Comment ID	From Page	From Line	To Page	To Line	Comment	Response
131547	0	0	0	0	Coherent spelling of South East Asia throughout the chapter [Hans Poertner and WGII TSU, Germany]	Thank you. Southeast Asia is used throughout the chapter.
33071	0		216		west asia and central asia is facing with aridity and drought, the role of water also in arid and semi- arid areas with refrence to the mentioned sub - regions needs to be highlighted [Sahar Tajbakhsh Mosalman, Iran]	Noted. Observed and predicted changes in drought and aridity are discussed in Sections 8.3 and 8.4. Specific regional information may be found in Chapter 12.
32741	0		216		west asia and central asia is facing with aridity and drought, the role of water also in arid and semi- arid areas with refrence to the mentioned sub - regions needs to be highlighted [sadegh zeyaeyan, Iran]	Noted. Observed and predicted changes in drought and aridity are discussed in Sections 8.3 and 8.4. Specific regional information may be found in Chapter 12.
19447	0		216		West Asia and Central Asia is facing with aridity and drought, the role of water also in arid and semi-arid areas with reference to the mentioned sub-regions needs to be highlighted. [Mostafa Jafari, Iran]	Noted. Observed and predicted changes in drought and aridity are discussed in Sections 8.3 and 8.4. Specific regional information may be found in Chapter 12.
13569	0				The use of simple thermodynamic models is very useful to increase knowledge of the processes associated with the water cycle, especially on small scales, however, to improve the projections associated with climate change, it is more realistic to apply dynamic downscaling using the best models for the region of interest. This can be done both through the international initiative CORDEX (Coordinated Regional Climate Downscaling Experiment), as well as national or multinational programs and projects in a coordinated way. The authors used an analysis of projected changes in zonal means precipitation, that is plausible, however, these eliminate the longitudinal variability, very important aspect in the tropical zones, particularly where the seasonal contrasts between wet and dry regimes with thermal gradients are remarkable, such as monsoon circulations associated with seasonal changes in these thermal contrasts, especially between the ocean and the continent. Summarizing, it is suggested to promote dynamic downscaling experiments, both through CORDEX and national or multinational initiatives by forcing regional updated models with global simulations of the CMIP6 experiment using the models with the best performance for the region of interest." [Maria Amparo Martinez Arroyo, Mexico]	Noted - The increasing use of RCMs has been briefly addressed in Section 8.5.1.2.2 but is mostly assessed in Ch10. Note however that RCM simulations are sometimes surprisingly less rigorous than GCM simulations in terms of radiative (e.g., anthropogenic aerosol) forcings so that they may not be more reliable than GCMs for projections (e.g., https://doi.org/10.1007/s00382-020-05153-1)
22281	0				The placement of box 8.1 feels very odd. Also, surely this should be a cross-chapter box with significant chapter 6 and possibly 7 involvement? [Peter Thorne, Ireland]	Noted. The proposal for Box 8.1 to become a cross chapter box was raised and turned down by the other chapter CLAs.
