mentioned
22817	28	44	28	44	Critical problems is too strong and risks undermining huge swathes of the report [Peter Thorne, Ireland]	Accepted: The word critical has been changed to "substantial"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
87407	28	44	28	44	In a similar concept, space-borne precipitation radar data are utilized to relate characteristics of rainfall systems, which cannot be represented in GCMs, to large-scale atmospheric fields, and enabled future projections of different types of precipitation systems by using CMIP5 RCP8.5 large-scale projections (Yokoyama et al., 2019). Reference: Yokoyama, C., Takayabu, Y. N., Arakawa, O. and Ose, T. (2019) A study on future projections of precipitation characteristics around Japan in early summer combining GPM DPR observation and CMIP5 large-scale environments, J. Climate, 32, 5251-5274. doi:10.1175/JCLI-D-18-0656.1. [Yukari Takayabu, Japan]	Accepted: This has been incorporated and used as things that we have never observed due to perpetual data sparsity.
22819	28	44	28	53	Paragraph (and indeed section) needs to make a far greater distinction between perpetual sparsity and changing coverage. At the moment the two issues are horribly conflated. There are two different problems here. 1. Things we have never observed; 2. things that we have observed but with changing spatiio-temporal efficacy. It makes no sense to conflate the two as done here. They lead to quite distinct problems leading to quite different challenges to performing regional assessments and the paragraph would be better split into two and to cover each aspect seperately. [Peter Thorne, Ireland]	Accepted: The paragraph has been split into two to separate data scarcity arising from changing coverage in observation station network resulting in problems with climate monitoring and perpetual data sparsity resulting in things we have never observed.
22815	28	45	28	47	What decline? There is very limited if any support for a decline globally and certainly insufficient to justify a virtually certain statement. The global holdings at daily, sub-daily and for many variables monthly show no decline. Some data centres and products may show declines but those are down to methodological and programmatic issues and not because we are actually taking fewer observations. If anything the global reporting as e.g. reported annually to GCOS AOPC meetings (I am the chair) is increasing. This statement cannot stand as it is. There are areas habitually not observed or where data is not shared and that can be stressed but to maintain a decline in observing capability globally as implied is not backed by available objective evidence. The bigger issue with long-term estimates of temperature and precipitation arises for the early period where scarcity is a true issue and where data rescue is key as discussed in both chapter 1 and chapter 2 where the reader should be directed to. [Peter Thorne, Ireland]	Accepted: The statement has been modified as follows: "It is virtually certain that the scarcity and decline of observations in some regions (but not necessarily globally), increase the uncertainty of the long-term global temperature and precipitation estimates."
1601	28	47	28	49	India is probably the worst country for making data available. You can but gridded data products. Again, in an assessment, come to some useful conclusions. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This has been used as an example to illustrate the statement that It is virtually certain that the scarcity and decline of observations in some regions (but not necessarily globally), increase the uncertainty of the long-term global temperature and precipitation estimates."
1603	29	3	29	34	If the data were more freely available, varieties of gridding algortithms could be compared. The assessment ought to conclude that studies are being constrained by the lack of available station data series. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
71607	29	5	29	7	The problem only affects temperature and precipitation? If there are not references this conclusion could be extended to all the variables. However, if the sentence is based on some contrasted publications they should be properly referenced in the text as has been done in the rest of the Chapter. [Sixto Herrera, Spain]	Noted.
41929	29	9	29	10	There is a third approach: combining observations, remote-sensing and reanalysis. For example, ENACTS - see Dinku et al., 2016 (https://www.researchgate.net/publication/310459256_THE_ENACTS_APPROACH_Transforming_climate_services_in_Africa_one_country_at_a_time_A_World_Policy_Paper/link/5ac92b31aca272abdc60ee25/download). More information at https://iri.columbia.edu/resources/enacts/. [Rupa Kumar Kolli, India]	Accepted. Text has been revised to include reanalysis and suggested reference has been included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
125645	29	9	29	20	This section is on gridding and challenges with regional climate change assessment. The text reads as if it is assumed that the only useful way to evaluate regional climate models is with precipitation data. As such sentences like "There are two main approaches to produce gridded datasets: (1) based on in-situ observations and (2) combining in-situ observations with remote-sensing data." However, there are data that can be observed and gridded from space without in situ. Are there even ground-based in-situ measurements for the TOA energy budget or cloud radiative effect? This section and chapter must consider the full range of climate system variables relevant to regional climate. Precipitation is not the only useful metric for model evaluation. This seems to be acknowledged in the sections discussing radiative forcing of regional climate. [Trigg Talley, United States of America]	Noted. In this subsection we have focused on high-resolution methodologies.
55489	29	10	29	13	Please include some South American examples, like Jones et al 2012, DOI: 10.1002/joc.3605 and Tencer et al 2011, DOI:10.1175/2011BAMS3148.1 [Matilde Rusticucci, Argentina]	Rejected. In this subsection we are focusing on methodologies not regional examples.
1605	29	12	29	14	There are new ensemble versions of E-OBS (Cornes et al. 2018) and also CRU TS 4 (Harris et al., 2020). The reason some merging with satellite data happens is that the observed station networks available in some regions are inadequate. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. In this section we have focused on high-resolution methodologies.
1337	29	13	19	13	If more examples are required for gridding rain gauges, there is an operational Norwegian product used for hydrological forecasting (Lussana et al., 2018; DOI: 10.5194/essd-10-235-2018). [Rasmus Benestad, Norway]	Rejected. In this subsection we are focusing on methodologies not regional examples.
44233	29	14	29	20	Moran's eigenvector filtering, Mixed effect models and Bayesian methods have also been used in the second category; for example: Nikoloudakis, N., et al., 2020: Spatial interpolation of urban air temperatures using satellite-derived predictors. Theoretical and Applied Climatology. doi:10.1007/s00704-020-03230-3; Beloconi, A., et al., 2018: Bayesian geostatistical modelling of PM10 and PM2.5 surface level concentrations in Europe using high-resolution satellite-derived products. Environment International, 121, 57 - 70; and Beloconi, A., et al., 2016. Estimating urban PM10 and PM2.5 concentrations, based on synergistic MERIS/AATSR aerosol observations, land cover and morphology data. Remote Sensing of Environment, 172, 148 - 164. [Nektarios Chrysoulakis, Greece]	Rejected. It seems that suggested literatures are not relevant for this subsection.
22821	29	17	29	20	These two afterthought call outs to specific efforts feel really odd and don't really fit with what otherwise is a fairly generic methodological paragraph. I would delete these from this paragraph. [Peter Thorne, Ireland]	Accepted. Text has been deleted.
108129	29	17	29	20	I suggest rephrasing the sentences in terms of assessment report text style instead of indicating individual papers results [Maria Bettolli, Argentina]	Noted. Text has been revised
22823	29	22	29	34	If you are going to discuss uncertainty in gridding then where is the substantive discussion of the quantification of uncertainty in gridded products? This has been a major innovation since AR5 with many more products now producing quantified uncertainty estimates oftejn using an ensemble based approach to sample parametric uncertainty. For example HadCRUT5, NOAA and NASA surface temperature products all now include ensembles. As does at the regional scale EOBs for multiple parameters. Where is the substantive discussion on challenges and improvements in uncertainty quantification and how they should be used at a regional scale? A single sentence at the end of the paragraph does scant justice to the substantive advances in this area. [Peter Thorne, Ireland]	Noted. In this section we have focused on high-resolution methodologies.
83635	29	22	29	34	I think that the work of Christoph Frei deserves more mentioning here, for example in the context of ensemble methods. Some of his publications are quoted already in other places of Ch 10, and more can be found at https://www.meteoschweiz.admin.ch/home/service-und-publikationen/publikationen/peer-reviewed.html, including most recent ones. [Petra Seibert, Austria]	Noted. But suggested website is inaccessible.
22825	29	24	29	25	This is not entirely correct. For DTR we showed that interpolation has huge impacts (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2015JD024584) . Temperature is able to be interpolated over somewhat larger scales but interpolation uncertainty is by no means 'small'. [Peter Thorne, Ireland]	Noted.
20651	29	24	29	25	Have you tried dealing with surface soil moisture measurements, for example? One gets the feeling, reading through this chapter, that climate is defined by surface temperature and precipitation. But this is not so, as every user will tell. Admittedly, this remark also applies to global climate reports and "assessments".. [philippe waldteufel, France]	Rejected. Not enough references for assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22827	29	26	29	26	Errors is the wrong term. Use uncertainties. If we knew an error we would correct it. An uncertainty is what you have here in that we know we are likely wrong but we do not know how we are wrong. [Peter Thorne, Ireland]	Accepted. Text has been revised.
24041	29	26	29	28	Beguería et al. (2015, https://doi.org/10.1002/joc.4561) highlighted that most gridding methods result in a reduction of the variance with respect to the observed data. [Roberto Serrano-Notivoli, Spain]	Noted.
82685	29	26	29	28	Also, a gridded data set may not representy extremes as well as it does means - an Australian example is King et al 2013 (https://doi.org/10.1002/joc.3588). [Blair Trewin, Australia]	Taken into account. An errata has been submitted to address the comment in the text and add the reference
24043	29	28	29	32	Please, consider adding, after the examples of effective resolution: "However, Serrano-Notivoli et al. (2017 https://doi.org/10.5194/essd-9-721-2017 and 2019 https://doi.org/10.5194/essd-11-1171-2019) used multiple logistic regressions to avoid this problem in high-resolution precipitation and temperature gridded datasets". This comment links with comment #3 [Roberto Serrano-Notivoli, Spain]	Rejected. Suggested literature does not seem relevant for this subsection since we have assessed methodologies.
24045	29	32	29	34	When using linear models (inference) instead of interpolation technniques, the uncertainty can be assumed as the error of the model, avoiding the high computational cost of the ensemble approach. Please consider includinng this reference explaining the process and with an updated literature review: Serrano-Notivoli et al., 2017, https://doi.org/10.3354/cr01476. [Roberto Serrano-Notivoli, Spain]	Rejected. Suggested literature does not seem relevant for this subsection since we have assessed methodologies.
71609	29	33	29	33	It should be datasets or data sets? The authors use both nomenclatures. [Sixto Herrera, Spain]	Accepted. Text has been revised.
22829	29	37	29	37	on not in as you are measuring the weather on the islands. [Peter Thorne, Ireland]	Not Applicable. Text has been put in the Cross-Chapter Box on small island.
83637	29	37	29	37	Observations are made on islands, not in islands. [Petra Seibert, Austria]	Not Applicable. Text has been put in the Cross-Chapter Box on small island.
78243	29	37	29	40	This sub-section (10.2.2.5) is quite incomplete. I think this sub-section is unnecessary if you don't give examples on the Maritime Continent or the South Pacific islands in, eg. sub-section 10.4.1.2. [Motoki NISHIMORI, Japan]	Not Applicable. Text has been put in the Cross-Chapter Box on small island.
71611	29	37	29	40	I suppose that this section will be fulfilled in a future version of the document, or the section simply refers to the case study on the Caribbean islands described in section 10.4.1.2.8? [Sixto Herrera, Spain]	Not Applicable. Text has been put in the Cross-Chapter Box on small island.
41077	29	37	30	29	These two sections (10.2.2.5 and 10.2.2.6) are not coherent to appear here while other sections in 10.2.2 talks about challenges from the perspective of data. [TSU WGI, France]	Accepted. Title of subsection 10.2.2.6 has been reformulated
67893	29	39	29	40	Sub-chapter 10.2.2.5 Observations in small island. This section gives example of the case of small islands in the Carribean. The Carribean is an archipelago, but the case presented is not a typical case of small island or of marine nature, but a case of drought. The results showed limited evidence and low agreement for the cause of the drying trend over the Carribean. It is unsure whether this trend is mainly caused by either internal decadal-scale variability or anthropogenic forcing. The case presented is also not a case of 'typical' climate change impacts for coastal areas, such as more frequent coral bleaching, harmful algae, and ocean acidification. There is a need for climate modeling case studies for islands with relevant climate change cases. [Ruandha Agung Sugardiman, Indonesia]	Not Applicable. Text has been put in the Cross-Chapter Box on small island.
82687	29	39	29	40	This section is obviously still to be developed, but one issue which should be mentioned is the lack of reference stations for homogenisation. McGree et al 2019 is a good reference for these issues for the Pacific islands - https://journals.ametsoc.org/doi/pdf/10.1175/JCLI-D-18-0748.1 [Blair Trewin, Australia]	Not Applicable here but reference has been put in the Cross-Chapter Box on small island.
80325	29	43	29	43	Condom et al. (2020) reviews the available measurement netwwork for hydrological and climatological variables in the Andes. This paper can be found at: https://www.frontiersin.org/articles/10.3389/feart.2020.00092/full [Paola Arias, Colombia]	Taken into account. The paper has been cited to support the sparseness in meteorological stations at high elevations. "High-altitude (>3000 m a.s.l.) permanent meteorological stations are limited and current knowledge is mainly based on valley bottom or low elevation meteorological stations (Qin et al., 2009; Lawrimore et al., 2011; Gultepe, 2015; Condom et al., 2020), which, generally, do not represent the higher elevation climate (Immerzeel et al., 2015; Shea et al., 2015). "
83639	29	49	29	50	“temperature, relative humidity and radiative fluxes are critical for climate model validation, but difficult to deal with due to complex interactions over mountainous terrain, and often need corrections (Gultepe, 2015)” The term “complex interactions” is not very clear. Actually, the problem is one of small-scale variability and representativity of station point measurements. [Petra Seibert, Austria]	Accepted. Text has been revised accordingly. "Measurements of wind speed, temperature, relative humidity and radiative fluxes are critical for climate model validation, but difficult to deal with due to their point-scale representativeness and small-scale spatiotemporal variability complex interactions over mountainous terrain, and often need corrections (Gultepe, 2015)"
22831	29	50	29	50	Adjustments instead of corrections. Corrections imply the changed data is absolutely correct whereas adjustments implies there may remain some uncertainty. [Peter Thorne, Ireland]	Accepted. "Corrections" has been replaced with "Adjustments"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83641	29	51	29	52	“Permanent meteorological stations are limited and current knowledge is mainly based on sporadic valley bottom or low elevation meteorological stations” In its generality, this statement is wrong. There are mountain observatories, foremost in the European Alps, but also elswhere, which have very substantially contributed to our knowledge. For example, the Sonnblick high-mountain (3100 m asl) observatory in Austria has an uniterrupted 130+ year record. The problem is not that we don't have mountain observatories, but that we don't have enough of them, and that there are vast mountain areas without any. [Petra Seibert, Austria]	Accepted. The sentence has been rephrased for a clear message. "High-altitude (>3000 m a.s.l.) permanent meteorological stations are limited and current knowledge is mainly based on valley bottom or low elevation meteorological stations (Qin et al., 2009; Lawrimore et al., 2011; Gultepe, 2015; Condom et al., 2020), which, generally, do not represent the higher elevation climate (Immerzeel et al., 2015; Shea et al., 2015)."
41199	29	54	29	54	wrong use of confidence language [TSU WGI, France]	Not applicable. Text no longer included in the subsection.
15645	29	54	30	1	I suggest to drop this sentence, or significantly rephrase it. The material assessed here focuses on methodological choices affecting trend analysis, and there is a spurious assessment on a trend itself. I think it is valuable to refer to data gaps at high elevation in mountain regions (see section 2.2.1 in SROCC Chapter 2), but there is absolutely no need to refer to EDW for justifying this. Furthermore, EDW has been assessed in SROCC Chapter 2 (Box 2.1 and section 2.2.1) and the evidence, when analyzed globally, is not unequivocal at all. I recommend utmost caution when referring to EDW almost as a statement of fact, because the evidence is far more heterogeneous. [Samuel Morin, France]	Accepted. The sentence has been dropped.
20653	30	1	30	2	"from" high elevations or "for" high elevations? Also, please explain why measuring precipitation amounts in mountainous areas is one of the most interesting tasks. [philippe waldteufel, France]	Taken into account. Text has been revised.
68185	30	1	30	4	maybe it would be worth mentioning mass balance measurements of glaciers in high mountain regions actually measure the solid precipitation, these are collected by the world glacier monitoring service (WGMS) [Guðfinna Aðalgeirsdóttir, Iceland]	Accepted. A sentence "However, the observed point mass balances at glacier accumulation area represent net snow accumulation (Zemp et al., 2009; Azam et al., 2018) and may be used to inversely estimate the precipitation amounts (Immerzeel et al., 2015; Sakai et al., 2015)." has been added.
83643	30	1	30	15	A new paragraph should start in the middle of line 1, which should be joined with the seoncd paragraph starting on line 6, as this belongs together. [Petra Seibert, Austria]	Taken into account. Text has been revised.
22833	30	2	30	2	avoid value laden judgements so replace is one of the most interesting but difficult tasks with particularly challenging [Peter Thorne, Ireland]	Accepted. "is one of the most interesting but difficult tasks" is replaced with "particularly challenging"
1339	30	4	30	4	The companies producing hydropower do have some idea of the amount of snow that melts in the summer and fills up their magazines. And there are some difficulties related to snow drift (wind-temperature history). So there is additional indirect information that perhaps could be mentioned. We are not as ignorant about the snow as the paragraph may suggest (e.g. Cherry et al, 2005; DOI: 10.1007/s11269-005-3279-z)? [Rasmus Benestad, Norway]	Noted.
41201	30	6	30	6	wrong use of confidence language [TSU WGI, France]	Not applicable. Text no longer included in the subsection.
22835	30	6	30	7	What is the defensible trace that can justify this? If it is the prior paragraph then it rests upon one cited study which doesn't justify very high confidence. I would expect a substantive set of supporting arguments and references to underpin such a very high confidence statement and I do not as presently presented see the obvious defensive trace here. [Peter Thorne, Ireland]	Noted. The justification is given in the same paragraph. Kindly note: among the given references, "Nitu, R., Roulet, Y.-A. R., Wolff, M., Earle, M., Reverdin, A., Smith, C., et al. (2018). WMO Solid Precipitation Intercomparison Experiment (SPICE) (2012- 2015). World Meteorological Organization (WMO)." is based on an extensive intercomparison experiment and provides the state of the art about challenges in snow measurements.
112621	30	10	30	11	Rain undercatch is also an issue, as errors can vary between 3 and 20% (see Prein and Gobiet, 2017: already cited in this chapter). [Marie-Estelle Demory, Switzerland]	Noted.
68945	30	11	30	11	Should this be "riming", not "rimming"? [Seth McGinnis, United States of America]	Accepted. "Rimming" has been replaced with "Riming."
3205	30	17	30	29	Recent studies have analyzed rainfall products over the Amazon-Andes transition region (where strong climatic contrasts are observed). For instance, Espinoza et al (2015.https://doi.org/10.1002/2014WR016273), Chavez and Takahahsi (2017. doi: 10.1002/2016JD026282.); Satgé et al (2019. doi: 10.5194/hess-23-595-2019.). In addition, studies have also documented the utility of rainfall products in the hydrological modelling (e.g. Zubieta et al (2017. https://doi.org/10.5194/hess-21-3543-2017), Wongchuig-Correa et al., 2017. doi: 10.1016/j.jhydrol.2017.04.019.) [Jhan Carlo Espinoza, France]	Noted.
38357	30	19	30	21	Taiwan is a province of China, not an independent country. The current expression is seriously wrong. The term “Taiwan” in line 20 shall be changed to “Taiwan, province of China”. [Yaming LIU, China]	Taken into account. Text has been revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22837	30	26	30	29	This is mixing gridded in-situ products with reanalysis based products with satellite based products. It should be split into three and the three types of data considered seperately. Where is ERA5-land which has far better orographic processes? Discussion of in situ should note limitations of interpolation and that some products (CRU in particular) revert to climatology when there is no constraint available. [Peter Thorne, Ireland]	Noted.
108121	30	27	30	27	Instead of the term “bias corrected” it might be more appropriate 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]	Accepted. "corrected" has been replaced with "adjusted"
15647	30	29	30	29	The recent study by Scherrer (2020 ; https://iopscience.iop.org/article/10.1088/1748-9326/ab702d) provides additional insights to the validity of reanalyses in mountain regions (case study in the European Alps). [Samuel Morin, France]	Noted.
22843	30	32	30	32	Section appears to be talking about structual uncertainty so why not call it that? [Peter Thorne, Ireland]	Accepted. Subtitle has been revised.
22839	30	34	30	35	How is this another source of uncertainty? Either the section is mis-titled or this doesn't logically belong here. [Peter Thorne, Ireland]	Accepted. Subtitle has been revised.
22841	30	35	30	36	observational structural uncertainty … model structural uncertainty. The term structural uncertainty has been used in earlier chapters to describe this spread and you should do so here for consistency. [Peter Thorne, Ireland]	Accepted. Subtitle has been revised.
108131	30	36	30	39	I suggest adding the following reference in this statement regarding the evaluation of observational uncertainty and model evaluation for precipitation Southeastern South America: Bettolli ML, Solman SA , da Rocha RP , Llopart M , Gutierrez JM , Fernández J , Olmo ME , Lavín-Gullón A , Chou SC , Carneiro Rodrigues D , Coppola E, Balmaceda Huarte R, Barreiro M , Blázquez J , Doyle M , Feijoó M , Huth R ,Machado L ,Vianna Cuadra S (2019) The CORDEX Flagship Pilot Study in Southeastern South America: A comparative study of statistical and dynamical downscaling models in simulating daily extreme precipitation events. Clim Dyn. Submitted Dec 2019. [Maria Bettolli, Argentina]	Accepted. Reference has been added.
38359	30	39	30	39	The expression of India-Tibet region here is ambiguous in location, and paralleling India (country) and Tibet (region) is not normative in expression. It is suggested that based on the SROCC relevant concept, ‘India-Tibet region’ be changed to ‘High Mountain Asia region’. [Yaming LIU, China]	Accepted. Text has been revised and one reference added for Central Asia high mountain area.
109761	30	43	30	43	Even over Western North America, observational uncertainty can lead to factor O(2) difference in multi-decadal precipitation trends (Lehner, F., C. Deser, I. R. Simpson, L. Terray (2018): Attributing the US Southwest’s recent shift into drier conditions, Geophysical Research Letters, DOI: 10.1029/ 2018GL078312). [Flavio Lehner, Switzerland]	Accepted. Text and reference has been added.
22845	30	44	30	47	This would have been a now replaced version of E-OBS without the ensemble presentation. Is this any longer thus a fair comparison to make? [Peter Thorne, Ireland]	Noted. No reference have been found addressing this issue.
41203	30	51	30	52	wrong use of confidence language [TSU WGI, France]	Accepted. Text has been revised
112623	30	51	30	53	Prein and Gobiet (2017) could be cited here, as it is the main recommendation of their study. [Marie-Estelle Demory, Switzerland]	Accepted. Reference has been added.
79577	30	51		53	This point highlights a robust approach to possibly reduce observational uncertainties in model evaluation, I think it is better to elaborate on it a bit more. For instance, “There is very high confidence (robust evidence and high agreement) that an ensemble of multiple observational references at a regional scale is fundamental for the model performance assessment”. An ensemble of multiple observations is, in my opinion, the best product for model evaluation especially in data-sparse regions. Also, it might be helpful to use this medium to state the need for Government meteorological agencies to make station data available to scientists in their countries, this will go in filling the data gaps. This and more are the critical issues discussed in Dike et al, 2018. Dike, V.N., Addi, M., Andang’o, H.A. et al. Obstacles facing Africa’s young climate scientists. Nature Clim Change 8, 447–449 (2018). [Victor Dike, China]	Accepted. Text has been revised and reference has been added in the appropriate subsection
22847	30	55	31	11	How does this differ from the earlier piece on interpolation and does it not thus belong there rather than here? [Peter Thorne, Ireland]	Accepted. Text has been revised. This part of the text is related to uncertainty propagation when downstream product are computed from satellite product that are subject to observational uncertainties.
1607	31	1	31	11	Maybe state how accurate PET estimates need to be, and at what timescale? [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The level of accuracy needed always depends on the purpose of study (for any variable)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
20215	31	3	32	4	Please refer at least to subsections 3.1.4.2 and 3.1.4.4. [philippe waldteufel, France]	Not applicable. Text no longer included in the subsection.
22849	31	16	31	19	There are many potential application pathways for observed products at the regional scale of which these two are not a comprehensive sample. Where is monitoring? Where is Downscaling or is that what you mean by post-processing techniques? I'm not sure this link text is actually helping here? [Peter Thorne, Ireland]	Not applicable. Text no longer included. General introduction has been put in the beginning of section2.
40351	31	16	31	19	How these two aspects repond to the following three sub-bulltets in 10.2.3? [TSU WGI, France]	Not applicable. Text has been removed. General introduction has been put in the beginning of section2.
112629	31	16	31	19	2 pathways are listed here, but there are 3 subsections, which are also not listed in the same order as the pathways, so I would suggest to reorganise this paragraph. [Marie-Estelle Demory, Switzerland]	Not applicable. Text has been removed. General introduction has been put in the beginning of section2.
13585	31	17	31	17	It is suggested to specify if it refers to climate change adaptation. [Maria Amparo Martinez Arroyo, Mexico]	Not applicable. Text has been removed. General introduction has been put in the beginning of section2.
38539	31	22	31	22	Could the section (or elsewhere in CH10) mention mention model tuning. There has been methods developed for this approach in a formalized way (see work of Hourdin et al) [robert vautard, France]	Rejected: Horrdin et al (2017) discussed on the model tuning for climate change itself. Here in this sub-sub-section, we focus on the tuning for the process of the physical scheme.
112625	31	22	31	22	The subtitle say "model evaluation" but the paragraph focuses on parametrization improvement. I would suggest to create another subsection specifically on model evaluation. [Marie-Estelle Demory, Switzerland]	Take into account: SOD section 10.2.3.1 has been partially merged into FGD section 10.2.1.1(P24L34-54), and removed.
125647	31	22	31	52	[ACCESSIBILITY] This section is neither a review of the ways that observations are used to evaluate models nor a collection of important messages assessing how observations are used and are not used. There are no conclusions about what ways observations are being used well or about how not to use observations to evaluate models. This is just a list of studies that use observations and compare against or constrain models. This information needs to be distilled into relevant messages. [Trigg Talley, United States of America]	Taken into account: SOD section 10.2.3.1 has been partially merged into FGD section 10.2.1.1(P24L34-54), and removed.
112627	31	22	31	52	About model evaluation, I would like to enhance that it is difficult to find precipitation observational datasets to validate high-resolution GCMs (such as HighResMIP) or RCMs (such as CORDEX) at 12-50 km resolution. Satellite observations are often too low resolution and national gridded datasets often lack a rain undercatch correction. [Marie-Estelle Demory, Switzerland]	Rejected: We have discussed on spatially and temporally high resolution satellite-based products as IMERG or GSMaP.
73821	31	22	31	53	This subsection is quite hard to follow. May be there is a lack of continuity in the different paragraphs? It might be much clearer if first assess how observations are used for model evaluation, give a summary statement and then assess the parameterization improvement part + summary statement [Rondrotiana Barimalala, South Africa]	Taken into account. This sub-section has been removed
35187	31	22		52	This paragraph seems to focus on very specific issues mentionning a lot soil moisture and satellite products. Is it generic and broad enough with such a general title ? I'm not sure [SAMUEL SOMOT, France]	Taken into account: 10.2.3.1 has been deleted
53535	31	22			This subsection could also mention the epistemological dimensions of model evaluation and parametrization development, especially the issues related to model tuning and parametrization overfitting (e.g., https://journals.ametsoc.org/doi/pdf/10.1175/2009BAMS2629.1). As an example, and although RCMs are usually evaluated in baseline simulations driven by atmospheric reanalyses, are we sure that they are never tuned to compensate for their driving GCM biases when they are used for regional projections? Is it a legitimate or a bad practice and are there clear recommendations about this in CORDEX? [Hervé Douville, France]	Accepted: However, this sub-section has been removed
104579	31	32	31	32	Text would be supplemented before 'Recent accumulation ...': 'Precipitation is not directly assimilated in the current atmospheric reanalyses and it's an ideal variable to evaluate the model parameterizations. Zhou and Wang (2017) used a highly dense network of observations to reveal large errors in precipitation charateristics (including frequency, intensity and amount) in eight reanalyses at sub-daily to multidecadal timescales. In particular, substantial improvement in parameterization of convective precipitation will be needed (Zhou and Wang, 2017).' Reference: Zhou, C., and K. Wang, 2017: Contrasting daytime and nighttime precipitation variability between observations and eight reanalysis products from 1979 to 2014 in China. J. Clim., 30, 6443-6464. [Chunlüe Zhou, United States of America]	Rejected: Zhou and Wang (2017) JC compared the high resolution in-situ observation data with global re-analysis data. It is well known that precipitation data of re-analysis date is strongly model dependent one and it wasn't used for the analysis. In the sub-sub-section, we refer only very high resolution observation data.
20213	31	42	3&	42	What is the SLH product? This acronym is found nowhere else in the SOD [philippe waldteufel, France]	Taken into account: SLH is "Spectral Latent Heating". However, this phrase has deleted.
22851	31	46	31	52	Chapter 8 also considered this. If anything your assessment is the better one in this regard. Coordination with chapter 8 on this issue would be highly advisable to assure consistency. [Peter Thorne, Ireland]	Not applicable. Section has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116953	31		31		Unfortunately there is no summary message on the observation site; this could be approached with the angle of "dimensions of feasibility" to use observations for regional climate information. [Valerie Masson-Delmotte, France]	Taken into account. Text has been revised.
110157	32	1	32	30	Section 10.2.3.2 is the first section of the Chapter to address bias adjustment, and the general move from the expression "bias correction" to "bias adjustment" is remarked. This is probably a good move, but probably not the most important move to do, if this IPCC AR6 is to be seen as one occasion to question some traditional expressions. Why sticking to expressions like "perfect prognosis", "perfect boundary conditions" and "perfect model" ? (throughout the chapter). This vocabulary is, at best, non-evocative, and at worst, misleading. For example, this leads to having, in this same section, on the one hand the adjective "perfect" appended to a practice that consists in using reanalysis as predictors, and on the other hand it is mentionned that reanalysis present discrepancies strong enough to question their use in statistical downscaling. [Patrick Grenier, Canada]	Noted. We follow existing terminology and don't see our role in pushing for new terminology. Bias adjustment is now widely used, but now alternatives for the “perfect” approaches exists. Of course we agree with the reviewer on the issues about this terminology, and highlight it as appropriate. Additionally, in particular in statistical downscaling the use of “perfect prognosis” is important, as it is well defined in contrast to other terms such as statistical downscaling or empirical statistical downscaling (which both may or may not include bias adjustment, depending on the community)
102507	32	3	32	3	Weather generators are in most cases (e.g. Fowler et al. 2007) classified as statistical downscaling though here they are their own type? Likewise, statistical downscaling based on a delta change approach, weather typing schemes and the socalled "analog" method is not mentioned here. Why is that? Lastly, bias adjustment is mentioned here (and below) as a downscaling method - thus in Table 10.1 it turns up too (alongside the delta change approach). Bias adjustment however in many cases being used complementary to different downscaling approaches ... [Philippe Tulkens, Belgium]	Noted. We follow the widely used classification first introduced by Rummukainen (1997) and then popularised by Maraun et al (2010) and the European VALUE initiative. The delta change approach is not a downscaling approach and only discussed in Section 10.3.3.1.4 for completeness. The analog method is a specific statistical downscaling method captured by the classification used here. Bias adjustment is explicitly *not* called a downscaling method here, see the explanation in the text.
1609	32	3	32	16	Again here you are complaining about the lack of available data. Also this reads like a review, and not an assessment. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text book like explanations are required as introduction, as this is the first time the IPCC handles these approaches in detail. The following text is an assessment of limitations for these approaches given observational issues. In response to the comment, we added a confidence statement.
1343	32	5	32	7	The main difference between downscaling and bias correction is that the former utilises the large-scales of the simulations that are larger than the models' minimum skillful scales, whereas bias correction does not involve inter-scale depencendies. There is also the model output statistics (MOS) that is a form of downscaling but where the model output are used as predictors to by-pass systematic errors. Also it should be noted that empirical-statistical downscaling is calibrated on observations so that biases also tend to be removed. [Rasmus Benestad, Norway]	Noted. This is not a section explaining the differences between model types, but the limitations caused by observational issues.
22853	32	9	32	10	Limited availability of sub-daily observations does not necessarily recognise the substantive efforts that have been undertaken to provide access to sub-daily data through e.g. HadISD and ongoing efforts to further improve that situation [Peter Thorne, Ireland]	Taken into account. The efforts to improve the situation are now mentioned in 10.2.2.3
15649	32	13	32	15	The ADAMONT method (Verfaillie et al., 2017 https://doi.org/10.5194/gmd-10-4257-2017) carries out statistical adjustment (season & weather regime-aware quantile mapping) of daily variables spanning not only temperature and precipitation but also wind speed, incoming radiation (shortwave and longwave) and relative humidity, and includes a time disaggregation step operating at the hourly time scale (not using a weather generator but also an analog method), used for several climate change impact studies in Europe (Verfaillie et al., 2018 https://doi.org/10.5194/tc-12-1249-2018, Spandre et al., 2019 https://doi.org/10.1038/s41598-019-44068-8 etc.). The method was applied using a national-scale reanalysis of meteorological variables at 1 hour resolution as input (SAFRAN). [Samuel Morin, France]	Accepted. The reference to ADAMONT paper has been added.
110647	32	14	32	16	Consider to cite the following work about statistical downscaling of non-standard variables and climate indices. It includes also a Table with an overview of existing literature on this at that time: Casanueva, A., Frías, M.D., Herrera, S. et al. Statistical downscaling of climate impact indices: testing the direct approach. Climatic Change 127, 547–560 (2014). https://doi.org/10.1007/s10584-014-1270-5 [Ana Casanueva, Spain]	Noted. But the focus of the suggested paper is on derived indices, not non-standard methods. It would therefore ring the wrong bell.
45545	32	18	32	22	Horton and Brönnimann (2018) made a comprehensive assessment of the uncertainty in downscaled precipitation due to the driving reanalysis of analogue downscaling methods. Horton, P., Brönnimann, S. (2019) Impact of global atmospheric reanalyses on statistical precipitation downscaling Climate Dynamics, 52, 5189–5211, https://doi.org/10.1007/s00382-018-4442-6 [Jean-Philippe Vidal, France]	Accepted. Reference has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45547	32	18	32	22	Merging downscaled reconstructions from global reanalyses and local surface observations through empirical methods (Bonnet et al., 2017) or formal data assimilation techniques (Devers et al., 2020) allow bulding reliable long-term high-resolution datasets. Bonnet, R., Boé, J., Dayon, G., Martin, E. (2017) Twentieth-century hydrometeorological reconstructions to study the multidecadal variations of the water cycle over France. Water Resources Research, 53, 8366-8382, https://doi.org/10.1002/2017WR020596 Devers, A., Vidal, J.-P., Lauvernet, C., Graff, B., Vannier, O. (2020) A framework for high-resolution meteorological surface reanalysis through offline data assimilation in an ensemble of downscaled reconstructions. Quarterly Journal of the Royal Meteorological Society, 146, 153-173, https://doi.org/10.1002/qj.3663 [Jean-Philippe Vidal, France]	Noted. The Devers reference has been added to the outlook of this section.
79647	32	20	32	22	Relevant reference to be cited in this sentence: https://journals.ametsoc.org/doi/10.1175/JCLI-D-14-00331.1. In there, the authors demonstrate that the choice of reanalysis used for calibration of the statistical model can lead to important differences in the local projected climate change signal [Rodrigo Manzanas, Spain]	Accepted. The reference has been added.
22855	32	20	32	22	Given the age of these references there is a real potential hazard that they are pointing to issues in now deprecated reanalysis products and that this finding may no longer hold true in the newest generation of reanalysis products used extensively in earlier chapters. There is thus a risk of inadvertently undermining key aspects of the remainder of the report. Great care is required to ascertain whether this still holds for the current generation of products. If this cannot be ascertained then it needs to be said so. [Peter Thorne, Ireland]	Taken into account. More recent references corroborating these issues have been added.
1341	32	20	32	23	The sentence "... this data source may not be suitable for statistical downscaling" is misleading. While it may be true for some cases (especially in Africa), it is not generally so - in particular for other types of downscaling. The type of downscaling discussed here can be referred to 'downscaling of weather', which I'm not conviced is the most viable strategy. The other approach is 'downscaling of climate' where aggregated information (parameters of the pdfs) is the subject for downscaling (Benestad, 2016: DOI:10.1093/acrefore/9780190228620.013.27). The downscaling community is still dominated by the former type, but experience suggest that the use of seasonal aggregated statistics from reanalysis very closely reproduce temperature and precipitation statistics from the observations (e.g. Benestad et al, 2016; DOI: 10.1088/1748-9326/11/5/054017). It would not be representative to make a general statement about empirical-statistical downscaling (ESD) based on a couple of studies since there are so many ways to implement ESD, as are there many opinions about how to do it in the best way. [Rasmus Benestad, Norway]	Taken into account. Inhomogeneities of reanalyses are also valid at seasonal time scales. Nevertheless, the corresponding section has been worked over, supported by further evidence, and rephrased more precisely.
110161	32	26			Not clear what a "nominal" resolution is. Maybe a definition of the term would be helpful. [Patrick Grenier, Canada]	Noted. The terms are introduced in the section on gridding in 10.2.2
21159	32	30	32	30	The last sentece in the paragraph is redundant to me. I am not sure why it is there and exactly what it says [Faranak Tootoonchi, Sweden]	Accepted. Sentence has been removed.
125649	32	33	32	33	[ACCESSIBILITY] This section is titled assimilation of data, including paleoclimate, but it only talks about paleoclimate data. This title is inaccurate. Also, other types of assimilation should be discussed here. What are the high-level messages? Is it that data assimilation of paleoclimate data has enabled significant advances in understanding of past regional climate change and that the approach needs to be continued or expanded? Why should anyone care? How does this section (and all of the sections of the chapter) get the reader to appreciate the (as yet ill-defined) regional climate messages? [Trigg Talley, United States of America]	Accepted – Text has been revised. Following the suggestion, the title has been changed to 'Paleoclimate data assimilation '. Due to space limitations, we cannot go in all the details but the contribution of paleoclimate data assimilation is highlighted in the second paragraph of the section.
20217	32	33	33	1	This reader makes, believe it or not, efforts not to appear offensive. Still, once in a while his duty is to highlight how the text appears to him big in terms of the number of lines and poor in terms of original content. This is the case here [philippe waldteufel, France]	Rejected. No clear suggestion for modification
85033	32	33	33	25	No comments [Katrine Husum, Norway]	Noted.
22857	32	33			This seems to be only about paleo reanalysis. So the section is at best misnamed or incomplete. How does this interface with the similar substantive assessment in chapter 1? [Peter Thorne, Ireland]	Accepted – Text has been revised. Following the suggestion, the title has been changed to 'Paleoclimate data assimilation '. For the link with Chapter 1, Chapter 1 provides a general description of reanalyses, in particular of paleo-reanalyses. The focus is different here, following the theme of the chapter, and includes results of reanalyses as well as from experiments with data assimilation that are not discussed in Chapter 1.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
91029	32	35	32	42	Such a "reanalysis" project, combining high-resilution dynamic climate modelling, vegetation models and various types of paleo proxy data has also been undertaken in southern Africa. Three relevant publications from this programme are: Engelbrecht F.A., Marean C.W., Cowling R., Engelbrecht C., Nkoana R., O'Neal D., Fisher E., Shook E., Franklin J., Neumann F.H., Scott L., Thatcher M., McGregor J.L., Van der Merwe J., Dedekind Z. and Difford M. (2019). Downscaling Last Glacial Maximum climate over southern Africa. Quaternary Science Reviews 226 105879 https://doi.org/10.1016/j.quascirev.2019.105879. Marean C.W., Anderson R.J., Bar-Matthews M., Braun K., Cawthra H.C., Cowling R.M., Engelbrecht F.A, Esler K.J., Fisher E., Franklin J., Hill K., Janssen M., Potts A.J. Zahn R. (2015). A New Research Strategy for Integrating Studies of Paleoclimate, Paleoenvironment, and Paleoanthropology. Evolutionary Anthropology 24 62-72. Cowling R.M., Potts A.J., Franklin J., Midgley G.F., Engelbrecht F. and Marean C.W. (2020). Describing a drowned Pleistocene ecosystem: Last Glacial Maximum vegetation reconstruction of the Palaeo-Agulhas Plain. Quaternary Science Reviews 235 [105866]. https://doi.org/10.1016/j.quascirev.2019.105866 [Francois Engelbrecht, South Africa]	Rejected. It is impossible to cite all the papers related to the subject here and we have selected some fitting with the goal of this section using data assimilation.
91031	32	52	33	1	Another paper the author's may want to consider referring to in this context, is that of Engelbrecht et al. (2019), which describes dynamic downscalings of Last Glacial Maximum climate over southern Africa. Of interest is the temperature and rainfall response within the presence of the equatorward displaced Southern Hemisphere westerlies: Engelbrecht F.A., Marean C.W., Cowling R., Engelbrecht C., Nkoana R., O'Neal D., Fisher E., Shook E., Franklin J., Neumann F.H., Scott L., Thatcher M., McGregor J.L., Van der Merwe J., Dedekind Z. and Difford M. (2019). Downscaling Last Glacial Maximum climate over southern Africa. Quaternary Science Reviews 226 105879 https://doi.org/10.1016/j.quascirev.2019.105879. [Francois Engelbrecht, South Africa]	Rejected. The topic of the paper is not fitting with this section on data assimilation.
80327	32	54	32	54	CH2 uses Medieval Warm Period (MWP) instead of Medieval Climate Anomaly (MCA) to refer to this warm period [Paola Arias, Colombia]	Accepted. Text has been revised. To avoid misunderstanding, the wording 'Medieval Climate Anomaly' has been removed.
10703	32	54			The "generally warm Medieval Climate Anomaly" does not appear to be as warm as the preceding centuries, (e.g., upto 950AD in Chapter 2's Fig 2.11; Neukom et al., "No evidence for globally coherent warm and cold periods over the preindustrial Common Era", Nature 2019). Perhaps it is finally time to retire the inaccurate term. At very least reference studies that are recent and cover the last 2000 years of climate change. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – Text has been revised. To avoid misunderstanding, the wording 'Medieval Climate Anomaly' has been removed.
20655	33	4	33	4	This title of 10.2.4 is misleading, inasmuch as while the subsection has to do with rescuing past observational data (which of course is an issue that deserves full attention), there is nothing specific to regional scales, as mentioned in the text itself on line 10 [philippe waldteufel, France]	Accepted. Text has been modified and more information has been added at regional scale.
125651	33	4	33	4	[ACCESSIBILITY] This section does not provide an assessment or outlook for improving obsevational data for regional climates. All that is covered is data rescue and citizen science. Citizen science is indeed an uptapped resource that could wield an enormous amount of power -- not just for data rescue, but for using cell phone and other tools to make observations. The GLOBE program and GLOBE observed app does this with Clouds, Mosquitos, and other data. Undiscussed is the future data needed for regional climate applications. Are the right observations being taken? What needs to be better understood? Are the right satellite instruments planned to support the development of regional climate messages? These questions are not cover and need to be. [Trigg Talley, United States of America]	Taken into account. Text has been revised, however, due to space limit the report can not go deep into the requested details.
59419	33	4	33	24	In the outlook, or earlier on, it could be of relevance to include a progressive paragraph on citizen science, crowd sourcing etc. as a potential way forward to fill some data gaps and to be used for specific regional studies. Of course, also highlighting the limitations to overcome in the future. A few references could be (among others): Overeem et al. 2013: Crowdsourcing urban air temperatures from smartphone battery temperatures: https://doi.org/10.1002/grl.50786 de Vos et al. 2019: Quality Control for Crowdsourced Personal Weather Stations to Enable Operational Rainfall Monitoring: https://doi.org/10.1029/2019GL083731 Uijlenhoet et al. 2018: Opportunistic remote sensing of rainfall using microwave links from cellular communication networks: https://doi.org/10.1002/wat2.1289 Langendijk et al. 2019: Three Ways Forward to Improve Regional Information for Extreme Events: https://doi.org/10.3389/fenvs.2019.00006 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text has been revised and reference has been added in the appropriate subsection