42761	0				While scale issues are mentioned in almost every page of the report, with a particular statement in Page 22, line 29 (Processes operating at local scales are capable of substantially modifying the regional water cycle), there is no specific discussion on the mismatch between atmospheric scales and continental water scales that are impelling in many research areas (whether dealing with models, observations, impacts). Yet this is a major issue from both a research and a policy perspectives: too often meteorologists that want to play with continental water or hydrologists that want to play with climate scenarios, just see this issue as a matter of resolution and downscaling. Climate model provide rainfall at a, say, 100-200 km resolution. Problem: this is too coarse to be used in a Hydrological model. Solution: downscale to 1-10 km either through RCMs or statistical algorithms. In doing so it is often forgotten that (among other things): i) the initial large scale rainfall forcing is already largely biaised ; ii) downscaling will not guarantee that small scale variability (absent from the initial large scale forcing) will be properly reconstituted; iii) time intermittence is fairly different whether grasped et a 100x100 km² scale or at a 1x1 km² scale. It would be nice to provide some perspective on this key scale issue in the introduction as well as in Key knowledge gaps section (8.7). [Thierry Lebel, France]	Rejected - This topic is briefly discussed in Section 8.5.1.1.3 but is mostly a downscaling issue that is the topic of Ch10 and an issue for hydrological impact studies which is the topic of WG2
67867	0				This chapter is the development of AR5 based on the post-AR5 Special Report, along with new challenges and opportunities that follow the development of science. In general this chapter is balanced and comprehensive, incorporating the latest scientific findings. [Ruandha Agung Sugardiman, Indonesia]	Noted - Thanks

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67869	0				Consistency between summary and descriptions in the chapter. [Ruandha Agung Sugardiman, Indonesia]	Noted - Thanks
22327	0				There is substantive overlap between the assessment carried out in much of 8.3.2 and the assessments in chapters 2-3-4. This is formally the domain of these earlier chapters as scoped. To the degree chapter 8 is to consider this issue it should be linked to the hydrological cycle aspects and each section should explicitly start by recounting the relevant findings from these antecedent chapters upon which it should build by providing both regional detail and a hydrological cycle view. Presently there is redundancy and a real risk of inviting readers to sspot the difference. Significant coordination work is required to avoid this. Only section 8.3.2.4 currently handles this well but even there recapituation of the key findings would be useful to the reader rather than simply pointing to the sections. [Peter Thorne, Ireland]	Thank you. In the FGD, we have addressed the issue of overlaps in 8.3.2 with chapters 2-3-4. As suggested, we have focused on the hydrological cycle aspects in 8.3.2 and provided links to other chapters. The hydrological cycle view and regional details are built in 8.3.2 after providing relevant links from the findings of the previous chapters. Thanks for the feedback on 8.3.2.4. The FGD includes a recapitulation of the key assessments for all the sections.
22367	0				There is huge overlap between the circulation assessments in several parts of 8.3.2 (but not all) and the assessments already performed in chapters 2 and 3. Worse still these prior assessments are not acknowledged and do not form the starting point for the assessments undertaken. This must be addressed. It is hugely unwise to be performing redundant assessments, Chapter 8 should start from the assessment results reported in chapters 2 and 3 and add any necessary regional detail / hydrological process detail. Repetition of prior assessments in other chapters should be absolutely avoided. [Peter Thorne, Ireland]	Thank you very much. Taken into account. In the FGD we have addressed the issue of overlaps in 8.3.2 with chapters 2-3-4. The assessments in these chapters are acknowledged and the focus in 8.3.2 is on hydrological cycle aspects.
132205	0				It does not seem suitable for chapter 8 to have an FAQ (FAQ 8,3) focused on drought given that chapters 8 and 11 agreed that the main assessment for drought would be in chapter 11 (and the main assessment on aridity in chapter 8). Maybe this FAQ could be on "What processes lead to increased aridity and how is this related to droughs?" or "What are projected changes in land water availability and why?". Note that I believe that readers will be just as interested in land water availabillity as in droughts. [Sonia Seneviratne, Switzerland]	Taken into account. For the final draft, FAQ 8.3 has been reviewed and agreed upon by both Chapters 8 and 11
132215	0				I would strongly recommend the following topic as a possible FAQ in chapter 8: "Is the water cycle intensifying?". To me the answer is "no", although this term if often used in the literature. I believe it would be an interesting FAQ. [Sonia Seneviratne, Switzerland]	Rejected. Nice idea but we do not have the space for another FAQ.
132221	0				Please add a statement in the ES on the concept of "global water cycle intensification". I would argue that this concept is ill-defined and not an appropriate description of the water cycle changes. It would be very valuable if the chapter 8 would address this question (a topic that could also be elevated to the TS and SPM). [Sonia Seneviratne, Switzerland]	Rejected. Intensity is mentioned in several places in the ES, but we have not inserted a stand-alone statement on water cycle intensification.
132227	0				There is a need for chapter 8 to harmonize its assessments on water cycle extremes (heavy precipitation, droughts, floods) with chapter 11 (and 12 for floods). [Sonia Seneviratne, Switzerland]	Taken into account. All drought/aridity assessments have been cross-checked with Chapter 11 for the final draft.