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112631	33	4	33	24	I don't know if this would fit here, but another example of a reconstruction is Humphrey et al, 2017. They reconstructed the GRACE satellite dataset to provide a longer timeseries that can be used for model evaluation of terrestrial water storage variability. (Humphrey, V., L. Gudmundsson, and S. I. Seneviratne (2017), A global reconstruction of climate-driven subdecadal water storage variability, Geophys. Res. Lett., 44, 2300–2309, doi:10.1002/ 2017GL072564.) [Marie-Estelle Demory, Switzerland]	Accepted. Reference has been added.
22859	33	4			Section is discussing data rescue and should be renamed as such. There is too much emphasis on the ACRE project at the expense of other projects and activities. For example the data rescue in the classroom activity by Ryan et al is not covered (https://journals.ametsoc.org/doi/10.1175/BAMS-D-17-0147.1). There is insufficient discussion of how much data exists - e.g. Bronimann et al. , 2019 (https://journals.ametsoc.org/doi/10.1175/BAMS-D-19-0040.1). More fundamentally, however, there was a section that did almost the exact same in chapter 1. There is no value in overt repetition. The section should start from chapter 1 discussion and then add any regional detail missing in that. There is no need to repeat an assessment already performed in chapter 1. [Peter Thorne, Ireland]	Accepted. Text has been revised and repetition with Chp1 & 2 has been removed. A reference has been added now to chap 1 and 2.
45543	33	6	33	17	Global extended reanalyses may further be downscaled to quantify the past variability of climate at the regional scale (Caillouet et al., 2016, Caillouet et al., 2019). Surface observations from data rescue efforst (Jourdain et al., 2015) may then be assimilated into these downscaled projections to derive long-term high-resolution gridded surface reanalysis (Devers et al., 2020). Caillouet, L., Vidal, J.-P., Sauquet, E., Graff, B. (2016) Probabilistic precipitation and temperature downscaling of the Twentieth Century Reanalysis over France. Climate of the Past, 12, 635-662, https://doi.org/10.5194/cp-12-635-2016 Caillouet, L., Vidal, J.-P., Sauquet, E., Graff, B., Soubeyroux, J.-M. (2019) SCOPE Climate: a 142-year daily high-resolution ensemble meteorological reconstruction dataset over France. Earth System Science Data, 2019, 11, 241-260, https://doi.org/10.5194/essd-11-241-2019 Devers, A., Vidal, J.-P., Lauvernet, C., Graff, B., Vannier, O. (2020) A framework for high-resolution meteorological surface reanalysis through offline data assimilation in an ensemble of downscaled reconstructions. Quarterly Journal of the Royal Meteorological Society, 2020, 146, 153-173, https://doi.org/10.1002/qj.3663 [Jean-Philippe Vidal, France]	Accepted. Reference has been added.
1611	33	6	33	24	Shouldn't this be in Ch 2. There is new paper for the smaller second paragraph. Brönnimann, S., Allan, R., Ashcroft, L., Baer, S., Barriendos, M., Brázdil, R., Brugnara, Y., Brunet, M., Brunetti, M., Chimani, B., Cornes, R., Dominguez-Castro, F., Filipiak, J., Founda, D., Garcia Herrara, R., Gergis, J., Grab, S., Hannak, L., Huhtamaa, H., Jacobsen, K.S., Jones, P.D., Jourdain, S., Kiss, A., Lin, K.E., Lorrey, A., Lundstad, W., Luterbacher, J., Maueishagen, F., Maugeri, M., Moberg, A., Neukom, R., Nicholson, S., Noone, S., Nordli, Ø., Ólafsdóttir, Pearce, P.R., Pfister, L., Pribyl, K., Przybylak, R., Pudmenzky, C., Rasol, D., Reichenbach, D., Řezníčková, L., Rodrigo, F.S., Rohr, C., Skrynyk, O., Slonosky, V., Thorne, P., Valente, M.A., Vaquero, J.M., Westcott, N.E., Williamson, F., Wyszyński, P., 2019: Unlocking pre-1850 instrumental meteorological records: A global inventory. Bulletin of the American Meteorological Society, 100, ES389-ES413, https://doi.org/10.1175/BAMS-D-19-0040.1. This has come from a C3S project. There is much more to refer to in other outputs of this Data Rescue Project. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text has been revised.
82689	33	8	33	8	Better described here as "weather data rescue" or just "data rescue". [Blair Trewin, Australia]	Not applicable. Text has been removed.
82691	33	12	33	12	There is a new version of 20CR (Slivinski et al 2019, citation in Chapter 1). In general Chapter 1 (p66-67) has more up to date information on data rescue which could be drawn on here. [Blair Trewin, Australia]	Taken into account. Text has been revised.
59259	33	27	42	18	Sections 10.3.1 and 10.3.2 are clearly set up to provide background information for the results following in sections 10.3.3 and 10.3.4. Though the previous sections are meant to provide definitions and explanations of models and model experiments, it is written in a complex way. It is written at too high a level to be useful. Readers who are already familiar with the concepts will understand it easily, but those who are not familiar I'm afraid will not gain much insight. The text provides numerous examples of the application of some of these modeling techniques, but I think the examples could be fleshed out a bit more. Again, this circles back to who is the intended audience. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The intended audience is very broad, from climate modellers to users including impact modellers as well as decision makers. The introductory sections have been written such that their general message is accessible for a broad audience, details for scientifically-trained audiences such as impact modellers.
40861	33	38	33	38	Suggest to change into "Model types" to be consistent with 10.2.1 (Observation types). [TSU WGI, France]	Taken into account. Has been adjusted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
20657	33	41	33	41	The reference to figure 10.4 seems irrelevant [philippe waldteufel, France]	Rejected. If the reviewer refers to the reference - here is the place for it. If the reviewer refers to the figure itself: it provides an overview of different model types which may not all been known to (but still relevant for) non-technical users
106581	33	50	33	52	The green and grey lines are very hard to distinguisj (even when printed on very white paper!), please change at least one. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The figure has been adjusted.
35189	33	50			Fig 10.4 : I think that indicating « Regional Climate Models » in the yellow box is relevant as this term is well known in the community. Also Regional Climate Models / Dynamical downscaling should be indicated graphically as GCM or statistical approaches that is to say in a smaller box with dark background color in order to get the same visibility as the 2 other families [SAMUEL SOMOT, France]	Noted. The term "RCMs" is used, but the type of the box needs to be different, as the standard RCMs provide input for the CPS ones.
35191	33	50			Fig 10.4 : I think that graphically the figure is not a success. I'm not sure people will re-use it a lot as it is. If you can give it to a graphic designer to re-draw it, it may help [SAMUEL SOMOT, France]	Noted. We have worked on the figure.
35193	33	50			Fig 10.4 : I would use the figure to add the acronyms used in the text such as GCM, RCM, … [SAMUEL SOMOT, France]	Accepted. The figure has been adjusted.
35195	33	50			Fig 10.4 : not sure that separating standard resoluton and high-resolution is relevant for GCMs as there is no « standard resolution » in CMIP and because there is no conceptual difference between standard and high … you may prefer to indicate the variable-resolution GCM sub-family instead. [SAMUEL SOMOT, France]	Noted. The purpose of the figure is to highlight the various existing approaches that are relevant but may not been known to users. High resolution GCMs are, as CPS RCMs not well known in particular by non-technical users and therefore deserve being highlighted.
35203	33	50			Fig 10.4 : in the grey box, I would add : « over a target period and a target zone » (this target zone is the globe for GCM but not for the other methods) [SAMUEL SOMOT, France]	Rejected. This is too detailed for this sketch.
79451	34	1	34	1	I ilke very much the idea and content of Table 10.1 and I think it might be VERY useful for the reader. While it is clear after reading the caption, I would make explicit that the cells contain "assumptions" by adding the word "assumption" as part of the title of each column (e.g., "Present climate assumptions"). A key aspect that is missing right now from the table is some kind of judgement about how good/bad these assumptions are or how often these assumptions might fail. For example, GCM assumptions are basically the same for large and regional scales but we know that th assumptions will fail much more often at regional compared to global scales (for example over mountainous regions). Maybe colors in the cell could be used to characterise the "quality" of each assumption? I understand this might be hard to implement. Maybe the calibrated language could be use to charatecerise the level of confidence we have in each assumption... Happy to have a chat if I might help with anything here. I would say that the RCM's assumption about large scales is that the driving model (GCM because reanalyses are not conisdered here) simulates well the large scale. But it seems odd to have the same assumption as the GCM for the large-scales. "Scale" could be "Spatial scale". And maybe another scale called local could be added so statistical downscaling can show its strength... GCMs could also be separated according to CGCMs, HRGCMs and VRGCMs. Please, if you find these ideas useful I would be happy to help their development. [Alejandro Di Luca, Australia]	Noted. We added "assumption" to each column. We have discussed the other suggestion of adding a qualifier for each box, also with the reviewer himself. Finally, we decided that such a qualifier would either be too simplistic or require added many additional elements to the table. The reason is that , depending on the region, variable and user context, a certain assumption may be more or less fulfilled by a certain model type. E.g. GCM performance over complex terrain will be much worse than over a wide flat region.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1347	34	1	34	15	There is an additional approach to ESD than 'Perfect Prog' which part of the downscaling community does not appreciate, involving common EOFs (see post on RealClimate: "Why not use a clever mathematical trick?"). It is a hybrid between "Perfect Prog" and "MOS" where anomalies from reanalysis and GCM simulations on the same grid are combined in time. Then an EOF analysis is carried out for the combined data set, and the part of the PCs describing the reanalysis is used to calibrate the downscaling models and the other part representing the GCM used to make projection. Hence, the downscaling model requires both reanalyses and GCM results in order to be calibrated. The advantage of this approach is that it provides a direct comparison between reanalysis and GCM results and that the PCs for a given EOF represent the exact same spatial covariance structure in reanalysis and GCM. I have tried to convince some of my peers that the use of common EOFs in ESD represent a hybrid between Perfect Prog and MOS (which only uses GCM results as predictor during calibration), but without success. I have published scientific papers showing that the use of common EOFs is superior to the Perfect Prog approach and gives a better handle on uncertainties (usually ignored by peers in the ESD community or possibly not understood). There is nevertheless difference of opinions, and only one side of this debate is represented in this chapter. The minimum solution should be tho drop 'Perfect Prog' and make the desciption more general. [Rasmus Benestad, Norway]	Noted. The approach is not a hybrid between MOS and PP, but a clever PP approach. As the reviewer states, the joint reanalysis/GCM data are used to construct common EOFs, but the predictor PCs used for the calibration are limited to those belonging to the reanalysis, which is a classical Perfect Prog approach. Therefore all underlying assumptions are identical, even though the performance of this particular approach might in some cases be superior to other perfect prog approaches. Therefore, no new categories are required. Note also that both figure and table does not include approaches we do indeed categorise as hybrid (new subsection in 3.1.4) to avoid overloading it. Note also that a reference to the common EOF method has been added to Section 3.1.4 under perfect prog.
21161	34	6	34	15	This table is unclear to me. I do not understand why "Bias adjustment of dynamical model" is under model type. I also do not understand what do you mean by the column "scale". If on scale part there is regional reolution for GCMs, why later also there is RCM on the model types. I guess the whole table can be framed in a more clearer way. [Faranak Tootoonchi, Sweden]	Noted. The table caption and labels have been improved. Bias adjustment is using a statistical model, the term model in the table is not limited to dynamical models, but includes also statistical ones as in the corresponding Section 10.3.1. Both GCM and RCM simulate aspects of regional climate, so there has to be a box "regional" for both.
59201	34	6	35		Table 10.1 should be inserted in one page. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. This is done in the final draft
110565	34	8	35	1	Table 10.1 is refeenced before the concept of "driving model" is introduced - making reading this table a little unclear. [Rachel McCrary, United States of America]	Noted. The right place of the table is the introduction to Section 10.3.1, it therefore has to be prior to the Section on RCMs and statistical approaches. We anyway expect readers to read it in parallel to the main text, such that the problem indicated by the reviewer might be negligible. An errata has been submitted to deal with this comment
110567	34	8	35	1	While this is stated in the table text - you could add an additional row above Present and Future climate that says "Assumptions" [Rachel McCrary, United States of America]	Taken into account. Cell text has been adjusted.
27529	34	8	35	2	It would be very interesting to add to this Table 10.1 (or to create another table) a discussion about the veracity / limitations of the assumptions. We know that some of them are clearly hard to defend. I think that for many researchers in different domains this would be one of the most important point of this chapter [Eric Brun, France]	Noted. We have discussed the other suggestion of adding a qualifier for each box. Finally, we decided that such a qualifier would either be too simplistic or require added many additional elements to the table. The reason is that , depending on the region, variable and user context, a certain assumption may be more or less fulfilled by a certain model type. E.g. GCM performance over complex terrain will be much worse than over a wide flat region. Links to individual subsections will guide the reader directly to those places where further details on the model performance are given.
1345	34	11	34	13	It is misleading to higlight stationarity assumption and only mention statistical downscaling and bias correction. This applies even more to GCMs and RCMS, where parameterisation schemes (also known as 'physics') indeed is an upscaling of unresolved small-scale processes to a bulk effect on the atmosphere. While these are based on physical considerations, they involve calibration ("tuning") which are equally to subject to a stationarity assumption. However, in this case, errors and biases will feed back into the whole system of calculations and it's debatable whether this has a stronger effect on the final solution. [Rasmus Benestad, Norway]	Noted. The issue is highlighted under "parameterisations are valid in future climate". Note also that "stationarity assumption" is not highlighted in the table, it is just mentioned in the caption because readers may expect it for statistical methods.
54379	34	15	34	15	Regarding "future assumptions" column in a table - the sentence "Parameterisations work in different climate" is unclear to me. Does it mean that parameterisations work correctly for different types of climate (eg. either monsoon and polar)? Or it's not about types of climate? [Gabriel Stachura, Poland]	Noted. The text has been be adjusted. Here it is meant that the parameterisation works in different future climate.
22863	34	15	35	1	Other chapters have generally used ESM. Why is GCM used here? Table should provide links not just to within chapter but also to key chapters where these are assessed in further depth in the FGD. [Peter Thorne, Ireland]	Taken into account. Not all GCMs used in Chapter 10 are ESMs. This is clarified in the beginning of 10.3.1.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106583	34	15	35	1	Need to add in the table something about used of RCMs driven by reanalyses and that an RCM should reproduce the large-scale behaviour of its driving model. Also, in the perfect prog/regional/future climate cell why not use language similar to "works in different" climate as used in the corresponding R/GCM cells. Finally, please clarify what "minor" in this table? [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The table is limited to climate change simulations, therefore reanalyses-driven RCMs do not appear, but are assessed in section 10.3.1.2 because they are a key element for the use of RCM simulations. The wording in the perfect prog row has been adjusted, in spite of the issue raised by the reviewer being often ignored by the community. The term minor is used in the table meaning negligible, more details are provided in the corresponding subsections.
35197	34				Table 10.1 : I would add the mention of « up to the model effective resolution » in the box GCM-Regional / Present Climate and also in the box RCM-Regional / Present Climate [SAMUEL SOMOT, France]	Noted. This is too technical for the table and takes too much space.
35199	34				Table 10.1 : In the box GCM-Regional / Present-Climate and Future-Climate, I would add the hypothesis that missing small-scales does not degrade regional scale (or even large-scale) information. [SAMUEL SOMOT, France]	Taken into account. Has been added.
35201	34				Table 10.1 : Box RCM-Large / Present-Climate. I don't think that we can indicate « As with GCM » because typically RCM do not include « all relevant large-scale forcings ». They need to import those info [SAMUEL SOMOT, France]	Noted. The "As with GCM" is in not in the mentioned box, but in "RCM-Regional"
13587	35	2	35	2	Include in the Tbale 10.1 the meaning of NA [Maria Amparo Martinez Arroyo, Mexico]	Taken into account. Not applicable has been spelled out.
79453	35	5	35	5	As I mentioned above I think that Table 10.1 is key for this chapter and in my opinion also the core of the chapter as it provides the means to interpret how regional-scale information is generated and what are the strengths/weakness of each technique. So I was expecting that subsections 10.3.1.i will develop further what was briefly presented in the table including more discussion about the assumptions of each technique and the possible consequences of these assumptions. However, this is not discussed explicitly. Instead, there is the discussion about the performance and the added value in section 10.3.3 but as a reader I think it appears a little disconnected and it would be better to integrate it in this section with each specific technique... Again, happy to discuss if anything is unclear. [Alejandro Di Luca, Australia]	Taken into account. In Table 10.1 the relevant sections have been included, which ensures the link between the table and the sections where the model types and experiments are discussed.
22865	35	5			These are generally referred to as ESMs in all earlier chapters. Change for consistency throughout this section and the chapter? [Peter Thorne, Ireland]	Taken into account. A clear consistent distinction has been made. Earth System Models (ESMs) are now used as a general concept. Models that do not have a carbon cycle are now denoted as General Circulation Models (GCMs).
79457	35	8	35	9	The concept of "effective resolution" is not used in the chapter at all. It sounds pretty unfair to use it here when refering to GCMs and not use it when talking about any other model so I suggest removing it. [Alejandro Di Luca, Australia]	Taken into account. Text has been revised. Effective resolution is also discussed for LAM's in other studies such as Skamarock (2004), which has been added as a reference.
96091	35	8	35	11	Since Klaver et al. is not yet submitted, please add an additional reference if possible. [Nicole Wilke, Germany]	Accepted. Skamarock 2014 is added as additional reference. Klaver et al. is now being accepted and published.
91033	35	9	35	9	It is a highly dubious statement to make, that a 100 km resolution global model has an effective resolution of 600-1000 km, based on a paper that has not been accepted. Models at this (100 km) resolution easily simulates, for example, tropical-cyclone like vortices, which clearly demonstrates a much finer effective resolution (~ 200 km). In fact, it will be impossible for a model with effective resolution of 600-1000 km to simulate tropical-cyclone like vortices, so this statement around effective resolution is obviously wrong. Moreover, classical theory (see Messinger and Arakawa, 1961) indicates that at a resolution of x km wavelengths of 2x km and longer are resolved, and this guideline has been accepted for 60 years. This statement should thus be toned down, or removed. [Francois Engelbrecht, South Africa]	Taken into account. Text has been revised. Paper has now been accepted. It is now clearly stated that the effective resolution applies to a reliable simulation of the kinetic energy spectrum (k to power -3). Indeed wavelengths of 2 x km are partially resolved, but not correctly. As a consequence, for instance, tropical cyclones in GCM are simulated weaker and larger than observed.
125653	35	9	35	9	Klaver et al. is now published. [Trigg Talley, United States of America]	Taken into account. The reference of Klaver has been updated.
35205	35	9			Klaver et al. is published [SAMUEL SOMOT, France]	Taken into account. The reference of Klaver has been updated.
91035	35	11	35	11	Replace "tension" with "scientific debate". [Francois Engelbrecht, South Africa]	Not applicable. Text has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35207	35	14			I think that the word « processes » is missused here. Personnaly I make a difference between « Improving parameterizations » that can be considered as adding new processes and « Complexifying models » such as adding the carbon cycle. So I would separate adding new processes by improving parameterizations and adding new eartch system components such as the carbon cycle [SAMUEL SOMOT, France]	Taken into account. Text has been revised.
22867	35	15	35	16	Chapter 3 was devoted to this. No cross-reference given and the text here is not consistent with what they assessed. [Peter Thorne, Ireland]	Taken into account. A reference to chapter 3 has been included.
55157	35	15	35	16	Needs references to support the conclusion that "Despite these efforts, since AR5 the progress in reducing biases and providing more credible regional projections by GCMs and ESMs has been moderate" -- Gusain, A., Ghosh, S., & Karmakar, S. (2020). Added value of CMIP6 over CMIP5 models in simulating Indian summer monsoon rainfall. Atmospheric Research, 232, 104680. -- Catalano, A. J., Loikith, P., & Neelin, J. D. (2020). Evaluating CMIP6 model fidelity at simulating non-Gaussian temperature distribution tails. Environmental Research Letters. -- FU, Y., LIN, Z., & GUO, D. (2020). Improvement of the simulation of the summer East Asian westerly jet from CMIP5 to CMIP6. Atmospheric and Oceanic Science Letters, 1-9. -- Priestley, M. D., Ackerley, D., Catto, J. L., Hodges, K. I., McDonald, R. E., & Lee, R. W. (2020). An overview of the extratropical storm tracks in CMIP6 historical simulations. Journal of Climate. -- Xin, X., Wu, T., Zhang, J., Yao, J., & Fang, Y. Comparison of CMIP6 and CMIP5 simulations of precipitation in China and the East Asian summer monsoon. International Journal of Climatology. [Nancy Hamzawi, Canada]	Taken into account. Text has been revised and reference to Chapter 3 has been added, where this is discussed.
35209	35	15		16	This statement is strong and needs to be developed and based on references. Perhaps it is too early in this chapter to write it. [SAMUEL SOMOT, France]	Taken into account. Text has been revised and reference to Chapter 3 has been added, where this is discussed.
73823	35	17	35	18	The list of the MIPs is not in Chapter 3, it's in Chapter 1 (Table 1.4) [Rondrotiana Barimalala, South Africa]	Accepted. Reference to table has been changed to Chapter 1 Table1.3
59241	35	19	35	19	Should be Zhou et al., 2016b [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference has been corrected. It refers now to Zhou, T et al. 2016
91027	35	23	35	23	Replace "locally" with "regionally". [Francois Engelbrecht, South Africa]	Accepted. Text has been revised.
91037	35	23	35	27	I would like to suggest that the discussion of variable resolution GCMs is also extended to include (spectral) nudging techniques (as opposed to the nesting of RCMs). Two of the pioneering papers describing these aspects are: ThatcherMand McGregor J L 2009 Using a scale-selective filter for dynamical downscaling with the conformal cubic atmospheric model Mon. Weather Rev. 137 1742–52 ThatcherMand McGregor J L 2010 A technique for dynamically downscaling daily-averaged GCMdatasets over Australia using the conformal Cubic atmospheric model Mon. Weather Rev. 139 79–95 [Francois Engelbrecht, South Africa]	Rejected. We refer to McGregor 2015 for an overview and mention other references that appeared recently.
20659	35	23	35	27	Of particular interest for regional scale would indeed seem the grids with variable resolution. They offer an elegant way to solve the problems encountered with nested grid configurations. It works, as demonstrated by the model ARPEGE of the French weather service, which is quoted by Fox-Rabinovitz et al. [philippe waldteufel, France]	Noted.
59243	35	24	35	25	SGMIP should be called out here [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text has been revised. SGMIP is now mentioned.
35211	35	24			If you want to use the pionneer references using variable-resolution GCMs in climate mode, please also use Déqué et al. 1998 or Gibelin and Déqué 2003. Déqué, M., Marquet, P., & Jones, R. G. (1998). Simulation of climate change over Europe using a global variable resolution general circulation model. Climate Dynamics, 14(3), 173-189. / Gibelin, A. L., & Déqué, M. (2003). Anthropogenic climate change over the Mediterranean region simulated by a global variable resolution model. Climate Dynamics, 20(4), 327-339. [SAMUEL SOMOT, France]	Rejected. The suggested references do not cover the assessment period of AR and key publications for variable resolution GMS including an overview paper are already included as references.
35213	35	26		27	Not sure this statement is valid. [SAMUEL SOMOT, France]	Taken into account. Likely has been changed in possible.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16089	35	27	35	27	Is the "likely" here meant in the assessment sense (in which case it should be in italics)? Otherwise you might want to use another word. [Gerhard Krinner, France]	Accepted. Likely has been changed into possible
112045	35	30	35	30	In the previous section it is mentioned that GCMs are generally used to derive climate information at continental to global scales … (Chapters 3 and 4). It would be good to include a similar sentence for RCMs and include Chapters 12, Atlas, where RCMs are used in addtion to GCMs for the purpose above mentioned. Moreover, we have information on CORDEX data in Atlas.3.4 and BOX ATLAS.1. Would be good to cross-reference. [jose manuel gutierrez, Spain]	Accepted. Wording has been added to recognize that one application of RCMs is to produce sub-continental climate information, with cross-references to later chapters added.
35215	35	34			domain boundaries : « domain boundaries ». I would be glad if you can mention that RCMs are driven not only are their LBC but also at their Surface Boundary conditions by SST and Sea-ice cover and also inside the model domain for forcing such as GHG, aerosols and ozone that are often not represented explicitely by RCMs. In the current phrasing, we have the impression that only lateral boundaries do matter. [SAMUEL SOMOT, France]	Taken into account. Text has been modified to clarify that "boundary" includes surface-boundary, when applicable (i.e., not applicable for an atmosphere-ocean RCM) as well as lateral-boundary input. Interior input from a GCM or reanalysis is discussed later. GHGs, aerosols and ozone are not necessarily from a GCM or reanalysis but may be specified on the basis of observations or a scenario.
96093	35	35	35	37	Two-way nested simulations are mentioned but it is not explained what advantages and disadvantages they offer compared to one-way nested simulations. Please explain or provide a reference. [Nicole Wilke, Germany]	Taken into account. The text has been revised to state that two-way nesting has been used to examine regional influence on the large-scale climate. There is no clear advantage or disadvantage. Two-way nesting is rare, in any case.
85995	35	35	35	37	Is there any difference in the performance of a one-way nested and a two-way nested simulations? [Debra Roberts and the Durban WGII TSU, South Africa]	Taken into account. The text has been revised to state that two-way nesting has been used to examine regional influence on the large-scale climate. There is no clear advantage or disadvantage. Two-way nesting is rare, in any case.
35217	35	35			reanalysis are based on GCM. [SAMUEL SOMOT, France]	Rejected. The literature makes a clear distinction between reanalyses, which are constrained by observations and thus are intended to replicate day-to-day observed behaviour and global climate models, which are not intended to replicate the observed day-to-day weather. Reanalyses ingest observations and GCM generally do not.
35219	35	37			You may want to also assess Déqué, M. (2010). Regional climate simulation with a mosaic of RCMs. Meteorologische zeitschrift, 19(3), 259-266. [SAMUEL SOMOT, France]	Rejected. The suggested document discusses a type of stretched-grid global model, not a limited-area regional model.
114757	35	40	36	1	I dont think we need to refer to this in the text. [Jan Fuglestvedt, Norway]	Accepted. Wording changed to indicate the problem without the colloquial language.
22869	35	40	36	1	I don't think that use of 'garbage in, garbage out' is appropriate for an IPCC report. Just think of the possible selective quotation that could be made by vested interests. [Peter Thorne, Ireland]	Accepted. Wording changed to indicate the problem without the colloquial language.
11409	35	40	36	1	“garbage-in, garbage-out”. I don’t think this is the appropriate terminology in a text like this. Also, we shouldn’t be overly critical, and describe climate model output as garbage, since this whole chapter builds on the assumption that it is actually possible and meaningful to do climate model simulations [Strandberg Gustav, Sweden]	Accepted. Wording changed to indicate the problem without the colloquial language.
116979	35		36		While the term "garbage in, garbage out" is commonly used in the RCM community, it is quite colloquial and could be misunderstood in an IPCC context. [Valerie Masson-Delmotte, France]	Accepted. The term has been removed.
1349	36	1	36	38	There are some unresolved(?) questions regarding the physical consistency surrounding RCMs, which may use different representation of small-scale processes (parameterisation schemes) than the GCMs. Furthermore, some case studies seem to indicate that RCMs have different aggregated outgoing longwave radiation (OLR) from the top of the atmosphere than their driving GCMs. This is expected if the RCM produces a different rain climate and different cloud climate to the driving GCM, and may perhaps explain some of the biases in the simulated results(?). Some of these points seem to be implicit in the present and following sections (e.g. coupling with the surface, dust). [Rasmus Benestad, Norway]	Noted. In the revised subsection, the text has been written to recognize that differences may arise between the GCM's and RCM's regional climate, to list factors that can cause differences and to indicate how greater consistency can be increased. There is also implicit recognition when discussing convection-permitting models that differences in how convection, in particular, is represented can produced differences in simulated climate.
35221	36	2		6	On this large-scale consistency issue, you may want to assess also Sanchez-Gomez et al. (2009) doi:10.1007/s00382-008-0502-7 and Sanchez-Gomez and Somot (2018), doi: 10.1007/s00382-016-3394-y [SAMUEL SOMOT, France]	Taken into account. The issue is not simply whether or not the RCM's simulation is more or less consistent with the GCM's, because the RCM's internal variability not resolved by the GCM may improve the simulation, including characterization of uncertainty due to internal variability. The more recent paper has been added as a reference because it further indicates the greater richness of the understanding of a region's climate when the internal variability acknowledged.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102509	36	5	36	6	"including additional unforced, internal variability (Nikiema et al., 2017, and references therin". This sentence can't stand alone. The point of downscaling with a physical process model in many cases is exactly to introduce better a better process representation, which by definition would imply new features (including local internal variability) not captured by the driving GCM. The way it is mentioned here - together with the next sentences on spectral nudging - it seems as if it is a bad thing? The discussion of whether spectral nudging (or not) is a good thing has gone on for many years .... [Philippe Tulkens, Belgium]	Taken into account.. Later subsections discuss the ways that better and additional process simulation can occur in an RCM. The sentence has been revised to focus on the RCM's development of its own unforced, internal variability, which may differ from the driving GCM's in the region. Regarding spectral nudging, we are simply noting here that it is a method that has been used to increase consistency with the driving model.
35223	36	12			CORDEX also provide the so-called « evaluation runs » where RCMs are driven by ERA-Int. Those runs are worst to be mentioned usefull for model evaluation and past climate trend attribution [SAMUEL SOMOT, France]	Rejected. The historical runs would also include the reanalysis-driven runs. Model evaluation is discussed later.
59245	36	13	36	13	Giorgi and Gutowski Jr., 2015 - Jr. should be included [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The standard citation is Giorgi and Gutowski.
35225	36	15			The phrasing is strange as CPRCM coordinated initiative (CORDEX Flagship Pilot Study on Convection) is also part of CORDEX. Worse to be mentioned. Coppola et al. 2018 being the reference for this initiative [SAMUEL SOMOT, France]	Rejected. The sentence refers to the typical resolution of CORDEX simulation, in part to set the stage for mentioning the finer-resolution cases. The small subset that are contained in the FPS mentioned by the reviewer are a special, non-typical, minority set within CORDEX.
106585	36	19	36	19	The meaning of "approximately resolving deep convection" is opaque, please clarify. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Wording changed to "approximately simulating" to recognize that convection is simulated directly but not necessarily with great precision.
54381	36	26	36	29	But the reason why these process are neglected is that they are sub-grid? Or maybe it depends on RCM's application, computing resources? I think more the latter but it should be in the text [Gabriel Stachura, Poland]	Noted. There could be several reasons why they were not part of the model, such as an expectation that they were not as important as those included, or for the reasons given by the reviewer. Going into all these details is outside of the intent of this paragraph. To give an indication of historical context, the opening of the sentence has been modified slightly to now state, "RCMs have often consisted of ..."
72073	36	26	36	38	Here air-sea coupling and representation of upper ocean mixing (in terms of proper MLD representation in the ocean model) is also important for regional climate simulation, such as Indian summer monsoon simulation. A recent study showed the role of narrow coastal Bay of Bengal SST front and MLD dynamics for proper atmospheric convection and Indian summer monsoon simulation in climate models. The study is important in this context and should be mentioned here. Samanta, D., Hameed, S. N., Jin, D., Thilakan, V., Ganai, M., Rao, S. A., & Deshpande, M. (2018). Impact of a narrow coastal Bay of Bengal sea surface temperature front on an Indian summer monsoon simulation. Scientific reports, 8(1), 1-12. [Samanta Dhrubajyoti, Singapore]	Taken into account. This statement is more important where we assess simulation of land-ocean interactions (10.3.3.5) and the paper has been cited there.
42725	36	27			'in standard RCMs SSTs are prescribed from GCM simulations' – this is the case but is raises the issue of the potential benefits of ocean downscaling. This is discussed later to a limited extent but it could also be usefully be reviewed in this section. A few coupled regional models have been developed. Of course, it will be most important in regions where coastlines processes are most important for the local climate, the multiple islands of the Maritime Continent being an obvious example (e.g. see: https://link.springer.com/article/10.1007/s00382-018-4367-0.) [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The intent of the paragraph is to show extensions of RCMs beyond their original formulation, such as to include atmosphere-ocean interaction. An overarching goal of the chapter is to assess the methodologies that allow the development of regional information from the foundation of global simulation, with an eye toward information relevant to users of it, primarily on land. Ocean-land interactions studied with RCMs have been cited later in the paragraph.
66553	36	29	36	41	Here, also coupled RCMs-DVM (dynamic vegetation models) should be mentioned (lines 29-33 and an additional section in 10.3.1 added). These model components also has an impact on the climate as shown for instance by Smith, B., Samuelsson, P., Wramneby, A., & Rummukainen, M. (2011). A model of the coupled dynamics of climate, vegetation and terrestrial ecosystem biogeochemistry for regional applications. Tellus A: Dynamic Meteorology and Oceanography, 63(1), 87–106. [Kjellström Erik, Sweden]	Noted. The list of possible submodels is not intended to be exhaustive, as implied by the wording. Also, detailed discussion of submodels has been deleted in the final draft.
42981	36	31	36	31	Another important ref: Dorn et al. 2019: https://doi.org/10.3390/atmos10080431 [Bodo Ahrens, Germany]	Rejected. The intent here is to cite foundational papers, and this one appears to be an incremental advancement of previous work in the coupling of atmosphere-ocean-ice simulation in regional models.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8941	36	31	36	31	Regional ocean-atmosphere coupled model is also developed for the CORDEX East Asian domain (Zou and Zhou, 2016). Zou Liwei, and Tianjun Zhou, 2016: A regional ocean-atmosphere coupled model developed for CORDEX East Asia: Assessment of Asian summer monsoon simulation, Climate Dynamics, 47, 3627–3640, doi: 10.1007/s00382-016-3032-8 [Liwei Zou, China]	Accepted. Referenced work has been cited.
8943	36	32	36	33	In addition to these model components, the wave model has also been included in the regional climate models (Zou et al. 2017). Zou Liwei, Tianjun Zhou, F. Qiao and W. Zhao 2017: Development of a Regional Ocean-Atmosphere-Wave Coupled Model and its Preliminary Evaluation over the CORDEX East Asia Domain, Int. J. Climatol., 37: 4478-4485 [Liwei Zou, China]	Accepted. Referenced work has been cited.
106587	36	33	36	33	Please add "improved consistency with the driving GCM," after "allows for". [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. These additional components may produce behaviour that departs from the GCM's.
35227	36	33			Recent Drugé et al. 2019 or Nabat et al. 2020 may be even more relevant than Nabat et al. 2015 in this sentence. https://doi.org/10.5194/acp-19-3707-2019 / https://www.atmos-chem-phys-discuss.net/acp-2019-1183/ [SAMUEL SOMOT, France]	Rejected The intent is to cite foundational papers. The two suggested papers are incremental changes compared to the one already cited.
91039	36	41	36	41	The following discussion on including aerosols is not addressing one of the most imporant categories, namely biomassburning aerosols! See Horowitz et al. (2017) for a discussion of an RCM including both biomassburning, dust and salt aerosols: Horowitz H.M., Garland R.M., Thatcher M., Landman W.A., Dedekind Z., Van der Merwe J., Engelbrecht F.A. (2017). Evaluation of climate model aerosol variability over Africa using AERONET. Atmospheric Chemistry and Physics 17 13999-14023. DOI 10.5194/acp-17-13999-2017. [Francois Engelbrecht, South Africa]	Noted. This sub-section builds on the assessment performed in Chapter 6.
68187	36	41	36	45	what is the reasoning for the selection of subcomponent models? models of for example surface mass balance of ice sheets could be mentioned, it is often downscaling procedure (Noel et al: https://www.the-cryosphere.net/14/1425/2020/) and subgrid methods (e.g. Vizcaino et al https://journals.ametsoc.org/doi/pdf/10.1175/JCLI-D-12-00615.1) or sea ice models? [Guðfinna Aðalgeirsdóttir, Iceland]	Not applicable. Section has been removed.
1613	36	43	39	45	This reads like a 'How you should do good studies'. There is little assessment, just a review. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The section has been reorganized, rewritten and the level of assessment has been increased.
116955	36		36		There is duplication on dust with the start of the chapter. Having an overview of how dust is addressed in the whole report (quick keyword seach) could help ensure coherency with other chapters and avoid duplication. The reference to the Syrian drought should build on the SR15 assessment (maybe providing an update). By the way, is the box in SR15 on this event following best practice for developing regional climate messages ? This could be a good example [Valerie Masson-Delmotte, France]	Taken into account. This subsection has been deleted in producing the final draft.
35231	37	1		2	Note that dusts have been successfully introcuded in RCMs with very good model behaviour (e.g. Nabat et al. 2012, 2015, 2020 for example over the North-Africa / Mediterranean zone). 2012 : doi:10.1007/s00382-014-2205-6 / 2015 : already cited in the chapter / 2020 : https://www.atmos-chem-phys-discuss.net/acp-2019-1183/ [SAMUEL SOMOT, France]	Accepted. The references are included in the FGD version.
87389	37	4	37	4	It might be useful to add a citation to recent review on the topic after "climate variabilty modes" to substantiate this sentence: Swingedouw D., Mignot J., Ortega P., Khodri M., Menegoz M., Cassou C. and Hanquiez V. (2017) Impact of explosive volcanic eruptions on the main climate variability modes. Global and Planetary Changes 150, pp. 24-45. [Didier Swingedouw, France]	Accepted. The reference is included in the FGD version and a link to the modes of variability Annex been added.
59247	37	4	37	15	The paragraph discussing volcanic aerosols mostly includes references about how volcanoes may trigger ENSO events, yet ENSO is never discussed here by name. I think ENSO should be specified, rather than just volcanoes influence regional climate. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The role of ENSO to mediate the regional response is now mentioned in the paragraph, although the literature illustrates that ENSO is not the only mode suspected to affect regional responses to volcanic aerosols.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
95843	37	9	37	12	Such efforts are undertaken yet and should be used in the present assessment. E.g. Brühl et al. (2018), Atm. Chem. Phys., doi:10.5194/acp-18-1-2018, simulates the aerosol evolution (2002-2012) taking into account stratospheric and tropospheric aerosols, including volcanic, dust and organic compounds. [Christine Bingen, Belgium]	Noted. The sub-section focuses on the impact of the aerosols on climate and not on the ability to reproduce the aerosols, which is part of the assessment in Chapter 6.
79327	37	18	37	18	Please check links with Chapter 6 and Chapter 8. [Prodromos Zanis, Greece]	Accepted. Links to previous chapters have been introduced in the text.
35233	37	18		26	A reference to a RCM including anthropogenic aerosols would be welcome here. Drugé et al. 2019 (https://doi.org/10.5194/acp-19-3707-2019,) is dedicated to the adding of the nitrate/ammonium species in a RCM for example [SAMUEL SOMOT, France]	Accepted. The reference is included in the FGD version.
106589	37	21	37	21	Please add "including simple models of sulphate chemistry or" before "specifiying" [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence has been completed with the reviewer's suggestion.
27531	37	21	37	21	About ' [...] by specifying the optical properties from observations [...]': In present climate simulations only, not in future projections… [Eric Brun, France]	Taken into account. The specification is from observations but refers to the effect of the aerosols in the atmosphere, not to the loads themselves. Observed loads are estimated from the observations in the historical period, for projections they come from the scenarios considered.
5681	37	29	37	40	This paragraph needs to be thoroughly expanded. It is not clear why you reduce the impact of land management (what should be covered in land management models) to irrigation and tillage only. What about evaporation, transpiration, what about agricultural management, forest management, …? [Joachim Rock, Germany]	Not applicable. Text has been removed due to space limitations.
100483	37	31	37	38	More recently, efforts have been made to account for water availabilty limitations in irrigation schemes implemented within land surface models (e.g. Guimberteau et al., 2012 for ORCHIDEE in LMDZ4 and Lawrence et al. 2019 for CLM5 in CESM) and to account for various irrigation techniques (e.g. Devanand et al., 2019 for CLM4 in WRF). But such features are, to my knowledge, not activated in current CMIP6 and CORDEX simulations. REFS: Lawrence, D. M., Fisher, R. A., Koven, C. D., Oleson, K. W., Swenson, S. C., Bonan, G., ... & Kluzek, E. (2019). The Community Land Model version 5: Description of new features, benchmarking, and impact of forcing uncertainty. Journal of Advances in Modeling Earth Systems.; Devanand, A., Huang, M., Ashfaq, M., Barik, B., & Ghosh, S. (2019). Choice of irrigation water management practice affects indian summer monsoon rainfall and its extremes. Geophysical Research Letters, 46(15), 9126-9135.; Guimberteau, M., Laval, K., Perrier, A., & Polcher, J. (2012). Global effect of irrigation and its impact on the onset of the Indian summer monsoon. Climate Dynamics, 39(6), 1329-1348. [Wim Thiery, Belgium]	Not applicable. Section has been removed.
80329	37	32	37	32	It should be "to implement" instead of "to implementing" [Paola Arias, Colombia]	Editorial – copyedit to be completed prior to publication
44235	37	42	37	45	Changes in albedo and its trends are estimated globally at high spatial resolution, using satellite observations (Chrysoulakis, N., et al. 2019: Exploiting satellite observations for global surface albedo trends monitoring. Theoretical and Applied Climatology, 137, 1171–1179). [Nektarios Chrysoulakis, Greece]	Not applicable. Section has been removed.
125655	37	48	38	9	Take note of an international collaboration of lake modeling and experiment scientists to improve understanding of climate change impacts on global lake systems: the Inter-Sectoral Impact Model Intercomparison Project (ISIMIP) lake sector (https://www.isimip.org/about/). This project analyzes lake responses (focus on water temperature and ice phenology) to RCP climate scenarios involving five well-known lake models. For more detail, contact the sector coordinators (rmarce@icra.ca or don.pierson@ebc.uu.se or wim.thiery@vub.be). [Trigg Talley, United States of America]	Noted. However that chapter only deals with coupled simulations.
82693	37	49	37	55	Is there anything worthwhile here which can be said about ephemeral lakes or those of highly variable extent? [Blair Trewin, Australia]	Not applicable. Section has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
100481	38	2	38	9	Lake models are also incorporated in GCMs (e.g. Subin et al., 2012 for CESM, Rooney et al., JULES/UKESM and Le Moigne et al., 2016 for CNRM-CM5). Similar to the RCMs, the lake schemes incorporated into GCMs are often either Flake or from the Hostetler-model family. The skill of most available 1D lake models has been assessed across a range of climates and lake types within the Lake Model Intercomparison Project (LakeMIP; Stepanenko et al., 2010; 2013; 2014; Thiery et al., 2014; Guseva et al., 2020). REFS: Subin, Z. M., Riley, W. J., & Mironov, D. (2012). An improved lake model for climate simulations: Model structure, evaluation, and sensitivity analyses in CESM1. Journal of Advances in Modeling Earth Systems, 4(1). ; Rooney, G. G., & Bornemann, F. J. (2013). The performance of FLake in the Met Office Unified Model. Tellus A: Dynamic Meteorology and Oceanography, 65(1), 21363.; Stepanenko, V. M., Goyette, S., Martynov, A., Perroud, M., Fang, X., & Mironov, D. (2010). First steps of a lake model intercomparison Project: LakeMIP. Boreal environment research, 15, 191-202. ; Le Moigne, P., Colin, J., & Decharme, B. (2016). Impact of lake surface temperatures simulated by the FLake scheme in the CNRM-CM5 climate model. Tellus A: Dynamic Meteorology and Oceanography, 68(1), 31274.; Stepanenko, V. M., Martynov, A., Jöhnk, K. D., Subin, Z. M., Perroud, M., Fang, X., ... & Goyette, S. (2013). A one-dimensional model intercomparison study of thermal regime of a shallow, turbid midlatitude lake. Geoscientific Model Development, 6(4).; Stepanenko, V., Jöhnk, K. D., Machulskaya, E., Perroud, M., Subin, Z., Nordbo, A., ... & Mironov, D. (2014). Simulation of surface energy fluxes and stratification of a small boreal lake by a set of one-dimensional models. Tellus A: Dynamic Meteorology and Oceanography, 66(1), 21389.; Thiery, W. I. M., Stepanenko, V. M., Fang, X., Jöhnk, K. D., Li, Z., Martynov, A., ... & Van Lipzig, N. P. (2014). LakeMIP Kivu: evaluating the representation of a large, deep tropical lake by a set of one-dimensional lake models. Tellus A: Dynamic Meteorology and Oceanography, 66(1), 21390.; Guseva, S., Bleninger, T., Jöhnk, K., Polli, B. A., Tan, Z., Thiery, W., ... & Stepanenko, V. (2020). Multimodel simulation of vertical gas transfer in a temperate lake. Hydrology and Earth System Sciences, 24(2), 697-715. [Wim Thiery, Belgium]	Not applicable. Section has been removed.
35427	38	12		21	I consider the inclusion of statistical approaches to the generation of regional projections correct. A very current vision of the different methods is given, which will make it easier for researchers to use them. [Gladys Linares-Fleites, Mexico]	Noted. We appreciate this positive comment.
22871	38	15	38	16	This may have been true in WG1 of AR5 but I very much doubt holds for WG2 where such techniques were frequently employed. Hence sentence likely requires revision. [Peter Thorne, Ireland]	Noted. But even though statistical downscaling approaches are used in AR5 WG2, there is neither an assessment of these methods nor a systematic presentation. Text has been modified to make the point clearer.
42727	38	16			'… have received little attention' – not clear what this is saying. Does it mean little attention in AR5? Or does in mean in terms of application? [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text has been modified to clarify that we refer to performance assessment.
59249	38	19	38	19	StaRMIP and BADJAM also related to Euro-CORDEX and Med-CORDEX (StaRMIP). Should this be stated? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The difference is that both StaRMIP and BADJAM are making use of CORDEX models, but they are not formally a part.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1351	38	20	38	48	The section on statistical downscaling is incomplete and ignores other important (and superior) ways of statistically downscaling climatic varibles. There are different research groups who work on empirical-statistical downscaling (ESD) and have different expert opinions about the best approach. In addition to Perfect Prog, there is an approach based on common EOFs that is a hybrid between the Perfect Prog and model output statistics (MOS). There are also a number of scientific publications demonstrating its success and its superiority to the Perfect Prog approach, but these studies have been ignored by many fellow downscalers. The section also needs to state that the traditional analog model approach, by design, is unable to predict new record-breaking extremes. An iid-test (independent and identically distributed), based on the number of record-breaking events over time, can be used assess whether the upper tail of downscaled results is distorted. Also, there is a need to consider the question of how to compare the predictors from reanalyses used to calibrate the models with predictors simulated by the GCMs in the Perfect Prog approach (taken care of with common EOFs). Another aspect is the two downscaling strategies: 'downscaling weather' (day-by-day) and 'downscaling climate' (the parameters of a pdf describing e.g. the daily statistics). Hence, this section gives a very incomplete picture of the progress within ESD. [Rasmus Benestad, Norway]	Noted. Considering the definition of perfect prog and model output statistics, the common EOF approach is a pure perfect prognosis approach. A reference to the approach has been added. The statement about the analog method is already explicitly included. A reference to "downscaling climate" has been added. The other points are details beyond the scope of this assessment report.
112047	38	36	38	36	Baño et al. 2020 is an specific reference of the application of deep learning to statistical downscaling (with comparison with standard downscaling methods using the VALUE framework described in 10.3.1.4). Available at https://doi.org/10.5194/gmd-13-2109-2020 [jose manuel gutierrez, Spain]	Accepted. Reference has been added.
27533	38	38	38	38	Many much older references exist: MARTIN, E., TIMBAL, B., BRUN, E. 1997. Downscaling of general circulation model outputs: simulation of the snow climatology of the French Alps and sensitivity to climate change. Climate Dynamics, Vol. 13, 45 56. [Eric Brun, France]	Accepted. Reference has been added.
20661	38	38	38	44	Concerning analogs one should refer to a paper by E.N. Lorenz in 1969. He was not very optimistic [philippe waldteufel, France]	Rejected. This paper is not relevant, as Lorenz was discussing the case of weather forecasting (which is a prediction in time), whereas here the analogue method is used for predictions in space (from large to local). See Maraun & Widmann, 2018, for details.
96095	38	42	38	44	Do these new analogue methods for perfect prognosis show similar results as regional downscaling? Are the results different? Are there any model intercomparison studies done yet? Please provide this information. [Nicole Wilke, Germany]	Noted. The assessment of these methods is provided in Section 10.3.3.
64859	38	48	38	49	"Bias adjustment is a statistical post-processing technique used to pragmatically reduce the errors in climate model outputs". As a practicing climate sceintists, I will not say that bias correction reduces climate model errors. In fact bias correction, if not well applied, adds errors to climate model outputs. Something more appropriate "bias correction is used to post-process climate model outputs to fit the purpose of the project case". [ELVIS ZILEFAC ASONG, Canada]	Taken into account. The caveats of bias adjustment are discussed in depth in the Chapter. But we have slightly adjusted the text to accommodate for the point made by the reviewer.
1353	38	48	38	55	It is important to note that bias adjustment does not involve the dependency between large-scale information (not subject to the models' minimum skillfull scale) and the local variability. Also, the chapter should in general avoid using reference to one specific strategy for ESD such as 'Perfect Prog'. There is no need for that and it's simply demonstrating that the chapter is ignoring relevant work. The chapter should not promote one strategy when there are several and some contention within the community about which is superior. I do not think that this chapter gives an objective representation of the work and the knowledge on ESD, and hope it will become more complete in the final version. [Rasmus Benestad, Norway]	Noted. The reviewer is likely pointing to the (missing) temporal synchronicity between large- and local scales. This is implicitly mentioned in the statement about "The most important difference". Note that an IPCC report is not a text book explaining details of methods, but rather a concise assessment report. Perfect prog approach is a general approach that encompasses all non-bias correction approaches, it is not selective (see the response to a previous comment by this reviewer).
108101	38	53	38	53	I suggest to change “the simulation being corrected” to “the simulation being adjusted” to be consistent with the denotation of the modification of the simulated values. [Claas Teichmann, Germany]	Accepted. The text has been adjusted.
87391	38	53	38	55	This sentence is very difficult to read. What is a sea temperature that is much slower? Please clarify the meaning here. [Didier Swingedouw, France]	Noted. Chapter, page numbers and/or subsection number seem to be wrong. There is no statement about "slow sea temperatures".
21163	39	3	39	3	Maybe a reference is needed to ditinguish between perfect prognosis and bias correction: https://doi.org/10.1002/joc.5877 [Faranak Tootoonchi, Sweden]	Taken into account. Relevant references have been added. The suggested reference is not relevant here (response by the author of that reference).
21165	39	3	39	3	by bias correction, the bias would not necessarily "vanish". It tends to improve. [Faranak Tootoonchi, Sweden]	Not applicable. Text has been shortened.
35235	39	9		1	Research has also been performed on bias-adjustment methods depending on the weather regime. Verfaillie et al. 2017, GMD, https://doi.org/10.5194/gmd-10-4257-2017 inspired by Driouech et al. 2010 https://doi.org/10.1016/j.gloplacha.2010.03.004 [SAMUEL SOMOT, France]	Accepted. The reference to Verfaillie has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42729	39	9			Would it not be clearer to say ' … mapping approaches that adjust the full statistical distribution'? [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. "Full" would include also extremes, but not all approaches attempt to do so, and those which attempt to do often fail.
38541	39	11	39	11	Maybe mention also Vrac et al 2016 for CDFt method, which is a variant of QM: Vrac, M., Noël, T. and R. Vautard, 2016: Bias correction of precipitation through Singularity Stochastic Removal: Because occurrences matter, J. Geophys. Res., 121(10), 5237-5258. [robert vautard, France]	Rejected. The discussion is about trend preserving approaches. The proposed method doesn't fit here, but rather proposes other improvements which, however, go into too much technical detail for this overall assessment.
59251	39	12	39	12	I believe this should be Lange (2019c). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. One Lange reference has been removed, the one here is now Lange 2019.
21167	39	13	39	14	Multivariate bias adjustment is an extendtion to the univariate methods. Univariate methods adjust statics of variables seperately, while multivariate methods are applicable on multiple variables simultaneously. [Faranak Tootoonchi, Sweden]	Rejected. The point is not about the simultaneous application, but about the JOINT adjustment including dependencies.
112863	39	13	39	18	This generalization of quantile mapping approaches to the multivariate case seems missing: L. Dekens, S. Parey, M. Grandjacques, D. Dacunha-Castelle. Multivariate distribution correction of climate model outputs: a generalization of quantile mapping approaches Environmetrics, 28 (2017), 10.1002/env.2454 I think It might also be important to mention that the issue of multivariate bias adjustment is not relevant only in the context of compound events, but also for many downstream modellling applications such as for agriculture and hydrology. [Paula Gonzalez, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The paper has been added. But the text does not mention compound events at all, so no need to change.
15651	39	25	39	31	I find it interesting that the chapter includes a subsection on Delta change, because it is still used for some impact studies. I think it would be appropriate to better highlight the many deficiencies of this method, which does not make it possible to represent changes in variability, seasonality or extreme values. It could also be useful to better highlight under which circumstances the "delta change" approach can be useful, e.g. for "futurizing" past extreme events and provide realistic and consistent sets of atmospheric variables corresponding to such events under future climate conditions. I however note that such approaches are ralso addressed in "pseudo global warming" experiments section. Maybe this needs some further checking. [Samuel Morin, France]	Noted. It is not true that the delta approach cannot adjust seasonality or extremes, see the discussion in the report. But we have added a statement about the limitations in modifying the spatial temporal dependence structure.
78245	39	25	39	31	Delta change approach (10.3.1.4.3) is too easy, but still effective for the beginning of impact studies. The delta method may not be well discussed in the AR5, so I recommend you would like to explain in a little more detail, not only with special cases like Webber et al. (2018), but also with basic and well-quoted article such as Hempel et al (2013). [Motoki NISHIMORI, Japan]	Taken into account. The delta change approach is kept short deliberately but still included for the sake of completeness. The recent reference is kept as the main source of information but the one mentioned by the reviewer has also been assessed.
111573	39	25	39	31	Not so many references here in the assessment of this simple but effective method of bias-adjustment. Methodology to build ensemble of RCMs for mean air temperature based on delta-method and "blind forecast" from past to recent climate period showed very good results including very low spacial differences in my study for Ukraine: Krakovska S. V. Optimal ensemble of regional climate models for the ssessment of temperature regime change in Ukraine. Nature Management, 2018, no. 1, pp. 114–126 (in Russian) [Volodymyr Osadchy, Ukraine]	Noted. The focus here is on methodological developments in delta change approaches, not their application.
125657	39	34	39	45	Statistical-stochastic models, trained on historical data, are often used for regional tropical-cyclone risk analysis. Some of these models also ingest output from GCM outputs to project hurricane hazard change, a version for statistical downscaling. Examples: Emanuel, K. A., Downscaling CMIP5 climate models shows increased tropical cyclone activity over the 21st century, Proc. Natl. Acad. Sci., 110, 12219-12224, 2013; and Lin, N. et al., Hurricane Sandy's flood frequency increasing from year 1800 to 2100, Proc. Natl. Adac. Sci, 113, 12071-12075, 2016. [Trigg Talley, United States of America]	Noted. The proposed paper by Emanuel is quite critical of the proposed approach and uses a different, dynamical downscaling approach. Also we cannot capture region-specific model developments in this broad presentation of methods.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112849	39	35	39	45	I think it is worth mentioning the type of regional climate generator introduced in Greene et al. 2012,2015. The method combines GCM information, statistical downscaling and weather generators to generate near-term climate projections that can be used in downstream modelling. It differs from typical generators in two aspects: it allows for multi-variate approaches which are often relevant for applications (e.g, temperature and precipitation for agriculture), and that GCM information is utilized at the regional scale and the subregional variability is modeled based on the observations. References: Greene AM, Hellmuth M, Lumsden T. 2012. Stochastic decadal climate simulations for the Berg and Breede Water Management Areas, Western Cape province, South Africa. Water Resources Research 48: W06504, 13 pp., 2012 doi:10.1029/2011WR011152 Arthur M. Greene, Lisa Goddard, Paula L.M. Gonzalez, Amor V.M. Ines and James Chryssanthacopoulos, A climate generator for agricultural planning in southeastern South America, Agricultural and Forest Meteorology, 10.1016/j.agrformet.2015.01.008, 203, (217-228), (2015). [Paula Gonzalez, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The WG does not seem to do anything substantially different to other such models (modelling the dependence, e.g., between temperature and precipitation is standard), and furthermore our focus is not on predictions but rather projections.
112851	39	35	39	45	Another important reference about the limitations of weather generations regarding the represented spatial scale : Breinl, K., Di Baldassarre, G., Girons Lopez, M. et al. Can weather generation capture precipitation patterns across different climates, spatial scales and under data scarcity?. Sci Rep 7, 5449 (2017). https://doi.org/10.1038/s41598-017-05822-y [Paula Gonzalez, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The suggested model resamples observations and is thus not representative of typical multi-site weather generators as discussed in this section.
125659	39	48	39	48	The title of Section 10.3.2 should be " Types of modeling experiments" (to differentiate it from lab experiments). [Trigg Talley, United States of America]	Accepted. Title has been adjusted.
35241	39	48			I miss the Big-Brother/Little-Brother experiments in which the RCM is driven by himself or by a GCM with the same resolution, physics as the RCM [SAMUEL SOMOT, France]	Noted. The BBLB experiment has been discussed in Section 10.3.3.2. Given its limited practical relevance (it is conceptually and computationally very demanding), we refrain from giving it more prominence.
66309	40	8	40	19	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]	Taken into account. Pichelli has been added as a reference.
68947	40	11	40	11	Change "on the contrary" in "in contrast". [Seth McGinnis, United States of America]	Accepted. Text has been changed.
69927	40	11	40	20	other time slice database called d4PDF has another noteworthy characteristics; that is high-resolution and large-ensemble-member climate simulations suitable for detecting various climate signals and weather extremes responding to given SSTs. The d4PDF has firstly been described by Mizuta et al. (2017), and various climate and impact assessment studies using d4PDF are summarized by Ishii and Mori (2020, submitted). Mizuta, R, Murata, A, Ishii, M, Shiogama, H, Hibino, K, Mori, N, Arakawa, O, Imada, Y, Yoshida, K, Aoyagi, T, et al.(2017) Over 5,000 years of ensemble future climate simulations by 60-km global and 20-km regional atmospheric models. Bulletin of the American Meteorological Society 98(7), 1383–1398. doi:10.1175/BAMS-D-16-0099.1 Ishii, M. and Mori, N. (2020) d4PDF: large-ensemble and high-resolution climate simulations for global warming risk assessment. Progress in Earth and Planetary Science (submitted) [Masayoshi Ishii, Japan]	Accepted. d4PDF with suggested references has been included in the revised text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35237	40	19			It is commonly accepted that time-slice experiments is a bad practice in the ocean community or in the community using coupled models as ocean is a long-term memory component and nobody knows how to initialize well the ocean or a regional sea in 2070. So reporting this practice here is contrary to the common practice. Planton et al. 2012 gives a receipe to perform « projection simulations with coupled AORCM » (section 8.4.1) including the advise of using transient runs and Soto-Navarro et al. 2020 (https://doi.org/10.1007/s00382-019-05105-4) illustrate well the problems of performing time-slices for ocean projections / Planton S., Lionello P., Artale V., Aznar R., Carillo A., Colin J., Congedi L., Dubois C., Elizalde Arellano A., Gualdi S., Hertig E., Jordà Sanchez G., Li L., Jucundus J., Piani C., Ruti P., Sanchez-Gomez E., Sannino G., Sevault F., Somot S. (2012) The climate of the Mediterranean region in future climate projections (chapter 8) In: Mediterranean Climate Variability, Ed. Lionello, P, Elsevier, pp. 449-502 [SAMUEL SOMOT, France]	Taken into account. The text has been revised. It now stresses the importance of attaining a stable stationary state in the deep ocean to avoid wrong projections, including the suggested references. The new text provides an overview of the practices together with the critics.
35239	40	19			I would replace this sentence by : « It is not advisable to perform time-slice experiments when using coupled ocean atmosphere RCMs (Planton et al. 2012, Soto-Navarro et al. 2020) ». Or I would remove the sentence in order not to promote a bad practice in the report [SAMUEL SOMOT, France]	Taken into account. The text has been revised. It now stresses the importance of attaining a stable stationary state in the deep ocean to avoid wrong projections, including the suggested references.. The new text provides an overview of the practices together with the critics.
35243	40	23			Please also asses the use PGW for future projections of extreme events statistics in Lenderink et al. 2019, Lenderink, G., Belušić, D., Fowler, H. J., Kjellström, E., Lind, P., van Meijgaard, E., ... & de Vries, H. (2019). Systematic increases in the thermodynamic response of hourly precipitation extremes in an idealized warming experiment with a convection-permitting climate model. Environmental Research Letters, 14(7), 074012. [SAMUEL SOMOT, France]	Taken into account. However, we already have an important number of references, thus we don't refer the paper
7925	40	25	40	31	Not only a bias in GCM circulation may be a reason to apply PGWs, also scientific curiosity to isolate thermodynamic effects, or a stakeholder question on "how would this event look like in a warmer climate?" is a good reason to apply PGW (see line 46-47 on same page) [Bart van den Hurk, Netherlands]	Noted. We agree with the reviewer about the different reasons for using the PGW approach. As mentioned by the reviewer, his point is given a couple of lines below. To be concise, we therefore decided not to introduce any redundancy here.
12301	40	33	40	36	I think that for most PGW simulations the statement "The large-scale dynamical fields are unchanged" is not true. Most of the PGW-simulations cited in this subsection also include some change in the mean wind as far as I know, which is a direct consequence of the thermal wind balance when modifying the 3-d temperature field as stated here. In my opinion a more correct statement would be: "These changes are added to the reanalysis by modifying the 3-dimensional temperature and moisture fields according to GCM-simulated changes. The large-scale dynamical fields are only changed to the extent required to maintain the hydrostatic and thermal wind balances." Side note: It is possible to have almost no dynamic changes in a PGW simulation (e.g. Brogli et al. 2019) but then you can only use a 1-d or 2-d temperature change to modify the boundary conditions otherwise the dynamical balace of the model is violated (which means that one acctually really should not assume that dynamic changes are "not influenced by the imprinted thermodynamic changes"). [Roman Brogli, Switzerland]	Noted: The section cites some papers that do take into account the large scale circulation difference between present and future simulations.
15561	40	37	40	42	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]	Accepted: Reference Chen et al (2020) is a good example of PGW applied for the typhoons. We have referred to this paper in the second paragraph of the section 10.3.2.2.
110915	40	39	40	39	A good example here, because of its much larger scale (for the entire U.S.), would be Liu et al. 2017. https://doi-org.cuucar.idm.oclc.org/10.1007/s00382-016-3327-9 [Melissa Bukovsky, United States of America]	Taken into account. However, could not access to the reference Liu et al (2017)
59253	40	42	40	42	Brogli et al. 2019 should include "a", Brogli et al., 2019a [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: Citation has been changed
15653	40	44	40	53	Such experiments could also be referred to as "delta change" applied to individual events. There may be interest in clarifying the difference between pseudo global warming and delta change. [Samuel Morin, France]	Rejected This paragraph focus on a specific meso-scale phenomena and investigate the effect of climate change by adding delta-T etc. to the environment. Someone can call it "delta" method, but "delta" has a meaning of parameter study, and that has much wider meaning.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67023	40	46	40	46	change "storylines" to "physical climate storylines" to maintain common terminology and differentiate from scenario storylines [Liese Coulter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Change "storylines" to "physical storyline"
20663	40	47	40	50	It is a bit surprizing that the PGW technique may be used for simulating cyclones, inasmuch as a developing wind shear is a major cause for cyclone weakening. Please comment. [philippe waldteufel, France]	Noted: Here we cited many papers, and some of them count the three dimensional environment change, as vertical wind shear.
125661	40	48	40	48	Add reference to Lau et al (2016): Lau, W., et al., What would happen to Superstorm Sandy under the influence of a substantially warmer Atlantic? Geophys. Res. Lett., 48, 802-811., 2016. [Trigg Talley, United States of America]	Taken into account: Lau K.M. et al (2016) is a kind of parameter change experiment, and try to find the mechanism of Hurricane Sandy's development. We add the reference.
79461	40	65	40	65	I found this figure very hard to understand with way too many boxplots. I think it would be great to have a simplified version of it that gives a simpler message. Also, you might want to have a look at a paper where a snow-sensitive bias correction was applied to RCM simulations over complex terrain to assess present and future snowpack. The projections obtained from the raw and bias-corrected data are very different at individual grid points and I would argue that are more plausible using the bias corrected data (https://link.springer.com/article/10.1007/s00382-017-3946-9#Sec141). [Alejandro Di Luca, Australia]	Not applicable. Figure has been removed.
132371	41	1	41	32	It can also be mentioned here that some sensitivity experiments can focus on the role of dynamic vs thernodynamic processes by nudging atmospheric circulation patterns in climate model simulations to a given reference state (e.g. Wehrli et al. 2018, 2019, 2020). References: a) Wehrli, K., Guillod, B. P., Hauser, M., Leclair, M., & Seneviratne, S. I. (2018). Assessing the dynamic versus thermodynamic origin of climate model biases. Geophysical Research Letters, 45, 8471–8479. https://doi.org/10.1029/2018GL079220; b) Wehrli, K., Guillod, B. P., Hauser, M., Leclair, M., & Seneviratne, S. I. (2019). Identifying key driving processes of major recent heat waves. Journal of Geophysical Research: Atmospheres, 124. https://doi.org/10.1029/2019JD030635; c) Wehrli et al., in review in ESD (submitted before December 31, 2019): https://www.earth-syst-dynam-discuss.net/esd-2019-91/ [Sonia Seneviratne, Switzerland]	Taken into account. This type of sensitivity experiments has been introduced in section 10.3.3.3.1 for the FGD and the relevant reference has been added.
125663	41	1	41	32	[ACCESSIBILITY] Section 10.3.2.3 provides nothing unique to regional climate. Is it covered elsewhere? If so, recommend removing. [Trigg Talley, United States of America]	Taken into account. The text has been revised and shortened to mainly focus on regional aspects. These experiments are not described elsewhere in the FGD.
64861	41	4	41	4	"on a given climate change or phenomenon." what is "a given climate change"? [ELVIS ZILEFAC ASONG, Canada]	Taken into account. The text has been revised and now reads as: "on regional climate change."
59255	41	5	41	8	"...two different frameworks." You should name them, especially as the following lines refer to the former and latter. I'm assuming the former and latter refer to the references. Then I think the citations should follow the description of each framework. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The former and the latter have been changed to the first and the second. They do not relate specifically to the references and they do not have specific names.
59257	41	7	41	7	Can you provide a common example of which external forcing would have prescribed changes and which variables are held to pre-industrial levels? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected: space constraints do not allow to add more details here. For information a simple example is the classical 1percentCO2 simulation that has been used for decades now.
21169	41	10	41	10	some efforts has been made to do the intercomparson between bias correction methods: https://www.earth-syst-dynam-discuss.net/esd-2020-10/ [Faranak Tootoonchi, Sweden]	Rejected. This is not the subject of this paragraph. This is covered elsewhere in section 3.
68485	41	17	41	18	Please cite Watanabe et al. (2014, NCC) which showed usefulness of partial coupling simulation with prescribing daily wind stress anomalies from reanalysis. - Watanabe, M., Shiogama, H., Tatebe, H. et al. Contribution of natural decadal variability to global warming acceleration and hiatus. Nature Clim Change 4, 893–897 (2014). https://doi.org/10.1038/nclimate2355 [Yukiko Imada, Japan]	Taken into account. The reference has been added for the FGD.
21171	41	18	41	18	another references for pseudo reality: https://doi.org/10.3390/cli6020033 [Faranak Tootoonchi, Sweden]	Rejected. The reference is about bias correction that is extensively covered elsewhere in section 3.
79171	41	18	41	20	Many of the studies cited in this paragraph identify the regional impacts of PDV as well as AMV. Delworth et al. (2015 J Climate, doi: 10.1175/JCLI-D-14-00616.1) examines PDV's regional influence by pacemaker simulations and is relevant here. [Yu Kosaka, Japan]	Accepted. The suggested reference has been added for the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4283	41	19	41	20	It seems strange to only mention that this method has been used for the AMV here when many more studies have used it to look at tropical Pacific influences. Suggest that the fact it has been used to study the tropical Pacific should be mentioned too. [Isla Simpson, United States of America]	Accepted. Some examples for PDV have been added for the FGD.
66555	41	30	41	32	Two examples of using RCMs for investigating the sensitivity to changes in land cover (afforestation/deforestation) over Europe include 1) Strandberg, G. and Kjellström, E., 2019. Climate impacts from afforestation and deforestation in Europe. Earth Interactions, 23(1), 1-27, DOI: 10.1175/EI-D-17-0033.1. and 2) Belusic, D., Fuentes-Franco, R., Strandberg, G. & Jukimenko, A. (2019). Afforestation reduces cyclone intensity and precipitation extremes over Europe. Environmental Research Letters, 14(7), Article ID UNSP 074009. [Kjellström Erik, Sweden]	Accepted. One reference has been added for the FGD.
35245	41	30		32	You may want to also assess Nabat et al. 2014 (already cited in this chapter) where a sensitivity experiment with a RCM is used to attribute observed past trends in Europe to the aerosol forcing imposing the observed internal varability thanks to reanalysis-driven runs. [SAMUEL SOMOT, France]	Taken into account. The reference has been cited for the FGD.
35247	41	30		32	You may want to assess Boé et al. (2020) in which the sensitivity of European future climate change to evolving aerosols is tested (see section 2.4) following one of the protocol of the FPS-aerosol [SAMUEL SOMOT, France]	Rejected. This study is already cited in another and more relevant context in section 3.
106591	41	31	41	32	Please add the following two references in the first and second set of references respectively: Rowell and Jones (2006) and Kendon et al., (2010): Rowell D. P. and R. G. Jones, 2006: Causes and uncertainty of future summer drying over Europe. Climate Dynamics DOI 10.1007/s00382-006-0125-9; Kendon EJ, Rowell, D.P. and Jones, R.G., 2010: Mechanisms and reliability of future projected changes in daily precipitation. Climate.Dynamics, 35:489–509, doi: 10.1007/s00382-009-0639-z [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We have given priority to the new papers versus those that have been largely assessed in previous reports.
125665	41	35	41	52	[ACCESSIBILITY] Section 10.3.2.4 provides nothing unique to regional climate. Is it covered elsewhere? If so, recommend removing. [Trigg Talley, United States of America]	Accepted. Chapter 3 introduces and uses the control simulations (rather abruptly) and makes many references to the use of picontrol simulations. Following the comment we reduced the sub-sub-section to the strict minimum keeping the part "These simulations are also used along with large ensembles of historical or scenario simulations to assess the characteristics of the regional internal climate variability (Olonscheck and Notz, 2017)" and made a explicit link to Chapter 3.
35249	41	35			For examples of control runs performed with Ocean-RCMs or coupled RCMs, see Somot et al. 2006 for ORCM doi :10.1007/s00382-006-0167-z) and Soto-Navarro et al. 2020 (only mentioned in suppl.mat. However, https://doi.org/10.1007/s00382-019-05105-4) [SAMUEL SOMOT, France]	Accepted. The sub-section has been substantially shortened making reference to Chapter 3, but the most recent reference has been included.
35251	42	1		8	The evaluation mode in RCMs can also be used to assess and attribute observed past trend using imposed natural variability what is always impossible with GCMs. Find examples in Lorenz and Jacob 2010 (doi: 10.3354/cr00973), Zubler et al. 2011 (already cited), Nabat et al. 2014 (already cited in the chapter), Gutierrez et al. 2018 (https://doi.org/10.1016/j.solener.2018.09.085), Drugé et al. 2019 (https://doi.org/10.5194/acp-19-3707-2019,) [SAMUEL SOMOT, France]	Accepted. The sentence has been modified.
110165	42	3	42	8	There is here (in Section 10.3.2.5) an assertion that in experiments using "perfect" boundary conditions (BCs), any simulated-observed discrepancy would be due only to errors and/or internal variability from the modelling system. Is this true ? Possibly it could be true, if and only if one could have access to "perfect" BCs. However, because "perfect BCs" is a misnomer (perfect BCs do not, and arguably cannot, exist), the assertion is false. Errors in the lateral BCs generally exist when a reanalysis is used to drive a model, and a priori they can be another source of the inner-domain simulation-observation discrepancies. [Patrick Grenier, Canada]	Taken into account; indeed reanalysis are not perfect boundary conditions. However, reanalysis driven runs are the standard method to assess errors by the RCM prior to downscale GCMs runs. It is assumed that reanalysis are closer to observed climate than GCMs (especially for assimilated variables) and, as such, provide the best available boundary conditions. Reanalysis driven runs are often used to evaluate the added value of the downscaling compared to the reanalysis simulated fields, like precipitation. The sentence has been clarified as follows "Although reanalysis can introduce biases especially for non assimilated variables (such as precipitation) it is assumed that in such a setting, discrepancies between the modelled and observed climate arise mostly from errors in the downscaling method"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23649	42	4	42	6	In a recent study, Bozkurt et al. (2020) evaluated the dynamically downscaled simulations of ERA-Interim over the Antarctic Peninsula and they showed that unlike the reanalysis, regional climate model simulations capture the persistent cooling trend (1991-2015) observed at the Larsen Ice Shelf station. Bozkurt, D., D. H. Bromwich, J. Carrasco, K. M. Hines, J. C. Maureira, and R. Rondanelli, 2020: Recent near-surface temperature trends in the Antarctic Peninsula from observed, reanalysis and regional climate model data. Adv. Atmos. Sci., 37, 477-493. https://doi.org/10.1007/s00376-020-9183-x. [Deniz Bozkurt, Chile]	Accepted. The reference has been added.
96097	42	6	42	8	Does recent research give information on how to discern differences between the modelled and observed climate resulting from errors in the downscaling method and internal climate variability generated by the downscaling method? [Nicole Wilke, Germany]	Taken into account; in order to assess differences between observed and modelled internal variability generated by downscaling, large ensembles of RCM simulations are needed. This is still a very new and challenging topic, with only very few studies available, which have been added to the text.
71613	42	10	42	18	Although only the main references have been included for the VALUE initiative, each of the referred analysis was published in an independent paper led by a particular author (Gutiérrez J.M.: DOI: https://doi.org/10.1002/joc.5462; Maraun D.: DOI: https://doi.org/10.1002/joc.5222; Widman M.: DOI: https://doi.org/10.1002/joc.6024; Hertig E.: DOI: https://doi.org/10.1002/joc.5469, etc.). [Sixto Herrera, Spain]	Rejected; two of the main VALUE papers are already cited and there is no need to cite other papers for the same experiment, also to keep the balance with other kinds of experiments and world regions.
72125	42	21	44	45	In Africa a tremendous number of studies evaluating GCMs VS. RCMs have recently been published. But from 0 to few have been cited in this section. This is not a balanced literature citation. To my opinion the literature citation should be balanced as much as possible. Therefore some of the available studies from Africa could be included rather than only taking examples from Europe, America or Asia. In fact, Gibba et al. 2019 did a comprehensive assement of CMIP5 vs. CORDEX to identify added value in simulating extreme precipitation in Africa (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). Sylla et al. 2016 did a comprehensive assessement of climate change using a multivariate approach and considering mutiple data sources (CMIP5, CORDEX and a designed high resolution regional climate change experiments) and exploring issues such as the sensitivity of the added value to ensemble size, resolution, the effect of local forcings for the historical and future climates (Sylla MB, N Elguindi, F Giorgi, D Wisser (2016): Projected Robust Shift of Climate Zones over West Africa in Response to Anthropogenic Climate Change for the Late 21st Century. Climatic Change 134: 241-253. DOI: 10.1007/s10584-015-1522-z1). There are many others: Taguela et al. 2019, DOI: https://doi.org/10.1029/2019JD031607; Gomora et al. (2018), DOI: http://dx.doi.org/10.1007/s00382-017-3886-4; Dosio et al. (2019), DOI: https://doi.org/10.1007/s00382-019-04900-3. [Mouhamadou Sylla, Rwanda]	Noted; however, sections 10.3.3 is not meant to provide region by region assessment of RCM performances. These assessments are provided in the Atlas, as explicitly mentioned in section 10.3.2.5 (for reanalysis driven RCM runs) and 10.3.3.10 (overall assessment of RCM performances.
22873	42	23	42	29	This paragraph should point to chapter 3 for a substantive assessment of global performance of ESMs so the reader knows where to go to find this. [Peter Thorne, Ireland]	taken into account. Reference has been added
106593	42	32	42	32	Please add assessment findings in this sub-section and consider if all the text/references are needed to support them. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Assessment statement added.
125667	42	32	42	50	The references for the use of meteorological regimes to evaluate GCMs are a bit light. Here are a few more: e.g., Barton et al. 2012; doi:10.1029/2012JD017589 and Taylor et al. 2019; https://doi.org/10.5194/acp-19-8759-2019). [Trigg Talley, United States of America]	Accepted. The reference has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1355	42	32	43	20	The section of evaluation lacks different perspectives for empirical-statistical downscaling: the first is the evaluation of the calibration of the models through cross-calibration without involving the GCMs; The second is the evaluation of the statistical models when used together with the GCM output; The third is to evaluate the downscaled climate variables using large multi-model ensembles; And fourth concerns the covariance structure within different predictands (sites). The cross-validation was well covered within the VALUE project (DOI: 10.1002/joc.5462) and CORDEX-ESD, but he other aspects are also important and need to be considered when the downscaling is used to inform decision-makers. The second aspect has been valuated though using the common EOFs (mentioned above) as a framework for the downscaling, ensuring the same spatial predictor patterns associated with local variability in the calibration process are used for the projection. The GCMs are not perfect, and hence systematic differences between reanalyses and GCMs degrade the skill to some extent (the GCMs can be bias-adjusted though the common EOF framework, DOI: 10.1007/s00704-005-0133-4). When it comes to the third point, an ensemble of downscaled resuts can be evaluated against historical data in terms of trend and interannual variability (see DOI: 10.1088/1748-9326/11/5/054017). When it comes to consistent covariance in the predictand, there is a study that compares ESD downscaling single stations individually and as a group by using PCA to represent the local data (DOI: 10.3402/tellusa.v67.28326). The use of PCA to represent predictand is particularly useful for a group of stations within a small region. [Rasmus Benestad, Norway]	Not applicable. This comment does not really fit in the context of the subsection (evaluation diagnostic). However, the first two points are too technical for the scope of the chapter, the covariant structure within different predictands is already discussed in this section, the need of using large multi model ensembles is discussed in 10.3.4 and the multi-site aspect is explicitly listed in section 10.3.3.7
90975	42	34	42	34	I think of evaluation as encompassing more than just comparison with observations. It includes, for instance, thinking about the way particular processes are represented (relative to theoretical understanding), about model resolution, about the extent of tuning, etc. This is reflected elsewhere in the chapter, but the language here makes it sound like evaluation involves just looking at performance. [Wendy Parker, United Kingdom (of Great Britain and Northern Ireland)]	Noted. However, the entire paragraph has been removed. As the reviewer states, these topics are addressed in other sections.
125669	42	34	42	38	The first paragraph of Section 10.3.3.1 is unnecessary for readers of this document. Recommend removal for brevity. [Trigg Talley, United States of America]	Accepted. The paragraph has been removed
65301	42	40		49	This list of evaluation diagnostic is very much atmosphere oriented whereas evaluation for the other components of the regional climate systems are also performed (river, sea, sea ice, aerosols, cities). I hope that the atmosphere-oriented text of this chapter is a choice and not only due to the author list composition. [SAMUEL SOMOT, France]	Taken into account. We have added some references for the land-atmosphere coupling (https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/joc.4274; https://doi.org/10.1175/BAMS-D-17-0001.1) and sea-atmosphere coupling (https://link.springer.com/article/10.1007/s00382-013-1783-z; https://doi.org/10.1175/JCLI-D-17-0137.1). However, given space constraints, it is not possible to list all possible metrics for the diverse components of the climate system.
65305	42	40		49	original evaluation of the record-breaking values in GCM (Bador, M., Terray, L., & Boé, J. (2016). Detection of anthropogenic influence on the evolution of record-breaking temperatures over Europe. Climate dynamics, 46(9-10), 2717-2735.) and RCM (Bador M., Terray L., Boé J., Somot S., Alias A., Gibelin A.-L., Dubuisson B. (2017) Future summer mega-heatwave and record-breaking temperatures in a warmer France climate. ERL, 12(7), http://iopscience.iop.org/article/10.1088/1748-9326/aa751c) [SAMUEL SOMOT, France]	Rejected; there are several papers in the literature addressing the issue of metrics for extreme temperature events, and they cannot be all cited (also, extreme events are dealt with in Ch11). Here we decided to list only a few representative ones, based on the standard ETCCDI indices.
65297	42	40			Concerning the evaluation of RCMs, the article Vautard et al. (in revision. Evaluation of the large EURO-CORDEX regional climate model ensemble. Journal of Geophysical Research – Atmospheres) is probably one of the must in terms of diversity of the indices/variables evaluated and size of the ensemble [SAMUEL SOMOT, France]	Accepted. The reference has been added.
38543	42	41	42	41	Maybe mention the new paper (revised version submitted in May 2020) of the large EURO-CORDEX ensemble which also evaluates contribution from RCMs and GCMs : 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]	Accepted. The reference has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
110569	42	43	42	47	Two other topics should be included. Bukovsky et al. (2017) developed diagnostics to study summer precipitation in the southern great plains and used them to evaluate precipitation (and future changes) in the southern great plains of the US. A number of other studies have developed diagnostics to evaluate African Easterly Waves in GCMs (McCrary and Randall, 2014; Martin and Thorncroft (2015); Martin, E. R., Thorncroft, C. (2015). "Representation of African Easterly Waves in CMIP5 Models." Journal of Climate, Vol. 28, No. 19, 7702–7715. McCrary, R. R., D. A. Randall, and C. Stan, 2014: Simulations of the West African Monsoon with a Superparameterized Climate Model. Part II: African Easterly Waves. Journal of Climate, 27, 8323–8341, https://doi.org/10.1175/JCLI-D-13-00677.1. Bukovsky, M. S., R. R. McCrary, A. Seth, and L. O. Mearns, 2017: A Mechanistically Credible, Poleward Shift in Warm-Season Precipitation Projected for the U.S. Southern Great Plains? Journal of Climate, 30, 8275–8298, https://doi.org/10.1175/JCLI-D-16-0316.1. [Rachel McCrary, United States of America]	Accepted. References have been added.
65303	42	46			Intercomparison of cyclone tracking in RCMs (if needed to complete the Neu et al. work on GCM) : Flaounas E., Kelemen F., Wernli H., Gaertner M.A., Reale M., Sanchez-Gomez E., Lionello P., Calmanti S., Podrascanin Z., Somot S., Akhtar N., Romera R., Conte D. (2018) Assessment of an ensemble of ocean-atmosphere coupled and uncoupled regional climate models to reproduce the climatology of Mediterranean cyclones. Climate Dynamics, 51(3), 1023-1040, doi: 10.1007/s00382-016-3398-7 [SAMUEL SOMOT, France]	Accepted. The reference has been added.
98097	42	47	42	49	This statement misses an important point and could be augmented as follows. Evaluating whether a model simulated trend is consistent with observations actually involves assessing whether the amount of disagreement between model and observed trend is within the range of disagreement expected due to internal variability alone. In other words, for long-term climate simulations, we don't expect the real world and model simulated internal variability to match up in phasing. So instead we either use large ensembles, to see if observations lie within the 5th to 95th percentile of the ensemble (if so, it might be called at least "not inconsistent"). If large ensembles are not available, we can instead use moderate sized ensemble to estimate the forced response in the model, and use control run variability to estimate the internal variability noise (or scatter about the mean due to internal variability). Finally it could be noted in this section that having a set of model historical run that is inconsistent with observed trends generally implies less confidence in projections with those models compared to a case where the model historical runs and observed trends are consistent for some representative historical trend periods. A reference for such methods discussed above would be Knutson et al. (2013) or Knutson and Zeng (2018) (in your reference list already). [Thomas Knutson, United States of America]	Noted: the sentence has been removed as evaluation of trends is discussed in Section 10.3.3.8, where the reviewers' comment has been already taken into account.
65299	42	47		49	This sentence is relevant for GCM but quite less for RCM, often evaluated in the so-called « evaluation-mode » in which the synoptic chronology is imposed. Nabat et al. 2015 (already cited) is a good illustration of cuh a case study oriented evaluation. We also do that a lot in regional oceanography for which long-term climatologies are rare but field campaign are worse to be used. This kind of case study evaluation is one of the strenght of the RCMs. We used it a lot in Med-CORDEX that is strongly associated to the HyMeX field campaign. [SAMUEL SOMOT, France]	Noted; the evaluation runs for RCMs are discussed in section 10.3.2.5
68949	42	51	42	51	The first sentence ("Diagnostics are a complex set.") doesn't really make sense to me. Delete or revise. (If it just means "diagnostics are complex", delete to save space.) [Seth McGinnis, United States of America]	Accepted. The text has been modified.
125671	42	51	42	53	The sentence needs to be rewritten (difficulty to follow): "To characterize compound events (Zscheischler et al., 2018), a family of events defined by several variables that might not be extreme individually, new diagnostics for multivariate dependencies are needed." Change it to "New diagnostics for multivariate dependencies are needed to characterize compound events (Zscheischler et al., 2018) because a family of events defined by several variables might not be regarded to occur independently." [Trigg Talley, United States of America]	Accepted. Sentence has been shortened and rephrased
116957	42		42		Please explain in section 10.3.3 what builds on the evaluation done in ex ch 3, 6, 7, 8, 9 and what is specific here. There could be similarity with the issue of ice sheet mass balance and polar simulations in ch 9. [Valerie Masson-Delmotte, France]	Taken into account. The structure has been changed. In the new introduction to Section 10.3.3 an overview has been given about assessments in previous chapters.
108123	43	1	43	1	Instead of the term "bias correction" I suggest to use the term "bias adjustment", which is explained in Chapter 10 Section 10.3.1.4.2 and used in Chapter 2, 8, 10 and 12. [Claas Teichmann, Germany]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
110917	43	7	43	7	Please also consider citing Bukovsky et al. 2017 here as an RCM example. It is relevant to the point you are making here about using process-based analysis. Bukovsky, M.S., R.R. McCrary, A. Seth, L.O. Mearns, 2017: A mechanistically credible, poleward shift in warm-season precipitation projected for the U.S. Southern Great Plains? J. Climate, 30, 8275-8298, doi: 10.1175/JCLI-D-16-0316.1. [Melissa Bukovsky, United States of America]	Accepted. References have been added.
66557	43	8	43	8	The Kjellström et al paper does not fit in here. It is about RCMs, not bias adjustment methods and it does not concern model evaluation but trends over the period 1961-2050. [Kjellström Erik, Sweden]	Accepted. The reference has been removed.
110571	43	11	43	12	Rhoades et al. (2018) developed user-relevant diagnisocs for assessing mountain snowpack in regional climate models. Rhoades, A. M., Jones, A. D., & Ullrich, P. A. (2018). Assessing mountains as natural reservoirs with a multimetric framework. Earth's Future, 6, 1221– 1241. https://doi.org/10.1002/2017EF000789 [Rachel McCrary, United States of America]	Accepted. References have been added.
112853	43	11	43	20	Here, a relevant reference to include is that of Nissan et al. 2020, stressing the need for targeted model evaluation for regional climate impact studies and climate services: Nissan, H., Muñoz, Á.G. and Mason, S.J., 2020. Targeted model evaluations for climate services: A case study on heat waves in Bangladesh. Climate Risk Management, 28, p.100213. https://doi.org/10.1016/j.crm.2020.100213 Also relevant for section 10.3.4.4 Designing and using ensembles for regional climate change assessments to take uncertainty into Account [Paula Gonzalez, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The reference has been added.
15655	43	16	43	20	The Verfaillie et al. (2017 https://doi.org/10.5194/gmd-10-4257-2017) provides an evaluation of the ADAMONT adjustment method for the French Alps, including through the use of an impact model (here, snow cover model with various snow cover indicators). [Samuel Morin, France]	Accepted. Reference added.
111575	43	18	43	19	Another recent impact study on forestry where GCMs and RCMs with bias-adjustment have been used is in Sustainability 2017, 9(7), 1152; https://doi.org/10.3390/su9071152 [Volodymyr Osadchy, Ukraine]	Rejected; the study is not based on bias-adjusted and/or statistical downscaling methods, and is focused on projections, rather than evaluation.
112049	43	19	43	19	A more recent reference for wildfire coud be https://doi.org/10.1007/s10584-013-0787-3 [jose manuel gutierrez, Spain]	Accepted. Reference has been added.
106595	43	23	43	23	Please add assessment findings in this sub-section and consider if all the text/references are needed to support them. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Section 10.3.3.10 "Synthesis of Model Performance at Simulating Regional Climate and Climate Change" gives assessment statements based on all the material presented in 10.3.3, including an assessment statement on added value in downscaling.
65307	43	23	44	33	Various comments concerning this sub-section : a lot of material in added-value but not enough on model improvement and in particular on the model improvements obtained by model development, improved parameterization, better forcing or new levels of complexity. Currently the text is a lot about resolution. [SAMUEL SOMOT, France]	Accepted. Although the subsection has been shortened and restructured, it has been rewritten to point to examples of model improvements in further subsections of 10.3.3 and in the Atlas.
65309	43	23	44	33	I like the discussion on the PAV but I miss a deeper discussion on the fact that added-value is often obtained in evaluation mode for RCM and that it does not guarantee an added-value in historical mode or even more for climate change signal. Articles such as Giorgi et al. 2016 (doi:10.1038/ngeo2761) could be added to illustrate the added-value in future climate [SAMUEL SOMOT, France]	Accepted. The analysis of added value in projected simulation included as a further example of the importance of assessing added value via evaluation of physical processes.
65311	43	23	44	33	Even if I know the quality of the work, there is really to many Di Luca et al. citations in this paragraph for a topics that has been tackle by many authors over the recent years. Just count to check. [SAMUEL SOMOT, France]	Taken into account. Since this is the first IPCC AR to give a detailed presentation on added value, we are citing the foundational papers. However, in restructuring the section and making it more succinct, some of the Di Luca references have been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65313	43	23	44	33	In my opinion, a good practice (rarely used) to evaluate added-value of RCMs is to run RCMs at the reanalysis or GCM resolution, that is to say low-resolution RCMs. Very few ref to my knowledge but they exist : Herrmann et al. 2011 (already cited), Ruti et al. 2016 (already cited, fig 3), and the very recent Wu et al. 2020 (https://doi.org/10.5194/esd-11-377-2020) [SAMUEL SOMOT, France]	Rejected. All of these papers examine how resolution affects their results. While results likely change with resolution, they do not examine how the finer resolutions give added value to the input reanalyses as a consequence of the coarse-resolution simulation's behaviour, other than to say, perhaps, that the coarse-resolution model is inadequate. One paper (Wu et al.) finds that model physics are more important for improving the simulation than increasing resolution. The section already states that, "Added value is not guaranteed simply by producing model output at finer resolution; it can depend on several factors,...". and the final draft uses nearly similar language. The papers do not, in themselves, add more to the discussion presented here.
64605	43	23	44	36	An element of this section that seems to be lacking is an identification of which types of climate phenomena would be expected to benefit most from dynamical downscaling. For example, irrespective of the variable concerned, the simulation of short timescale (daily and shorter) processes and the description of climate over complex terrain and coastal areas would be expected to benefit most from these approaches. It would be appropriate to cite appropriate research work for examples of these applications here. [Charles Curry, Canada]	Noted. The text of the final draft does list several examples of where added value might occur and then specific phenomena are discussed in section 10.3.3, and we refer to that section. However, it is not possible to give and exhaustive list beyond stating the conceptual considerations of where added value is possible, because, as we have noted, what constitutes added value depends in part on the needs of the users of the climate information.
72075	43	25	44	33	Here reducing mean-state biases (such as dry bias over Indian landmass) for summer monsoon similation is important. Here air-sea coupling and representation of upper ocean mixing (in terms of proper MLD representation in the ocean model) is also important for regional climate simulation, such as Indian summer monsoon simulation. A recent study showed the role of narrow coastal Bay of Bengal SST front and MLD dynamics for proper atmospheric convection and Indian summer monsoon simulation in climate models. The study is important in this context and should be mentioned here. Samanta, D., Hameed, S. N., Jin, D., Thilakan, V., Ganai, M., Rao, S. A., & Deshpande, M. (2018). Impact of a narrow coastal Bay of Bengal sea surface temperature front on an Indian summer monsoon simulation. Scientific reports, 8(1), 1-12. [Samanta Dhrubajyoti, Singapore]	Taken into account. This statement is more important where we assess simulation of land-ocean interactions (10.3.3.5) and the paper has been cited there.
27535	43	27	43	28	About 'This further useful information is often referred to as added value [...]': This definition of added value is ambiguous, and "useful" is extremely subjective. In any case, "added value" is a ambiguous notion, but it could be better discussed before using the expression "added value" very frequently. [Eric Brun, France]	Taken into account. The issue of what is "useful" is indeed subjective, as it depends in part on the needs of those using climate information. We now refer to Section 5, where the issue of useful information is discussed.
51543	43	27	43	30	There is a good discussion on added value in Giorgi (2019) sec. 4.2.1 that could be referenced here. Giorgi, 2019, 'Thirty years of Regional Climate Modeling: Where are we and where are we going next', JGR Atmosphere, DOI: 10.1029/2018JD030094 [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. That paper is largely a review, and specific issues reviewed there are already covered here, citing the primary sources.
112981	43	28	44	33	There is useful discussion on added value. Discussion on added value for different regions of RCMs the reader maybe refered to the relevant section of Chapter Atlas. [Muhammad Amjad, Pakistan]	Accepted. Text added noting that the Atlas gives examples of model improvement and added value.
51541	43	32	43	36	It would be good to acknowledge the added value of downscaling over urban areas. For example the following study could be cited: Argueso, D., J.P. Evans, L. Fita, K.J. Bormann (2014) Temperature response to future urbanization and climate change, Clim Dyn, 42:2183–2199, doi 10.1007/s00382-013-1789-6 [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
23645	43	34	43	35	In connection with the importance of added value over complex terrain, I recommend citing a recent study showing the important role of the spatial resolution on added value in complex terrains southwest South America (Andes Mountains): 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]	Accepted. The reference has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
91041	43	34	43	35	Another dynamic downscaling paper clearly demonstrating added value through the enhanced representation of topography is the paleo modelling paper of Engelbrecht et al. (2019): Engelbrecht F.A., Marean C.W., Cowling R., Engelbrecht C., Nkoana R., O'Neal D., Fisher E., Shook E., Franklin J., Neumann F.H., Scott L., Thatcher M., McGregor J.L., Van der Merwe J., Dedekind Z. and Difford M. (2019). Downscaling Last Glacial Maximum climate over southern Africa. Quaternary Science Reviews 226 105879 https://doi.org/10.1016/j.quascirev.2019.105879 [Francois Engelbrecht, South Africa]	Rejected. An interesting paper, but the added value of downscaling compared to a coarser-resolution model is not discussed nor analysed.
65315	43	36	43	38	I feel that some references could be added when speaking about the added-value for precipitation due to the very large literature concerning this topics over the recent years in particular with CPRCM [SAMUEL SOMOT, France]	Rejected. This is not a review. The added value capabilities of CP RCMs is discussed later, in Section 10.3.3.4.1.
51545	43	36	43	38	references for added values in precipitation (with convective permitting models) can be found in the work of Kendon et al., (2012, 2014, 2019) and Chan et al., (2014a, 2018) [Chan S.C., E.J. Kendon, H.J. Fowler, S. Blenkinsop, N.M. Roberts, C.A.T. Ferro (2014) The value of high-resolution Met Office regional climate models in the simulation of multi-hourly precipitation extremes. J Climate, 27, 16, 6155-6174, doi 10.1175/JCLIM-13-00723.1 ; Chan, S.C., Kahana, R., Kendon, E.J. et al. Projected changes in extreme precipitation over Scotland and Northern England using a high-resolution regional climate model. Clim Dyn 51, 3559–3577 (2018). https://doi.org/10.1007/s00382-018-4096-4] [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This is not a review. The added value capabilities of CP RCMs is discussed later, in Section 10.3.3.4.1.
10943	43	36	43	38	A suitable reference to the sentence "Precipitation, … in duration" could be the following: Olsson, J., Berg, P., and A. Kawamura (2015) Impact of RCM spatial resolution on the reproduction of local, sub-daily precipitation, J. Hydrometeorol., 16, 534–547, doi:10.1175/JHM-D-14-0007. [Jonas Olsson, Sweden]	Not applicable. The referenced sentence has been deleted.
109899	43	45	43	49	I suggest including the warning made by Giorgi (2019) when biases inherent in the RCM give low (or negative, depending on the method) Added Value and they are misinterpreted as a failure in downscaling technique. "Also, it should not be expected, as it is instead often done, that an RCM should be better than the driving GCM in all respects. Because different models are affected by different specific systematic biases, the AV is necessarily variable dependent." Giorgi, F., 2019. Thirty Years of Regional Climate Modeling: Where Are We and Where Are We Going next? J. Geophys. Res. Atmos. 124, 5696–5723. https://doi.org/10.1029/2018JD030094 [Vanesa Pántano, Argentina]	Accepted. This comment actually applies more broadly than to only the statistical methods discussed in the lines cited. The paper, with its implications for added value, has been cited in the new synthesis section, 10.3.3.10.
20665	43	51	43	51	Are metrics available for added value? [philippe waldteufel, France]	Not applicable. Paragraph has been deleted in making the section more succinct.
64607	43	52	43	53	The single references provided here do not do justice to the amount of published work on these topics. E.g., consider adding citations to: Curry, C. L., Tencer, B., Whan, K., Weaver, A. J., Giguère, M., & Wiebe, E. (2016). Searching for added value in simulating climate extremes with a high-resolution regional climate model over western Canada. Atmosphere-Ocean, 54(4), 364-384 at line 52, and Curry, C. L., Tencer, B., Whan, K., Weaver, A. J., Giguère, M., & Wiebe, E. (2016). Searching for added value in simulating climate extremes with a high-resolution regional climate model over Western Canada. II: basin-scale results. Atmosphere-Ocean, 54(4), 385-402 at line 53, respectively. [Charles Curry, Canada]	Not applicable. Paragraph has been deleted in making the section more succinct and replaced by a single sentence that notes the need for a variety of performance measures.
110919	43	53	43	53	I suggest also citing Ciarlo` et al. 2020 with Soares and Cardoso 2018. In Ciarlo` et al., added value is assessed across both CORDEX-CORE and Euro-CORDEX simulations using a PDF-based method. Ciarlo`, J.M., and Coauthors, 2020. A new spatially distributed added value index for regional climate models: the EURO-CORDEX and CORDEX-CORE highest resolution ensembles. Climate Dynamics, submitted Dec. 2020 (revised and resubmitted May 2020). [Melissa Bukovsky, United States of America]	Not applicable. The paragraph has been deleted in making the section more succinct.
110921	44	7	44	7	I think that saying "the downscaling method lacks usefulness by that measure" would be more accurate. [Melissa Bukovsky, United States of America]	Rejected. The statement is based on the cited work and is stated to be an implication of the lack of PAV.
22875	44	15	44	15	"big brother" probably isn't appropriate to use in an IPCC report [Peter Thorne, Ireland]	Not applicable. Paragraph has been deleted in making the section more succinct.
22877	44	18	44	24	It feels like you have already said this. Can the similar text be merged to avoid a feeling of repetition here? [Peter Thorne, Ireland]	Accepted. The text has been shortened and combined with previous statements to be overall more succinct.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
91043	44	18	44	24	The paleo time-scale can also be added, in terms of where dynamic downscaling has been demonstrated to add value: Engelbrecht F.A., Marean C.W., Cowling R., Engelbrecht C., Nkoana R., O'Neal D., Fisher E., Shook E., Franklin J., Neumann F.H., Scott L., Thatcher M., McGregor J.L., Van der Merwe J., Dedekind Z. and Difford M. (2019). Downscaling Last Glacial Maximum climate over southern Africa. Quaternary Science Reviews 226 105879 https://doi.org/10.1016/j.quascirev.2019.105879 [Francois Engelbrecht, South Africa]	Rejected. An interesting paper, but the added value of downscaling compared to a coarser-resolution model is not discussed nor analysed.
66585	44	20	44	23	Another factor that may be of relevance is the model formulation in itself. In a study with two RCMs run at resolutions from the GCM-scale (200 km) in finer and finer steps down to typical CORDEX resolution (50km) Wu et al finds that resolution is only partly improving the realism of the results compared to the driving GCMs, also the model formulation plays role. They conclude "Our results show that improvements in the ability of RCMs to simulate precipitation in Africa compared to their driving reanalysis in many cases are simply related to model formulation and not necessarily to higher resolution. Such model formulation related improvements are strongly model dependent and can, in general, not be considered as an added value of downscaling." Wu, M., Nikulin, G., Kjellström, E., Belušić, D., Jones, C., and Lindstedt, D., 2020. The impact of RCM formulation and resolution on simulated precipitation in Africa, Earth Syst. Dynam., 11, 377–394, DOI:10.5194/esd-11-377-2020. [Kjellström Erik, Sweden]	Noted. Other factors besides resolution have already been noted, and the papers that have been cited make this point earlier in the literature.
5501	44	23	44	23	Insert between "Xue et al., 2014). and Unforced": "Kim et al. (2018) showed that finer spatial resolutions of an RCM do not necessarily improve downscaling based on an added-value analysis of RCM simulations of multiple resolutions." [Jinwon Kim, United States of America]	Rejected. The referenced paper does not add significantly to the more foundational papers already cited. In addition, the referenced paragraph has been deleted and text restructured in making the section more succinct.
16941	44	26	44	27	This is a very important point and it has been observed in applications at different spatio-temporal scales. I recommend to add other references which corroborate this issue, e.g. in the case of urban climate applications (Mussetti et al., 2020). Reference: Mussetti, Gianluca, et al. "Simulating urban climate at sub-kilometre scale for representing the intra-urban variability of Zurich, Switzerland." International Journal of Climatology 40.1 (2020): 458-476. https://doi.org/10.1002/joc.6221 [Gianluca Mussetti, Switzerland]	Rejected. An interesting paper, but the added value of downscaling compared to a coarser-resolution model is not discussed nor analysed in that paper. The paper gives a sensitivity analysis of how simulation of an urban environment is affect by resolution.
23647	44	26	44	27	Bozkurt et al. (2019) also pointed out that lack of sufficient and high-quality observational data over the extreme environments such as the high Andes Cordillera and Patagonia can lead to a number of potential uncertainties in assessing the added value, and have a large impact on the robustness of the observational gridded product used as "observed truth". 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]	Accepted. The reference has been added.
22879	44	27	44	29	Which is precisely what the community are actively pursuing https://journals.ametsoc.org/doi/10.1175/BAMS-D-16-0165.1 [Peter Thorne, Ireland]	Accepted. Referenced work has been cited.
52199	44	36	44	54	Surprising the performances of the dynamic models, RCM, in Figures 10.5 and 10.6. How are special high-resolution models guaranteed to improve performance? [Maritza Jadrijevic Girardi, Chile]	Taken into account. The figure is meant as an opener for the assessment in the following subsections, where the question of the reviewer is addressed. This is made clear in the new introduction.
112635	44	38	44	54	Demory et al. (submitted to GMDD) show that EUR-44 (50 km resolution) GCM-driven CORDEX simulations tend to be too wet over Europe compared to observations. Increasing the resolution from EUR-44 (50 km) to EUR-11 (12 km) show an improvement in the spatial distribution of precipitation but not in the mean. PRIMAVERA GCMs (25-50 km resolution) generally show significant improvements compared to EUR-44 and EUR-11 for intense precipitation and for winter and spring wet biases in Central and Eastern Europe. For weak and moderate precipitation, PRIMAVERA distributions are generally within the range of EUR-11 and EUR-44 RCMs. CMIP5 GCMs, however, lack the spatial resolution to properly capture high-intensity precipitation (see also review comments by Ségolène Berthou) [Marie-Estelle Demory, Switzerland]	Not applicable. Section 10.3.3.3 has been moved. Region specific assessments have been deleted. A reference to the Atlas has been added, where such assessments are provided.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
90977	44	41	44	41	Why not just say "accuracy" here, rather than "fitness"? Fitness is usually relative to a purpose or application, whereas here you seen concerned just with how model results fit observations. [Wendy Parker, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The subsection has been removed.
102511	44	42	44	42	Christensen, J.H., Larsen, M.A.D., Christensen, O.B. et al. Robustness of European climate projections from dynamical downscaling. Clim Dyn 53, 4857–4869 (2019). [Philippe Tulkens, Belgium]	Not applicable. Section 10.3.3.3 has been moved. Region specific assessments have been deleted. A reference to the Atlas has been added, where such assessments are provided.
22881	44	49	44	49	systematic biases rather than systematic errors (errors imply the true state is not just knowbale but known which cannot be true). This point applies elsewhere where the term is used such as the caption to figure 10.5. [Peter Thorne, Ireland]	Taken into account. The terminology has been harmonised.
4285	44	49	44	49	“class, performance” → “class with performance” [Isla Simpson, United States of America]	Not applicable. The subsection has been removed.
116959	44		44		Please check that the chapter does not have a "textbook" style, but is an assessment of methods, state of the art, and develops summary statements (which can use the confidence language) to wrap up the key findings (which can be linked to methodological developments and limitations etc). [Valerie Masson-Delmotte, France]	Taken into account. Section 10.3.3.10 "Synthesis of Model Performance at Simulating Regional Climate and Climate Change" has been added. It gives assessment statements based on all the material presented in 10.3.3.
125673	45	1	45	21	The Figure 10.5 caption doesn't describe the figure well for the box-and-whisker plots. The black box-and-whiskers are called observations. What does the difference mean among the black box-and-whiskers? Only a single observational-mean value is indicated in the caption. And what are the black symbols across the whole panel? [Trigg Talley, United States of America]	Accepted. The caption has been rewritten and now clarifies all these points.
96099	45	1	45	23	Figure 10.5: Top row maps are rather small. I recommend splitting them up into two figures, one for JJA temperature and precipitation, respectively. The legend is missing in the box-and-whisker plot for JJA temperature. [Nicole Wilke, Germany]	Noted. The figure is an illustration, it is not possible to show all the detail mentioned in the comment because there are many different experiments to show and there is no space in the chapter (we are limited to 80 pages) to split it in two figures. More detail can be found in the Interactive Atlas. Concerning the legend for the temperature panel, it is the same as for the precipitation panel. This is clarified in the updated version of the figure.
65317	45	3			Fig 10.5 : not clear what is the family of point that is used to create the box-and-whisker plots. Is it the various grid cells of each dataset or the different years ? [SAMUEL SOMOT, France]	Accepted. The caption has been rewritten and clarifies that the box-and-whiskers have been built using areal means, with one value per year.
65319	45	3			Fig 10.5 Not clear if Euro-CORDX runs are the evaluation or the historical runs ? [SAMUEL SOMOT, France]	Accepted. These are the historical simulations. The caption in the new version of the figure explains where to find all the figure information.
65321	45	3			Fig 10.5 coupled RCMs from Med-CORDEX could have been added to show the improvment or not of coupled RCMs. Coupled RCMs from Med-CORDEX should not be available thanks to the Atlas’ work [SAMUEL SOMOT, France]	Noted. This is the kind of comparison that can be performed with the Interactive Atlas. We decided not to overcomplicate the figure.
65323	45	3			Fig 10.5 I don’t understand the point to compute the biases versus different reference dataset ? In addition E-OBS is not recommanded to evaluate RCM at 12km (see Fantini et al. 2018 for better dataset, doi:10.1007/s00382-016-3453-4). This does not facilitate the figure understanding. If you want to illustrate large-scale biases, you can use BEST and compute the bias after interpolating the RCMs to lower-resolution. If you want to illustrate the biases of the GCM over the coast or mountain, then it is better to compute the biases at higher-resolution using E-OBS or even better products (see Fantini et al. 2018) [SAMUEL SOMOT, France]	Accepted. The procedure recommended by the reviewer has been followed.
54385	45	12	45	13	Regarding Figure 10.5 - there is no legend in the chart in a bottom row of (a). I assume that it is the same as in (b) chart, but should be at both charts [Gabriel Stachura, Poland]	Noted. The legend of the bottom panel of the a) part is the same as in the b) part. This is clarified in the legend of the revised version.
54383	45	15	45	15	I can't see where the description of (b) starts [Gabriel Stachura, Poland]	Accepted. The revised version has now the "b)" in the corresponding place of the caption".
54387	45	28	45	29	Use (a) and (b) in the descprition as it stands in the chart, or delete (a) and (b) and then you can say top and bottom chart [Gabriel Stachura, Poland]	Not applicable. Figure 10.6 has been removed for FGD
65325	45	28			Fig 10.6 I would recommand to use the best observation dataset available for this figure in order to evaluate honestly the tail of the pdf. This means not relying on E-OBS for example but on national datasets (see Fantini et al. 2018 doi:10.1007/s00382-016-3453-4 or even Fumière et al. 2019 for France doi:10.1007/s00382-019-04898-8). Following this comment, the box chosen are likely not the best choice as it will be difficult to find references for the MD or SC zones. [SAMUEL SOMOT, France]	Not applicable. Figure 10.6 has been removed for FGD. The challenge of using suboptimal observational references is discussed in 10.2 and 10.3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67571	45	37	45	40	Add the reference Tian and Dong (2020) for the precipitation and double-ITCZ bias in CMIP3/5/6 models. Tian, B., & Dong, X. (2020), The Double-ITCZ Bias in CMIP3, CMIP5, and CMIP6 Models Based on Annual Mean Precipitation, Geophys. Res. Lett., 47(8), e2020GL087232, https://doi.org/10.1029/2020gl087232 [Baijun Tian, United States of America]	Not applicable. The chapter focuses on regional changes taking place mainly over land.
22885	45	37	45	41	This passage of text risks severely undermining the in-depth assessment performed in chapter 3. It would be better to instead point the reader to chapter 3 for an assessment of overall model performance and then continue the paragraph with the sentence starting line 43 which is clearly within chapter 10 scope. [Peter Thorne, Ireland]	Accepted. The initial SOD sentences do not undermine the SOD Chapter 3 text, they are in line with it and complement it with material like the current figure 10.6. However, the sub-section has been completely rewritten and the corresponding links to Chapter 3 made.
72077	45	37	45	49	Many important and recent referencres are missing. Such are the following recent papers could be mentioned-- Samanta, D., Karnauskas, K. B., & Goodkin, N. F. (2019). Tropical Pacific SST and ITCZ biases in climate models: double trouble for future rainfall projections?. Geophysical Research Letters, 46(4), 2242-2252. ---- Samanta, D., Karnauskas, K. B., Goodkin, N. F., Coats, S., Smerdon, J. E., & Zhang, L. (2018). -- Coupled model biases breed spurious low frequency variability in the tropical Pacific Ocean. Geophysical Research Letters, 45(19), 10-609. [Samanta Dhrubajyoti, Singapore]	Noted. These references are more suitable for the assessment performed in chapters 8 and 9, which deal with large-scale phenomena (in particular those over the oceans), rather than for chapter 10.
55159	45	37	46	9	Substantial improvements in performance, taking into account internal variability, are found between CMIP5 and CMIP6 for both frequency and persistence of continental-scale atmospheric circulation types. A modest relationship between model resolution and skill is found. -- Cannon, A.J., in press. Reductions in daily continental-scale atmospheric circulation biases between generations of Global Climate Models: CMIP5 to CMIP6. Environmental Research Letters. doi:10.1088/1748-9326/ab7e4f [Nancy Hamzawi, Canada]	Accepted. The Cannon et al. reference is now included in a completely rewritten sub-section.
109783	45	39	45	39	"national" is not a good geographic scale descriptor; how about "regional"? [Flavio Lehner, Switzerland]	Accepted. The sentence has been simplified by removing the list of spatial scales to avoid being prescriptive.
109791	45	44	45	44	It is still rare to go further and evaluate a model's regional sensitivities, rather than just mean and seasonal cycle, but with longer observational records such analysis become possible now (Lehner, F., A. W. Wood, J. A. Vano, D. M. Lawrence, M. P. Clark, J. S. Mankin (2019): The potential to reduce uncertainty in regional runoff projections from climate models. Nature Climate Change, DOI: 10.1038/s41558-019-0639-x). [Flavio Lehner, Switzerland]	Accepted. The emphasis on the emerging literature showing how to bring in process-based evaluation information into the generation of climate information has been made in the revised text.
66965	45	44			GCMs also struggle with correctly resolving regional circulations associated with extreme precipitation. Assessment of model circulation associated with extremes (are models getting extremes for the right dynamical reasons) is an important aspect of model evaluation and fitness for downscaling. Agel, L., M. Barlow, J. Polonia, and D. Coe, 2020: Simulation of Northeast US Extreme Precipitation and Its Associated Circulation by CMIP5 Models. In review. Agel, L, and M. Barlow, 2020: How Well Do CMIP6 Historical Runs Match Observed Northeast US Precipitation and Extreme Precipitation-related Circulation? In review. 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]	Noted. This is assessed in Chapter 8. A link to chapter 8 has been included in the rewritten sub-section.
76855	45	51	45	57	Relative roles of horizontal grid spacing and parameter tuning on the simulation of Indian Summer Monsoon in GCMs have been reported in: Anand, A., Mishra, S.K., Sahany, S. et al. Indian Summer Monsoon Simulations: Usefulness of Increasing Horizontal Resolution, Manual Tuning, and Semi-Automatic Tuning in Reducing Present-Day Model Biases. Sci Rep 8, 3522 (2018). https://doi.org/10.1038/s41598-018-21865-1 [Sandeep Sahany, Singapore]	Accepted. The reference has been introduced in the discussion of the GCMs performance.
65327	45	51		57	Please cite more references showing that some of the GCM biases are not improved by increasing GCM resolution. [SAMUEL SOMOT, France]	Accepted. Some of the latest references illustrating this issue are included in the FGD, plus the links to previous chapters help to support the statement.
78763	45	54	45	57	It is suggested that the major conclusion of the following paper can be included here, which shows the evidence of the improvement of simulated precipitaiton over East Asia as the horizontal resolution increases. Li, Jian, Rucong Yu, Weihua Yuan, Haoming Chen, Wei Sun, and Yi Zhang (2015), Precipitation over East Asia simulated by NCAR CAM5 at different horizontal resolutions, Journal of Advances in Modeling Earth Systems, 7(2), 774-790. [jian li, China]	Noted. Given the limited space, references using multi-model analyses have been given preference.
20667	45	55	45	57	The discussion about the improvement of resolution is going on. It has been repeated over and over that the effects are positive but not over every aspect. Here, the reader would simply like to read an interpretation as to why the Asian monsoon rainfall is improved while the major dry bias (whatever it is) is not [philippe waldteufel, France]	Accepted. The sentence has been revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4287	45	56	45	57	It's a bit unclear what "major dry bias" this is referring to since it hasn't been mentioned before. I assume it's in the Asian monsoon rainfall anchored to orography previously discussed. If so, suggest "fails to solve the major dry bias in this region". [Isla Simpson, United States of America]	Accepted. The sentence has been revised.
116961	45		46		"there is general agreement that increasing resolution imrpoves some long standing biases" : please refer to the assessment (esp linked to Highresmid) in chapters 3, 7, 8, 9 and make sure that the chapter builds on the assesment. The underlying evidence is not explicitely assessed in this chapter only. [Valerie Masson-Delmotte, France]	Accepted. The appropriate links to other chapters have been introduced in the revised text.
71225	45				Figure 10.6, SC title needs more space, currently overlaps with the units of the FR graph above [Nesha Wright, Canada]	Not applicable. Due to the need to shorten the chapter, the figure has been removed.
100841	46	1	46	3	When stochastic term is included to account fo unresolved subgrid scale processes (Berner et al. 2017), some improvement in the representation of internal variability are found. Specifically: MJO (Wang et al. 2016), ENSO variability (Christensen et al. 2017; Yang et al. 2019), precipitation variability (Davini et al. 2017; Watson et al. 2017). More recently Vitale et al. 2020 found that Stochastic Physics improves the simulation of Tropical Cyclones, in particular the seasonal cycle of the TC frequency. (Berner, J., Achatz, U., Batté, L., Bengtsson, L., Cámara, A. de la, Christensen, H. M., et al. (2017). Stochastic Parameterization: Toward a New View of Weather and Climate Models. Bull. Am. Meteorol. Soc. 98, 565–588. doi:10.1175/BAMS-D-15-00268.1) (Christensen, H. M., Berner, J., Coleman, D. R. B., and Palmer, T. N. (2017). Stochastic parameterization and El Niño- southern oscillation. J. Clim. 30, 17–38. doi:10.1175/JCLI-D-16-0122.1.) (Davini, P., Von Hardenberg, J., Corti, S., Christensen, H. M., Juricke, S., Subramanian, A., et al. (2017). Climate SPHINX: Evaluating the impact of resolution and stochastic physics parameterisations in the EC-Earth global climate model. Geosci. Model Dev. 10, 1383–1402. doi:10.5194/gmd-10-1383-2017.) When stochastic term is included to account fo unresolved subgrid scale processes (Berner et al. 2017), some improvement in the representation of internal variability are found. Specifically: MJO (Wang et al. 2016), ENSO variability (Christensen et al. 2017; Yang et al. 2019), precipitation variability (Davini et al. 2017; Watson et al. 2017). More recently Vitale et al. 2020 found that Stochastic Physics improves the simulation of Tropical Cyclones, in particular the seasonal cycle of the TC frequency. (Berner, J., Achatz, U., Batté, L., Bengtsson, L., Cámara, A. de la, Christensen, H. M., et al. (2017). Stochastic Parameterization: Toward a New View of Weather and Climate Models. Bull. Am. Meteorol. Soc. 98, 565–588. doi:10.1175/BAMS-D-15-00268.1) (Christensen, H. M., Berner, J., Coleman, D. R. B., and Palmer, T. N. (2017). Stochastic parameterization and El Niño- southern oscillation. J. Clim. 30, 17–38. doi:10.1175/JCLI-D-16-0122.1.) (Davini, P., Von Hardenberg, J., Corti, S., Christensen, H. M., Juricke, S., Subramanian, A., et al. (2017). Climate SPHINX: Evaluating the impact of resolution and stochastic physics parameterisations in the EC-Earth global climate model. Geosci. Model Dev. 10, 1383–1402. doi:10.5194/gmd-10-1383-2017.) (Vidale and Co-authors 2020 under submission to the J. of Climate - Impact of stochastic physics and model resolution on the simulation of Tropical Cyclones in climate GCMs) [Corti Susanna, Italy]	Noted.
76777	46	3	46	3	Maybe you can add something along these lines on the comparison between high resolution GCMs and RCMs: Demory et al. (2020) show that EUR-44 (50 km resolution) GCM-driven CORDEX simulations tend to be too wet over Europe compared to observations. Increasing the resolution from EUR-44 (50 km) to EUR-11 (12 km) show an improvement in the spatial distribution of precipitation but not in the mean. PRIMAVERA GCMs (25-50 km resolution) generally show significant improvements compared to EUR-44 and EUR-11 for intense precipitation and for winter and spring wet biases in Central and Eastern Europe. For weak and moderate precipitation, PRIMAVERA distributions are generally within the range of EUR-11 and EUR-44 RCMs. Demory, M.-E., Berthou, S., Sørland, S. L., Roberts, M. J., Beyerle, U., Seddon, J., Haarsma, R., Schär, C., Christensen, O. B., Fealy, R., Fernandez, J., Nikulin, G., Peano, D., Putrasahan, D., Roberts, C. D., Steger, C., Teichmann, C., and Vautard, R.: Can high-resolution GCMs reach the level of information provided by 12–50 km CORDEX RCMs in terms of daily precipitation distribution?, Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2019-370, in review, 2020. [Ségolène Berthou, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The reference has been introduced in the discussion of the GCMs performance.
22889	46	5	46	10	I believe chapter 3 undertook a very similar assessment. These should be cross-checked and possibly discussion is required to resolve this duplication of assessment (in this case chapter 10 may be the better place to undertake the substantive assessment and chapter 3 would point to it?) [Peter Thorne, Ireland]	Accepted. The sentence has been revised in the light of the statements made in Chapters 3 and 8 and moved to a consolidated new section on overall model performance at the regional scale.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65331	46	6			could you give indications of the spatial and temporal scales for which GCMs give useful regional information. Example : Daily / 1000 km. It may be usefull to speeak here about effective resolution of GCMs (see Klaver et al. 2020, https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/asl.952) [SAMUEL SOMOT, France]	Taken into account. Usefulness depends on the specific application (as described in section 10.5), which requires different variables and data frequencies, and hence is an aspect too vast to be described here. As for effective resolution, this has been added to the paragraph with the corresponding reference.
65329	46	7		9	This statement is quite strong knowing that a large number of systematic biases are not improved by resolution. I feel that the phrasing of the statement is too much positive for model resolution. Reassess [SAMUEL SOMOT, France]	Noted. The statement is applicable to "a number of systematic errors" and makes clear that many others are not improved with global resolution.
31409	46	11	46	38	It is quite unfortunate that this section 10.3.3.3.2 hardly at all refers to original literature. This is very different from the preceding section on GCMs and also from the next section, on statistical downscaling. It would be useful to provide reasonable amount of substance for the reader and to enable the reader to readily access the literature behind the assessment. [Markku Rummukainen, Sweden]	Taken into account; the aim of this subsection has been re-designed. The subsection is placed now at the end of section 10.3.3, instead of the beginning. By so doing, the general assessment of RCMs performances (and the relative assessment statement) is based on what has been assessed (including all the relevant literature) in the previous subsections, and other chapters (Ch 11 for extremes, the atlas for mean climatology). In addition, some relevant references have been added to this section.
18363	46	11	46	38	Section 10.3.3.3.2 RCMs: one common issue with dynamic climate downscaling using RCMs is that their lateral forcing is derived from one single model run, and thus it is not representative of the ensemble mean of the CMIP models for the forced response AND it contains substantial internal changes that complicate the interpretation of the downscaled changes. These issues are discussed by Dai et al. (2017), who proposed a new to method to construct a representative lateral forcing for RCMs. Ref cited: . Dai, A., R.M. Rasmussen, K. Ikeda, and C. Liu, 2017: A new approach to construct representative future forcing data for dynamic downscaling. Climate Dynamics, DOI: 10.1007/s00382-017-3708-8. [Aiguo Dai, United States of America]	Noted: however the remark does not particularly fit the aim of this section. Ensembles (and their construction) are discussed in section 10.3.4.4
125675	46	11	46	38	It is known that upscale feedback from processes unresolved by a GCM can change the GCM simulated large-scale circulation patterns both locally and in remote regions. Without these feedbacks, GCM projections of regional climate are inherently biased and such biases in large-scale will propagate to projections made with an RCM which are "one-way" nesting from GCMs. Given these, comparison of RCM simulations with those from a modeling framework that allows "two-way" interaction between coarse and fine scale processes (e.g., a GCM with regional refinement capability such as MPAS) is crucial for establishing the robustness and usefulness of dynamical downscaling. Unfortunately such comparisions are not discussed adequately in this chapter. [Trigg Talley, United States of America]	Noted: literature on the influence of small scales on the large ones is limited. Explicit comparison of high resolution GCMs (highresMIP) with RCMs is presented in Figure 10.6 and 10.6.4 (Mediterranean case study).
83645	46	13	46	15	I am wondering whether Zanna is the right reference here (ocean modelling!). Instead of “upscale energy cascade” it would be better to speak about scale interaction. The first sentence is also not grammatically correct. In the next sentence, the wording “small-scale interactions” is not proper. The problem is not primarily about interactions within small-scale processes. The problem is that important processes are taking place at unresolved scales. This leads (1) to unresolved subscale variability which is, however, important (for impacts, but also in oder to make a statement at all, for example for snow cover in mountains), and (2) because of nonlinear scale interaction, errors appear also at larger scales which are hard to avoid through parameterisation because of the complex nature of the smaller-scale process (not necessarily small scale - a GCM will not recolve meso-gamma, beta as well). [Petra Seibert, Austria]	Accepted; the sentence has been rephrased (now in Section 10.3.310) and the reference removed.
31411	46	14	46	15	Suggest a shorter meaning: "Unresolved small-scale interactions and local feedbacks result in a degradation of the model performance compared to models with higher resolution." [Markku Rummukainen, Sweden]	Accepted; the sentence has been rephrased
31413	46	17	46	19	The "usually" and "some studies" may be too categorical, considering the research by and since AR5. The literature is hardly predominantly on basic temperature and precipitation climatologies". [Markku Rummukainen, Sweden]	Not applicable. Sentence has been removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23823	46	17	46	20	This parts builds on a too simple idea on how RCM evaluation is done. “some studies” on line 18 is a gross underestimation of the modelling efforts. There is also a lack of references. I Suggest the following: … at an acceptable computational cost, the so called “added value” (Giorgi and Mearns, 1991; Rummukainen, 2010). There is, however no clear definition of this as it depends on amongst others: region, spatial and temporal scale, processes and variables (Di Luca et al. 2015; Rockel 2015; Rummukainen 2016). Performance assessment focuses not only on mean values, but also trends and extremes (Rummukainen, 2010; Giorgi, 2019). Specific studies do also investigate the ability of RCMs to correctly reproduce processes and phenomena (Sections 10.3.3.4 to 10.3.3.7). Giorgi F, Mearns LO (1991) Approaches to the simulation of regional climate change: a review. Rev Geophys 29:191. https://doi. org/10.1029/90RG02636 Rummukainen, M.: State-of-the-art with regional climate models, WIREs Clim. Change, 1, 82–96, 2010. Di Luca A, de Elía R, Laprise R (2015) Challenges in the quest for added value of regional climate dynamical downscaling. Curr Clim Change Rep 1:10–21. https ：//doi.org/10.1007/s40641-015-0003-9 Rockel B (2015) The regional downscaling approach: a brief history and recent advances. Curr Clim Change Rep 1:22–29. https://doi.org/10.1007/s40641-014-0001-3 Giorgi, F. (2019). Thirty years of regional climate modeling: Where are we and where are we going next? Journal of Geophysical Research: Atmospheres, 124, 5696–5723. https://doi.org/10.1029/2018JD030094 Rummukainen M (2016) Added value in regional climate modeling. Wiley Interdiscip Rev Clim Change 7:145–159. https://doi.org/10.1002/wcc.378 [Strandberg Gustav, Sweden]	Taken into account; the aim of this subsection has been re-designed. The subsection is placed now at the end of section 10.3.3, instead of the beginning. By so doing, the general assessment of RCMs performances (and the relative assessment statement) is based on what has been assessed (including all the relevant literature) in the previous subsections, and other chapters (Ch 11 for extremes, the atlas for mean climatology). In addition, some relevant references have been added to this section.
23825	46	22	46	27	This part is a bit too simple, and lacks references. I suggest: The performance assessment of RCMs is carried out by evaluating simulations of the climate of the recent past or the current climate with boundary forcings provided by reanalysis products in a comparison with the best observations available, in so called “perfect-boundary experiments” (Giorgi, 2019). There are also examples of when RCMs are evaluated in simulations of paleo climate (Ludwig et al., 2018) Giorgi, F. (2019). Thirty years of regional climate modeling: Where are we and where are we going next? Journal of Geophysical Research: Atmospheres, 124, 5696– 5723. https://doi.org/10.1029/2018JD030094 Ludwig P, Gómez-Navarro JJ, Pinto JG, Raible CC, Wagner S, Zorita E. Perspectives of regional paleoclimate modeling. Ann N Y Acad Sci. 2019;1436(1):54-69. doi:10.1111/nyas.13865 [Strandberg Gustav, Sweden]	Not applicable. Sentence has been removed
23827	46	29	46	34	This part is a bit too simple, and lacks references. I suggest: When RCMs are driven by GCMs much of the uncertainty in the RCM simulation is introduced by the GCM; its biases and its atmospheric state. Sometimes the bias in a RCM is completely explained by the GCM, sometimes the RCM (Kjellström et al., 2018; Sörland et al., 2018; Christensen and Kjellström, 2020). Kjellström, E., Nikulin, G., Strandberg, G., Christensen, O. B., Jacob, D., Keuler, K., Lenderink, G., van Meijgaard, E., Schär, C., Somot, S., Sørland, S. L., Teichmann, C., and Vautard, R.: European climate change at global mean temperature increases of 1.5 and 2 °C above pre-industrial conditions as simulated by the EURO-CORDEX regional climate models, Earth Syst. Dynam., 9, 459–478, https://doi.org/10.5194/esd-9-459-2018, 2018. Christensen, O.B., Kjellström, E. Partitioning uncertainty components of mean climate and climate change in a large ensemble of European regional climate model projections. Clim Dyn 54, 4293–4308 (2020). https://doi.org/10.1007/s00382-020-05229-y [Strandberg Gustav, Sweden]	Accepted, sentence has been rephrased and references added. The paragraph has been moved to Section 10.3.3.10
42731	46	29			I’m concerned that the main point being made in this paragraph is being under-stated. For example, if a particular GCM has a very poor simulation of a regional monsoon circulation, is there any point in downscaling the GCM to produce regional precipitation projections? If the RCM domain is large it may improve the monsoon circulation but otherwise this seems a somewhat pointless exercise. Of course, there are multiple examples of the above principle. I’d suggest strengthening the ‘rubbish in, rubbish out’ message. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Noted; we specifically claim (in 10.3.3.10) that "RCMs are typically not able to mitigate GCM biases in large-scale dynamical processes, if such biases are substantial, and if the corresponding large-scale processes are important drivers of regional climate, downscaling is questionable (Section 10.3.3.3). "
66561	46	31	46	34	Another example showing added value in RCMs over GCMs over Europe is by Sørland, S., Lüthi, D., Schär, C. and Kjellström, E., 2018. Bias patterns and climate change signals in GCM-RCM model chains. Environ. Res. Lett., 13, 074017, DOI: 10.1088/1748-9326/aacc77. [Kjellström Erik, Sweden]	Noted: added value is discussed in 10.3.3.2. The suggested reference is already in the reference list
65335	46	36	46	38	Overall I feel that sections 10.3.3.3.1 and 10.3.3.3.2 are relatively weak and poor in references despite strong statements. I understand that those sections are completed by the coming 10.3.3.x but are they really usefull ? Do they add something ? Please reconsider. [SAMUEL SOMOT, France]	Taken into account; the aim of this subsection has been re-designed. The subsection is placed now at the end of section 10.3.3, instead of the beginning. By so doing, the general assessment of RCMs performances (and the relative assessment statement) is based on what has been assessed (including all the relevant literature) in the previous subsections, and other chapters (Ch 11 for extremes, the atlas for mean climatology). In addition, some relevant references have been added to this section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106597	46	36	46	38	Please add evidence to support these statements, either references or links to relevant Atlas sections. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, relevant links to the Atlas have been added in the relevant text providing evidence to the statement, now in Section 10.3.3.10
38545	46	36	46	38	This statement should have references [robert vautard, France]	Accepted: Relevant references have been added in the text preceding and providing evidence to the statement. The entire section has been moved to 10.3.3.10
20669	46	36	46	38	When considering regions with heterogeneous surface characteristics, the spatial resolution of the models is bound to have an effect. Then it becomes relevant to compare the performance of RCM with the one of a GCM enjoying the same spatial resolution. Has this been done and reported? Is there still a significant "added value" of the RCM? [philippe waldteufel, France]	Noted: added value is discussed in 10.3.3.2 Comparison of high resolution GCMs (highresMIP) with RCMs is presented in Figure 10.5 and 10.6.4 (Mediterranean case study). However, added value is usually defined as the improvement of performances in a RCM when compared to a lower resolution GCM. Over regions where horizontal resolution is fundamental to represent the main drivers of the local climate (e.g. complex terrain) it is expected that GCMs and RCMs with a similar resolution (and physical parameterization) would perform similarly.
112633	46	36	46	38	Should it be mentioned here that RCMs are commonly tuned or calibrated to simulate the climate of the region of interest? This, together with its usually higher spatial resolution, give RCMs the potential to add value to GCMs that cannot be tuned over a specific region. [Marie-Estelle Demory, Switzerland]	Noted: the issue is mentioned in 10.3.3.9
66559	46	36	46	48	This part is very thin. There are ample of references showing the benefit of RCMs over GCMs on the regional scale. See for instance reviews by Giorgi, F. (2019). Thirty years of regional climate modeling: Where are we and where are we going next? Journal of Geophysical Research: Atmospheres, 124, 5696– 5723. https://doi.org/10.1029/2018JD030094; Rummukainen M (2016) Added value in regional climate modeling. Wiley Interdiscip Rev Clim Change 7:145–159. https://doi.org/10.1002/wcc.378. In addition to the examples of complex orography and heterogeneous surfaces also areas with smaller seas that are poorly represented by GCMs may be improved in coupled regional models as being discussed in Kjellström, E. and Christensen, O.B., 2020. Regional Climate Modelling for the Baltic Sea Region. In: von Storch, H., (ed.). Climate of the Baltic Sea region. Oxford Research Encyclopedia of Climate Science. Oxford University Press USA. DOI: 10.1093/acrefore/9780190228620.013.700 [Kjellström Erik, Sweden]	Accepted; references added in the relevant paragraph now in 10.3.3.10
65333	46	36			Again a very strong and key statement. I agree with it but it is strange not to see any supporting reference in this section before reaching the statement. Is it well placed ? As for the GCM above, could you give indications of the spatial and temporal scales for which RCMs add value to GCM. I'm not aware of specific publications concerning the effective resolution of RCMs but they may be relevant here. [SAMUEL SOMOT, France]	Taken into account; the aim of this subsection has been re-designed. The subsection is placed now at the end of section 10.3.3, instead of the beginning. By so doing, the general assessment of RCMs performances (and the relative assessment statement) is based on what has been assessed (including all the relevant literature) in the previous subsections, and other chapters (Ch 11 for extremes, the atlas for mean climatology). In addition, some relevant references have been added to this section.
23829	46	38	46	38	Add references. For example: Giorgi, F. (2019). Thirty years of regional climate modeling: Where are we and where are we going next? Journal of Geophysical Research: Atmospheres, 124, 5696– 5723. https://doi.org/10.1029/2018JD030094 Rummukainen M (2016) Added value in regional climate modeling. Wiley Interdiscip Rev Clim Change 7:145–159. https://doi.org/10.1002/wcc.378 [Strandberg Gustav, Sweden]	Accepted, sentence has been rephrased and references added. The paragraph has been moved to Section 10.3.3.10
96101	46	43	46	43	Table 10.2: Do these performance indicators for "local weather" hold for each and every region, including e.g. the Mediterranean and mountainous areas? Please specify [Nicole Wilke, Germany]	Noted. The performance of statistical downscaling and bias adjustment depends strongly on the quality of the simulated predictors, as discussed in the main text. The table discusses explicitly only the performance of the methods, given skilful predictors. This model performance holds much more general than the climate model performance in simulating predictors. This is the topic of basically the whole Section 10.3.3. Nevertheless, the point has been made much clearer now in the text and caption by adding a reference to all relevant subsections.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
69239	46	43	47	2	Changing the symbols used in Table 10.2 is suggested. Here, "+" means "should work reasonably well based on empirical evidence and/or expert judgement", and "o" implies "problems may arise depending on the specific context". "o" is usually used in a positive sense. It would be helpful if "o" represented "should work reasonably well based on empirical evidence and/or expert judgement" and some other symbol was used for "problems may arise depending on the specific context". [Kaoru Magosaki, Japan]	Accepted. Final layout will be decided by copy editors.
110169	46	43			It is not clear how to read Table 10.2. For example, let's consider the row "Precipitations, marginal, Extremes" crossed with the four columns for "Bias adjustment" (which are "Additive/scaling", "QM empirical", "QM parametric", and "QM extremes"). Then what kind of information (assessment) is contained in QM columns other than "QM extremes", as the row is about precipitation extremes ? Moreover, it is surprising that for precipitation extremes the QM-related columns contain either "+" or "o" evaluations, considering a relatively widespread opinion among bias adjustment practitionners that most QM algorithms generate "some extreme values" with no clear meaning. Possibly, no one knows what is the value of the extremes generated by QM, beyond some return period. [Patrick Grenier, Canada]	Noted. The labels and table caption have been adjusted. Note that the statements about extremes explicitly (see caption) refer to moderate extremes occurring at least once every 20 years. We agree that we do not know much about the performance of these methods for extrapolating to unobserved extremes.
59203	47	1	47	1	Table 10.2 symbols looks confusing. A legend should be inserted close to the table. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Final layout will be decided by copy editors.
1357	47	1	47	1	The symbols in table are not explained ('-','o'.'+'). Replace 'Perfect Prog' with empirical-statistical downscaling (ESD) since Perfect Prog is just a subset of the downscaling strategies. This table only takes into account perspective #1 on evaluation, the typical cross-validation of models before involving the GCMs. The important evaluation in practical sense should be for the results after the application of the methods with the imperfect GCMs. Also, it should be noted in the main text that 'inflation' is not recommended (von Storch, 1999; DOI: 10.1175/1520-0442(1999)012<3505:OTUOII>2.0.CO;2) because it adds variance to a predictor that the regression analysis tells us does not explain much of the variance (the solution is to add a noise term -The use of inflation should be a thing of the past). Also, ESD can be tailored to predict quantities such as variance (e.g. downscale σ over seasonal samples), aggregated statistics such as wet-day frequency (usually quite skillfull) and wet-day mean precipitation (more tricky), spell duration (which is geometrically distributed), number of events (Poisson distributed) as long as they have a systematic dependencies upon the large-scale predictors (this is an example of downscaling 'climate', i.e. the parameters of their pdfs). The hard way is to downscale the local variable on a day-by-day basis and then estimate these statistics when it can be done in a more statistically sound way. [Rasmus Benestad, Norway]	Noted. The draft formatting has moved the caption to the previous page. Perfect prog is the correct term, as all approaches discussed here (including common EOFs frequently put forward as a hybrid approach in other comments by the reviewer) fall into this framework. The table explicitly is not based on a "weather-like" forecast evaluation, as stated in the caption. However, it explicitly is limited to the performance of the downscaling methods irrespective of the driving models, to avoid muddling the performance of both. The performance of the driving models is discussed separately across the Section. This point has been made clearer in the Caption. A statement on inflation has been added. We acknowledge that complex statistics can be downscaled directly, but first, including these would substantially increase the length of the table, and second, one may argue about the soundness of this approach for climate change applications.
68953	47	1	47	1	Table 2 caption needs some explanation of what "QM extremes" means. (It's not obvious even to a specialist in the subject.) [Seth McGinnis, United States of America]	Taken into account. The labels and caption have been revised.
125677	47	6	48	36	Due to the limited number of regions evaluated and a lack of proper stratification by seasons and climate/weather regimes, Table 10.2 has very limited value despite it appearing quite comprehensive. The discussion that follows does not offer enough physical insights about performances of statistical downscaling since it fails to connect evaluation metrics with specific regions and weather/climate regimes. [Trigg Talley, United States of America]	Noted. The table assess the performance of statistical methods only, which does depend on region and season much less than the overall performance, which is very much dominated by the performance of the driving climate models at simulating predictors. The climate model performance is assessed throughout Section 3.3. To accommodate for residual seasonal and regional variations in performance, we deliberately choose only a simple classification of three classes.
1359	48	1	48	1	Limit this only to a subset of the ESD strategies? [Rasmus Benestad, Norway]	Noted. The assessment covers all major approaches, including the approaches used by the reviewer (which, e.g., participated in the VALUE intercomparison experiment under the label perfect prog). Only hybrid approaches (new Section 10.3.1.4.5) have not been included, as they have not been systematically intercompared.
1361	48	7	48	7	By using extended EOFs it is possible to capture a more realistic temporal dependency (e.g. Benestad, 2009; DOI: 10.1007/s00704-009-0158-1): greater 1-day persistence. If it is important to capture evolution (e.g. a moving storm), then it may make sense using extended EOFs with applied to segments of more than a couple of days and on an hourly basis. [Rasmus Benestad, Norway]	Noted. In this assessment, due to space constraints, we do not get into the detail of individual implementations of broader types of methods.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1363	48	7	48	10	The spatial consistency is also preserved when PCA is used to represent the predictands (Benestad et al., 2015; DOI: 10.3402/tellusa.v67.28326). Furthermore, the use of PCA improves the signal-to-noise ratio since it emphasises the coherent variability over the sites that is more closely linked to the large-scale conditions. [Rasmus Benestad, Norway]	Noted. In this assessment, due to space constraints, we do not get into the detail of individual implementations of broader types of methods.
51547	48	10	48	12	Statistical downscaling methods in general, and not just analogue methods as currently stated in the text, they are inherently limited in representing climate change. They assume regression relationships built on observed present-day variability apply (and in some cases can be extrapolated) to the future climate. This is an important limitation of statistical downscaling approaches that should also be stated in the text. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Such limitations apply to all types of models, including dynamical models. These limitations are discussed explicitly in Section 10.3.3.10 (SOD version). The limitations of the analog method are even more fundamental and therefore mentioned here as well.
66563	48	14	48	36	Even if it is implicit in any discussion about bias adjustment I think it needs to be spelled out here that the adjustment in itself will not make things better unless observations are reliable. There are numerous examples of bias correction against precipitation data not corrected for undercatch etc. [Kjellström Erik, Sweden]	Noted. This applies to all statistical techniques and is explained in Section 10.2
4289	48	15	48	15	“are good to” → “work well to” [Isla Simpson, United States of America]	Noted. Text has been revised, but the formulation "good" is still used.
21173	48	26	48	26	I suggest to add a reference to show that though multivariate methods can adjust all statstical aspects of multivariate distribution, they come with larger uncertainties. [Faranak Tootoonchi, Sweden]	Noted. The statement "larger uncertainties" is quite vague. Related issues are now discussed in the Cross Chapter Box 10.2
78247	48	26	48	27	I would like to quote the following papers comparing multiple BC methods in multivariable (temperature and precipitation). Ishizaki, N. et al. (2020):Evaluation of two bias-correction methods for gridded climate scenarios over Japan. SOLA (online letter of Meteorological Society of Japan), https://doi.org/10.2151/sola.2020-014 *key improvement / the key evaluation outcome* A choice of appropriate bias-correction (BC) method or the comparison of different BC methods is encouraged for local mitigation and adaptation planning in addition to the selection of different GCMs and RCPs. [Motoki NISHIMORI, Japan]	Not applicable. The text has been deleted here. A concise version has been added to the Cross-Chapter Box 10.2, where space was seriously limited. Given that the reference does not add further insight compared to the cited papers, we could not include it.
21175	48	34	48	34	The high cofidence part: if it is abuot bias ocrrection generally, I think it is better to change marginal distribution to something more general. If this sentence is specific to multivariate bias correction, then it is better to add it is talking about multivariate mode. [Faranak Tootoonchi, Sweden]	Noted. The statement, as is clear from the text, refers to marginal distributions and any type of bias adjustment method. The new structure makes this point even clearer.
15657	48	36	48	36	A good example of the need to be cognizant of intervariable inconsistency issues is the rain/snow partitioning, which plays a critical role in the mountain environment. The Verfaillie et al. (2017, https://doi.org/10.5194/gmd-10-4257-2017) pays specific attention to this issue, which implies multiple adjustment steps (see publication for details). [Samuel Morin, France]	Noted. The text has been removed from this section. But the reference has been included in the description of BA approaches (Section 10.3.1.3)
51549	48	38	48	47	Weather generators driven by change factors from coarse resolution climate models can provide added detail at fine spatial and temporal scales. However, they do not represent local climate change processes and so may miss key aspects of the future change at local and hourly scales. For example, they will not capture the enhancement of extreme precipitation increases with warming due to local storm feedbacks (Lenderink G. and E. van Meijgaard, 2008: Increase in hourly precipitation extremes beyond expectations from temperature changes, Nature Geosci., 1, 511-514). It would be good to acknowledge this key deficiency of weather generators in the text. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. A discussion of this issue has been added in Section 10.3.3.9 (SOD: 10.3.3.10).
112055	48	50	48	50	The following reference could be relevant here: https://doi.org/10.1007/s10584-016-1683-4 [jose manuel gutierrez, Spain]	Accepted. Reference has been considered in the new Section 10.3.3.10.
66565	48	50	49	6	It is unclear if this section is about representing the past climate or if it is about projections or both. It starts with talking about "performance" giving the impression that it is only about the past climate but then "added value" is used which may also involve projections and information about future climate. As stated previously in the chapter simpler statistical methods cannot add value to projections. There should also be a sentence or two about internal consistency between different fields as a dynamical model produces internally consistent model states for each time step while statistical methods adjusting variables against different observational data may result in data sets not being internally consistent. It may not be a problem unless these fields need to be used simultaneously as in hydrological models. [Kjellström Erik, Sweden]	Not applicable. The section has been removed, the material included elsewhere in a way accounting for the reviewers concern.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1365	48	50	49	6	It is also important to compare the projected change from RCMs and ESD, and a comparison over Poland suggests that the two approaches give similar results for temperature and less so for precipitation (Mezghani et al., 2019; DOI: 10.1175/JAMC-D-18-0179.1). However, a more crucial aspect is the fact that most RCMs tend to involve small ensemble, analogous to small statistical samples and subject to 'the law of small numbers'. A small ensemble (of GCMs) with pronounced random regional internal variability, such as Euro-CORDEX, will have severe limitiation due to substantial random sampling fluctuations. It is also important to emphasise the fact that RCMs and ESD draw on information from different and independent sources: coded equation representing the laws of physics and information embedded in the past observations respectively. When ESD and RCMs give similar results, then we can have greater confidence in the projections. However, this only applies to projections involving large multi-model ensembles. The Euro-CORDEX ensemble tends to represent the modest changes compared to the CMIP5 GCMs (Mezghani et al., 2019; DOI: 10.1175/JAMC-D-18-0179.1). [Rasmus Benestad, Norway]	Noted. The section is on performance in present climate, not projections. The issue of internal variability is discussed in detail in Section 10.3.4. A synthesis of statistical and dynamical approaches can be found in Section 10.3.10.
65337	48	50			You may want to assess Vrac et al. 2012 for this section : Vrac M., Drobinski P., Merlo A., Herrmann M., Lavaysse C., Li L., Somot S. (2012) Dynamical and statistical downscaling of the French Mediterranean climate: uncertainty assessment. Nat. Hazards Earth Syst. Sci., 12, 2769-2784, doi:10.5194/nhess-12-2769-2012 [SAMUEL SOMOT, France]	Given that this is a pre-AR5 reference, we prefer to cite the more recent reference by a similar author team (Vaittinada Ayar et al 2016).
110649	48	52	48	55	It could be worth to mention that improvement of SDMs over RCMs in basic statistics is expected by construction and simply by correcting the mean, RCMs are as good as SDMs in PDF statistics and spells: Casanueva, A., Herrera, S., Fernández, J. et al. Towards a fair comparison of statistical and dynamical downscaling in the framework of the EURO-CORDEX initiative. Climatic Change 137, 411–426 (2016). https://doi.org/10.1007/s10584-016-1683-4 [Ana Casanueva, Spain]	Accepted. The reference has been considered in the rewritten sub-section 10.3.3.10 but its inclusion is subject to be accepted in the corrigenda process.
51551	48	52	48	55	The relative performance of dynamical compared to statistical downscaling is not fairly represented here. In particular RCMs are superior to statistical methods for understanding future regional climate change, since they are based on realistic physical representation of relevant climate processes, rather than relying on statistical relationships based on past observations. It is important that this point is acknowledged in the text. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The section is on performance in present climate, not projections. In the new version, the section is moved, and the point made by the reviewer has been included.
110573	48	53	48	55	While somewhat "alluded" to with the language here - calibrated models will outperform RCMs and GCMs for most statistical metrics especailly if the model is calibrated specifically for that metric (e.g. calibrating for extremes rather than the mean etc.). Bias relative to observations cannot be our only metric for how well statistical methods perform - the real question for these statistical methods is why should we have confidence in their future projections. Its offten assumed that a lcak of bias makes a dataset "better" - but while it may make it more useable, it does not necesearily improve its "fiedlity" for climate change studiesl This idea seems to be missing in this discussion. [Rachel McCrary, United States of America]	Noted. The section is on performance in present climate, not projections. In the new version, the section is moved, and the point made by the reviewer has been included.
22891	49	3	49	6	This degree of specificity married to dependency upon single studies feels unwarranted and unhelpful. The paragraph would be better without the use of these examples. Either that or they should be explicitly stated as being examples and ideally each be backed by more than one study. [Peter Thorne, Ireland]	Noted. The Maraun et al. reference summarises an intercomparison of some 40 different methods within the VALUE initiative, published in a series of papers (cited in other places of this report, but here, for space reasons, only the synthesis is cited).
84727	49	13	49	13	it would be useful to specify the section (not only the chapter) [Annalisa Cherchi, Italy]	Noted. Chapter 11 discusses this issue across several subsections (mechanisms and drivers for different extreme events). We therefore refer to the Chapter as a whole.
55491	49	15	49	15	How high resolution GCM can represent large scale circulation related to regional precipitation and temperature in Southern South America in studied in Zazulie et al (doi:10.1007/s00382-017-3560-x) [Matilde Rusticucci, Argentina]	Noted. The introduction is not intended to present individual papers.
22893	49	15	49	16	This was assessed in chapter 3. Where is the cross-link and why are you not using their assessment here as the starting point explicitly but rather starting from your own characterisation which may differ from theirs? [Peter Thorne, Ireland]	Taken into account. A reference to Chapter 3 has been added, including a summary of their assessment.
109793	49	16	49	17	It's not clear to me than *any* statement will be highly uncertain. For example, we have confidence in a poleward shift of the jet, even though models have large biases in the many aspects of the jet, such as position, strength, etc. [Flavio Lehner, Switzerland]	Noted. These statements are explicitly limited to regional climate change. Even if we know consistent shifts in the jet, the location bias will introduce huge uncertainties in the projections of regional surface climate.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66567	49	18	49	18	Here, "cannot reduce" would depend on the scale and magnitude of the error. As shown by Sørland, S., Lüthi, D., Schär, C. and Kjellström, E., 2018. Bias patterns and climate change signals in GCM-RCM model chains. Environ. Res. Lett., 13, 074017, DOI: 10.1088/1748-9326/aacc77 reductions also on larger scales can be a result from RCM simulations. I would rephrase into "can partly reduce" adding a notion on scale and magnitude. [Kjellström Erik, Sweden]	Noted. But the mentioned paper does look into surface variables, which are typically tuned. The authors do not look into circulation biases, and it therefore cannot be concluded that the reduction in biases of surface variables stems from an improvement in the circulation (which is discussed here).
68955	49	19	49	19	Do not hyphenate "large domain" [Seth McGinnis, United States of America]	Accepted.
125679	49	19	49	19	Change "RCMs run" to "RCMs" [Trigg Talley, United States of America]	Not applicable. Text has been rephrased.
10983	49	22	49	23	These two sentences don't seem to describe the folowing parts very well. Especially for the blocking, it's not obvious from the text that the RCMs do add value. [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentences have been revised.
73825	49	26	50	50	More recent studies using CMIP6 could be added here as the assessment is now mainly based on CMIP5 (e.g. https://doi.org/10.1175/JCLI-D-19-0928.1). A summary statement should be provided at the end of the subsection. [Rondrotiana Barimalala, South Africa]	Noted. Much of the assessment material about blocking and storm-tracks has been moved to chapter 3 for consistency.
65347	49	26			I would cut this section in two to facilitate the reading. One for blocking and one for cyclones [SAMUEL SOMOT, France]	Taken into account. Much of the material of this section has been moved to chapter 3 for the FGD. The section now focuses more on links between circulation biases and surface biases.
4183	49	27	49	31	This section mentioned the basic characteristics of atmosphere blocking and concomitant synoptic wave behaviour, such as embranchment. Previous some of observed analysis, theoritical and model researches revealed these phemonena and I think they're desirable referred. (Shutts,1983,"The propagation of eddies in diffluent jetstreams - eddy vorticity forcing of blocking flow-fields"; Nakamura and Wallace, 1992,"Synoptic behavior of baroclinic eddies during the blocking onset"; Luo et al. 2014, "A nonlinear multiscale interaction model for atmospheric blocking: The eddy-blocking matching mechanism".) [Wenqi Zhang, China]	Rejected. We have decided to give priority to the more recent papers as the old ones have already been assessed in previous reports.
22895	49	27	49	55	This is duplicative of a similar assessment which was performed in chapter 3. These need to be reconciled with one another and ideally the duplication should be removed. Chapter 10 should point to chapter 3 for the global circulation assessment using ESMs and then go on to discuss the regional aspects? [Peter Thorne, Ireland]	Accepted. Much of the chapter 10 assessment on blocking and storm-tracks has been moved and merged in chapter 3.
79185	49	27	50	36	CMIP model performance in blocking and storm track simulations is assessed in Chapter 3 Section 3.3.3.3. Coordination is needed. [Yu Kosaka, Japan]	Accepted. Much of the chapter 10 assessment of blocking and storm-tracks has been moved and merged in chapter 3.
65339	49	27		55	I think that Fig 10.7 is not cited as much as it could be in this paragraph, in particular line 51 whereas it is very relevant [SAMUEL SOMOT, France]	Taken into account. figure 10.7 has been referenced in the paragraph and the text has been revised due to moving/merging of some material in chapter 3.
10985	49	36	49	37	Is there a reference for the claim that short lived blocks dominate the underestimate? [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Much of the blocking assessment has been moved to chapter 3. The specific sentence has not been kept in chapter 3 nor chapter 10.
4291	49	40	49	40	I have a cursory assessment of blocking in CMIP6 models in a paper that is submitted to JGR-Atmospheres, I will send this to the authors in case this is a useful citation here as this bias does persist in CMIP6 but also quite a lot of it is simply due to a mean state bias as opposed to the variability being wrong. [Isla Simpson, United States of America]	Not Applicable: thanks for the reference, but much of the blocking assessment has been moved to chapter 3.
4293	49	46	49	46	I think this is also related to the overly zonal nature of the jet so that it is too strong in this sector e.g., Ummenhofer et al (2013), J. Clim., 26, 8476–8494 [Isla Simpson, United States of America]	Not applicable. Thanks for the reference, but much of the blocking assessment has been moved to chapter 3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
93659	49	50	49	52	The sentence "In general, blocking… SST fronts" sounds a bit odd. I'd suggest rephrase: In general, blocking […] resolution (Anstey et al., 2013, Davini and D'Andrea, 2016, Schiemann et al., 2017), underrepresentation of low frequency variabiity (Athanasiadis et al., 2014), mean state biases. Most common model biases include errors in the parameterization of orographic effetcs, location of the jet stream and misrepresentation of the Gulf Stream SST front (O'Reilly et al., 2016; Pithan et al., 2016). ref. Athanasiadis, P. J., Bellucci, A., Hermanson, L., Scaife, A. A., MacLachlan, C., Arribas, A., Materia S., Borrelli A. & Gualdi, S. (2014). The representation of atmospheric blocking and the associated low-frequency variability in two seasonal prediction systems. Journal of Climate, 27(24), 9082-9100. [Stefano Materia, Italy]	Not Applicable. Most of the text about blocking has been moved and merged in chapter 3 for the FGD.
125681	49	50	49	52	The list of the causes of biases here is not a complete sentence. [Trigg Talley, United States of America]	Taken into account. Note that the text has been largely revised and shortened due to the moving of much of the blocking material to chapter 3.
100819	49	50	49	55	About the underestimation of blocking frequencies: It should be mentioned that has been found a robust positive correlation between CMIP6 models' horizontal resolution and the decrease in the winter blocking frequencies biases in most of the blocking regions, particularly in the Central Europe sector (see Figure 3 in Davini and D'Andrea 2020 – under revision). Horizontal resolution plays also a role in reducing the bias in blocking average persistence and blocking frequencies in AMIP simulations – e.g. Davini et al 2017 (figure 10), Strommen et al 2019, Schiemann et al. 2017– (Davini P., S. Corti, F. D'Andrea, G. Riviere, J. von Hardenberg 2017, Improved winter European atmospheric blocking frequencies in high-resolution global climate simulations, J. Adv Model Earth Sy. 9, 2615–2634. https://doi.org/10.1002/2017MS001082), (Strommen, K., Mavilia, I., Corti, S., Matsueda, M., Davini, P., von Hardenberg, J., et al. 2019. The sensitivity of Euro-Atlantic regimes to model horizontal resolution. Geophysical Research Letters, 46, 7810–7818. https://doi.org/10.1029/2019GL082843),). (Schiemann, R., Demory, M. E., Shaffrey, L. C., Strachana, J., Vidale, P. L., Mizielinski, M. S., et al. (2017). The resolution sensitivity of Northern Hemisphere blocking in four 25-km atmospheric global circulation models. Journal of Climate, 30(1), 337–358. https://doi. org/10.1175/JCLI-D-16-0100.1) [Corti Susanna, Italy]	Not Applicable. Thanks for the references but much of the blocking assessment has been moved to chapter 3.
10987	49	51	49	55	In addition there are other factors which contribute to blocking biases, eg physics schemes such as convectinon. See the Woollings et al review for example (https://doi.org/10.1007/s40641-018-0108-z) [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text has been largely revised and shortened due to the moving of much of the blocking material to chapter 3.
13589	50	1	50	1	Change Sanchez-Gomez by Sánchez-Gómez [Maria Amparo Martinez Arroyo, Mexico]	Noted. Thanks, but the text related to reference has been removed for the FGD.
4295	50	1	50	1	"(Sanchez-Gomez et al., 2009)" → "Sanchez-Gomez et al., 2009" [Isla Simpson, United States of America]	Noted. Thanks but the text related to the reference has been removed.
125683	50	1	50	6	Are there RCM simulations driven by GCMs? If so, it would be nice to summarize the results here. [Trigg Talley, United States of America]	Noted. As indicated in the text, RCMs are driven by reanalysis in this study (ERA-interim)
43289	50	1			Read "Sanchez-Gomez et al. (2009) show that RCMs reproduce" rather than "(Sanchez-Gomez et al., 2009) show that RCMs reproduce" [Cyriaque Rufin Nguimalet, Central African Republic]	Noted. Thanks but the text related to reference has been removed for the FGD.
125685	50	4	50	6	Does Jury et al. (2018) suggest that the simulation of blocking frequency over Europe is not sensitive to model resolution? The seems to contradict earlier statements and a bit more discussion might be necessary. [Trigg Talley, United States of America]	Noted. Our assessment is that the model performance is not sensitive to resolution because the constraint by the reanalysis boundary conditions dominate over the RCM resolution.
54389	50	11	50	11	Regarding Figure 10.6 - the chart labels a-f are extremely hard to notice. I would get rid of them since they are not mentioned in the figure description [Gabriel Stachura, Poland]	Not Applicable. Figure 10.7 has been completely changed for the FGD.
106525	50	12	50	12	Statement ...and processes like the atmospheric general circulation or large- ...". I recommend replace "or" with "and" [Joseph Mutemi, Kenya]	Noted. The comment does not correspond to the indicated page and line (P50L12) but, rather to P12 L50. However the sentence is clear as it is.
20671	50	15	50	15	What is the "area mean blocking frequency"? [philippe waldteufel, France]	Not applicable. Figure has been changed substantially.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66963	50	20			CMIP6 models vary widely in their ability to reproduce the observed ENSO-related teleconnection that influences Southwest Asia and the Middle East; this appears to be due to both representation of the tropical ENSO signal and the jet structure the modulates the propagation of the response into the region (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]	Noted. Inclusion in FGD pending on relevance and acceptance of the paper (published online the 03 Feb. 2021, so after the official IPCC deadline for papers)
1367	50	21	50	36	Parding et al. (2019; DOI: 10.1175/JAMC-D-17-0348.1) used ESD to downscale the storm track density for the CMIP5 multi-model ensemble RCP4.5 and RCP8.5, based on EOFs of cyclone density. The cyclone density was aggregated from storm tracks, derived from 6-hour instantanteous SLP fields from the ERAINT reanalysis. Two different types of predictors were used, seasonal mean SLP and the 500-hPa height. Both scored high in terms of a cross-validation, but gave different projections for the future. This difference was explained by increased temperature in the air column that affected the future 500-hPa height, hence representing a violation of the sationarity assumption. There was a large spread for the multi-model SLP-based projections, although the median value indicated an increase in the cyclone density over the North-Atlantic and the Norwegian Sea during the winter season. [Rasmus Benestad, Norway]	Noted. The blocking and storm-track assessment has been moved to chapter 3.
22897	50	21	50	36	This is duplicative of a similar assessment which was performed in chapter 3. These need to be reconciled with one another and ideally the duplication should be removed. Chapter 10 should point to chapter 3 for the global circulation assessment using ESMs and then go on to discuss the regional aspects? [Peter Thorne, Ireland]	Accepted. The blocking and storm-tracks assessment related to GCMs has been moved to chapter 3.
65341	50	21			McSweeney et al. 2015 (already cited) would be also relevant here for the storm track evaluation for CMIP5 models and other large-scale evaluation by the way. [SAMUEL SOMOT, France]	Not applicable. Much of the material about blocking GCM performance has been moved and merged in chapter 3.
4297	50	23	50	23	For the storm tracks being too weak, I also have a cursory assessment of this in the CMIP6 models in the above-mentioned paper submitted to JGR. [Isla Simpson, United States of America]	Noted. Thanks for the reference. The blocking and storm-track assessment has been moved to chapter 3.
125687	50	31	50	33	This is primarily true during winter months only. See Simpson and Polvani, 2016 (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016GL067989). [Trigg Talley, United States of America]	Not Applicable. Much of the material about blocking GCM performance has been moved and merged in chapter 3.
4299	50	32	50	32	suggest “leading to” → “associated with” because it’s not clear that the equatorward bias is the ultimate cause of the larger projected poleward shift. It could be that both have a common cause e.g., errors in eddy-mean flow feedbacks or something. [Isla Simpson, United States of America]	Noted. Note that the text has been removed and the full assessment is now in chapter 3
65343	50	33		36	Any reference concerning the potential improvment or not due to higher resolution in GCM ? (no article in HighResMIP?) [SAMUEL SOMOT, France]	Noted. This reference has been added https://doi.org/10.5194/wcd-1-277-2020
66569	50	38	50	38	In some instances RCMs can actually mitigate large-scale biases from GCMs. This is shown for Europe by Sørland, S., Lüthi, D., Schär, C. and Kjellström, E., 2018. Bias patterns and climate change signals in GCM-RCM model chains. Environ. Res. Lett., 13, 074017, DOI: 10.1088/1748-9326/aacc77. I would rephrase into "can in some cases". [Kjellström Erik, Sweden]	Rejected. Subsequent papers have challenged the view that differences in your set of GCM-RCM pairs are GCM biases, rather they suggest they are due to different prescription of aerosol forcings in GCM and RCM.
68957	50	38	50	39	Do not hyphenate "large domains" [Seth McGinnis, United States of America]	Accepted.
65345	50	44			RESM → RCSM : no human component in those model (RCSM : Regional Climate System Model) [SAMUEL SOMOT, France]	Accepted. Text has been changed to refer to RCM with and without air-sea coupling.
125689	50	45	50	46	The authors might want to clarify that air-sea coupling has a rather weak impact on cyclone climatology and intensity aside from regions of intense oceanic eddies and SST fronts (e.g., Kuroshio Extension). [Trigg Talley, United States of America]	Taken into account. The sentence has been rephrased to clearly state that this concerns the Mediterranean region.
73827	50	53	51	32	Please note that Chapter 3 also asseses ENSO teleconnection (Chapter 3, section 3.7.3) [Rondrotiana Barimalala, South Africa]	Taken into account. The text has been revised, references to chapter 3 and Annex VI have also been added to ensure consistency and reduce overlap.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102277	50	53	51	54	The interactions and teleconnections between different ocean basins play very important roles in triggering regional climate changes around the globe, considering that the tropical ocean SST variabilities from different ocean basins are the key drivers of the global regional climate variability (as demonstrated in 10.1.4.2 or this Chapter). Many recent (and classic) studies indicated that the variability of one tropical ocean basin may actively influence the variability of another. It is necessary to indicate the importance of the inter-basin interactions in triggering the regional climate changes. I think this paragraph is a good place (although not the only one) to introduce the interactions between different ocean basins. Here's some of recent papers about inter-basin interacions: 1. M. A. Alexander, I. Blade, M. Newman, J. R. Lanzante, N.-C. Lau, J. D. Scott, The Atmospheric Bridge: The influence of ENSO teleconnections on air-sea interaction over the global oceans. J. Clim. 15, 2205–2231 (2002). 2. Cai, Wenju, Lixin Wu, Matthieu Lengaigne, Tim Li, Shayne McGregor, Jong-Seong Kug, Jin-Yi Yu, Malte F. Stuecker, Agus Santoso, Xichen Li, et al. "Pantropical climate interactions." Science 363, no. 6430 3. M. H. England, S. McGregor, P. Spence, G. A. Meehl, A. Timmermann, W. Cai, A. Sen Gupta, M. J. McPhaden, A. Purich, A. Santoso, Recent intensification of wind-driven circulation in the Pacific and the ongoing warming hiatus. Nat. Clim. Chang. 4, 222-227 (2014). 4. X. Li, S.-P. Xie, S. T. Gille, C. Yoo, Atlantic induced pan-tropical climate change over the past three decades. Nat. Clim. Chang. 6, 275–279 (2016). 5. Ruprich-Robert, Y., Msadek, R., Castruccio, F., Yeager, S., Delworth, T. and Danabasoglu, G., 2017. Assessing the climate impacts of the observed Atlantic multidecadal variability using the GFDL CM2. 1 and NCAR CESM1 global coupled models. Journal of Climate, 30(8), pp.2785-2810. 6. Abram, N.J., Wright, N.M., Ellis, B., Dixon, B.C., Wurtzel, J.B., England, M.H., Ummenhofer, C.C., Philibosian, B., Cahyarini, S.Y., Yu, T.L. and Shen, C.C., 2020. Coupling of Indo-Pacific climate variability over the last millennium. Nature, 579(7799), pp.385-392. 7. Ham, Y. G., Kug, J. S., Park, J. Y., & Jin, F. F. Sea surface temperature in the north tropical Atlantic as a trigger for El Niño/Southern Oscillation events. Nature Geosci. 6, 112-116 (2013). [Xichen Li, China]	Noted. Thanks for the references. One reference has been added for the FGD but the subject of most of the other papers does not entirely fit with our remit in this sub-section. Note that ES statement has been added to state that non-stationarity aspects related to pan-oceanic interaction are important
59305	50	53	51	54	In the FOD there was a paragraph in that subsection about the representation of monsoon systems by CMIPs models. I'm wondering if authors have decided to remove it arbitrarily or if authors just forgot to include that paragraph again. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Monsoon systems and their representation by CMIP models are dealt with in chapter 8.
22899	50	53			Although the segment links to chapter 3 the assessment performed still feels quasi-redundant with that performed there which undertook substantive assessments of ESM performance for ENSO, MJO etc. There is still likely too much overlap here and the present chapter should be more explicitly taking the chapter 3 assessment and then adding necessary detail about how to derive regional information from these. The section lacks any real detail on how RCMs perform in this region. There are several CORDEX domains that include the tropics. [Peter Thorne, Ireland]	Taken into account. Text has been revised to ensure consistency and reduce overlapping with other chapters. Note also that the MJO assessment has been moved to chapter 8.
116963	50		50		For storm tracks, please check coherency with the related assessment in ch 3 and 8. [Valerie Masson-Delmotte, France]	Accepted. Much of the material regarding blocking and storm-tracks has been moved and merged in chapter 3. Only a short text and RCM studies are kept in chapter 10.
59205	51	1	51	55	Author(s) should consider adding EL NINO and LA NINO concepts clearly before expanding more on ENSO. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. These have already been introduced in the report. Note also that references have been made to the technical annex on modes of variability.
100831	51	4	51	4	The following recent papers also discuss the non-stionary aspects of teleconnections: O'Reilly, 2018; O'Reilly et al., 2019; Weisheimer et al., 2020). (O'Reilly, C.H. (2018):Interdecadal variability of the ENSO teleconnection to the wintertime North Pacific. Clim. Dyn., 51: 3333. https://doi.org/10.1007/s00382-018-4081-y. ; O'Reilly, C.H., T. Woollings, L. Zanna and A. Weisheimer (2019). An interdecadal shift of the extratropical ENSO teleconnection during boreal summer. Geophys. Res. Lett., 6,https://doi.org/10.1029/2019GL084079.) Weisheimer, A., D. Befort, D. MacLeod, T.N. Palmer, C. O'Reilly and K. Strommen (2020). Seasonal forecasts of the 20th Century Bull. Amer. Meteor. Soc.doi:10.1175/BAMS-D-19-0019.1 [Corti Susanna, Italy]	Taken into account. Thanks for the references, two have been added for the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10989	51	6	51	20	There are also model biases in the northern hemisphere summer response to ENSO, due to biases in the Asian jet (O'Reilly et al 2018; https://doi.org/10.1175/JCLI-D-17-0451.1) [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Thanks for the reference, it has been added for he FGD.
109897	51	10	51	20	Regarding the performance of GCMs in representing the observed teleconnections, it is only mentioned the response of polar vortex to ENSO events. There are other studies such as Iacovone et al (submitted), who assess the representation of El Niño Oceanic and Modoki influences over extreme precipitation (dry and wet consecutive days -CDD and CWD, respectively-). They find that regions with stronger signal are simulated adequately, especially for CDD. Also, Tedeschi and Collins (2017) analyse the influence of different ENSO types over the precipitation of South America, from CMIP5 outputs. Also, Iacovone, M.F., V. Pántano, O. Penalba (submitted). Consecutive dry and wet days over South America and their association with ENSO events, in CMIP5 simulations. Theoretical and Applied Climatology. Submitted. Tedeschi, R. G., and Collins, M. (2017). The influence of ENSO on South American precipitation: simulation and projection in CMIP5 models. International Journal of Climatology, 37(8), 3319-3339. [Vanesa Pántano, Argentina]	Taken into account. Thanks for the references, one particularly relevant paper has been added for the FGD.
4301	51	16	51	17	I'm assuming this weakening of the polar vortex during ENSO is primarily referring to the NH polar vortex. Best be clear about that. [Isla Simpson, United States of America]	Taken into account. It has been specified for the FGD
4303	51	16	51	17	This sentence also seems completely at odds with the previous one. It is intended to state that while they capture the sign, they don't capture the magnitude? I'm not sure. [Isla Simpson, United States of America]	Taken into account. Thanks, the sentences have been revised to clearly indicate the specific polar vortex (northern or southern) being assessed
125691	51	16	51	17	In the observational record, central Pacific warming type of El Niño sometimes led to a strengthened stratospheric polar vortex instead of a weakened one and the ultimate response exhibits clear sub-seasonal evolution and depends on the initial extratropical circulation when warming happens. The discussion here should be made more accurate. [Trigg Talley, United States of America]	Taken into account. An errata has been submitted to modify the sentence and add the reference
45133	51	47	51	49	The sentence is confusing and needs to be corrected "However the propagaion speed of some CMIP5 models …". Propagation speed of what? [Krishnan Raghavan, India]	Not applicable. The MJO assessment has been moved to chapter 8 for the FGD.
20673	51	53	51	54	This passage is believed to illustrate a blatant misuse of the IPCC confidence statements [philippe waldteufel, France]	Not applicable. Sentence has been removed.
116965	51		51		For ENSO, same remark, please check how this is addressed in other chapters (eg 2,3,4, 8) and make sure that the discussion of CMIP3, CMIP5, CMIP6 . What about a focus on teleconnections here? [Valerie Masson-Delmotte, France]	Accepted. The text has been revised to focus on specific teleconnections and on aspects complementary to what can be found in chapter 3. The MJO assessment has been entirely moved to chapter 8.
65349	52	1			I miss the discussion of regional phenomena that are not atmospheric and also relevant for regional climate change such as sea ice dynamics for example in regional seas (Baltic), SST of regional seas (Mediterranean), aerosol optical depth (Africa), North-Atlantic THC (for Europe) and related SST, … I'm not sure the feedback section really answers this need. [SAMUEL SOMOT, France]	Noted. We agree that these phenomena are relevant, but they are beyond the scope of our Chapter.
22901	52	18	52	19	But you are in the section you are referencing? [Peter Thorne, Ireland]	Taken into account. This was a leftover from a reorganisation and has been adjusted.
51553	52	28	52	28	Suggest change "3km" to "5km". Convection-permitting models with grid-spacings of ~4km have been run over Africa (Stratton et al, 2018, Kendon et al 2019) and over the US (Liu et al 2016) and shown good performance in representing convection. Stratton, R. A. et al. A pan-Africa convection-permitting regional climate simulation with the Met Office Unified Model: CP4-Africa. J. Climate; https://doi.org/10.1175/JCLI-D-17-0503.1 (2018). Kendon E.J., R.A. Stratton, S. Tucker, J.H. Marsham, S. Berthou, D.P. Rowell and C.A. Senior (2019) Enhanced future changes in wet and dry extremes over Africa at convection-permitting scale. Nature Comms. doi: 10.1038/s41467-019-09776-9; Liu C., Ikeda K., Rasmussen R., Barlage M., Newman A.J., Prein A.F., Chen F., Chen L., Clark M., Dai A., Dudhia J., Eidhammer T., Gochis D., Gutmann E., Kurkute S., Li Y., Thompson G., Yates D. (2016) Continental-scale convection-permitting modeling of the current and future climate of North America. Clim Dyn. https://doi.org/10.1007/s00382-016-3327-9 [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text has been modified to 4km. We have included the Stratton reference here, but not the others. Kendon et al is about future changes and is considered in the fitness for projections Section, the Liu paper unfortunately does not provide a coarser resolution reference to clearly assess the effect of resolution.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
110923	52	28	52	35	Some of these studies are based on 4km simulations. And I'd argue that 4km is a more appropriate value on line 28. Please consider changing 3km to 4km. [Melissa Bukovsky, United States of America]	Accepted. Text has been modified.
51557	52	28	52	40	It would also be relevant to mention here that convective permitting models (although in coarser resolution then your definition (4.5 km)) can also improve larger -scale features as the annual cycle of tropical-extratropical cloud band rainfall systems over Africa (Hart et al., 2018) [Hart, N. C., Washington, R., & Stratton, R. A. (2018). Stronger local overturning in convective-permitting regional climate model improves simulation of the subtropical annual cycle. Geophysical Research Letters, 45, 11–14.] [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. A reference has been added and the sentence slightly rephrased.
65351	52	28		40	Ban et al. (in revision) and Pichelli et al. (in revision) are worse to be assessed here as it is the first multi-model studies (eval mode and hist model) for such phenomena and using an ensemble of CPRCMs. In particular, it is worse noting that the improvment of the diurnal cycle strongly depends on the CPRCM choice what was not clear in previous literature. [SAMUEL SOMOT, France]	Accepted. Both references have been added.
66311	52	30	52	31	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]	Accepted. Both references have been added.
51555	52	33	52	33	It would be good to also add the fact that convection-permitting models give an improved representation of the intensity-duration characteristics of rainfall (Kendon et al 2012,2014), hourly precipitation extremes (Chan et al 2014), and propagating convective systems over Africa (Kendon et al 2019). Kendon, E. J., N. M. Roberts, C. A. Senior, and M. J. Roberts (2012) Realism of rainfall in a very high resolution regional climate model. J. Climate, 25, 5791–5806, doi: https://doi.org/10.1175/JCLI-D-11-00562.1; Kendon E.J., N.M. Roberts, H.J. Fowler, M.J. Roberts, S.C. Chan and C.A. Senior (2014), Heavier summer downpours with climate change revealed by weather forecast resolution model, Nature Climate Change, 4, 570-576, doi: 10.1038/NCLIMATE2258; Chan S.C., E.J. Kendon, H.J. Fowler, S. Blenkinsop, N.M. Roberts, C.A.T. Ferro (2014) The value of high-resolution Met Office regional climate models in the simulation of multi-hourly precipitation extremes. J Climate, 27, 16, 6155-6174, doi 10.1175/JCLIM-13-00723.1; Kendon E.J., R.A. Stratton, S. Tucker, J.H. Marsham, S. Berthou, D.P. Rowell and C.A. Senior (2019) Enhanced future changes in wet and dry extremes over Africa at convection-permitting scale. Nature Comms. doi: 10.1038/s41467-019-09776-9 [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. These papers have partly been assessed already in AR5, the Kendon et al. 2014 paper is cited in the fitness-for-projections Section.
100871	52	42	52	44	It might be worth mentioning here that for instance, Stochastic Physics has a nearly equivalent effect to that of resolution on the mean number and distribution of TCs (Vidale et al. 2020, submitted to J. Clim). [Corti Susanna, Italy]	Noted. Unfortunately, the paper has not been published yet.
83647	52	42	52	50	It is trivial that the eyewall will be smoothed in 20 km simulation compared to a 1 km mesh simulation. More interesting would be, for example, quantitative information about peak wind speeds and total precipitation amounts for different resolutions (GCM, conventional RCM, convection-permitting). [Petra Seibert, Austria]	Noted. Unfortunately, no observations of peak windspeeds are available for comparison. But the overall effect is well illustrated by the figure as is.
82695	52	45	52	45	Should be Typhoon Haiyan. [Blair Trewin, Australia]	Accepted. Text has been modified.
4305	52	45	52	45	“Hayan” → “Haiyan”. I think it’s “Haiyan” but even if it’s not then the figure caption says “Haiyan” so there is an inconsistency and one of them is wrong. [Isla Simpson, United States of America]	Accepted. Text has been modified.
54391	52	46	52	46	Wrong number of a figure - should be 10.8 [Gabriel Stachura, Poland]	Accepted, although figure numbering has changed again.
59309	52	46	52	46	The label for "(Figure 10.7)" in the text shall be replaced with "(Figure 10.8)". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, although figure numbering has changed again.
4307	52	46	52	46	I think it should perhaps be Figure 10.8 instead of Figure 10.7. [Isla Simpson, United States of America]	Accepted (although numbering has changed again).
20219	52	46	52	46	Actually it is Figure 10.8 [philippe waldteufel, France]	Accepted, although figure numbering has changed again.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
125693	52	46	52	46	This sentence is referring to Figure 10.8, not Figure 10.7. [Trigg Talley, United States of America]	Accepted. Although figure numbering has changed again.
20675	52	48	52	50	There is no discussion that improved spatial resolution improves the simulation of tropical cyclones. The open question is whether the critical physical feature is convection, or it is something else associated with the 3D wind field for example [philippe waldteufel, France]	Noted. We agree that the improvement may not be directly linked to convection. We added a further reference where the effect of coupling with SST in high resolution simulations on convection has been demonstrated.
65353	52	50			Representation of medicanes have been evaluated in RCMs in a multi-model framework recently testing the effect of resolution and air-sea coupling by Gaertner et al. 2018, this may be relevant here : doi: 10.1007/s00382-016-3456-1 [SAMUEL SOMOT, France]	Taken into account. The issue (incl. reference) is discussed in the feedback section.
116967	52		52		For issues related to the representation of convection, there are also cross cutting issues with chapter 7 (cloud feedbacks) that could be relevant to consider, to better ground chapter 10 as the interface between the large scale and process chapters, and the regional information. For instance, CMIP6 models [Valerie Masson-Delmotte, France]	Taken into account. We have added a short paragraph on cloud-resolving GCMs with links to Chapters 7 and 8.
65355	53	11		12	To complete Dafka et al., I guess that Herrmann et al. 2011 (already cited) and Obermann et al. 2018 (already cited for wind over land) could be assessed and very relevant there too for the regional winds over the sea in RCMs. Mistral and Tramontane over the sea for Obermann, All Mediterranean regional winds for Herrmann et al. with in addition a RCM resolution ranging from 125km to 10km. [SAMUEL SOMOT, France]	Noted. We decided to focus on land in this Chapter.
83649	53	15	53	15	Some kind of daytime up-slope wind will develop even in coarse models, but because of the unrealistic representation of topography these winds will not be realistic. Rather they will, together with numerical effects (along-model-level diffusion) lead to wrping placement and of uncrealistic magnitude of moisture and precipitation. [Petra Seibert, Austria]	Taken into account. The sentence has been slightly modified.
59227	53	15	53	19	Reprhase: A reanalysis-driven RCM simulation at 4 km resolution showed good skill in simulating the diurnal cycle of temperatures and wind on days of weak synoptic forcing in both the Rocky Mountains in North America (Letcher and Minder, 2007) and the Tianshan mountains in Central Asia (Cai et al., 2020), whereas a 1 km resolution RCM was required in the European Alps (Zängl et al., 2004). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The phenomena looked at are not identical, so putting this together into one sentence does not work.
59225	53	19			Missing citation: Cai, P., Hamdi, R., He, H., Luo, G., Wang, J., Zhang, M., Li, C., Termonia, P., & De Maeyer, P. (2020). Numerical Study of the Interaction between Oasis and Urban Areas within an Arid Mountains-Desert System in Xinjiang, China. Atmosphere, 11(1), 85. https://doi.org/10.3390/atmos11010085 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The study does not compare different resolutions and thus does not allow for an assessment of the required resolution.
83651	53	21	53	21	It is a long-standing mistake to believ that foehn winds require precipitation on the windward side, see e.g. http://dx.doi.org/10.1127/0941-2948/2012/0398, [Petra Seibert, Austria]	Taken into account. The text has been adjusted.
125695	53	21	53	22	"Fohn winds are regional-scale synoptically-driven winds that cause orographic precipitation." The statement does not appear to be correct. Fohn wind is down-slope wind that causes a warm and dry condition, instead of precipitation (see AMS Glossary, glossary.ametsoc.org). Needs to be rewritten to be consistent with the Fohn definition. [Trigg Talley, United States of America]	Noted. The dry conditions are on the downwind side. Precipitation on the windward side increases the dryness. But nevertheless, the text has been adjusted.
23651	53	21	53	24	In more complex terrains, such as the cordillera of Antarctic Peninsula, recent study by Turton et al. (2017) found that a 1.5 km was able to capture the foehn winds, whereas a 5 km version could not. On the other hand, Bozkurt et al. (2018) demonstrate that a 2 km version of the same model was still unable to resolve effectively foehn warming. Turton, J. V., Kirchgaessner, A., Ross, A. N., and King, J. C.: Does High-Resolution Modelling Improve the Spatial Analysis of Föhn Flow over the Larsen C Ice Shelf?, Weather, 72, 192–196, https://doi.org/10.1002/wea.3028, 2017. Bozkurt, D., Rondanelli, R., Marin, J. C., and Garreaud, R.: Foehn Event Triggered by an Atmospheric River Underlies Record-Setting Temperature Along Continental Antarctica, J. Geophys. Res.-Atmos., 123, 3871–3892, https://doi.org/10.1002/2017JD027796, 2018. [Deniz Bozkurt, Chile]	Noted. In our Chapter, we do not focus on regional climate in the Arctic regions.
80331	53	22	53	22	Lifting Condensation Level is the common term [Paola Arias, Colombia]	Not applicable. The text has been modified.
83653	53	23	53	24	One cannot easily state a required grid spacing for resolving foehn. Some features will be resolved with even coarser spacing than 10 km, on the other hand, if the detailed distribution of high-wind areas is desired, 100 m would be more appropriate. Detailed resolution of wave structure, rotors, etc requires LES. [Petra Seibert, Austria]	Noted. The literature we assessed points to the stated minimum resolution. As we cite only one reference, we do not provide any generalising confidence statements.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83655	53	26	53	35	It is problematic to put bora into the category of gap flow or channelled winds. Bora is basically the waterfall-like flow of cold air over the Dinaric coastal mountain range to the Adriatic Sea. A 0.44 deg simulation can show this feature, but obviously it cannot show the detailed structure caused by the fine-scale topography, which is responsible for the extreme gust values in certain places. It is better characterised as a foehn-like (orographic wave) feature, and the remarks about required resultion made in the comment to Ch 10 p53 l23/24 apply here as well. [Petra Seibert, Austria]	Noted. While Bora is not limited to the gap winds, a key feature of Bora is the strong enhancement of wind speeds in mountain passes in the Dinaric Alps. Note also that Bora is NOT a katabatic wind as suggested by the reviewer. We therefore keep the sentence as is.
59311	53	29	53	30	I would suggest to add "(1° is about 110 km)" after the 'latitude' to make it very clear for the reader and help connect the dots, since all the prior statements were in 'km' scale. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Resolution is now given in km.
4309	53	31	53	31	The sentence beginning “Nevertheless, 0.44...” seems contradictory to the previous sentence. Maybe it should read something like “While these features can be resolved in 0.44deg RCMS, such models do not realistically represent...” [Isla Simpson, United States of America]	Taken into account. After re-examining the paper in the previous sentence, the statement about RCMs in that sentence has been dropped.
23653	53	37	53	39	Bozkurt et al. (2019) also illustrated that 10 km regional climate model simulations resolve the coastal-valley Andes transitions over central Chile. 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]	Noted. The section is on wind systems whereas the suggested reference is on temperature and precipitation.
83657	53	37	53	39	As 10 km is not a hard boundary, and as many features would rather require 1 km model resolution, it would be better to phrase this for example as ““There is very high confidence that realistic simulation of flow features related to the fine-scale topography in high mountains requires a correspondingly high resolution, which may vary according to the specific features aimed at.” One should also note that resolution alone is not enough, both physical input parameters and numerical formulations have to be adequate as well, which is not trivial. [Petra Seibert, Austria]	Noted. Although we agree that many features require higher resolutions than 10km, the literature suggests that a resolution of about 10km are the minimum for realistically representing these phenomena. Both points are captured by our statement (although we rephrased it because the term "better" does not make sense).
5615	53	42	54	53	The grouping between Coastal winds and lake effects is surprising and on the coast there is not only the wind effect [Benoit Laignel, France]	Noted. But many phenomena we assess are relevant for both cases, therefore we decided to discuss these together.
59313	53	52	53	52	I would suggest to add "in the Iberian Peninsula" after 'aridity' for clarification of this statement. You correctly referred to it in the next page (54) and line 13. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The statement has been added.
5505	53	53	53	53	Add to the end of the paragraph ending at line 53: "Blocking by high coastal mountains and associated mountain-parallel low-level jets in western US also play a critical role in the winter precipitation distribution over the Sierra Nevada mountains in California. Kim and Kang (2007) showed that a regional model of 18km horizontal resolutions is capable of simulating the Froude-number dependent orographic blocking effects on winter precipitation in the Sierra Nevaeda mountains." [Jinwon Kim, United States of America]	Noted. We focus on post-AR5 literature and cite older papers only if they add a very new perspective, which is not the case for the suggested paper.
71227	53				Figure 10.8, caption has space between the value and the units however the figure does not. (ex. 20km (figure), 20 km (caption) [Nesha Wright, Canada]	Taken into account. Has been adjusted
110925	54	5	4	54	GCM should be "GCMs" - the plural - here, as multiple GCMs were assessed. [Melissa Bukovsky, United States of America]	Taken into account. Note that we have replaced GCMs by global models, which we define as AGCMs, AOGCMs and ESMs.
59231	54	13			Taylor et al. (2013a) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted- The reference is not relevant here (it is on Sahel precipitation whereas the sentence is about the Etesian winds in the Mediterranean.
1369	54	15	54	15	The new ERA5 reanalysis has a resolution of ~30 km and may perhaps resolve the land-sea breeze? [Rasmus Benestad, Norway]	Noted. But it has not been assessed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3683	54	18	54	18	There is work that shows whilst RCMs can simulate the sea breeze, convection-permitting simulations are needed to establish the observed coupling of sea breeze convergence with rainfall, and also to achieve a coupling of the climate change response of rainfall and sea breeze convergence. https://journals.ametsoc.org/doi/10.1175/JCLI-D-14-00850.1 AND https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-19-0328.1?mobileUi=0 [Declan Finney, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We decided not to cite this paper as it switches off the deep convection parameterisation at a resolution of about 17km. While this might be justified in the context, it would require substantial explanations which would not be justified given the limited space available.
59229	54	19			Cuba and Florida statement needs citation [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The reference has been added.
38547	54	36	54	36	Together with orographic winds, fronts I think this should be a headline statement in the executive summary. GCMs do not currently simulate weather features and systems while RCMs do when reaching 10 km. I would really want to see that upfront! [robert vautard, France]	Taken into account. A similar statement has been added to the ES.
20677	54	36	54	38	Here as in most other cases, it makes a lot of sense that improved resolution improves the quality of simulations. Therefore, the confidence assessment makes sense, although it applies to a regrettably qualitative statement: what does "better simulating" mean? The key question might be as follows: while obvious reasons for the positive effect of improved spatial resolution are improved representation of bottom boundary conditions and initial conditions if any (topography, land cover, fluxes…), are there other reasons, and what would be their relative influence? [philippe waldteufel, France]	Noted. While we agree that the word "better" is too vague (the text has been rephrased more specifically), we are not aware of many assessments disentangling the reasons why higher resolution improves the realism of these features. Such a statement is therefore not supported by the literature. In any case, the statement has been rephrased to make it more precise.
42983	54	36	54	38	Why "high confidence" only? Of course, "sufficiently high" resolution is needed. What I am confident about is that with better than used today resolution (0.11°) these phenomena are better represensented. [Bodo Ahrens, Germany]	Taken into account. The statement is now more specific.
59315	54	37	54	37	Please add "coastal" before the 'low-level jets,' to convey a precise message to the reader. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Has been inserted.
69931	54	50	54	53	Time slice experiments of the d4PDF regional outputs suggests that heavy snow fall is expected even at the end of century (Kawase et al. 2016). Kawase et al. (2020) reported images of future snow cover changes particularly for mountainous areas with dynamical downscaling by 5-km and 1-km resolution regional climate modes. The above-listed references could be suitable for the topics of this paragraph. Kawase, H, Murata, A, Mizuta, R, Sasaki, H, Nosaka, M, Ishii, M, Takayabu, I (2016) Enhancement of heavy daily snowfall in central Japan due to global warming as projected by large ensemble of regional climate simulations. Climatic Change 139(2), 265–278. doi:10.1007/s10584-016-1781-3 Kawase, H, Yamazaki, T, Sugimoto, S, Sasai, T, Ito, R, Hamada, T, Kuribayashi, M, Fujita, M, Murata, A, Nosaka, M, et al.(2020) Changes in extremely heavy and light snow-cover winters due to global warming over high mountainous areas in central Japan. Progress in Earth and Planetary Science 7(1), 1–17 [Masayoshi Ishii, Japan]	Noted. The section is about model performance, not about changes in snow fall.
1371	54	51	54	52	Note that in winter when the lakes are ice-covered (and with snow), the atmosphere does not know the difference between lakes and meadows. But when the climate changes, the warmer temperatures also affect the ice conditions on the lakes, and in some cases, there may no longer be a seasonal ice-cover difference. [Rasmus Benestad, Norway]	Noted. This section is on model performance, not changes in lake ice.
82697	54	52	54	52	Is this the intended wording? The evidence (at least in some places, e.g. downwind of the Great Lakes) of local lake influence on precipitation is a lot stronger than medium, but was this sentence meant to refer to the model representation of such? [Blair Trewin, Australia]	Taken into account. The statement has been deleted.
22903	55	2	55	2	three-dimensional surely? [Peter Thorne, Ireland]	Noted. The surfaces are two dimensional objects in three dimensional space.
30659	55	2	55	6	Make reference here also to analysis of Papritz, L. et al., 2014: The role of extratropical cyclones and fronts for Southern Ocean freshwater fluxes. J. Climate, 27, 6205-6224, doi: 10.1175/JCLI-D-13-00409.1. [Ian Simmonds, Australia]	Noted. We do not focus on impacts on the oceans.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38549	55	16	55	16	Chapter 10 could spend more time on the assessment of reanalyses. This is a very important topic which could deserve a subsection [robert vautard, France]	Taken into account in Section 10.2.
72079	55	29	56	31	Here air-sea coupling and representation of upper ocean mixing (in terms of proper MLD representation in the ocean model) is also important for regional climate simulation, such as Indian summer monsoon simulation. A recent study showed the role of narrow coastal Bay of Bengal SST front and MLD dynamics for proper atmospheric convection and Indian summer monsoon simulation in climate models. The study is important in this context and should be mentioned here. Samanta, D., Hameed, S. N., Jin, D., Thilakan, V., Ganai, M., Rao, S. A., & Deshpande, M. (2018). Impact of a narrow coastal Bay of Bengal sea surface temperature front on an Indian summer monsoon simulation. Scientific reports, 8(1), 1-12. [Samanta Dhrubajyoti, Singapore]	Accepted. The reference has been added.
65371	55	29			the assessment of the city-climate feedback is missing : see for example Daniel et al. (2019, already cited). This is likely dealt with in box 10.2 but this box may be cited here for readers looking for climate-city feedbacks [SAMUEL SOMOT, France]	Noted; however, as the reviewer points out, climate-city feedback are already discussed in the relevant box and there is no need to discuss it further here. A link to the relevant box has been added)
68189	55	31	55	33	If surface mass balance models are included the performance could be included here, see comment above, suggested papers Noel et al and Vizcaino et al and probably more, Fettweis et al 2020 compare RCMs of surface mass balance of Greenland for example: https://www.the-cryosphere-discuss.net/tc-2019-321/ [Guðfinna Aðalgeirsdóttir, Iceland]	Accepted; however, the topic is already discussed in Chapter9 where modelled and observed Greenland's ice mass balance is assessed. A sentence has been added with the suggested literature and a link to ch9
125697	55	32	55	32	"Atmospheric-land feedback" is too specific. It seems that "atmospheric-surface feedback" is intended since the sentence pertains to both the SRCCL and the SROCC. [Trigg Talley, United States of America]	Accepted
1373	55	35	55	48	Perhaps make it more explicit that the snow representation in the RCMs is affected by biases in both the temperature and precipitation, part of which may be related to smooth model topography and biases in the simulated storm tracks. [Rasmus Benestad, Norway]	Not applicable: the text has been shortened
20679	55	47	55	48	Medium confidence? So be it, but why select a statement such as "RCMs considerably improve"? What would the confidence statement be for "RCMs significantly improve..."? [philippe waldteufel, France]	Accepted; . The sentence has been rephrased "There is high confidence (medium evidence and high agreement) that RCMs considerably improve the representation of the snow albedo effect in complex terrain."
65373	56	2		4	results confirmed recently by Boé et al. 2020, doi:10.1007/s00382-020-05153-1 in multi-model (fig 7, section 4.2). [SAMUEL SOMOT, France]	Accepted. The reference has been added.
102513	56	10	56	10	Larsen M. A. D, Christensen, J. H., Drews, M., Butts, M. and Refsgaard J. C. Local control on precipitation in a fully coupled climate-hydrology model. Sci. Rep. 6:22927, 2016. [Philippe Tulkens, Belgium]	Accepted. The reference has been added.
76769	56	19	56	22	I would say "to understand and simulate" instead of "simulate". I would also add to the Berthou et al (2015) reference the Berthou et al. (2016), which show quasi-systematic lagged effects of strong winds on heavy precipitation events through the ocean mixed-layer memory for Spanish heavy precipitation events. Berthou, S., Mailler, S., Drobinski, P., Arsouze, T., Bastin, S., Béranger, K., & Lebeaupin Brossier, C. (2016). Lagged effects of the Mistral wind on heavy precipitation through ocean-atmosphere coupling in the region of Valencia (Spain). Climate Dynamics, 51(3), 969–983. https://doi.org/10.1007/s00382-016-3153-0 [Ségolène Berthou, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Note that the published version of the suggested paper Berthou et al (2016) is Berthou et al (2018)
8945	56	19	56	33	The regional ocean-atmosphere coupling has positive effects on the simulations of climatology and interannual variability of summer monsoon rainfall over East Asia (Zou et al. 2016). Zou Liwei, Tianjun Zhou, and Dongdong Peng, 2016: Dynamical downscaling of historical climate over CORDEX East Asia domain: A comparison of regional ocean-atmosphere coupled model to standalone RCM simulations. J. Geophys. Res.-Atmos., 121, 1442-1458, doi: 10.1002/2015JD023912 [Liwei Zou, China]	Accepted. Reference added.
65357	56	19		31	There are also evidence that ocean coupling in RCMs improves the representation of the air-sea fluxes with likely influence on heat and humidity advection over land (Sevault et al. 2014 http://dx.doi.org/10.3402/tellusa.v66.23967, Akhtar et al. 2017 DOI 10.1007/s00382-017-3570-8, Lebeaupin-Brossier et al. 2015, doi:10.1007/s00382-014-2252-z) as also reported in the synthesis Ruti et al. 2016 (already cited) [SAMUEL SOMOT, France]	Accepted. Text modified and references added.
65359	56	19		31	In addition to assess the feedbacks, I would say that assessing the way coupled RCM simulate regional pattern of SST and SIC would be very relevant here as those quantities may impact future climate change along the coast. [SAMUEL SOMOT, France]	Noted; the fact that ocean coupled models are needed to better simulate (past and future) ocean-atmosphere feedbacks and related phenomena that impact the climate of coasts and inland areas is already explicitly mentioned.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65361	56	19		31	It may even be important to assess the way those models represent oceanic regional phenomena such as ocean deep convection, large eddies, key currents, mixed layer depth, upwelling, … as this will have impact on the quality of the climate change information provided to the users interested in the marine ecosystems and maritime activities [SAMUEL SOMOT, France]	Rejected: Ocean mesoscale behaviour are already discussed elsewhere in the chapter (e.g., 10.3.3.4.3), and, in the subsection the comment is referring to, we do talk explicitly about ocean-atmosphere RCMs and feedbacks (including an assessment statement). A more detailed discussion of ocean-atmosphere modelling would be out of the scope of ch10.
79329	56	36	56	36	Please also consider a recent study discussing why climate models miss most of the coarse dust in the atmosphere - https://advances.sciencemag.org/content/6/15/eaaz9507 [Prodromos Zanis, Greece]	Accepted. The reference has been added.
65363	56	36			If not in large-scale, feedbacks or regional phenomena subsections (see my previous comments), the way models represent SST and Sea-Ice-Cover could also be documented in drivers as there are often considered as external drivers for RCMs. See for example the difference obtained over the Mediterranean Sea between GCM and RCM in Boé et al. 2020, Boé J., Somot S., Corre L., Nabat P. (2020) Large differences in Summer climate change over Europe as projected by global and regional climate models : causes and consequences. Climate Dynamics, doi:10.1007/s00382-020-05153-1 [SAMUEL SOMOT, France]	Accepted. The reference has been included in 10.3.3.9.
65365	56	36			I think that the assessment of the quality of the aerosols driver in GCMs at regional scale is weak. I'm not a specialist for GCMs but this forcing is a key driver for the past and future regional climate change for a large number of regions and assessing the way GCMs and ESM are able to reproduce it is very relevant for performing relevant regional climate information. In particular I see nothing on CMIP6 evaluation of AOD from AerchemMIP and also nothing on anthropogenic aerosols, the ones that evolved a lot in the past and are likely to evolve in the future. [SAMUEL SOMOT, France]	Accepted. Many of these aspects are covered in chapters 6, 7 and 8. The paragraph has been rewritten building upon what is already included in those chapters plus specific references that illustrate the limitations of current approaches, and links to subsections of chapter 10 where the role of aerosols in specific cases is assessed.
65367	56	36			The assessment of the way the current generation of RCMs (CORDEX, FPS-aerosol) are representing this forcing is totally missing here whereas this driver is key in understanding past climate and simulating future climate change. Sorry, I will mostly cite references from my lab but I'm sure that others very good references do exist from ICTP at least using RegCM or in institutes using COSMO or WRF. For the Euro-Mediterranean area, you can find references for the aerosol representation in standard RCMs (aerosols imposed as external forcing) in Nabat et al. 2014 (already cited), Nabat et al. 2015a (not the same paper as the one already cited in this chapter doi:10.1007/s00382-014-2205-6), Gutierrez et al. 2018 (https://doi.org/10.1016/j.solener.2018.09.085), Boé et al. 2020 (doi:10.1007/s00382-020-05153-1), Gutierrez et al. 2020 (https://doi.org/10.1088/1748-9326/ab6666) [SAMUEL SOMOT, France]	Accepted. These references have been assessed and additional literature considered. A sentence has been added.
65369	56	36			same comment as above but with references for the evaluation of RCMs with interactive aerosols : Nabat et al. 2012 (doi:10.5194/acp-12-10545-2012), Nabat et al. 2015 (already cited in the chapter), Drugé et al. 2019 (https://doi.org/10.5194/acp-19-3707-2019), Nabat et al. 2020 (https://www.atmos-chem-phys-discuss.net/acp-2019-1183/), Zubler et al. 2011 (already cited) [SAMUEL SOMOT, France]	Accepted. These references have been assessed and additional literature considered. A sentence has been added.
95845	56	37	56	39	This might be improved by using new merged aerosol products from more satellite experiences (Sogateva et al. (2020), Atm. Chem. Phys., doi:10.5194/acp-20-2031-2020). [Christine Bingen, Belgium]	Noted. This kind of assessment is done in Chapter 6.
91045	56	37	56	43	It may be worth noting that RCMs exist with full prognostic aerosol schemes. For example, Horowitz et al. (2017) has successfully simulated the seasonality of aerosol optical depths over Africa using an RCM that includes, amongst other species, emission-driven biomass burning aerosols, dust and marine-derived aerosols. [Francois Engelbrecht, South Africa]	Accepted. More details about models with prognostic aerosol schemes added.
33211	56	45	56	47	Zuo et al. (2019) revealed the reduced monsoon precpitation following tropical and Northern hemispheric volcanic eruptions. I suggest adding this references here. References:Zuo, M., T. Zhou, and W. Man, 2019: Hydroclimate Responses over Global Monsoon Regions Following Volcanic Eruptions at Different Latitudes. Journal of Climate, 32, 4367-4385.doi:10.1175/jcli-d-18-0707.1 [Meng Zuo, China]	Taken into account. The reference is now added to Cross-Chapter Box 4.1, which is referred to in this section.
71489	56	45	56	54	This section overlaps a lot with Chapter 8, Section 8.5.2.3. I suggest to delete the discussion here and merge the material into Chapter 8, including a reference from here. [Douglas Maraun, Austria]	Accepted. Many of these aspects are covered in chapters 6, 7 and 8 and Cross-Chapter Box 4.1. The paragraph has been rewritten and shortened building upon what is already included in those chapters.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64843	56	45	56	54	Barnes et al. (2016, see the reference in the previous comment) highlighted that the climate response to volcanic eruptions in terms of NAO-like signals is correctly simulated with GCMs. They explain however that these signal does not project exactly on NAO pattern, and need to be investigated at the su-seasonal (monthly) timescale for an adequate comparison between model and observations. Also the recent paper Hermason et al. (2020) suggest that teh winter warming occurring in Eurasia can be simulated with current GCMs by using large multi-model ensemble, leading to NAO-like patterns similar to the oberved ones, even if the simulated response is generally smaller in magnitude that in the observations. Reference: Hermanson, L., Bilbao, R., Dunstone, N., Ménégoz, M., Ortega, P., Pohlmann, H., Robson, J.I., Smith, D.M., Strand, G., Timmreck, C. and Yeager, S., 2020. Robust multi-year climate impacts of volcanic eruptions in decadal prediction systems. Journal of Geophysical Research: Atmospheres, p.e2019JD031739. [Martin Ménégoz, France]	Accepted. The sentence has been modified to say that recent model versions capture part of the NAO signal and that large ensembles are required.
64845	56	45	56	54	Khodri et al (2016) suggest that climate models are able to reproduce the chain of El-Niño - la Niña events after volcanic eruptions and they explain the mechanisms behind. You might consider this reference: Khodri, M., Izumo, T., Vialard, J., Janicot, S., Cassou, C., Lengaigne, M., Mignot, J., Gastineau, G., Guilyardi, E., Lebas, N. and Robock, A., 2017. Tropical explosive volcanic eruptions can trigger El Niño by cooling tropical Africa. Nature communications, 8(1), pp.1-13. [Martin Ménégoz, France]	Taken into account. ENSO is not considered in detail in this chapter, although the ENSO teleconnections that have a role in explaining regional climate variability over land are. The reference is more relevant to the Cross-Chapter Box 4.1, which is referred to in the sub-section.
22905	56	45	56	54	The analysis of Bethke et al., 2017 in Nature Climate Change and used in chapter 4 touched on some of these issues and should be incorporated into revisions to this paragraph. [Peter Thorne, Ireland]	Accepted. The reference has been added as a basic reference that illustrates the impact of correctly representing the volcanic forcing.
59233	56	45			Both proxy analyses and simulations have demonstrated reduced Asian monsoon after tropical and Northern Hemisphere volcanic eruptions dur to reduce humidity and divergent circulation. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The comment is incomplete, a reference is missing.
7927	56	46	56	46	insert "volcanic" before "eruptions" [Bart van den Hurk, Netherlands]	Accepted. The change is made.
125699	56	47	56	51	"For the NAO, GCM experiments …" is confusing. Seems to suggest that an effect is revealed by GCM experiments (volcanic eruptions cause NOA shift), but that GCMs can't reproduce the effect. Rewrite for clarity. [Trigg Talley, United States of America]	Accepted. The sentence has been modified to say that recent model versions capture part of the NAO signal but that a large part of the signal is missing.
1615	56	47	56	54	Positive NAO leads to milder winters in Northern Europe. Europe tends to get cooler summers after volcanic eruptions. The summer cooling is unrelated to the NAO. These few lines are confusing. Models may not be very good at it, but how do CMIP6 ones do? [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence has been rewritten and the seasonality of the role of the NAO clarified. As for results with CMIP6 models, no recent literature has been identified.
59235	56	47			Liu et al., 2016 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The comment is incomplete, a reference is missing.
22907	57	1	57	5	This feels unwise at a minimum without making explicit linkages to the substantive assessments undertaken in chapters 6, 7 and 8. It may be better to point the readers to those chapters, characterise their results and then add any necessary details lacking (but from memory everything you include here was covered in a far more substantive manner by these preceding chapters). [Peter Thorne, Ireland]	Taken into account. This paragraph, which has been completely rewritten, focuses now on the regional aspects of anthropogenic aerosols, building upon and duly referencing Chapters 6 and 8.
110757	57	1	57	5	a recent warming has been observed over Europe, a cooling over East Asia monsson region, What change has been observed concerning the west Africa monsoon? Is it a decrease? [Bruno Korgo, Burkina Faso]	Noted. The paragraph deals with changes identified in the model response when aerosols are not properly dealt with in models. No references were found for the impact of the misrepresentation over Africa.
79331	57	1	57	5	Check links with Chapter 6 and Chapter 8 . [Prodromos Zanis, Greece]	Accepted. Many of these aspects are covered in chapters 6, 7 and 8. The paragraph has been rewritten building upon what is already included in those chapters, but also in the Atlas.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38141	57	2	57	5	I would like to another research paper is cited about cooling over East Asia monsoon region by anthropogenic aerosol like this. "Some examples are the recent enhanced warming over Europe (Nabat et al., 2014; Dong et al., 2017), the cooling over the East Asia monsoon region (Shim et al., 2019), leading to a weakening of the monsoon (Song et al., 2014; Wang et al., 2017c), as well as the observed monsoon precipitation in West Africa and South Asia (Under et al., 2018)." - Effects of anthropongenic and natural forcings on the summer temperature variations in East Asia during the 20th century (Shim et al., 2019) [Junhee Lee, Republic of Korea]	Noted.
100485	57	16	57	26	Thiery et al., 2017 provide a comprehensive assessment of the added value of accounting for irrigation in a GCM (CESM 1.2.2) and conclude that including irrigation leads to a small, yet robust increase in model skill across a range of regions and near-surface climate variables, including extreme temperature indices TXx and TNx (figure 3). REF: Thiery, W., Davin, E. L., Lawrence, D. M., Hirsch, A. L., Hauser, M., & Seneviratne, S. I. (2017). Present-day irrigation mitigates heat extremes. Journal of Geophysical Research: Atmospheres, 122(3), 1403-1422. [Wim Thiery, Belgium]	Accepted. Reference has been included.
73829	57	35	57	44	I wonder if more elaboration can be done here. As it is now, it does not fully read like an assessment. [Rondrotiana Barimalala, South Africa]	Noted. This text has been integrated into the main text on statistical downscaling.
78249	57	35	57	44	I think this subsection (10.3.3.8) is extremely important in relation to the regional information of climate projections used in the climate impact assessment.I recommend you discuss much more about the added value by referring a number of results in the VALUE project comparing between process-based SD and bias adjustment. [Motoki NISHIMORI, Japan]	Noted. The VALUE results are discussed in the sections on statistical model performance in general. The material on process-based evaluation of statistical models is very limited. Only one VALUE paper explicitly focusses on a process-based evaluation. This discussion has therefore been integrated now into the overall discussion.
1375	57	37	57	44	Should not limit this disussion to the subset perfect prognosis. It applies to all types of ESD, even MOS. E.g. Benestad et al., (2019; DOI: DOI: 10.1080/16000870.2019.1652882) used a combination of ESD and MOS to explain why decadal forecasts have low skill over continental Europe - due to weak coupling between the ocean state and the atmospheric circulation. [Rasmus Benestad, Norway]	Noted. The discussion is not only on perfect prog. Note that in climate change projections, MOS is limited to bias adjustment, which is discussed here. Furthermore, this Chapter focusses on long-term projections, not climate predictability. Also the suggested paper is on using statistical downscaling to assess the skill of predictions, whereas the discussion here is on the evaluation of the downscaling methods themselves.
110173	57	38	57	40	In Section 10.3.3.8, it is not clear what "observed sensitivity" refers to in the sentence "Within the VALUE initiative, Soares et al. (2018) analysed whether statistically downscaled and bias-adjusted model data could represent the observed sensitivity of local weather to a range of phenomena relevant to European climate". Maybe a couple of examples among the "range of phenomena" would help. [Patrick Grenier, Canada]	Not applicable. The text has been removed.
112061	58	2	58	2	Reference to Figure 10.8 should be 10.9 [jose manuel gutierrez, Spain]	Noted. This comment appears to be misplaced, as there is no figure cited in SOD section 10.3.3.9. However, all figure citations have been examined carefully when producing the final draft with a goal of ensuring appropriate citation in the text.
98099	58	6	58	16	When there is disagreement between observed trend and a model simulated trend for a region, the disagreement could be due to internal variability as noted. If the amount of disagreement is significantly more than expected due to internal variability, we can conclude that the model/forcing the observations are not consistent. To elaborate more on the reasons for such inconsistency, we consider that it can arise from four basic sources: 1) the model response to forcing at the regional scale for the variable of interest may not be correct; 2) the climate forcing agents used to force the climate model simulation (e.g., sulfate emissions, or aerosol concentrations, etc.) may be incomplete or incorrect; 3) internal variability may be underestimated meaning that it is too easy to be inconsistent between models and observations; of 4) there may be deficiencies in the observations that contribute to the model-observation differences. [Thomas Knutson, United States of America]	Noted. While we agree with the comment, this detail is not justified here, also given that the point is discussed in more detail (for large-scale trends) in Chapter 3.
65375	58	6			Mariotti et al. 2015 seem particlarly relevant for evaluating regional trends in CMIP5 : Mariotti, A., Pan, Y., Zeng, N., & Alessandri, A. (2015). Long-term climate change in the Mediterranean region in the midst of decadal variability. Climate Dynamics, 44(5-6), 1437-1456. [SAMUEL SOMOT, France]	Noted. The text has been shortened substantially. However, the reference has been added at a later place in Section 10.4.
22909	58	7	58	7	I would suggest opening this by pointing the reader to chapter 3 for the assessment at global and large scales and perhaps characterising their conclusions upon performance at these scales. [Peter Thorne, Ireland]	Noted. We have added a reference to Chapter 3. But given that the performance varies from variable to variable and aspect to aspect (see e.g., 3.3.1 and 3.3.2), we do not give details about the actual model performance at large scales

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59237	58	15			van Haren et al., 2013 is not in the references section. Instead an Haren, R., Haarsma, R. J., de Vries, H., van Oldenborgh, G. J., and Hazeleger, W. (2015). Resolution dependence 49 of circulation forced future central European summer drying. Environ. Res. Lett. 10, 55002. Available at: 50 http://stacks.iop.org/1748-9326/10/i=5/a=055002. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable, text has been shortened.
65383	58	19			Evaluation of trends in RCM and RCSM is also investigated for the Mediterranean region in Sevault et al. 2014 (http://dx.doi.org/10.3402/tellusa.v66.23967 section 5.3 : Evaluation and understanding of trends in RCSMs) and in Somot et al. 2018 (doi: 10.1007/s00382-016-3295-0, section but for a regional deep sea)3.4 [SAMUEL SOMOT, France]	Taken into account. The Sevault et al. paper has been cited as an example from a region not previously covered in the text for this section. The Somot et al. paper has not been included as it discusses a trend in deep-ocean temperature that is not as directly relevant as the surface temperature trends discussed in the section.
65395	58	19			Concerning the trend evaluation in RCMs, Nabat et al. 2014 (already cited) have evaluated the impact of the RCM internal variability on the trend estimate in RCMs using a 10-member ensemble (the same used in Sanchez-Gomez and Somot, 2018). We show in the paper that the RCM internal variability has a weak impact (as expected) on the estimated trend. This information, minor, can however be relevant for this section. It means that 1 RCM run driven by reanalysis is enough to evaluate trends in RCMs. [SAMUEL SOMOT, France]	Rejected. The bigger issue is not the internal variability generated by the RCM, especially if the RCM domain is relatively small, as it could be for a European domain. The variability coming from boundary conditions in addition to that generated internally can obscure the trend.
91047	58	24	58	27	Engelbrecht et al. (2015) demonstrated that an RCM nudged in CMIP5 simulations was able to simulate the differential trends in annual temperature across the southern African region: Engelbrecht F.A., Adegoke J., Bopape M-J., Naidoo M., Garland R., Thatcher M., McGregor J., Katzfey J., Werner M., Ichoku C. and Gatebe C. (2015). Projections of rapidly rising surface temperatures over Africa under low mitigation. Env. Res. Letters. 10 085004. [Francois Engelbrecht, South Africa]	Rejected. The paper uses GCM boundary conditions, but does not show if the downscaling yields any improvement over contemporary-climate trends simulated by the GCMs themselves. It is not clear if the trends are an outcome of bias correction or actual model performance.
22911	58	31	58	32	What is a sensible predictor and what is the implicit criticism of the preceding studies beiung cited accordingly? This strikes me as a dangerous value-laden statement as currently made. [Peter Thorne, Ireland]	Taken into account. Sensible has been replaced by informative.
59239	58	32			Maraum et al. (2017b) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The reference has been updated to Maraun et al. 2019b.
1377	58	35	58	35	It is important to say whether the calibration uses detrended or data containing the trend. The default used by the esd-tool developed by the Oslo downscaling group is to use detrended data for the calibration of the downscaling model and then add the trend back to see if the model is able to reproduce the historical trends (this is explained in e.g. Benestad, 2010; DOI: doi: 10.1175/2010JCLI3687.1). [Rasmus Benestad, Norway]	Noted. The calibration was done individually by each contributor, but in general the methods have not been detrended. It is actually questionable whether such a detrending is sensible, as the calibration should explicitly identify predictors capturing long-term changes.
65377	58	37		42	not sure this paragraph worses to be in the report. The method used in Racherla et al. 2012 was strongly criticized indeed at that time. My feeling is that a review would cite Racherla et al. but not an assessment. I let you re-assess this study. [SAMUEL SOMOT, France]	Accepted. This paragraph has been removed. We do retain the point made by Laprise (2014) that "[even] on multidecadal time scales, an agreement between observed and individual simulated trends would be expected to occur only by chance".
42733	58	37			This paragraph is rather surprising. As stated, we don't expect the RCM to improve the simulation of trends when the internal variability is large. Suggest removing this paragraph. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The paragraph has been removed, though we have retained this important point: "Even on multidecadal time scales, an agreement between observed and individual simulated trends would be expected to occur only by chance (Laprise, 2014)".
38551	58	44	58	55	Include refeerence Boé et al 2020 on the effect of lack of aerosol change in EUROCORDEX on summer climate [robert vautard, France]	Rejected. Boe et al focus almost exclusively on differences between projected and contemporary climates and only marginally consider a trend over the historical period (in surface solar radiation). Their analysis is simply a small extension of Nabat et al. (2014), which is already cited.
65379	58	46			the results obtained in this study with WRF seems to be contrary to the ones obtained by Kroner et al. 2016 (section 4.3) with COSMO. To be assessed. [SAMUEL SOMOT, France]	Rejected. The wording is that not including the time-varying GHGs "may" misrepresent - i.e., a caution, not a universal conclusion. Kroner et al. report only one simulation and not a thorough evaluation of impacts of not including time-varying GHGs.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65381	58	48			To be more precise, it is not « Including anthropogenic sulphate aerosols » but « Including the past trend in anthropogenic sulphate aerosols ». The difference between the sensitivity experiments is the fact we applied or not a trend in the sulphate AOD. [SAMUEL SOMOT, France]	Accepted. The wording has been changed to refer to the aerosol trend.
65385	58	48			Nabat et al. 2014 results have been confirmed by Boé et al. 2020, doi:10.1007/s00382-020-05153-1 with the Euro-CORDEX ensemble (fig 7, section 4.2). [SAMUEL SOMOT, France]	Rejected. This is not a review but an assessment, and the point of the reference is to note that including aerosol trends may be important.
108981	58	50	58	55	I suggest to add more references on the importance of including land cover change, urbanization to better simulate (historical) climate/climate change [Gemma Teresa Narisma, Philippines]	Noted. The point of the paragraph is to indicate potential areas of concern, not to provide a review of the topic.
78251	59	1	59	2	"Overall, there is low evidence that dynamical downscaling adds value in simulating regional trends, but there is high confidence that including all relevant forcings is a prerequisite for reproducing historical trends." This sentence, especially the first half, seems to be misleading that dynamical downscaling may not add value in simulating regional trends. [Motoki NISHIMORI, Japan]	Noted. The sentence does not preclude the possibility of dynamical downscaling adding value. The statement is simply an outcome of the mixed results for simulating trend that were presented in this subsection.
66315	59	2	59	2	it should be Figure10.9 [Erika Coppola, Italy]	Rejected. The comment may be misplaced, but there is no figure cited at the identified page and line number, nor is there any figure cited in this subsection. Preparation for the final draft did include checking all figure citations for correctness.
68959	59	8	59	8	Change "A climate model credibility" to either "Climate model credibility" or "A climate model's credibility" [Seth McGinnis, United States of America]	Accepted. Text has been modified.
90979	59	8	59	8	"model" should be "model's" [Wendy Parker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been modified.
90983	59	10	59	11	Here "credibility" sounds like a synonym for "quality". Credibility in the sense of believability can depend on more than the actual quality of the ingredients of the method/tool -- it can depend for instance on whether the tool builders are considered trustworthy. See related comment regarding the meaning of credibility at p.11 line 44 and p.91, line 4. [Wendy Parker, United Kingdom (of Great Britain and Northern Ireland)]	Noted. In principle we agree, but this is a quote from AR5.
20683	59	12	59	33	Of course the interest of philosophers in climate modeling is welcome. However, their arrival complicates the issues, as recognized par Parker (2009) herself. The "fitness-for-purpose" concept implies that some models are better fitted for simulating some situations and phenomena, implicitly, they are expected to be less well fitter for other cases for which other model will be better fitted. [philippe waldteufel, France]	Noted.
20685	59	12	59	33	Followed But does it have to be this way? Intuitively, a constraint ought to be introduced, such as a limit in computing power/speed, or money. In absence of such a constraint, should not the most powerful of all the numerical models be fitted for every purpose? Let us consider lines 31-33. Is there any reason here for using a standard RCM while convection permitting simulations will do as well or better? There has to be a constraint; Hence more work is needed from the philosophers. Meanwhile, under constraint, one has to build the best trade-offs in view of the objective pursued (here regional projections for example). This is not so complicated. [philippe waldteufel, France]	Noted. First, even the most powerful model may not be fit for every purpose (as our knowledge may be limited), and second, as in practice, our models are always constrained by computational limitations, the concept is very relevant as it directly applies to imperfect models.
90987	59	13	59	14	The notion of fitness might be more appropriate here than adequacy, insofar as you seem to be concerned with the accuracy of projections, which is something that can be achieved to a greater or lesser extent. It also seems that getting the right result for roughly the right reasons is what you have in mind; you might for instance say: "From a regional perspective, one may ask about the fitness of a model for simulating changes in specific aspects of a regional climate. Usually the aim is not just to accurately simulate the changes, but to do so for the right reasons, that is, by accurately simulating important underlying processes." [Wendy Parker, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We don't think accuracy is the right concept here, but we have implemented the suggestion using "fitness".