22407	0				My feeling is that 8.3 and 8.4 would be better if integrated together than kept apart. By the time as a reader I return to a topic so much has occurred in the interim that I find myself having to flip back to section 8.3 to see what it said. Similar structure only helps so far in this regard and it would likely be more accessible to consider observations, attribution and future projections together per aspect of the hydrological cycle than seperated as is presently the case. [Peter Thorne, Ireland]	Rejected - The chapter outline was agreed at LAM1 and has not been changed.
132233	0				Chapter 11 was not consulted on the FAQs 8.2 and 8.3 which address changes in extremes (droughts, floods). [Sonia Seneviratne, Switzerland]	Taken into account. All drought/aridity assessments have been cross-checked with Chapter 11 for the final draft.
22439	0				There are very frequent cases where an acronym is defined only for then several variants of the acronym to be used. This is particularly prevalent in the monsoons sections but a general issue. For example, in section 8.4.2.2.4 the North American Monsoon System is defined as (NamerM) and then referrred to within 12 lines as: NAMS; NAmerM, NAM, NAMS and then NAmerM again. [Peter Thorne, Ireland]	This issue is addressed in the FGD. The acronym for the North American Monsoon (NAmerM) is distinct from the acronym for Northern Annular Mode (NAM).
28597	0				The report is well written and structured. [Moctar Dembélé, Switzerland]	Noted - Thanks

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22463	0				Care is needed over nomenclature on emissions scenarios. In particular low-mitigation and high-mitigation are terms not used elsewhere in the first seven chapters. Rather they talk about low and high emissions scenarios. These are diametrically opposite but could easily be confused by the lay reader. Chapter 8 should follow the lead of chapter 4 when it comes to categorising scenarios. [Peter Thorne, Ireland]	Noted - Thanks
32209	0				Relatively comprehensive Chapter but with some illustrations that could be improved or more closely linked to the key messages. Section 8.3 often overlooks the difficulties of models in simulating the quantities or phenomena studied, whereas the fit-for-purpose of the models must be assessed chapter by chapter. GMMIP (extended AMIP) simulations are not more analyzed than in Chapter 3. Section 8.4 on projections (the most expected by the readers we suppose) is far from being exhaustive and is perfectible. The link with the illustrations and some figures in Chapter 4 and the summary of the key messages are to be improved. Walker's circulation (although important for the tropical water cycle and its variability) needs to be better assessed in both Section 8.3 and 8.4. The issue of low-probability high-impact scenarios is hardly addressed in Section 8.4 and is essentially reduced to the issue of abrupt changes (Section 8.6), which seems reductive. [Eric Brun, France]	Thank you. The FGD includes discussions of difficulties in model simulations to capture observed phenomena and changes (e.g., ITCZ and rain belts, regional monsoon precipitation, Walker circulation, …). There is a subsection on fit-of-purpose of models in Section 8.5. The future projections Section 8.4 has been expanded substantially in the FGD. Walker circulation changes and links to water cycle variability are better assessed in Sections 8.3 and 8.4 in the FGD. Low likelihood and high impact scenarios are discussed in Section 8.6 and also in Section 8.4 (e.g., likelihood of extreme droughts).
13553	0				A review of section 8.4 in Chapter 8 of the IPCC Report: What are the projected water cycle changes? was carried out, analyzing the information and knowledge used to evaluate the projected water cycle changes and its global and regional impacts at different temporal and spatial scales, which are associated with different climate change scenarios. In this Report the updated information available was used, both from observations and numerical models, and was based also in the state of the art. [Maria Amparo Martinez Arroyo, Mexico]	Noted with thanks.
13555	0				The Report was oriented to dynamical and thermodynamical processes in the climate system from regional to global scales, analyzing different components of the water cycle and their effects on precipitation, evaporation and surface temperature associated with the projected changes, including extremes values. This approach is extremely useful to understand processes that occur in the climate system, however, to include atmosphere and ocean dynamics at different scales, analysis of both regional and global processes and its impacts on precipitation and surface temperature are required, unfortunately, these processes are not adequately reproduced in the CMIP6 models due to its low resolution and there are still no dynamic downscaling results using this set of models. [Maria Amparo Martinez Arroyo, Mexico]	Noted - The resolution issue is briefly discussed in Section 8.5.1.2 but is mostly assessed in Ch10.