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
90989	59	21	59	33	I wonder if it would be worth saying somewhere in here that not all applications require highly accurate projections/results. Sometimes even getting the order of magnitude right is sufficient for a given decision or purpose. The discussion here is focused on fitness for the purpose of accurate simulation of particular aspects of future climate change, which is fine, but it also means that much of the discussion could just as well have been framed in terms of accuracy (of results) rather than fitness; the latter is, at least in my mind, meant to be a broader concept that leaves room for cases where great accuracy is not required. [Wendy Parker, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We agree with the statement about sufficient fitness. We believe, however, that even for getting the order of magnitude of a change right, one has to get the main processes right. We nevertheless added a statement in the preceding paragraph.
110927	59	23	59	26	The papers cited here, are all big picture/broad papers (or the last assessment report), where the need for processes understanding is stated. This section could really use examples of research where this is actually done though. Please add a sentence or two with works that have actually tried to assess the credibility of regional projections through process understanding, as examples. It would be really beneficial to future readers looking for explicit examples. Consider using Bukovsky et al. 2017 as an example. It is exceptionally relevant to the point you are making in this paragraph. It clearly discusses credibility/confidence in projections from a process-level point of view in both baseline and future simulations. (As do Bukovsky et al. 2013 and 2015, already cited herein, but the example presented between those two papers is not as tidy.) Bukovsky, M.S., R.R. McCrary, A. Seth, L.O. Mearns, 2017: A mechanistically credible, poleward shift in warm-season precipitation projected for the U.S. Southern Great Plains? J. Climate, 30, 8275-8298, doi: 10.1175/JCLI-D-16-0316.1. [Melissa Bukovsky, United States of America]	Taken into account. The mentioned paragraph indeed sketches the big picture only, but the reference is considered in the following paragraphs, which goes more into specific examples.
65087	59	25	59	26	In addition to Collins et 2018, we have also looked at the "fitness" of CMIP5/CMIP6 models to produce extreme precpitation in the Northeast US in accordance with observed dynamical processes (large-scale meteorological patterns). The citations are: "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]	Noted. The section here is not about model performance, but linking model performance to the confidence in projections.
66313	59	32	59	33	The main results of Giorgi et al 2016 are based on convection parametrized 12 km RCM and not CP models. [Erika Coppola, Italy]	Noted. Nothing else has been stated in the text.
65387	59	35		38	I would add that past trends are not always driven by the same forcings that will drive the future changes [SAMUEL SOMOT, France]	Accepted. A sentence has been added.
90991	59	40	59	40	Suggest making the purpose clearer, e.g. "The fitness of statistical downscaling and bias adjustment for purposes of enhancing the accuracy of specific aspects of regional climate projections...". Here is a place, though, where talk of fitness might not be needed. It might be that one could just say: "Whether statistical downscaling and bias adjustment increases the accuracy of specific aspects of regional climate projections..." [Wendy Parker, United Kingdom (of Great Britain and Northern Ireland)]	Noted. But the statement is really not about accuracy but rather fitness. In some cases, even the sign of the projected changes is not clear.
1379	59	40	59	45	I think think the statement 'has been scarcely been addressed' is grossly incorrect, but the work has in reality been ignored by a good part of the community. This is the whole point of using common EOFs, which was introduced to ESD by Benestad (2001; DOI 10.1002/joc.703). Perhaps it's not well-understood. The GCM's fitness for projecting the regional climate can be evaluated though an analysis of common EOF as in Benestad et al. (2016; DOI: 10.1088/1748-9326/11/5/054017). By comparing the statistics of the part of the principal components representing the reanalyses and that representing th GCM, it is possible to say whether the GCM represents the observed large-scale condition sufficiently well for the use in downscaling. Usually only a small number of EOFs are needed. In this case, the common EOFs are applied to seasonal or monthly aggregated predictor in order to simulate pdf parameters of interest. The large-scale surface temperature from reanalysis and GCM is typically used to simulate seasonal mean temperature, whereas seasonal mean sea-level pressure is used to downscale the wet-day frequency. When the predictors are aggregated, it is usually no need for more than one single predictor variable. Also, the variables which have the closest physical connection with the predictand is chosen in order to reduce the risk of violating the assumption of stationarity (which is tested to some extent by calibrating the models on detrended data and then evaluated against the original ones). [Rasmus Benestad, Norway]	Rejected. The assessment of whether large-scale conditions in GCMs are well simulated in present climate does not directly allow for an assessment of the fitness for future projections, as should be clear from the assessment throughout this section.
20681	59	43	59	45	Any interpretation of this specific failure? [philippe waldteufel, France]	Taken into account. A statement has been added (note the paragraph has been moved towards the end of the subsection).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116969	59		59		some considerations on fitness for purpose could be nuanced for instance on trend evaluation buidling on the assessment of CMIP6 models, and the use of an independent assessment of climate sensitivity in chapter 4. The wording here (fitness, credibility) differs from the one in the ES (confidence). [Valerie Masson-Delmotte, France]	Noted. A reference to Chapter 3 has been added in the new Section 10.3.8, which contains the CMIP6 trend evaluation. Our assessment of fitness-for-purpose focusses on regional processes, not on with large-scale issues such as the plausibility of climate sensitivities. Note also that a regional projection might still be fit, if the global ECS is not well represented, e.g.,. when considering changes per K global warming.
65391	59		60		I really like section 10.3.3.10. Thanks to the authors [SAMUEL SOMOT, France]	Noted. With thanks, though this comment appears to belong to the new Section 3.3.9, not the new 3.3.8 (where it was placed), since the page numbers point to “Fitness of climate models for projecting regional climate”.
125701	60	2	60	2	This sentence is referring to Figure 10.9, not Figure 10.8. [Trigg Talley, United States of America]	Accepted. Figure references had to be adjusted anyway.
27537	60	2	60	2	Don't you mean Figure 10.9 (not Figure 10.8)? [Eric Brun, France]	Accepted. Figure references had to be adjusted anyway.
59317	60	2	60	2	The label for "(Figure 10.8)" in the text must be replaced with "(Figure 10.9)". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Figure references had to be adjusted anyway.
1381	60	10	60	10	While it is true that uncertainty is added through in every step of the modelling chain, it is also true that new information constraining the solutions is added. The question is whether the cascading addition of uncertainty and information leads to more uncertainty of information at the end - it there is a net increase in uncertainty, then there is no point in downscaling. This is discussed in Benestad et al (2017; DOI: DOI: 10.1038/NCLIMATE3393). It may help thinking of the process in terms of working with information. Another way to describe the situation is as you proceed, you get more 'known unknowns' compared to a big set of 'unknown unknowns' at the start of the process. [Rasmus Benestad, Norway]	Noted. We thank the reviewer for this insight and refer to the discussion in Section 10.3.4 where a similar argument is made.
65393	60	19		38	Concerning the adding of new components of the system and how it may modify the simulated climate change : I would like to ask you to assess two recent articles (multi-model) that show convincingly I hope that having or not evolving aerosols in RCMs can drastically change simulated climate change over Europe. I think that they illustrate quite well the statement « there is high confidence that [...] adding relevant model components can increase the fitness for some aspects of regional projections when are accompanied by a process-understanding analysis.]: Boé et al. 2020 doi:10.1007/s00382-020-05153-1 and Gutierrez et al. 2020 https://doi.org/10.1088/1748-9326/ab6666. Very recent studies but I hope very relevant for this section [SAMUEL SOMOT, France]	Taken into account. A statement and the references have been added.
65389	60	20			Instead of RESM, I would prefer the term AORCM (this is how the coupled RCMs were called at the time of those publications) or RCSM for Regional Climate System Models as there is no human component in those coupled models. For me, using « Earth System » means that the human component has been added in the model (anthropogenic CO2, aerosols, land use, ...). But I may be wrong. Moreover calling « Earth » something that is not covering the whole Earth is strange. An endless debate si far [SAMUEL SOMOT, France]	Noted. We use RESM as used in the literature.
66317	60	28	60	34	Please consider revising this paragraph in view of the paper results shown in 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]	Accepted. The references have been integrated.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
76771	60	30	60	32	This paragraph need to be updated with more recent literature, here are some suggestions: "a qualitatively different response of short duration extreme summer precipitation in some regions (Chan et al., 2014b, 2014a; Ban et al. 2015, Tabari et al., 2016; Kendon et al. (2017), Vanden Broucke et al., 2018, Berthou et al. 2019, Kendon et al. 2019, Chan et al. 2020), but not in others (Ban et al., 2015; Fosser et al., 2017, Chan et al. 2020)." Chan et al. 2020 are in both sets of brackets since they find a larger increase in extreme precipitation in central Europe and the Mediterranean coast (in SON) but not in the UK. Kendon, E.J., N. Ban, N.M. Roberts, H.J. Fowler, M.J. Roberts, S.C. Chan, J.P. Evans, G. Fosser, and J.M. Wilkinson, 2017: Do Convection-Permitting Regional Climate Models Improve Projections of Future Precipitation Change?. Bull. Amer. Meteor. Soc., 98, 79–93, https://doi.org/10.1175/BAMS-D-15-0004.1 Berthou, S., Kendon, E., Rowell, D. P., Roberts, M. J., Tucker, S. O., & Stratton, R. A. (2019). Larger future intensification of rainfall in the West African Sahel in a convection-permitting model.Geophysical Research Letters, 46, 13299– 13307. https://doi.org/10.1029/2019GL083544 Kendon, E. J., Stratton, R. A., Marsham, J. H., Rowell, D. P., Senior, C. A.,. (2019). Kendon, E. J., Stratton, R. A., Tucker, S., Marsham, J. H., Berthou, S., Rowell, D. P., & Senior, C. A. (2019). Enhanced future changes in wet and dry extremes over Africa at convection-permitting scale. Nat. Comm. Chan, S.C., Kendon E. J., Berthou S., Fosser G., Lewis E. and Fowler H. J. Europe-wide precipitation projections at convective permitting scale with the Unified Model https://doi.org/10.1007/s00382-020-0519-8, Clim. Dyn [Ségolène Berthou, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. For space reasons, we considered only some of the suggested papers. Also our assessment has changed and is also slightly different than the suggested one here: whether or not CPS and parameterised convection models show different responses does not only seem to be a question of region, but also of the specific set up and the season.
76773	60	32	60	34	I would add "Higher resolution models also allow more process-understanding of changes in convective rainfall (e.g. Prein et al. 2017, Fitzpatrick et al. 2020)." Prein, A. F., Liu, C., Ikeda, K., Trier, S. B., Rasmussen, R. M., Holland, G. J., & Clark, M. P. (2017). Increased rainfall volume from future convective storms in the US. Nature Climate Change, 7(12), 880–884. https://doi.org/10.1038/s41558-017-0007-7 Fitzpatrick, R.G., D.J. Parker, J.H. Marsham, D.P. Rowell, F.M. Guichard, C.M. Taylor, K.H. Cook, E.K. Vizy, L.S. Jackson, D. Finney, J. Crook, R. Stratton, and S. Tucker, 2020: What Drives the Intensification of Mesoscale Convective Systems over the West African Sahel under Climate Change?. J. Climate, 33, 3151–3172, https://doi.org/10.1175/JCLI-D-19-0380.1 [Ségolène Berthou, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text has been modified (though different to the suggestion) and the references have been added.
112857	60	35	60	44	Another example from Gonzalez et al. (2019) looks at European near-surface wind changes, applied the approach to separating the influences of North Atlantic jet changes from boundary-layer local effects. Reference:Gonzalez, P.L.M., Brayshaw, D.J. & Zappa, G. The contribution of North Atlantic atmospheric circulation shifts to future wind speed projections for wind power over Europe. Clim Dyn 53, 4095–4113 (2019). https://doi.org/10.1007/s00382-019-04776-3 [Paula Gonzalez, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The suggested study does not assess differences in fitness of different model types, and therefore doesn't fit.
90993	60	37	60	37	"can increase the fitness for some aspects of regional projections" -- Might be clearer as something like: "can increase the credibility and fitness-for-purpose of projections of some aspects of regional climate change". [Wendy Parker, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text has been adjusted partly (we avoid credibility in that statement).
79465	60	41	60	41	I would expoect to see some discussion about how the different models or techniques can be used to estmate uncertainties at the regional scale. Very commonly we end up using GCM becase the number of models and simulations in a given region are much larger than those available using RCMs so the uncertainty can be estimated much better. Maybe this was discussed somewere else? [Alejandro Di Luca, Australia]	Noted. This point is discussed in Sections 10.3.4 and 10.5.
7929	60	43	60	46	As usually, the experimental design is ignored as a reason for (regional) climate projection uncertainty. Not only statistical downscaling but also dynamic downscaling suffers from uncertainty linked to the choice of RCM, the lateral forcing etc. Further uncertainty comes from the choice of the reference period, physiography, aggregation level of output etc [Bart van den Hurk, Netherlands]	Noted. These issues are not made explicit but included in the statement about "imperfect knowledge and implementation of the response of the climate system" and the statement about downscaling. This is all discussed in the next section in more detail.
20687	60	43	60	49	This introductory paragraph should ideally help the reader to understand how the section is structured. This is not however straightforward for him. 3 sources of uncertainty are first listed (the reader is ready for 3 paragraphs); then comes a 4rth source of uncertainty. The section structure turns out however to be quite different. [philippe waldteufel, France]	Taken into account. The introductory paragraph has been adjusted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64863	60	52	60	52	On the propagation of uncertainty through impact models, let is look at inconsistency in data sets used for assessing climate change impacts on water resources, for example. It is common practice in the hydrological modelling community to calibrate hydrological models (HMs) with observational data sets for the historical epoch. This entails that parameters of HMs are constrained on observed values. For scenario development in a given future period, outputs from AOGCMs/ESMs are used to derive future hydrologic components and conclusions are derived. This obviously is flawed in many ways. This Chapter 10 has not addressed this issue or at least guide users of climate model outputs on how to address this important issue. [ELVIS ZILEFAC ASONG, Canada]	Noted. This important issue goes beyond the scope of the WG1 assessment and is (likely) discussed in the WG2 report.
38553	61	1	61	1	I think the concept of cascade of uncertainty is elegant, widely used, but flawed for downscaling. The simple fact that RCMs do resolve new phenomena as compared to GCMs mentioned in earlier sections makes it possible that actually uncertainty is reduced by downscaling for some types of phenomena or variables. An example is orographic wind like mistral wind which is not simulated by GCM is simulated by RCMs. In this case uncertainty is reduced. This section should mention that uncertainty does not always accumulate. [robert vautard, France]	Noted. We agree with the reviewer and discuss the point in principle. But we did not find a specific reference.
105831	61	7			For uncertainty partitionning, please also assess the recent Christensen and Kjellstrom 2020. Partitioning uncertainty components of mean climate and climate change in a large ensemble of European regional climate model projections. Climate Dynamics, 1-16. [SAMUEL SOMOT, France]	Taken into account. The reference has been added.
90995	61	15	61	15	Nice point. It might be clearer to say "a better understanding and increased fitness for purpose, where here the purpose of interest is gauging the extent of current uncertainty". It is probably an ensemble rather than a single model whose fitness is at issue. [Wendy Parker, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Actually this is a valid point as well, but this was not our intention. We mean that the individual RCMs may be more fit than the GCMs in resolving important processes, but the uncertainty in simulating these processes may increase the model spread. This latter point is then related to the point made by the reviewer. We adjusted the text to clarify the point.
72279	61	18	62	7	A post-AR5 study on future summer East Asian monsoon (Ose et al., 2020, accepted) indicated a possibility of the climate storyline approches for a regional projection, where the future summer East Asian sea-level pressure pattern and associated precipitation in CMIP5 ensemble are successfully resolved into physically interpreted several modes, reflecting the NH-scale land-sea warming contrast, the future tropical and sub-tropical SST patterns and the weakened vertical monsoon circulations from the Indian Ocean to the Pacific. This study also showed the importance of the simulated present-day monsoon precipitation as a key to reduce a spread of the projections. (Reference) Ose, T., Y. Takaya, S. Maeda, and T. Nakaegawa, 2020: Resolution of Summertime East Asian Pressure Pattern and Southerly Monsoon Wind in CMIP5 Multi-Model Future Projections. J. Meteor. Soc. Japan, 98, doi:10.215/jmsj.2019-0149 (accepted). [Tomoaki Ose, Japan]	Accepted. Reference has been added.
1383	61	20	61	30	It is possible to evaluate how the multi-model ensemble represent the statistics such as trends over a common historical period and the interannual variability (e.g. testing the number of observed cases against the binomial distribution given a 90% confidence interval). Furthermore, through the use of common EOFs, one can bring in the aspect of the models ability to reproduce the dominant spatio-temporal covariance structure. The evaluation of multi-model ensembles is discussed in Benestad et al. (2016; DOI: 10.1088/1748-9326/11/5/054017) and Mezghani et al (2019; DOI: 10.1175/JAMC-D-18-0179.1). [Rasmus Benestad, Norway]	Noted. But this is not the topic of this paragraph.
27539	61	22	61	23	The climate response uncertainty is not characterized "by the multi-model mean change": We recommend removing. [Eric Brun, France]	Noted. The text has been changed to more precisely convey our point.
31649	61	25	61	26	A good example of the need to consider high ends is sea-level rise. (Ch9). [Gonéri Le Cozannet, France]	Noted. A reference to Chapter 9 has been added
22913	61	26	61	27	Chapter 4 includes a substantive assessment of low probability high impact and the relevant section should be pointed to here. [Peter Thorne, Ireland]	Taken into account, a reference has been added.
7931	61	52	61	52	I would not claim that the use of storylines helps the "representation" of uncertainty, since a (discrete) selection of storylines per definition undersamples uncertainty. One could argue that it helps "interpretation" (or attribution) of uncertainty [Bart van den Hurk, Netherlands]	Accepted. Representation has been replaced by interpretation.
22915	61	54	61	55	Emergent constraints are also considered in several other chapters - most notably 4, 5 and 7 - and these sections should be cross-referenced here. [Peter Thorne, Ireland]	Noted, references have been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
72281	61	54	62	7	A post-AR5 study on future summer East Asian precipitation (Ose, 2019b) showed that the future projections of the weakened summer mosoon circulation and the dynamically induced tendency of monthly precipitation over the northern summer East Asia are physically dependent on the simulations of the present-day westerly jet and precipitation. This result indicates that the verification of the climate model simulation using the observation data is necessary for better future regional projections, at least to some extent. (Reference) Ose, T., 2019b: Future Changes in Summertime East Asian Monthly Precipitation in CMIP5 and Their Dependence on Present-Day Model Climatology. J. Meteor. Soc. Japan, 97, 1041-1053, doi:10.2151/jmsj.2019-055. [Tomoaki Ose, Japan]	Noted, a reference has been added.
116971	61		61		On storylines, the chapter could also refer to the use of storylines in other chapters of the report. [Valerie Masson-Delmotte, France]	Taken into account. There is a reference to Chapter 1, which introduces the concept for the whole report. Section 10.5 includes links to other chapters using storylines.
105833	62	3		7	Brunner et al. (submitted, J. Climate) may be very relevant for Europe here as it is a multi-method approach . Brunner et al. (submitted) « Comparing methods to contrain future European climate projections using a consistent » framework. [SAMUEL SOMOT, France]	Noted. But the study does not assess emergent constraints. The reference is, however, relevant for Section 10.3.4.
31463	62	5	62	5	A paper by Zhou et al. in ERL 2019 (10.1088/1748-9326/ab547c) applies the emergent constraint on the projection of the Easi Asia summer monsoon. I suggest adding this reference here. [Shijie Zhou, China]	Taken into account, a reference has been added.
76775	62	6	62	7	Other examples of using emergent constraints (Chapter 4) in a regional context are Brown et al. (2016), Li et al. (2017), Vogel et al. (2018) and Giannini and Kaplan (2019). Vogel, M. M., Zscheischler, J., & Seneviratne, S. I. (2018). Varying soil moisture-atmosphere feedbacks explain divergent temperature extremes and precipitation projections in Central Europe. Earth System Dynamics Discussions, 2018, 1–29. https://doi.org/10.5194/esd-2018-24 [Ségolène Berthou, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, Brown and Li have been added.
117281	62	10	63	19	this subsection should mention and check consistency with sectiion 1.4.2: Variability and emergence of the climate change signal [Maisa Rojas, Chile]	Accepted. The emergence section has been moved to 10.4.3 after discussion within all AR6 chapters.
37701	62	10	63	19	In this chapter, extensive use is made of two large ensemble data sets, MPI-GE and d4PDF-GE. It might be a good idea to mention what they are in the text. [Masahide Kimoto, Japan]	Taken into account. For the FGD, the two large ensembles have been defined in the figure description and the chapter technical annex.
105835	62	10			I'm surprised not to read in this sub-section that the weight of internal variability as an uncertainty decreases when the variables are averaged in time and space, meaning that we may find spatial and temporal scales where internal variability rôle is reduced, especially at the end of the 21st century for strong scenarios. [SAMUEL SOMOT, France]	Taken into account. This point has been included in the FGD, section 10.4.3, that focuses on the emergence concept.
72081	62	12	62	44	One relevant reference could of study that showed low-frequency internal variability in the tropical pacific oceans can emerges from the westward extended ENSO variability in the tropical Pacific. This spurious centennial scale variability can alter historical trends and futrue projections, therefore, caution intepretations are required. -- Samanta, D., Karnauskas, K. B., Goodkin, N. F., Coats, S., Smerdon, J. E., & Zhang, L. (2018). Coupled model biases breed spurious low frequency variability in the tropical Pacific Ocean. Geophysical Research Letters, 45(19), 10-609. [Samanta Dhrubajyoti, Singapore]	Noted.
125703	62	15	62	16	The comment that "Internal variability is an irreducible source of uncertainty for mid- to long-term projections" does not seem accurate. A larger averaging period means a smaller magnitude of internal variability noise. [Trigg Talley, United States of America]	Taken into account. The text has been re redacted, including the point of the reviewer: "Internal variability is an irreducible source of uncertainty for mid-to-long-term projections with an amplitude that typically decreases with increasing spatial scale and 39 lead time (Section 1.4.3; Section 4.2.1).
42735	62	15			'… in particular at mid-to-high latitudes' – this implies that at low-latitudes internal variability may not be such an issue. Bearing in mind the unpredictability of ENSO beyond a year or so and the chaotic nature of the PDV, the comments made in this section would also be valid in the tropics. For example, for precipitation, from figure 4.27 where, even for SSP5-8.5, there are many regions where the change signal is less than the variability (hatched regions) in the wider tropics. Indeed, it's at higher latitudes that the change signal emerges from the variability. Suggest removing '… in particular at mid-to-high latitudes. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The statement has been entirely rewritten for the FGD.
125705	62	18	62	18	Remove "likely" since the likelihood of the statement is already quantified by "very high confidence". [Trigg Talley, United States of America]	Not Applicable. The statement has been entirely revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
107969	62	18	62	21	This is ambiguous: it could either mean that the models used in previous assessments had internal variability that was both too weak compared with observations and weaker than the models used in AR6, or it could mean that there isn't a particular change in the magnitude of simulated internal variabillity but that new experimental designs/strategies now allow the role of internal variability to be better explored and better represented. The latter doesn't mean that internal variability has been underestimated, but the current wording may be (mis-)understood that way. [Timothy Osborn, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Thanks for the two comments. The ES statement has been fully rephrased and shaped as a more positive statement as suggested by the reviewer.
107971	62	18	62	21	The finding reported here is in the SPM draft so it is essential to get it right. The cited literature uses large ensembles to better separate out the contributions of internal variability, model error and response to forcings. The cited papers do not, however, support the claim here that there "is very high confidence that the role of internal variability has likely been underestimated in previous assessments of regional climate projections" -- the previous assessments included internal variability and model error within the multi-model ensembles and these papers do not claim that this combined uncertainty has been underestimated. The separation of these terms has been helped by the new large ensembles, and this should be included as a positive statement about improvements in scientific understanding (we can now understand the contributions to the multi-model ensemble spread much better) rather than as a statement that will be mis understood as implying that internal variability is now someone greater than previously thought. [Timothy Osborn, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Thanks for the two comments. The ES statement has been fully rephrased and shaped as a more positive statement as suggested by the reviewer.
100833	62	18	62	23	The externally forced responses and internal variability estimated on regional scales from initial-condition large ensembles can be calibrated against observational data to improve the reliability of probabilistic regional climate projections over the near and mid-term, i.e. 2021-2060 (O'Reilly et al., 2020). [O'Reilly, C. H., Befort, D. J., and Weisheimer, A. (2020): Calibrating large-ensemble European climate projections using observational data, Earth Syst. Dynam. Discuss.,https://doi.org/10.5194/esd-2020-6, under revision. [Corti Susanna, Italy]	Taken into account. Reference has been added for the FGD.
22917	62	18	62	25	This should also cross-reference the large single model ensembles outlined and assessed in some detail in chapter 4? [Peter Thorne, Ireland]	Taken into account. The reference to chapter 4 has been added for the FGD.
27541	62	19	62	20	About '[…] initial condition, single-model large ensembles […]': it would be good to use the same terminoloy through the chapter, and the report: ICE, SMILE etc. [Eric Brun, France]	Taken into account. The use of acronyms for large ensembles has been homogenized for the FGD.
18365	62	20	62	20	Here and other places, Dai and Bloecker (2018) should be updated to Dai, A., and C.E. Bloecker, 2019: Impacts of internal variability on temperature and precipitation trends in large ensemble simulations by two climate models. Climate Dynamics, 52, 289–306. https://doi.org/10.1007/s00382-018-4132-4. [Aiguo Dai, United States of America]	Accepted. The reference has been updated for the FGD
7933	62	20	62	20	Worth adding reference to Aalbers, E. E., Lenderink, G., van Meijgaard, E., & van den Hurk, B. J. J. M. (2018). Local-scale changes in mean and heavy precipitation in Western Europe, climate change or internal variability? Climate Dynamics, 50(11–12), 4745–4766. https://doi.org/10.1007/s00382-017-3901-9 [Bart van den Hurk, Netherlands]	Accepted. The reference has been added for the FGD.
42737	62	27			In relation to the above comment, this paragraph could usefully refer to figure 4.27 and figure 4.12. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. References to earlier global chapters have been made when deemed relevant.
68487	62	30			Please cite d4PDF (Mizuta et al. 2017, BAMS, already in References) here, too. The d4PDF large ensemble database includes large-ensemble simulations both of AGCM and dynamical downscaling with 20-km RCM. [Yukiko Imada, Japan]	Not Applicable. The text has been removed for the FGD
27543	62	38	62	40	About 'the time of emergence can be misleading for the assessment of rare events, as the associated hazard may increase even when the signal-to-noise ratio is low': this part of the sentence is difficult to understand. We suggest a rewording. [Eric Brun, France]	Taken into account. The section about emergence has been entirely rewritten and moved to 10.4.3. Note that signal-to-noise ratio is just a metric to quantify the emergence of the signal, i.e. the mean forced response. Here we mean that the frequency of hazards, for instance linked to the tails of the distribution, can increase even though the emergence for the signal has not occurred yet. This can happen for instance if there are also changes to the shape of the distribution in addition to changes in the mean.
54371	62	49	62	49	Regarding Figure 10.10a) - what does "y" denote as a unit of scalebar? It's not clear to me. [Gabriel Stachura, Poland]	Taken into account. Figure has been revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116973	62		62		check other examples of emergent constraints in ch 4, 5, 7, 8 [Valerie Masson-Delmotte, France]	Taken into account, references have been added.
116975	62		62		Time of emergence is a key concept for the report (as developed in ch 1, TS, SPM). Is it possible to place an emphasis on this aspect in the case studies developed in the chapter? [Valerie Masson-Delmotte, France]	Taken into account. We have included literature on ToE in Section 6.
71229	62				Figur 10.10, the order that the figure sections are described is not intuitive. The caption discusses a-d after describing e-f. [Nesha Wright, Canada]	Noted. But this is not true.
98101	63	9	63	11	While large ensemble is the preferred method for account for internal variability in determining whether an observed trend and a model historical run (or set of runs) are consistent (if a modeling center/group chooses to devote the necessary resources to it), an alternative approach is to use a smaller ensemble just to estimate the mean (forced) response of the model, and use long control runs to estimate the internal variability influence (which assumes the internal variability does not change over time due to the forced response of the model. In some cases this alternative approach should work quite well, while in other cases (e.g., Arctic sea ice) it may fail badly.These same type of approaches can be used to quantify the potential role of internal climate variability in future climate projections. Two sample references that use the alternative approach or examine it in more detail include Knutson et al. (2013) and Thompson et al. (2015). [Thomas Knutson, United States of America]	Noted. For many variables such as hydrological and circulation-related, a small-size ensemble is not good enough to estimate the forced response. The points raised by the reviewer are also addressed in section 4
45541	63	9	63	14	Vidal et al. (2016) found that both large-scale and local-scale internal variability account for a large part of the total uncertainty in yearly and even 30-year average projections of hydrological (low-flow) variables. 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]	Taken into account. The reference has been added for the FGD.
27545	63	10	64	10	About '[…] are needed to capture […]': Insufficiently specific: define precisely (mathematically) "to capture". [Eric Brun, France]	Not Applicable. The specific text has been removed for the FGD.
42739	63	18			'…and is more acute in the extra-tropics … ' for the key precipitation variable, figures 4.27 and 4.12 do not support this statement. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The statement has been removed for the FGD.
79649	63	22	63	23	This is a general comment for Section 10.3.4.4: An important issue which has recently emerged and could enrich the interesting discussion presented in this section is the role that the choice of domain may have in dynamically downscaled simulations (for regions where simulations from more than one domain are simultaneously available). Two recent works assess this topic using CORDEX data, and try to develop appropriate ensemble methods to make the most of the available information. These are the references: Spinoni et al. 2019 (https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2019GL086799), Legasa et al. 2020 (https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2019GL086799) [Rodrigo Manzanas, Spain]	Rejected. While using simulations from regions that have some overlap might be useful, when opportunity arises, ensembles of opportunity are expected to be suboptimal for assessing sources of uncertainty, an issue discussed in more detail in Chapter 4. In addition, RCM simulation programs generally try to avoid placing locations of interest near the boundaries of the simulation domain, so extracting behaviour along boundary regions tends to be of marginal interest. The utility of partially overlapping domains is not central to the primary focus of the subsection: designing the most appropriate ensembles.
22919	63	22			This section feels like it would greatly benefit from an illustrative example via addition of a figure that outlines the design considerations being discussed. It would also be useful to actually assess how well a well-balanced experimental design has been achieved to date for as many regions as is feasible as this will greatly support downstream chapters and presumably remaining sections in the present chapter as well as WG2? [Peter Thorne, Ireland]	Rejected. The suggested figure would illustrate a narrow point that does not merit the space for it in this assessment. Also, we are not aware of any literature that could be used for an assessment that analyses how well a balanced design has been achieved.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66531	63	41	64	7	Here (for instance p63, l 41), the ENSEMBLES project is mentioned which is a good example. However, there are more recent results from CORDEX that I think would be worthwile discussing here (Three papers submitted end of 2019 by i) Vautard et al, ii) Coopola et al, with results from a 55-member GCM-RCM ensemble from EURO-CORDEX, and iii) Christensen and Kjellström addressing a completely filled sub-matrix (5 GCMs and 4 RCMs) out of that larger 55-member ensemble. The completely filled sub-matrix studied by C&K constitutes a test-bed for GCM-RCM ensembles and can be used to test findings from sparsely filled matrices (which has always been the case until now). C&K find, that for seasonal mean features (three-month averages) of temperature, precipitation and wind speed, climate change for a particular GCM-RCM combination with very few exceptions can be determined by adding effects from the GCM and from the RCM separately implying that subsampling of matrices could be a viable approach. They also demonstrate that areas where the largest impact of the RCMs are seen are areas with complex topography or areas where sea ice, snow, or soil moisture are impacting the results. The three papers of which two are in review are: 1) Vautard, R., Kadygrov, N., Iles, C., Boberg, F., Buonomo, E., Bülow, K., Coppola, E., Corre, L., van Meijgaard, E., Nogherotto, R., Sandstad, M., Schwingshackl, C., Somot, S., Aalbers, E., Christensen, O.B., Ciarlo, J.M., Demory, M.-E., Giorgi, F., Jacob, D., Jones, R.G., Keuler, K., Kjellström, E., Lenderink, G., Levavasseur, G., Nikulin, G., Sillmann, J., Sørland, S.L., Steger, C., Teichmann, C., Warrach-Sagi, K. and Wulfmeyer, V., 2019. Evaluation of the large EURO-CORDEX regional climate model ensemble. Submitted to J. Geophys. Res. 2) Coppola, E., Nogherotto, R., Ciarlo, J.M., Giorgi, F., Somot, S., Nabat, P., Corre, L., Christensen, O.B., Boberg, F., van Meijgaard, E., Aalbers, E., Lenderink, G., Schwingshackl, C., Sandstad, M., Sillmann, J., Bülow, K., Teichmann, C., Iles, C., Kadygrov, N., Vautard, R., Levavasseur, G., Sørland, S.L., Demory, M.-E., Kjellström, E. and Nikulin, G., 2019. Assessment of the European climate projections as simulated by the large EURO-CORDEX regional climate model ensemble. Submitted to J. Geophys. Res. 3) Christensen, O.B. and Kjellström, E., 2020. Partitioning uncertainty components of climate change in a large ensemble of European regional climate model projections. Clim. Dyn., DOI:10.1007/s00382-020-05229-y. [Kjellström Erik, Sweden]	Taken into account. The Vautard et al. and Coppola et al. papers have been cited as they provide examples of an even larger GCM-RCM ensemble and its application. The third paper, however, is not used, as it does not add further, substantial methodological advancement beyond the papers already cited.
105837	63	41			see also Vautard et al. (submitted) for an even bigger matrix in Euro-CORDEX. See also Christensen, O. B., & Kjellström, E. (2020). Partitioning uncertainty components of mean climate and climate change in a large ensemble of European regional climate model projections. Climate Dynamics, 1-16. … for an example of nearly-filled matrix [SAMUEL SOMOT, France]	Taken into account. The Vautard et al. paper has been cited as it is an example of an even larger GCM-RCM ensemble. The other paper, however, has not been not used, as it does not add further, substantial methodological advancement beyond the papers already cited.
105839	63	42			resources is even more an issue with the coming CPRCM (cf. Pichelli et al. submitted) [SAMUEL SOMOT, France]	Noted.
105841	63	43		46	McSweeney et al. 2015 is relevant here too for GCM selection before downscaling [SAMUEL SOMOT, France]	Rejected. This paper is largely an application of the same principles in the earlier papers cited and so does not add further information.
42741	63	44			One issue that it would be useful to discuss in this section (or elsewhere in the chapter) is a specific issue arising in AR6. Users will want regional projections that span the likely outcomes that are consistent with the global projections outlined in Chapter 4. However, if CMIP6 models are used that have a higher climate sensitivity, inconsistencies will arise (see figure 4.9 and the associated discussion) because the likely ranges in Chapter 4 do not only use the GCM projections. Put simply, if the ranges from all CMIP6 models are used to define the expected range, then the corresponding RCM results will not be consistent with the global AR6 likely ranges. It will be useful for this chapter top give some guidance on this issue, and to discuss whether it should be regarded as important. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Illustrations of the potential impact of AR6 vs AR5 GCMs appears in Section 10.6 examples. Wording has been changed to note that the range of climate sensitivity in available GCMs may be inconsistent with the likely range of sensitivity, with reference to Chapter 4.
79463	63	46	63	46	Not sure what do you mean by " These GCMs may also be selected to represent physically self-consistent changes in regional climate (Zappa and Shepherd, 2017)." I would say that all GCMs provide physically consistent changes. GCMs are sometimes selected according to their performance and independence for example in NARCliM (https://www.geosci-model-dev.net/7/621/2014/gmd-7-621-2014.html) [Alejandro Di Luca, Australia]	Accepted. Wording changed to say, "different physically self-consistent changes in regional climate".
15659	63	49	63	49	See also the method by Evin et al. (2019) on partitioning uncertainty components of an incomplete ensemble of climate projections using data augmentation, https://doi.org/10.1175/JCLI-D-18-0606.1 [Samuel Morin, France]	Accepted. The paper gives a distinctive approach to the partitioning of uncertainty, accounting for incomplete matrices of scenario/GCM/RCM combinations.
105845	63	56	64	1	reference for such statistical methods to fill the matrix would be helpful. Personnaly I know only Déqué et al. 2012 (cited just above) with 2 different methods (ANOVA and Weather regimes-based emulator) and Evin et al. 2019 (ANOVA, fig 9). If you have more, please add. [SAMUEL SOMOT, France]	Accepted. While other papers might be cited, these two introduce the methods, with the approach of Déqué et al. (2012), in particular, followed by others. A further, distinctive reference, Heinrich et al. (2014) has also been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66319	63		63		The section 10.3.4.4 could benefit from addind a review of the papers assessing the GCM-RCM matrix designation Christensen, O.B., Kjellström, E. Partitioning uncertainty components of mean climate and climate change in a large ensemble of European regional climate model projections. Clim Dyn (2020). https://doi.org/10.1007/s00382-020-05229-y [Erika Coppola, Italy]	Taken into account. The reference has been added in 10.3.4.1
105843	64	1		3	We did apply this sub-sampling of the Euro-CORDEX ensemble to get a sparse but balanced matrix if you need a reference here. Tramblay Y., Somot S. (2018) Future evolution of extreme precipitation in the Mediterranean. Climatic Change, 151(2), 289-302, https://doi.org/10.1007/s10584-018-2300-5 [SAMUEL SOMOT, France]	Rejected. The cited reference does not use a balanced GCM-RCM matrix: the number of GCMs driving each RCM is not the same for all RCMs.
54393	64	18	64	33	I think this paragraph shouldn't be so detailed and I would shorten it by deleting everything from line 21 on [Gabriel Stachura, Poland]	Accepted. Much of this paragraph does seem redundant with earlier text, and the most relevant parts of the paragraph were added to what is now the second paragraph of the section.
66571	64	27	64	27	A major undertaking relating to weighting of RCMs was taken in the ENSEMBLES project. A special issue was devoted to this. A key reference here is Christensen, J.H., Kjellström, E., Giorgi, F., Lenderink, G., Rummukainen, M., 2010. Weight assignment in regional climate models. Climate Research, 44(2-3), 179-194. (this also refers to the other studies in that excercise) [Kjellström Erik, Sweden]	Not applicable. This paragraph has been deleted, with portions pertaining to a priori choices of models combined with earlier text.
110929	64	29	64	30	Please consider following up on the sentence ending at line 29 with a discussion of the following important point, as is not discussed elsewhere. Weighting based on quality metrics may also not statistically significantly change projections from an ensemble, and it may produce results that are contrary to those produced using a processed-based analysis (Bukovsky et al. 2019). The latter implies that universally applicable metrics are missing important region-specific processes. This lends support to the use of process-based binary weighting. Additionally, regarding the process-based binary weighting, line 30 only mentions GCMs, but I suggest you state RCMs and GCMs here, as you might throw out an RCM for not simulating an important regional process it should be able to resolve, but a GCM cannot, so the GCM would never have been weighted for that process to start. As evidence that this is supported in the RCM community... Bukovsky et al. 2019 point out and discuss the binary process-based quasi-metric that was explored in Bukovsky et al. 2015. Bukovsky, M.S., Thompson, J.A. and Mearns, L.O., 2019. Weighting a regional climate model ensemble: Does it make a difference? Can it make a difference?. Climate Research, 77(1), pp.23-43. Bukovsky MS, Carrillo CM, Gochis DJ, Hammerling DM, McCrary RR, Mearns LO (2015) Towards assessing NARCCAP regional climate model credibility for the North American monsoon: future climate simulations. J Clim 28:6707−6728 [Melissa Bukovsky, United States of America]	Taken into account. This paragraph has been deleted, with portions pertaining to a priori choices of models combined with earlier text. The Bukovsky et al. (2019) reference is now used in the combined paragraph to mention that RCMs as well as GCMs can also be discarded if their simulation of relevant processes is unrealistic.
88833	64	40	64	40	Box 10.1 instead of 10.2. [Krishna AchutaRao, India]	reject. No.
1617	64	42	68	13	This is a very long box on bias adjustment. Again it is about how you should do things. If there is nothing to assess, then say so. There is a lot of good stuff here. It ought to be in a scientific journal. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We assess methodological issues as published in the literature.
116977	64		64		please link the CCBox on bias adjstments to chapters of this report where it is discussed (ex ch 8, 9) [Valerie Masson-Delmotte, France]	Taken into account. Links have been added.
71231	64				Box 10.2, forgive me but what do the acronyms mean? Looking at just the figure AC1 and FA20 are not defined [Nesha Wright, Canada]	Not applicable: figure has been removed
112865	65	12	65	16	This is also very relevant for energy sector applications. See for example: Philip D. Jones, Colin Harpham, Alberto Troccoli, Benoit Gschwind, Thierry Ranchin, Lucien Wald, Clare M. Goodess, Stephen Dorling, Using ERA-Interim reanalysis for creating datasets of energy-relevant climate variables, Earth System Science Data, 10.5194/essd-9-471-2017, 9, 2, (471-495), (2017). [Paula Gonzalez, United Kingdom (of Great Britain and Northern Ireland)]	noted. But the statement is about studies showing an improvement of impact modelling, when fed with bias adjusted data. The suggested paper is only on the creation of the bias adjusted dataset.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
111577	65	14	65	17	As extention of impact studies where bio-productivity forest models were fed with bias-adjusted climate data: Vulnerability of Ukrainian Forests to Climate Change, Sustainability 2017, 9(7), 1152; https://doi.org/10.3390/su9071152 And further sentense on threshold-based climate indices - another study (in Ukrainian) want to propose: Climatic projections of heating season in Ukraine up to the middle of the 21st century by S. V. Krakovska, L. V. Palamarchuk, T. M. Shpytal DOI: https://doi.org/10.24028/gzh.0203-3100.v41i6.2019.190072 [Volodymyr Osadchy, Ukraine]	Rejected. The study does not even mention any bias adjustment methods, in particular it does not compare impact simulations based on bias adjusted with non adjusted simulations.
59319	65	21	65	21	I think the term "change" should be replaced with "mean". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	rejected. "Change" is correct.
54395	65	40	65	55	No information about which variables ale presented in charts (the chart titles suggest it but not every title is clear) [Gabriel Stachura, Poland]	Not applicable. The figure has been deleted for space reasons.
110651	65	40	65	56	Please update the figure to that in the accepted manuscript and adapt the caption accordingly. [Ana Casanueva, Spain]	Not applicable. The figure has been deleted for space reasons.
59321	65	55	65	55	"Casanueva et al. (submitted)" should be changed to "Casanueva et al. (2020)" since the paper is published/online recently. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. The figure has been deleted for space reasons.
59221	65	56	65	56	The some of the information in Cross-Chapter Box 10.2, Figure 1 adopted from Casanueva et al. (submitted). Casanueva et al. (submitted) can be replaced as Casanueva et al.2020 as the article has been published. Reference- Casanueva, A., Herrera, S., Iturbide, M., Lange, S., Jury, M., Dosio, A., Maraun, D. and Gutiérrez, J.M., 2020. Testing bias adjustment methods for regional climate change applications under observational uncertainty and resolution mismatch. Atmospheric Science Letters, p.e978. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. The figure has been deleted for space reasons.
15661	66	14	66	14	"is still required" is prescriptive. [Samuel Morin, France]	Rejected. It is a statement of fact.
27547	66	22	66	23	About ' […] but not the overall climatological bias.': Insufficiently specific. What is this "overall climatological bias"? => the bias that is due to errors in weather types occurrence here. [Eric Brun, France]	Noted. We believe the statement is clear, in particular in the context of the preceding sentences.
68963	66	27	66	27	Change "In presence" to "In the presence" [Seth McGinnis, United States of America]	Accepted. Changed.
110177	66	35	66	36	Box 10.2 is very interesting. However, the following stand-alone sentence-paragraph "There is medium confidence that the selection of climate models with low biases in the synoptic-scale atmospheric circulation may increase the validity of bias adjustment." would benefit from being expanded and a bit more supported. [Patrick Grenier, Canada]	Noted. Unfortunately, we are not aware of further publications on the issue.
13591	66	39	66	39	It is suggested to change the term mitigate by some synonym such as reduce, due to the context of the document. [Maria Amparo Martinez Arroyo, Mexico]	Not applicable. The text has been removed.
105847	66	46		48	This approach of correcting SST before forcing an intermediate AGCM before the RCM is a long-term practice in the community, not really a modified version of the previous method. It was used for example in Cantet, P., Déqué, M., Palany, P., & Maridet, J. L. (2014). The importance of using a high-resolution model to study the climate change on small islands: the Lesser Antilles case. Tellus A: Dynamic Meteorology and Oceanography, 66(1), 24065. and Hopuare, M., Pontaud, M., Céron, J. P., Déqué, M., & Ortega, P. (2015). Climate change assessment for a small island: a Tahiti downscaling experiment. Climate Research, 63(3), 233-247. [SAMUEL SOMOT, France]	Noted. Due to space constraints only one of the references has been added, as they cover essentially the same setting.
105849	66	46		48	concerning modified SST from GCM, please also see a recent adaptation for the sea-ice in Julien Beaumet, Michel Déqué, Gerhard Krinner, Cécile Agosta, Antoinette Alias. Effect of prescribed sea surface conditions on the modern and future Antarctic surface climate simulated by the ARPEGE atmosphere general circulation model. The Cryosphere, Copernicus 2019, 13, pp.3023 - 3043. ⟨10.5194/tc-13-3023-2019⟩. [SAMUEL SOMOT, France]	Noted. The reference was not added, as we do not capture the polar regions in our Chapter, so an isolated discussion of sea ice bias adjustment would be out of scope.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68965	67	4	67	5	Swap "point scale" and "finer resolution". (Currently, the phrase can be read as "point scale, or a resolution even finer than that," which is nonsensical.) [Seth McGinnis, United States of America]	Taken into account. The text has been adjusted (but slightly different)
54397	67	4	67	15	The whole paragraph doesn't contain any new information, everytihing was mentioned in previous sections, I would delete it [Gabriel Stachura, Poland]	Rejected. None of these issues has been stated before.
27549	67	48	67	48	About '[...] strong influence of internal variability and thus of the choice of calibration period on the calibration [...]': we think this is a fundamental issue that should be better discussed. Almost all bias correction methods suppose that a model simulation and observations should be identical on a common period, which is conceptually and fundamentally wrong. It is only true if the common period is sufficiently long to average out the impact of internal variability. It may happen on areas with long-term observations when only the mean is corrected, but for more sophisticated methods, dealing with higher moments, it is almost impossible. [Eric Brun, France]	Noted. We believe the current in-depth discussion is sufficient, given the space constraints and the other important issues.
110181	67	51	68	2	In Box 10.2, the paragraph on cross-validation is not representative of the related ideas found in the scientific literature. In particular, it does not address one fundamental question : what exactly is "evaluating" in a cross-validation procedure ? For example, Lafon et al. (2013) and Gennaretti et al. (2013) have completely different approaches, as the former perform statistics on one-to-one differences (simulation-to-observation), whereas the latter use a metric that compares the observation with the whole ensemble of adjusted simualtions (with verification rank histograms) [as a matter of transparency, I am co-author on the latter paper]. It seems to me that if the notion of "evaluating" in the specific context of cross-validation is not defined, then it is possibly not worth discussing all that. [Lafon et al., 2013, Int. J. Climatol. 33: 1367–1381, doi:10.1002/joc.3518] [Gennaretti et al., 2015, J. Geophys. Res. Atmos., 120, doi:10.1002/2015JD023890]. [Patrick Grenier, Canada]	Noted. But the text is clear in that we refer to case 2 only ("climate change simulations").
15663	68	4	68	4	IPCC is not tasked with providing "Recommendation", this is policy-prescriptive. [Samuel Morin, France]	Rejected. First, these are not policy recommendations, but recommendations on the use of methodologies after a thorough assessment, and second these are not our recommendations but published recommendations.
22927	68	19			The chosen regional case studies are very unbalanced to hydrological extremes with relatively few temperature extremes and no storminess extremes such as e.g. the increase in N. Atlantic basin hurricanes. [Peter Thorne, Ireland]	Taken into account. The number of examples has been strongly reduced (only 3 left) and the narrative of the section has been changed. The main focus on hydrological attribution studies is related to the large number of published papers
20689	68	21	68	33	For this reader, atmospheric internal variability is an intrinsic part of climate. Therefore, while it is conceivable to consider variability on the global scale as a driver of regional climate, it is not possible when speaking of regional variability, which is a characteristic of regional climate. While section 1.4.1 of this SOD (quoted here) seems irrelevant, section 1.4.2 makes a clear distinction between climate change and variability. Cross Chapter Box 3.1 is another example: it addresses the "Slower Surface Global Warming over the Early 21st Century". While internal climate variability is identified as a partial cause of this behaviour, the episode itself is never considered as a climate change! As already pointed out, a basic weakness of this chapter is that regional climate is never defined [philippe waldteufel, France]	Rejected. The definition of regional climate change used in chapter 10 is in line with the AR6 definition of climate change (which is identical to that of AR5 and the three special reports): Climate change refers to a change in the state of the climate that can be identified (e.g., by using statistical tests) by changes in the mean and/or the variability of its properties, and that persists for an extended period, typically decades or longer. Climate change may be due to natural internal processes or external forcings such as modulations of the solar cycles, volcanic eruptions and persistent anthropogenic changes in the composition of the atmosphere or in land use.
39221	68	22	68	24	Explain "higher moments", please. [Lourdes Tibig, Philippines]	Not Applicable. The section 4 introduction has been largely changed and shortened for the FGD.
59207	68	23	68	24	Here as stated that the session deals with "transient' change are a state of the climate. However, the complete section explains about multidecadal studies. Hence, the "transient" change of climate term may not apply. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not Applicable. This part of section 4 has been removed for the FGD.
1385	68	23	68	24	Changes to the mean and/or higher moments only refer to the marginal distribution of climate variables. There are also temporal aspects such as change in persistence and frequency which strictly are not included in this list. [Rasmus Benestad, Norway]	Not Applicable. The section 4 introduction has been largely modified and the specific sentences concerned by the comment have been removed from the FGD.
84737	68	24	68	33	these lines coul be simplified: there are some repetitions that could be avoided to express the concept that external natural and athropogenic forcing as well as atmospheric internal variability both contribute to regional climate hange as interpreted here, and both have to be considered for regiona climate change assessment [Annalisa Cherchi, Italy]	Taken into account. The section 4 introduction has been completely rewritten and shortened.
102515	68	26	68	26	PDV = PDO?, ADV = ADO? (cf. Chapter 2) [Philippe Tulkens, Belgium]	Rejected. Acronyms are defined once for the whole chapter the first time they are encountered.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22921	68	26	68	26	Suggest add a reference to the modes of variability annex. [Peter Thorne, Ireland]	Not Applicable. This part of section 4 has been removed for the FGD.
41157	68	26	68	26	What do "AMV" and "PDV" stand for? [TSU WGI, France]	Rejected. Acronyms are defined once for the whole chapter the first time they are encountered.
100821	68	26	68	26	AMV and PDV should refer to Annex VI.6 and VI.7 [Corti Susanna, Italy]	Not Applicable. This part of section 4 has been removed for the FGD.
22923	68	29	68	33	While true this risks implying that the balance of the report uses the UNFCCC definition which, I think, would be incorrect, given the focus across many chapters on modes of variability. This text thus maybe is a hostage to fortune and if retained should come in chapter 1 and not buried in the middle of chapter 10. [Peter Thorne, Ireland]	Noted. The definition is in line with the AR6 definition, which is also that of AR5 and of the three special reports: Climate change refers to a change in the state of the climate that can be identified (e.g., by using statistical tests) by changes in the mean and/or the variability of its properties, and that persists for an extended period, typically decades or longer. Climate change may be due to natural internal processes or external forcings such as modulations of the solar cycles, volcanic eruptions and persistent anthropogenic changes in the composition of the atmosphere or in land use.
125707	68	31	68	33	Global-mean climate variations (not just regional climate change) can also be related to internal variability (ENSO/PDO affects global-mean surface temperature). [Trigg Talley, United States of America]	Noted. Here we focus on climate variations at regional scale.
39223	68	31	68	33	Is this the basic reason why attribution to anthropogeic climate change in trends of regional climate such a difficult thing to do, in particular distinguishing the role of each,including those of any confounding factors? [Lourdes Tibig, Philippines]	Noted. Yes, and this is related to a decreasing signal to noise ratio, the signal being the response to anthropogenic external forcing and the noise the internal variability, when going from global to regional scales.
44237	68	32	68	33	Land use and albedo changes may also be mentioned here as important drivers. [Nektarios Chrysoulakis, Greece]	Not Applicable. This part of the section 4 introduction has been removed for the FGD.
84739	68	35	68	50	also this part is quite long and could be simplified and shortened. However, it should also include few words on the reasons for the choice of the cases/examples selected and shown in Fig. 10.11 [Annalisa Cherchi, Italy]	Accepted. The paragraph has been rewritten and strongly shortened and the number of examples has been reduced to three.
106599	68	44	68	45	The rationale for following the WG II chapter order is not clear and potentially confusing. The rationale should be to demonstrate different examples of the causes of changes and the interplay between these, maybe in a sequence which allows them to build on each other. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The number of examples has been drastically reduced for the FGD leading to a different rationale for the choice of the examples.
59209	68	45	68	50	In this section, as mentioned that section 10.4 deals with the context of ongoing anthropogenic influence on the global climate. But in the subsection explanation(line 45 to 50), nowhere it is mentioned about anthropogenic effect/impact. However, it resembles that the topic is more focused on internal variability rather than anthropogenic influence. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Lines 47 and 48 explicitly refer to both internal variability and external forcing as possible drivers of future climate change
59211	69	3	69	4	In Figure 10.11, the time series of surface air temperature or precipitation anomalies have taken relative to the 1951–1980 period. But the author has not mentioned why the data is comparable to 1951-1980. The relevant context or reference will give comprehensive information. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. The figure 10.11 has been removed for the FGD
22925	69	17	69	17	Please use Berkeley Earth rather than BEST. BEST is a value laden acronym which should not be used anywhere in the report. Chapter 10 should follow the chapter 2 adopted approach to naming products used. [Peter Thorne, Ireland]	Not applicable. The figure has been removed for the FGD. Noted for the use of Berkeley instead of BEST in other parts of the section.
84773	69	26	69	26	section 10.4.1: in most of the figure same large ensembles have been used but in the text the usage of the ensembles is not well introduced, and in most of the examples is not well explained while referencing to the corresponding figure [Annalisa Cherchi, Italy]	Taken into account. The text has been revised to better explain the use of large ensembles and the interpretation of figures using them.
105853	69	26			Overall, I'm very surprised by the fact that RCM-based studies are not so much used in this regional attribution section whereas evaluation runs are good candidates to contribute to this topics at least to show if they do or do not reproduce the observed trends. [SAMUEL SOMOT, France]	Rejected. Evaluation runs cannot easily be used as a basis for an exhaustive attribution study as the boundary conditions do contain the fingerprint of the total external forcing. They are useful as a first step before using RCMs in attribution studies (which almost do not exist for RCMs).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106601	69	28	69	34	Needs rephrasing to refer to the Ch 1 box on attribution. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The paragraph is already in phase with the cross-chapter box 1.4
1387	69	28	69	41	What's the difference between these factors and what has earlier been referred to as 'teleconnections'? Perhaps mention teleconnections here to make a link to that discussion? [Rasmus Benestad, Norway]	Rejected. Here causal factors refer to both external (both natural and anthropogenic) forcings and internal variability drivers (typically a mode such as ENSO and associated remote teleconnections).
22929	69	31	69	32	It matters more how it differs from attribution approaches used elsewhere within the present report and the sentence should be recast accordingly. [Peter Thorne, Ireland]	Taken into account. The text has been revised to better acknowledge the specificity of this section within the whole AR6 WG1 report.
84741	69	31	69	37	2 points should be distinguished: how attribution here differ from what defined in ch 3 (if it differs) and how attribution at regional scale differs from attribution at global scale (unless the two points coincide). Differences from the definition in AR5 should be eventually mentioned/explained in ch 1? [Annalisa Cherchi, Italy]	Taken into account. The text has been revised to better acknowledge the specificity of this section within the whole AR6 WG1 report.
13641	69	32	69	34	We suggest explaining why detection is not necessary in attribution studies. It is not clear how it is defined that a variable is undergoing change. [Maria Amparo Martinez Arroyo, Mexico]	Taken into account. At regional scale, internal variability can be a very important (even the dominant one) factor of multidecadal changes. Attribution of the observed change then focuses on understanding and quantifying the influence of all possible drivers, including internal variability. The text has been slightly revised to better explain why detection is not a necessary step for attribution.
102517	69	33	69	34	PDV = PDO?, ADV = ADO? (cf. Chapter 2) [Philippe Tulkens, Belgium]	Rejected. The acronyms have already been defined in chapter 10 before section 4, and the agreed rule is not to repeat the definitions.
125709	69	36	69	36	[RISK] Internal variability is not just noise at the global-scale. Arguably, the recent hiatus in global-mean surface temperature warming was attributable (at least in part) to internal variability. [Trigg Talley, United States of America]	Taken into account. The text has been revised to specify that the sentence refers to the detection and attribution methods used at global scale.
106603	69	50	72	3	This whole subsection needs to draw on and be consistent with the Ch 1 box on attribution and also contains a lot of interesting material but it is not clear what the assessment findings are and how much of the material is required to support these findings. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The way attribution has been considered in section 4 appears explicitly in the chapter 1 box on attribution. The amount of material has been drastically reduced as we describe only three examples instead of eight.
71233	69				Figure 10.11, the caption is quite long. Perhaps to minimize the wording put the location of the plot on the figure rather than in the caption. [Nesha Wright, Canada]	Not Applicable. The figure 10.11 has been removed for the FGD
71235	69				Figure 10.11, The axis (in my opinion) should al be on the same axis. For example you have some y axis on the right and some n the left and the units are sometimes with the axis label and sometimes on the opposite side. The units should be located next to or "above" the axis label. [Nesha Wright, Canada]	Not applicable. the figure 10.11 has been removed for the FGD
71237	69				Figure 10.11, In the caption you speak of magenta, you mean red? [Nesha Wright, Canada]	Not applicable. The figure 10.11 has been removed for the FGD
105851	70	1		4	I think that the approach used in Nabat et al. 2014 (already cited) using RCMs driven by the true past large-scale variability (ERA-Int driven runs) may constitute another approach for attribution of regional trends allowing to separate the large-scale drivers from the regional drivers. This has been only rarely used by the RCM users but I think that it is promising. Not sure it is covered by the current list of these lines. It is a model-based approach with less statistics but can probably contribute to the attribution debate for a given zone. [SAMUEL SOMOT, France]	Noted. This is only one type of the needed simulations that are used in a complete detection and attribution framework.
81245	70	6	70	10	Thank you for including new references regarding the attribution of the warming in such regions. It would be good to coordinate with the Atlas to avoid unconsitency regarding this aspect. [Fatima Driouech, Morocco]	Taken into account. Coordination with the Atlas chapter has been done in the regional groups
84743	70	9	70	10	sentence quite useless, it seems it could be removed without loosing any meaning of the assessment [Annalisa Cherchi, Italy]	Rejected. This is a very important result from the papers that are cited above. We do not understand why it is useless.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1389	70	10	70	12	I'm not sure if this sentence can be correct: "The contribution of the GHG forcing to the observed temperature change varies among the different regions, ranging between 60 to more than 100%." It's also not quite clear what that means. There is a great danger that when examining many different regions, one ends up with some interesting-looking results that is not a real result after all. It may be an artefact of many parralel tests - Wilks (2016; DOI: 10.1175/BAMS-D-15-00267.1) has a paper on 'problem of multiplicity' og 'field tests'. [Rasmus Benestad, Norway]	Not Applicable. This sentence has been removed for the FGD.
22931	70	12	70	12	How much more than 100%? It is odd to give a precise lower bound and a vague upper bound and in keeping with rigor of an IPCC assessment arguably the upper bound should be specified to the same precision as the lower bound otherwise it looks like you are hiding something. [Peter Thorne, Ireland]	Taken into account. The text has been rewritten without giving numbers.
1391	70	20	70	22	While the sencence "Finally, model error is rarely included in the statistical model used in detection and attribution regional studies, while it has been shown to have a strong impact on the stability of scaling factors and confidence intervals when increasing the spatial dimension" may be partially true, since most of the statistical downscaling has ignored work based on common EOFs, it would be nice to acknowledge work that actually has included the perspective of model error (e.g. Benestad et al. 2016; DOI: 10.1088/1748-9326/11/5/054017 is the latest in a long series of publications on ESD based on common EOFs). The motivation behind using common EOFs in ESD is precicely the concern about model error. [Rasmus Benestad, Norway]	Rejected. Section 4 is not about statistical downscaling and the work based on Common EOFs is referenced in chapter 10 section 3
70917	70	26	71	3	This literature seems a bit skewed. I can see no difference between dynamical adjustment and conditioning on circulation types (or analogues), so unless I am somehow mistaken, a connection should be made to that literature, otherwise it could be confusing to the reader. Some relevant refereces are Cattiaux et al. (2010 doi: 10.1029/2010GL044613), Horton et al. (2015 doi: 10.1038/nature14550), Kretschmer et al. (2018 doi: 10.1175/BAMS-D-16-0259.1). [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Almost all dynamical adjustment methods add one additional step, related to optimization of the link between atmospheric circulation and surface climate, to go beyond the simple conditioning on circulation types.
108133	70	26	71	3	I suggest rephrasing the paragraphs in terms of assessment report text style instead of indicating individual papers results. [Maria Bettolli, Argentina]	Taken into account. The text has been rephrased to be more like an assessment
98103	71	5	71	14	A little more elaboration here: After the first sentence I would add: "For an individual gridpoint or a regional area average observed time series, the method first assesses whether there is a detectable trend in the observations (i.e., if the observed trend is highly unusual compared to model-simulated natural variability trends, accounting for internal variability as well. Then if an observed trend is found to be detectable, it can also compared to model historical "All-Forcing" runs that include both anthropogenic and natural forcings plus internal variability. If the trend is found to be inconsistent with natural forcing only, but consistent with anthropogenic plus natural forcing, it is concluded that anthropogenic forcing has contributed to the detectable trend. As a variant on this, if the observed trend is detectable but significantly larger than the modeled trend that includes anthropogenic and natural forcing, it is again concluded that anthropogenic forcing has contributed to the detectable trend, since the trend is in the proper direction estimated by the model(s) – it is just underestimated in the models." Then at the end mention that: This methodology has also been used to find evidence for detectable anthropogenic increases and decreases in regional precipitation since 1901 (Knutson and Zeng 2018), and detectable anthropogenic increases in regional summertime wet bulb globe temperature (an index of human heat stress) since 1973 (Knutson and Ploshay 2016). Ref: . 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. [Thomas Knutson, United States of America]	Rejected. Due to space constraints, it is not possible to have this level of detail for the description of the attribution methods. When relevant, the reviewer papers are assessed and cited in the chapter.
1393	71	27	71	27	Consider adding something like this: Similar to the ensemble empirical mode decomposition method, common EOFs can be used to assess model simulations in terms of trend and variability. One example is where decadal forecasts were compared with reanalyses (Benestad et al. 2018; DOI: 10.1080/16000870.2019.1652882). According to search on google.scholar.com, the use of common EOFs is not widespread in the climate research community, but it does offer a means to compare the covariance structure in various data sets and evaluate how well e.g. decadal models predict the main modes of variability. [Rasmus Benestad, Norway]	Rejected. In this section we assess methods that have been used in attribution studies. Common EOFs is an interesting technique that is used to evaluate models as pointed by the reviewer. It is now cited in section 3
59213	71	30	71	30	Hung et al.(Submitted) can be replaced by Hung et al.2020 since the paper has been published recently.The reference - Huang, X., Zhou, T., Turner, A., Dai, A., Chen, X., Clark, R., Jiang, J., Man, W., Murphy, J., Rostron, J. and Wu, B., 2020. The Recent Decline and Recovery of Indian Summer Monsoon Rainfall: Relative Roles of External Forcing and Internal Variability. Journal of Climate, (2020). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The reference has been updated for the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59215	71	37	71	38	Hung et al.(Submitted) can be replaced by Hung et al.2020 since the paper has been published recently.The reference - Huang, X., Zhou, T., Turner, A., Dai, A., Chen, X., Clark, R., Jiang, J., Man, W., Murphy, J., Rostron, J. and Wu, B., 2020. The Recent Decline and Recovery of Indian Summer Monsoon Rainfall: Relative Roles of External Forcing and Internal Variability. Journal of Climate, (2020). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The reference has been updated for the FGD.
100845	71	54	71	55	A reference to Annex VI on the Modes of Variability should be included here as well. [Corti Susanna, Italy]	Accepted. The reference to the annex on modes of variability has been added.
116981	71		71		Could regional attribution techniques, similarities and differences with those of chapter 3 be discussed? [Valerie Masson-Delmotte, France]	Accepted. Differences and/or similarities with methods used in chapter 3 have been added for the FGD.
1395	72	1	72	3	It is expected that we will always see variation in apparent skill over space based on the assumption of sampling fluctuation. This is an artefact of many parralel tests - Wilks (2016; DOI: 10.1175/BAMS-D-15-00267.1) has a paper on 'problem of multiplicity' og 'field tests'. It's a more difficult question to say whether there is genuine difference in skill, but with careful analysis of large ensembles, we may be able to do that. There is also the complicating factor that observations may be scarce or have errors (large differences between different reanalyses over parts of Africa (Benestad, 2011; DOI: 10.1175/2010JCLI3687.1)). Here, there should be reference to such studies. [Rasmus Benestad, Norway]	Noted. These observational issues are dealt with in chapter 10, section 2.
45123	72	1	75	6	There is considerable overlap with Chapter 8 on the observed changes and attribution on the West African Monsoon precipitation. [Krishnan Raghavan, India]	Taken into account. The FGD version of the West African monsoon/Sahel attribution case study has been significantly shortened compared to the SOD version, taking care to ensure that an assessment, rather than a review is performed. This begins from the substantive assessment made in Ch8 pertaining to historical observed and modelled variations in the West African monsoon, but expanding on discussions surrounding its attribution that pertain to Section 10.4
38555	72	6	72	6	This section is confusing and makes an imbalance in the chapter. Regional climate change attribution is one example of line of evidence to form a regional message. It is developed in a number of examples, which is nice, but takes a lot of space, while other methodological aspects do not have examples. One example would be enough. Maybe to be combined with the case studies below [robert vautard, France]	Taken into account. The number of examples has been strongly reduced (only 3 left) and the text has been reduced.
78309	72	6	88	47	The Centre for Climate Research Singapore (CCRS) notes that sea level rise in tropical areas could be up to 30% larger than the global average. Hence, it is important that AR6 includes examples for tropical regions and Southeast Asia, so as to better examine the impact of climate change on these regions, and be more policy relevant to us. [Leonie Lee, Singapore]	Noted. These specific regional aspects are dealt with in chapter 12 and the Atlas (and also chapter 9 for sea level rise projections).
125711	72	6	90	3	[ACCESSIBILITY] Section 10.4.1.2 (and the entire SOD) is too long. Here's an opportunity to trim. There are eight regional attribution examples (Sahel, East Asia, South Australia, SE South America, Central Eurasia, Western Europe, SW North America, Caribbean). Recommend removing two or three. [Trigg Talley, United States of America]	Accepted. The number of examples in section 4 has been reduced to three. Note also that attribution is also dealt with in section 6 examples.
125713	72	6	90	3	[ACCESSIBILITY] The introductory description of the background climate leading each regional subsection is inconsistent. South Australia starts with 23 lines of background climate description; Caribbean Islands starts with 9 lines; and the Sahel, East Asia, SE S America, Central Eurasian, Western Europe and SW N America have little or no background climate introduction. Homogenize. The South Australia background is too long. A short couple-line paragraph for each subsection would be about right. [Trigg Talley, United States of America]	Accepted. The number of examples has been strongly reduced and the introductory sections have been homogenized and shortened for the FGD.
65477	72	6	90	6	Figure 10.13. to Figure 10.19. It’s not clear why the ensembles extremes maps are included and what is the message that the authors try to give with them. I think that there has to be some clarifications in the text if they are going to be included. [Leandro Diaz, Argentina]	Taken into account. The ensemble extreme maps have been included to illustrate the possible role of internal variability on the spatial patterns of multidecadal trends, which is further discussed in the text.
84751	72	8	72	8	section 10.4.1.2.1 could be syntethized and harmonized. Part of the assessment of past changes of west african monsoon (i.e. lines 9-25) can be shortened and referred to section 8.3.2.4 [Annalisa Cherchi, Italy]	Accepted. The FGD version has been considerably shortened compared to the SOD version and references the substantive assessments already made in Chapter 8.
79333	72	8	72	8	Check links and consistency for the discussion of anthropogenic aerosol effects on Sahel and West African monsoon with Chapter 6 and Chapter 8. [Prodromos Zanis, Greece]	Taken into account. The FGD version has ensured consistency with the anthropogenic aerosol statements made in Chapter 8. Note that since the SOD, the Chapter 6 contents on the role of anthropogenic aerosol on monsoon regions has been considerably reduced, so no cross-reference is made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106605	72	8	75	6	This subsection contains a good example of combining multiple lines of evidence to generate a clear climate message and so should be placed in that section of the chapter (it is not an attribution study). Also, it contains a very large amount of detail, not all of which is required to generate the relevant assessment findings (which are partly methodological and thus fit in Chapter 10 but are also partially climatological so should be placed in other chapters). [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The FGD version has been considerably shortened compared to the SOD version and references the substantive assessments already made in Chapter 8. Irrelevant aspects that do not relate to the attribution methods described in this section have been removed.
1619	72	8	92	25	These are well-studied examples, but they all read more like a Review than an Assessment. Some could be shortened to half or more of their size. Again much of it is good text, and ought to be in a scientific journal. As an aside, was there any rationale about which study areas were chosen? [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The FGD version has been considerably shortened compared to the SOD version and references the substantive assessments already made in Chapter 8. In reference to the aside comment FAO LAURENT
22935	72	8			There is a lot of really useful information in here but it often felt more review than assessment and I didn't get a strong natural narrative arc. Efforts to better synthesise and also to reorder along a coherent narrative would greatly help in making this more accessible and serve to better support the conclusions reached. [Peter Thorne, Ireland]	Accepted. The FGD version has been considerably shortened compared to the SOD version and references the substantive assessments already made in Chapter 8. The subsection now is an assessment rather than a review.
45115	72	9	72	9	Check for consistency with Chapter 8 regarding the boundary of the West African Monsoon domain. Specification of the coordinates of the regional monsoon domain boundaries is best avoided. [Krishnan Raghavan, India]	Taken into account. After resurveying the literature, the eastern boundary of the domain used for area-averaging in the SOD figure has been altered by moving it westward, to 30E for the FGD. This is approximately the same as the boundary depicted for the West African monsoon in Ch8. Note that some of the Sahel climate change literature does use our previous boundary at 40E. Making the region smaller in this way strengthens the drying and recovery signals shown in the time series. Following a similar assessment of the literature, the FGD version also uses a rainy season defined as June-September (JJAS) to make sure that September is included. For the West African monsoon affecting the Sahel, September is a wetter month than June, such that a JJA-based summer average would be inadequate.
45119	72	9	72	32	The text is long and reads more like review, rather than assessment. The text may be shortened suitably. [Krishnan Raghavan, India]	Accepted. The FGD version has been considerably shortened compared to the SOD version and references the substantive assessments already made in Chapter 8. The subsection now is an assessment rather than a review.
45117	72	12	72	12	Specification of coordinates of the Sahel domain can be avoided, unless it is essential. [Krishnan Raghavan, India]	Accepted. References to the precise coordinates of regions used in assessed studies have been avoided in the FGD. (Naturally, we must include the coordinates of box-averaging regions used to produce time series in the relevant Figure.)
84745	72	14	72	15	definition of what is intended in this chapter and in the rest of the report for west african monsoon and sahel should be consistent [Annalisa Cherchi, Italy]	Taken into account. After resurveying the literature, the eastern boundary of the domain used for area-averaging in the SOD figure has been altered by moving it westward, to 30E for the FGD. This is approximately the same as the boundary depicted for the West African monsoon in Ch8. Note that some of the Sahel climate change literature does use our previous boundary at 40E. Making the region smaller in this way strengthens the drying and recovery signals shown in the time series. Following a similar assessment of the literature, the FGD version also uses a rainy season defined as June-September (JJAS) to make sure that September is included. For the West African monsoon affecting the Sahel, September is a wetter month than June, such that a JJA-based summer average would be inadequate.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79579	72	16		18	Although, there is consensus on the recovery of rainfall over West African Sahel. Observational uncertainty is still an issue to contend with, given that the conclusion of each publication is dependent on the choice of the dataset used (as such variant opinion as to the spatial extent of the recovery). For instance, Lebel and Ali (2009) reported that precipitation over the central Sahel increased by about 10% from 1990 through 2007 compared to 1970–1989, while in the west Sahel precipitation deficit remains unchanged. While the century-long rain gauge time series of Nicholson et al., (2018) suggests more robust precipitation recovery in the western part of the Sahel than in the eastern part. Somewhat consistent with the findings by Sanogo et al., (2015). Although, trends are dependent on the period considered. The story is different when a particular dataset is used. Therefore, in the absence of rain gauge time series, an ensemble multiply gridded observations may be more robust to reduce uncertainties in individual observations and perhaps used to describe this part. In absence of that, I think a more sent study Nicholson et al, 2018 should be cited here. Nicholson, SE, Fink, AH, Funk, C. Assessing recovery and change in West Africa's rainfall regime from a 161-year record. Int J Climatol. 2018; 38: 3770– 3786. Furthermore, in a personal analysis using multiple datasets (ARC2, CHIRPS, CMAPS, CPC, CRU, GPCP, GPCC, PRECL, and UDEL), we found the robustness of precipitation trends is strongly dependent on the choice of dataset. Some datasets show spatially coherent significant increasing trends across the Sahel (1983 – 2017). The other datasets how overall weaker trends, with statistical significance, concentrated in one part of the Sahel or the other. However, an ensemble of the aforementioned datasets shows a statistically significant increasing precipitation trend over the Sahel and decreasing trend over the Guinea Coast during the June, July, August and September months. [Victor Dike, China]	Taken into account. Assessment has been performed on the Nicholson et al. (2018, IJOC) study, which is now included in the FGD. The SOD version had already considered the caveat of Lebel and Ali (2009), that increases in rainfall were mainly taking place over the central rather than western part of the domain. A sentence issuing a caveat over the choice of dataset has been added to the FGD. The FGD also takes care to refer to a "partial" recovery where appropriate.
27551	72	50	72	52	A great importance is given to the d4PDF-GE and MPI-GE ensembles in the entire chapter. Therefore the two models should better be skilful, representative etc. It should be discussed, some elements should be given etc. By the way, it would be useful to say in the legend that there is an amip and a coupled (as I understand it) ensemble. [Eric Brun, France]	Taken into account. The use of single-model initial condition large ensembles (SMILEs) has been introduced in more detail earlier in the FGD, both in 10.3.4.3 (role of internal variability) and in 10.4.1 (methodologies for regional climate change attribution). Note that it is beyond the scope of this Chapter to perform an assessment of these models themselves - that is left to the published literature. The suggestion by the reviewer to clearly identify the atmosphere-only (SST-forced) or coupled SMILEs has been made in the FGD in the figure caption and text.
116983	72		90		Cross chapter coordination is needed on regional monsoon changes; please make sure that the assessment is consistent with ch 2-3, 6 7 and 8, on the role of aerosol forcing. [Valerie Masson-Delmotte, France]	Taken into account. Coordination with the other relevant chapters has taken place to maintain consistency of the assessment among the different chapters
22933	73	1	73	9	This reads like a review rather than a synthesis and assessment. The paragraph should be rewritten in such a way as to draw out similarities and differences between the studies and not discussing each in turn. [Peter Thorne, Ireland]	Accepted. The FGD version has been considerably shortened compared to the SOD version and references the substantive assessments already made in Chapter 8. The subsection now is an assessment rather than a review.
45121	73	1	75	6	The text is too long and reads more like review. Considerable shortening of the text is suggested. Also the assessment may be improved. [Krishnan Raghavan, India]	Accepted. The FGD version has been considerably shortened compared to the SOD version and references the substantive assessments already made in Chapter 8. The subsection now is an assessment rather than a review.
17931	73	11	73	14	The work of Martin et al (2014) is largely based on Giannini et al 2013 [in Environ Res Lett] is regards to the use of a "North Atlantic Relative Index", that is, the need to consider North Atlantic sea surface temperatures in relative terms, whether relative to global or tropical warming. [Alessandra Giannini, France]	Rejected. Martin et al. (2014, J. Clim) does not cite Giannini et al. (2013, ERL). While the ERL study was published online on 18 April 2013, the Martin et al. J. Clim study was received by that journal on 23 April 2013. It seems unlikely that it could have been based on the ERL study and drafted entirely within 5 days, and we have no evidence to suggest the study is largely based on the Giannini 2013 ERL paper. Nonetheless, the Giannini ERL 2013 study is useful in this context and its citation here has been included in the FGD.
4311	73	12	73	12	I think this should be referring to changes in the SST gradient of a particular signs as opposed to just “changes in the SST gradient” [Isla Simpson, United States of America]	Not applicable. The sentence has been altered and no longer refers to the SST gradient.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115289	73	32	73	50	There is a more extensive literature on this mechanism, which would be worthwhile to cite. This includes Rotstayn 2015 (https://doi.org/10.1038/ngeo2353) and Allen 2015 (https://doi.org/10.1175/JCLI-D-15-0148.1) who both provide a multi-model overview of the relationships between aerosol changes, SST and ITCZ shifts. Allen 2015, for example, shows that only models which included aerosol-cloud processes could capture the magnitude of observed ITCZ change (and this magnitude of ITCZ change lay outside what could be explained from control climate simulations). [booth ben, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The Allen et al. (2015) study has been assessed and cited in the FGD. The reviewer incorrectly referred to Rotstayn et al. (2015), which pertains to GMST rather than the ITCZ. However, the DOI listed by the reviewer takes us to Ridley et al. (2015): Aerosol forcing of the position of the intertropical convergence zone since ad 1550, which is mostly based on a reconstructed aerosol index for Belize. Since the scope of this subsection pertains only to attribution of the late-20th century rainfall trend in WAM/Sahel, then we don't cite this study here.
59217	73	48	73	48	(Shonk et al submitted) can be updated with the Shonk et al.2019. The paper has been published online. Reference - Shonk, J.K., Turner, A.G., Chevuturi, A., Wilcox, L.J., Dittus, A.J. and Hawkins, E., 2019. Uncertainty in aerosol radiative forcing impacts the simulated global monsoon in the 20th century. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The reviewer(s) are incorrect. The online published version of the reference mentioned by the reviewer is to a preprint server, prior to peer review of the work. The final accepted version of the paper has been included for the FGD.
104573	74	2	74	4	To be more accurate, 'the amplified Saharan warming ... of Cook and Vizy (2019).' would be revised as: 'the amplified Saharan warming trend was confirmed by multiple observational and satellite-based data (Zhou and Wang, 2016; Vizy and Cook, 2017), while all the reanalysis products were revealed to significantly underestimate the warming rate (Zhou and Wang, 2016).' References: Zhou, C., and K. Wang, 2016: Land surface temperature over global deserts: means, variability and trends. J. Geophys. Res. D Atmos., 121, 2016JD025410. Vizy, E. K., and Cook, K. H. (2017). Seasonality of the Observed Amplified Sahara Warming Trend and Implications for Sahel Rainfall. J. Clim. 30, 3073–3094. doi:10.1175/JCLI-D-16-0687.1. [Chunlüe Zhou, United States of America]	Taken into account. The additional reference (Zhou and Wang) has been assessed for the FGD and cited therein, although a different form of words was used.
59219	74	3	74	3	The author has mentioned that the observations show that Saharan warming was maximum from July to October. However, the period of analysis (year range) has not mentioned in the article. It might be essential to provide complete information. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. The text pertaining to this comment has been removed, referring to July to October. Therefore no extra details have been added to the FGD.
15249	74	27			Clarify whether Dong and Stutton's results suggests that previous results on the mediterraean SST influence on Sahel rainfall were not sufficiently robust. [Piero Lionello, Italy]	Noted. Dong and Sutton did not specifically comment on the role of Mediterranean SST, although they suggested that the Sahel response to subtropical North Atlantic SST change found in previous studies was a much smaller signal than the observed trend. They also highlighted the possibility of a model dependence to the result - which should be clear by the single-model nature of their study, which is highlighted. The sentence has also been amended in the FGD to refer to North Atlantic SST. The overall narrative of the Sahel section in the FGD assesses that there are both GHG and anthropogenic aerosol contributions to the controls on Sahel rainfall decline and recovery.
84747	74	28	74	30	should fig.10.12d include also results from hist-nat? These CMIP6 results are not well embedded with published literature (i.e. Giannini and Kaplan 2018 that used CMIP5 outputs, mentioned in paragraph below) [Annalisa Cherchi, Italy]	Rejected. Hist-nat is unnecessary and would crowd the figure. Regarding CMIP6, the IPCC is limited to assessing the published literature. As many CMIP6 publications as possible pertaining to WAM/Sahel have been assessed for the FGD.
65475	74	28	74	30	It is not clear the relation between the previous sentence and the figure. Is it supporting the results of Dong and Sutton (2015)? I think that a better clarification should be added in the text. [Leandro Diaz, Argentina]	Taken into account. The position of the sentence referring to the figure has been adjusted in the FGD to avoid this misinterpretation.
125715	74	32	74	32	Giannini and Kaplan is 2019, not 2018. [Trigg Talley, United States of America]	Accepted. Due to a mistake, one of the mentions to this study was given with the incorrect year in the SOD. This has been corrected to 2019 in the FGD.
125717	74	32	74	34	The text states that the CMIP5 historical-period ensemble-mean precipitation curve of Giannini and Kaplan (2019) "largely follows observations of the decline and recovery of Sahel rainfall." This curve would seem natural to include in Figure 10.12(d). But the statement also seems to contradict the symbols above Figure 10.12(e). The CMIP5 ensemble-mean symbols show very little decline or recovery. Clarification is required to reconcile the Giannini and Kaplan (2019) result and the figure. [Trigg Talley, United States of America]	Taken into account. A caveat has been added to the FGD pointing out the smaller size of the modelled trends compared to observations. These are broadly the same between CMIP5 and CMIP6 - although the CMIP6 recovery trend is larger than that in CMIP5.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98105	74	35	74	37	You could elaborate here on Knutson and Zeng's (2018) assessment for this region on various time scales: "Knutson and Zeng (2018) concluded, based on a CMIP5 model ensemble and GPCC rainfall data, that there was a detectable (unusual compared to natural variability) Sahel drying trend over the extended period (1901-2010) to which anthropogenic forcing apparently contributed. However over the medium period (1951-2010) the Sahel drying trend was still assessed as detectable but it was in the opposite direction to the CMIP5 10-model ensemble which showed a moistening trend over that period. For the more recent "recovery" period (1981-2010) they found some gridpoints in the region have detectable moistening trends to which anthropogenic forcing contributed, but many of the gridpoint in the region did not have detectable trends over 1981-2010 according to the CMIP5 model-estimated natural variability. While their analysis points to a detectable anthropogenic influence on the trends it does not rule out a large contribution to the trends from natural internal variability. Moreover, Vellinga et al. (2016) caution… therefore internal variability may be playing an even bigger role that estimated in current models. " [Thomas Knutson, United States of America]	Rejected. The IPCC report is an assessment report, rather than a comprehensive review. Whole paragraphs on single studies cannot be included. The main point from Knutson and Zeng (2018) pertaining to our assessment has already been included, namely that a modelled long-term trend is visible over the full 20th century, although is smaller than in observations. Note that the reviewer's additional statement regarding Vellinga's paper has been corrected following intervention of another reviewer (#115287).
115287	74	37	74	40	"and therefore internal variability still plays a role." -- The Vellinga paper makes no argument about the implication of internal variability playing a role (I have no idea where this comes from). Instead, Vellinga, 2016 argue that the models lack the processes (resolution dependent) to translate historical SST changes into Sahel rainfall impacts (those SST changes could be driven by modes of internal variability or themselves be externally forced). [booth ben, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence suffix, "and therefore internal variability still plays a role" has been removed in the FGD version. An additional note on the resolution-dependent processes has been added.
115293	74	46	74	50	There is a larger literature looking at the role of near term aerosol emission reductions on Sahel rainfall. Allen, 2015b (https://doi.org/10.1002/2015JD023623) links future aerosol reduction to ITCZ shifts (linked to the same mechanism discussed in paragraph starting page 73, lines 32-50 but looking at future changes). Westervelt 2017 (https://doi.org/10.1002/2017JD026756) and Westervelt 2018 (https://doi.org/10.5194/acp-18-12461-2018) both focus on the impact of future anthropogenic aerosols, including on the Sahel. I wonder whether it is worth separating the Scannell paper out, and including a short paragraph on the future outlook drawing on these references? [booth ben, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. However, the reviewer is reminded that the scope of Section 10.4 (and case studies such as this) is purely on attribution over the period of interest within the 20th century. However, the Scannell paper does already allude to potential aerosol impacts on the near-term future. In the FGD, the relevant sentence has been made more general and also assesses this other literature.
84749	74	50	74	53	Acronyms d4PDF and MPI-GE not defined/introduced. Also what is shown in fig. 10.12e and how relevant is for regional attribution assessment is not clear [Annalisa Cherchi, Italy]	Taken into account. The dfPDF, MPI-GE (now MPI-ESM) and other model acronyms have been used sparingly in the FGD, instead we generally refer to "a coupled model", "an atmosphere-only GCM", "a large-ensemble" etc. Spelling out all model acronyms used in Chapter 10 would disrupt the flow of text. The interested reader can refer to the published reference in each case. The statement pertaining to panel (e) of the figure has been more carefully located in the FGD, within the paragraph pertaining to the importance of SSTs controlling the Sahel rainfall.
115291	74	50	74	53	I suggest moving this sentence to the earlier paragraph (page 73, line 11, perhaps). It feels odd where it is. It might be read as a concluding sentence to a wide discussion in its current paragraph -- and may be interpreted as conclusive attribtion of Sahel rainfall changes to SST rather than aerosol changes (where as these SSTs could themselves be aeosol driven -- the MPI runs lack the aerosol representation to capture these). The underlying point, however, that SSTs are likely to have a large role in understanding historical changes is worth while making. [booth ben, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. In the FGD, the relevant sentence has been moved to an earlier paragraph in which the impact of changes in SST and their gradients is discussed.
27553	74	50	74	53	For all the analyses based on d4PDF and MPI-GE in this chapter, it is supposed that the differences between their results are only the results of their differences of SSTs, neglecting the role of model formulation. It is probably true to a large extent, but it is clearly a weakness of these analyses. It is unfortunate that a completely satisfactory solution could not be found for something like the IPCC report. [Eric Brun, France]	Noted. It is the IPCC's role to assess published literature, not to produce new research. If the literature (or models on which they are based) are not published, the IPCC cannot assess them. It would be the role of the international climate modelling community to find a solution, for example a multi-model collection of large-ensembles under some coordinated experimental design.
27555	74	50	74	53	About '[…] recovery period Figure 10.12e).': It is not that clear for the recovery period. [Eric Brun, France]	Accepted. We agree that the distinction is not valid for the recovery period. Thus in the FGD the statement has been reworded to refer only to the declining period. Note that in response to comment #115291, the position of this sentence has been altered too.
45125	74	55	75	6	Check for consisency with Chapter 8 regarding the assessment summary of WAM. [Krishnan Raghavan, India]	Accepted. Discussions were held between Ch8 and Ch10 to ensure there are mutually consistent assessment statements for WAM attribution over the late 20th century in the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98107	75	2	75	6	Rephrase this slightly to say: "…the patterns of SST variability are themselves at least partly driven by anthropogenic emissions…" [Thomas Knutson, United States of America]	Rejected. The possibility of the partial role plated by anthropogenic forcing in the SST variation is accounted for in deciding the confidence level ascribed. Further qualifiers such as "at least…partly" are not required.
79335	75	9	75	9	Check links and consistency for the discussion of anthropogenic aerosol effects on East Asia monsoon with Chapter 6 and Chapter 8. [Prodromos Zanis, Greece]	Accepted. The whole EASM subsection has been removed and some relevant material merged within chapter 8, section 8.3.2.4.2
45127	75	9	77	7	The text is too long and reads more like review. Considerable overlap with Chapter 8 on the observed changes and attribution of the East Asian monsoon. The text needs to be shortened and improved. [Krishnan Raghavan, India]	Accepted. The whole EASM subsection has been removed and some relevant material merged within chapter 8, section 8.3.2.4.2
106607	75	9	77	7	This subsection is a good example of natural variability and anthropogenic drivers resulting in regional climate change so fits the remit of the section. However, there is probably too much detail for the assessment findings that it is demonstrating (though some of the material could be relevant in other regional chapters). [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The whole EASM subsection has been removed and some relevant material merged within chapter 8, section 8.3.2.4.2
80333	75	10	78	10	Monsoon acronyms need to be in agreement with those used in other chapters. CH8 uses EAsiaM. [Paola Arias, Colombia]	Taken into account: the whole EASM subsection has been removed and relevant material merged within chapter 8, section 8.3.2.4.2. EAsiaM has been used.
125719	75	12	75	14	"Figure 10.13(c) shows ... smaller than -30 mm month-1 …" is confusing. Figure 10.13(c) shows trends in the regional precipitation difference over the 44-year period, not the actual regional difference. What's being referred to in this sentence? [Trigg Talley, United States of America]	Not applicable: the figure has been removed and parts of the EASM subsection merged within Chapter 8
4313	75	12	75	14	This is a very confusing sentence. Suggest rather than "smaller than -30mm/month" refer to "a decline of more that 30mm/month" (I think that is what is meant). Also suggest "precipitation differences from observations" --→ "observed precipitation differences" because at the moment it sounds like it is modelled differences from observations that are being referred to. [Isla Simpson, United States of America]	Not applicable: the figure has been removed and parts of the EASM subsection merged within Chapter 8
38129	75	25	75	44	The areas in Figures 10.3, 10.11, and 10.13 are too small to represent East Asia. It's just East China. [Junhee Lee, Republic of Korea]	Not applicable: the figure 10.13 has been removed and parts of the EASM subsection merged within Chapter 8
78755	75	47	75	48	The importance of various contributing factors is different with Chapter 8. [jian li, China]	Accepted. The whole EASM subsection has been removed and relevant material merged within chapter 8, section 8.3.2.4.2.
84753	75	47	76	17	paragraph to be harmonized in terms of assessment of interdecadal SST changes (PDV, AMV, Indian and Pacific Oceans). Also, it is more a review than an assessment [Annalisa Cherchi, Italy]	Not applicable: the whole EASM subsection has been removed and relevant material merged within chapter 8
38127	75	47	76	17	Please insert the sentence below in 10.4.1.2.2. In addition, central Pacific type El Niño, which has appeared frequently since the 1990s, also influences monsoon change (Lee and McPhaden, 2010). [Junhee Lee, Republic of Korea]	Not applicable: the whole EASM subsection has been removed and some relevant material merged within chapter 8
4315	75	53	75	53	suggest being a bit more specific about what SSTs are causing the land-sea temperature contrast change since different SST anomalies have previously been mentioned. I assume it's the warmer tropical SSTs that are relevant here and not the cooler North Pacific? [Isla Simpson, United States of America]	Not applicable: the whole EASM subsection has been removed and relevant material merged within chapter 8
71239	75				Figure 10.13, delete the fourth word (from) in your caption, it isn't necessary. [Nesha Wright, Canada]	Not applicable: the figure has been removed and the EASM subsection merged within Chapter 8
71241	75				Figure 10.13, the units are in your figure so I am not sure they need to be in your caption, just makes the caption longer. [Nesha Wright, Canada]	Not applicable: the figure has been removed and the EASM subsection merged within Chapter 8
71243	75				Figure 10.13, in the caption (c) is awkwardly worded. Perhaps " Distribution of difference in summer precipitation trends" instead? [Nesha Wright, Canada]	Not applicable: the figure has been removed and the EASM subsection merged within Chapter 8
13593	76	5	76	5	Change ,2013) In by ,2013). In…. [Maria Amparo Martinez Arroyo, Mexico]	Not applicable: the whole EASM subsection has been removed and relevant material merged within chapter 8
43291	76	5			Read "EASM (Ding et al., 2009; Duan et al., 2013; Si and Ding, 2013). In addition to the PDV influence, the " rather than "EASM (Ding et al., 2009; Duan et al., 2013; Si and Ding, 2013) In addition to the PDV influence, the " [Cyriaque Rufin Nguimalet, Central African Republic]	Not applicable: the whole EASM subsection has been removed and relevant material merged within chapter 8.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
95847	76	19	76	20	While considering the role of anthropogenic aerosols, it seems important to mention the existence of the Asian Tropopause Aerosol Layer (ATAL) that constitutes a pathway for the transport of anthropogenic aerosol aerosol into the lower stratosphere (Vernier et al. (2015), J. Geophys. Res., doi:10.1002/2014JD022372; Yu et al. (2017), PNAS, doi:10.1073/pnas.1701170114). The role of anthropogenic aerosols in the EASM has been widely studies during the StratoClim campaign (Brunamonti et al. (2018), Atmos. Chem. Phys., doi:10.5194/acp-18-15937-2018; Lee et al.(2019), Atmos. Chem.Phys., doi:10.5194/acp-19-11803-2019), and showed their role to hydrate the tropical tropopause layer and the lower stratosphere. [Christine Bingen, Belgium]	Noted. The whole EASM subsection has been removed and relevant material merged within chapter 8, section 8.3.2.4.2
21097	76	32	76	34	In addition to the weakening EASM circulation under historical aerosol forcing, Li et al. (2015) further diagnosed the decreasing Asian monsoon rainfall under aerosol forcing in CMIP5 models and revealed the contributions of both thermodynamics (reduced moisture availability) and dynamics (weakening monsoon circulation) in the drying trend. It would be good to include some physical explanations on the decreasing EASM rainfall under aerosol forcing. [Li X, Ting M, Li C, et al. Mechanisms of Asian summer monsoon changes in response to anthropogenic forcing in CMIP5 models[J]. Journal of Climate, 2015, 28(10): 4107-4125.] [Wenxia Zhang, China]	Noted. The whole EASM subsection has been removed and relevant material merged within chapter 8, section 8.3.2.4.2
21099	76	37	76	39	A recent attribution study revealed the different anthropogenic influence on different types of heavy rainfall in East Asian monsoon region depending on event time scales. In particular, anthropogenic forcing has reduced the risk of summertime persistent heavy rainfall in East Asia, but increased the risk of short-term extreme rainfall. The reduced persistent heavy rainfall is associated with weakening EASM under aerosol forcing (Zhang et al. 2020). Thus I suggest to modify the statement as: "Although the anthropogenic forcing has led to an overall decrease in total monsoon rainfall and persistent heavy rainfall events, model simulations suggest that the most extreme heavy rainfall events become shorter in duration and more intense (Burke and Stott 2017b; Zhang et al. 2020)." [Zhang W, Li W, Zhu L, et al. Anthropogenic Influence on 2018 Summer Persistent Heavy Rainfall in Central Western China[J]. Bulletin of the American Meteorological Society, 2020, 101(1): S65-S70.] [Wenxia Zhang, China]	Not applicable: the whole EASM subsection has been removed and relevant material merged within chapter 8, section 8.3.2.4.2. Changes in extremes are dealt with in chapter 11.
84755	76	46	76	48	where is weaker than in observations? [Annalisa Cherchi, Italy]	Not applicable: the whole EASM subsection has been removed and relevant material merged within chapter 8
78757	76	47	76	51	The importance of various contributing factors is different with Chapter 8. [jian li, China]	Taken into account: the whole EASM subsection has been removed and some relevant material merged within chapter 8, section 8.3.2.4.2
84757	76	49	76	49	what ensembles? [Annalisa Cherchi, Italy]	Not applicable: the whole EASM subsection has been removed and relevant material merged within chapter 8
98109	76	53	77	1	Recommend you elaborate on the Knutson and Zeng (2018) assessment for this region as follows: “Knutson and Zeng (2018) found little evidence for a large-scale anthropogenic weakening of the EASM based on the comparison of observed precipitation trends over 1901-2010 with CMIP5 ensemble trends. They found that the CMIP5 historical run ensemble does not well match the pattern of southern moistening and northern drying over either the period 1951-2010 or 1981-2010, implying that it is difficult for the CMIP5 model ensemble to simulate the moistening/drying pattern as a forced response. In addition, they found that the precipitation trends in the region were mostly not detectable compared to CMIP5 model natural variability, either over 1951-2010 or 1981-2010. This further supports the notion that internal variability has dominated over anthropogenic influence in this region, at least in terms of precipitation trends over the past century. “ [Thomas Knutson, United States of America]	Not applicable: the whole EASM subsection has been removed and relevant material merged within chapter 8
69949	76	55	77	1	I would like to add result of Shim et al.(2019). According to Shim et al.(2019)*, temperature change over 1901~2005 in East Asian Monsoon region is characterized into 3 phases for each 30-year and this change depends on intensity of GHG, Aerosol forcing and their interaction. That is, this paper supports that non-linear interaction between anthropogenic forcing(GHG, Aerosol) is also related to change in temperature over the East Asian Monsoon region. * Shim, S., J. Kim, S. S. Yum, H. Lee, K.-O. Boo, and Y.-H. Byun, 2019: Effects of Anthropogenic and NAtural Forcings on the Summer Temperature Variations in East Asia during the 20th Century. Atmosphere, 10, 690; doi:10.3390/atmos10110690. [Young-Hwa BYUN, Republic of Korea]	Noted. The whole EASM subsection has been removed and relevant material merged within chapter 8, section 8.3.2.4.2
21101	77	1	77	1	It might be useful to note that, associated with the decadal weakening of EASM circulation, the East Asian monsoon extreme rainfall also exhibited an increase in the south and a decrease in the north since 1950s which resembles the mean SFND pattern (Zhang and Zhou 2019). This can be viewed as evidence of mean monsoon circulation affecting the monsoon extremes. [Zhang W, Zhou T. Significant increases in extreme precipitation and the associations with global warming over the global land monsoon regions[J]. Journal of Climate, 2019, 32(24): 8465-8488.] [Wenxia Zhang, China]	Rejected. Extremes are dealt with in chapter 11. Note that the whole EASM subsection has been removed and relevant material merged with chapter 8

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22937	77	3	77	5	It feels odd to lead with what you are least confident in and to phrase it in this manner. The text would support leading with the point about PDV and rephrasing the anthropogenic statement to stress the second component over the first. [Peter Thorne, Ireland]	Noted. The whole EASM subsection has been removed and relevant material merged within chapter 8, section 8.3.2.4.2
39225	77	3	77	7	How do you differentiate between the assessment of high confidence (robust evidence and meium agreement) in lines 3-4 and that of high confidence (robust evidenceand high agreement) in lines 6-7? [Lourdes Tibig, Philippines]	Not applicable: the whole EASM subsection has been removed and relevant material merged within chapter 8
125721	77	9	77	9	Why not mention bushfires in the section of regional climate change and drying in Australia? Seems like it deserves a mention even without an attribution. [Trigg Talley, United States of America]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
45129	77	9	79	18	This pertains to the case study of rainfall decline over southern Australian during the recent decades. It is a good example of regional climate change attribution and doesn't overlap with Chapter 8. However, the text is too long and reads more like review. The text needs to be shortened. [Krishnan Raghavan, India]	Not Applicable. The whole southern Australia subsection has been removed due to shortening constraints
106609	77	9	79	18	This subsection does not really add anything to the demonstration of methodologies for constructing regional information or messages and so suggest relevant material is moved to the Atlas (assuming it is not there already). [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The whole southern Australia subsection has been removed due to space constraints.
44987	77	9	79	20	In addtion to Grose et al., 2019, please check Grose et al. 2020. "Insights From CMIP6 for Australia's Future Climate", Earth's Future, 8, e2019EF001469. [Mustafa Tufan Turp, Turkey]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
84761	77	11	77	11	and what is the summary of the assessment done in AR5? [Annalisa Cherchi, Italy]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
84759	77	16	77	24	not clear need of this introduction on the description of climate for the region (it should be part of the Atlas and could be referenced there) [Annalisa Cherchi, Italy]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
82701	77	17	77	17	Are these maximum or mean temperatures? Either could be valid depending on how "southern Australia" is defined. (There is perhaps a limited need to quote the specific values since this section is mostly not about temperature). [Blair Trewin, Australia]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
30661	77	26	77	30	Important point here in connection with the poleward expansion of the tropical belt and the southward shift of the transients. Rudeva, I. et al., 2019: Midlatitude fronts and variability in the Southern Hemisphere tropical width. J. of Clim., 32, 8243-8260, doi: 10.1175/JCLI-D-18-0782.1 present evidence that the latter are leading the former. ie changes partially driven by the midlatitudes [Ian Simmonds, Australia]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
30663	77	32	77	39	Valuable to round out this aspect by referencing Rehman, S. U. et al., 2019: Links between Tasmanian precipitation variability and the Indian Ocean subtropical high. Theor. Appl. Climatol., 138, 1255–1267, doi: 10.1007/s00704-019-02891-z who relate decreases in Tasamina rainfall to changes in the strength and position of the South Indian Ocean subtropical high. [Ian Simmonds, Australia]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
70919	77	38	77	39	An important reference for this point is Byrne et al. (2017 doi: 10.1175/JCLI-D-17-0097.1) who showed that there has been substantial aliasing between the Antarctic polar vortex variability and ENSO during the reanalysis record, which confounds the observed relation between ENSO and the SAM. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
54399	77	42	77	42	As the whole case study deals with precipitation trend from 1960 to 2014, I suggest that the temperature trend should also be within this frame, otherwise it's not comparable and therefore merely informative [Gabriel Stachura, Poland]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
82703	77	42	77	45	The ACORN-SAT data set starts in 1910 so the quoted trends will be for 1910-2018, not 1900-2018 (unless a different data set is used). The most current reference for the data set (replacing Trewin 2013) is Trewin et al 2020 (Geoscience Data Journal, in press). [Blair Trewin, Australia]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
84763	77	48	77	48	the assessment of the case of the southern Australia rainfall decline could directly start from here, the paragraphs before could be drastically reduced or removed as out of scope or redundant for section 10.4.1.2.3 [Annalisa Cherchi, Italy]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
54401	77	48	78	12	Similar to above comment - as the Figure 10.14 depicts rainfall from 1960-2014, I see no reason to mention previous years as well a subsequent years (as in line 54). Or maybe you could broaden the time window of Figure 10.14a [Gabriel Stachura, Poland]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
125723	77	49	77	51	There looks to be large multi-decadal variability in rainfall in this region. Stating that there was a downward trend in southwestern Australian rainfall since the 1960s is a bit misleading, as Figure 10.14(d) clearly shows that there was a rainy period in this region in the 1990s. [Trigg Talley, United States of America]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
82705	77	53	77	54	The wet spring/summer periods associated with La Nina were 2010-11 and 2011-12. There was then a further wet period in winter/spring 2016 associated with a strongly negative Indian Ocean Dipole, with little ENSO influence. [Blair Trewin, Australia]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
125725	78	2	78	4	This statement is incorrect as written. The ozone hole (which is clearly high latitude) has its largest influence on southeastern Australian precipitation during DJF (summer). See Thompson et al., 2011 (https://www.nature.com/articles/ngeo1296/). [Trigg Talley, United States of America]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
30665	78	5	78	5	Additional reference of value here is Meneghini, B. et al., 2007: Association between Australian rainfall and the Southern Annular Mode. Int. J. Climatol., 27, 109-121, doi: 10.1002/joc.1370. [Ian Simmonds, Australia]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
43293	78	45			Read "Delworth and Zeng (2014) found that the observed " rather than "(Delworth and Zeng, 2014) found that the observed " [Cyriaque Rufin Nguimalet, Central African Republic]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
109411	78	47	78	47	please specify "stratospheric" ozone [Sophie Szopa, France]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
4317	78	50	78	50	Perhaps it should be mentioned that it's surprising that ozone should have that much of an influence on the Autumn? [Isla Simpson, United States of America]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
70921	78	54	78	55	Mindlin et al. is now published (2020): doi: 10.1007/s00382-020-05234-1 [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
98111	78	56	79	2	Add to the second sentence about Knutson and Zeng (2018) as follows: "It also suggests a detectable decrease in annual precipitation in near-coastal southeast Australia extending into Tasmania (1951-2010 trends), and detectable anthropogenic wetting annual mean trends in northern Australia over various periods (1901-2010, 1951-2010, and 1981-2010)." [Thomas Knutson, United States of America]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
84765	79	5	79	5	not clear if what shown in fig. 10.14 is something new/updated with respect to the outputs in Delworth and Zeng (2014) [Annalisa Cherchi, Italy]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
82707	79	9	79	9	Replace "spring" with "September" (the paper was about September, and spring as a whole did not set records, except very locally). [Blair Trewin, Australia]	Not applicable. the whole southern Australia subsection has been removed due to shortening constraints
125727	79	10	79	11	"... globally observed SST trends that probably have a component of anthropogenic climate change in them." This appears to say that a role for anthropogenic forcing in positive global-SST trends is only "probable", when in fact there is high confidence for such a role. This may not have been the intent of the sentence. It needs rewording at least. [Trigg Talley, United States of America]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
131413	79	15	79	18	Rainfall and precipitation are used intermingeld throughout the section, which is fine for Australia I guess. In the confidence statements this is a little bit confusing though. It is not clear if this is a consciouss destinction between the two. [Hans Poertner and WGII TSU, Germany]	Not applicable. The whole southern Australia subsection has been removed due to shortening constraints
125729	79	21	81	32	A more balanced discussion of the roles of greenhouse gases and stratospheric ozone depletion needs to be provided. As written, stratospheric ozone depletion is only briefly mentioned, and it is well known that it plays a key role in recent Southern Hemisphere climate change during the DJF season. [Trigg Talley, United States of America]	Accepted. The text now emphasizes that the anthropogenic forcing includes both GHG emissions and stratospheric ozone depletion. In particular the text has been re organized so that the SOD p80, l32: "However, in contrast to these findings, other studies have attributed the positive precipitation trend to anthropogenic GHG emissions." has been changed to "The positive trend has also been attributed to anthropogenic forcing including GHG emissions as well as stratospheric ozone depletion."

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
100413	79	21	81	32	The vast majority of the text is focused on Argentina and LPB features. Extend further to other areas of the SES [Lincoln Alves, Brazil]	Not applicable. In the SOD, the first paragraph of the section (p79, l22-35) made two references specific to Argentina while the rest of the section treats the entire South-eastern South America. The introduction has been shortened for the FGD due to space limitations with the result that these country-specific references are not longer made.
100415	79	21	81	32	A lot of references before 2013 (eg.: Rusticucci and Penalba, 2000; Zak et al., 2008; Barros et al., 2004). Recommended to cite recent literature. [Lincoln Alves, Brazil]	Accepted. All these references has been removed, some of them due to shortening of the text.
100417	79	21	81	32	Reads like a scientific literature review paper not an assessment. The assessment is not clear in many paragraphs. Recommended reformulation of subsection. [Lincoln Alves, Brazil]	Accepted. The text has gone through substantial reformulation.
45131	79	21	81	32	The text is too long and reads more like review.. The text needs to be shortened. [Krishnan Raghavan, India]	Accepted. The text has gone through substantial reformulation and shortening.
106611	79	21	81	32	This is a good example of warming and circulation changes and influences driving a regional change and the difficulty of quantifying contributions which should be made clear nad perhaps does not need the level of detail to demonstrate these findings. Need also to check if some of this material would be relevant in the Atlas. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text has gone through substantial reformulation and shortening.
84043	79	21			At the begging of the chapter the authors mention that the specific regions mentioned here are not always equivalente to what is later on stablished for the other chapters in the report, but considering that the South-Southeastern South America region defined by AR6 WGI at the Atlas chapter, for example, is much larger than the region that the majority of studies here mentioned deal, mostly Argentina. Wouldn't it be more appropriate for this section title to be "south south-eastern South America summer wetting"? [Marco Tulio Cabral, Brazil]	Not applicable / Rejected. This comment is partly not applicable in the FGD since all explicit mentions of Argentina are no longer present in the text and the introduction paragraph (SOD p79, l22-35) has been heavily shortened due to space limitations. Furthermore, the comment is rejected since the wettening of the region is known in the literature as the wettening of "South-eastern South America". To distinguish from the AR6 region SES we do not use that acronym.
108135	79	22	79	23	I suggest rephrasing the statement to be consistent with Figure Atlas.39. b where trends in annual precipitation over Southeastern South America are not significant for the 1980-2014 period. The term “robust” seems to be somehow contradictory from this point of view. [Maria Bettolli, Argentina]	Accepted. The introduction sentence has been changed to "A robust positive trend in summer (December-February) precipitation has been detected in south-eastern South America since the beginning of the 20th century".
54403	79	40	79	40	The subtitles to each chart are doubled, it applies particularly to 10.15a) [Gabriel Stachura, Poland]	Accepted. Subtitles have been changed.
54405	79	40	79	40	Regarding Figure 10.15d - please give some colours to the mean ensamble and horizontal trends. Otherwise it's hard to notice them and it's hard to see, which line refers to which chart [Gabriel Stachura, Poland]	Not applicable. Panel d has changed.
108137	79	41	79	43	Figure 10.15 (b), Is it possible to indicate the significance level of the trends? [Maria Bettolli, Argentina]	Rejected. The case studies (including region, variable and period of trend) have been chosen because many studies have confirmed significant observed trends for these specific case studies. Therefore we focussed the assessment on the different mechanisms contributing to the trend, the ability of different modelling tools in reproducing them, and on the uncertainty in the modelling of the trends.
23223	79	45	79	47	Cross-reference to the chapter 10 case study here? [Peter Thorne, Ireland]	Editorial – copyedit to be completed prior to publication
84045	80	1	80	1	Borges et al. 2014 (DOI 10.1007/s00704-013-0947-4) can contribute to the evidence of increase of precipitation amounts in the rainy season. [Marco Tulio Cabral, Brazil]	Rejected. The suggested publication treats a small region north of 20˚S, this region does not form part of the region treated here.
22941	80	4	80	6	Be aware that CRUTS reverts to climatology with no observational constraint. All grid boxes so flagged should be removed and then the series recalculated if not already done so. The flags are in the data files as a second field. This comment will apply to any other application of the CRUTS series. [Peter Thorne, Ireland]	Accepted. Where relevant, masking has been applied to GPCC and CRUTS in all figures of the chapter.
108139	80	8	80	8	I suggest adding the following reference that evaluate trends in extreme events: 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, Minor revisions needed. [Maria Bettolli, Argentina]	Rejected. The phrase already has many references.
4319	80	32	80	32	It doesn’t seem like studies discussing the role of GHG’s need necessarily be in contrast with those that discuss the influence of North Atlantic SST variability because anthropogenic greenhouse gas emissions could have contributed to e.g., warming of the tropical Atlantic. If that is a fair statement then “in contrast to these findings” could just be deleted. [Isla Simpson, United States of America]	Accepted. “in contrast to these findings” has been deleted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
108141	80	32	81	1	I suggest adding additional references of works that analyzed the performance of GCMs in reproducing precipitation trends over SESA: Maenza R, Agosta Scarel EA, Bettolli ML. 2017. Climate change and precipitation variability over the western "Pampas" in Argentina. International Journal of Climatology 37: 445–463. doi: 10.1002/joc.5014 Marengo et al 2010 An intercomparison of observed and simulated extreme rainfall and temperature events during the last half of the twentieth century: part 2: historical trends. Climatic Change (2010) 98:509–529. DOI 10.1007/s10584-009-9743-7 [Maria Bettolli, Argentina]	Rejected. Due to space constraint the section has been shortened and in particular the assessment of model performance could not be expanded further.
108143	80	32	81	11	I suggest rephrasing the paragraphs in terms of assessment report text style, instead of indicating individual papers results. [Maria Bettolli, Argentina]	Accepted. Text has been rewritten.
98113	80	41	80	41	Elaboration on Vera and Diaz (2015): "Their results were supported for the region by Knutson and Zeng (2018) based on univariate detection/attribution analysis of annual mean trends for the 1901-2010 and 1951-2010 periods." [Thomas Knutson, United States of America]	Accepted. Text and reference have been included.
4321	80	44	80	44	"a ensemble" → "an ensemble" [Isla Simpson, United States of America]	Editorial – copyedit to be completed prior to publication
109413	80	55	80	55	please specify "stratospheric" ozone [Sophie Szopa, France]	Accepted. "stratospheric" ozone has been specified throughout the section.
70923	81	3	81	3	Mindlin et al. is now published (2020): doi: 10.1007/s00382-020-05234-1 [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Reference has been updated.
112859	81	3	81	3	Mindlin et al. Has already been published: Mindlin, J., Shepherd, T.G., Vera, C.S., Marisol, O., Giuseppe, Z., Lee, R.W. and Hodges, K.I., 2020. Storyline description of Southern Hemisphere midlatitude circulation and precipitation response to greenhouse gas forcing. Climate Dynamics, 54(9-10), pp.4399-4421. [Paula Gonzalez, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Reference has been updated.
22943	81	13	81	21	That the real world may be an extreme manifestation of natural variability is implied but never said here. My feeling is it would be worth stating explicitly? [Peter Thorne, Ireland]	Taken into account. Thanks for this comment, it revealed that the text was biased towards the Seager et al. 2010 study, who quantified the contribution of the AMV, but using the tools of that time. Since then, literature based on more solid and modern evidence is supporting the role of anthropogenic forcing, through various lines of evidence such as multi-model-means, storylines, univariate detection and attribution. The text has been changed in the following way (as a result of this and other comments as well as cross-chapter discussions): The sentence "They concluded that a large part of the wetting trend from the mid-20th century was forced by cooling of the tropical Atlantic resulting from the AMV cold phase (Seager et al., 2010)." has been removed since the study of Seager is based only on one model. Furthermore, as a result of harmonization with the other chapters, the final assessment statement is made on the role of anthropogenic forcing (GHG and stratospheric ozone),instead of as for the SOD separating these and is now: "There is high confidence that anthropogenic forcing has contributed to the south-eastern South America summer precipitation increase since 1950, but very low confidence on the relative contribution of each driver to the precipitation increase."
109415	81	26	81	26	please specify "stratospheric" ozone [Sophie Szopa, France]	Accepted. "stratospheric" ozone has been specified throughout the section.
88835	81	28	81	28	This would be more precise if it says "stratospheric ozone depletion" as in page 81, line 9.Also in the ES? [Krishna AchutaRao, India]	Accepted. "stratospheric" ozone has been specified throughout the section and also in the ES.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45137	81	35	84	34	The text presented in "The central and eastern Eurasian winter cooling" contains useful scientific and policy-relevant information. The text is too long and reads more like review. It will be great if the text can be synthesized and presented as a shortened assessment. [Krishnan Raghavan, India]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
111579	81	35	84	34	This 3-page section on recent winter cooling in Eurasia (plus figure 10.16) is too much and should be shortened significantly. Moreover, all text is rather review of existing literature and not assessment of the phenomenon cause with supporting evidence in literature. But the most dangerous on my opinion to maintain very bad "habit" to calculate trends on very short time periods. It already cost IPCC a lot of problems in communication since many politicians, journalists and others are still refer to hiatus from 1998 to 2012 and don't want to read AR5 SPM where explaned "trends based on short records are very sensitive to the beginning and the end dates.." I would add - and should not be used in climate study. Here even shorter period trends are discussed and it should be clear stated that it is not climate but interannual variability. Moreover, please, look at Regional ES on North Asia in Atlas where trend values are presented and explained. The region of Eurasia in Ch.10 corresponds to at least 3 subregions of AR6 reference regions - EEA, WSB and ESB. In Interactive Atlas observational dataset Berkeley (as like as others) for period 1980-2014 gives the highest positive trends in mid-latitudes of Eurasia for EEA that contradict with stated in this subsection [Volodymyr Osadchy, Ukraine]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
7935	81	35	84	34	It seems a bit out of balance to spend 3 pages on this winter cooling phenomenon, given the large divergence in hypotheses and results [Bart van den Hurk, Netherlands]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
82709	81	36	81	43	Somewhere in this section it should be mentioned that warming has resumed since 2014 (2020 has been particularly extreme). This is more obvious in Figure 10.12 (with its extra few years of data) than in Figure 10.16. [Blair Trewin, Australia]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
125731	81	37	81	37	It is not accurate to say that Central Eurasian cooling ended in 2014. Central Eurasian cooling, or at least below normal temperarures, lasted until 2019 (see SI Figure 3d in Cohen, 2020). Winter 2019/20 was obviously a warm winter but still too early to say the trend is broken. [Trigg Talley, United States of America]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
54407	81	48	81	48	The subtitles to each chart are doubled. The majority of charts in the chapter has only letters beside a chart, description is only at the bottom. It should be unified [Gabriel Stachura, Poland]	Not Applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
71245	81				Figure 10.16, the bar graph in (b) of your image are too dark, maybe use the same shade you use in Figure 10.15 (c)? [Nesha Wright, Canada]	Not applicable, Figure 10.16 has been removed for the FGD.
79173	82	14	82	15	Denoting hiatus without quotation may attract criticism. "the so-called "hiatus" in global mean surface temperature" would be better. Also please cite Cross-Chapter Box 3.1. [Yu Kosaka, Japan]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
79175	82	19	82	19	Section 10.3.2.4 -> Section 10.3.2.3 [Yu Kosaka, Japan]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
30667	82	23	82	31	The role of blocking in the Ural region has recently attracted much attention in connection with Eurasina cold temperatures. This features tend to 'anchor' the teleconnections patterns from the Artic to Eurasia enhancing the persistence of the cold events. References to cited here are: Li et al., 2020: Anchoring of atmospheric teleconnection patterns by Arctic sea ice loss and its link to winter cold anomalies in East Asia. International Journal of Climatology, doi: 10.1002/joc.6637 AND Luo et al., 2019: The winter midlatitude-Arctic interaction: Effects of North Atlantic SST and high-latitude blocking on Arctic sea ice and Eurasian cooling. Climate Dynamics, 52, 2981-3004, doi: 10.1007/s00382-018-4301-5 AND Luo et al., 2016: Impact of Ural Blocking on winter Warm Arctic–Cold Eurasian anomalies. Part II: The link to the North Atlantic Oscillation. Journal of Climate, 29, 3949-3971, doi: 10.1175/JCLI-D-15-0612.1. [Ian Simmonds, Australia]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
54409	82	33	83	40	I find the discussion too long - there could be less examples of studies, since there are several in every type of "result". They could be grouped more concisely [Gabriel Stachura, Poland]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
84767	82	48	83	40	too long, and it is more a review than an assessment [Annalisa Cherchi, Italy]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
79177	82	48	83	40	The paragraph is too long. Why don't you split it into two at P83L8? [Yu Kosaka, Japan]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
22945	82	48	83	40	This very long paragraph is entirely a play by play review. There is no attempt made at synthesis and assessment. It would help the reader enormously if you could synthesise this drawing out where there is agreement and where there is disagreement rather than describing each study in turn. [Peter Thorne, Ireland]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
83379	82	48	83	40	This is a very long paragraph that is difficult to follow. [Robert Massom, Australia]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30669	82	49	83	40	This is a very long paragraph and it covers a number of complex ideas. Much better to break this up into two or three paragraphs at some appropriate ponts. [Ian Simmonds, Australia]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
70925	82	56	83	5	Zappa et al. (https://arxiv.org/abs/1911.10777) submitted a Comment on Mori et al. (2019), which is still languishing with NCC. It argues that MCA was misapplied by Mori et al. and that the results are fundamentally flawed. You might want to consider the arguments made in the Comment. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Not Applicable. Thanks, but the whole Eurasian cooling subsection has been removed due to shortening constraints
116987	82		84		The text on Eiropean blcoking and Arctic sea ice is very long and duplicates box 10.1. [Valerie Masson-Delmotte, France]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
79179	83	42	83	46	Trenberth et al. (2014) prescribed diabatic heating rather than SST. They argued remote influence from the tropical Pacific varibility to northern high latitude, but not focusing on Central-Eastern Eurasia. Their model simulation (their Fig. 6a) does not show a strong response in Central-Eastern Eurasia, and I am afraid that this is not a relevant citation for this section. [Yu Kosaka, Japan]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
125733	83	43	83	43	"blamed" is an odd word here. "caused by" or "attributed to" might be better. [Trigg Talley, United States of America]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
4191	83	55	84	6	Relevant researches about Eurasian blocking linked with Arctic sea ice decline are mentioned in the view of observation and reanalysis, but cannot elaborate the causality. Recently, the theoretical researches by Luo et al. (2019, JAS, "A Nonlinear Theory of Atmospheric Blocking: A Potential Vorticity Gradient View") shed light on the internal physics of Arctic warming affecting the planetary-scale waves. They think the Arctic warming can weaken the meridional gradient of potential vorticity, which weaken the dispersion of the blocking system, hence the blocking can maintain a long lifespan. I think it's meaningful for societies and researchers to understand the causal linkage of the blocking variance and Arctic warming and this theory is based on rigorous mathematical foundation. [Wenqi Zhang, China]	Not Applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
4185	84	6	84	9	According to the study of Luo et al. (2017), the spatial cooperation of positive-phase NAO and Ural blocking is the most favorale pattern causing the BKS sea ice loss by transporting the Atlantic water vapor into BKS region. This work is significant and I thinkl it is desirable refered. (Luo et al. 2017, Atmospheric circulation patterns which promote winter Arctic sea ice decline.) [Wenqi Zhang, China]	Not Applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
125735	84	7	84	11	Another important reference here is Hegyi and Taylor (2018; doi: 10.1029/2017GL076717) as they illustrate the influence of moisture intrusions on sea ice and downwelling LW radiation using satellite observations. [Trigg Talley, United States of America]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
30671	84	14	84	26	Important points are made here in connection with the conditional amplification and (in the next paragraph) episodic behavior. Recent work is now making clear some of the conditions which must be in place before amplification (and teleconnections) can occur. Luo et al., 2019 (Weakened potential vorticity barrier linked to recent winter Arctic sea ice loss and midlatitude cold extremes. J. Clim., 32, 4235-4261, doi: 10.1175/JCLI-D-18-0449.1) demonstrate the role of the 'on/off' role played by the subarctic potential vorticity distribution. Another backgound feature which plays an analagous role is the Atlantic Multidecadal Oscillation (see paper of Luo, D., and co-authors, 2017: Winter Eurasian cooling linked with the Atlantic Multidecadal Oscillation. Env. Res. Letts, 12, 125002, doi: 10.1088/1748-9326/aa8de8). Cite these papers in this context. [Ian Simmonds, Australia]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
22947	84	21	84	21	tremendous is value laden. Please find a different word such as substantial. [Peter Thorne, Ireland]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
30673	84	21	84	21	I don't like the word 'tremendous' here. Something like 'large, 'significant', 'considerable' or 'substantial' would be much more appropriate' [Ian Simmonds, Australia]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
10991	84	28	84	28	Why is this link highlighted as 'episodic' in the summary? I didn't get that particularly from the preceding discussion, though I know it has been suggested (eg https://doi.org/10.5194/wcd-2019-11) [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79181	84	30	84	32	Deser et al. (2017a)'s result indicates that there is comparable influence from coupled variability in the tropical Pacific and atmospheric internal variability to the cooling, according to the text. Together with Mori et al. (2019)'s estimate, "at least 50%" sounds too strong. How about "50% or more"? [Yu Kosaka, Japan]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
111581	84	33	84	33	It is not clear after all discussion why and of what robust evidence reported here. But the whole paragraph is pretty acceptable conclusion on the problem [Volodymyr Osadchy, Ukraine]	Not applicable. The whole Eurasian cooling subsection has been removed due to shortening constraints
45141	84	36	86	18	The subsection "10.4.1.2.6 Western Europe summer warming" contains useful scientific and policy-relevant information. The text is long and can be shortened. [Krishnan Raghavan, India]	Not applicable. The whole Western Europe subsection has been removed.
106613	84	36	86	18	This subsection does not really add much to demonstrating the construction of regional information or messages and any additional points could be made much more succinctly so suggest removing or cutting back and merging elsewhere.. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The whole Western Europe subsection has been removed.
105883	84	36	86	18	Overall, I like this sub-section. To the point and clear with clear final statements. The word GHG or anthropogenic-induced climate change is possibky missing in the final statement paragraph. It is hidden (too much hidden for some readers) behind « lapse-rate feedback » [SAMUEL SOMOT, France]	Not applicable. The whole Western Europe subsection has been removed.
54411	84	37	84	37	I can't see any 10.17d, there's only a-c [Gabriel Stachura, Poland]	Not applicable: the Figure 10.17 has been removed.
45139	84	37	84	39	There are several references prior to AR5, Possibility of reducing the number of "pre-AR5" references may be considered. [Krishnan Raghavan, India]	Not applicable. The whole Western Europe subsection has been removed.
15251	84	37			delete second word European and rephrase " Rapid summer warming has […] increase (van Oldenborgh et al., 2009) in western and central Europe and in the Mediterranean.", In fact, most of Europe (its eastern parts) was not affected by the warming [Piero Lionello, Italy]	Not applicable. The whole Western Europe subsection has been removed.
109763	84	42	84	42	"last three decades" is ambigious, suggest to provide exact time period used in Luterbacher et al. [Flavio Lehner, Switzerland]	Not applicable: the whole Western Europe subsection has been removed.
105855	84	48			the introduction of the section mentions clearly the last 30 years as the fast warming period, Fig 10.11 and 10.17 also underline the period from the 1980s. Therefore I don't understand why the trend computation in Fig 10.17 are based on the 1950-2014 period. Personnaly, I think that the 1980-2010 or 1985-2014 or 1980-2020 periods would be quite more relevant for this case. This would better fit with the scientific focus of section 10.4.1.2.6 [SAMUEL SOMOT, France]	Not applicable: the whole Western Europe subsection has been removed due to shortening
105857	84	48			Following Nabat et al. 2014, Drugé et al. 2019, Nabat et al. 2020 and Boé et al. 2020 (see previous comments for the ref), I would apreciate to see RCM outputs (both evaluation and historical-scenario runs) on Fig 10.17 to show there ability to reproduce past trends for this case. This may include the Euro-CORDEX multi-model ensemble and some specific simulations with/without trends in aerosols as in Nabat et al. 2014, Drugé et al. 2020. This may serve (1) to determine if RCMs are able to reproduce such trend in evaluation and historical mode and (2) to illustrate the rôle of the aerosol forcing as discussed in page 85 [SAMUEL SOMOT, France]	Not applicable: the whole Western Europe subsection has been removed
98115	85	1	85	1	Similarly, Knutson and Ploshay (2016) find that summertime temperature trends over Europe over 1973-2012 are not inconsistent between observations and CMIP5 models, at least when a possible temporary mismatch of trends due to internal variability is accounted for in the assessment." Ref: 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 [Thomas Knutson, United States of America]	Not applicable: the whole Western Europe subsection has been removed due to shortening
7937	85	9	85	9	Chapter 8 also attributes enhanced land warming to constrains on evaporative cooling (limited soil reservoir) and the associated moisture transport from sea to land [Bart van den Hurk, Netherlands]	Not applicable. The whole Western Europe subsection has been removed.
12303	85	9	85	9	Maybe it would be better here to say "lapse-rate adjustments" (or something like that) instead of "the lapse-rate feedback" as the latter usually refers to the feedback of lapse rates on the outgoing long wave radiation, which is not what is discussed by Brogli et al. (2019a) or Kröner et al. (2017). Also since both these references investigate future changes and not directly the past warming it may make sense to add another reference. In chapter 4 the following reference is used most often concerning the land-ocean contrast: Byrne, M. P., & O'Gorman, P. A. (2018). Trends in continental temperature and humidity directly linked to ocean warming. Proc. Nat. Acad. Sci., 115, 4863–4868. https://doi.org/10.1073/pnas.1722312115 [Roman Brogli, Switzerland]	Not applicable. The whole Western Europe subsection has been removed.
105859	85	11		18	on this topics, please also check Boé et al. 2020 doi:10.1007/s00382-020-05153-1 (fig 7, section 4.2) using RCM in historical mode [SAMUEL SOMOT, France]	Not applicable: The whole Western Europe subsection has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66573	85	20	85	27	Parding et al 2017,https://journals.ametsoc.org/doi/10.1175/JCLI-D-15-0476.1 analysing trends in temperature in northern Europe in relation to changes in synoptic scale variability as manifested by Grosswetterlagen concludes that factors other than the large-scale circulation (e.g., decreasing aerosol emissions) also play an important role to explain temperature trends in MAM and JJA in northern Europe [Kjellström Erik, Sweden]	Not applicable. The whole Western Europe subsection has been removed.
30675	85	20	85	27	In this circulation context, add here reference to Simmonds, 2018: What causes extreme hot days in Europe? Environ. Res. Letters, 13, 071001, doi: 10.1088/1748-9326/aacc78. [Ian Simmonds, Australia]	Not applicable. The whole Western Europe subsection has been removed.
38557	85	29	85	31	Also Quesada et al (Nature Climate Change) showed that feedback depends on weather regime [robert vautard, France]	Not applicable: The whole Western Europe subsection has been removed.
7939	85	32	85	32	Chapter 8 page 103 (line 4-17) does make a link between Arctic sea ice change and midlatitude drying [Bart van den Hurk, Netherlands]	Not applicable. The whole Western Europe subsection has been removed.
100847	85	32	85	33	Recent literature (Hanna et al. 2016 and Davini and D'Andrea 2020 –under revision) reports a significant increase in the frequency of summertime blocking over Greenland in historical periods, which by the way is not simulated by CMIP models that show instead a decrease in frequency (see also Hanna et al. 2018)). DD2020 also reports that the observed trend in wintertime blocking frequency over Greenland is negative, however due to the large natural variability in this region the trend is not significant. Hanna, E., T. E. Cropper, R. J. Hall, and J. Cappelen, 2016: Greenland blocking index 1851–2015: a regional climate change signal. International Journal of Climatology, 36 (15), 4847–4861. Hanna, E., X. Fettweis, and R. J. Hall, 2018: Brief communication: Recent changes in summer greenland blocking captured by none of the cmip5 models. The Cryosphere, 12 (10), 3287– 3292. - Davini and D'Andrea 2020 under submission already cited in the chapter. [Corti Susanna, Italy]	Not applicable: the whole Western Europe subsection has been removed.
38559	85	37	85	37	Both RCMs and GCMs largely underestimate heatwave intensity changes (Vautard et al., 2020, ERL, in revision) [robert vautard, France]	Not applicable: The whole Western Europe subsection has been removed.
98873	85	37	85	52	Bibliography: role of aerosols on regional climate projections: 10.1088/1748-9326/ab6666 [Enrique Sanchez, Spain]	Not applicable: The whole Western Europe subsection has been removed.
105861	85	37		39	This underestimation for GCMs seems to be in contradiction with Fig 10.17bc in which the ensemble means match quite well with the observed trends. [SAMUEL SOMOT, France]	Not applicable: the whole Western Europe subsection has been removed.
109767	85	39	85	39	Lehner et al. (2017) also show that a small overestimation of European summer temperature variability in models leads to a later emergence of the forced response in models than in observations. Lehner, F., C. Deser, L. Terray (2017): Towards a new estimate of "time of emergence" of anthropogenic warming: insights from dynamical adjustment and a large initial-condition model ensemble. Journal of Climate, DOI: 10.1175/JCLI-D-16-0792.1 [Flavio Lehner, Switzerland]	Not applicable: the whole Western Europe subsection has been removed.
7941	85	40	85	40	Boé J., Somot S., Corre L., Nabat P. (2020) Large differences in Summer climate change over Europe as projected by global and regional climate models : causes and consequences. Climate Dynamics, doi:10.1007/s00382-020-05153-1 (published on-line) [Bart van den Hurk, Netherlands]	Not applicable. Section 10.4.1.2.6 has been removed.
105863	85	44			Bartok et al. 2017 do not discuss so well the role of aerosols contrary to Nabat et al. 2014 (already cited), and the recent Boé et al. 2020 doi:10.1007/s00382-020-05153-1 and Gutierrez et al. 2020 https://doi.org/10.1088/1748-9326/ab6666. Note in particular that the annex of Gutierrez et al. 2020 document the way RCM takes into account aerosol evolution in (Euro-)CORDEX showing clearly the missing processes mentioned at line 38 [SAMUEL SOMOT, France]	Not applicable: the whole Western Europe subsection has been removed.
116989	85	45	85	45	Please check with chapters 2,3 and 7 and make sure that this statement (underestimation of aerosol effect in CMIP) is grounded in an explicit assesment (currently not provided). Please also check the validity of the statement about differences between simualtions and reconstructions for last millennium (this should be addressed in ch 2-3). [Valerie Masson-Delmotte, France]	Not applicable: the whole Western Europe subsection has been removed.
105865	85	46			Perhaps, better good to add that these results are obtained in a pair of reanalysis-driven RCM runs in which observed natural variability is imposed. [SAMUEL SOMOT, France]	Not applicable: the whole Western Europe subsection has been removed.
10705	85	50	85	52	This is almost word for word the same sentence as used in the abstract of Luterbacher et al (2016), except they don't use "Medieval Warm Period". [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The whole Western Europe subsection has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10707	85	50	85	52	Luterbacher et al (2016) use different definitions for "Medieval Climate Anomaly" and "Little Ice Age" than this report uses. That should at the very least be noted. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The whole Western Europe subsection has been removed.
10709	85	50	85	52	As far as I can tell Luterbacher et al (2016) did not show that differences were significant. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The whole Western Europe subsection has been removed.
109747	85	51	85	52	The large amplitude in reconstructions than in simulations on a wider global scale has also recently been analysed in: Ljungqvist, F.C., Zhang, Q., Brattström, G., Krusic, P.J., Seim, A., Li, Q., Zhang, Q., and Moberg, A. 2019: Centennial-scale temperature change in last millennium simulations and proxy-based reconstructions. Journal of Climate, 32: 2441–2482. [Charpentier Ljungqvist Fredrik, Sweden]	Not applicable: the whole Western Europe subsection has been removed.
105867	85	54			I disagree with the period chosen for Fig 10.17 that does not fit with previous discussion. Periods starting in 1980 or 1990 would be more logical with the text above [SAMUEL SOMOT, France]	Not applicable: the whole Western Europe subsection has been removed.
98117	86	1	86	3	"...Deser et al. 2016), although smaller ensembles combined with multi-century control runs can also reasonably be used for this purpose at least for some cliamte variables (Knutson et al. 2013; Thompson et al. 2015)." [Thomas Knutson, United States of America]	Not applicable: the whole Western Europe subsection has been removed.
105869	86	1			Fig 10.17d does not exist [SAMUEL SOMOT, France]	Not applicable: the Figure 10.17 has been removed
105873	86	3		6	Not sure I agree with this sentence for this specific case. Indeed the observed trend matches with CMIP6-multi model mean and MPI-GE mean and it lies in the middle of the CMIP6 and MPI-GE ensemble range. Personnaly I would conclude that the signal for this specific case is mostly forced. This conclusion can also come from fig b in which the multi-model means seem to capture the trend quite well. Exceptionnal conditions of internal variability does not seem necessary to reproduce the trend contrary to Fig 10.13 and Fig 10.14. [SAMUEL SOMOT, France]	Not applicable: the whole Western Europe subsection has been removed.
105871	86	3			Fig 10.17(e) does not exist [SAMUEL SOMOT, France]	Not applicable: the Figure 10.17 has been removed.
109769	86	4	86	5	"indicating that natural variability strongly has affected the historical warming" --> "suggesting that observed and simulated trends might be consistent if a suffieicntly large ensemble is considered" [Flavio Lehner, Switzerland]	Not applicable: the whole Western Europe subsection has been removed.
12305	86	8	86	8	I think the term "lapse-rate feedback" might be misleading as this statment does not refer to the radiative feedback which is usually meant by lapse-rate feedback. I suggest to use "lapse-rate changes", just "land-ocean warming contrast" or a combination of the two. [Roman Brogli, Switzerland]	Taken into account: specific text has been removed, but this comment has been taken into account for the Mediterranean summer warming section 10.6.4 where lapse-rate feedback has been changed into lapse-rate changes..
22951	86	8	86	18	I found this really confusing. In particular that the dominant factor has only medium confidence whereas other factors have high confidence. Also, the primary factor is buried mid-paragraph. I think this needs reordering and simplifying so that the causes are much more clearly delineated than is the case presently. [Peter Thorne, Ireland]	Not applicable. The whole Western Europe subsection has been removed.
70927	86	9	86	10	This statement is in contradiction with what is stated on p.85, lines 33-35 [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The whole Western Europe subsection has been removed.
105875	86	10			summer warming -> western European summer warming : to allow direct citation of this part of the text [SAMUEL SOMOT, France]	Not applicable: the whole Western Europe subsection has been removed.
105877	86	12			add « western » (same reason as above) [SAMUEL SOMOT, France]	Not applicable: the whole Western Europe subsection has been removed.
105879	86	15			not sure the medium confidence is in agreement with previous text. Please re-assess the confidence level here [SAMUEL SOMOT, France]	Not applicable: the whole Western Europe subsection has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
105881	86	17			Not sure the current generation of RCMs can do it except for the few RCMs that do take into account past trends in aerosol forcing, what they mostly did not (see Boé et al. 2020, annex of Gutierrez et al. 2020) [SAMUEL SOMOT, France]	Not applicable: the whole Western Europe subsection has been removed.
45143	86	21	88	11	Again, the text in "10.4.1.2.7 The south-western North America drought" contains useful scientific and policy-relevant information. The text is long and can be shortened. [Krishnan Raghavan, India]	Taken into account. The text has gone through substantial reformulation and shortening.
106615	86	21	88	11	A good example of multi-decadal variability and anthropogenic change combining but again includes a lot of detail all of which is probably not necessary so suggest reducing the text accordingly. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text has gone through substantial reformulation and shortening.
109771	86	27	86	27	add "in certain regions" between "that" and "is" [Flavio Lehner, Switzerland]	Accepted.
109773	86	34	86	34	add "and affecting evaporation (Williams et al., 2020, 10.1126/science.aaz9600)" [Flavio Lehner, Switzerland]	Accepted. The reference has been added for the FGD.
105645	86	34	86	38	Suggest some additional nuance for this conclusion. (Note that this suggestion will bring this conclusion more in line with that made in chapter 8 (section 8.2.2.1; page 19, lines 15-19). For the SW North America, the LGM is not (in the strict sense) a reverse analogue for the future, due to the complicating influences of the Laurentide Ice Sheet. Instead, these citations suggest that future projections of drying in the region are a continuation of the long-term drying trend from pluvial conditions at the Last Glacial Maximum, with similar atmospheric thermodynamic and dynamic processes acting in both the past and future. [Carrie Morrill, United States of America]	Taken into account. The text has been revised following the reviewer suggestion.
109775	86	35	86	35	~21ka "ago" [Flavio Lehner, Switzerland]	Accepted.
79183	87	3	87	20	Delworth et al. (2015 J Climate, doi: 10.1175/JCLI-D-14-00616.1) also supports the contribution from PDV. [Yu Kosaka, Japan]	Accepted. The reference has been added for the FGD.
98119	87	4	87	5	Recommend to modify the sentence beginning “Analysis of observe…Zeng, 2018)” as follows: “A univariate detection/attribution analysis of observed and CMIP5-simulated precipitation trends over the periods 1901-2010, 1951-2010 and 1981-2010 suggests that detectable negative trends in precipitation are rare for gridpoints over the southwestern U.S., with the rare exceptions being a few gridpoints for March-May, June-August, and September-November trends, where detectable anthropogenic decreases were inferred. These results suggest a strong influence of natural, internal variability in the climate system on precipitation trends in this region.” [Thomas Knutson, United States of America]	Rejected. Strong space constraints do not allow lengthy and very detailed description of a single paper
68495	87	29			GCM -> CGCM (coupled GCM) or AOGCM (atmosphere and ocean GCM) [Yukiko Imada, Japan]	Taken into account. GCM has been used throughout the chapter and coupled, atmospheric or oceanic have been added when relevant.
54413	87	39	87	39	The subtitles to each chart are doubled. All figures in the section should be presented in a similar way [Gabriel Stachura, Poland]	Taken into account. The figures have been homogenized for the FGD.
71247	87				Figure 10.18, see comment number 12 regarding the grey colour [Nesha Wright, Canada]	Taken into account. The figure has been changed to follow the TSU graphical recommendations and to maintain consistency with other section 4 figures.
98121	88	2	88	5	Modify to: “Unlike the precipitation deficit, the warming of south-western North America is clearly detectable over the period 1901-2010 and has an anthropogenic component (Knutson et al. 2013); the warming was found to be primarily driven by anthropogenic forcing from GHGs rather than atmospheric circulation variability and may help enhance the drought …” [Thomas Knutson, United States of America]	Taken into account. The reference has been added for the FGD.
109777	88	7	88	7	"important fraction" --> "most" [Flavio Lehner, Switzerland]	Accepted.
54415	88	9	88	11	The main issue of this subsection is the precipitation, so I see no reason to mention about temperature change attribution in conclusions [Gabriel Stachura, Poland]	Rejected. The main issue is the attribution of the drought and it has two distinct components: the precipitation deficit (mainly from internal variability) and an increase in evapotranspiration linked to warming (mostly anthropogenic).
109779	88	13	88	13	Now also shown explicitly and with multiple large ensembles in Lehner, F., C. Deser, N. Maher, J. Marotzke, E. Fischer, L. Brunner, R. Knutti, E. Hawkins (2020): Partitioning climate projection uncertainty with multiple Large Ensembles and CMIP5/6. Earth System Dynamics, DOI: 10.5194/esd-11-491-2020 [Flavio Lehner, Switzerland]	Taken into account. The reference has been added for the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
80335	88	14	88	14	There are other papers that discuss recent changes in the region and the influence of low-level jets. For instance: Durán-Quesada et al. (2017). See: https://www.earth-syst-dynam.net/8/147/2017/ [Paola Arias, Colombia]	Rejected. The text was reworked to contribute to Cross-Chapter Box Atlas.2 and Atlas 7.1.
45145	88	14	90	3	Again the text in "10.4.1.2.8 The Caribbean small islands summer drought" contains useful scientific and policy-relevant information. The text is long and can be shortened. [Krishnan Raghavan, India]	Taken into account: The text has been revised and incorporated into Cross-Chapter Box Atlas.2
57449	88	14	90	3	The midsummer drought is a characteristic feature of most of the Central America and Caribbean region. I suggest to change " Caribbean and small islands summer drought" to " Central Ammerica and Caribbean region summer drought" . [Daniel Martinez Castro, Cuba]	Noted. Comment is no longer applicable. The text has been reworked to contribute to Cross-Chapter Box Atlas.2 with a new focus.
106617	88	14	90	3	This subsection contains some interesting material but is not really relevant to the aims of the section and with its focus on changes in mean climate should be considered for moving to the Atlas. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The whole Caribbean subsection has been removed and relevant material has been merged in a cross-chapter box located in the Atlas
100017	88	14			This example is very useful and we want to thank the authors for this thorough assessment. However, it would be great if some specific information for the eastern Caribbean could also be added here, if available. [Caroline Eugene, Saint Lucia]	Taken into account. Text has been revised to contribute to Cross-Chapter Atlas.2
84157	88	14			This example is very useful and we want to thank the authors for this thorough assessment. However, it would be great if some specific information for the Eastern Caribbean could also be added here, if available. [Jeffers Cheryl , Saint Kitts and Nevis]	Taken into account. Text has been revised to contribute to Cross-Chapter Atlas.2
1621	88	30	88	36	There is a need to state which version of the numerous CRU datasets are being used. In CRU, we have developed version numbering, traceability etc. We like to think that people will use it. Many journal papers don't, but you are writing an IPCC Report, so you ought to refer to the correct version and the paper. There was this paper as well. Jones, P.D., Harpham, C., Harris, I., Goodess, C.M., Burton, A, Centella, A., Taylor, M., Bezanilla, A, Campbell, J.D., Stephenson, T.S., Joslyn, O., Nicholls, K. and Baur, T., 2016: Long-term trends in precipitation and temperature across the Caribbean. Int. J. Climatol. 36, 3314-3333, DOI: 10.1002/joc.4557. In this paper, we refer to version numbers for CRU products. Cavazos et al (2019) just refer to CRU, so readers have no idea what version was used. Versions do differ. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The text and figures were reworked to contribute to Cross-Chapter Box Atlas.2 and Atlas 7.1. Comment is no longer applicable.
98123	88	33	88	37	Using grid-point based detection attribution analysis of GPCC annual mean precipitation data and CMIP5 models, Knutson and Zeng (2018) show…" [Thomas Knutson, United States of America]	Noted. The text and figures were reworked to contribute to Cross-Chapter Box Atlas.2 and Atlas 7.1. Comment is no longer applicable.
54417	88	52	88	52	Regarding presentation of observed precipitation trends in Figure 10.19 (and previous figures from other parts of the world) - here a unit of trend is mm/decade, in every Figure this unit is calculeted per different timescale. Is it possible, to recalculate the observational trends from previous figures to mm/decade? Unification would be beneficial for reader [Gabriel Stachura, Poland]	Not Applicable. The figure 10.19 has been removed for the FGD.
54419	88	52	88	52	The subtitles to each chart are doubled. All figures in the section should be presented in a similar way [Gabriel Stachura, Poland]	Taken into account. The figures for the remaining examples in the FGD have been homogenized in term of format.
13595	89	29	89	29	Change Magana by Magaña [Maria Amparo Martinez Arroyo, Mexico]	Taken into account. Change incorporated in Atlas 7.1
84771	89	29	89	35	a recent published paper (Barcikowska et al 2020 ESD https://doi.org/10.5194/esd-11-161-2020) evidence how improved representation of land surface and soil moisture processes in CM2.5 may help in reducing the bias in the overestimation of the warming drying simulated pattern in southwestern Europe [Annalisa Cherchi, Italy]	Noted. The whole Western Europe subsection has been removed and relevant material merged in the appropriate section 6 example
35429	90	6		30	This brief evaluation, with the examples shown above, opens lines of research for future work in other regions. [Gladys Linares-Fleites, Mexico]	Noted. Additional information for other regions can be found in the Atlas chapter.
98125	90	10	90	12	I recommend the following adjustment based on Knutson et al. 2013 (for temperature) and Knutson and Zeng (2018) for regional precipitation: "While the influence of anthropogenic forcing on regional temperature trends at the century time scale has been detected and the warming at least partly attributed to anthropogenic forcing in most regions, a robust emergence of human influence on precipitation trends--even at the century scale over land regions--is less prevalent, with one study finding detectable human influence over about 30% of analyzed land regions for 1901-2010 trends." [Thomas Knutson, United States of America]	Rejected. An assessment summary cannot refer to only one publication