13557	0				Because of its importance, the relationship between multi-scale processes should be included in the analysis, such as tropical and extra tropical cyclones, ITCZ intensity and migration, as well as extratropical and tropical modes such as El Niño (ENSO), Madden-Julian Oscillation (MJO), North Atlantic Oscillation, (NAO) including its impact on global circulation and therefore on the water cycle. [Maria Amparo Martinez Arroyo, Mexico]	Noted. The implications of the modes of variability on the water cycle changes are assessed in section 8.3.2.9 and 8.4.2.9. The relationship between the multi-scale processes listed and the related implications on the water cycle is embedded in the assessment of the different modes.
13559	0				Due to the multiscale nature of the analysis, both, theories and numerical models of the CMIP5 experiments were used in this Report, as well as 7 global models of CMIP6 that were available at the date of the analysis, (not published yet), which is a significant weakness, due to the importance to use peer reviewed publications in this type of documents. [Maria Amparo Martinez Arroyo, Mexico]	Rejected - The assessment is mostly if not only based on published peer-reviewed papers and the FGD includes more CMIP6-related papers. Figures based on CMIP6 results are not the basis for the assessment but rather illustrate that the published key findings are generally not contradicted by the latest-generation global climate models.
13561	0				For some regions, due its resolutions, global models does not reproduce the atmospheric dynamics with spatial scales less than 50 km, the analysis in this Report was complemented with information derived from papers that reports results using simple thermodynamic models, that is because the local processes associated to abrupt orography and its impact on intense convective systems, as well as thermal gradients oceanic contrasts, these conditions predominate in the tropics and are not adequately represented in global models, seeing that there is not enough information and knowledge for these scales. The dynamical downscaling is a good tool to get this information. [Maria Amparo Martinez Arroyo, Mexico]	Noted - The resolution issue is briefly discussed in Section 8.5.1.2 but is mostly assessed in Ch10.

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88057	0				None of the two specific glacier sections makes explicit reference to SROCC where glaciers and water are assessed comprehensively. Please do so. [Georg Kaser, Austria]	In FGD, we have already cited SROCC in 8.2.3.1, lines 16-17, "The SROCC concluded there is high confidence that the peak runoff has already been passed for some smaller glaciers (Hock et al., 2019); & in 8.3.1.5, lines 9-11, "SROCC found that there is robust evidence and high agreement that discharge due to melting glaciers has already reached its maximum point and has begun declining with smaller glaciers (SROCC 2.3.1.1).", and we have also cited SROCC extensively in 8.4.1.7.1
13563	0				One of the greatest scientific challenges in evaluating impacts associated with climate change is the non-uniformity of projected changes in the hydrological cycle (including tropical cyclones and modes of tropical and extra-tropical variability of different scales), this is associated to spatial variability driven by thermodynamic and dynamic processes that involve global circulations, water vapor transport and cloud microphysics, which have not yet been systematically identified in observations [Maria Amparo Martinez Arroyo, Mexico]	Done. This challenge was discussed and assessed in many sections of this chapter.
13565	0				For this reason, it becomes essential to promote multinational programs and projects to applied dynamic downscaling, taking advantage of the improved regional numerical models, which already include in its dynamic cores, the non-hydrostatic approach, as well as the "convection permitting" option, eliminating the clouds parameterizations. [Maria Amparo Martinez Arroyo, Mexico]	Noted - This has been emphasized in Section 8.5.1.2.2
22269	0				There is a tendancy for several assessment findings to not sufficiently traceably arise from the underlying assessment. In all cases there should be a direct line of sight between the assessment summary finding and the preceding text. You cannot introduce new subjects in summary assessments and the text must directly build to the assessment finding. In many sections this is the case but then in many others it is not. [Peter Thorne, Ireland]	Thanks, good point and something we have worked hard to address in the FGD revisions. We have added summary statements where they were missing and have revised to the text to provide the requisite line of sight throughout.