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66575	90	10	90	13	In some areas it may even be so that no changes in precipitation are expected at all. For instance, in Europe in the area between decreasing precipitation in the south and increasing precipitation in the north there is a zone that may not see any change at all. And if there is a small change it is very small compared to the internal variability. This if for example discussed in the perspective of "time of emergence" in Kjellström, E., Thejll, P., Rummukainen, M., Christensen, J.H., Boberg, F., Christensen, O.B., Fox Maule, C., 2013. Emerging regional climate change signals for Europe under varying large-scale circulation conditions, Clim. Res. 56, 103–119, DOI: 10.3354/cr01146. [Kjellström Erik, Sweden]	Noted. It is difficult to claim that there is no change at all. As pointed out by the reviewer and written in the text, it is better to say that the signal to noise can be really small due to large internal variability
88837	90	11	90	12	Has this even been attempted for "most regions" let alone not found. Perhaps you mean to say "... most regions where this has been studied"? [Krishna AchutaRao, India]	Taken into account. The text has gone through substantial reformulation and the confidence statements have been modified.
88839	90	13	90	13	When I read the sentence where large internal variability is added on as an "as well as", it gives more importance to observational uncertainty & model error. That may not be the right message to convey. The large internal variability makes the signal to noise characteristics for precipitation as a variable are quite poor (compared to temperature say). [Krishna AchutaRao, India]	Taken into account. The text has gone through substantial reformulation and the confidence statements have been modified.
1623	90	17	90	21	Statements like this are pretty meaningless. 24% and 76% and 43% make little sense. If you need to go back to the papers to find out what has been done, then it's not a very good assessment. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text has been revised for the FGD and the statement pointed by the reviewer has been removed.
1397	90	19	90	19	It's a controversial question whether there really was a 'hiatus'. Trends in other observations, such as the global mean sea level can be taken as a contradiction. Furthermore, analysis by e.g. Cowtan and Way (2013; DOI: 10.1002/qj.2297) suggests that the apparence of a flattening was due to incomplete sampling of temperature, especially in the Arctic where the recent warming has been the strongest. [Rasmus Benestad, Norway]	Not applicable. This text has been removed for the FGD
45147	90	23	90	30	The Assessment Summary 10.4.1.3 is very good. It will be great to have cross-chaper consistency with regard to confidence statements and attribution of regional changes especially Chapters 2, 3, 8, 11, Atlas. [Krishnan Raghavan, India]	Taken into account. Coordination with chapter 3, 8 and the Atlas has been organized to achieve consistency in the assessment
88841	90	26	90	27	The ordering here of the different sources of difficulty is different from line 13 on this page. This would be the better ordering - but the sentences seem to have the "as well as" that perhaps adds an unnecessary dimension to the statement. [Krishna AchutaRao, India]	Taken into account. The paragraph has been largely modified for the FGD leading to only one mention of the different sources.
88843	90	29	90	29	This should actually be stratospheric ozone depletion? On Page 81, Line 9, "stratospheric ozone depletion" is mentioned. This phrase has progressively dropped key words in every subsequent mention and ends up losing important meaning.See also ES. [Krishna AchutaRao, India]	Accepted. The sentence has been modified to include "stratospheric ozone".
109417	90	29	90	29	please specify "stratospheric" ozone [Sophie Szopa, France]	Accepted. The sentence has been modified to include "stratospheric ozone".
22971	90	33			You have implied that the projections for the case study regions will be discussed but I saw little to no such discussion and certainly not differentiated by region and structured in such a way as to allow a reader to easily cross-compare 10.4.1 analyses with those in 10.4.2. [Peter Thorne, Ireland]	Not Applicable. The substance of the two subsections has drastically changed due to the removal of most of the examples.
4323	90	37	90	37	I think it would be appropriate to cite Hawkins and Sutton (2009) here too. In general, throughout this chapter, there seems to be a lot of citation of the newest papers, which is obviously appropriate. But there is a lack of citation of some of the original studies and I think they should still be mentioned. [Isla Simpson, United States of America]	Not Applicable. This part of section 4 has been removed for the FGD. Note that the suggested reference is now cited in the next section 10.4.3.
4325	90	41	90	41	I don’t think the internal variability that leads to uncertainty in future projections necessarily has to be “low-frequency”. If “low-frequency” is intended to refer also to random sampling of a white noise process that leads to some apparent low-frequency variability then that’s ok, but I think it would be safer to just remove “low-frequency” to ensure that sampling of higher frequency noise is also included. [Isla Simpson, United States of America]	Accepted. "low-frequency" has been deleted
36331	90	48	90	48	sentence construction makes it difficult to understand what it is trying to convey (the rest is OK). Propose deletion or revision. [PENDO MARO, Belgium]	Not Applicable. This part of section 4 has been removed for the FGD.
22953	90	50	90	52	I think you can say this more cleanly. Something like: The response to external forcing can also be expressed through changes in internal modes of variability making a formal seperation between forced response and internal variability very challenging. [Peter Thorne, Ireland]	Not Applicable. This part of section 4 has been removed for the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
90985	91	4	91	4	Here "credible" seems to mean "accurate" or "accurate enough". See related comments regarding p.11, line 44 and p.59, line 10. The question is what credible means. [Wendy Parker, United Kingdom (of Great Britain and Northern Ireland)]	Not Applicable. This part of section 4 has been removed for the FGD.
1399	91	6	91	7	It is also possible to assess the the credibility of simulated internal variability based on downscaled variables (e.g. seasonal mean temperature), assume a binomial distribution for the number of observed values outside the simulated 90% confidence interval (based on the ensemble spread). When the downscaling is based on a framework with common EOF, it provides a simultaneous evaluation of both the downscaling itself and how the multi-model represents the interannual variability (Benestad et al., 2916; DOI: 10.1088/1748-9326/11/5/054017). However, the assessment is limited by the availability of local observations as well as the interval covered by the reanalysis (e.g. NCEP/NCAR reanalysis 1: 1958-present). [Rasmus Benestad, Norway]	Noted. This point has been discussed in chapter 10, section 3.
87393	91	14	91	18	The impact of variations in the ocean circulation in the North Atlantic is forgotten in this chapter. External forcing might also influence the AMOC and therefore the AMV (Swingedouw et al. 2015) over the recent period, one study also highlights the potential impact of abrupt cooling in the subpolar gyre as a major source of uncertainty at the decadal scale for Europe (Sgubin et al 2019), a projection that has been dynamically downscaled within CORDEX. A word on this major source of uncertainty might be worth in this chapter since decision maker might need to also account for this type of low-probability high impact event, which might share similarities in this respect with COVID crisis. Ref: Sgubin G. , Swingedouw D., Garcia de Cortazar-Atauri I.,Ollat N. and van Leeuwen C. (2019) The Impact of Possible Decadal-Scale Cold Waves on Viticulture over Europe in a Context of Global Warming. Agronomy, 9, 397; doi:10.3390/agronomy9070397. Swingedouw D., P. Ortega,J. Mignot,E. Guilyardi,V. Masson-Delmotte, P. G. Butler and M. Khodri (2015) Bidecadal North Atlantic ocean circulation variability controlled by timing of volcanic eruptions. Nature Communications 6, pages: 6545 [Didier Swingedouw, France]	Not Applicable. This part of section 4 has been removed for the FGD.
22955	91	14	91	18	This is largely repeating in slightly different ways points already made earlier in the section? [Peter Thorne, Ireland]	Not Applicable. This part of section 4 has been removed for the FGD.
98129	91	18	91	25	In my view these long future detection timescales are too pessimistic. Based on Knutson et al. 2013 (Fig. 10 for temperature) and Knutson and Zeng 2018 (Fig. 3) for regional precipitation: "The influence of anthropogenic forcing on regional temperature trends at the century time scale has already been detected and the warming at least partly attributed to anthropogenic forcing in most regions; however, a robust emergence of human influence on precipitation trends--even at the century scale over land regions--is less prevalent, with one study inferring detectable human influence over about 30% of analyzed land regions, based on 1901-2010 trends." [Thomas Knutson, United States of America]	Not Applicable. This part of section 4 has been removed for the FGD.
105885	91	20			This page is difficult to read. Could it be revised for an easier reading. [SAMUEL SOMOT, France]	Not Applicable. This part has been removed for the FGD.
22957	91	32	91	32	But there are 4 RCPs. [Peter Thorne, Ireland]	Not Applicable. This part of section 4 has been removed for the FGD.
22959	91	33	91	47	Do you really want to highlight a study using forcing scenarios that are now two generations old? This is, I think, the first study in the 9 and a bit chapters to date to use SRES scenarios and also implies it is on a CMIP3 model which may therefore no longer be applicable either with the new scenarios herein or newer CMIP 6 models? [Peter Thorne, Ireland]	Not Applicable. This part of section 4 has been removed for the FGD.
109781	91	43	91	45	the term "random uncertainty" is not explained and I'm not familiar with it. Isn't it just internal variability? [Flavio Lehner, Switzerland]	Not Applicable. This part of section 4 has been removed for the FGD.
98127	92	1	92	1	Recommend: "In contrast, Knutson and Zeng (2018) conclude that some anthropogenic influence is already detectable in regional precipitation trends over 1901-2010 for about 30% of analyzed land regions, according to comparisons of observed precipitation trends with CMIP5 model historical and control run simulations. " [Thomas Knutson, United States of America]	Noted. The reference is one of those on which the assessment is based.
105887	92	6			Results for the Mediterranean zone for precipitation may be worse to be added as it is one of the zone for which the signal is the most robust over the last IPCC reports [SAMUEL SOMOT, France]	Not Applicable. The figure has been removed for the FGD.
43295	92	9		10	Read " as in Deser et 9 al. (submitted)" rather than " as in (Deser et 9 al., submitted)" [Cyriaque Rufin Nguimalet, Central African Republic]	Not Applicable. The figure has been removed for the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59435	92	30			Section Box 10.2 Urban Climate: I suggest increasing information on the contribution of urban green spaces and how they can mitigate, to some extent, the urban warming. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The mitigation aspect will be discussed deeply in WG2 and in the cross-chapter box on cities. In WG1 we do not focus on mitigation/adaptation aspect.
105899	92	30			what about mentioning somewhere the minimum spatial resolution required in climate models to hope to reproduce the city effects. [SAMUEL SOMOT, France]	Rejected. Difficult to specify one minimum resolution for climate model, in fact, when grey zone resolution (4km) might be enough, depending on the application, for megacities such as Paris even kilometric spatial resolution is not enough to study the urban climate of small cities or villages.
78773	92	32	92	45	The waste heat should be specifically listed more among the antropogenic effects as there is sufficient evidence that it is one of the biggest sources of urban heating especially in (sub)tropical and arid areas [e.g. Salamanca et al. (2014) Antropogenic heating of the urban environment due to air conditioning, JGR Atmospheres 119(10), p. 5949-5965]. [Yasemin Aktas, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference has been added together with others dealing with the air conditioning and the urban heat island in Europe and Japan.
78775	92	32	92	45	More critically, it is very worrying that the urban humidity, still to this day, and even within IPCC, fails to be a part of urban microclimate discourse: Humidity is an aggravator of rising temperatures in an urban area, with extremely important implications on health and wellbeing and building energy use potential. Maia-Silva et al., (2020) have shown that accounting for the humidity levels drastically change the energy demand models for a given area. Aktas (2020) have shown that despite the lower temperature values in urban parks in a tropical context, the thermal comfort in these locations is so poor during the day as to pose health risks due to much higher humidity values (and lower ventilation in case of thickly wooded green areas). [Maia-Silva, D, R Kumar, and R Nateghi. 2020. “The critical role of humidity in modeling summer electricity demand across the United States.” Nature Communications 11. Aktas et al. (2020) Outdoor thermal comfort and energy use potential in different land-use areas in tropical cities: case of Kuala Lumpur. Atmosphere. Forthcoming article.] There is no consensus regardign "urban dryness island" - in fact, now more and more scholars are talkig about urban moisture island! [e.g. Wang, Z, J Song, P W Chan, and Y Li. 2020. “The urban moisture island phenomenon and its mechanisms in a high-rise high-density city.” International Journal of Climatology. doi:10.1002/joc.6672.] [Yasemin Aktas, United Kingdom (of Great Britain and Northern Ireland)]	Rejected and accepted. The first part of the comment is not the focus of WGI but more on the health impact which will be taken into account in Chap6 of WGII and in the cross working group box on cities. The second part, related to the urban moisture island is accepted and suggested reference read, however, in order to avoid confusion and to make the statement about urban drying island more clear we modified the text add new reference and say that urbanization induce a drying island which is a well known and agreed phenomena and which fit with the purpose of this box being on the urban climate so with decadal time scale.
78777	92	32	92	45	The role of the building materials in relation to climate resilient urban design should be integrated here, or be otherwise a part of the report. While it is true that building materials have indeed higher heat capacity than the materials forming a natural landscape, there is a significant different between different building materials' heat absorption and storage capacity. Please see Aktas et al., (2017) for a discussion as to how differenting concrete, masonry, timber etc can lead to substantially different temperature perturbations in a neighbourhood scale urban heat island modelling. Furthermore the cooling potential of porous building materials is an underresearched topic which can greatly change the way we see urban climate resiliency if further detailed. See e.g. Aktaş, Y.D., Stocker, J., Carruthers, D., Hunt, J. (2017) A Sensitivity Study Relating to Local Urban Climate Modelling within the Built Environment, Procedia Engineering 198, pp. 589-599, doi: 10.1016/j.proeng.2017.07.113 and Goncalves et al., (2015) Evaporation from porous building materials and its cooling potential. Journal of materials in Civil Engineering 17(8) [Yasemin Aktas, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Climate resilient urban design is not the purpose of WGI report but will be discussed in Chp6 of WGII, please advice the suggested reference to the SOD of the WGII report.
1625	92	32	97	45	This is the biggest box I've seen - only looked at parts of Ch 2 and Ch 10. Ths one is far too big. Also it needs to be an Assessment, rather than the review this reads like. This box is no different from the main text. I expect a box to be a more readable synthesis. This isn't. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text has been revised.
16943	92	33	92	35	This is a very general statement which need to be supported by a general review on the topic. I suggest removing the reference to Kuang, 2019, which focus on an individual city. I think Oke et al. (2017) is still the most adequate reference for this sentence. Reference: Oke, T. R., Mills, G., Christen, A., and Voogt, J. A. (2017). Urban Climates. Cambridge: Cambridge University Press. doi:10.1017/9781139016476 [Gianluca Mussetti, Switzerland]	Rejected. The reference Kuang 2019 and the new one added in the list are presenting a new method for UHI attribution.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44239	92	33	92	35	Cities are often several degrees warmer compared to the surrounding rural area during the night: the Urban Heat Island (UHI) is in general a night-time phenomenon. Furthermore, it should be clear in Box 10.2 that the canopy layer UHI is considered and not the surface UHI, or the boundary layer UHI, as defined by Oke, T.R., et al., 2017: Urban Climates. Cambridge University Press, Cambridge. Chapter 7 Urban Heat Island, pp. 197 - 237. [Nektarios Chrysoulakis, Greece]	Accepted. Text has been revised and the canopy UHI is explicitly mentioned in the box.
44241	92	34	92	34	It is better to rephrase and remove “due to”, because the Urban Heat Island is not the driver that causes the temperature difference, but rather the term that used to describe the phenomenon. [Nektarios Chrysoulakis, Greece]	Accepted. Text has been revised.
20691	92	35	92	35	An appropriate seminal reference would be e.g. " The energetic basis of the urban heat island", Oke, Quart. J. R. Met. Soc. (1982), 108 [philippe waldteufel, France]	Rejected. This reference is already assessed in the chapter book by Oke et al. 2017 and Bader et al. 2018
15563	92	35	92	45	Suggest including the reference below (Peng et al., 2018) which showed how urbanization can lead to significant wind speed reduction within a city: Peng, L, J.P. Liu, Y. Wang, P.W. Chan, T.C. Lee, F. Peng, M.S. Wong, Y.G. Li, 2018 : Wind weakening in a dense high-rise city due to over nearly five decades of urbanization, Building and Environment, 138, 207-220. https://doi.org/10.1016/j.buildenv.2018.04.037 [SAI MING LEE, China]	Accepted. Reference has been added to the list.
44243	92	37	92	39	The anthropogenic heat flux is also released from industrial activities and from human metabolism (Sailor, D. J., 2011. A review of methods for estimating anthropogenic heat and moisture emissions in the urban environment. International Journal of Climatology, 31, 189 - 299). [Nektarios Chrysoulakis, Greece]	Accepted. Reference has been added to the list.
44245	92	37	92	39	Earth Observation methods to estimate the Urban Energy Balance components at local scale, included the anthropogenic heat flux, have recently been developed (Chrysoulakis N., et al. 2018: Urban energy exchanges monitoring from space. Scientific Reports, 8, 11498). [Nektarios Chrysoulakis, Greece]	Accepted. Reference has bee added to the list.
131415	92	39	92	39	It says that one of the three main factors for the UHI is the 3d urban geometry which I find too general. It could be described more specific in terms of city/ urban area size, density etc. (e.g. Zhou, B., Rybski, D. & Kropp, J.P. The role of city size and urban form in the surface urban heat island. Sci Rep 7, 4791 (2017). https://doi.org/10.1038/s41598-017-04242-2; or Ze Liang, Shuyao Wu, Yueyao Wang, Feili Wei, Jiao Huang, Jiashu Shen, Shuangcheng Li,: The relationship between urban form and heat island intensity along the urban development gradients, Science of The Total Environment, Volume 708, 2020, [Hans Poertner and WGII TSU, Germany]	Accepted. Text has been revised.
44247	92	39	92	39	Actually, there are four main factors: the three that are mentioned in text plus the loss of vegetation, which drives the evaporative cooling (Stone, B., et al., 2012. Managing climate change in cities: Will climate action plans work? Landscape and Urban Planning, 107, 263– 271). [Nektarios Chrysoulakis, Greece]	Taken into account, although shorthly due to the strict work limits of this box: "While green and blue infrastructures can mitigate the urban heat island effect, three main factors contribute to its development.."
6835	92	39	92	40	Replace the sentence to read as "Four main factors contribute to the establishment of the urban heat island: 3-D urban geometry, radiative and thermal characteristics of impervious surfaces, greenery and anthropogenic heat fluxes. [Constantinos Cartalis, Greece]	Rejected. Greenery do not induce urban heat island but mitigate it instead.
59423	92	42	92	43	More literature available on Moisture and urban-rural contrasts/UDI that could be of interest to assess (also showing an urban moisture excess is some cases, e.g. Kuttler et. al. 2007): Kuttler et al. 2007: https://doi.org/10.1002/joc.1558 • Jåuregui. 1997. doi:10.1002/(SICI)1097-0088(199702)17:2<187::AID-JOC114>3.0.CO;2-P. • Unkašević. 2001. doi:10.1007/s007040170054. • Unger. 1999. doi:10.1002/(SICI)1097-0088(19991115)19:13<1509::AID-JOC453>3.0.CO;2-P. • Lokoshchenko. 2017. doi:10.1175/JAMC-D-16-0383.1. • Moriwaki. 2013. doi:10.2208/journalofjsce.1.1_521. • Ackerman. 2002. doi:10.1175/1520-0450(1987)026<0427:cocaur>2.0.co;2. • Tapper. 1990doi:10.1016/0957-1272(90)90005-F. • Hage et al. 1975. doi:10.1175/1520-0450(1975)014<1277:urhd>2.0.co;2. • Fortuniak et al. 2006. doi:10.1007/s00704-005-0147-y. Modelling based study: Langendijk et al. 2019: https://doi.org/10.3390/atmos10120730 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The focus here is to assess literature that study relative humidity effect on long-time scale where historical urbanization has occurred and not on case studies or specific atmospheric situation.
59453	92	44	92	44	the citation for Lokoshchenko, 2017b was not listed in the reference section. Could it be an ommision or you were meant to cite Lokoshchenko, 2017 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference has been corrected.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59421	92	44	92	45	Some studies show wind speeds might get higher in/above cities, e.g. this study by WUR: "Average wind speed sometimes higher in cities. By using a model of the atmospheric boundary layer, Droste, Steeneveld, and Holtslag discovered that the average wind speed in a city can be surprisingly higher than in a rural area under certain atmospheric conditions, despite the greater surface “roughness” in cities, which weakens the wind. “It’s a remarkable discovery”, says Gert-Jan Steeneveld. “It goes against what you would initially think. But that’s exactly what makes science so much fun."" Ref to paper by Droste et al. 2018: https://iopscience.iop.org/article/10.1088/1748-9326/aad8ef Further follow-up studies were presented at EGU 2020 online: https://meetingorganizer.copernicus.org/EGU2020/EGU2020-19457.html [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Again here the focus is on long time scale where urbanization has occurred and not on case studies or specific atmospheric situation where the wind speed might be bigger in cities than in the rural surrounding areas.
82711	92	48	92	52	Two other factors which contribute to limited knowledge on urban climate are that many phenomena of interest in urban areas occur on small spatial scales (<1 km) which are not resolved by traditional observing networks, and that those observation sites which do exist in urban areas are representative of only particular parts of the urban environment which are often suboptimal for fully detecting urban effects (e.g. sites in city parks). [Blair Trewin, Australia]	Accepted. Text has been revised.
20693	92	49	92	51	This criticism of WMO observation rules seem uncalled-for. The reference's title is "Guide to the WMO Integrated Global Observing System 2019". In view of the purpose of the Global Observing System, it was obviously appropriate to avoid as much as possible deteriorations of representativity, such as contamination by heat island effects. Now running an observation network in order to learn about urban climate is a specific objective, for which there is no reason to comply with Global Observing System rules [philippe waldteufel, France]	Accepted. Text has been revised
44249	92	55	92	56	However, the use of urban flux towers (for direct measurement of heat and CO2 emissions at local scale) is still limited (Feigenwinter, C., et al., 2012: Eddy Covariance Measurements Over Urban Areas. In Aubinet M., Vesala T., Papale (eds): Eddy Covariance - A Practical Guide to Measurement and Data Analysis, Springer. Chapter 16, pp. 377 – 397; Crawford, B., et al., 2018: Variability of urban surface temperatures and implications for aerodynamic energy exchange in unstable conditions. Quarterly Journal of Royal Meteorological Society, 144, 1719 - 1741). [Nektarios Chrysoulakis, Greece]	Noted.
116991	92		92		see my remark on the Es of the chapter on the statement lines 18-25. [Valerie Masson-Delmotte, France]	Accepted. The statement has been completely modified for the FGD to reflect that temperature detection and/or emergence has already happened in almost all sub-continental regions of the world.
71249	92				Figure 10.20, There is a grey highlight behind the caption of this figure [Nesha Wright, Canada]	Not Applicable. Figure 10.20 has been removed from the FGD.
71251	92				Figure 10.20, the figure labels (a) and (b) on the image are different. In (b) there is a hook, where as a there is not. [Nesha Wright, Canada]	Not Applicable. Figure 10.20 has been removed from the FGD.
59437	93	5	93	5	I suggest adding the following citation as urban heat island monitoring using cars: Vicente-Serrano, S.M., Cuadrat-Prats, J.M., Saz-Sánchez, M.A., 2005. Spatial patterns of the urban heat island in Zaragoza (Spain). Clim. Res. https://doi.org/10.3354/cr030061 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference has been added to the list.
59425	93	5	93	6	Other examples of new observational techniques could be: Droste et al 2017: DOI: 10.1175/JTECH-D-16-0150.1 (battery temperatures in Sao Paolo) Overeem et al. 2013: Crowdsourcing urban air temperatures from smartphone battery temperatures: https://doi.org/10.1002/grl.50786 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. References have been added to the list.
20221	93	19	93	19	"displacement height" is not found anywhere else in this chapter, nor in the Glossary. Please supply an adequate reference [philippe waldteufel, France]	Rejected. The word length for this box was very limited. Therefore, we were not able to define "displacement height", but the interested reader can find this concept defined in the cited literature.
6831	93	20	93	21	The sentense may read as "Energy balance is also modified due to net all wave radiation, the radiation trapped by the urban canopy, heat storage, latent and sensible heat fluxes and anthropogenic heat fluxes (Agathangelidis et al. 2019). Agathangelidis, E., Cartalis, C., Santamouris, M., 2019. Integrating Urban Form, Function, and Energy Fluxes in a Heat Exposure Indicator in View of Intra-Urban Heat Island Assessment and Climate Change Adaptation. Climate 2019, 7(6), 75; https://doi.org/10.3390/cli7060075. [Constantinos Cartalis, Greece]	Noted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16947	93	30	39	30	(continue from above) Reference: Lee, S.-H.et al. "Impacts of in-canyon vegetation and canyon aspect ratio on the thermal environment of street canyons: numerical investigation using a coupled WRF-VUCM model", Q. J. Roy. Meteor. Soc., 142, 2562–2578, https://doi.org/10.1002/qj.2847, 2016; Redon, E. C. et al. "Implementation of street trees within the solar radiative exchange parameterization of TEB in SURFEX v8.0", Geosci. Model Dev., 10, 385–411, https://doi.org/10.5194/gmd-10-385-2017, 2017.; Mussetti et al. "COSMO-BEP-Tree v1.0: a coupled urban climate model with explicit representation of street trees", Geosci. Model Dev., 13, 1685-1710, https://doi.org/10.5194/gmd-13-1685-2020, 2020. [Gianluca Mussetti, Switzerland]	Accepted. References has been added to the list.
16945	93	30	93	30	I recommed to add a sentence regarding the on-going effort of improving their representation of urban vegetation (Lee et al., 2016; Redon et al., 2017; Mussetti et al., 2020). I suggest adding a sentence like: "Recently, model development focused on improving the represention of urban (in-canyon) vegetation (Lee et al., 2016; Redon et al., 2017; Mussetti et al., 2020)." [Gianluca Mussetti, Switzerland]	Accepted. References has been added to the list.
59427	93	32	93	32	Langendijk et al. 2019 (https://doi.org/10.3390/atmos10120730) shows that the RCMs used in the EURO-CORDEX ensemble all show a UHI & UDI in Berlin, through their urban scheme. The models have a Bulk-scheme in the set-up for the EURO-CORDEX runs (urban as a sub land use type). As mentioned in AR6, some RCMs have developed more sophisticated schemes, though often not turned on for the standard EURO-CORDEX runs. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference has been added to the list.
105889	93	32			Any attempt in GCM ? If not, good to tell it. [SAMUEL SOMOT, France]	Accepted. New references from GCM has been added.
13597	93	33	93	33	Change Daniel et al.,,2019 by Daniel et al., 2019 [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Reference has been corrected.
16951	93	36	39	36	(continue from above) References: Ward, Helen C., et al. "Surface Urban Energy and Water Balance Scheme (SUEWS): development and evaluation at two UK sites." Urban Climate 18 (2016): 1-32 https://doi.org/10.1016/j.uclim.2016.05.001 ; Meili, N.et al. "An urban ecohydrological model to quantify the effect of vegetation on urban climate and hydrology (UT&C v1.0)" Geosci. Model Dev., 13 (2020), 335–362, https://doi.org/10.5194/gmd-13-335-2020. Yang, Jiachuan, et al. "Enhancing hydrologic modelling in the coupled weather research and forecasting–urban modelling system." Boundary-Layer Meteorology 155.1 (2015): 87-109. https://doi.org/10.1007/s10546-014-9991-6; Mussetti et al. "COSMO-BEP-Tree v1.0: a coupled urban climate model with explicit representation of street trees", Geosci. Model Dev., 13, 1685-1710, https://doi.org/10.5194/gmd-13-1685-2020, 2020. [Gianluca Mussetti, Switzerland]	Accepted. Reference has been added to the list.
16949	93	36	93	38	I recommend to reconsider this sentence. I think this was an issue with the first generation of urban climate models (Grimmond et al., 2012). Since then, several applications demonstrated good model performance in terms of latent heat flux, especially with off-line urban canopy models (e.g. Ward et al., 2016; Meili et al., 2020) but also on-line (e.g. Yang et al., 2015; Mussetti et al., 2020). [Gianluca Mussetti, Switzerland]	Accepted. Reference has been added to the list.
44251	93	36	93	41	See above comments No 2 and No 3. [Nektarios Chrysoulakis, Greece]	Noted.
105891	93	36			could we say something about the performance of coupled city-climate models to reproduce the UHI as evaluated in Daniel et al. 2019 for Paris and probably in others articles (I don't know well the literature). Are the city-RCM good enough already for providing local climate information or are they still very limited ? Indeed the city-RCM performance were not assessed in the performance sub-section. [SAMUEL SOMOT, France]	Taken into account. Text has been revised.
111985	93	38			… when simulating latent heat fluxes and with the simple ones they have problems with wind speed and mixing layer height as well. [Tomas Halenka, Czech Republic]	Noted.
20695	93	43	93	48	There are no indications in the content of the box to support these confidence/agreement statements [philippe waldteufel, France]	Taken into account. Text has been revised.
52201	93	43	93	48	The average annual response of surface air temperature to urbanization is negligible. This means insignificant effects of the heat islands ?, studied by: Gartland, L. (2008). Heat islands: understanding and mitigating heat in urban areas. In Earthscan. https://doi.org/10.1080/00207233.2012.670477. Oke, T. R. (1987). Boundary Layer Climates (2.nd ed.). Routledge Print. https://doi.org/10.1016/0012-8252(90)90005-G [Maritza Jadrijevic Girardi, Chile]	Noted.
54421	93	52	93	52	I guess surface AIR temperature [Gabriel Stachura, Poland]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
131417	93	52	94	8	Figure 1 Box 10.2: This is an important and illustrative figur, but the different aspects of background general warming and specific warming due to historical urbanization for the cities is not very clear with the used symbols. [Hans Poertner and WGII TSU, Germany]	Taken into account. Figure has been modified.
105893	93	52			No point for Paris? or London ? Strange [SAMUEL SOMOT, France]	Noted. No reference has been found that estimates the long-term urban warming of the city of Paris due to historical urbanization. Having the same background climate condition, the estimate from Brussels can be representative for both Paris and London.
105895	93	52			Not sure adding the hexagones above the color bar is very readable. Why don't you add the city names at the right place on the color bar ? [SAMUEL SOMOT, France]	Rejected. This will be even worse in terms of readability as some of the names will overlapping in both horizontal and vertical dimensions.
71253	93				Box 10.2 Figure 1, Perhaps I missed it but why are there circles for China and Japan instead of specific cities and hexagons? [Nesha Wright, Canada]	Noted. This is to differentiate between assessment on cities and countries. For China and Japan, several evidences on urban warming exist on the country level.
59431	94	1	94	5	The following 2 papers by Baklanov could be interesting to assess in the context of planning & adaptation strategies: Highlighting the need for integrated planning/studies on urban climate: Baklanov et al 2018: https://doi.org/10.1016/j.uclim.2017.05.004 Different stratgies/integrated services in 4 case studies (with different Urban Climate information sources mentioned): Baklanov et al. 2020: https://doi.org/10.1016/j.uclim.2020.100610 Possibly also assess the WMO guide on Integrated Urban Service: https://elioscloud.wmo.int/share/s/Rf3EW264RZWGJuLrCuzo9w [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. References has been added to the list.
43297	94	1		2	Read "This map has been compiled from several studies (…)" rather than "This map has been compiled using the following studies: (…)" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. Text has been revised
22961	94	12	94	13	There is no trace to support this assessment statement. You need to show the basis underlying such a very high confidence statement. [Peter Thorne, Ireland]	Accepted. Text has been revised
82713	94	15	94	15	Could reasonably say "smaller or non-existent" here - AR5 Chapter 2 cited a number of studies which found no anomalous trends at sites which were in urban environments when they were established (even if the broader metropolitan area had grown over the period of record). It is also interesting that in the Tokyo time series in Figure 1, the difference between the two sites appears to grow steadily between 1920 and 1960 and then stabilise after 1960 (it might be of value to add a difference time series to this plot). [Blair Trewin, Australia]	Taken into account. Text has been revised.
125737	94	27	94	30	[CONFIDENCE] This passage about the influence of ground subsidence and groundwater withdrawal on local/relative sea level rise should have "very high confidence" appended. [Trigg Talley, United States of America]	not applicable text has been removed.
22963	94	28	94	30	While correct presently this implies that this is ubiquitously an issue for coastal cities whereas presumably it applies to differing degrees from not applicable through to a dominant effect. Should this not be refelected in nuancing this statement which given the lack of likelihood / confidence could currently be construed as being a statement of fact? [Peter Thorne, Ireland]	not applicable text has been removed.
82715	94	29	94	29	Presumably this will occur in many coastal cities but not all (as implied by the current wording) - there will be some cities where groundwater extraction is not relevant. [Blair Trewin, Australia]	not applicable text has been removed.
20223	94	34	94	39	Several attempts were made to understand this sentence and were unsuccessful [philippe waldteufel, France]	Taken into account. Text has been revised.
82717	94	36	94	36	"One order of magnitude" seems rather precise given the "very uncertain" assessment at line 33 - is it meant to be an upper bound? If so, would be best reworded. [Blair Trewin, Australia]	Accepted. Text has been revised
59433	94	41	94	46	It might also be interesting to mention, that some studies observed a change in UHI due to soil moisture changes in the rural areas projected under climate change, complementing the warming effect in urban areas as a main cause. (discussed during EGU 2020, by Eunice Lo (for UK cities, based on UK CP simulations), and by Heidelinde Trimmel who found similar results for Vienna) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted.
44253	94	41	94	46	In general, the feedbacks between climate change and drivers of urban transformation are not considered by the current RCMs. [Nektarios Chrysoulakis, Greece]	Noted.
22965	94	48	94	50	I'm not convinced that a single study (one line of evidence) can justify an assertion of very high confidence. [Peter Thorne, Ireland]	Accepted. List of reference has been added to support the statement.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
20697	94	48	94	50	What is meant by "under different background climate"? [philippe waldteufel, France]	Not applicable. Expression is no longer used.
82719	94	50	94	50	Presumably this result holds only in rapidly urbanising environments, in which case "locally comparable" would be more appropriate. [Blair Trewin, Australia]	Accepted. Text has been revised
59429	94	50	94	50	Possibly Argueso could be assessed as well in this context: Argüeso et al. 2014 DOI:10.1007/s00382-013-1789-6 (1km modelling, WRF, Sydney, urbanization) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference has been added to the list.
20699	94	53	95	1	at most 0.6°C" reflects the opinion of chapter 10 authors that the difference is modest; yet 0.6°C reaches half the magnitude of anthropogenic global warming so far. This increase is more critical since it is noted in night time. Possibly, the habitants will not consider this difference is so small. Although scenario (line 54) is a word familiar in the IPCC environment, one should not trust it excessively. In general terms, according to the literature (Ljubenovic, 2014, DOI: 10.2298/FUACE1401081S), there is nothing to warrant that urban planning scenarios take climate into consideration [philippe waldteufel, France]	Accepted. Text has been revised
116993	94		94		It is a pity that the box on urban climate does not touch the interplay with air quality in coordination with ch 6. We need to bring together these aspects in our TS. [Valerie Masson-Delmotte, France]	Accepted. Information on air pollution/Aerosol has been integrated in the text for the FGD.
105897	95	1		3	not quite clear the meaning of this sentence. Rephrase [SAMUEL SOMOT, France]	Accepted. Text has been revised
68191	95	12	97	48	suggest to coordinate with Atlas section 5.10.1 about HKH [Guðfinna Aðalgeirsdóttir, Iceland]	Accepted. After discussion, relevant points about the HKH in the Atlas chapter have been merged into Cross-Chapter Box10.4 in the FGD, with appropriate authorship.
82721	95	14	97	45	There is also a section on the HKH in the Atlas (section 5.10.1). Consideration should be given to consolidating these as there is considerable overlap between them. [Blair Trewin, Australia]	Taken into account: Contents on the HKH in the Atlas have been merged into Cross-Chapter Box10.4 in the FGD, with appropriate authorship.
24431	95	14	97	45	Here, little is assessed on future projections of HKH climate. Although CMIP5/6 GCMs are too coarse to reasonably resolve high-mountain area, there are some very high resolution AGCM projections. For example, a 20-km grid high-resolution AGCM reproduces a double peaks of seasonal precipitation over the western Tibeta Plateau as observed, which is associated with westerly disturbances in spring, and projects an east-west contrast in surface climate change over the Tibetan Plateau (TP) where over the western TP surface temperature increases are higher, an increasing rate of precipitation is greater, soil moisture becomes wetter, and runoff increases more than over the eastern TP (Kitoh and Arakawa, 2016). Kitoh, A., and O. Arakawa, 2016: Reduction in the east-west contrast in water budget over the Tibetan Plateau under a future climate. Hydrol. Res. Lett., 10, 113-118, doi:10.3178/hrl.10.113. [Akio Kitoh, Japan]	Accepted. Future projections for the HKH have been assessed for Cross-Chapter Box10.4 in the FGD and the suggested reference has also been cited.
132373	95	16	95	16	Please check spelling of author's name Subimal Ghosh. [Sonia Seneviratne, Switzerland]	Accepted. Name corrected in the FGD.
15665	95	20	95	20	I understand that the Himalaya is the "largest collection of glaciers", but I fail to understand what it means to be the "largest collection of snow cover". This probably requires some clarification. [Samuel Morin, France]	Accepted. Text has been revised in the FGD, by removing reference to the large snow cover.
20225	95	28	95	28	Please correct to "of the complex" [philippe waldteufel, France]	Not applicable. This text has been removed in the FGD.
16307	95	35	95	45	There is a high precipitation bias over the northeastern Tibet for many models, which should also be mentioned here. [Cunde Xiao, China]	Not applicable. The assessment has been greatly shortened for the FGD to fit in the confines of a Cross-Chapter Box. While model biases and the difficulty in evaluating them are mentioned generally in the FGD, specific regional model biases are not assessed in a comprehensive way.
15667	95	47	95	47	It should remain clear that EDW refers here to EDW in the HKH, and the conclusions of the assessment shall not be considered universally valid for all mountain regions on Earth (see ES) [Samuel Morin, France]	Accepted. Clear caveats have been given in the FGD that EDW appears to have strong regional dependence.
22967	95	53	95	54	I assume some allusion to warming / increase is missing here for this to make sense? [Peter Thorne, Ireland]	Accepted. The text of the FGD has been revised to avoid such unclear statements.
116995	95		95		Please refer to SROCC for specific cryopshere aspects related to the HKH. [Valerie Masson-Delmotte, France]	Accepted. Assessments made in the SROCC have been used as starting points for the cryosphere work described in the FGD.
98467	96	46	96	47	Recitation (Palazzi et al., 2013) of reference which is also found in chapter Atlas (Atlas-111, line 44-47) [Mehwish Ramzan, Pakistan]	Rejected. Each chapter in AR6 has its own reference list that is separate and independent to that in others. Furthermore, the Atlas follows Ch10 rather than precedes it.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98469	96	47	96	48	Recitation/overlap (Roxy et al., 2015) of reference which is also found in chapter Atlas (Atlas-57, line 31) [Mehwish Ramzan, Pakistan]	Rejected. Each chapter in AR6 has its own reference list that is separate and independent to that in others. Furthermore, the Atlas follows Ch10 rather than precedes it.
116997	96		96		You need to coordinate with ch 6 7 and 9 on the effect of aerosol on snow to make sure that the assessment is consistent. [Valerie Masson-Delmotte, France]	Accepted. Cross-chapter coordination has taken place for Cross-Chapter Box10.4 in the FGD, reflected in the authorship.
45153	97	24			The Madhura et al. (2015) paper discusses changes in winter precipitation extremes over the Western Himalayas and their links to Western Disturbances (WDs), and not about the southwest monsoon cito rculation. So line 24 is not the correct place to cite Madhura et al. (2015). The citation of the Madhura et al. (2015) paper may be moved to line 22, page 97, after Dimri et al. (2015). The Madhura et al. (2015) reference is missing and may be included in the reference list.. Madhura R, R. Krishnan, J.V. Revadekar, M. Mujumdar and B.N. Goswami (2015): Changes in western disturbances over the Western Himalayas in a warming environment. Climate Dynamics 44, 1157–1168. [Krishnan Raghavan, India]	Accepted. The text has been revised in the FGD to avoid this confusing statement. In fact, the Madhura reference is not the most appropriate here and has been removed.
22969	97	27	97	29	Are you sure that this characterisation of the monsoon behaviour is consistent with the substantive assessment in chapter 8? It is critical that there not be a real or implied inconsistency arising here. [Peter Thorne, Ireland]	Accepted. The FGD has been revised to avoid making such an unclear and inconsistent statement. The assessment of Ch8 has of course been followed closely in the FGD.
59439	97	35	97	35	I suggest adding the following reference about the orography effects on heavy precipitation modelling in Andorra: Trapero, L., Bech, J., Lorente, J., 2013. Numerical modelling of heavy precipitation events over Eastern Pyrenees: Analysis of orographic effects. Atmos. Res. https://doi.org/10.1016/j.atmosres.2012.09.014 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This Cross-Chapter Box pertains to the Hindu Kush-Himalaya region and not other mountain regions such as the European Alps.
20701	97	38	97	42	In their short introductory summary, Rimi et al (2019) write: " Anthropogenic climate change doubled the likelihood of the 2017 pre-monsoon extreme 6-day rainfall event at northeast Bangladesh". Chapter 10 authors state however, referring to this very article, that there is no significant increase in the likelihood of extreme rainfall in Bangladesh in 2017 attributable to anthropogenic climate change. Please explain [philippe waldteufel, France]	Not applicable. No text pertaining to Bangladesh remains in the Cross-Chapter Box10.4 / HKH in the FGD.
59403	97	51	107	49	In Section 10.5; It would it be nice if authors could describe about predictability of regional climate in Distillation section. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The section is about climate projections and not climate predictability.
111585	97	53	98	5	It will be good to mention here Atlas.6 as like [Volodymyr Osadchy, Ukraine]	Rejected. This section is about method of distillation. Atlas has a different focus.
55161	97	54	97	54	inform adaption and policy decisions' should be revised to: 'inform adaptation and policy decisions' [Nancy Hamzawi, Canada]	Accepted. Editorial
4327	97	54	97	54	“adaption” → “adaptation” [Isla Simpson, United States of America]	Accepted. Editorial
66321	97		97		section 10.5 overlap with Ch12 Cross-Chapter Box 12.2 [Erika Coppola, Italy]	Taken into account: Text on climate service has been revised. Reference is now made to Chapter 12 and Cross-Chapter Box12.2
36333	98	1	98	1	Inconsistency and a possible gap in the assessment: Indigenous knowledge is mentioned here but not discussed in previsous sections of this chapter, for example, section 10.3.1. [PENDO MARO, Belgium]	Noted. Section 10.3.1 other sections discusses the methods and results used in the peer reviewed scientific literature. Only in the distillation process indigenous knowledge enters the process.
7943	98	2	98	2	"the role of the context": what context? [Bart van den Hurk, Netherlands]	Noted. This is further explained in section 10.5.2. “User” has been added
99417	98	8	100	45	While the concept of "climate message" is very relevant and important, "message" has the conotation of communication by media which often is in the form of simplified, abbreviated information (suitable for headlines). For practical decision-making, however, details and complexities matter, and this is also why in adaptation research frameworks like robust decision making or decision scaling have gained importance. [Birgit Bednar-Friedl, Austria]	Accepted. The term “message” has been dropped.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57471	98	10	98	10	I suggest adding the following two references to the Lourenco et al. (2016) one, both of which describe the major international activity in this area, the Global Framework for Climate Services, and provide wider context: Hewitt, C. D., S. Mason and D. Walland, 2012: The global framework for climate services, Nature Climate Change, 2, 831-832, doi:10.1038/nclimate1745 Hewitt, C. D., E. Allis, S. J. Mason, M. Muth, R. Pulwarty, J. Shumake-Guillemot, A. Bucher, M. Brunet, A. M. Fischer, A. M. Hama, R. K. Kolli, F. Lucio, O. Ndiaye and B. Tapia, 2020: Making society climate-resilient: international progress under the Global Framework for Climate Services, Bull. Amer. Meteor. Soc., E237-E252, DOI: 10.1175/BAMS-D-18-0211.1 [Chris Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, the references have been added.
39227	98	10	98	14	Has there been any efforts to quantify the differencesbetween benefits (vaues) of using commercialized climate services and tgat provided by national meteorological/climatological servces? Is this discussed anywhere in this chapter? [Lourdes Tibig, Philippines]	Not applicable. For space reasons, the section on climate services has been substantially shortened.
85061	98	14	98	14	Comment provided by Stacey New: At the end of this sentence I think there are a couple of papers that could be referenced: 1. Hewitt, Chris, Simon Mason, and David Walland. "The global framework for climate services.", Nature Climate Change (2012): 831-832. 2. Golding, Nicola, Chris Hewitt, and Peiqun Zhang. "Effective engagement for climate services: Methods in practice in China." Climate services 8 (2017): 72-76. [Stacey New, United Kingdom (of Great Britain and Northern Ireland)]	Noted. For space reasons only the first has been added.
57473	98	14	98	14	I suggest adding two references which describe co-design with users: "where the context is defined through co-design with users (Vincent et al., 2018, Golding et al., 2020)": Vincent, K., Daly, M., Scannell, C., Leathes, B., 2018: What can climate services learn from theory and practice of co-production? Climate Services, 12, 48-58. Golding, N., C. D. Hewitt, A. Taylor, J. Strachan, R. Parfitt and L. Vilarkin : The Rules of Engagement: Refining approaches to effective engagement for climate services, Climate Services (Submitted) [Chris Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. For space reasons the paragraph has been deleted. But the Vincent et al 2018 reference is cited later in this section.
105901	98	26			CGM → GCM [SAMUEL SOMOT, France]	Accepted. Changed.
1627	98	34	100	5	Section 10.5.1.1 starts by saying: 'Regional climate information may be constructed….' This is how to do something, not an assessment of what has been done. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The sentence is not a recommendation, but introducing an assessment of what is done.
125739	98	36	99	13	The first paragraph of Section 10.5.1.1, including the bullet list, is covered elsewhere in the chapter. Recommend removing for brevity. [Trigg Talley, United States of America]	Noted. To recall the variety of approaches in addition to the standard ones, we prefer to keep this list.
7945	98	41	98	43	Atlas.6.1.1 pays attention to viable applications of extrapolation of trends [Bart van den Hurk, Netherlands]	Not applicable. Atlas has been rewritten.
38561	98	41	98	56	In the formation of a climate change message, event attribution is an important process. Although this is the realm of CH11, CH10 should better mention event attribution and refer to CH11 for more development [robert vautard, France]	Accepted. Added.
38563	98	41	98	56	The approach used here is one way and misses the important interactions with society when forming messages, right from the start. It is mentioned later, but I would expect that this shows up right from the beginning (actually maybe near line 10). The section gives the idea that messaging comes directly from scientists, in a top down approach, while iterations are required. [robert vautard, France]	Rejected. Here, only sources of information are discussed, not how information is constructed (in a possible co-design with users).
105903	98	43			paragraph not well aligned with the following bullets [SAMUEL SOMOT, France]	Noted. Corrected.
7947	98	44	98	49	Atlas.6.1.2 pays attention to the use of GCM output, going a bit further than addressing bias correction or weighting [Bart van den Hurk, Netherlands]	Not applicable. Atlas has been rewritten.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59441	98	47	97	49	Consider adding the following citation referred to bias adjustment issues: Maraun, D., 2013. Bias correction, quantile mapping, and downscaling: Revisiting the inflation issue. J. Clim. https://doi.org/10.1175/JCLI-D-12-00821.1 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The reference is discussed in detail in the Cross Chapter Box.
45149	98	48	96	48	The citations Krishnan et al. 2013 and 2016 may be included. The Krishnan et al. 2013 reference is: Krishnan, R., T.P. Sabin, Ayantika Dey Choudhury, M. Sugi, A. Kitoh, H. Murakami, A. Turner, J.M. Slingo and K. Rajendran (2013): Will the South Asian monsoon overturning circulation stabilize any further? Climate Dynamics, 40, 187-211, DOI: 10.1007/s00382-012-1317-0 [Krishnan Raghavan, India]	Rejected. No references are needed, as links to other sections are provided for detail.
1401	98	50	98	53	When it comes to theory, there is also statistical theory, e.g. extreme value theory for fitting the upper tail of the pdfs. Moreover, statistical theory can also include expected properties of different types of numbers such as number of events (Poisson-type), duration of events (geometric - 'successive failures'), given the quantification of the parameters defining the shape of the pdf. A simple example is the mean and the standard deviation for e.g. temperature or the trend estimates for a multi-model ensemble (which too is quite close to being normally distributed; Benestad et al. 2016; DOI: 10.1088/1748-9326/11/5/054017). Local climate can be interpreted as 'weather statistics' which also implies that statistical theory also applies on par with physical laws (be it sampling, the law of small numbers, field significance or the properties of different pdfs). [Rasmus Benestad, Norway]	Rejected. The discussion here is about process understanding, not the application of statistical theory (which might anyway be used in the construction of the information, but it is no source).
7949	98	54	99	1	Atlas.6.1.5 pays attention to storylines as form of idealized scenarios [Bart van den Hurk, Netherlands]	Not applicable. Atlas has been rewritten. The short Section Atlas.2.4.2.3 is more or less a pointer to Chapter 10, so cross referencing is not justified.
45151	98	58	98	58	Replace "Krishnan et al. (2018) found a rising trend of WDs activity", by "Krishnan et al. (2019a) found a rising trend in the amplitude of the WDs" [Krishnan Raghavan, India]	Noted. The comment seems to be in the wrong place.
55163	98		99		Has there been consideration of how to incorporate community and citizen-based science and/or monitoring? What about Traditional or Indigenous Knowledge? Inclusion of this local perspective also assists in message development and ensuring usability and relevance of information or services being provided [Nancy Hamzawi, Canada]	Taken into account. A discussion about indigenous knowledge has been added in section 10.5.1
22973	99	2	99	2	This is ambiguous - what do you mean by not peer-reviewed? Grey literature would be clearer? [Peter Thorne, Ireland]	Not applicable. The text has been rewritten.
13599	99	8	99	8	It is suggested to include the meaning of UNFCCC: United Nations Framework Convention on Climate Change [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Changed.
67025	99	12	99	13	change "construction of event storylines" to "construction of physical climate storylines focused on events" [Liese Coulter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed.
1403	99	15	99	22	The crucial question is whether the different sources of information and uncertainty are independent. If they are independent, then the overall uncertainty may be reduced by combining different types of information (Benestad et al., 2017; DOI: 10.1038/NCLIMATE3393). [Rasmus Benestad, Norway]	Noted. Also if information sources are dependent, uncertainties can in principle be reduced (apart from the theoretical case that two sources of information provide identical information up to a scaling factor).
42743	99	24			This paragraph would be better placed at the beginning of the next section. An implication of the important (and often experienced) point being made in this paragraph is the need for producers and users to work together – and this issue is addressed in the following section. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The text has been rearranged, but as the paragraph discusses the abundance of information sources, we believe it is still well placed in the section on information sources.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
85063	99	26	100	5	Comment provided by Stacey New: The Global Framework for climate services (GFCS) should be mentioned in this section. An extract taken from the GFCS website (https://gfcs.wmo.int/node/219) is "The results will be an effective global partnership for identifying and meeting user needs for climate information; the effective application of climate observations, socio-economic data, models and predictions to solving national, regional and global problems; a system for transforming data into information products and services to inform decision making; and increased capacity around the world for producing and using climate services". I think this fits within section 10.5.1.2. [Stacey New, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The text has been merged, the reference has been added in a separate short section on climate services (10.5.4).
38565	99	34	100	5	Here or elsewhere could be intoduced the concept of search of climatic impact drivers for a given application, and refer to CH12 for more details. [robert vautard, France]	Noted. A reference has been added in the introductory paragraph of Section 10.5.
7951	99	45	99	45	there are quite a few types of bottom-up approaches, not all of them are linked to robust decision making (see e.g. Berkhout, F., B. van den Hurk, J. Bessembinder, J. de Boer, B. Bregman and M. van Drunen (2014): Framing climate uncertainty: using socio-economic and climate scenarios in assessing climate vulnerability and adaptation; Regional and Environmental Change 14 (3), 879-893 [Bart van den Hurk, Netherlands]	Taken into account. We have added 10.5.3.3 discussing these approaches.
59451	99	45	99	45	Culley et al., 2016 was cited but not listed in the reference section [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Has been corrected.
13643	99	46	99	46	It is suggested to mention what the text refers to with non-climatic stressors. [Maria Amparo Martinez Arroyo, Mexico]	Noted. The whole section 10.5 has been substantially revised. The term "non-climatic stressors" is now clear from the context.
1407	100	10	100	17	There may be a difference for academics in universities and those working in national meteorological services. As an employee at the Norwegian Meteorological Institute, we do get a great deal of queries and questions asked by people from various sectors of the society, and based on these questions and dialogues, we do also have a fairly good idea what people are asking for. One useful way to find out is to ask them how they intend to use the piece of information they request. Also, what would make them choose different options and how would it affect them if the infromation turned out to be wrong? (e.g. a seasonal forecast). [Rasmus Benestad, Norway]	Noted. Text restructured, comment no longer relevant
13601	100	16	100	17	Remove the gray mark [Maria Amparo Martinez Arroyo, Mexico]	Noted. Text restructured, comment no longer relevant
57475	100	17	100	17	The recent Hewitt et al. (2020) paper also summarises the historical role and development of climate services globally. This sentence could perhaps be slightly reworded to read " The historical development and role of climate services is discussed in Chapter 12, and summarised in Hewitt et al. (2020)": Hewitt, C. D., E. Allis, S. J. Mason, M. Muth, R. Pulwarty, J. Shumake-Guillemot, A. Bucher, M. Brunet, A. M. Fischer, A. M. Hama, R. K. Kolli, F. Lucio, O. Ndiaye and B. Tapia, 2020: Making society climate-resilient: international progress under the Global Framework for Climate Services, Bull. Amer. Meteor. Soc., E237-E252, DOI: 10.1175/BAMS-D-18-0211.1 [Chris Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Section rewritten
57477	100	19	100	19	We are currently undertaking a large European project exactly as a result of the point made in this sentence, and including most of the people and organisations that are currently described in the text. I suggest adding a citation which expands on the point in this sentence: "Different climate service providers use different approaches for constructing regional information (Hewitt and Lowe, 2018)": Hewitt, C., and J. Lowe, 2018: Towards a European Climate Prediction System, Bull. Amer. Meteor. Soc., 99, 1997-2001, doi:10.1175/BAMS-D-18-0022.1 [Chris Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Section rewritten.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
85065	100	19	100	40	Comment provided by Stacey New: Can more examples be added here, for example there is a lot of climate service work occurring in China through the CSSP China project. 1. Golding, Nicola, Chris Hewitt, and Peiqun Zhang. ""Effective engagement for climate services: Methods in practice in China."" Climate services 8 (2017): 72-76. 2. Another case study could be referenced which has been through all stages of the prototyping cycle is CSSP China Yangtze River Basin seasonal forecast - Golding, N., C. Hewitt, P. Zhang, M. Liu, J. Zhang and P. Bett, 2019: Co-development of a seasonal rainfall forecast service: Supporting flood risk management for the Yangtze River basin. [Stacey New, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Section rewritten
1405	100	19	100	40	On a technical side, there are ways to present high-resolution maps of large multi-model ensembles in reasonably small data volumes through the help of singular vector decomposition (SVD). The SVD distills the salient information embedded in the vast data volumes. There is a demonstration for the RCPs 2.6, 4.5, & 8.5 (254 runs, each which included 4 seasons over the perdio 1900-2100) this concept for the Barents region (Benestad et al., 2017; DOI: 10.1016/j.cliser.2017.06.013). With the help of the SVD-representation of the data, it's possible to dissect the data and filter it with respect to a set of e.g. GCMs. One limitation is that it is most suitable for seasonal aggregates. However, if the SVD-repersenation is used for e.g. seasonal mean temperature and standard deviation (of daily values), then it's also possible to estimate probabilities for more extreme values (e.g. warm and cold days). This type of approach, however, raises the question of what is the best format for downscaled multi-model ensembles. [Rasmus Benestad, Norway]	Noted. Text restructured, comment no longer relevant
44443	100	19	100	40	The choice of exapmles for climate services in this paragraph seems arbitrary and don't have a regional balance. Such cherry picking should be avoided. References used could be taken up in section 12.6 in Ch12 were climate services are treated more generic. [Jana Sillmann, Norway]	Noted. Text restructured, comment no longer relevant
105905	100	19			It would be fair to cite other climate service example for which Euro-CORDEX ensemble was the background information with bias correction, so relying on community efforts (France, Swiss). See Jouzel, J., Ouzeau, G., Deque, M., Jouini, M., Planton, S., & Vautard, R. (2014). Le climat de la France au XXIème siècle. Volume 4. Scénarios régionalisés: éditions 2014 pour la métropole et les régions d'outre-mer. For the presentation of the ensemble used in the French DRIAS climate service. [SAMUEL SOMOT, France]	Noted. Section rewritten.
42745	100	19			It seems to me that there are some key messages in this paragraph that only come across as incidental comments. These are: 1)A strong caution about using a too limited GCM selection that does not span the range of uncertainty. A consequence will be projections that are misleading for adaptation planning. 2)The use of high-resolution (e.g. convective scale) downscaling of only one GCM/RCM. These types of simulations are valuable for process investigations but caution is required not to over-interpret these as quantitative projections for use in planning, at least without demonstrating their placement within a wider ensemble of projections. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text restructured, comment no longer relevant
10941	100	31	100	31	Another more recent example of a climate service providing very high-resolution downscaling at the urban scale, including IDF curves (but for Europe), is this one: Gidhagen, L., Olsson, J., Amorim, J.H., Asker, C., Belusic, D., Carvalho, A.C., Engardt, M., Hundecha, Y., Körnich, H., Lind, P., Lindstedt, D., Olsson, E., Rosberg, J., Segersson, D., and L. Strömbäck (2019) Towards climate services for European cities: lessons learnt from the Copernicus Climate Change Service Urban SIS, Urban Clim., 31, 100549, doi: 10.1016/j.uclim.2019.100549. [Jonas Olsson, Sweden]	Noted. Section rewritten.
41205	100	32	100	32	wrong use of confidence language [TSU WGI, France]	Noted. Text restructured, comment no longer relevant
7953	100	34	100	36	Often it stated that users need to get engaged in research to generate proper scientific uptake in decision making. We hardly ever explicitly point at the need (or even possibility) for scientists to exhibit curiosity of users' contexts and drivers, and transfer these insights into their research. This is probably a more effective way to steer (also underpinning) research into a direction that helps users' needs then the (often cumbersome) process to try to familiarize non-scientific users into the practice of research definition, execution and interpretation [Bart van den Hurk, Netherlands]	Noted. Text restructured, comment no longer relevant
13603	100	36	100	37	Remove the gray mark [Maria Amparo Martinez Arroyo, Mexico]	Noted. Text restructured, comment no longer relevant

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
87227	100	36		40	This point touches on the role of knowledge in climate governance. A couple of references to that might be useful: Allan, B.B. (2017). Producing the Climate: States, Scientists, and the Constitution of Global Governance Objects. International Organization, 71 (1), 131-162; Allan, B.B. (2017). Second Only to Nuclear War: Science and the Making of Existential Threat in Global Climate Governance. International Studies Quarterly, 61 (4), 809-820; Leino, H., & Peltomaa, J. (2012). Situated knowledge–situated legitimacy: Consequences of citizen participation in local environmental governance. Policy and Society, 31 (2), 159-168; Van Kerkhoff, L., & Pilbeam, V. (2017). Understanding socio-cultural dimensions of environmental decision-making: A knowledge governance approach. Environmental Science and Policy, 73, 29-37. [Rodolfo Sapiains, Chile]	Noted. Text restructured, comment no longer relevant
57479	100	37	100	37	I suggest adding the following reference after Bremer and Meisch (2017) which also discusses the diversity of perspectives: Hewitt, C. D., R. C. Stone and A. B. Tait, 2017: Improving the use of climate information in decision-making, Nature Climate Change, 7, 614-616. [Chris Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Section rewritten..
105907	100	39		40	Is there any sign in the literature that this co-construction could lead to deform the provided climate change information under the influence of climate-skeptic lobbying or national-oriented objective or sectorial-oriented objective. I have no reference for this but the risk does exist as for other scientific fields in the past. [SAMUEL SOMOT, France]	Rejected. Need evidence from literature
20227	100	42	100	43	There is something wrong with this sentence [philippe waldteufel, France]	Noted. Text restructured, comment no longer relevant
57481	100	42	100	45	I don't understand the points being made here, and I think there are some missing words because the two sentences don't make sense. In particular, I don't understand why "by periodic reports such as national assessment reports" is written here. There are many more examples of how climate services are assisting in decision-making. Perhaps rewrite to something like "Climate services also provide a platform for the operational generation of climate information and knowledge. Climate services are assisting in decision-making, in some cases through regular national assessment reports (Vincent et al., 2017) and IPCC reports, and in other cases through direct and close engagement between climate service providers and decision-makers (for example, Golding et al., 2017a, 2019). ": Golding, N., C. Hewitt and P. Zhang, 2017a: Effective engagement for climate services: Methods in Practice in China, Climate Services, 8, 72.76. Golding, N., C. Hewitt, P. Zhang, M. Liu, J. Zhang and P. Bett, 2019: Co-Development of a Seasonal Rainfall Forecast Service: Supporting flood risk management for the Yangtze River Basin, Climate Risk Management, 23, 43-49, https://doi.org/10.1016/j.crm.2019.01.002 [Chris Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text restructured, comment no longer relevant
7955	100	43	100	43	"reports also entails" sounds strange [Bart van den Hurk, Netherlands]	Noted. Text revised
1629	100	48	129	33	What is the purpose of Section 10.5.2? Much of this and the later sections reads like how to do something, not an assessment of what's been done. Why is this in an IPCC Report? I wouldn't expect to find it there. Some is a good review, but it's not what I expect an IPCC Report to cover. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Concepts presented in 10.5.2 contribute to the assessment statements that have been added to the end of 10.5.3. The intent of 10.5.2 and later sections is to present and assess the methodologies for producing climate information that can be a foundation for communicating with stakeholder affected by climate change. It is vital to develop an understanding of how this information is attained and what are the strengths and limitations to various approaches. It is part of the charge to this chapter.
111587	100	52	100	55	Why informing only adaptation and policy? What about general public or educational institutions - schools, universities? Climate information can be used for rising awareness in societies, since there are still many deniers or just ignorant people. But without the support of society nothing can be done on any level from local to regional and international [Volodymyr Osadchy, Ukraine]	Noted. The phrase "informing adaptation and policy" covers all of those who would be informed. The text has been retained as is: there is not room to provide an exhaustive list and discussion of every element of society that could be informed , including those given by the reviewer.
3367	100		10	26	I think it is important to be more specific in the contributions mentioned here, in order to continue with the progress presented by the document in the following paragraphs, which in themselves are very valuable [Eduardo Erazo Acosta, Colombia]	Noted. Text restructured, comment no longer relevant
116999	100		100		please check references to cimate services in SR15, SRCCL and SROCC too. [Valerie Masson-Delmotte, France]	Noted. Section about climate services has been moved to 10.5.4 and has been shortened and is now more extensively discussed in Chapter 12

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39229	101	1	101	53	This discussion on the different contextual elements in constructing regionl climate messages is very informative. Lines 28-30 is particularly true in most deveoping countries. Are there specific exmples on how these could be addressed? [Lourdes Tibig, Philippines]	Noted. Section 10.6 provides examples of contextual elements, and two of the three examples pertain to developing regions. Also, some of the citations in this part of the text (e.g., Doron et al. and Scott et al.) are based on experiences in developing regions.
20703	101	5	101	8	Water managers unable to recognize the dependency of the city on different water resources would certainly not keep their jobs. Besides, this belongs to politico economic context rather than social. Please try to provide a more adequate example [philippe waldteufel, France]	Rejected. The cited reference and the example in 10.6.2 show that there is more complexity involved than suggested by the reviewer's comment and entails considerations that are not simply political.
87229	101	10		37	Similar to this are all the particularities of the Global Southaffecting not only the production of science but also the role science plays in each society. A paper under review addresses these ideas. This is a quote: "In the Global South, multi-level, multi-actor climate governance occurs in a context of inequality and asymmetric power relations, rising environmental conflicts, and a lack of tradition or mechanisms for community participation. Addressing the problem of climate change here will require an approach that acknowledges the State alone cannot solve the issue: different disciplines and perspectives must be integrated and wider participation of diverse stakeholders promoted". Sapiains, R.; Ibarra, C.; Jiménez, G.; O´Ryan, R.; Blanco, G.; Moraga, P. & Rojas, M. (under review). Exploring the contours of Climate Governance: An interdisciplinary systematic literature review from a Southern perspective" resubmitted to Environmental Policy and Governance. [Rodolfo Sapiains, Chile]	Accepted. This paper has been cited as evidence supporting this contextual contrasts in value systems such as the different views of the Global North compared to those of economies in transition or under development.
7957	101	16	101	16	one "a" too many in this sentence [Bart van den Hurk, Netherlands]	Rejected. The usage is needed to identify specific cases, and it has been accepted by the copy editor.
54423	101	33	101	51	One concrete example of a schematic depicted in Figure 10.22 would be extremely helpful for a reader to better understand and imagine it [Gabriel Stachura, Poland]	Taken into account. The figure is now part of Box 10.2, which discusses storylines and their application in detail. The caption to Fig. 10.22 directs the reader to Sheperd el a. (2018) for examples.
67027	101	35	101	35	change "storylines" to scenario and physical climate storylines" to clarify both are valued [Liese Coulter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The term "storylines" expresses the contrast with quantitative information. References to additional text have been updated to point to Section 1.4.4 and Box 10.2, which provide further details.
67029	101	35	101	35	change (Section 1.4.3) which does not relate to storylines to (Section 1.4.4) which relates to storylines [Liese Coulter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text has been updated now to refer to section 1.4.4.
7959	101	36	101	36	Although I like figure 10.22, it is not readily interpretable as a "risk framework" [Bart van den Hurk, Netherlands]	Accepted. Wording changed to that used by Shepherd et al. (2019) to describe the figure, "a causal network describing regional climate risk".
87231	102	1		22	Same as above [Rodolfo Sapiains, Chile]	Rejected. Presumably referring to comment 87229. It is not clear that the paper referenced addresses explicitly issues of values.
22975	102	1			This is a very nice and well written piece but I wonder whether it overtly steps all over WG2 remit (and somewhat WG3 remit) in a manner which may not be acceptable to those WGs. It feels very far removed from the WG1 central charge. [Peter Thorne, Ireland]	Noted. This section fits the remit of Chapter 10 to present and assess issues involved with developing climate information that is relevant for regions.
55165	102	3	102	22	Behavioural change science/research is likely important here as well. https://www.unenvironment.org/news-and-stories/story/five-ways-behavioural-science-can-transform-climate-change-action [Nancy Hamzawi, Canada]	Noted. The reference provided is not citable by the assessment report, but behavioural science underlies many of the references cited in this subsection that have been retained for the final draft.
7961	102	5	102	5	I don't understand this sentence on targeting regions here [Bart van den Hurk, Netherlands]	Accepted. Sentence removed.
54425	102	39	102	44	It should be followed with an example, as in p.103, 16-19 [Gabriel Stachura, Poland]	Rejected. This paragraph has been shortened considerably to present concepts involved with producing climate information.
20705	102	51	103	1	These caveats induce to wonder whether this section 10.5.2.2. is closer to a message or to an information. Let us hope that chapter 10 authors make the best of this short remark [philippe waldteufel, France]	Noted. The relationship between data, information and messages has changed, and the term message is no longer used. Rather, the discussion focuses on regional climate information.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70929	102	52	102	52	The word "However" suggests that this sentence is somehow providing evidence against the previous sentence. In fact, they seem to be quite consistent with each other. Suggest changing to "Indeed" or similar. This is not simply editorial, as it affects the scientific logic of what is being said. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This subsection has been substantially rewritten, with the "however" now used appropriately.
68969	102	52	102	52	Remove "However". [Seth McGinnis, United States of America]	Taken into account. This subsection has been substantially rewritten, with the "however" now used appropriately.
7963	103	12	103	12	Here it is implied (but not explicitly stated) that the climate message is about a climate change features. Messages about climate measures are also climate messages, and this may confuse the fact that resistance against climate measures can also come from a "climate message" [Bart van den Hurk, Netherlands]	Rejected. The "measures" are responses to the climate information.
112855	103	54	104	1	An appropriate reference would be: Zappa, G. Regional Climate Impacts of Future Changes in the Mid–Latitude Atmospheric Circulation: a Storyline View. Curr Clim Change Rep 5, 358–371 (2019). https://doi.org/10.1007/s40641-019-00146-7 [Paula Gonzalez, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Reference has now been made at that point to Box 10.2, which gives much further detail and citations concerning storylines.
67031	103	55	103	55	change "process-based storylines" to "physical climate storylines" to maintain common terminology [Liese Coulter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Change has been made.
117001	103		103		Could it be possible to define terms here ("skeptical person") or use terms well grounded in social sciences (maybe to check with authors of WGII related to perception, values, beliefs, or SR15 authors involved in the corresponding chapter?) [Valerie Masson-Delmotte, France]	Noted. The term "sceptical" does not appear in this chapter, and the papers cited here come from the social sciences and so reflect their terminology.
7965	104	1	104	1	An example of a large-scale storyline affecting local information on sea level rise is given by Haasnoot, Marjolijn; Kwadijk, Jaap; Van Alphen, Jos; Le Bars, Dewi; van den Hurk, Bart; Diermanse, Ferdinand; van der Spek, Ad; Essink, Gualbert; Delsman, Joost; Mens, Marjolein: "Adaptation to uncertain sea-level rise; how uncertainty in Antarctic mass-loss impacts the coastal adaptation strategy of the Netherlands" ; ERL 15-3; DOI 10.1088/1748-9326/ab666c (https://iopscience.iop.org/article/10.1088/1748-9326/ab666c) [Bart van den Hurk, Netherlands]	Accepted. Conceptual example replaced with this concrete example.
39231	104	13	104	28	Compound events are becoming very frequent in recentyears. Most developing countries re most vulnerable, I am a bit confused as to how this chapter delivers the handshake between WG I to WG II. [Lourdes Tibig, Philippines]	Noted. This chapter is part of a set of chapters (10, 11, 12) and the Atlas that collectively form the handshake with WG2..The subsection covering compound events has been deleted.
132375	104	13	104	50	Section 11.8 addresses compound events in substantial detail. Please add a reference to that section here. [Sonia Seneviratne, Switzerland]	Noted. This subsection has been deleted in the final draft.
22977	104	15	104	17	Arguably the more impactful events are linked rather than concurrent events. For example late winter snow followed by flash drought (NW Europe, 2018) or hurricane followed by heatwave (Florida, 2018) which stresses human, physical and biological systems. Allusion to such events would be useful here. e.g. Matthews, T, Wilby, R, Murphy, C (2019) An emerging tropical cyclone-deadly heat compound hazard, Nature Climate Change, 9, ISSN: 1758-678X. DOI: 10.1038/s41558-019-0525-6. [Peter Thorne, Ireland]	Noted. This subsection has been deleted in the final draft.
1409	104	15	104	28	Might as well mention that a combination of a pandemic like Covid-19 and flooding/heatwave will make life even more difficult. [Rasmus Benestad, Norway]	Noted. Discussion of the relationship with the pandemic is occurring elsewhere in WG1, and this particular subsection has been deleted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
20707	104	15	104	41	What you write is certainly true; however, this complexity is well understood and accounted for by the climatological branches of weather services, when they are exchanging with specific communities of users. It is a mystery that this chapter never mentions the activities of operational climatology services. To criticize them would be more useful than to ignore them. While producing information about the future goes beyond what is expected from the operational climatology services, their know-how for dealing with non-traditional variables is operational and available [philippe waldteufel, France]	Taken into account. Climate services were, in fact, the topic of the SOD's Section 10.5.1.3. Sections 1.2.3, and 12.6, and Cross-Chapter Box 12.2 have addressed climate services more thoroughly.
44445	104	17	104	17	replace "hazard" by climatic impact drivers" as is the main term used in Ch12. [Jana Sillmann, Norway]	Noted. This subsection has been deleted in the final draft.
88481	104	28	104	28	It might be relevant to note something like "Compound storm types consisting of co-located cyclone, front and thunderstorm systems have been found to cause a significantly 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]	Noted. However, this subsection has been deleted from the final draft.
69905	104	30	104	41	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. 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.	Rejected. This statement is not relevant to the presentation of non-traditional variables and the subsection has been deleted from the final draft.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66875	104	30	104	41	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]	Rejected. This statement is not relevant to the presentation of non-traditional variables and the subsection has been deleted from the final draft.
66877	104	30	104	41	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]	Rejected. This statement is not relevant to the presentation of non-traditional variables and the subsection has been deleted from the final draft.
68449	104	30	104	41	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]	Rejected. This statement is not relevant to the presentation of non-traditional variables and the subsection has been deleted from the final draft.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68451	104	30	104	41	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]	Rejected. This statement is not relevant to the presentation of non-traditional variables and the subsection has been deleted from the final draft.
105909	104	30			Tourism Confort Index could be relevant here. See for example Dubois et al. 2016, https://doi.org/10.1186/s40322-016-0034-y [SAMUEL SOMOT, France]	Noted. This paragraph has been deleted, so the comment is no longer applicable.
22979	104	32	104	33	There are many more recent papers on heat stress and even comparing heat stress indexes than a 1980 paper. This can be improved by citing any number off papers that have appeared since AR6 instead of a paper that predates the IPCC process. [Peter Thorne, Ireland]	Noted. This paragraph has been deleted, so the comment is no longer applicable.
82727	104	35	104	35	Both the heat index used and the 40.6 C threshold are specific to the United States (as described in the Anderson et al 2013 paper). Suggest adding "by the U.S. National Weather Service" after "dangerous". [Blair Trewin, Australia]	Noted. This subsection has been deleted in the final draft.
67033	104	44	104	44	change "(Sections 1.4.3 ..." which does not relate to storylines to" (Sections 1.4.4 ..." which relates to storylines [Liese Coulter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Change has been made.
106619	105	1	105	42	The paper Jack et al., 2020 is very relevant in this section (and maybe elsewhere in 10.5, e.g. 10.5.2) so suggest its findings are considered to be included in the assessment. Jack, C., R. G. Jones, L. Burgin and J. Daron, 2020: Climate Risk Narratives: An iterative reflective co-production process for producing and integrating climate knowledge, Climate Risk Management (accepted). [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This paper has been included in the revised text and is now contained in Box 10.2 on storylines.
35431	105	1		42	The inclusion of these approaches is of extraordinary importance. The investigation of climate change, as an essential topic in environmental studies, must deal with the paradigm of complexity and focus on the nature-society binomial. [Gladys Linares-Fleites, Mexico]	Noted. The information in this subsection has been highlighted in Box 10.2, which focuses in detail on storylines.
77699	105	1			Narratives and storylines are mentioned throughout the document. This section appears halfway through but does not contain a definition or example of what narratives and storylines are. Would it be worth including these early in the chapter? [Emer Griffin, Ireland]	Taken into account. There is now a separate box, Box 10.2, that discusses storylines and their use in detail.
13605	105	4	105	4	Change & by and [Maria Amparo Martinez Arroyo, Mexico]	Noted. Text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106621	105	4	105	4	Jack et al. 2020 would be relevant to add after Scott et al., 2018: Jack, C., R. G. Jones, L. Burgin and J. Daron, 2020: Climate Risk Narratives: An iterative reflective co-production process for producing and integrating climate knowledge, Climate Risk Management (accepted). [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text revised
7967	105	5	105	5	Atlas.6.1.5 also pays attention to storylines [Bart van den Hurk, Netherlands]	Noted. Text revised
67037	105	6	105	6	Change "Narratives/storylines have a purpose" to "Storyline narratives can be used" to clarify that these two are different, have many purposes and are useful in a climate context. [Liese Coulter, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text restructured, comment no longer relevant
68971	105	6	105	10	These two sentences are confusing and need revision. Maybe something like "A narrative is a description of some state of the past, present, and future climate based on a particular set of evidence. Storylines are plausible events or evolutionary sequences from the narrative that connect it to user context in terms of pathways, events, impacts, or consequences." [And then maybe an example of a specific CMIP6-based narrative and a storyline built from it.] [Seth McGinnis, United States of America]	Accepted. Text revised
67039	105	7	105	7	change "storylines of evolution and events may" to "storylines of events and their evolutions may" to avoid confusion with biological evolution [Liese Coulter, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text restructured, comment no longer relevant
67041	105	10	105	10	Change "It is recognized that there is need for expert judgment of projections of changing climate" to "There is recognized need for expert judgment in applying climate projections" for brevity, clarity and ease of reading [Liese Coulter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text revised
67043	105	12	105	13	Change "Storylines built on narratives of the projected change that can arise in many ways, allow tailoring them for their intended use" to "Storylines built on narratives of projected change can arise in many ways and be tailored for their intended use." for grammer, clarity and ease of reading [Liese Coulter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text revised
13607	105	16	105	16	Delete Cobarllis, 2019 because it is repeated in the following line. [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Text revised
67045	105	20	105	20	Add final sentence to paragraph: The term 'physical climate storylines' is used to reference self-consistent and plausible unfoldings of a physical trajectory of the climate system (Section 1.4.4). [Liese Coulter, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text restructured, comment no longer relevant

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39233	105	22	105	29	Yes, the use of the terms narratives and storylines is not consistent in the literature. And when policymakers are told these storylines are meant for decision-making, that is where the problem of getting the important messages lies. it is quite difficult for scientists to explain there are storylines of historical events, and others. For quite sometime storylines to practitioners in the field have just been climate scenarios. Now, we have to distinguish between a description of the state of the past, the present and the future.. [Lourdes Tibig, Philippines]	Noted. Text restructured, comment no longer relevant
67049	105	22	105	29	All references remain the same, only the text has been edited for clarity and consistency. Replace existing paragraph with: The terms narratives and storylines have not been consistently applied in the literature. They have variously referred to potential changes in regional precipitation based on expert elicitation and climate processes (Dessai et al., 2018), plausible drivers of change in atmospheric circulation and other physical processes (Hazeleger et al., 2015; Zappa and Shepherd, 2017) (Figure 10.22) and to discourse analysis in energy transitions and climate action (Moezzi et al., 2017). In the climate context, the use of these terms has developed from early compound phrasing of narrative storylines (Schneider, 2001) to transdisciplinary narrative framing (Scott et al., 2018) and physical climate storylines of plausible changes in atmospheric circulation (Zappa and Shepherd, 2017). [Liese Coulter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text revised
1411	105	31	105	31	It may be more correct to say information-based rather than data-based (information and data are different things and statistics can be considered as the art of getting objective information out of data). [Rasmus Benestad, Norway]	Noted. Text rewritten
67051	105	31	105	31	change "data-based" to "quantitative" for accuracy. [Liese Coulter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text revised
71015	105	31	105	36	It could be added that storylines are also useful for understanding complex extreme events involving multiple causal factors, such as ecosystem impacts (Lloyd and Shepherd 2020 doi 10.1111/nyas.14308). [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text restructured, comment no longer relevant
22981	105	32	105	33	Is it worth linking to the relevant chapter 4 section here? [Peter Thorne, Ireland]	Noted. Text restructured, comment no longer relevant
44447	105	33	105	33	In chapter 1 and other places (e.g. SPM) the wording "low-likelihood high impact" is used, and should be made consistent here. [Jana Sillmann, Norway]	Noted. Text restructured, comment no longer relevant
44449	105	36	105	36	Hazeleger et al. 2015 is cited many times in this section, add also other references: Hegdahl, T.J., K. Engeland, M. Müller and J. Sillmann, 2019: Atmospheric River induced floods in western Norway – under present and future climate, J. Hydrometeorology, now in press. AND/OR Schaller, N., J. Sillmann, M. Mueller, R. Haarsma, W. Hazeleger, T. Jahr Hegdahl, T. Kelder, G. van den Oord, A. Weerts, and K. Whan, 2019: The role of spatial and temporal model resolution in a flood event storyline approach in Western Norway, Weather and Climate Extremes, in press. [Jana Sillmann, Norway]	Noted. Text restructured, comment no longer relevant
111589	105	38	105	38	The text is really good, well-written with all possible information included. Just small addition -please, add countries with economies in transition to "developing countries" here [Volodymyr Osadchy, Ukraine]	Noted. Text restructured, comment no longer relevant
105915	105	45			I'm surprised not to see any reference to articles underlining conflicting messages between various sources of climate information. For example conflicting messages between GCM and RCM have been put forward over Europe recently at least for surface shortwave radiation and surface air temperature at large-scale and mostly for Europe. See Bartok et al. 2017 (already cited), Sorland et al. 2018 (already cited), Schwingshackl et al. 2019 https://doi.org/10.1088/1748-9326/ab4949, Boé et al. 2020 doi:10.1007/s00382-020-05153-1, Gutierrez et al. 2020 https://doi.org/10.1088/1748-9326/ab6666 [SAMUEL SOMOT, France]	Rejected. The concept of distillation carries with it the potential for inconsistent information from different sources, and this has been explicitly acknowledged in the listing of approaches to constructing information that include testing differences in simulations, with citations to previous subsection in the chapter.
117003	105		105		Please check the overall use of storylines in IPCC so far (SR15, chapter 3, box 3.8 is an attempt?), and in this WGI report (example of attempt in chapter 9 on sea level) and provide suggestions for consistent wording (for instance, ch 9 calls these descriptions "pathways"). [Valerie Masson-Delmotte, France]	Taken into account. The use of storylines, introduced in this report in Chapter 1, has been harmonized. Box 10.2 now contains a detailed description of storylines.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
54427	106	5	106	5	I highly recommend creating such definition and adding to glossary as it is a crucial term, occuring over 50 times in this chapter, including several times in Technical Summary [Gabriel Stachura, Poland]	Rejected. The point here is that there have been multiple uses of the term, and this section cannot simply define away different uses, instead acknowledging them.
1413	106	5	106	12	Does the word 'synthesis' describe what is meant by destillation? It's probably a good idea to use common terms. Definition of synthesis: 'the composition or combination of parts or elements so as to form a whole; the combining of often diverse conceptions into a coherent whole' (merriam-webster.com) [Rasmus Benestad, Norway]	Noted. The term distillation carries with it, in addition, the specific aim of "from different sources of information in a given context", not simply synthesizing elements of information.
105911	106	5			So we can say that writing the IPCC report is a form of distillation (of the first aspect), especially for the regional-oriented chapter as various source of information are available [SAMUEL SOMOT, France]	Noted. As the reviewer states, the IPCC report covers the first aspect, but it does not explicitly cover the second aspect.
20709	106	14	106	14	Following the earlier reference to Kahan (2013) the presence of an expert in science of science-communication might be mandatory [philippe waldteufel, France]	Noted. Such a statement would be too prescriptive here.
105913	106	33			not sure McSweeney et al. really address the « impact models ». Their selection is mostly for RCM inputs. [SAMUEL SOMOT, France]	Noted. However, their motivation is to drive RCMs so that they produce "downscaled climate change information for consistent multi-regional assessment of climate change impacts and adaptation.."
68973	106	47	106	47	change "models, identification" to "models and identification" [Seth McGinnis, United States of America]	Accepted. Change made.
117005	106		106		This part of the assessment (information construction) reads a bit like a guidance note rather than an assessment. I think that it is designed to reflect best practice and methodologies. Some aspects are still quite implicit (how do you define and measure "confidence in climate information"). [Valerie Masson-Delmotte, France]	Taken into account. The subsection has been partially restructured and now concludes with assessment statements.
67053	107	1	107	1	change "event storyline" to "physical climate storyline" for consistency [Liese Coulter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The list of approaches to constructing information has been substantially revised.
67055	107	16	107	16	change "events/storylines" to "climatic events or storylines" for clarity [Liese Coulter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. That bullet item in the list has been substantially rewritten.
1415	107	46	107	49	I'm not sure if I understood this sentence, but just some thoughts: For most recipients, it's more useful to focus on what do we know and how do we know it. To tell the story behind our conclusions and the synthesis of all sources of information will also give the recipient an idea of the uncertainties. If we start with the uncertainties, then we may as well not bother in my experience. Everybody needs to deal with uncertainty all the time, from the time they get out of bed until when they go to sleep. Uncertainties are present in economic life, health, weather, social interactions, security (defence/fire), events (accidents), etc. If we were to start the day by conidering all the uncertainties that we face, then we would not get out of bed. Scientists are a different breed to most people, since researchers job involves charting the unknown and uncertain. We have this thing about uncertainty which may paralyse the decision-makers who just want a number. [Rasmus Benestad, Norway]	Noted. However, the comment overlooks a key phrase, "tailoring the uncertainty information to specific decision frameworks".
20711	108	3	128	24	This section presents a blatant contradiction with the previous one. It has been argued and repeated that messages ought to be co-constructed with the users, accounting for the context. In the 3 examples to follow, there are no users, no context, no exchange, no co-construction. In spite of some valuable contributions, this weakens enormously the overall value of this chapter 10 contribution to WG1 SOD. While the rest of the draft incites to several additional criticisms, I will stop to comment it. [philippe waldteufel, France]	Accepted. We recognize that an IPCC report, by its nature, cannot engage in with stakeholders in the co-construction of climate information. We have revised the text in accord with this restriction by (1) noting where the climate information presented sets the stage for further work that could be done by a co-production process and (2) citing, when possible and where relevant, the climate statements produced by national and regional assessments that engaged a broader community.
9989	108	9	108	9	"framed within a human dimension" is unclear. How many dimensions are there for the framing of climate messages? [Renard Siew, Malaysia]	Accepted. Wording has been changed to "The examples are framed taking into account societal perspectives that provide context..."

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
84729	108	10	108	10	here "South Asian monsoon" but in Table of content and related subsection it is speciied "Indian summer monsoon" [Annalisa Cherchi, Italy]	Accepted. Wording has been changed to "Indian summer-monsoon trends" to be consistent.
22985	108	26	108	38	Given that this is essentially a summation of prior sections I'm not sure what value it adds and would be minded to suggest its removal as it is adding nothing new for the reader not covered thus far in the chapter. [Peter Thorne, Ireland]	Rejected. The intention of the examples, as stated at the start of 10.6.1, is to illustrate, "steps for distilling regional climate information from the multiple sources of regional climate information presented in this chapter. These examples build on the general framework presented in Section 10.5, examining in particular the strengths and challenges in linking the different sources, while also exposing the assumptions behind and consequences of decisions made in the process. " A similar statement was in the SOD."
22983	108	30	108	33	You had a whole section saying just this. Does it really need to be repeated here, and particularly as if it is some new and novel insight without reference to the prior assessment section? Suggest to replace with a shortened cross-reference to the substantive earlier assessment within the chapter. [Peter Thorne, Ireland]	Accepted,. Part of that sentence combined with the previous sentence that already refers to section 10.4, retaining the phrase, ""attribution must account for the interplay between externally forced signals and unforced, internal modes of variability
84731	108	31	108	33	is this specification needed? [Annalisa Cherchi, Italy]	Accepted. The text has been revised to note the interplay between externally forced signals and unforced internal variability, and that ensembles of simulations can explore the interplay.
84733	108	46	108	46	the number should be compared to the mean consumption of water, just to emphasize how severe it has been [Annalisa Cherchi, Italy]	Accepted. The pre-drought value and appropriate reference have been included
117007	108		108		What about modes of variability and surprises? (for instance a major volcanic eruption is a surprise and not an abrupt change). [Valerie Masson-Delmotte, France]	Noted. We discuss modes of variability (e.g., SAM) where appropriate and where there is literature supporting the discussion. The examples are intended to be diverse in geographic distribution and relevant processes, so each one does not necessarily cover every potential contributor to a region's climate change.
117009	108		108		please explain the choice of the examples. [Valerie Masson-Delmotte, France]	Taken into account. As stated in the text, we chose these three examples because "they provide a geographically diverse set of locations and relevant processes and because most of the components for constructing regional climate information outlined in Chapter 10 are directly relevant to each case."
82731	109	3	109	15	Somewhere, either in the text or through a variation to Figure 10.24, it would be useful to have data on how far the 3-year total rainfall for 2015-2017 was below the long-term average and below the previous record low for a 3-year period. [Blair Trewin, Australia]	Rejected. The strength of the anomaly is already described by its return period, i.e. 1 in 300 years, which is how the anomaly has been expressed in the literature. Expressing it in % or mm would require additional explanations for it to be defensible and would not add to the key point: the drought was rare in intensity.
84735	109	4	109	4	few words more should be included about the meaning of this "severity" measure [Annalisa Cherchi, Italy]	Taken into account. The rarity of the event is now also noted by way of it being a 1 in 300-yr event.
59375	109	7	109	7	in effect decoupling…: this part of the sentence is not clear to me. Did you mean the water saving actions effectively mitigate the stress of water demand by population growth? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Wording has been revised to point to a previous coupling of growth in water demand with growth in population.
59377	109	7	109	7	the link in the DWA, 2013 blibliography is missing. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The correct URL is now included.
59379	109	9	109	9	Perhaps an additional “However,” to the sentence “The water-saving actions…” would emphasize the unforeseen consequences of those good saving actions and changing priorities. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Change made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1417	109	9	109	13	I would say the opposite is true: that internal variability is an aspect of the regional climate and it provides information about present processes and conditions - like a finger print of the regional climate system. The more pronounced, the more sensitive. In the mid-latitudes, it's influenced by prevailing winds, storm tracks, and fronts. There are monsoon systems. The climate models are able to reproduce many of their characteristics and the typical seasonality and geographical distribution gives an indication of processes. We also know that it's stochastic and part of the non-deterministic chaotic behaviour, but still we can quantify probabilities for exceeding given threshold states. Statistics is more predictable than outcomes. However, I'd agree if the message was that internal variability is a contrubutor to weather uncertainty. [Rasmus Benestad, Norway]	Noted. This comment appears to be misplaced, perhaps applying to p. 129, lines 9-13 of the SOD. However, the statement there is indeed saying that internal variability is an important contributor to regional climate that must be better understood, and this issue is retained in the FGD, Section 10.7.
54429	109	20	109	20	Regarding Figure 10.24c - some lines seem to change their color (e.g."GPCC v2018" and "station based" in the lower chart) [Gabriel Stachura, Poland]	Accepted. This is fixed in the updated figure.
59381	109	30	109	30	CRU is shown in a magenta line, not green as indicated by the text [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. This is fixed in the updated figure.
42985	109	32	109	32	COREX -> CORDEX [Bodo Ahrens, Germany]	Accepted. Correction made.
59383	109	46	109	53	This paragraph describes the impact of the drought and gives a clear picture of how the crisis was manifested in South Africa. However, it seems to hang by itself. Consider combine this paragraph with the first paragraph of 10.6.2.1. For example, perhaps move this paragraph to Line 52 of the 10-108 after the sentence "…developing countries.", and move "The crisis was widely…" to the end of the previous paragraph ending on Line 15 of the current page to summarize the implications and causes of the crisis. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Text in that paragraph has been moved to be part of the first paragraph.
4329	109	48	109	48	"increased city's" → "increase the city's" [Isla Simpson, United States of America]	Taken into account. That sentence was deleted in the reorganizing of 10.6.2.1.
71255	109				Figure 10.24 (c) SAM index in the figure has empty (), maybe delete if there is nothing inside. [Nesha Wright, Canada]	Noted. The "( )" has been used to indicate clearly that the SAM index is dimensionless.
59385	110	8	110	9	It is surprising to me that while Cape Town area receives most of its rainfall in the austral winter, the city is surrounded by summer rainfall regime. Is it topographically enhanced thunderstorms in the summer over the mountains or cutoff lows? Could you expand a bit on this? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The sentence was deleted since not immediately relevant to Cape Town's hydroclimate, which is Mediterranean, a feature we now note in the text. This climatology is further explained by the text already present.
1631	110	36	110	41	This is an assessment statement. The availability of data is an access issue. What you need is a statement that we need much more of the available data that has been made, digitsed if it isn't and made available if it's not. We could then look at regions knowing what is really available, [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The problem is not availability of data, but rather a lack of observing sites. The wording has been changed to clarify this point.
82733	110	41	110	41	The reference to Figure 10.25 is incorrect. There does not seem to be anything relevant in Figure 10.24 so possibly this refers to a figure which has been deleted? [Blair Trewin, Australia]	Accepted. The reference to a figure has been deleted. The point made is covered by the publication cited.
59387	110	41	110	41	Fig 10.24 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The reference to a figure has been deleted. The point made is covered by the publication cited.
59389	110	43	111	4	This paragraph should be in the next (10.6.2.4) section [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The paragraph has been moved as suggested.
82729	110	51	110	54	It may be worth drawing a connection with recent dry anomalies in some of these regions too, e.g. through cross-reference to the southern Australian drought material earlier in this chapter, or to section 10.6.2.8. [Blair Trewin, Australia]	Taken into account. That sentence was deleted in shortening 10.6.2.4.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10711	110	51	110	54	The dates for "Medieval Climate Anomaly" and "Little Ice Age" are different to those used elsewhere in this report. This highlights the problem of using such poorly defined terms. When used with other evidence it gives false confidence that such periods were consistently warm/cold. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. That sentence was deleted in shortening 10.6.2.4.
80337	110	53	110	53	CH2 uses Medieval Warming Period to refer to this warm period. Also, the period is defined between 950 and 1250 [Paola Arias, Colombia]	Taken into account. That text was deleted in shortening 10.6.2.4.
59391	111	7	112	27	10.6.2.4 includes many drivers of the rainfall variability in this region and are highly relevant to the discussions. However, paragraphs describing same process but with different properties are scattered throughout the section. For readers to follow more easily, consider group same process in one or two paragraphs and then move on to the next. For example, first talk about AAO and SAM, their decadal variability, how they are impacted by ozone and GHGs. Then move on to ocean (SST, ENSO and offshore current) and atmospheric rivers. Finally put this 2017-2019 event in the context of climate and analyze its drivers. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Section 10.6.2.4 has been reorganized and shortened, taking the comment into account. We continue to refer to these drivers elsewhere in 10.6.2 where relevant to the discussion in other subsections.
22987	111	11	111	12	The rest of the report uses SAM so suggest to use just SAM to avoid confusion and refer to the annex on modes of variability here. [Peter Thorne, Ireland]	Accepted. We now refer to Sections 3.3, 3.7, 4.3 and Annex IV.2.2, where general characteristics of the SAM are discussed in more detail, and we note here that the same phenomenon is sometimes referred to as the AAO in publications.
125741	111	14	111	17	The southern annular mode, by definition, describes a north-south shifting of the Southern Hemisphere polar jet, at least in the zonal mean. So, while the SAM isn't necessarily linked to fluctuations in the Hadley cell or subtropical jet position, by definition it should be linked to the polar jet position. [Trigg Talley, United States of America]	Noted. The text has been revised to refer to Sections 3.3, 3.7, 4.3 and Annex IV.2.2 for more general discussion of physical characteristics of the SAM. .
59393	111	43	111	43	the models typically underestimate observed trends" is not consistent with Line 38 in the previous paragraph where "the lack of robust long-term trend in observations" is stated. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The two paragraphs are talking about trends in two different fields (precipitation and then SAM), so they are not required to be the same.
22989	111	46	112	27	Suggest just using SAM here for within report consistency and text readability. Comment also applies to next sub-section [Peter Thorne, Ireland]	Accepted. Text has been changed to refer only to the SAM after recognizing that it is sometimes referred to as the AAO in literature.
125743	111	51	111	55	Somewhere in this section, it should be noted that the seasonality of the observed trends (largest during DJF and MAM) is consistent with both ozone and greenhouse gas forcing. See Ivy et al. (2017) for ozone (https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-16-0394.1) and Figure 12 of Barnes and Polvani (2013) or Figure 3 of Grise and Polvani (2016) for greenhouse gases (https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-12-00536.1; https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2015JD024687). It is often not appreciated that greenhouse gases appear to impart a seasonal influence on Southern Hemisphere circulation trends. [Trigg Talley, United States of America]	Taken into account. We added reference to Sections 3.3, 3.7, 4.3 and Annex IV.2.2, where general characteristics of the SAM are discussed in more detail.
125745	111	55	111	55	Change "tropical high pressure cell" to "Hadley circulation". [Trigg Talley, United States of America]	Accepted. Wording changed as requested.
22991	112	9	112	17	These linkages were the subject of a substantive assessment in chapter 3. Rather than repeat that analysis and inviting a reader to play spot the difference you should articulate what chapter 3 found and refer the reader to their analysis for further details here. [Peter Thorne, Ireland]	Taken into account. That text was deleted in shortening 10.6.2.4. Reference has been added to Sections 3.3, 3.7, 4.3 and Annex IV.2.2, where general characteristics of the SAM are discussed in more detail.
4331	112	15	112	15	"austral summer the SAM" → "austral summer SAM" [Isla Simpson, United States of America]	Taken into account. That text was deleted in shortening 10.6.2.4.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4333	112	37	112	37	Is it really correct to say that the models are inconsistent with the observations here when the observations is only one realization? It seems like that statement should rather be based on the extent to which the error bars of the observations and the models overlap with each other i.e., is there a less than a 5% chance that the models and the observations are drawn from the same distribution? Even for some of those models that show significant declines over 1933-2017 the whole model uncertainty range looks like it lies within the observed range. If this is a fair comment then perhaps it could be rephrased to "...GCMs simulate a significant decline in total annual rainfall, while there is no robust long-term trend in observations" [Isla Simpson, United States of America]	Taken into account. Statistical significance testing has been applied to the model results to indicate how many have a significant drying trend and the discrepancy with observations has been stated.
98131	112	38	112	38	Add here: Knutson and Zeng's (2018) univariate detection/attribution and consistency analysis for precipitation trends over 1901-2010, 1951-2010, and 1981-2010 using a 10-member CMIP5 model ensemble and GPCC precipitation data find no detectable (unusual compared to natural variability) trends for the gridbox in the Cape Town region. In a gridbox just north of Cape Town, there is a significant increasing trend in annual precipitation over 1901-2010, which is inconsistent with CMIP5 model historical runs that simulate a drying trend since 1901. There is another gridbox further north along the west coast where observations show a detectable anthropogenic drying since 1901, with a smaller drying trend also simulated by the CMIP5 models ensemble. [Thomas Knutson, United States of America]	Rejected. This contention is not explicitly described in Knutson and Zeng and does not add substantially to the multimodel presentation in Fig. 10.24.(now Fig. 10.18).
13645	112	40	112	41	It is recommended to mention what kind of models can capture the observed main hemispherical processes. [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Text changed to refer to global climate models.
43299	112	52		53	Read "(Almazroui et al., submitted; Figure 10.24)." rather than "(Almazroui et al. 52 (submitted); Figure 10.24)." [Cyriaque Rufin Nguimalet, Central African Republic]	Noted. The formatting using "submitted" was to indicate publications that have not yet been accepted. The reference in the final draft has been updated to include publication year.
69929	113	18	113	40	A large-ensemble simulation is another approach for obtaining future climate information particularly on extremes. Nosaka et al. (2020) demonstrated future changes in weather extremes using thousands-years climate simulations, that is, a subset of d4PDF. In addition, scalability is a key factor when understanding future climate. Nosaka, M, Ishii, M, Shiogama, H, Mizuta, R, Murata, A, Kawase, H, Sasaki, H (2020) Scalability of future climate changes for +1.5K, +2K, and +4K global warming in NHRCM large ensemble simulations. the same issue of Progress in Earth and Planetary Science (accepted) [Masayoshi Ishii, Japan]	Rejected. Nosaka et al. (2020) does not provide information relevant to the Cape Town case. GCM large-ensemble output is included in Fig. 10.18 of the FGD.
31453	113	18	113	40	Projected climate change impact has been assessed on a regional scale 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]	Rejected. The suggested paper does not apply to the Cape Town case.
91049	113	23	113	23	The gray literature sited (DEA, 2013, 2018) is based on the peer-reviewed paper of Engelbrecht et al. (2015): Engelbrecht F.A., Adegoke J., Bopape M-J., Naidoo M., Garland R., Thatcher M., McGregor J., Katzfey J., Werner M., Ichoku C. and Gatebe C. (2015). Projections of rapidly rising surface temperatures over Africa under low mitigation. Env. Res. Letters. 10 085004. [Francois Engelbrecht, South Africa]	Rejected. The suggested reference does not contain the information presented in the paragraph – it is concerned with overall continental assessment and with air temperatures mostly.
22993	113	23	113	30	These statements (ln 24-27 and ln 27-30) as written contradict one another and can't both be true. What is your assessment as to why? [Peter Thorne, Ireland]	Accepted. The text has been rewritten to note the consistency in precipitation decrease, but uncertainty in the characteristics of the decrease.
66323	113	24	113	30	This paper could be added ad a reference for S. Africa drought 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]	Rejected. The paper did not appear to have been accepted for publication by the deadline (Web of Science), and it did not appear to give substantial addition to what is already cited.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
91051	113	32	113	40	The authors may want to completely disregard this statistical downscaling result from the discussion. It is based on gray literature, and it contradicts peer-reviewed findings from global models and dynamic downscaling (which all consistently indicate a poleward expansion of the westerlies and associated reductions in frontal rainfall over the southern tip of South Africa). The statement that "thermodynamic considerations in a warming climate" may result in an opposite signal of increasing rainfall sounds vague and not defensible. [Francois Engelbrecht, South Africa]	Accepted. Reference text has been removed for the reasons given by the reviewer.
13609	114	8	114	8	Change 12K by 12,000 [Maria Amparo Martinez Arroyo, Mexico]	Taken into account. That text was deleted in shortening 10.6.2.4.
67059	114	12	114	12	for consistency after "approaches" add "for Cape Town" [Liese Coulter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The proper heading follows the bulleted list in 10.6.1.
59395	114	12	114	28	Section 10.6.2.9. This section summarizes the results from the future projections, which show consistent drying with strong dynamical arguments. However, I think it is also important to caveat that those simulations haven't been able to reproduce the historical records. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted The text has been revised, with much of the text moved to the final subsection, where the summarizing assessment statements recognize the inconsistency with the historical record, which reduces confidence in the projections to medium.
67057	114	14	114	15	For clarity and grammar, replace sentence with: Rainfall projections are relatively consistent based on the general understanding of rainfall drivers and the influence of warming on circulation dynamics and rainfall patterns in the region. [Liese Coulter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The focus of the sentence is on the drivers, not the precipitation. The suggested edit changes the meaning of the sentence. Sentence edited slightly to improved clarity.
59397	114	24	114	24	I don't recall we have discussed warming locally over Cape Town. Please refer to those studies. If you are referring to the general warming at the global scale, please clarify. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Text on regional warming has been removed as part of the shortening of the text.
13611	114	28	114	28	Resize degrees symbol [Maria Amparo Martinez Arroyo, Mexico]	Taken into account. Text on regional warming has been removed as part of the shortening of the text.
125747	114	35	114	35	Change "tropical high pressure cell" to "Hadley circulation". [Trigg Talley, United States of America]	Accepted. Change made.
22995	114	45	114	51	Why does the paleo evidence - which was pretty strong - not have a role to play here? Surely it would strengthen the evidence basis underpinning this assertion and add an indepenendent line of evidence? [Peter Thorne, Ireland]	Accepted. Paleoclimate behaviour added as supporting evidence.
70931	114	50	114	51	The role of stratospheric ozone change seems to be strongly overplayed here, since we are mainly talking about wintertime precipitation. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text removed since the focus is on winter precipitation and driving conditions, which are less subject to possible compensating influences of GHGs and Antarctic ozone.
45163	115	1	122	52	A National Climate Change Assessment Report for India entitled "Assessment of climate change over the Indian region" - Eds Krishnan et al. (2020) by Springer is being published shortly. This book contains useful material on regional climate change over the Indian region. Cross-referencing this report could help in shortening the text in Chapter 10. [Krishnan Raghavan, India]	Accepted. The National Climate Change Assessment Report has been assessed for the FGD and included in the final "multiple lines of evidence" subsection.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41931	115	1	122	52	Indian summer monsoon need not be just about India. Its regional character affecting all the South Asian countries needs to be adequately brought out. Further, this section is too long, focuses only on rainfall and can be substantially reduced. [Rupa Kumar Kolli, India]	Rejected. Given the space available in which to perform this fully integrated end-to-end case study of constructing regional climate information, we cannot expand the remit to consider other countries such as Nepal, Bangladesh, Pakistan etc in sufficient depth yet simultaneously reduce its length. However, the FGD version of 10.6.3 is substantially shortened and builds from earlier assessment statements made in Chapter 8.
69797	115	1	122	54	Rationale for assessing the Indian Summer Monsoon versus the South Asian Summer Monsoon, consistent with ATLAS and elsewhere, for example Figure 10.3 [Bhupesh Adhikary, Nepal]	Rejected. The region assessed here is the Indian summer monsoon. Given that this is a fully integrated case study showing an end-to-end assessment of how to construct regional climate information, there is not the space to expand to other regions of South Asia, such as Nepal, Bangladesh etc. Also, for India alone, there is the literature available to explore all aspects of the various methods discussed in Chapter 10.
84775	115	12	115	14	reference missing? [Annalisa Cherchi, Italy]	Accepted. References to the assessments made in appropriate chapters of the AR4 and AR5 have been made in the FGD.
22997	115	12	115	19	It feels disingenuous not to include a link back to the substantive assessment performed in chapter 8 here. [Peter Thorne, Ireland]	Accepted. Supporting links to the assessment of observed changes and future projections in Chapter 8 have been given in the FGD for this introductory subsection. In the further, more detailed subsections, the Chapter 8 assessments have also been used as suitable starting points.
72083	115	12	115	27	Based on recent study there is also clear understanding that Indian summer monsoon are declining during La Nina years (which is historically the wetter years) after 1980 relative to pre-1980 due to weaker La Nina events and warming of tropical Indian ocean. The relevant reference is also should be mentioned. --- Samanta, D., Rajagopalan, B., Karnauskas, K. B., Zhang, L., & Goodkin, N. F. (2020). La Niña's Diminishing Fingerprint on the Central Indian Summer Monsoon. Geophysical Research Letters, 47(2), e2019GL086237. [Samanta Dhrubajyoti, Singapore]	Rejected. This reference is not appropriate to the assessment and in particular this introductory subsection.
98471	115	14	115	16	Recitation/overlap (Goswami et al., 2016b)of reference which is also found in chapter Atlas (Atlas-57, line 25) [Mehwish Ramzan, Pakistan]	Rejected. Each chapter in AR6 has its own reference list that is separate and independent to that in others. Furthermore, the Atlas follows Ch10 rather than precedes it.
98473	115	21	115	22	Recitation (Sperber et al., 2013) of reference which is also found in chapter Atlas (Atlas-58, line 7) [Mehwish Ramzan, Pakistan]	Rejected. Each chapter in AR6 has its own reference list that is separate and independent to that in others. Furthermore, the Atlas follows Ch10 rather than precedes it.
22999	115	21	115	22	Chapter 8 should be the reference not an afterthought citation. Chapter 10 should point to the substantive assessment rather than perform its own interpretation here. [Peter Thorne, Ireland]	Accepted. The FGD contains appropriate references to the substantive assessments made in Chapter 8, although the specific issues relating to long-running failings in GCMs at simulating the Indian monsoon are still discussed in the FGD.
98479	115	22	115	25	Recitation (Choudhary et al., 2018)of reference which is also found in chapter Atlas (Atlas-21) [Mehwish Ramzan, Pakistan]	Rejected. Each chapter in AR6 has its own reference list that is separate and independent to that in others. Furthermore, the Atlas follows Ch10 rather than precedes it.
98475	115	26	115	27	Recitation (Dessai et al., 2018)of reference which is also found in chapter Atlas (Atlas-119, line 38-41) [Mehwish Ramzan, Pakistan]	Rejected. Each chapter in AR6 has its own reference list that is separate and independent to that in others. Furthermore, the Atlas follows Ch10 rather than precedes it.
98477	115	26	115	27	Overlap (Dessai et al., 2018) with chapter Atlas (Atlas-120, line 6-8) [Mehwish Ramzan, Pakistan]	Rejected. Each chapter in AR6 has its own reference list that is separate and independent to that in others. Furthermore, the Atlas follows Ch10 rather than precedes it.
88845	115	30	115	39	The words "northeast coast" here is like to cause confusion. The northeast (of India) is understood as the region between Bangladesh, China, and Myanmar. Perhaps this is better called upper/northern " parts of the east coast". [Krishna AchutaRao, India]	Accepted. The FGD wording has been revised accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41933	115	30	115	42	This sub-section is superfluous (with even some incorrect conjectures such as terming the monsoon flow as Somali Jet), and can be safely deleted without any loss of information. [Rupa Kumar Kolli, India]	Rejected. The subsection is not superfluous since it introduces the reader to further motivating factors pertaining to regional water resources as well as important aspects of the meteorology, particularly relating to orography, which is a theme that recurs in the observational and regional modelling subsections. Furthermore, subsections such as this have been kept in the FGD in order to maintain a consistent structure between all the fully integrated case studies of Section 10.6. Taken to account: with reference to the "Somali Jet" has been removed for simplicity, although we note that the AMS glossary of meteorology defines the Somali Jet as, "A low-level south-westerly jet over the Arabian Sea in the summer months, off the coast of Somalia. It is the northern branch of a cross-equatorial flow, giving rise to a major supply of moisture in support of the Asian summer monsoon".
84777	115	32	115	42	reference only for the last part related to the Himalaya. Probably some references would be needed also for the text before [Annalisa Cherchi, Italy]	Accepted. In the FGD, references have also been added for the orographic rainfall over the Western Ghats (Shige et al., 2017) and an assessment of the contribution of monsoon depressions to the rainfall in central India (Hunt and Fletcher, 2019).
45219	115	32	116	37	The description of orographic precipitation over the Western Ghats needs to be corrected. Monsoon flow and Somali jet are not the same. Ref: Shige et al. (2017): Role of orography …Western Ghats and Mynamar Coast, J. Clim, 30, 9360-9381. [Krishnan Raghavan, India]	Accepted. The reference to Shige et al. (2017) has been included in the FGD to support the statements relating to Western Ghats rainfall. Reference to the Somali Jet has been removed for simplicity, although we note that the AMS Glossary of Meteorology defines the Somali Jet as, "A low-level south-westerly jet over the Arabian Sea in the summer months, off the coast of Somalia. It is the northern branch of a cross-equatorial flow, giving rise to a major supply of moisture in support of the Asian summer monsoon."
83873	115	39	115	40	Suggesting a reference: Krishnamurthy and Ajayamohan, 2010, Composite structure of monsoon low pressure systems and its relation to Indian rainfall, Journal of Climate, doi:10.1175/2010JCLI2953.1 [Ajaya Mohan Ravindran, United Arab Emirates]	Taken into account. A more recent study (post-AR5) of the contributions of monsoon depressions to rainfall over central India has been assessed and included in the FGD.
41935	115	45	116	21	Using the extensive network of raingauges with long records for climate change studies over India did not start with the availability of gridded data. A vast amount of literature is available on the development of homogeneous spatial averages of rainfall for a range of domains from all-India to smaller sub-divisions, using several innovative approaches. These studies, starting from the 1970s, have made significant contributions to our knowledge on observed climate change in India, both in terms of rainfall and surface temperature, and have been cited in previous ARs right from AR1. It is not appropriate to characterize the quality of observations over India just based on comparative analyses of a few gridded datasets. [Rupa Kumar Kolli, India]	Taken into account. This section and the AR6 in general does not attempt to perform an authoritative review of all observational products. It focuses on results since the AR5 and Special Reports, referring to earlier references only where necessary for supporting material. This subsection on "Observational issues for India" has been introduced more carefully to refer to the acknowledged declining rainfall trends, observed in multiple datasets, in the second half of the 20th century as assessed in Chapter 8. Key findings from the published literature since the AR5 have been included related to the quality of observational products and any impact that may have on model evaluation or climate trend analysis. Nowhere has the subsection attempted to suggest that climate change studies began only with gridded data. It remains the case that gridded observational products are the main source of India climate trend data that is available to the international community. It remains the case that gridded observational products are what is available to the international community.
82735	115	47	116	12	Since they are the numbers most readily available operationally, it may be worth commenting on the value (or otherwise) of all-India and regional rainfall time series published by the India Meteorological Department. [Blair Trewin, Australia]	Accepted. The all-India rainfall pre-computed countrywide average available from IITM/IMD has been added to the figure in the FGD and its declining trend mentioned in the text.
83875	115	48	115	48	Reference missing: Khouider et al., 2020, A novel method for interpolating daily station rainfall data using stochastic lattice model, Journal of Hydrometeorology, doi: 10.1175/JHM-D-10-0143.1 [Ajaya Mohan Ravindran, United Arab Emirates]	Accepted. This study covers a useful review of Indian historical products and introduces a new interpolation scheme. It has been added to the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41937	115	50	115	50	It is incorrect to assume that these 306 stations were the only ones operating since 19th century. These were selected based on some homogeneity criteria and plain stations, and there was a much larger number of stations running into a couple of thousands since the late 19th century. Further, as clearly documented by Sontakke et al. (2008), the number of suitable stations before 1900 were substantially less than 306 as we go back to the early 19th century. and the spatial means were adjusted to account for the reduction in the network density. It is important to include here an assessment of such spatially aggregated rainfall datasets for regional climate change studies. [Rupa Kumar Kolli, India]	Noted. The structure of this paragraph has been altered and we no longer go into detail as to how many stations were used. The point being made here is that long-record observational products exist, which exhibit decadal variability. However, we made no assumption that the 306 stations were the only ones operating since the 19th century. We are glad that the reviewer recognises the importance of assessing spatially aggregated rainfall datasets for regional climate change studies. It is beyond the scope of this chapter to perform a thorough comparison of all observational products for India, however, or any other region. The objective here is to alert the reader to the challenges likely to arise from imperfections in observational products, based either on the availability of the raw observations themselves or of the methods in constructing those datasets.
117011	115		115		"simulation… with CMIP class models is poor" = please check coherency with the assessment of skills of CMIP6 in ch 3-8, coherency with ch 6 and 8 on aerosol forcing. Cross chapter coordination on monsoon is needed to ensure coherency in the approach of the case study and the outcome of the assesment in other chapters. Please also consider this recent publication, Huang et al, J Clim 2020 https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-19-0833.1 (clear role of IPO in the decline and recovery of Indian summer monsoon rainfall). [Valerie Masson-Delmotte, France]	Noted. Simulation of the Indian summer monsoon with CMIP-class GCMs is poor, although the FGD makes appropriate references to assessments made earlier in the report, for consistency.
80339	116	6	116	6	"Those changes" instead of "that changes" [Paola Arias, Colombia]	Not applicable. The paragraph has been restructured and the stated sentence is not present in the FGD. However, the reviewer's reading of the grammar in the SOD sentence is incorrect. The correct construction as was used in the SOD is, "…critical assessment leads to the suggestion… that changes in…" and not: "…critical suggestion leads to the suggestion… those changes".
45155	116	8	116	10	The sentence may be omitted "At its worst, Lin and Huybers (2019) stated that ….mean rainfall; they highlighted the desire for openness of raw meteorological information to allow improved assessments". [Krishnan Raghavan, India]	Rejected. The reviewer does not justify why this sentence should be removed. Indeed it was an assessment of an earlier part of Ch10 that lack of availability (lack of transparency) of the raw data used in the creation of the commonly used gridded observational products prevents adequate assessment of the various and differing methods used in producing them. Indeed Lin and Huybers make this very point, and our Chapter would be lacking if we were not to assess that study.
41939	116	9	116	10	It may not be just "openness" of raw data; in most cases there simply may not be observations with long records of sufficient quality, which necessitates gridding algorithms to make do with variable network I think the emphasis here should be on working with raw station data instead of relying on gridded data, rather than stressing open access to such data. [Rupa Kumar Kolli, India]	Rejected. A contradiction exists in the reviewer's comment. It is the very essence of the problem that the national and international scientific community cannot work with raw station data [to assess the impact of various gridding/kriging methods] if that gauge data is not available to them. In the FGD, the wording has been revised diplomatically to clarify the importance of transparency and availability of data.
41941	116	10	116	12	This is a rather hand-waving statement not really borne out by the reference cited (Knutson and Zeng, who only make some passing references to "small regions" of India in this context; further, they don't state that trends over India for 1901-2010 were inconclusive). Number of competing drivers (?) acting over long periods need not necessarily make trends inconclusive. [Rupa Kumar Kolli, India]	Taken into account. Figures of assessed papers can also be considered, in addition to direct statements. The analysis of Knutson and Zhen over India as a whole is inconclusive. The wording for the FGD has been altered to indicate that a discussion of the attribution of the trends will take place in the subsequent subsection. The wording for the FGD has been altered to indicate that a discussion of the attribution of the trends will take place in the subsequent subsection.
41943	116	16	116	17	Collins et al. (2013a) [Rupa Kumar Kolli, India]	Editorial. Reference to the correct paper has been ensured in the FGD, which appears as Collins et al. (2013) in the FGD version pre-government review. This is paper 10.1038/nclimate2012. Final checking will be performed by copyeditors.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41945	116	20	116	21	This is really not observational uncertainty but data uncertainty, in terms of packaging and homogeneity issues. [Rupa Kumar Kolli, India]	Taken into account. This seems to be a semantic issue. One could use the same argument to describe "model uncertainty" as "data uncertainty". From the perspective of a modeller using observations either to validate their model or to compare the trends, this is an observational uncertainty. "Data" muddies the water and is not specific enough - it is ultimately still an issue of uncertainty in the raw/input observations or of the methods used to compile those observations into a usable product. For the FGD we have taken care to not refer to observational uncertainty as such, but instead to uncertainty in observational products.
41951	116	24	117	2	It is a well-established fact that ENSO is the most dominant natural driver of Indian monsoon variability. It is important to include related aspects relevant to regional climate change. [Rupa Kumar Kolli, India]	Rejected. We will not be assessing the broad scope of ENSO (interannual) change here due to space constraints. The focus of the whole Section (10.6.3) is on the mean climate of the Indian summer monsoon, and this subsection (10.6.3.4) is about relevant drivers of long-term changes in that climate.
41947	116	27	116	27	Add "summer" before "monsoon". India comes under the influence of both summer (southwest) and winter (northeast) monsoons, though the later affects relatively a very small part of the country in the southern peninsula. Simply referring to "Indian monsoon", which is seen in several places in this chapter, can cause confusion. It is better to be explicit, even at the risk of repetition. [Rupa Kumar Kolli, India]	Taken into account. The reader should be able to determine from the section title "10.6.3 Indian summer monsoon" that the monsoon in question is during summer. However, in the FGD version we have made sure that on each first mention of the monsoon, "summer" is included.
45157	116	33	116	33	Krishnan et al. (2013) discussed the effect of increased GHG on slowing down of the Indian summer monsoon circulation. This study may be cited after Cherchi et al. (2011) i.e., "lower troposphere (Cherchi et al. 2011, Krishnan et al. 2013)". [Krishnan Raghavan, India]	Accepted. Citation of Krishnan et al. (2013) has been made in the FGD.
95849	116	39	116	40	Another useful reference is: Vernier et al. (2015), J. Geophys. Res. Atmos., doi:10.1002/2014JD022372. [Christine Bingen, Belgium]	Rejected. The suggested reference is about UTLS aerosol measurements, rather than any measured or modelled impacts on monsoon rainfall trends.
19523	116	43	116	45	with emphasise of this paragraph it is nesseserry to allocate fund for IRAN to prevent dust and snd storm over IRAN and region [Hamideh Dalaei, Iran]	Not applicable. The bullet point in the SOD pertained mainly to a natural driver with no major trends, so has been removed from the FGD.
23247	116	44	116	46	this sentences "their interaction with anthropogenic black carbon aerosols may also drive change in the monsoon" isnot relevant to befor sentences and must be ommited from here. [Hamideh Dalaei, Iran]	Not applicable. The bullet point in the SOD pertained mainly to a natural driver with no major trends, so has been removed from the FGD
88847	116	47	116	51	Mathur & AchutaRao(2019; doi:10.1007/s00382-019-05090-8) show (using a global model) that this could have an impact on the rainfall pattern (with some caveats relating to crop phenology). [Krishna AchutaRao, India]	Accepted. The suggested reference has been assessed and added to the FGD.
82737	116	55	117	2	ENSO is also an influence which should be mentioned in this context. [Blair Trewin, Australia]	Rejected. We will not be assessing the scope of ENSO change here due to space constraints. While of course ENSO is the chief known tele connected driver of monsoon interannual variability, the focus of the whole Section (10.6.3) is on the climate of the Indian summer monsoon, and this subsection (10.6.3.4) on relevant drivers of long-term changes in that climate.
21103	116	55	117	2	Suggest to include some recent literatures in this statement, e.g., Watanabe and Yamazaki (2014), and Zhang et al. (2017) revealed the decadal advance of South Asian summer monsoon onset since 1970s due to the phase transition of PDV. [Watanabe, T., and K. Yamazaki (2014), Decadal-scale variation of South Asian summer monsoon onset and its relationship with the Pacific Decadal Oscillation, J. Clim., 27, 163–5173. /// Zhang W, Zhou T, Zhang L. Wetting and greening Tibetan Plateau in early summer in recent decades[J]. Journal of Geophysical Research: Atmospheres, 2017, 122(11): 5808-5822.] [Wenxia Zhang, China]	Taken into account. While assessment of these suggested reference has been undertaken, they are not the most pertinent to this piece of text, which is a succinct bullet point describing the influence of coupled ocean-atmosphere modes of internal variability on the monsoon. Other more pertinent references have been included in the FGD.
88849	117	1	117	1	Is this really a "forcing" or a modulation from decadal scale variability? [Krishna AchutaRao, India]	Accepted. The wording has been revised from forcing to modulated in the FGD.
83877	117	1	117	2	Reference Missing indicating decadal variability: Sabeerali et al. , 2019, Atlanatic zonal mode: An emerging source of Indian summer monsoon varaibility in a warming world, Geophysical Research Letters, doi: 10.1029/2019GL082379 [Ajaya Mohan Ravindran, United Arab Emirates]	Rejected. In order to limit the scope, the assessment of the Indian monsoon has largely avoided changes to interannual variability and its teleconnections (e.g. with ENSO and the IOD). This subsection pertains to potential drivers of "climate change" in the monsoon on time scales of multiple decades or longer. The suggested reference on the Atlantic zonal mode as a possible driver of interannual monsoon variability is therefore out of scope.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23001	117	5			This section overlaps to an uncomfortable degree with both chapters 3 and 8. I would urge careful coordination and cross-linking to their assessments. [Peter Thorne, Ireland]	Accepted. The FGD text has been much compressed compared to the SOD and begins from the starting-point assessments of the South Asian monsoon in Ch8 (and to a lesser extent the global monsoon in Ch3) and anthropogenic aerosol being the dominant driver.
41953	117	7	117	7	Replace "was not increasing" with "did not show any long-term trend". [Rupa Kumar Kolli, India]	Not applicable. The structure of this paragraph has been altered in the FGD and the relevant sentence is no longer present.
98133	117	7	117	14	Add here: Knutson and Zeng's (2018) univariate detection/attribution and consistency analysis for precipitation trends over 1901-2010, 1951-2010, and 1981-2010 using a 10-member CMIP5 model ensemble and GPCC precipitation data find detectable drying trends (unusual compared to natural variability) for JJA precipitation only for a small region of northwest India and for Sri Lanka. Similar results are seen for annual means. For other locations in the Indian monsoon region, the long-term trends are either positive or not unusual compared to natural variability. [Thomas Knutson, United States of America]	Noted. This reference has already been discussed in a more appropriate subsection of the FGD pertaining to observational issues for India
41955	117	8	117	9	This argument is a bit convoluted, as it attempts to link past changes with future projections. Instead, the focus should be on the past GHG forcing and the associated simulations of the Indian summer monsoon. [Rupa Kumar Kolli, India]	Taken into account. The revised version of this subsection for the FGD uses a more logical structure, with the future climate information in a subsequent subsection. Instead, the new sentences here for the FGD make reference to the theoretical reasons contained in Chapter 8 as to why we might expect the monsoon to increase in a warming world.
41957	117	16	117	16	Declining rainfall trend over which period and which region? Observational evidence has to be presented to establish this. [Rupa Kumar Kolli, India]	Not applicable. A revised discussion of the observational trend including all key details has been given in the "Observational issues for India" subsection of the FGD. For any references to the observational trend in this subsection of the FGD, we have been careful to refer to a countrywide average in the second half of the 20th century.
88851	117	33	117	33	"... was a spread of around 0.5 mm day-1 less rainfall in...". This is not clear. Suggest rephrasing. [Krishna AchutaRao, India]	Accepted. This statement has been rephrased in the FGD for improved clarity.
13613	117	50	117	50	Change Zuo et al., 2013, Zuo et al., 2018 by Zuo et al., 2013, 2018 [Maria Amparo Martinez Arroyo, Mexico]	Not applicable. In order to perform a more succinct synthesis, the containing paragraph has been removed from the FGD.
43301	117	50			Read " (Zuo et al., 2013; Zuo et al., 2018). " rather than " (Zuo et al., 2013, Zuo et al., 2018). " [Cyriaque Rufin Nguimalet, Central African Republic]	Not applicable. In order to perform a more succinct synthesis, the containing paragraph has been removed from the FGD.
23003	117	52	118	7	The term adopted is PDV and not IPO throughout the report. Replace acronym and add a reference to the annex of modes of variability. [Peter Thorne, Ireland]	Accepted. The term "IPO" has been replaced by "PDV" (with a cross reference to the Annex IV on Modes of Variability) in the FGD.
88853	118	12	118	12	Similar to Mathur & AchutaRao (2019) who had ran idealized experiments. [Krishna AchutaRao, India]	Accepted. The suggested reference has been added to the FGD as supporting evidence.
100487	118	17	118	19	An irrigation-induced reduction in rainfall across India is also found in Thiery et al. (2017). But also note that irrigation-induced precipitation increases have been advanced as a hypothesis for explaining slower/absence of glacier melting in parts of high-mountain Asia (de Kok et al., 2018). REFS: Thiery, W., Davin, E. L., Lawrence, D. M., Hirsch, A. L., Hauser, M., & Seneviratne, S. I. (2017). Present-day irrigation mitigates heat extremes. Journal of Geophysical Research: Atmospheres, 122(3), 1403-1422.; de Kok, R. J., Tuinenburg, O. A., Bonekamp, P. N., & Immerzeel, W. W. (2018). Irrigation as a potential driver for anomalous glacier behavior in High Mountain Asia. Geophysical research letters, 45(4), 2047-2054. [Wim Thiery, Belgium]	Taken into account. Assessment of the Thiery et al. (2017) study gas been added to the FGD. However, since the other studies pertain to high mountain Asia, they are out of scope for this assessment.
98465	118	24	118	31	Recitation (Krishnan et al., 2016) of reference which is also found in chapter Atlas (Atlas-57, line 25-30) [Mehwish Ramzan, Pakistan]	Rejected. Each chapter in AR6 has its own reference list that is separate and independent to that in others. Furthermore, the Atlas follows Ch10 rather than precedes it.
13615	118	34	118	34	Missing add parenthesis [Maria Amparo Martinez Arroyo, Mexico]	Editorial. Rather than using a closing bracket, the earlier bracket has been removed in the FGD.
43303	118	34			Read "internal variability, supported by the review of Wang et al. " rather than "internal variability (supported by the review of Wang et al. " [Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. This typo has been corrected in the FGD.
88855	118	35	118	36	Suggest changing to "... relative warming in the equatorial Indian Ocean". [Krishna AchutaRao, India]	Not applicable. The containing sentence has been removed from the FGD in the interest of making an assessment statement using the calibrated uncertainty language.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59401	118	40	120	30	Section 10.6.3.6 summarizes various studies from global simulations and provide detailed information from each studies. However, it is not easy for a reader to piece together the information from all the studies. Several components are touched upon: thermodynamic vs dynamic control, temperature gradient change, aerosol impact, duration change of rainy seasons and spatial variability. Perhaps group the studies by these topics and emphasize the consistent and inconsistent results. Together with an overarching paragraph/sentences to open the section, this would greatly improve the flow. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The subsection has been thoroughly reorganised in the FGD to group together different mechanisms. Further, there is now a logical arrow of time from near-term to long-term projections within the signal, grouping the aerosol and internal variability messages in the near term.
88857	118	40	120	35	This section is missing an assessment of how synoptic systems (mentioned on p.115) will be affected in the future. These are vital for the Central India region and need to be addressed under dynamical changes that can occur. There is some literature out there - for e.g. Sandeep et al. www.pnas.org/cgi/doi/10.1073/pnas.1709031115 Also, there is work suggesting shifts in the monsoon low level jet that has implications for the observed and future differences in regional trends. DOI 10.1007/s00382-014-2261-y [Krishna AchutaRao, India]	Accepted. The Sandeep et al. reference on changes in monsoon synoptic storms has been assessed and added to the FGD. The Sandeep and Ajayamohan (2015) reference on the low-level jet has also been assessed and added to the FGD.
72127	118	43	118	50	I came across a recent study that analyzed the CMIP6 models and found an increase in mean summer monsoon rainfall over Indo-Pakistan region, under three Shared Socioeconomic Scenarios (i.e., SSP1-2.6; SSP2-4.5; SSP4-8.5). I think this is very relevant for this section. It is: Almazoui et al. (2020) available here https://link.springer.com/article/10.1007/s41748-020-00157-7 [Mouhamadou Sylla, Rwanda]	Accepted. The suggested reference has been assessed and included in the FGD.
88859	118	47	118	48	There is no evidence to say the trend was not driven by GHG. The equatorial Indian Ocean warming for example is stated to be a consequence of GHGs. It is just that the trend is overwhelmingly from the ones pushing it downward. [Krishna AchutaRao, India]	Rejected. Although acknowledging that "absence of evidence is not evidence of absence", the assessed finding of Chapter 8 for the observed negative trend in the South Asian monsoon rainfall is that it was caused by aerosol and not due to GHG, which in contrast would have been expected (from substantial theoretical and modelling evidence) to lead to an increase. No substantive new evidence has been found to point to GHG leading to declining monsoon rainfall. A more obvious reference to the findings of Ch8.3.2.4.1 has been given in the FGD.
117013	118		120		The text reads as very descriptive of individual studies, and could be turned to an assessment, with a summary statement using the confidence language (here, should likely be italicized and is it the outcome of an assessment)? For which time horizon and which scenario is the statement valid? [Valerie Masson-Delmotte, France]	Accepted. The FGD version is much compressed and written as an assessment, taking care to build from substantive assessments made elsewhere in the report (Chapter 8). The summary statements use the calibrated confidence and uncertainty language. The detailed descriptions of individual studies have been reduced or removed.
54431	119	3	119	3	Regarding Figure 10.25c - what does white-background time period denote? The reference period? It's not [Gabriel Stachura, Poland]	Rejected. The white background is simply the blank paper where it has not been shaded grey to represent either the past or future periods used in the difference maps or for the PDF/histogram periods.
59399	119	11	119	11	hist-GHG is a blue line in Figure 10.25 while hist-aer a grey line. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The caption has been corrected in the FGD.
88861	119	33	119	33	These are AMIP runs from the corresponding coupled models. [Krishna AchutaRao, India]	Not applicable. The form of words in which this reference is cited in the FGD means such a specification is no longer necessary.
21105	119	37	119	38	Study has also demonstrated that the weak multi-model ensemble mean response of Indian monsoon rainfall in CMIP5 -- associated with the large spread across models -- is related to the divergent Indo-Pacific warming patterns in multi-model projections (Chen and Zhou 2015). It would be useful to note this in the explanations. [Chen, X., and T. Zhou (2015), Distinct effects of global mean warming and regional sea surface warming pattern on projected uncertainty in the South Asian summer monsoon, Geophys. Res. Lett., 42, 9433–9439] [Wenxia Zhang, China]	Accepted. This paper has been assessed and incorporated into the FGD and helps explain the spread in future rainfall responses of the Indian monsoon.
88863	119	44	119	47	This needs a citation. It is not from Singh & AchutaRao (2018) as it currently implies. [Krishna AchutaRao, India]	Accepted. Thanks for pointing this out. It had been erroneously ascribed to Singh & AchutoRao (2018) whereas it should have been associated with Sabeerali & Ajayamohan (2018) from earlier in the paragraph. This has been corrected in the FGD.
83879	119	50	119	51	A poleward shift in monsoon low level jet is found in AR5 models. Full Reference: S. Sandeep and R. S. Ajayamohan, 2015, Poleward shift in Indian summer monsoon low level jet stream under global warming, Climate Dynamics, doi: 10.1007/s00383-014-2261-y [Ajaya Mohan Ravindran, United Arab Emirates]	Accepted. This is an important study that has been assessed and incorporated for the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13617	120	27	120	27	Standardize the format of 21st [Maria Amparo Martinez Arroyo, Mexico]	Editorial. In the FGD all uses of "21st" within Section 10.6.3 have been standardized to the superscript format, although it will be a matter for copyeditors at final production as to the final form this will take.
83881	120	32	120	35	One aspect that is missing in the review is the decline in monsoon synoptic activity in AR5 models which contributes a lot to the central Indian rainfall. Reference: Sandeep et al, 2018, Decline and poleward shift in Indian summer monsoon synoptic activity in a warming climat. Proc. Natl. Acad. Sci., doi: 10.1072/pnas.1709031115 [Ajaya Mohan Ravindran, United Arab Emirates]	Accepted. This is an important study that has been assessed and incorporated for the FGD.
59327	120	40	120	42	This section of text refers to previous outputs, but which sections those are in is unclear. To better direct the reader to the relevant outputs I recommend that the authors provide the relevant report sections. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. In shortening the text for the FGD, the sentence in question has been removed.
59329	120	41	120	41	It is not clear on what this 'value' is, please explain what is meant by using this term and edit the text to clarify. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Although the particular sentence is not given in the same form in the FGD, mentions of added value have been given cross-references to elsewhere in Ch10 and the Atlas on the concept of added value in relation to methods such as downscaling.
23005	120	44	120	46	The NCEP-NCAR reanalysis is over 20 years old and contains numerous biases. As noted by chapter 1 it is better to use newer reanalyses. This study should be commensurately downweighted as it is unclear to what extent NCEP-NCAR with its very coarse resolution and dated data assimilation can simulate key aspects. [Peter Thorne, Ireland]	Taken into account. The reviewer's point is well taken and reference to this study has been suitably downweighed, serving as an example. We note that our assessed future projection is not dependent on this study.
59331	120	50	120	50	It is unclear as to why a bias adjustment would be necessary and how this improves this information. Please briefly outline this to help the readers understand the novelty or importance of such approaches. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The existing sentence already drew attention to the general discussions on bias correction (and therefore why it is needed) - now located in Section 10.3.1.3.2 and Cross-Chapter Box 10.2. In particular, the cross-chapter box discusses the justification and need for bias adjustment. A cross-reference to the earlier subsection in which GCM performance has been assessed for the Indian monsoon has been added in the FGD, justifying the specific use of bias correction here.
59333	120	51	120	51	The term "month-wise" is a little unclear. Does this mean monthly or can be applied to any specific month? Please rephrase to clarify. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. It means separate conversions are used for each of the twelve months of the year. However, since this detail is not required, the sentence has been removed from the FGD.
66327	120		120		This paper could be revised in section 10.6.3.7 Ashfaq et al, Robust Late 21st Century Shift in the Regional Monsoons in RegCM-CORDEX Simulations, Climate Dynamic accepted [Erika Coppola, Italy]	Accepted. This study has been assessed and included in the FGD.
59335	121	5	121	5	Providing examples of the 'added detail' Madhuoodhanan et al. (2018) would allow the readers to become more critical of the information being presented here. Please briefly outline into the text. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The very nature of statistical downscaling is to add detail, using statistical methods to effectively increase the resolution at which information (in this case a climate change signal of precipitation) is presented. This is explained extensively in Section 10.3 and will not be covered again here. In the FGD, due to shortening of this text to form an assessment, the phrase "added detail" has been removed.
59337	121	8	121	8	The authors have stated that there was significant inter-model differences which is an important point to raise, however, it needs re-enforcing with a quantified or illustrated example to provide this evidence and therefore weight to this important point. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. The sentence has been restructured and no longer makes this specific point, instead, in the FGD, the point has been made that the uncertainty of trends is increased.
59339	121	10	121	10	It is unclear which section is meant by 'as explained above'. Please specify by adding the relevant section detail. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. This phrase has been replaced in the FGD by reference to the subsection on Observational issues for India

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59341	121	12	121	13	The statement that "key physical processes are compromised" is weakly supported in this section. I recommend that the authors bring in more supportive evidence (or refer to relevant sections of the report if appropriate) into the previous text to better illustrate this to the readers. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. (Incidentally, the reviewer has misquoted the SOD. Instead, it stated, "...the downscaled product may provide high spatial detail at the expense of neglecting key physical processes that cannot be resolved at the GCM scale".). In the FGD, the text has been more carefully worded to convey to the reader that with coarse GCMs and calibration data, representing truthful physical relationships to downscaled precipitation at 0.05degree scale is going to miss important processes that have not been resolved. An example is given of the circulation around topography.
88865	121	13	121	13	Is the word "distributions" really necessary? It has the potential to cause more confusion. [Krishna AchutaRao, India]	Accepted. The word was unnecessary and caused confusion rather than increasing understanding. It has been removed from the FGD. (Note the surrounding sentence has been revised anyway to make it clearer.)
76853	121	15	121	21	There have been other important studies comparing the added value of dynamical downscaling over the South Asian region, which have been left out. Following are some of them:1) Mishra, S.K., Sahany, S. & Salunke, P. CMIP5 vs. CORDEX over the Indian region: how much do we benefit from dynamical downscaling?. Theor Appl Climatol 133, 1133–1141 (2018). https://doi.org/10.1007/s00704-017-2237-z, 2) Jain et al., Advantage of NEX-GDDP over CMIP5 and CORDEX Data: Indian Summer Monsoon, Atmospheric Research, https://doi.org/10.1016/j.atmosres.2019.05.026 [Sandeep Sahany, Singapore]	Taken into account. Both studies have been examined. The Mishra study has been added to the FGD with a sentence discussing its main message that dynamical downscaling worsens the spatial pattern and magnitude of summer rainfall errors compared to the driving GCMs. However, the Jain study has not been added, since it adds nothing to our assessment.
23007	121	15	121	43	Your job is to syntesise and assess. This looks like a literature review. Please perform instead a synthesis here. [Peter Thorne, Ireland]	Taken into account. As with all aspects of this case study (10.6.3), this subsection has been significantly shortened in the FGD with a synthesis approach.
15253	121	17			"dry warm or hot summers", according to Koeppen: add warm [Piero Lionello, Italy]	Accepted. Text has been revised
15255	121	20			I wouldwrite that "... are among the most important natural ..", infact, can you compare fires to the hazard posed by pests and diseseas? (eg.Xilella for olives in Apulia) [Piero Lionello, Italy]	Not applicable. Text has been deleted, because focus of 10.6.4 has been changed to summer warming.
98463	121	30	121	30	Recitation (Sabin et al. (2013) of reference which is also found in chapter Atlas (Atlas-57, line 43). [Mehwish Ramzan, Pakistan]	Rejected. Each chapter in AR6 has its own reference list that is separate and independent to that in others. Furthermore, the Atlas follows Ch10 rather than precedes it.
59343	121	32	121	32	The description used 'coarsening the grid outside' is unclear, please rephrase to calrify the approach taken here. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. The intended meaning was that the overall number of grid points from the 1-degree global control model was fixed, and in the zoomed experiments the extra grid points within the 35-km region were at the expense of reducing the number of grid points in the rest of the globe. However, the sentence pertaining to this comment has been removed from the FGD.
88867	121	37	121	39	It is important to include results from a global high-res model that indicate the opposite in the previous section.See earlier comment about modelled changes in synoptic systems. [Krishna AchutaRao, India]	Accepted. This has been accomplished by addressing comments 88857 (same reviewer) and 88831 (another reviewer). The work of Sandeep & Ajayamohan has now been cited in the previous subsection (pertaining to global modelling) of the FGD.
88869	121	47	121	47	I am not sure if there are any "changes" tied to orography. I see that improvements documented in this section relate to mean climate over orographic regions. Unless there is specific evidence of trends over orographic regions being better simulated, this word needs to be dropped. [Krishna AchutaRao, India]	Taken into account. I don't think this review comment is fully justified. Of the material covering dynamical downscaling, all of the examples discussed in SOD 10-121 lines 15-33 discussed improvements due to RCM downscaling in orographic rainfall regions (such as the Western Ghats). However, in the FGD the wording of the closing assessment statements of this subsection have been improved to make this clearer, avoiding the use of the "tied to" wording.
59345	121	47	121	47	I'm only reading from section 10.6.3.7 in this report so maybe this has been explained previously in the report (which therefore should be referred to). The statement that "precipitation changes tied to orography" is not well supported from the text provided in this section of the report. If this has not previously been mentioned that I recommend providing clearer statements and examples to support this argument through this section. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. I don't think this review comment is fully justified. Of the material covering dynamical downscaling, all of the examples discussed in SOD 10-121 lines 15-33 discussed improvements due to RCM downscaling in orographic rainfall regions (such as the Western Ghats). However, in the FGD the wording of the closing assessment statements of this subsection have been improved to make this clearer, avoiding the use of the "tied to" wording.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23009	121	50			Section ignores, entirely, the potential wildcard of a Samalas-sized eruption which paleo evidence and modelling robustly points to multi-year failure. Why is this omitted here? It would seem key and there are model studies such as the Bethke et al ensemble in chapter 4 using NorESM which can illuminate. [Peter Thorne, Ireland]	Accepted. The useful reference to Bethke et al. pertaining to future projections containing an ensemble of eruptions has been added to the FGD, along with further discussion starting from the assessment of possible volcanic-related abrupt change in Chapter 8 (Section 8.5.2.3). Note, however, that the monsoon signal arising from the model experiments in Bethke et al. (2017, Figure 4e and S5) can in no way be described as monsoon "failure" as suggested by the reviewer. These signals (units of 10s mm/summer, even when considering the extreme-case difference between the wettest control member versus the driest VOLC member) are smaller than the observed declining trend since 1950. The Cross-Chapter Box4.1 on volcanic eruptions has also been cross-referenced in the FGD.
76865	122	9	122	52	Historical and Future Projections on Arabian Sea and Bay of Bengal should be included, if possible. [Oo Kyaw Lwin, Myanmar]	Rejected. These regions are out of the scope of this Section, which does not serve to be a comprehensive assessment for the whole Asian monsoon region. Furthermore, it is not within the scope of Chapter 10 to serve as a comprehensive, region-by-region assessment of regional climates. The focus of this subsection (and Section 10.6 as a whole) is on worked examples that illustrate the construction of climate information for some regions of the world, using the methodologies described earlier in the Chapter.
4335	122	13	122	13	“tile” → “tilt” (?) [Isla Simpson, United States of America]	Accepted. Typo "tile" was changed to "tilt".
117017	122	21	122	26	Please check exactly what is in chapter 3, SR15 on monsoon change for different levels of warming (the other sentences on Paris Agreement are not needed here). [Valerie Masson-Delmotte, France]	Taken into account. The SOD text already referenced SR15 Ch3 (Hoegh-Guldberg et al., 2018), although treatment of the Asian monsoon in SR15 is rather minimal and did directly not assess any literature on the effects of 1.5C or 2C warming on the Asian/Indian monsoon (its Ch3p33: "there appears to be no specific assessment of changes in monsoon precipitation at 1.5°C versus 2°C global warming in the present literature"). We had already cited one such study (Chevuturi et al., 2018) in the SOD and have added more recent works in the FGD in which monsoon changes have been assessed in units of %/K. The SR15 also expressed low confidence in monsoon projections (in general, not regionally specific), relying on the assumption of, "low confidence in observed trends in monsoons" (its Ch3p33) which is inconsistent with the assessed negative rainfall trend for the South Asian/Indian monsoon found in AR6 Ch8 (and, indeed, the AR5: Ch14, p1229). Other sentences on the Paris agreement have been removed as suggested. Note that in the FGD, these topics have been rearranged, with those aspects pertaining to GWLs now residing in 10.6.3.6 on global model projections.
13619	122	24	122	24	Standardize the degree symbol size [Maria Amparo Martinez Arroyo, Mexico]	Editorial. In the FGD all uses of the degree symbol have been standardized, although it will be a matter for copyeditors at final production to check the chapter/report for consistency.
13621	122	30	122	30	Standardize the degree symbol size [Maria Amparo Martinez Arroyo, Mexico]	Editorial. In the FGD all uses of the degree symbol have been standardized, although it will be a matter for copyeditors at final production to check the chapter/report for consistency.
59347	122	31	122	31	Recommend checking that the 'half-degree' is consistent with the reports format for presenting data (i.e. 0.5oC). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This discussion has been moved to subsection 10.6.3.6 in the FGD and this form of words is no longer included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98135	122	45	122	52	Based on Knutson and Zeng's (2018) precipitation trends univariate detection/attribution analysis, I would not conclude that there is very high confidence in an contribution of aerosols to the negative rainfall trend in the Indian monsoon over the 20th century, because the study actually didn't find any large-scale detectable negative century-scale trend in precipitation in the region except for a small region in northwest India and over Sri Lanka. So I think the text in this section is making too strong a claim and should be considerably tempered. [Thomas Knutson, United States of America]	Rejected. Knutson and Zeng found such a small trend because they measured it over such a long period, for which the anthropogenic aerosol driver was not active for the entirety. It is the assessed view of Ch8 - on which we build - that aerosol is the primary anthropogenic driver of declining monsoon rainfall trends in the second half of the 20th century.
45159	122	47	122	47	To be corrected as "in the Indian monsoon over the second half of the 20t century". [Krishnan Raghavan, India]	Accepted. Concluding statements made in the FGD pertaining to the observed declining trend now include this important caveat.
45161	122	47	122	48	The sentence "There is limited evidence of the spatial distribution of historical and projected changes, made worse by the substantial observational uncertainty" needs to be suitably modified. The word "limited evidence" is not correct for the observed datases. Although there are differences in the magnitude of trends across datasets, the decreasing trend of monsoon rainfall ove India during the last few decades is evident in multiple datasets (see Ramesh and Goswami, 2014). Secondly "made worse by the substantial observational uncertainty" is also not correct. Ghosh et al (2016) noted that decreasing trend of monsoon precipitation was observed mostly over the major water surplus river basins of India. Ref: (1) Ramesh, K.V. and P. Goswami (2014): Assessing reliability of regional climate projections: the case of Indian monsoon. Scientific Reports, 4 : 4071 | DOI: 10.1038/srep04071. (2) Ghosh S, Vittal H, Sharma T, et al. (2016): Indian Summer Monsoon Rainfall: Implications of Contrasting Trends in the Spatial Variability of Means and Extremes. PLoS One. 2016;11(7):e0158670. Published 2016 Jul 27. doi:10.1371/journal.pone.0158670oon. [Krishnan Raghavan, India]	Taken into account. The closing assessment statements in the FGD have been worded more carefully to draw out the robust evidence and high agreement for the India-wide negative rainfall trend since the 1950s, whereas there is only medium agreement of trends at the regional scale.
41959	122	48	122	48	This depends on what aspects are being considered, and what metrics are being used. If one goes by the large-scale monsoon indices such as seasonal rainfall totals for India as a whole, homogeneous regions and meteorological subdivisions, there is very little disagreement between the various data sets. Much of the uncertainty aspects highlighted in this section arise out of the way the data were packaged in different studies, particularly in gridding of daily data, and not due to problems with the original data. India has some of the best observed climate data records in the world, and one has to be careful in putting a question mark over them. [Rupa Kumar Kolli, India]	Taken into account. The closing assessment statements in the FGD have been worded more carefully to draw out the robust evidence and high agreement for the India-wide negative rainfall trend since the 1950s, whereas there is only medium agreement of trends at the regional scale. The reviewer highlights the uncertainty among observational products, and we have modified our statement in the FGD to illustrate how this hinders downscaling, model evaluation and some trend analysis, particularly for extremes, supported by our revised (earlier) observational subsection and works such as that of Lin and Huybers. It is for the scientific community to judge the quality of observational datasets; however, these datasets can only be fairly assessed if they are openly available (including the raw gauge data).
59349	122	51	122	51	No contradictory evidence is found for downscaling methods appears to contradict the information and statements made on page 121, line 45. If the sections are distinctly different then information needs to be provided in the text to better highlight this. If not then the authors need to review this information and adjust the statement to better reflect the evidence provided in this section of the report. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The reviewer appears to have misunderstood the sentence. The statement in this closing subsection means that downscaling studies do not contradict GCM results; the SOD sentence pointed to at the end of the subsection on results from downscaling/regional modelling stated that, "There are mixed messages as to whether downscaling methods add value to climate projections". Where it does add value it is at high resolution, especially near orography. There is no contradiction here.
117015	122		122		I suggest to consider insights from ch 8 on abrupt change rather than Lenton et al 2008. The paleoclimate information does not arrive in a logical flow here : being more explicit on the thermodynamic - dynamic part and what can be leartn from the mid Holocene (and limits to analogies) is to be better explained. [Valerie Masson-Delmotte, France]	Accepted. In the FGD, the tipping-point literature has been removed, and the authoritative assessments of Chapter 8 have been cross-referenced. The paleoclimate information has also been introduced more carefully, explaining the thermodynamic vs. dynamic contributions and their differences in the past and future in more detail.
117019	122		122		please refer to decline and recovery of rainfall and also check the robustness of the statement with this paper : https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-19-0833.1 (weak forced trend). [Valerie Masson-Delmotte, France]	Taken into account. Note that the study the reviewer refers to was already cited in the SOD, and it is unclear why the reviewer comment has been made with reference to subsection 10.6.3.8 on "potential for abrupt change", which usually pertains to "tipping points" or AMOC collapse etc. Nonetheless, in the FGD, reference to the Huang et al. (2020, J. Clim) study has been made more carefully at the end of subsection 10.6.3.5 on "model simulation and attribution of drying over the historical period". This has been done in conjunction with the observed study of Jin and Wang (2017), discussing the decline and recovery of Indian monsoon rainfall since 1950.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59351	123	13	123	13	As many southern and eastern land systems… should ideally be quantified to clarify the scale. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The "Mediterranean" has been included in the text as further regional specification
99249	123	13	123	14	The paragraph is verging on an assessment of impact for agricultural systems in Europe. As we show in WGII chapter 13, yield are impacted by a wide range of issues and hence the link here from irrigation to yield without further differentiation does not reflect the complexity. I would appreciate if the reference to yields would be removed to avoid information being different between WGI and WGII on a topic which is assessed by WGII [Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The reference to yields has been removed. No assessment is made.
99251	123	18	123	22	Similar comments as to the above paragraph, infrastructure efforts for adaptation are assessed by WGII and not the remit of this assessment, wildfires are a complex question which is strongly influenced by management action and as such I would like the section to closely collaborate with WGII to ensure that the messaging is consistent and a reference to chapter 13 and the cross chapter paper made [Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Statements that potentially might overlap with WGII have been removed.
34721	123	27	123	40	There is no agreement on how the North Atlantic Oscillation (NAO) contributes in rainfall over the East Mediterranean region. For instance, (Ben-Gai et al., 2001) and (Ziv et al., 2006) found no correlation between the NAO and rainfall over Israel. (Donat et al., 2014) found that the NAO is less influencing precipitation extremes in the EM region. Ben-Gai, T., Bitan, A., Manes, A., Alpert, P., Kushnir, Y., 2001. Temperature and surface pressure anomalies in Israel and the North Atlantic Oscillation. Theoretical and Applied Climatology 69, 171-177. https://doi.org/10.1007/s007040170023. Donat, M.G., Peterson, T.C., Brunet, M., King, A.D., Almazroui, M., Kolli, R.K., Boucherf, D., Al-Mulla, A.Y., Nour, A.Y., Aly, A.A., Nada, T.A.A., Semawi, M.M., Al Dashti, H.A., Salhab, T.G., El Fadli, K.I., Muftah, M.K., Dah Eida, S., Badi, W., Driouech, F., El Rhaz, K., Abubaker, M.J.Y., Ghulam, A.S., Erayah, A.S., Mansour, M.B., Alabdouli, W.O., Al Dhanhani, J.S., Al Shekaili, M.N., 2014. Changes in extreme temperature and precipitation in the Arab region: long-term trends and variability related to ENSO and NAO. International Journal of Climatology 34, 581-592. https://doi.org/10.1002/joc.3707. Ziv, B., Dayan, U., Kushnir, Y., Roth, C., Enzel, Y., 2006. Regional and global atmospheric patterns governing rainfall in the southern Levant. International Journal of Climatology 26, 55-73. https://doi.org/10.1002/joc.1238. [Salah Ajjur, Qatar]	Accepted. A sentence has been included, with the suggested references, that the impact of NAO is not clear for some regions of the Mediterranean
105917	123	27		40	extreme precipitation during the Fall season is also worse to be mentioned as an key element of the Mediterranean climate as well as regional winds, sometimes violent due to channeling effect [SAMUEL SOMOT, France]	Accepted. This has been now also discussed as a key element including references.
111591	123	31	123	32	Should be not twice summer [Volodymyr Osadchy, Ukraine]	Taken into account. There exists two dominant patterns over the Atlantic: the NAO, which is predominant during the winter and the summer NAO. Sentence has been rewritten to make this clear.
5617	123	31	123	32	The interannual variability of the climate is also influenced by NAO. Cf. ref Zamrane et al., 2016 Atmosphere; Turki et al., 2016a Arabian Journal of Geological Sciences, Turki et al, 2016b Arabian Journal of Geological Sciences; Jemai et al., 2018, Arabian Journal of Geosciences... [Benoit Laignel, France]	taken into account. Most relevant references have been included.
125749	123	33	123	33	Misspelling: upper level trough. [Trigg Talley, United States of America]	Editorial – copyedit to be completed prior to publication
30677	123	35	123	37	Additional reference of relevance here is Kouroutzoglou et al., 2015: On the dynamics of a case study of explosive cyclogenesis in the Mediterranean. Meteor. Atmos. Phys., 127, 49-73, doi: 10.1007/s00703-014-0357-x. [Ian Simmonds, Australia]	Accepted. Reference has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15257	123	35			It should be mentioned that the Mediterranean is characterized by local cyclogeneis and a separate branch of the mid laitude storm track (e.g. Lionello P, Trigo IF, Gil V, Liberato ML, Nissen KM, Pinto JG, Raible CC, Reale M, Tanzarella A, Trigo RM, Ulbrich S, Ulbrich U (2016) Objective climatology of cyclones in the Mediterranean region: a consensus view among methods with different system identification and tracking criteria. Tellus A 68:29391. doi:10.3402/tellusa.v68.29391 [Piero Lionello, Italy]	Taken into account .Text has been revised and suggested reference has been added
15259	123	36			The Medicanes are NOT the most intense and destructive storms. It depends on what is the metric used for intensity. The energy content of mid-latitude cyclones in the Mediterranean is much larger than that of medicanes, which are comparatively small systems. Further, records of damages and victims produced by Medicanes are much smaller than those produced by cyclones (floods,damages to the coastal structures,landslides) and possibly also of those produced by convective summer thunderstorms (e.g. heavy damages to crops). The sentence distract the readers from those which are the most important and destructive storms in the Mediterranean [Piero Lionello, Italy]	Taken into account. Sentence has been revised, but Medicanes are still mentioned as they pose a different thread compared to mis-latitude storms
15261	123	37			"The Mediterranean has a semi-arid climate" is not appropriate.Only a fraction of the areas around the Mediterranean (mainly along its southern coast) are semi-arid (see figure 1 of https://doi.org/10.1016/B978-0-12-416042-2.00012-4) [Piero Lionello, Italy]	Taken into account. Text with semi-arid climate has been removed. It is now stated that the Mediterranean is partly semi-arid.
15263	123	47			the role of political problems and civil strife is plausible, but I think a reference should be found to support this statement. Further in the late 19th and first half of the 20th century, lack of scientific development (and institutional governance) has been a major reason for lack of observations [Piero Lionello, Italy]	Not applicable. Text has been removed in FGD
105919	123	52			Concerning uncertiainties in Mediterranean observation, you can also see Flaounas et al. 2012, doi:10.1088/1748-9326/7/2/024017 [SAMUEL SOMOT, France]	Accepted. Reference has been added
59325	123		128	51	These sections are well written and I have only comment about the second point of the limits: “There is a substantial shortage of observed variables needed for both validation……, such as soil moisture……”. Page 10-128 For soil moisture, many efforts have been already made by the remote sensing community and we have currently nearly 4 decades of soil moisture observations at the global scale. https://www.esa-soilmoisture-cci.org/node/93. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. This is true, but satellite-based soil moisture is somehow limited by the shallow depth the satellite instruments can measure (the top few centimetres). For many climate processes, deeper soil moisture is very relevant and can only be estimated by in-situ observations, which are scarce.
15265	124	1			Local data are used for estimating bias... Is this an independent dataset? Are those data not used to produce MERRA, and CRU? [Piero Lionello, Italy]	Noted. The phrase "local data are used for estimating bias" does not appear in the text
59353	124	3	124	4	The text refers to sparse monitoring networks but the region which this applies to is unclear. This needs clarification. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Text has been changed with reference to Fig. 20bc, where the sparse regions can be identified.
59355	124	3	124	4	Sparse monitoring networks are referenced to Section 10.2. However, referring to this section simply leads to the overall section on monitoring. Ideally the specific section number where this information is presented should be supplied and if appropriate, this text could also refer Figure 10.26b which visualizes the Med. stations. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Reference to section 10.2.2.3 and Fig.10.20b , which replaces Fig. 10.26b in the SOD, has been added.
7971	124	9	124	14	Is land use change a driver of Mediterannean climate change? Millán, M. M. (2014). Extreme hydrometeorological events and climate change predictions in Europe. Journal of Hydrology. 518: 206-224. DOI: 10.1016/j.jhydrol.2013.12. 041. [Bart van den Hurk, Netherlands]	Taken into account. Anthropogenic land-use change is included as a potential driver
125751	124	9	124	14	The NAO should also be mentioned in this section. [Trigg Talley, United States of America]	Accepted. Text has been changed and NAO is now also discussed.
125753	124	11	124	11	Misspelling: to a lesser extent [Trigg Talley, United States of America]	Not applicable. Text has been deleted
79337	124	17	124	17	You may also consider a recent publication on the links of the summer circulation over East Mediterranean and Indian summer monsson in CMIP5 by Logothetis et al. (2019 , https://doi.org/10.1002/joc.6259) [Prodromos Zanis, Greece]	Accepted. Reference included.
105923	124	17	125	22	I would split this section in various parts : characterization of the past trends using observations, performance of the climate models for the aera in general and for simulating past trends, attribution of the trends. Currently, I think that the section is mixing all issues being not very easy to read and not very logical. To be reorganised at least [SAMUEL SOMOT, France]	Taken into account. The section has been reorganised and rewritten.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
105931	124	17	125	22	This section is not very well written and not completely targeting its objective « Model simulation and attribution over the historical period » of the Mediterranean Summer warming past trend. I feel that this section is very patchy, speaking about many things (SST, extreme events, long-term biases) but without a clear structure and without clearly addressing the evaluation of model trends and the attribution of this trend. For me, to be revised [SAMUEL SOMOT, France]	Taken into account. 10.6.4 as a whole including 10.6.4.5 has been revised and restructured, with a better structure and addressing the evaluation of model trends and the attribution of this trend.
105937	124	17	125	22	What about a cloud feedback effect ? Decreasing cloud cover → enhanced warming. Not discussed at all. [SAMUEL SOMOT, France]	Taken into account. Text has been revised and cloud feedbacks are now discussed
23011	124	17			This reads more like a literature review than synthesis and assessment. Further efforts are required to synthesise, compareand assess these studies here. [Peter Thorne, Ireland]	Taken into account. Text has been modified to provide a better synthesis and assessment of the literature.
105933	124	19		26	the characterization of the past trends require more quantitative estimates and consistency between values cited in the text and fig 10.26. We may also want to know what is the value (or range of values) for the ratio between the Mediterranean warming and the global warning. Also be clear if you speak about land values, land+sea values, all-year or summer as expected from the title of the section. Currently the phrasing remains unclear. [SAMUEL SOMOT, France]	This has been elaborated in the new Fig. 10.21. where the difference between global and Mediterranean warming is plotted.
12307	124	21	124	21	I suggest to replace "lapse-rate feedback" with "lapse-rate changes" since the term lapse-rate feedback commonly refers to a radiative feedback. Also one could refer the reader to Figure 10.26a here where the lapse-rate difference between land and ocean is visualized. [Roman Brogli, Switzerland]	Taken into account. Every where in text lapse-rate feedback has been changed into lapse-rate changes and a reference is made to Fig. 10.20a which replaces Fig. 10.26a of the SOD.
15267	124	21			the statement "annual mean temperatures are in 2018" is not precise (I would say it is actally not corrrect).. On the basis of Cramer 2018 et al, the correct statement is that the last two decades (or , if you prefer, the early decades of the 21st century) are approximately 1.4 C above the late 19th century levels [Piero Lionello, Italy]	Taken into account. Text has been changed. Specific reference to 2018 has been removed and replaced by early decades of 21th century.
105921	124	21			could be worse to better explain the lapse-rate feedback here, not evident for all readers [SAMUEL SOMOT, France]	Taken into account. A reference has been made to Fig. 10.20a, which replaces Fig. 26a of the SOD where the lapse-rate is visualized.
105935	124	24			Is a warming over 20 years reallyrelevant ? Is that period not too short to estimate a trend with some robustness ? [SAMUEL SOMOT, France]	Noted. Although 20 years is a short period for a trend it is relevant to mention the rapid recent warming in the Mediterranean. Phrase has been changed to recent decades.
15269	124	33			Does medium confidence refer to the importance of aerosols (general) or specifically on its effecton clouds optical properties? aerosols might have warmed also by direct atmospheric absorption [Piero Lionello, Italy]	Taken into account. Discussion about aerosols has been extended Part of text of section 10.4.1.2.6 of SOD has been merged into 10.6.4.5.
105925	124	34			attributing the « reduction of aerosol concentrations » to « air pollution control legislation" is a bit simple. European desindustrialisation is likely part of the explanation. This simplist explanation came different times in the section often associated to Turnock et al. 2016 (page 85, page 124 2 times) [SAMUEL SOMOT, France]	Noted.
105927	124	37			Nabat et al. 2014 also show the impact of the aerosol decrease on the summer air temperature warming, not only on SST. With respect to the previous sentence, the aerosol effect is here associated to direct aerosol effect and not to indirect. [SAMUEL SOMOT, France]	Taken into account. In the revised text this has been explained more in detail
15271	124	40		47	the paragraph is misplaced. It interrupts the assessment of the causes of warming by discussing its detection in climate models. I would move it after line 4 page 126 [Piero Lionello, Italy]	Accepted. Paragraph has been moved to page 109 in FGD, where it is now better placed.
105929	124	44		47	using the CORDEX evaluation runs here would be helpful in order to disentangle the rôle of the internal variability for RCM as the internal variability is set to the observed one in evaluation runs [SAMUEL SOMOT, France]	Rejected. For a correct role of internal variability much longer periods are needed. The role of internal variability is discussed in 10.3 and 10.4 using SMILES.
59357	124	45	124	45	less than observed is unclear please rephrase to clarify the meaning of this phrase. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Text has been revised and made more specific.
59359	124	45	124	45	What do the authors mean by "large spread"? Please rephrase and/or expand to clarify this point [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Sentence has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98137	124	47	124	47	Add: "Concerning precipitation trends, an analysis of observed vs. modeled precipitation trends over 1901-2010 (Knutson and Zeng 2018) shows for the Mediterranean region detectable decreases, with some inferred anthropogenic forcing contribution to the decreases. Hoerling et al. (2012) also found a detectable human contribution to Mediterranean region decreasing precipitation trends during boreal winter." This is needed to support the statement in the p. 128, line 18-20 summary for the section. [Thomas Knutson, United States of America]	Not applicable. Section 10.6.4. has now a focus on summer warming. Changes in the hydrological cycle are no longer discussed.
7973	124	49	124	50	Would it possible to add a panel describing the diffence in mean and extreme temperature trend in figure 10.26? [Bart van den Hurk, Netherlands]	Rejected. This section focuses on mean summer warming. Extremes are discussed in CH11, and Fig. 10.26 of SOD (Fig. 10.20 in FGD) is already very crowded
15273	125	4			I would add "although its effect on the atmospheric moisture budget is substantially counteracted by changes of the mean and transient eddy components of the atmospheric circulation (D'agostino and Lionello, 2020)". The ref is D'Agostino, R and Lionello, P. (2020) "The atmospheric moisture budget in the Mediterranean: mechanisms for seasonal changes in the Last Glacial Maximum and future warming scenario" submitted to Quaternary Science Reviews (present state: minor revision) [Piero Lionello, Italy]	Not applicable. Text has bee removed
105939	125	6		9	Concerning performance of RCMs over the region, I'm surprised that none of the multi-model Med-CORDEX articles is referenced despite many articles dedicated to model evaluation over the area incuding also the added-value of aerosol representation or higher-resolution. List of published papers can be found here https://www.medcordex.eu/publications.php . Also see the editorial of the Med-CORDEX special issue in Climate Dynamics (Somot et al. 2018, already cited in the chapter) [SAMUEL SOMOT, France]	Taken into account. More articles from Med-CORDEX have been cited.
105941	125	6		9	Concerning RCM performance over the region, Euro-CORDEX based literature is also worse to be mentioned and in particular the Vautard et al. (in rev.) in which the MED IPCC box is used for model evaluation in historical mode. See also Kotlarski et al. 2014 for evaluation in evaluation mode. [SAMUEL SOMOT, France]	Taken into account. Vautard et al. has been included
105943	125	11		18	For coupled model evaluation over the ara, see also Sevault et al. 2014 http://dx.doi.org/10.3402/tellusa.v66.23967 and Nabat et al. 2015b, doi:10.1007/s00382-014-2205-6 but also many more articles from the Med-CORDEX community. [SAMUEL SOMOT, France]	Not applicable. Paragraph has been removed.
105945	125	20		22	Macias et al. 2018 is not targeting to improve SST in RCMs but in Ocean regional models when they are driven by Atmosphere RCM. For RCM performance for SST representation, I advise to assess the coupled model literature in Med-CORDEX as it may be relevant for future projections with such models. You can find that in Sevault et al. 2014 http://dx.doi.org/10.3402/tellusa.v66.23967, Nabat et al. 2015 doi:10.1007/s00382-014-2205-6, Nabat et al. 2014 (already cited), Darmaraki et al. 2019a doi: 10.1007/s00382-019-04661-z , Darmaraki et al. 2019b doi:10.1029/2019GL082933, Soto-Navarro et al. 2020 https://doi.org/10.1007/s00382-019-05105-4 , the lastest studies being multi-model studies. [SAMUEL SOMOT, France]	Taken into account. Most of the suggested literature has been added and discussed.
108107	125	21	125	21	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]	Not applicable. Sentence has been removed
100843	125	25	126	33	The recent results from Zappa et al. 2020 are relevant for this subsection and might be included somewhere. They found that in the Mediterranean the projected drying is substantially accelerated relative to global warming (which is already reported in the sub-section). However in the paper they show that the time evolution of climate responses critically depends on distinct shifts in the regional atmospheric circulation associated with the existence of distinct fast and slow SST warming patterns. As a result, Mediterranean drying is in quasi-equilibrium with GHG concentrations, meaning that the drying will not continue after GHG concentrations are stabilized. This last result seems relevant also for WGII and WGIII. (Zappa G, Ceppi P & Shepherd TG, PNAS https://doi.org/10.1073/pnas.1911015117) [Corti Susanna, Italy]	Not applicable. Focus of 10.6.4 is now on summer warming. Discussion on drying has ben removed. Zappa et al. 2020 is mentioned 10.4.3.1
112733	125	25	127	24	There is really too many citations using mono-model approach in this section whereas multi-model studies do existe and are not always cited. To be corrected as multi-model studies are more likely to give robust results. [SAMUEL SOMOT, France]	Taken into account. Multi-model studies are indeed preferred, but for specific regions only single model studies are available. We have still have used them in those cases, but have mentioned that these are model only studies and therefore have less confidence.
112753	125	25	127	24	Overall, I don't like much sections 10.6.4.6 and 10.6.4.7 (see strong comment above concerning organisation, literature assessment and literature choice). I have no much time to go in much details but these sections require to be largely revisited, perhaps involving some other authors of the chapter. [SAMUEL SOMOT, France]	Taken into account. Structure will remain. Is similar for all cases of section 10.6 and discussed with all LA's. Focus of 10.6.4 is now more on summer mean temperatures. The text of sections 10.6.4.6 and 10.6.4.7 has been substantial revised and the logical order has been improved.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65187	125	27	126	33	Additionally, the amplitude of the seaonsal cycle in the mediterranean is robustly projected to increase according to CMIP5 models and large ensembles (with summers warming faster than winters) e.g. Dwyer et al 2012 [doi: 10.1175/JCLI-D-11-00741.1] Yetella and England 2018 [doi: 10.1029/2018JD029066] [Mark England, United States of America]	Taken into account. Reference of Yettella and England has been added and discussed. Dwyer et al. is too old to be included.
15275	125	35			page 125 line 35 I would add "Temperature extremes extremes will be affected as well,with a dramatic increase of wam days and reduction of cold nights (Lionello and Scarascia, 2020)". The ref is 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 and suggested text have been added.
112721	125	39		40	This results is not supported by Fig 10.26h in which the Balkans seem to be the hotspot for CMIP5 and Spain and Turkey for CMIP6. [SAMUEL SOMOT, France]	Taken into account. Text has been revised. In the larger CMIP6 ensemble used for the FGD, Balkans, Spain and Turkey are all hotspots.
15277	125	39		40	Lelieveld et al have focused on the eastern and southern areas of the Mediterranean region. This statement is not really correct at regional scale where continental areas north of the basin warm more than the south-eastern part (indeed, see your figure 10.226) Lionello and Scarascia 2018 show that "Warming will be particularly large in summer (approximately 50% larger than global warming) and for the land areas located NORTH of the basin (locally up to 100% larger than global warming). [Piero Lionello, Italy]	Not applicable. Sentence has been removed.
112723	125	39		46	Concerning comparison of CMIP5 and CMIP6 ensemble for the Mediterranean summer warming, please also consider the Coppola et al. (in revision) article : Coppola E., et al. (2020) Assessment of the European climate projections as simulated by the large EURO-CORDEX regional and global climate model ensemble. Journal of Geophysical Research – Atmospheres (submitted). In particular this article uses the IPCC MED box for its analysis and JJA as one of the season. [SAMUEL SOMOT, France]	Accepted. Paper of Coppola et al. has been cited and discussed.
79339	125	45	125	46	This is an important point to be further clarified. Is this also related to the smaller number of CMIP6 member used. Maybe it would be useful to compare ensembles of similar models from CMIP5 and CMIP6. [Prodromos Zanis, Greece]	Not applicable. Sentence has been removed. For the FGD many more CMIP6 members were available.
54433	125	51	125	51	Regarding Figure10.26b - the quality of a chart is too low [Gabriel Stachura, Poland]	Taken into account. Fig. 10.26 has been split into Fig. 10.20 for historical and 10.21 for projections. Plots have been revisited for better reading. Quality has been improved.
15279	126	17			the sentece of lines 2-4 at page 124 could be moved to this paragraph [Piero Lionello, Italy]	Rejected. Text does fit better in 10.6.4.3
12309	126	19	126	20	I suggest to replace "the lapse-rate feedback" with "lapse-rate changes" since the lapse-rate feedback refers to a radiative feedback connected with lapse-ratc changes. [Roman Brogli, Switzerland]	Accepted. Text has been revised
4337	126	19	126	20	This statement that the Mediterranean precip decline in summer is predominantly caused by land-sea contrast seems at odds with the statement at l2-4 on pg 125 that relates the precipitation decline to high pressure over the Atlantic and Europe. They could both be playing a role, but the way they are each mentioned here might be confusing to readers. [Isla Simpson, United States of America]	Not applicable. Text has been deleted, because focus of 10.6.4 has been changed to summer warming and not drying.
59367	126	21	126	21	Whan et al., 2015 needs to be added to the reference list. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference has been added.
79341	126	23	126	24	The sentence maybe needs rephrasing. [Prodromos Zanis, Greece]	Not applicable. Text has been deleted, because focus of 10.6.4 has been changed to summer warming and not drying.
5619	126	28	126	29	Not only. The interannual fluctuations of NAO influences also the interanual variability of precipitations and hydrology (streamflow and groundwater) [Benoit Laignel, France]	Noted. Focus of 10.6.4 has been changed to summer warming.
15281	126	29		30	page 126 line 29-30. The link to the Hadley circulation needs a reference. In fact, I am not sure about the link in summer. D'agostino et al (2020) show that the eastern Mediterranean is among the very few regions where different indicators of subtropical margin position show a consistent northward shift in climate model simulations, but they refer to winter (D'Agostino R et al. (2020) "Northward Shift of Subtropics in winter: Time of Emergence of Zonal versus Regional Signals", submitted to Geophysical Research Letters [Piero Lionello, Italy]	Accepted. Link with Hadley circulation has been removed.
125755	126	31	126	32	How can correctly simulating the circulation features offset anthropogenic warming? The text is supposed to state that circulation features can either add to or mitigate anthropogenic warming. [Trigg Talley, United States of America]	Taken into account. Statement has been made more specific, making clear that it can mitigate the anthropogenic warming in the Western and Central Mediterranean.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79343	126	36	126	36	You may also consider a recent publication on Twenty-First-Century Changes in the Eastern Mediterranean Etesians and Associated Midlatitude Atmospheric Circulation based on EURO-CORDEX regional climate simulations at the 12-km grid resolution by Dafka et al. (2019, https://doi.org/10.1029/2019JD031203) [Prodromos Zanis, Greece]	Rejected. Focus of 10.6.4 is on Mediterranean summer warming. Suggested reference is not relevant for this topic.
15283	126	41			page 126 lines 41 I would add after "Mediterranean" that "The importance of regional downscaling for investigating the subregional details caused by the complex morphology of the Mediterranean region is a well know issue in the literature (Planton et al. 2012), which has been addressed in many studies after IPCC-AR5." the ref is Planton, S et al.(2012) The Climate of the Mediterranean Region in Future Climate Projections doi 10.1016/B978-0-12-416042-2.00008-2 in book: The Climate of the Mediterranean Region, Publisher: Elsevier, Editors: Lionello, P, pp.449 - 502 [Piero Lionello, Italy]	Accepted. Suggested text and reference have been added.
112727	126	42			Please use the offial reference for the ENSEMBLES project (Van der Linden and Mitchell 2009, Christensen et al. 2010 or Déqué et al. 2012). You may aso want to refer here to the PRUDENCE project (Christensen et al. 2002) who first deliver coordinated projections over the Mediterranean zone and the Mediterranean-dedicated CIRCE project (Gualdi et al. 2013 doi: 10.1175/BAMS-D-11-00136.1, Dubois et al. 2012 DOI 10.1007/s00382-011-1261-4.) in which the first coordination of coupled RCM was achieved. You may find interesting history of those projects in Christensen et al. 2019 https://doi.org/10.1007/s00382-019-04831-z [SAMUEL SOMOT, France]	Accepted. Suggested projects and references have been added
112725	126	43			EURO-CORDEX and Med-CORDEX started at the same time. We can not classify EURO-CORDEX as earler activity. Med-CORDEX started as CORDEX in 2009. [SAMUEL SOMOT, France]	Taken into account. Text has been revised no difference in start time is implied in the revised text
112739	126	43			you could add reference to CORDEX Africa and MENA as they also cover the zone. See the interesting multi-domain analysis Zittis, G., Hadjinicolaou, P., Klangidou, M., Proestos, Y., & Lelieveld, J. (2019). A multi-model, multi-scenario, and multi-domain analysis of regional climate projections for the Mediterranean. Regional Environmental Change, 19(8), 2621-2635. [SAMUEL SOMOT, France]	Accepted. Reference has been added.
112731	126	45			The coming Coppola et al. (in revision) is unmissable in this paragraph for the use of the largest Euro-CORDEX ensemble ever : Coppola et al. (in revision) article : Coppola E., et al. (2020) Assessment of the European climate projections as simulated by the large EURO-CORDEX regional and global climate model ensemble. Journal of Geophysical Research – Atmospheres (submitted). In particular this article uses the IPCC MED box for its analysis and JJA as one of the season. [SAMUEL SOMOT, France]	Accepted. Coppola et al. has been included and discussed.
112741	126	45			This paragraph is organised somehow per project. It does not seem to be a good idea. Better to use all available results to assess future climate change information over a specific zone or variable or season [SAMUEL SOMOT, France]	Taken into account. Text has been revised and restructured. There is now more a focus on different regions.
82739	126	48	126	48	"tropical nights" should be defined (or replaced with "nights above 20 C", assuming that is what is meant) [Blair Trewin, Australia]	Accepted. Tropical nights have been replaced by nights above 20C
112735	126	51			I think that a sentence on projected extreme precipitation changes worses to be added somewhere. Jacob et al. 2014 (already cited), Rajczak and Schär 2017 (already cited) and Tramblay and Somot 2018. Tramblay Y., Somot S. (2018) Future evolution of extreme precipitation in the Mediterranean. Climatic Change, 151(2), 289-302, https://doi.org/10.1007/s10584-018-2300-5 [SAMUEL SOMOT, France]	Not applicable. Focus of 10.6.4 has been changed to summer warming. Extreme precipitation is not discussed any more.
112729	126	52			interesting multi-model study for the dry spell : Raymond et al. 2019, https://doi.org/10.1007/s10113-019-01526-3 [SAMUEL SOMOT, France]	Accepted. Reference has been added.
112737	126	53			Speaking about the Mediterranean marine heatwaves is relevant but I would also add a sentence on the expected SST evolution as simulated by GCMs (Mariotti et al. 2015 already cited or Alexander et al. 2018, Alexander, M. A., Scott, J. D., Friedland, K. D., Mills, K. E., Nye, J. A., Pershing, A. J., & Thomas, A. C. (2018). Projected sea surface temperatures over the 21st century: Changes in the mean, variability and extremes for large marine ecosystem regions of Northern Oceans.) and as simulated by coupled RCMs such as in Darmaraki et al. 2019 (already cited) and Soto-Navarro et al. 2020 (https://doi.org/10.1007/s00382-019-05105-4), all based on multi-model approaches [SAMUEL SOMOT, France]	Not applicable. Discussion on Marine heatwaves has been removed. Focus of 10.6.4 has been changed to mean summer warming
54435	126	54	126	56	Maximum temperature > 50°C? So far none of European countries have recorded such temperature. So how can we say about a change in occurance with respect to 1971-2000, when there was no such days? I would say that such days are expected to occur [Gabriel Stachura, Poland]	Noted. We discuss here Southern Mediterranean, not Southern Europe.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
82741	126	54	127	1	The Almazroui 2019 paper reports results for "European" domains but the maps in that paper (e.g. Figures 7e-7h) show those domains to be in Africa - this is presumably an error in the paper. (Even under the stated scenarios, five days/year over 50 C in most southern European locations in 2070-2099 seems too hot to be plausible). Unless the results from this paper can be properly reconciled this sentence should be deleted. [Blair Trewin, Australia]	Taken into account. In Mansour 2019 paper Fig. 7, the hot days >50C is Fig. 7(a-d) and cold days <5C is Fig. 7(e-h). According to the figure 7, for the northern side of the domain (southern Europe) the change in hot days in far future 2070-2099 compared to 1971-2000 is only 1 days. In the Mansour 2019 paper it is written: Within the analysis domain, the number of hot days will increase for the European regions in the far future and the greater increase will be under the RCP8.5 scenario as compared to RCP4.5. Text has been revised from 5 days to a few days.
66325	126		126		This paper could be added as a reference sinche it shows the Mediterranean warming and drought signal based on the whole EURO-CORDEX 12km ensemble , CMIP5 and CMIP6. 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. Paper has been included in the main text.
112743	127	1			Is this study based on a large multi-model ensemble ? If not, do not cite non-robust numbers. [SAMUEL SOMOT, France]	Taken into account. Two GCMs are used in Mansour 2019. A new study by Driouech 2020 has been added. The text has been revised now specifying ranges instead of specific numbers.
112745	127	3		6	again too many statements based on study using a limited (sometimes 1) number of RCMs or GCMs. To be avoided especially if other studires are available. [SAMUEL SOMOT, France]	Taken into account. For the southern and eastern regions of the Mediterranean only a few studies are available. It is important to mention those studies, but also point to the limitations of a few studies. Text has been revised.
4341	127	10	127	10	Suggest “Important identified drivers are” → “Important identified drivers during winter are” (just to make clear that this statement is also referring to winter). [Isla Simpson, United States of America]	Accepted. Text has been revised according to suggestion
112747	127	16			Macias et al. 2018 is not related to future climate change projection. This citation is missplaced. You can use Darmaraki et al. 2019 or Soto-Navarro et al. 2020 instead, see above [SAMUEL SOMOT, France]	Accepted. References to Macias et al. has been removed and suggest references have been included.
112749	127	17		20	The study by Bartok et al. has been completed and contradicted recently in Boé et al. 2020 doi:10.1007/s00382-020-05153-1 Gutierrez et al. 2020 https://doi.org/10.1088/1748-9326/ab6666. Please assess the 3 publications togather to deliver the final statement. [SAMUEL SOMOT, France]	Taken into account. Text has been revised. References of Boe and Gutierrez have been added and the consequences for the omission of time varying anthropogenic aerosols for the Mediterranean climate is discussed.
27557	127	18	127	20	Boé et al (2020) show that the main cause of differences in solar radiation between RCMs and GCMs over Europe is the absence of time-variations in the aerosol forcing in most current EURO-CORDEX RCM. This is confirmed by Gutierrez et al. (2020) who show that the absence or not of time variations in the aerosol forcing in RCMs has a major impact on their response in solar radiation. Gutiérrez C., Somot S., Nabat P., Mallet M., Corre L., van Meijgaard E., Perpiñán O., Gaertner M.A. (2020) Future evolution of surface solar radiation and photovoltaic potential in Europe: investigating the role of aerosols. ERL (accepted in dec 2019) Boé, J., Somot, S., Corre, L. and Nabat, P. Large discrepancies in summer climate change over Europe as projected by global and regional climate models: causes and consequences. Clim Dyn 54, 2981–3002 (2020). [Eric Brun, France]	Taken into account. Text has been revised. References of Boe and Gutierrez have been added and the consequences for the omission of time varying anthropogenic aerosols for the Mediterranean climate is discussed.
112751	127	22		24	This statement is too vague. Better assessment of the literature is needed. [SAMUEL SOMOT, France]	Taken into account. Text has been revised. A more detailed assessment of Jacobeit et al. (2014) has been given. The reference to Hertig and Tramblay (2017) has been removed, because the focus of 10.6.4 is now seasonal mean summer temperatures.
15285	127	26		46	This subsection describes dramatic and important changes, but none of them looks "abrupt" to me.T hey are all gradual changes [Piero Lionello, Italy]	Taken into account. Sub-section 10.6.4.8 has been removed.
112755	127	26			I think that the content of the text is sometimes more related to impacts (WG2) than to « abrupt change ». I’m not aware of any abrupt change for this region except for perhaps the strong and quick weaking of the Western Mediterranean thermohaline circulation (Somot et al. 2006 doi :10.1007/s00382-006-0167-z and confirmed in multi-model by Adloff et al. 2015 doi:10.1007/s00382-015-2507-3. and Soto-Navarro et al. 2020 https://doi.org/10.1007/s00382-019-05105-4) that may have abrupt feedbacks on the regional climate. [SAMUEL SOMOT, France]	Not applicable. Sub-section 10.6.4.8 has been removed
125757	127	28	127	29	Warming levels above the Paris Agreement temperature goals? Clarify. [Trigg Talley, United States of America]	Not applicable. Sub-section 10.6.4.8 has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
125759	127	28	127	29	There is no reference to "target levels" in the actual text of the Paris Agreement. This sentence is misleading. If the sentence is referring to the temperature goals of 1.5 and 2°C, then the text should read as such (i.e., not using the word target), and also be consistent with wording of the temperature goals in other chapters. [Trigg Talley, United States of America]	Not applicable. Sub-section 10.6.4.8 has been removed.
7975	127	31	127	32	strange sentence: how can land area contract? Is the area that is arable or inhabitable that is contracting? [Bart van den Hurk, Netherlands]	Not applicable. Sub Section 10.6.4.8 has been removed.
99253	127	35	127	46	Similar comments I made to p123, population affected, land ecosystem changes, desertification, are impacts assessed by WGII. As such I would like the section to closely collaborate with WGII to ensure that the messaging is consistent and a reference to chapter 13 and the cross chapter paper made. Not a single reference to the assessment of the impacts is made here which raises a risk of different messages. [Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Sub Section 10.6.4.8 has been removed.
4339	127	36	127	36	I'm sure many regions in Europe are indeed "heavenly", but I think that this is supposed to read "heavily" [Isla Simpson, United States of America]	Not applicable. Sub Section 10.6.4.8 has been removed.
59361	127	40	127	41	The statement of major expansion of desert areas is substantial and perhaps should have an indicative level of confidence or similar appropriate measure (e.g. agreement/robust evidence) of sensitivity used throughout the report. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Sub Section 10.6.4.8 has been removed.
15287	127	40			replace deserts with dry-areas [Piero Lionello, Italy]	Not applicable. Sub Section 10.6.4.8 has been removed.
13623	127	42	127	42	Standardize the degree symbol size [Maria Amparo Martinez Arroyo, Mexico]	Not applicable. Sub Section 10.6.4.8 has been removed.
125761	127	44	127	44	Misspelling: coast of the Mediterranean [Trigg Talley, United States of America]	Not applicable. Sub Section 10.6.4.8 has been removed.
104521	127	47	127	47	extra space [Sergio Aquino, Canada]	Not applicable. Sub Section 10.6.4.8 has been removed.
67061	127	49	127	49	for consistency after "approaches" add "for the Mediterranean" [Liese Coulter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. In none of the sub-sub sections of 10.6 the specific region is mentioned. The regions are indicated and to apply for the whole of 10.6.2, 10.6.3 and 10.6.4
15289	127	49	128	4	this looks to me misplaced. Its content could be inserted in section 10.6.4.4-5 [Piero Lionello, Italy]	Rejected. Story approaches are a tool for construction information in the distillation process of which the Mediterranean summer warming is presented as an example. Story lines are discussed and explained in box 10.2.
125763	127	51	128	4	See also recent paper by Garfinkel et al. (2020): https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-19-0232.1 [Trigg Talley, United States of America]	Accepted. Reference has been added and discussed in additional sentence.
117021	127		127		incorrect reference to the Paris Agreement, just refer here to levels of warming. You can refer to the corresponding assessment in SR15 too (which already assessed Guit et al 2016). SRCCL also has findings which are very relevant on increase in fire weather for the Mediterranean region (see ch 7 and SPM ember figure) and could be included here. [Valerie Masson-Delmotte, France]	Not applicable. Section 10.6.4.8 about "potential for abrupt change" has been removed.
112765	128	7		24	This concluding statements have to be revised to better synthese the text of the section and the title of the section. [SAMUEL SOMOT, France]	Taken into account. Section 10.6.4.9 which replaces 10.6.4.10 of SOD has been rewritten.
112759	128	9		15	I would say that there is much more to say as a synthesis of this section on Mediterranean summer warming [SAMUEL SOMOT, France]	Taken into account. Section 10.6.4.9 which replaces 10.6.4.10 of SOD has been rewritten

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15291	128	9			Delete "The Mediterranean has semi-arid climate", which is an oversimplification. It applies only to faction of the areas along the southern shore of the basin. The mediterranean climate type is characterized by dry SUMMERS, not by overall dryness. Further, areas around the Mediterranean basin (alps, eastern shore of the Adiatic Sea) have precipitation along the whole year. See fig.1.1 and 1.4 of Lionello, which you cite in 10.6.4.2 The statement should mention limited and irregular water resources in many areas of the region (which is a technically different statement)amdnot refer generically to a semi-arid climate [Piero Lionello, Italy]	Taken into account. Semi-arid as a characteristic of the Mediterranean climate has been removed. Focus of 10.6.4 has been moved to mean summer warming
112757	128	12		13	Northern Hemisphere summer mean was never mentioned before. Better to refer to global warming level I would say as done in the text of the section [SAMUEL SOMOT, France]	Accepted. Text has been revised and now refers to global warming levels
59363	128	13	128	14	Stronger reference to the mean northern hemisphere temperature is needed in the Mediteranean section of this report. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Reference to Northern Hemisphere warming has been removed and replaced by global warming level.
34719	128	17	128	24	This statement needs a reference (There is robust evidence and high agreement and, thus, high confidence that summer precipitation in the Mediterranean region will decrease toward the end of the 21st century) [Salah Ajjur, Qatar]	Rejected. This is a summary statement and is backed up by earlier references.
112761	128	17		24	I don't understand why the conclusion of the section speaks about precipitation whereas the title is Mediterranean Summer Warming. Something is illogical here. [SAMUEL SOMOT, France]	Accepted. Discussion about precipitation has been removed.
15293	128	18		20	This reason for "high confidence" is, in my opinion, not correct and should be deleted. The attribution of observed precipitation changes is rather weak (which studies show robust attribution? where are they described in this assessment?). The robustness might derive from the consensus among models (which anyway disagree on the amount, which in turn strongly depends on the scenario). I would replace this statement with "The reason for the high cofidence is the agreement among climate models on the negative sign of the precipitation change, though the value of the decrease depends on the scenario (it strongly increases with the emission level) and it varies among models."... or a similar statement [Piero Lionello, Italy]	Not applicable. Text about precipitation has been removed. Focus of 10.6.4 has been moved to Mediterranean summer warming
112763	128	20		21	Not really WG1 statements [SAMUEL SOMOT, France]	Accepted. Text not belonging to WG1 statements have been removed.
112771	128	27	129	34	I do like this final part. Thanks [SAMUEL SOMOT, France]	Noted with thanks.
66577	128	30	128	38	Also observational data representing small-scale short-lived phenomenon like cloudbursts are largely missing, not just in remote areas but also in densely populated areas as central Europe. The small-scale features of convective clouds implies that heavy precipitation may not be adequately sampled. Moreover, where high-frequency observational series do exist they may not be long enough to adequately sample enough variability for assessing changes in extremes. An example discussing the need for observations is Blenkinsop, S., Fowler, H.J., Lewis, E., Guerreiro, S., Li, X.-F., Chan, S. C., Barbero, R., Lenderink, G., Westra, S., Kendon, E., Dunn, R., Ekström, M., Tye, M.R., Holland, G., Prein, A. F., Evans, J. P., Alexander, L., Allan, R., Jones, R., Kjellström, E., Berg, P., Mishra, V., Lettenmaier, D., Klein-Tank, A. and Sheffield, J., 2018. INTENSE: INTElligent use of climate models for adaptatioN to non-Stationary hydrological Extremes. Adv. Sci. Res., 15, 117-126, DOI: 10.5194/asr-15-117-2018. [Kjellström Erik, Sweden]	Taken into account. The issue of sub-daily data is treated in Section 10.2.1.1.
23013	128	33	128	34	The undersampling has nothing really to do with WMO requirements and everything to do with the practicalities of observing in such places. I would strongly advocate removing this misleading statement. [Peter Thorne, Ireland]	Accepted. WMO standards are no longer mentioned.
41961	128	33	128	34	This is a strong and rather disturbing statement. Does it mean WMO standards are preventing observations to be carried out ? It is hard to believe that common observational standards jointly adopted and implemented by WMO Members (meaning countries) do not necessarily promote reference stations. No evidence is presented in this chapter to support this argument. Please consider deleting this attribution to the apparent lack of observations in some areas, which are mostly linked to the availability of resources to the National Meteorological and Hydrological Services. [Rupa Kumar Kolli, India]	Accepted. WMO standards are no longer mentioned.
111593	128	33	128	34	The sentence about WMO sounds rather negative. Better, please, rewrite [Volodymyr Osadchy, Ukraine]	Accepted. WMO standards are no longer mentioned.
23015	128	35	128	36	This fails to acknowledge the substantial improvements since AR5. The situation remains far from ideal but it is churlish not to acknowledge improvements in data sharing since AR5 here. [Peter Thorne, Ireland]	Accepted. The treatment of data access and sharing has been harmonized with Chapter 2 and the particular sentence here has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23017	128	39	128	44	This could note the ongoing efforts to address this need spurred by the GCOS Implementation Plan and described in Thorne et al., 2018 (linked in an earlier comment) [Peter Thorne, Ireland]	Taken into account. This paper has been included in section 10.2.1.1.
112767	128	40			also air-sea fluxes and variables over the sea and in the sea [SAMUEL SOMOT, France]	Rejected. Since Ch10 only assesses observations of land and atmosphere variables, it does not seem appropriate to explicitly include regional climate change information over the ocean in this section. Note however that we have added "among others" to the list of variables needed for evaluation of climate models.
68967	129	19	129	20	This sentence is confusing. If accurate, it would read better as "Reduced availability of long-term monitoring stations in cities strongly limits the constraint of uncertainties in those locations, as mentioned above." [Seth McGinnis, United States of America]	Taken into account. The sentence has been re formulated.
112769	129	21			I would add that there is very limited regional climate change information over the ocean and especially in the regional seas, not well resolved by GCMs. Therefore sometimes large community of users are left without good source of information [SAMUEL SOMOT, France]	Rejected. Since Ch10 only assesses observations of land and atmosphere variables, it does not seem appropriate to explicitly include regional climate change information over the ocean in this section. Note however that we have added "among others" to the list of variables needed for evaluation of climate models.
39235	129	30	129	34	Aren't evidence fromGCMs or downscaed GCMs also a line of evidence? [Lourdes Tibig, Philippines]	Noted. Yes, information derived from GCMs and downscaled GCMs are lines of evidence, but this sentence speaks about the lack of regional climate change studies based on multiple lines of evidence, as opposed to studies based on one line of evidence as it could be considering only RCMs and GCMs alone.
40459	130	0			it a bit hard to understand the logic behind the structure of the text. [TSU WGI, France]	Accepted. The structure has been revised as part of shortening the text.
40977	130	0			The link between the text and the figure could be reinforced (e.g. distillation is central in the figure but a bit buried and unclear in the text). You don't really emphasize in the text that the distillation is key to provide useful information for stakeholders. conversely you finish with a last paragraph on storylines (so I deduce it is important) but its importance is not really highlighted in the figure [TSU WGI, France]	Accepted. The FAQ was shortened, restructured and the link to the figure made clearer, with each of these points addressed.
41137	130	0			very interesting FAQ10.1 with a very nice summary. [TSU WGI, France]	Noted. Thank you for the positive comment
40385	130	0			I think the take-home message is clearly expressed in the summary and in the figure but not as much in the text. [TSU WGI, France]	Taken into account. The FAQ has been restructured and shortened to bring out important points more clearly.
40939	130	0			the FAQ is a bit too long it should be 650-750 words long. (the text could be a bit streamlined in some places e.g. L40-41 are very similar to L54- p131 L1) [TSU WGI, France]	Accepted. The FAQ was shortened.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
108083	130	1	130	55	Section: FAQ 10.1: How can we provide useful climate information for regional stakeholders? To input the below text before as separate Section: Exchange of External Debt for Ecological projects (Central Asia/ Kyrgyz experience) On May 28th, 2020, the President of the Kyrgyz Republic suggested developing a Program on an exchange of external debt for projects in the field of environmental sciences, climate change and the green economy. He requested to conduct "deep" restructuration of external debt in the epoch of the COVID-19 and after it". Internal political peripeteias have pushed to the background the ecological security issues in the Kyrgyz Republic, which is the most important factor in the sustainable development of the entire Central Asian region. Mountain ecosystems continue to degrade, and the potential of their economic services is declining. The development and adoption of a model law for the CIS and EEU States will allow coordinating collective efforts to develop mountain communities. Another problem of high mountains in Kyrgyzstan is the consequences of global climate change. Obviously, the degradation of glaciers, snowfields, and permafrost will trigger a Domino effect, at all the ecosystems of Central Asian countries, and then a multidirectional and irreversible transformation of ecosystems will begin with predictable and unpredictable consequences. First of all, in arid Central Asia, the environment and food security issues will become acute, where for centuries there have been permanent social conflicts for water and land, and where the population is growing exponentially. In 2012, the Resolution of the UN Conference on sustainable development - RIO-20 included the proposals of the National Center for Mountain Development in the Kyrgyz Republic of the International University of Kyrgyzstan the followings: i) exchange of Kyrgyzstan's external debt by multilateral creditors for sustainable mountain development and climate change adaptation to reduce the main threats to Kyrgyzstan's sustainable development; ii) creation of a group of developing mountain countries (which are isolated and landlocked) at the United Nation.	Rejected. There is no room in an FAQ for such detail.
59261	130	1	131	22	I think the communication of climate information in FAQ 10.1 is lacking in the co-production of knowledge among indigenous communities. Integrating local knowledge into the storyline provides a framework for conveying insight and information most useful to those communities. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The FAQ already points to the importance of engaging local users of the climate information, which can include indigenous and other communities. Reference to indigenous knowledge has been added.
39237	130	1	132	50	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. FAQ 10.1 is very concise.and a very easy read , both for decision makers/policy makers and practitioners in the field. FAQ 10.2 , however, even if it points out the gaps/limitations on the different aspects of heat island effect, and thus, the research gaps, is unclear on its role, if any, on regional climate change. What is the message conveyed in lines39-40? [Lourdes Tibig, Philippines]	Noted. Text has been revised.
104519	130	3	130	3	these important FAQs should have a more objective and shorter answer with maybe one graph. The busy user should be able to quickly navigate through the FAQ's of the report. [Sergio Aquino, Canada]	Rejected. The FAQs are intended to be about 600-750 words to give more substance than just one paragraph.
108219	130	3	130	3	Buildings trap heat (no need to quote) and store it during daytime, even if not in close proxmity (even though dense building structures additionally reduce nocturnal cooling in street canyons by geometrical reasons). Heat is then released during the night. See also following para! This is the major cause of the UHI and that should be made clear already in the chapeau para.. [Petra Seibert, Austria]	Taken into account. Text has been revised
41963	130	3	130	3	Creating regional, national and local platforms is essential to facilitate all the relevant stakeholders to come together, interact, co-design and co-produce decision-relevant climate information. Regional Climate Forums (RCFs) and National Climate Forums (NCFs) being actively promoted by WMO and the Global Framework for Climate Services (GFCS) can be quite effective in this regard, and need appropriate mention. [Rupa Kumar Kolli, India]	Accepted. The revised text refers directly to the utility of these "boundary organizations".
38837	130	5	130	5	I appreciate the fact that social factors are adressed next to physical features of our planet, but the expression "physically and culturally diverse" might be difficult to interpret for some readers. To avoid confusion and leave room for a variety of interpretations, would it be an option to simply say "In our diverse world, challenges posed..." [Maike Nicolai, Germany]	Rejected. There is even more opportunity with that suggestion for readers to overlook either the physical or cultural diversity.
1419	130	5	130	12	The question also depends on what information the users already possesse and how they make use of this information. [Rasmus Benestad, Norway]	Noted. This aspect was already covered in the FAQ

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38839	130	6	130	8	The repetitions of "climate change" and "information" in this sentence might not be necessary, and "information" could perhaps be specified a little further. My suggestion would be: "Information from science and other sources can help address the many aspects of people's daily life that are affected by climate change, but only as long as it is relevant for imminent decisions regarding our future." [Maike Nicolai, Germany]	Rejected. There is a clear distinction between "information" and "climate change" in the way the sentence was written.
13647	130	8	130	8	Information also contributes to the improvement of knowledge and to strengthen informed public participation in government decisions [Maria Amparo Martinez Arroyo, Mexico]	Rejected. The requested words to add were encompassed by "when the information is relevant" and, thus, unnecessarily lengthen the text.
38841	130	9	130	10	I would argue that each and everyone of us faces decisions, e.g. about lifestyle. [Maike Nicolai, Germany]	Noted. The FAQ focuses on decisions related to climate and is inclusive of all in its wording.
38843	130	9	130	10	What does the "limitations and uncertainties" refer to exactly? [Maike Nicolai, Germany]	Taken into account. The sentence in question was revised to be clearer about the context of those terms.
108221	130	10	130	10	land mass -> land surface [Petra Seibert, Austria]	Not applicable, text has been removed.
13649	130	14	130	14	Values, beliefs and economic and political interests [Maria Amparo Martinez Arroyo, Mexico]	Accepted. The text was revised to note the values, beliefs and interests are all relevant.
38845	130	14	130	19	Climate information (at least the type addressed here) is often most effective when it is developed together with those who will use it. But the words "delivery" (line 14) and "delivered and provided" (line 18) might still give the impression that climate information is developed by one group of people and then passed one to another one (the intended users). This also contradicts the notion of "partnership" and "involvement" reflected in this praragraph (as well as the following ones). I would suggest to emphasize the latter and drop the word delivery/delivered. [Maike Nicolai, Germany]	Taken into account. The FAQ makes clear that and emphasizes strongly that the climate information must be a product of all involved.
13625	130	16	130	16	Indicate a point at the end of the sentence. [Maria Amparo Martinez Arroyo, Mexico]	Editorial – copyedit to be completed prior to publication
108223	130	20	130	20	light trapping -> reduced emission of thermal radiation (light = shortwave, not “trapped”) [Petra Seibert, Austria]	Accepted. Text has been revised
38847	130	21	130	22	Suggestion to rephrase: "...of all parties involved, including different cultural backgrounds and various ways of knowing." You could also consider omitting the added half sentence (end with a full stop after "all parties involved") because cultural backgrounds and various ways of knowing are highlighted in the following sentences. [Maike Nicolai, Germany]	Taken into account. Different ways of knowing are now covered by the revised opening paragraph. Cultural diversity is often overlooked and so needs to be emphasized.
38849	130	21	130	22	It is unclear what "this" in "challenges like this" refers to. Perhaps it is sufficient to say "these" instead" But "require" might also sound prescriptive. So another way to rephrase this could be "This is particularly true for climate change – a global issue posing challenges that vary by region. Exchanging information between groups that may be culturally diverse and from different (scientific) disciplines and domains of expertise helps address these challenges." [Maike Nicolai, Germany]	Taken into account. The sentences in question have been rewritten to make the context clearer.
4343	130	30	130	31	I can’t think of many situations in regional climate change where “extending historical trends forward into the future” would be a valid or adviseable thing to do. Perhaps this should be omitted or clear examples of where that might work could be provided. [Isla Simpson, United States of America]	Accepted. This was an older practice that was included for completeness on types of methodologies, but it was dropped as a practice no longer considered acceptable.
38851	130	32	130	33	This might sound prescriptive again. You could rephrase "effective climate information builds on…" or "...draws upon…" or "integrates…" [Maike Nicolai, Germany]	Rejected. The suggestions are also prescriptive, and a point of the FAQ (and indeed the chapter) is that constructing useful climate information does need to at least consider all available sources.
68977	130	34	130	34	Change "distil" to "distill" [Seth McGinnis, United States of America]	Editorial – copyedit to be completed prior to publication
13651	130	35	130	35	Distilled information identifies the social and cultural factors of the people involved, for example, the levels of education and literacy of men and women, the differences between rural and urban women, and rural men and urban men. [Maria Amparo Martinez Arroyo, Mexico]	Noted. This is implied in the text, which is already too long to go into highly specific details.
38853	130	35	130	37	This again sounds like a one-way approach to me (one group provides information to another one), and it is focused on the sender: There might not just be "intended recepients" but also a demand for information. In addition, the factors listed might not be purely "non-climatic". [Maike Nicolai, Germany]	Taken into account. Structure has been revised to emphasize further the need for co-production of information.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
108225	130	39	130	40	Even though I don't have literature at hand which documents that, it seems very likely that climate change will amplify UHI effects at least in certain climates: 1) Solar radiation increase. 2) More anticyclonic conditions mean less ventilation. 3) Longer lasting and more intense heat waves will lead to more heat storage in structures, thus the heat wave intensification by the UHI effect will be prolongued. Also, there can be nonlinear impact effects, such as that nocturnal temperatures may rise such that indoor temperatures can't be sufficiently regulated by opening windows in the night. [Petra Seibert, Austria]	Rejected. So far the sign of the change of UHI under climate change is still not very well established as several methods give contrasting results. Recently, more and more evidence on UHI using urban dedicated urban parameterization, indicate that UHI is slightly impacted by climate change and even will slightly decrease under warmer conditions and keeping the urban land use constant in future. Combination of UHI and heat waves will have an impact on human health but still the synergy between UHI and heat wave under future climate is still not very well studied and is research gap.
4345	130	40	130	40	“have the” → “have in the” [Isla Simpson, United States of America]	Editorial – copyedit to be completed prior to publication
38855	130	45	130	53	May I suggest to drop this entire paragraph? It does not present new information, but is sometimes phrased in a prescriptive and generalising manner. Line 54 to line 1 also connects well with line 43. [Maike Nicolai, Germany]	Accepted. The structure has been revised to make FAQ more succinct, which has included dropping the examples.
59223	130		132		FAQs are important in aspect of finding answers to all their questions in one place.However only two FAQS have included to chapter 10. Addition of more FAQs to this chapter will be very usefull for the soceity to understand the problem and answers. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The choice of the FAQs was settled prior to producing the first-order draft and in accordance with the TSU.
38857	131	3	131	3	Is this use of "stories" and "storylines" in line with the way these words are used elsewhere in the report (e.g. in conenction with scenarios)? These words usually have a different connotation outside the (climate) science community, and it might not be fully clear what the storylines referred to here might look like. [Maike Nicolai, Germany]	Noted. This should be much clearer in the final version of the WGI report. The term storyline has been defined in the AR6 cross-WG glossary and is followed in the WGI report. In Ch10 the use of storylines for producing information about regional climate change is treated in BOX 10.2.
4347	131	3	131	3	“stories” → “storylines” [Isla Simpson, United States of America]	Accepted
67063	131	3	131	11	For accuaracy and clarity replace this papagraph with the following: Narratives can connect experiences of past weather and climate with new information through story telling. Physical climate storylines can make climate information more accessible and provide concrete illustrations of climatic change in a warming world. For example, a temperate region storyline may link to a common experience around the timing and duration of a winter storm to show how snowfall and winds can change in ways that may never have been experienced before. Storylines can be informed by stakeholders’ expertise to address their areas of prime concern, such as water-resource managers and health professionals who seek to develop appropriate response measures. Carefully developed storylines can convey nuances of climate information by building on common experiences that gives it more meaning and enhance the information’s usefulness. [Liese Coulter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The FAQ has been revised to use the term storyline to be consistent with the Glossary and aligned with Chapter 10's usage.
40969	132	0			the language could be simplified in places to make the text more accessible to a lay audience (e.g. avoid acronyms, jargon etc) [TSU WGI, France]	Accepted. Text has been revised
41049	132	0			there is a lack of link between the text and the figure. [TSU WGI, France]	Accepted. Text and figure has been revised
40117	132	0			FAQ10.2 :interesting FAQ nicely contextualized and with a strong conclusion [TSU WGI, France]	Noted.
40947	132	0			the flow/structure of the text does not always seem logical and can be hard to follow [TSU WGI, France]	Accepted. Text has been revised
38859	132	1	132	40	I think either the question or the answer needs to be rephrased - the text highlights one single aspect of cities' interaction with climate change, but there are many others, for example changes in precipitation, flooding or othere risks especially for coastal citites. A more suitable question might be "Why are cities hotspots of global warming? or "What challenges do cities experience in the face of rising temperatures?" [Maike Nicolai, Germany]	Accepted. Text has been revised
59443	132	1			Section FAQ 10.2: Consider to add information relative to the contribution of green areas to the reduction of the urban heat and how it can reduce the global warming impact in these areas. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This is more a WGII expertise and we prefer to keep here only information that is WGI-related.
65055	132	3	132	40	Could the question whether the urban heat island effect exaggerate the global warming signal be addressed here? [Magnus Joelsson, Sweden]	Taken into account Text has been revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68979	132	5	132	5	Change "are creating" to "create" [Seth McGinnis, United States of America]	Accepted. Text has been revised
44257	132	5	132	6	As the Urban Heat Island is a night-time phenomenon, it is better to write here "..which causes cities to experience higher than average temperatures than their surrounding area during the night". [Nektarios Chrysoulakis, Greece]	Accepted. Text has been revised
4349	132	9	132	9	"Cities are on front line" → "Cities are on the front line" [Isla Simpson, United States of America]	Not applicable, text no longer in the chapter
78311	132	9	132	11	Suggest to reconcile the messaging in this section, particularly the line that states "cities are responsible for up to 70% of current emissions", with the earlier BOX 10.2 which states that "there is medium evidence but high agreement that the global annual mean surface air temperature response to urbanisation is negligible" (Page 93, Lines 44 to 45). [Leonie Lee, Singapore]	Not applicable Text no longer included in the revised version.
38861	132	9	132	14	This is a great introduction that should tell every reader very clearly why this is an important issue. [Maike Nicolai, Germany]	Noted.
65051	132	11	132	11	What is the current share of the world's population living in urban areas? [Magnus Joelsson, Sweden]	Not applicable Text no longer included in the revised version.
68981	132	12	132	12	Change "90% of these is" to "90% of whom are" [Seth McGinnis, United States of America]	Not applicable Text no longer included in the revised version.
38863	132	15	132	17	Can information from more recent events be added here? For comparison, it would also be helpful to say how long those heat waves were or how many people died per day. At the moment, I would wonder why there were so many more deaths in France than in India. [Maike Nicolai, Germany]	Taken into account Text has been revised
104517	132	17	132	18	new paragraph [Sergio Aquino, Canada]	Accepted. Text has been revised
44259	132	18	132	18	Man-made materials in cities may have high albedos, however the bulk albedo of the urban areas is lower that their surroundings. The main reason for this is the three-dimensional structure of the urban surface and the shadow effect in urban canyons, as it is evident in numerous cities globally (Chrysoulakis, N., et al., 2019: Exploiting satellite observations for global surface albedo trends monitoring. Theoretical and Applied Climatology, 137, 1171 - 1179). [Nektarios Chrysoulakis, Greece]	Noted. This is too technical to be put in a FAQ we prefer to keep a simple yet correct description of albedo in cities
16953	132	18	132	21	The description of the causes of the urban heat island misses one element: moisture content/availability (Oke et al., 2017 Table 7.2). Moisture availability is reduced compared to natural soil, due to the use of mostly-impervious building materials (e.g. concrete). This produce a larger partition of heat into sensible heat over latent heat, with consequent increase in air temperature. [Gianluca Mussetti, Switzerland]	Accepted. Text has been revised
38865	132	21	132	21	I would omit the word "surface" here, because it might be confusing. You spoke about different "surfaces" in the previous sentences, but this one is air, not any of the surfaces mentioned before. [Maike Nicolai, Germany]	Taken into account. Text has been revised to focus only on near surface air temperature
6833	132	21	132	23	They are therefore often associated with elevated surface air temperature and land surface temperature, a phenomenon referred to as the atmospheric urban heat island and the surface urban heat island respectively where night-time urban air and surfaces temperatures are substantially higher (several degrees) than the corresponding temperatures in the surrounding rural areas. [Constantinos Cartalis, Greece]	Taken into account. Text has been revised to focus only on near surface air temperature
38867	132	22	132	22	Introduce the acronym UHI here, please. [Maike Nicolai, Germany]	Accepted. Text has been revised
38869	132	23	132	25	Can be said by up to how many degrees or the intensification be illustrated otherwise? [Maike Nicolai, Germany]	Noted.
23021	132	27	132	27	better understood than what? [Peter Thorne, Ireland]	Accepted. Text has been revised
44261	132	27	132	29	Beyond in-situ measurement networks, high spatial/temporal resolution satellite thermal infrared observations are also still lacking. Furthermore, direct urban heat and CO2 emission observations at neighbourhood scale are also missing in most cities. [Nektarios Chrysoulakis, Greece]	Taken into account. Text has been revised
38871	132	27	132	40	Your readers might appreciate it if you could address some potential ways of how the UHI effect can be minimised or dealt with, instead of expanding on uncertainties. As you say, the challenge is there, even though the details are not clear yet - and advice how to tackle it might be considered helpful. [Maike Nicolai, Germany]	Rejected. As this question relate more to WGII expertise
65053	132	28	132	28	"… remain are lacking ..." [Magnus Joelsson, Sweden]	Accepted. Text has been revised
4351	132	28	132	28	delete "remain" [Isla Simpson, United States of America]	Accepted. Text has been revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9991	132	32	132	32	Such data is important to help with the future masterplanning of cities using green building/instrastructure tools that have been developed (Siew et al., 2013). See A Review of Building/Infrastructure Sustainability Reporting Tools (SRTs) https://www.emerald.com/insight/content/doi/10.1108/SASBE-03-2013-0010/full/html [Renard Siew, Malaysia]	Noted. We do not cite references in FAQ text.
23023	132	36	132	37	Is this italicisation really necessary? [Peter Thorne, Ireland]	Accepted. Text has been revised
59365	133	48	133	53	There are two Akhter et al 2019 references which need to be separately identified through out this section of the report using a and b after the year. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
13653	137	9	137	9	From a social perspective, in the following paragraph, seems inadequate to include the population (men, women, children, etc.) within the connotation of "asset": "Furthermore, a climate risk exists only when a climate hazard has the potential to affect an asset that is both exposed to and vulnerable to that hazard". [Maria Amparo Martinez Arroyo, Mexico]	Noted. We think the reviewer might have wanted to direct this comment to another chapter. We do not mention "asset" in Ch10.
110653	140	48	140	50	This paper has been published, please update the reference: Casanueva, A, Herrera, S, Iturbide, M, et al. Testing bias adjustment methods for regional climate change applications under observational uncertainty and resolution mismatch. Atmos Sci Lett. 2020;e978. https://doi.org/10.1002/asl.978 [Ana Casanueva, Spain]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
59323	140	48	140	50	The citation for "Casanueva et al. (submitted)" must be updated in the list of reference (2020, DOI: 10.1002/asl.978) in page 140 and line 48. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
41949	143	28	143	29	Incorrect bibliographic details. Should be cited as Collins, M., R. Knutti, J. Arblaster, J.-L. Dufresne, T. Fichefet, P. Friedlingstein, X. Gao, W.J. Gutowski, T. Johns, G. Krinner, M. Shongwe, C. Tebaldi, A.J. Weaver and M. Wehner, 2013: Long-term Climate Change: Projections, Commitments and Irreversibility. In: Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change [Stocker, T.F., D. Qin, G.-K. Plattner, M. Tignor, S.K. Allen, J. Boschung, A. Nauels, Y. Xia, V. Bex and P.M. Midgley (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA. [Rupa Kumar Kolli, India]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
59263	145	47	145	48	Deng et al., reference. Volume 33, pg 281-301 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
125765	146	2	146	3	This paper is now published: Deser, C., F. Lehner, K. B. Rodgers, T. Ault, T. L. Delworth, P. N. DiNezio, A. Fiore, C. Frankignoul, J. C. Fyfe, D. E. Horton, J. E. Kay, R. Knutti, N. S. Lovenduski, J. Marotzke, K. A. McKinnon, S. Minobe, J. Randerson, J. A. Screen, I. R. Simpson and M. Ting, 2020: Insights from earth system model initial-condition large ensembles and future prospects. Nat. Clim. Change, doi: 10.1038/s41558-020-0731-2. [Trigg Talley, United States of America]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
59265	146	31	146	33	Di Sante et al. reference. vol. 53, 759-778 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
59369	147	36	147	48	There are two Dong et al 2017 references which need to be separately idenitfied through out this section of the report using a and b after the year. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59267	148	30	148	32	Dubrovsky et al. reference. 2020, vol 139, 1031-1044 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
59269	150	56	150	56	Fox-Rabinovitz et al. reference. Accents needed for authors Côté and Déqué [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
59271	151	51	151	53	Galmarini et al. reference. Vol 13, 65-69 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
59273	152	55	152	55	Lowercase Giorgi and Gao reference [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
59275	155	7	155	9	Gutiérrez et al. reference. 2019, vol 39, 3750-3785 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
59277	155	13	155	14	Gutmann et al. 2018 reference vol. 31 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
59279	158	41	158	42	Hirsch et al. 2018 reference. Vol. 24, 4758-4774 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
83899	158	51	158	53	Please note that the ref Hock et al.2019 is published long ago, and not longer "in press" [Ulf Molau, Sweden]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
5503	163	46	163	46	Add a reference: Kim, J., and Kang H. (2007). The impacts of the Sierra Nevada on low-level winds and water vapor transport. J. Hydromet., 8, 790-804. [Jinwon Kim, United States of America]	Rejected. The mentioned paper discussed the processes whereas our Chapter assesses the representation of processes in climate models (which is not discussed in the paper).
5507	163	51	163	51	.Kim, J., Guan B., Waliser, D.E., Ferraro, R.D., Case, J.L., Iguchi, T., Kemp, E., Putman, W., Wang, W., Wu, D., and Tian B. (2018). Winter precipitation characteristics in western US related to atmospheric river landfalls: observations and model evaluations, Climate Dyn., 50, 231-248. doi 10.1007/s00382-017-3601-5. [Jinwon Kim, United States of America]	Noted. Atmospheric rivers are assessed in Chapter 8.
110643	165	1	165	5	The reference Kotlarski et al. 2019 is repeated. [Ana Casanueva, Spain]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
125767	167	45	167	46	This reference is now in press: https://www.earth-syst-dynam-discuss.net/esd-2019-93/ [Trigg Talley, United States of America]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
59281	168	14	168	14	León et al. reference. "and Imberger, J." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59283	169	48	169	50	Liu et al., 2018b reference. Vol 45, 13041-13049 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
59285	171	54	171	54	Maraun et al. 2017b reference, page numbers should be 764-773 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
59287	172	1	172	2	Maraun et al., 2018 should be 2019 vol 39, 3692-3703 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
59289	174	8	174	10	Ménégoz et al. 2018a, 064022 instead of 64022 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
70933	174	41	174	43	Mindlin et al. is now published (2020): doi: 10.1007/s00382-020-05234-1 [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
5497	187	33	187	33	Shim, S., Kim, J., Yum, S., Lee, H., Boo, K., Byun, Y. (2019) Effects of anthropogrnic and natural forcings on the summer temperature variations in East Asia during the 20th century. Atmosphere, 10, 690: doi:10.3390/atmos10110690. [Jinwon Kim, United States of America]	Not applicable. The East Asian summer monsoon has been removed from the chapter due to space issues.
59291	190	7	190	9	Stevenson et al. 2017 reference remove LP- from page numbers [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
67035	190	54	190	54	Article title should start with ESD Ideas … not just Ideas … [Liese Coulter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
59293	191	26	191	28	Takayabu et al. 2016 reference. Some names are capitalized. Von Storch is out of order [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
59295	194	19	194	21	Vergara-Temprado et al. reference. vol. 33, 1915-1933, year 2020 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59297	195	10	195	11	Wada et al., 2014 reference. Vol 5, 15-40 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
5493	195	24	195	24	Waliser, D., Kim, J., Xue, Y., Chao, Y., Eldering, A., Fovell, R., Hall, A., Liou, K., McWilliams, J., Kapnick, S., Vasic, R., De Sale, F., Yu, Y. (2011) Simulating cold season snowpack: Impacts of snow albedo and multi-layer snow physics. Climatic Change, v. 109, S95-S117. [Jinwon Kim, United States of America]	Noted.
59299	196	10	196	11	Wang et al., 2015c reference. Pg 263-273 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
82723	199	15	199	17	The weblink for the Yasutomi et al 2011 reference does not work (although I found it at Development of a Long-term Daily Gridded Temperature Dataset 16 and Its Application to Rain/Snow Discrimination of Daily Precipitation). That PDF suggests it was published in Global Environmental Research. [Blair Trewin, Australia]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
59301	201	43	201	45	Zubler et al., 2011 reference. J. Geophys. Res. Atmos. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
13627	202	2	202	3	In the figure change experiment s a experiments [Maria Amparo Martinez Arroyo, Mexico]	Accepted. The figure has been entirely redone.
117023	202		202		What about "scientific publications" rather than literature. Can you define "climate experts"? (scientists, practitioners?). [Valerie Masson-Delmotte, France]	Accepted. The following addition to the figure 10.1 has been requested to the corrigenda "Literature refers to scientific and technical literature, and climate experts to climate scientists, practitioners and local communities, as defined in section 10.5".
112037	202		202		The first block (input sources of information) includes RCMs, bias adjustment, etc. which is mostly described in the regional chapters (at lest for model data description, e.g. CORDEX and CORDEX-CORE in ATLAS). Ch 3,4,8,9 are described here and not sure how to address this so I am just pointing this out. [jose manuel gutierrez, Spain]	Noted. The figure has been entirely redone.
87771	202		202		Fig.10.1: It is an interesting diagram, but I don't understand the structure of empty hexagonal cells under "information construction". It takes a lot of space and explains nothing. Maybe it is a spurious remainder of previous versions of this figure? [Sergio Henrique Faria, Spain]	Not applicable. The figure has been completely redrawn and the hexagons have disappeared.
4353	203	1	203	1	ENSO doesn't seem quite at the right timescale here. I think it should go closer to QBO so that it appears closer to years than decades. [Isla Simpson, United States of America]	Accepted: Change "decade" to "year" position
13629	203	1	203	1	In the figure 10.2 change Nino por Niño [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Change the character
23025	203	1	203	1	Figure would benefit from addition of a self-describing title so that it could be used in standalone mode in presentations nad outreach. Some of the fonts are on the small size. Axis labels would be helpful. Some of the acronyms there is space to long-hand these and that would improve accessability and reduce overall figure clutter. Where you can longhand - e.g. LES without detriment suggest doing so. Earlier chapters use ESMs in preference to GCMs. [Peter Thorne, Ireland]	Take into account: On handling ESM and GCM should follow to the other part of this report
79459	203	4	203	4	This caption is not very precise. The figure does not show "relevant interacting space and time scales to regional climate change information. " but a rough estimation of temporal/spatial scales of a number of limited processes that occur in the climate system [Alejandro Di Luca, Australia]	Take into account: we revised the legend.
27559	203	4	203	4	About Figure 10.2: It is not clear whether the figure shows the resolved scales by the different models or simply their resolution. Actually, it seems to be a mix of both. It should be discussed in the legend. [Eric Brun, France]	Take into account: we revised the legend.
110641	203	4			The reference should be inserted as an inline reference. [Ana Casanueva, Spain]	Accepted. The reference format has been corrected.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59189	203		203		Figure 1.2 Consider increasing the font size of this figure. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Revise the figure
87773	203		203		Fig.10.2: The word "Earth" refers to the planet, and therefore should be capitalized. [Sergio Henrique Faria, Spain]	Accepted. Revise the figure
34387	203				Figure 10.2. X axis: separate number from unit. [Guiomar Rotllant, Spain]	Accepted. Revise the figure
23027	204	1	204	1	Figure would benefit from addition of a self-describing title. [Peter Thorne, Ireland]	Not applicable. Figure 10.3 has been removed.
78759	204	4	204	4	This plot shows the regions used in Ch10. However, the definition of the rgions are different with the other regional Chapters. For example, the definition of East Asia is different with Atlas, and it is also different with that shown in Figure 10.13. [jian li, China]	Not applicable. Figure 10.3 has been removed.
59191	204		204		Figure 1.3 Consider increasing the font size of this figure. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Figure 10.3 has been removed.
87775	204		204		Fig.10.3: Couldn't we replace the rectangular regions by their counterparts in the set of AR6 WGI Reference Regions? Among other advantages, this would facilitate their relation to the Interactive Atlas. [Sergio Henrique Faria, Spain]	Not applicable. Figure 10.3 has been removed.
1633	205	1	236	1	All plots need to state which version of datasets like NOAA, GISS, GPCC, CRU, BEST, HadCRUT4.X which versions have been used. Some are OK, but others are not. You've been much better with the models used. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. However, Cross-Chapter Box 10.1, Fig. 1 is a conceptual figure
45109	205	2	205	5	In my opinion, the upper part of Figure 1 in Cross-Chapter Box 10.1 (with 'Impact of Global Climate Change' and 'Impact of Natural Variability') also deserves a brief comment in the figure caption. [Dmitry Kovalevsky, Germany]	Taken into account. Figure caption modified. Upper part representing Arctic warming is now specifically mentioned in the figure caption.
125769	205	2	205	5	The polar vortex in Cross-Chapter Box 10.1 Figure 1 is spinning in the wrong direction. The vortex should be rotating counterclockwise about the pole in the Northern Hemisphere. Also, given the low confidence in these processes discussed in Cross-Chapter Box 10.1, the figure needs to better convey that the processes are speculative hypotheses and are highly uncertain/controversial. [Trigg Talley, United States of America]	Taken into account. Figure has been modified. Rotation of spinning has been revised. More emphasis is now on speculative character in figure.
117025	205		205		The figure is nice, but it does not report the outcome of the assessment (low confidence). [Valerie Masson-Delmotte, France]	Taken into account. Figure has been revised. Low confidence statement is now included in the figure.
59445	205				Cross-Chapter Box10.1, Figure 1. Consider improvimg the visualisation of this figure, especially in the case of the font type and color used. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Figure has been modified.
68951	206	1	206	1	In Figure 10.4, the shade of green used for the green arrows is difficult to distinguish from the gray used for the gray arrows. I suggest revising this figure using a brighter, more saturated green with less brown. [Seth McGinnis, United States of America]	Accepted. The figure has been adjusted.
59303	206	1	206	6	Figure 10.4: the color of the green arrows is difficult to distinguish from the gray. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The figure has been adjusted.
102519	206		206		The way the authors have categorised different model types and chains used in modelling regional climate could be improved. The classical linear "model" akin to the cascade of uncertainty is favoured. [Philippe Tulkens, Belgium]	Rejected. A key issue of the Chapter is to highlight the various alternative approaches for generating regional information. A simple linear chain would exactly limit users to the standard GCM / RCM / BA path which may not be the best option in a given context.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112051	206		206		The use of Dynamical downscaling for perfect prog is not straightforward and is not a typical application. I would either drop that or include this as a dashed line. This could be done by drawing the vertical line from dynamical downscaling straight down to Perfect progr with the last segment dashed (just an idea in case it helps). [jose manuel gutierrez, Spain]	Accepted. The figure has been adjusted.
23031	207	1	207	2	Please use Berkeley Earth rather than BEST in top panel. It is very unclear what the two box-whisker plots represent. Can a title be added to each? Why are there varying numbers of whiskers by source type? [Peter Thorne, Ireland]	Taken into account. Figure has been revised.
23033	207	3	207	3	systematic errors implies the truth is known / knowable which cannot be true. Systematic uncertainties would be better phrase here. [Peter Thorne, Ireland]	Taken into account. Figure has been revised.
125771	207	3	207	21	More detail needs to be provided in the caption about what is exactly shown in the box and whisker plots. For example, what are the black dots? Are these outliers? Why are they colored black as opposed to the colors of the respective boxes? [Trigg Talley, United States of America]	Taken into account. Figure has been revised.
87785	207	15	207	15	Figure caption: the label "(b)" is missing at the beginning of the sentence. [Sergio Henrique Faria, Spain]	Taken into account. Figure has been revised.
112053	207		207		This is a very nice illustrative figure of the complexity of dealing with multiple lines of evidence (datasets). There is the potential to include this region in the Interactive Atlas and extend the analysis considering projections. This could be used to develop an example of what information can be obtained from the Interactive Atlas, highlighting also the limitations. Would be worth it to explore. [jose manuel gutierrez, Spain]	Noted. Collaboration with the Atlas has been considered.
87777	207		207		Fig.10.7: This is a complicate figure, which the average reader may not understand. What is the scale in the horizontal axis of the two graphs? What is the "take home message" of this figure? [Sergio Henrique Faria, Spain]	Noted. We believe the key message of the figure is clear to a broad range of readers: that models are biased at the regional scale.
34389	207				Figure 10.5. Need to add X axis labels (different models data correspond to …?). [Guiomar Rotllant, Spain]	Taken into account. Figure has been revised.
80613	208	1	208	1	Fig. 10.6 mentions HighResMIP in the caption, but does not have any HighResMIP data on the plots. [Malcolm J. Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Figure 10.6 has been removed for FGD
23035	208	1	208	1	The key map is only needed once (just gross duplication otherwise). This may enable the data panels to be made larger following rearrangement. Spell out the domains in panel figures rather than using code - there is room to write out in full France, Scandinavia etc. An overall figure title would aid accessability. [Peter Thorne, Ireland]	Not applicable. Figure 10.6 has been removed for FGD
27561	208	4	208	4	About Figure 10.6: It would be very interesting to add the results of HighResMIP low and high resolution simulations. [Eric Brun, France]	Not applicable. Figure 10.6 has been removed for FGD
87779	208		208		Fig.10.8: This figure still needs several aesthetic improvements (fonts, spacing, etc.) to be more understandable. [Sergio Henrique Faria, Spain]	Not applicable. Figure 10.6 has been removed for FGD
4355	209	1	209	1	Perhaps it would be worth stating in the figure caption what blocking index is used for readers just glancing at figures. [Isla Simpson, United States of America]	Noted. However, due to space reasons only a reference to the Schiemann et al. paper is given.
23037	209	1	209	1	This needs considerable work to be accessible. Everything is written in code including the panel titles. This is simly inaccessible unless someone knows all these acronyms. Give self-describing titles including an overarching figure title. What is ERA/IV? What are the triangles and stars? These need to be denoted for the figure to stand a chance of being interpretable in its own right. [Peter Thorne, Ireland]	Taken into account. Figure has been revised.
59307	209	1	209	8	In the figure caption the meaning of the whisker for ERA interim data is not explained. In addition, it is not clear to me why the values in ERAI change from the left column to the right column [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Figure has been revised.
34391	209				Figure 10.7. Meaning of abbreviation in top-right in each figure need to be explained. [Guiomar Rotllant, Spain]	Taken into account. Figure has been revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23039	211	1	211	2	What is PR change? Do you mean projected precipitation change? If so, spell that out in the colour bar label. Label values and also the lat / lons are arguably too small. Figure lacks an overall title such as e.g. Impacts of use of regional climate models to downscale precipitation over the Alps [Peter Thorne, Ireland]	Taken into account. Figure has been revised.
23041	212	1	212	2	font is so small as to be barely legible at native resolution. Is the CRU dataset subsampled to only the gridboxes with an observational constraint? It is critical to do so because they infiill with climatology in other periods and that will alter the series in important ways. What are the box-whiskers in right hand panels? Figure would be better split into two (one for Asia one for Africa) so that the panels can be made much larger and thus more legible to the reader. Figure lacks an overall self-describing title. [Peter Thorne, Ireland]	Taken into account. Figure has been revised.
59409	212	1	212	16	General comment on the Figure 10.10 (a) (b) (c) Would be nice to see only shaded box region [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The comment is not quite clear. The larger box has been plotted to show longitudes/latitudes in 5° steps.
59411	212	1	212	16	Figure 10.10 (e) and (f): Would be better to use grey shading for all ensemble members rather than grey lines [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Individual lines are essential to highlight the fact that these are possible individual futures.
81247	212		212		Figure 10.10 "Observed and projected changes in seasonal mean (December to February in the left column and June to 5 August in the right one) precipitation" Please mention the region in the title of the figure [Fatima Driouech, Morocco]	Taken into account. Figure has been revised.
87781	212		212		Fig.10.10: Everything in this figure is so tiny! It needs some rearrangement to increase size and improve readability. [Sergio Henrique Faria, Spain]	Taken into account. Figure has been revised.
59447	212				Figure 10.10. Consider increasing the font size of this figure. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Figure has been revised.
68961	213	1	213	1	This figure is confusing and hard to read. It needs to be modified for greater clarity. I think it would be easier to understand if the x-axes were sorted in a different order. Right now, it goes Resolution > Method > GCM or RCM > Obs, and I think it should probably go GCM or RCM > Resolution > Obs > Method. I think it would also help to use color to indicate the bias adjustment method instead of GCM vs RCM, and to group the labels together for the different categories, so that you just have one label spanning a group of boxplots in a given category rather than repeating it over and over, which is hard on the eyes. To highlight the fact that bias adjustment can change the trend, add a line at the median of the boxplot for the raw data. (This will be relatively easy if the sort order of the x-axis is changed as suggested above.) [Seth McGinnis, United States of America]	Not applicable. Figure has been removed.
23043	213	1	213	2	Suggest use the internationally respected convention of tm, tx and tn for the three temperature elements. Figure lacks a self-describing title. [Peter Thorne, Ireland]	Not applicable. Figure has been removed.
96103	215	1	215	8	Figure 10.11: Check color "magenta". Appears as dark red. [Nicole Wilke, Germany]	Not Applicable. The figure 10.11 has been removed for the FGD
65473	215	12	215	12	In SESA region there is not a drought process. Instead, there is an increase of rainfall [Leandro Diaz, Argentina]	Not Applicable. The figure 10.11 has been removed for the FGD
112063	215		215		It looks like the timeseries values respresented are anomalies w.r.t 1951-1980 for the temperatures, but not for precipitation. [jose manuel gutierrez, Spain]	Not Applicable. The figure 10.11 has been removed for the FGD
23045	216	1	216	2	Much of the text is so small as to be unreadable. Figure lacks a self-describing title. The dashed grey lines are distracting in the lower right. GPCC product cannot be as complete as implied unless infilled in which case has masking been applied correctly? It must be the case that many of these are either climatology or very low confidence values. Especially if infilled with climatology it may have a large effect on panels b and d. I would only include data where there is a meaningful direct observational constraint or use their non-infilled product. The current 'complete' field serves to undermine arguements vis-a-vis data sparsity made in the main text. [Peter Thorne, Ireland]	Taken into account. In the FGD we have balanced readability and amount of information as best as possible. For the spatial map of the observed trend, the GPCC data has been replaced with CRU TS and masking is used to obscure some incomplete data. Both GPCC and CRU TS datasets have been shown in panel (e) showing small uncertainty in the observed trend in comparison to the multi-model data. The distracting dashed grey lines have been removed from the FGD.
87783	216		216		Fig.10.12: Same as Fig.10.10... Everything in this figure is so tiny! It needs some rearrangement to increase size and improve readability. [Sergio Henrique Faria, Spain]	Accepted. In the FGD we have balanced readability and amount of information as best as possible.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23047	217	1	217	1	Comments made to the prior figure also apply here. In addition the CRU TS product absolutely needs to be filtered to show values only where an observational constraint is applied using the second data field and as done in chapter 2. CRU TS defaults to climatology and the trends in CRUTS will be unrealistic unless this is done. [Peter Thorne, Ireland]	Not applicable. The figure 10.13 has been removed for the FGD. Note that the reviewer comment has been followed for all precipitation datasets used in the chapter figures
81667	218	1	218	1	The map plots of Autralia should include Tasmania [Michael Grose, Australia]	Not applicable. The figure 10.14 has been removed for the FGD due to shortening constraints
23049	218	1	218	1	See comments upon the prior two figures which equally apply here. In particular the spatial maps of observations clearly suffer from different choices as to how to cope with data incompleteness and require revision to omit areas without an observational constraint (applies in particular to CRU TS) [Peter Thorne, Ireland]	Not applicable. The figure 10.14 has been removed for the FGD due to shortening constraints
82699	218	1	218	10	Local rainfall data sets also exist and could be used alongside GPCC and CRU TS (and would also allow updating beyond 2014) - AWAP is the current operational BOM data set (www.bom.gov.au/climate/change), but it is likely that a new data set will be available by the time of FGD. [Blair Trewin, Australia]	Not Applicable. Thanks, but figure 14 has been removed for the FGD due to shortening constraints
125773	218	16	218	16	Please consider making the NCAR-GE a different color in panel e. The pink bars in the previous two figures referred to d4PDF-GE, not the NCAR-GE. It would be helpful if there were some consistency in the colors used across figures. [Trigg Talley, United States of America]	Not Applicable. Thanks, but figure 14 has been removed for the FGD due to shortening constraints
68489	218				Please add legends explaining histograms in Fig. 10.14c in the same manner as Fig. 10.13c. [Yukiko Imada, Japan]	Not applicable: figure 10.14 has been removed for the FGD due to shortening constraints
4357	219	1	219	1	This may just be my pdf viewer, but a lot of the i's seem to be missing from the schematic portion of the figure. [Isla Simpson, United States of America]	Accepted. The figure has been revised to correct the missing i's.
23051	219	1	219	1	The CRU estimates in panel b need to be masked fo observational constraint availability as there is no chance all those gridpoints have data. The masked values should also be used to construct the estimate used in panel d [Peter Thorne, Ireland]	Accepted. Masking has been applied to GPCC and CRUTS for all section 4 figures of the FGD
125775	219	3	219	4	There are lots of missing letters in the words in the schematic in panel a of Figure 10.15. [Trigg Talley, United States of America]	Accepted. The figure has been revised to correct the missing i's.
23053	220	1	220	1	In addition to comments upon similar earlier figures which equally apply here please avoid using the label BEST as noted in other comments - this is value laden. [Peter Thorne, Ireland]	Not Applicable. The figure 10.16 has been removed for the FGD
80341	220	1	220	1	In general, for figures dealing with atrribution of regional changes, it would be convenient to show a small map with the geographic region. In particular, for Fig. 10.16 is not easy to identify the regional domain. [Paola Arias, Colombia]	Not Applicable. The figure 10.16 has been removed for the FGD
111583	220		220		There are many questions for the Figure 10.16: (1) why January-March is called winter when in this region calendar winter starts in December; (2) observation maps are in conflict with Interactive Atlas, moreover, the most prononced warming was recorded in the north-east part of Ukraine and EEA sub-region and definetely not negative trend. So, it is not clear how these maps were ploted; (3) from graph (b) is clear artificial nature of so big negative trend reported for the short period started from the warmest year. So, this is a good example how climate data SHOULD NOT be used to form climate message; (4) anyway trend should be reported as degrees per decade (not 12 years) [Volodymyr Osadchy, Ukraine]	Not Applicable. The figure 10.16 has been removed for the FGD
68491	220				Please add legends explaining histograms in Fig. 10.16c in the same manner as Fig. 10.13c. [Yukiko Imada, Japan]	Not Applicable. The figure 10.16 has been removed for the FGD
23055	221	1	221	1	Please avoid using the acronym BEST here - value laden [Peter Thorne, Ireland]	Not Applicable. The figure 10.17 has been removed for the FGD
23057	222	1	222	2	Both GPCC and CRUTS will have substantial regions where the value is either a climatology (CRU TS) or an unconstrained estimate (GPCC). Masking should be applied to remove all such cases and the figures redrawn. Otherwise prior comments on similar figures also apply here [Peter Thorne, Ireland]	Accepted. Masking has been applied to GPCC and CRUTS for all section 4 figures of the FGD
68493	222				Please add legends explaining histograms in Fig. 10.18c in the same manner as Fig. 10.13c. [Yukiko Imada, Japan]	Taken into account. The figures for the remaining examples in the FGD have been homogenized in term of format.
23059	223	1	223	2	See comments on prior similar figures in particular on the use of spatially complete CRU TS and GPCC products and issues therein. Why the area mismatch between panels a and b? [Peter Thorne, Ireland]	Not applicable. The figure 10.19 has been removed for the FGD
125777	223	16	223	16	"this four stations" should be "these four stations" [Trigg Talley, United States of America]	Not applicable. The figure 10.19 has been removed for the FGD

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59413	224	1	224	12	Figure 10.20 seems not clear like color balance and sharpness, Need revise figure. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not Applicable. The figure has been removed for the FGD.
27563	224	4	224	4	About Figure 10.20: The figure is too small and hard to read. [Eric Brun, France]	Not Applicable. The figure has been removed for the FGD.
87787	224		224		Figure 10.20: As in Figs. 10.10 and 10.12: Everything in this figure is so tiny! It needs some rearrangement to increase size and improve readability. Furthermore, it needs several aesthetic improvements, like a vertical line separating near from long term, etc. The grey background in the legend for CSIRO-GE may confuse the reader, who may think that this has some relation to the grey boxes in the plots. [Sergio Henrique Faria, Spain]	Not Applicable. The figure has been removed for the FGD.
59449	224				Figure 10.20. Consider increasing the font size of the text of the x & y axis [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not Applicable. The figure has been removed for the FGD.
34393	224				Figure 10.20. In figure (b), order of figures is weird. I will interchange Caribbean with East Asia. [Guiomar Rotllant, Spain]	Not Applicable. The figure has been removed for the FGD.
38361	225	1	225	1	In Box 10.2, Figure 1, two icons are used to represent country and city, while no legend is given. In the Figure, China and Japan are countries, while Hong Kong and others are cities. It is suggested to delete countries from the Figure or add a legend in order to avoid confusion. [Yaming LIU, China]	Taken into account. Figure has been modified.
59405	225	1	225	17	Box 10.2 names of region should be in one format (Area, Country) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Figure has been modified.
59407	225	1	225	17	Box Figure 10.2 We can clearly see that northern hemisphere cities are warming more. Everywhere is annual mean has been displayed, But why seasonal (DJF) changes have been shown over Barrow, Alaska? I think annual mean and seasonal mean change would be different. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Figure has been modified.
117029	225		225		Could the figure report the level of land surface warming for the period of interest, for each contitnent or all land areas to give an element of comparison ? [Valerie Masson-Delmotte, France]	Taken into account. Figure has been modified.
34395	225				Box 10.2, Figure 1. The selection of cities is surprising; there are large cities as Beijing not included. Legend need to be improved. I guess China and Japan are cycles because these data belong to the average of the whole country? In Barrow (Alaska) only data from DJF while all other cities the temperature is the average of the whole year and a single observatory? [Guiomar Rotllant, Spain]	Taken into account. Figure has been modified.
23061	226	1	226	1	CRU TS should be masked for where the estimate is a climatology. See in addition stylistic comments on similar figures given earlier [Peter Thorne, Ireland]	Taken into account: Consistent masking has been done on all Chapter 10 FGD figures, following discussions with data providers.
82725	226	1	226	8	India and Pakistan appear to be masked in the APHRODITE temperature map - why? (the paper suggests that data density is lower there than in some other regions, but not zero). [Blair Trewin, Australia]	Taken into account. An updated version of the APHRO-MA dataset has been used in the FGD and this masking is no longer present.
104515	226	8	226	8	CRUTS? [Sergio Aquino, Canada]	Taken into account. Acronyms for observational datasets have been homogenized in the chapter.
117031	227		227		it is confusing to write "CMIP6" when what is shown is the result of an ensemble with only one model. [Valerie Masson-Delmotte, France]	Taken into account. The figure has been considerably revised in the FGD and the caption worded more carefully.
104513	229	5	229	5	suggest a space between Figure 10.22: Schematic. All graphs should have a space between figure number and title [Sergio Aquino, Canada]	Taken into account. The figure has been recast as part of moving it into Box 10.2 on storylines.
125779	229	5	229	14	In Figure 10.22, couldn't other forcings (such as aerosols) also contribute to regional warming? In the schematic at present, other forcings appear to only drive dynamical conditions, not regional warming. Perhaps change "contribute to" --> "affect". [Trigg Talley, United States of America]	Taken into account. The figure has been recast as part of moving it into Box 10.2 on storylines and implicitly allows for a variety of factors that could contribute to regional warming
87789	229		229		Fig.10.22: This is very similar to Fig.1.11 (Ch1). Coordination with Ch1 is advisable here, to ensure consistency and avoid unnecessary repetition. [Sergio Henrique Faria, Spain]	Noted. The text accompanying this figure refers to Section 1.4.4. The figure here, while similar to that in Chapter 1, serves a different purpose and is part of the added Box 10.2 on storylines.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
104511	230	2	230	2	graph has missing bullet points [Sergio Aquino, Canada]	Noted. As stated in the figure caption, "The subheadings in each category are illustrative and not all-inclusive." In particular, the bullets with " ..." (ellipsis) are there to further make the point that the list is not all-inclusive and that more could be added, if space had allowed it.
85067	230		230		Comment provdided by Stacey New: Figure 10.23 The arrows in this diagram are a little confusing, can the links between this various variables be resprested in clearer way, are the arrows needed at all? [Stacey New, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The key at the bottom of the figure gives the roles of the two sets of arrows They are a necessary part of the figure, showing the interactions required. The arrows show the links needed for constructing well the needed distilled climate information, illustrating that all parties must interact with each other and bring their knowledge and values to the other groups (hence the variable shading along the curved arrows.
113707	231	14	231	14	"(Harris et al., 2014) (violet line)." instead of "(Harris et al., 2014) (green line)." [Agnieszka Kowalczyk, Poland]	Accepted. This is fixed in the updated figure
87793	231		231		Fig.10.24d: What is the scale of the x-axis? [Sergio Henrique Faria, Spain]	Taken into account. The x-axis has no scale. As described in the figure caption, "GCMs in each CMIP group ordered according to the magnitude of trend in rainfall, and the same order is maintained in panels showing trends in the SAM."
125781	232	1			[ENSEMBLES] Different numbers of CMIP6 models are used to generate regional projections across the chapter (e.g., Fig. 10.25 and 10.26); however, no clear justification is provided for the varied number of models. A clear justification for the ensemble of models used must be provided. [Trigg Talley, United States of America]	Taken into account. Where possible, consistent numbers of CMIP models have been used across the chapter in producing the FGD figures.
88871	232	6	232	7	The figure panel has 0.05 \degree_symbol instead of 0.5 degree which is in the caption. [Krishna AchutaRao, India]	Rejected. This distinction is correct and is part of the methodology used by Yatagai in constructing the APHRODITE dataset (see e.g. DOI:10.2151/sola.2009-035). Prior to making the 0.5 degree (or the alternative 0.25 degree) gridded Monsoon Asia product, Yatagai and team first interpolate their rain gauge data onto a much finer 0.05-degree grid. This variable thus indicates how many (in %) of the one hundred 0.05-degree grid points within a single 0.5-degree grid box contain a valid gauge measurement. We have made this more clear in the figure caption for the FGD.
113709	232	13	232	13	"grey line) and greenhouse gas-only (hist-GHG, 9 models, blue line)." instead of "blue line) and greenhouse gas-only (hist-GHG, 9 models, grey line)." [Agnieszka Kowalczyk, Poland]	Accepted. The order of colours mentioned in the figure caption has been corrected for the FGD.
23063	234	1	234	1	This figure is very very busy and the panels are very very small with text often impossible to read. Please avoid labelling Berkeley Earth as BEST. Please ensure that CRUTS is masked to only observationally constrained values. What is NCDC in panel c? NCDC no longer exists as an entity. [Peter Thorne, Ireland]	Taken into account. Fig. 10.26 has been split into Fig. 10.20 for historical and 10.21 for projections. BEST is not longer used. NCDC is replaced by NOAA global temp. Missing data is marked by grey. Plots have been revisited for better reading.
125783	234	1			[ENSEMBLES] Different numbers of CMIP6 models are used to generate regional projections across the chapter (e.g., Fig. 10.25 and 10.26); however, no clear justification is provided for the varied number of models. A clear justification for the ensemble of models used must be provided. [Trigg Talley, United States of America]	Taken into account. Fig. 10.26 has been split into Fig. 10.20 for historical and 10.21 for projections. We have used the total number of models available for every ensemble. For each plot the ensemble number has been added.
7969	234	4	234	4	Are these plots prepared by the Interactive Atlas? Make sure the temperature scale in panel d and h are similar [Bart van den Hurk, Netherlands]	Taken into account. Fig. 10.26 has been split into Fig. 10.20 for historical and 10.21 for projections. The plots have not been prepared by the interactive Atlas, but the data sources are the same. The temperature scale for the panels for historical and future are not the same, because of the different temperature ranges and we want to highlight the differences between the model ensembles, which can only be accomplished by different temperature scales.
59371	234		234		Subtext within several subfigures is small and blurry. This will need to be increased in size to make the text legible. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Fig. 10.26 has been split into Fig. 10.20 for historical and 10.21 for projections. Plots have been revisited for better reading.
87795	234		234		Fig.10.26: That is a crowded figure! It is very interesting, but there is too much information squeezed into a space too small for it. It needs some rearrangement, to make the individual items larger and easier to read. [Sergio Henrique Faria, Spain]	Taken into account. Fig. 10.26 has been split into Fig. 10.20 for historical and 10.21 for projections. Plots have been revisited for better reading.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17673	234				Fig10.26 : add at least CORDEX EUR-11 on panel (g) and (h) and perhaps more CORDEX runs [SAMUEL SOMOT, France]	Accepted. Fig. 10.26 has been split into Fig. 10.20 for historical and 10.21 for projections. Eur-11 has been included and more CORDEX runs. Number of runs is indicated in the panels.
17675	234				Fig10.26 : consider how to include Med-CORDEX ensemble in this figure [SAMUEL SOMOT, France]	Rejected. Fig. 10.26 has been split into Fig. 10.20 for historical and 10.21 for projections. With CORDEX-44 and CORDEX-11 we sufficiently cover the Mediterranean. Due to space limitation and insufficient added value Med-CORDEX is not included.
17677	234				Fig10.26 : add CORDEX on panel (e) and (f). [SAMUEL SOMOT, France]	Accepted. Fig. 10.26 has been split into Fig. 10.20 for historical and 10.21 for projections. CORDEX has been added to 10.20e and10.20f and to 10.21a and 10.21b
17679	234				Fig10.26 : CORDEX evaluation runs are very relevant to assess the RCM performance in reproducing past trends conditionnally to the observed large-scale (given by ERA-Int). Evaluation runs start only in 1979 but they could bring interesting information concerning panel e, f, g. (see for example Nabat et al. 2014, doi:10.1002/2014GL060798) [SAMUEL SOMOT, France]	Taken into account. Fig. 10.26 has been split into Fig. 10.20 for historical and 10.21 for projections. CORDEX simulations have now fully been included in the evaluation.
17681	234				Fig10.26 : on panel (g) add « historical-RCP85 » in the title of the CORDEX panels as for CMIP5. Add also the relevant indication for HighResMIP. [SAMUEL SOMOT, France]	Taken into account. Fig. 10.26 has been split into Fig. 10.20 for historical and 10.21 for projections. Panels 20.g and 20c are now harmonized with indication of period, experiment, and number simulations.
17683	234				Fig10.26 : I would apreciate to have somewhere in the figure an estimate of the plausible range of the past trends with the numbers written. They are likely in the text somewhere but adding them on the figure will make it more self-consistent. For example on panel © or (f) [SAMUEL SOMOT, France]	Taken into account. Fig. 10.26 has been split into Fig. 10.20 for historical and 10.21 for projections. Plausible ranges of past trends have been indicated by shading and whiskers.
17685	234				Fig10.26(a) : I m surprised to see no mention of a dynamical stability effect that limits convection-precipitation-cloud cover and therefore likely enhances the warming. Decrease in cloud cover in particular is likely as important as decrease in dimming effect. In addition, even if I agree that the land-sea contrast is a key player for the enhanced Mediterranean warming, I'm not sure that it does translate well all the results obtained concerning the lapse rate effect by Kröner et al. (2016, DOI 10.1007/s00382-016-3276-3) and Brogli et al. (2019 https://doi.org/10.1175/JCLID- 18-0431.s1) [SAMUEL SOMOT, France]	Noted. The cloud-feedback has been discussed in the text. The figure represents the most important feedbacks and mechanisms.
17687	234				Fig 10.26 : I don't understand why did you stop the panel (h) in 2050 ? An illustration of the enhanced Med warming at the end of the 21st century is likely relevant too. [SAMUEL SOMOT, France]	Noted. Fig. 10.26 has been split into Fig. 10.20 for historical and 10.21 for projections. Panel c of Fig. 21 stops in 2050, because for the pattern comparison we want to include HighResMIP for which the simulations end in 2050. The area mean warming up to 2100 is shown in Fig. 21.a
17689	234				Fig 10.26 : In this figure I miss a characterization of the « enhanced » that is to say a panel that allows to compare the Med summer warming to the global warming (or to the global warming over land) in order to illustrate the « enhanced » wrt something. [SAMUEL SOMOT, France]	Taken into account. Fig. 10.26 has been split into Fig. 10.20 for historical and 10.21 for projections. In Fig. 10.21 panel de has been added that compares the Mediterranean warming with the global mean warming.
17691	234				Fig10.26(g) : Model biases for the past trend are so strong that I'm wondering if it is not more relevant to plot the model trend maps insted of the model trend biases. An indication by hatched areas where this trend is significantly different from BEST could be added. [SAMUEL SOMOT, France]	Rejected. Fig. 10.26 has been split into Fig. 10.20 for historical and 10.21 for projections. For Fig. 20.g we keep the trend biases, because we argue that it is important to show the model biases for the past trend.
17693	234				Fig 10.26dh (and perhaps 10.26g, see my previous comment). If possible I would have kept the same color palette for those panels in order to be able to compare past and future trends. A trade-off between the color palette of 10.26d and 10.26h seems feasible. Another option is to have a double color palette for 10.26h keeping the same as 10.26d for the lower values and using a purple palette for the higher range. [SAMUEL SOMOT, France]	Taken into account. Fig. 10.26 has been split into Fig. 10.20 for historical and 10.21 for projections. We have tried different options for the colour palette. Because past trends are smaller than future trends and we want to highlight the differences between the models, we argue that the chosen colour palette options are the optimal.
17695	234				Fig 10.26 : I don't understand why MPI-GE is plotted in panel e, f but not anymore in g and h. I know that we have a lot of ensembles to look at now but, after choosing the most relevant ensembles, consistency throughtout the figure is advisable I guess. Mixing HighResMIP and CMIP6 or HighResMIP and CORDEX-44 are options to reduce the number of ensembles to consider. [SAMUEL SOMOT, France]	Taken into account. Fig. 10.26 has been split into Fig. 10.20 for historical and 10.21 for projections. Consistent use SMILES such as MPI-GE has been made for historical and future plots. We have kept Mixing HighResMIP and CMIP6 or HighResMIP and CORDEX-44 to illustrate differences between those ensembles, as this is one of the key points of this figure to show a range of different sources.
40181	235	0			Fig FAQ10.1: very nice figure! [TSU WGI, France]	Noted. Thanks for the positive comment. The figure benefited greatly from the work of the TSU graphic artist

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
85069	235		235		Comment provided by Jennifer Weeks: FAQ 10.1, Figure 1 The distillation circle itself is relatively large compared to the text and arrows at the top and bottom going into it. This makes the important text harder to read, especially since the text itself it very small. Could this distillation circle be made smaller and the information text and arrows around it larger, perhaps by restructuring the arrows so that they are staggered? This diagram would be really useful for climate service scientists, e.g. in presentations, so it would be good to have a diagram that is really clear and easy to read. [Stacey New, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The FAQ10.1 figure has been revised with the graphic artist to be clearer on important details.
81249	235		235		FAQ10.1 is very aesthetic but requires more work to make it more informative, in particular on the top part: classification of the different components and links between them [Fatima Driouech, Morocco]	Noted. It is not clear what the reviewer is requesting. The figure and caption have been revised in consultation with the graphic artist.
59373	235		235		Theme text within the figure is small and blurry. This will need to be increased in size to make the text legible. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The figure has been developed with a TSU graphic artist and has been further revised with the artist to improve clarity.
87797	235		235		Fig. FAQ 10.1: Very interesting figure! My only comment is that the colour of the text "must we consider" is too light and difficult to read. A darker shade would be better. [Sergio Henrique Faria, Spain]	Taken into account. The FAQ10.1 figure has been revised with the graphic artist to be clearer on important details.
40183	236	0			Fig FAQ10.2: Nice figure but could be linked better to the text, maybe emphasizing more which cities will be most impacted and why? [TSU WGI, France]	Taken into account. Figure has been revised.
18983	Fig10.1				Chapter 11 also includes a lot of press understanding, all proceses related to extremes. [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The new figure 10.4 now collects some of the links across chapters in terms of climate information.
29473	Pag 33	Line 18	Pag 33	Line 18	sec 10.2.4 Outlook for improving observational data for regional climates. [Mercedes Andrade, Mexico]	Accepted. Text has been revised accordingly
29475	Pag 33	Line 18	Pag 33	Line 18	Line 18... Other source of the data grid, is CRU (Harris et al. 2014) , this database have a good spatial resolution for studies in those regions where the observational information lacks (Andrade-Velázquez, M. 2020, Andrade-Velázquez & Medrano-Pérez in press). [Mercedes Andrade, Mexico]	Rejected. However, this reference is already mentioned in section 2.1.2.
29477	Pag 33	Line 18	Pag 33	Line 18	Harris I, Jones P, Osborn T and Lister D. 2014. Updated high-resolution grids of monthly climatic observations the cru ts3.10 dataset. International Journal of Climatology, 34 (3), 623642. https://doi.org/10.1002/joc.3711 [Mercedes Andrade, Mexico]	Noted
29479	Pag 33	Line 18	Pag 33	Line 18	Andrade-Velázquez, M. (2020). To face to Climate Variability: Risk and vulnerability in the coast zones. Chapter in Book: Coasts and Seas Gobernance and Management front the Uncertainity. Coord. Rivera-Arriaga, E. A and coords. RICOMAR-EPOMEX. Pag. 155-169. [Mercedes Andrade, Mexico]	Rejected. This reference do not fit with the purpose of the subsection.
29481	Pag 33	Line 18	Pag 33	Line 18	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. This reference do not fit with the purpose of the subsection.
29483	Pag.38,	line 44.	Pag.38	line 44.	Pag.38, sec. 10.3.1.4.1. Perfect prognosis. line 44. and pag 43. sec 10.3.3.1 Evaluation diagnostics. Line 20: Other similar methods have been propose for regional studies (Giorgi and Mearns 2002; Colorado et al. 2018, Andrade-Velázquez & Montero-Martínez, 2019) for model evaluation and climate data post-processing improvement in tropical areas. [Mercedes Andrade, Mexico]	Rejected. The RAE approach used in these publications is not a downscaling method, but a model weighting approach, taking into account certain reliability criteria.
29485	Pag.38,	line 44.	Pag.38	line 44.	Giorgi F and Mearns LO. 2002. Calculation of average, uncertainty range and reliability of regional climate changes from AOGCM simulations via the Reliability Ensemble Averaging (REA) method. J. Clim. 15:11411158. [Mercedes Andrade, Mexico]	Rejected. The RAE approach used in these publications is not a downscaling method, but a model weighting approach, taking into account certain reliability criteria.
29487	Pag.38,	line 44.	Pag.38	line 44.	Colorado-Ruiz G, Cavazos T, Salinas JA, De Grau P and Ayala R. 2018. Climate change projections from Coupled Model Intercomparison Project phase 5 multi-model weighted ensembles for Mexico, the North American monsoon, and the mid-summer drought region. Int. J. Climatol. 38(15): 5699-5716. [Mercedes Andrade, Mexico]	Rejected. The RAE approach used in these publications is not a downscaling method, but a model weighting approach, taking into account certain reliability criteria.
29489	Pag.38,	line 44.	Pag.38	line 44.	Andrade-Velázquez M and Montero-Martínez MJ. 2019. Fiabilidad de los modelos del CMIP5 para la cuenca del río Usumacinta bajo el método REA. DIGITAL CIENCIA@UAQRO, 12(1), 14-21. [Mercedes Andrade, Mexico]	Rejected. The RAE approach used in these publications is not a downscaling method, but a model weighting approach, taking into account certain reliability criteria.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29491	Pag.70.	Line16.	Pag.70.	Line16.	Sec. 10.4.1.1 Methodologies for regional climate change attribution. Line16. At tropical areas at NAM there are not significative precipitation changes (Aguilar et al. 2005, Montero-Martínez et al. 2018). [Mercedes Andrade, Mexico]	Rejected. These two papers are not relevant here as they simply estimate linear trends on observed data with no attempt to attribute changes to causal drivers
29493	Pag.70.	Line16.	Pag.70.	Line16.	Aguilar E, Peterson TC, Ramírez-Obando P, Frutos R, Retana JA, Solera M, Soley J, González-García I, Araujo RM, Rosa-Santos A, Valle VE, Brunet M, Aguilar L, Alvarez L, Bautista M, Castañón C, Herrera L, Ruano E, Sinay JJ, Sánchez E, Hernández-Oviedo GI, Obed F, Salgado JE, Vázquez JL, Baca M, Gutiérrez M, Centella C, Espinosa J, Martínez D, Olmedo B, Ojeda-Espinoza CE, Núñez R, Haylock M, Benavides H and Mayorga R. 2005. Changes in precipitation and temperature extremes in central america and northern south america, 19612003. Journal of Geophysical Research: Atmospheres, 110 (D23). [Mercedes Andrade, Mexico]	Noted. See also answer to comment 29491
29495	Pag.70.	Line16.	Pag.70.	Line16.	Montero-Martíinez 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]	Noted. See also answer to comment 29491
38131			56	43	Please add the following phrase - "Some examples are the recent enhanced warming over Europe (Nabat et al., 2014; Dong et al., 2017), the cooling over the East Asia monsoon region (Shim et al., 2019), leading to a weakening of the monsoon (Song et al., 2014; Wang et al., 2017c), as well as the observed monsoon precipitation in West Africa and South Asia (Under et al., 2018)". [Junhee Lee, Republic of Korea]	Not applicable. The East Asia Monsoon example has been removed for the FGD. The number of examples has been strongly reduced in section 4 due to space constraints (only three examples are kept).
38133			70	9	Please add the following reference - Min, 2020 (related "human fingerprint method") [Junhee Lee, Republic of Korea]	Rejected. This is a News and Views article which is not peer reviewed.
38135			70	16	Please add the following reference - Paik et al., 2020 (related "preipitation changes due to human influence") [Junhee Lee, Republic of Korea]	Noted. This paper is about the attribution of changes in extremes and is relevant for chapter 11.
38137			92		Suggestion for the inclusion of urban climate research related to mega-city (Box 10.2). [Junhee Lee, Republic of Korea]	Noted. The urban climate box is discussing urban climate research done partly in megacities but a cross working group box on megacities is in preparation.
38139			204		In Figure 10.3, Blue box of East Asia region just include western china. But, analysis contents and reference show larger area. Request to modify the figure that East Asia region includes both Korea, China and Japan. [Junhee Lee, Republic of Korea]	Not applicable. The figure has been removed.
87791			230		Fig.10.23: It is a very nice diagram, but I don't get why introducing the names "P, U, and R" if the explanation is also given inside the circles. It would be better to leave just three large "P, U, and R" inside the circles and put all explanations at the side, in a legend that would include also the explanation of the arrows (currently at the bottom). This would make the Venn diagram simpler, the explanations easier to read, and the page space would be better used (currently there is a lot of empty space at the sides of the diagram). Finally, I hope the colours are just temporary: there are much more attractive combinations of colours available. [Sergio Henrique Faria, Spain]	Taken into account. A new version of the figure has been made for the FGD. , U, R have been removed as indicators. Categories are now more clearly explained.
53537					There is a kind of hiatus between CH10 on the one hand and CH11 and 12 on the other hands. CH10 nicely assesses the complexity of delivering reliable regional climate information and the multiple related methodological issues, but such issues are not so much emphasized in the subsequent chapters (as far as I can see but I did not review these chapters) as if raw outputs from global climate models were enough to provide accurate regional climate change information about extremes and hazards. May be CH10 should check that its key messages are considered and further supported by CH11 and 12. [Hervé Douville, France]	Accepted. Chapter 10 has created closer links to the other regional chapters in section 10.1, with a new figure to describe the flow of thought. Chapter 10 has raised this issue to the other regional chapters and some integration has taken place, as Cross-Chapter Box 10.3 now illustrates.
117027					Congratulations for impressive work on figures [Valerie Masson-Delmotte, France]	Noted with thanks
117033					Chapter 10 is already significantly too long (by around 11%). Please shorten and sharpen the assessment; you can also use tables to avoid descriptiive parts. [Valerie Masson-Delmotte, France]	Accepted. The chapter has been substantially shorter for the FGD.
34385					General. Typographic, semantic, abbreviation, unit format and references errors have not been checked. [Guiomar Rotllant, Spain]	Editorial – copyedit to be completed prior to publication
41041					There are no supporing sentences for headlines in several bullets in ES, e.g., Page 8 Line 29-50, Page 9 Line 3-11. [TSU WGI, France]	Accepted. All ES bold headline statements now have supporting non-bold sentences.
81241					Thank you to the authors for the work done [Fatima Driouech, Morocco]	Noted with thanks
93529					Figure 10.6 could present also results in other parts of the domain shown [Omar Chafki, Morocco]	Not applicable. Figure 6 has been deleted for space reasons.
125785					There are many unnecessary hyphens throughout the text, such as between soil moisture and climate change. Recommend removing them. [Trigg Talley, United States of America]	Editorial – copyedit to be completed prior to publication

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
81243					More assessment in regional processes is still needed to ensure the chapter linking the global to regional satisfactorily [Fatima Driouech, Morocco]	Noted. Because of the broad scope of the Chapter and the limited space available, it is not possible to assess "more regional processes" without further specification from the reviewer.
125787					[ACCESSIBILITY] There is a serious need to reduce the information content of this chapter and distill the chapter into succinct, clear messages. [Trigg Talley, United States of America]	Taken into account. The information content has been reduced in Sections 3, 4 and 6. Sections 1 and 5 have been reorganized. However it is our mandate to assess all the issues that arise in producing climate information for regions.
125789					Be careful with the many references to the Mediterranean region. This is a global report. This happens frequently across the chapter. [Trigg Talley, United States of America]	Noted. The methodology assessment is illustrated using a handful of regions. The chapter is not trying to be exhaustive. This is left to the other regional chapters. The Mediterranean region is used to illustrate the variety of aspects that contribute to the formulation of climate information, trying to build an example that touches at as many sources as possible. This is made clearer in section 10.1.
40031					Consider adding assessments regarding the influence of urbanization on climate change should be included in SPM. [TSU WGI, France]	Accepted. Assessment on urban climate change has been added to the SPM
125791					This chapter needs to acknowledge that regional model evaluation is more than just precipitation. Other metrics are useful. [Trigg Talley, United States of America]	Noted. The chapter assesses a range of different variables and processes well beyond precipitation as is evident from the long list of regional phenomena assessed in Section 3.3 as well as in the examples given in Sections 4 and 6.
125793					[ACCESSIBILITY] There seems to be no theme or throughline for the chapter. Some sections have it where others seems to have lost it and are just collections of information that the author of the section is familiar with whether that be Europe, precipitation, or otherwise. The common themes should be the importance and generation of regional climate messages. Each section needs to contribute to this goal and if it does not provide useful information towards this end it should be removed. A collection of facts and journal paper references is not useful information. [Trigg Talley, United States of America]	Taken into account. Section 10.1 describes now a clearer thread through the different sections, which work towards a common goal that is the production of regional climate information from a number of sources taking into account the context and values of those involved.
125795					[RISK] Two high-profile regional hazards with climate-change signals that are barely mentioned or not at all are sea-level rise and coastal flooding and tornadoes. These topics are discussed in other chapters, but should at least be cross-referenced here. [Trigg Talley, United States of America]	Rejected. Hazards are treated in detail in chapter 12 while chapter 10 focuses more on methodological issues relevant to the distillation of climate information. Furthermore, the mentioned hazards are not directly simulated by climate models, especially tornadoes.
109421					FAQ 10.2 : shouldn't the air pollution appear on this figure? [Sophie Szopa, France]	Noted. For the FGD we have agreed with TSU that this FAQ will only treat the interaction between heat waves and UHI.
91001					Information and assessment of regional processes are still required [Omar Chafki, Morocco]	Noted. Because of the broad scope of the Chapter and the limited space available, it is not possible to assess more "regional processes" without further specification from the reviewer.
29327					very good work. [Zangari del Balzo Gianluigi, Italy]	Noted with thanks
40851					Sub-sections in this chapter should be better organised. Some paragraphs are very short and less informative, e.g. section 10.1.4.1.6 and 10.2.2.5. [TSU WGI, France]	Taken into account. We have reduced the fourth order as much as possible, and in the FGD it is only employed extraordinarily in Section 3. We also avoid very short sections.
35229					dsdsd [SAMUEL SOMOT, France]	Noted, this seems to be a typing error when introducing the comment
112039					The regions used in Ch10 (shown in Figure 10.3) could be included in the Interactive Atlas (e.g. Ch10 regions) as an additional category (besides the IPCC-WGI reference regions and other typological/special regions sucha as moonsoons, etc.). That would be useful if the Interactive Atlas (IA) allows to futher explore some of the information which is used in the examples, so some coordination would be required here. The IA includes CMIP5/6 and CORDEX so in principle there would be good to explore this particularly for the examples of constructing regional climate messages in 10.6. [jose manuel gutierrez, Spain]	Noted. We have exchange all the necessary information with the Atlas CLAs. However, the IA does not in principle include all the large ensembles and HighResMIP data used in Chapter 10 to allow for a full comparison with the sources used in section 10.6.
112057					There are some overlaps with Chapter 12, page 13, lines 38-53. They refer there to standard bias-adjusment methods (which overlap with Ch10 material) and to more specific applications to adjust climatic indices (including threshold-dependent ones). It would be good to coordinate this and keep the main description in Ch10 and futher comment on applications to relevant indices to Ch12 here. A similar comment has been submitted for Ch12. [jose manuel gutierrez, Spain]	Accepted. This overlap has been resolved. Chapter 13 material has been moved to Chapter 10, a reference has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115921					FAQ10.1 Please also consider biological diversity (ecosystems); define what "actionable" mean in the WGI context (here, glossary). The issue of the relevance of extending recent trends into the future needs careful attention here and in the main text (explain the underlying rationale). I would suggest to think carefully if the last paragraph (page 10- 130, lines 45-53) is fully needed ("likely controversial" etc). Altogether, it reads more like framing than like a FAQ. For the visual representation, what about infrastructures or tourism? What about references to regional climate impact drivers in the distillation process? [Valerie Masson-Delmotte, France]	Taken into account. The FAQ has been substantially rewritten in collaboration with TSU, taking into account these remarks.
79315					There are a number of acronyms across the chapter that they are not initially defined r.g. AMV, MERRA, UDEL etc. [Prodromos Zanis, Greece]	Accepted. For the FGD the acronyms has been defined with guidance from the TSU.
115923					FAQ 10.2 I have the impression that the answer is too restrictive. Interplays also include air quality and SLCF, downstream effects, effects on runoff, correction of urban heat effects to estimate global temperature change, and it does not refer to possibilities to limit the urban heat island effect or runoff effects (city design, greening cities etc). I would suggest to have authors of WGII and WGIII also have a look at the FAQ so that it is designed to facilitate integration with the other WG too. I am not sure that the last sentence correctly reflects the message of authors and previous points. Figure : what about effects of air conditioning (for thermal aspects) too? Other aspects (eg downstream effects of cities, SLCF and air quality are not mentioned). [Valerie Masson-Delmotte, France]	Not applicable. The FAQ has been rescoped in collaboration with the TSU and does not only treat the urban heat island.
87769					Congratulations to all CLAs, LAs, CSs and REs of Ch10 for the amazing improvement in length, clarity, and readability of Ch10 SOD, compared to the FOD version. It is much more pleasant to read it now. Well done! [Sergio Henrique Faria, Spain]	Noted with thanks