13567	0				The processes of change in evaporation over continents are not sufficiently supported by the knowledge and information derived from global CMIP6 models (due to its spatial resolution), however, in Chapter 8 the authors complement the analysis with information from CMIP5 models, however new climate change scenarios are not considered in these models, therefore it is important to promote the use of the CMIP6 simulations, as well as its dynamic downscaling. [Maria Amparo Martinez Arroyo, Mexico]	Rejected - The SOD and FGD include as many CMIP6 models and CMIP6-related papers as possible. Yet, the assessment is mostly based on post-AR5 publications which are also based on CMIP5 models and scenarios. RCMS are briefly discussed in Section 8.5.1.2.2 but are mostly assessed in Ch10.
22271	0				I have issues with the structure as presented in the SOD from the viewpoint of a reader. Section 8.2 comes too early and precedes much of the evidence that supports it. Sections on observations / attribution and modelling are somewhat antithetical to the scoping charge of integration of observations, models, theory, projections and attribution. The structure is an outlier within the family of process oriented chapters (5-9) and therefore does not support a whole-of-report assessment as well as it should. At a minimum I think 8.2 needs to be moved later in the piece. But I would rather see a more process oriented integrated assessment which would require a complete redesign and reallocation and reintegration of existing material. [Peter Thorne, Ireland]	Rejected - We understand your point but the overall outline was agreed at LAM1 and has not be changed until the FGD submission
7997	1	1	115	1	This chapter should be checked for other grammatical issues all the way through. The information contained in the parts I reviewed was very strong and referenced appropriately. It's just the presentation that needs improvement,. [Anthony Lupo, United States of America]	Noted.
33057	1	1	115	55	what is the role of population growth in water use and consequently water cucle? Could it affect the projected changes in water cycle [Sahar Tajbakhsh Mosalman, Iran]	Noted. This issue has to be dealt in WGII and WGIII.
33059	1	1	115	55	how would be the interaction between water cycle, climate change and industrial water usage? [Sahar Tajbakhsh Mosalman, Iran]	Noted. This issue has to be dealt in WGII and WGIII.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33061	1	1	115	55	how would climate change after water usage in different sectors? [Sahar Tajbakhsh Mosalman, Iran]	Noted. This issue has to be dealt in WGII and WGIII.
33063	1	1	115	55	what would be the role of technology in climate change impact on water cycle? [Sahar Tajbakhsh Mosalman, Iran]	Noted. This issue has to be dealt in WGII and WGIII.
33065	1	1	115	55	how would water usage in different sectors change under climate change?and what would be its impact on water cyvle? [Sahar Tajbakhsh Mosalman, Iran]	Noted. This issue has to be dealt in WGII and WGIII.
32727	1	1	115	55	what is the role of population growth in water use and consequently water cucle? Could it affect the projected changes in water cycle [sadegh zeyaeyan, Iran]	Noted. This issue has to be dealt in WGII and WGIII.
32729	1	1	115	55	how would be the interaction between water cycle, climate change and industrial water usage? [sadegh zeyaeyan, Iran]	Noted. This issue has to be dealt in WGII and WGIII.
32731	1	1	115	55	how would climate change after water usage in different sectors? [sadegh zeyaeyan, Iran]	Noted. This issue has to be dealt in WGII and WGIII.
32733	1	1	115	55	what would be the role of technology in climate change impact on water cycle? [sadegh zeyaeyan, Iran]	Noted. This issue has to be dealt in WGII and WGIII.
32735	1	1	115	55	how would water usage in different sectors change under climate change?and what would be its impact on water cyvle? [sadegh zeyaeyan, Iran]	Noted. This issue has to be dealt in WGII and WGIII.
19433	1	1	115	55	What is the role of population growth in water use and consequently water cycle ? could it affect the projected changes in water cycle ? [Yashar Falamarzi, Iran]	Noted. This issue has to be dealt in WGII and WGIII.
19435	1	1	115	55	How would be the interactions between water cycle, climate change and industrial water usage? [Yashar Falamarzi, Iran]	Noted. This issue has to be dealt in WGII and WGIII.
19437	1	1	115	55	How would climate change alter water usage in different sectors? [Yashar Falamarzi, Iran]	Noted. This issue has to be dealt in WGII and WGIII.
19439	1	1	115	55	What would be the role of technology in climate change impacts on water cycle? [Yashar Falamarzi, Iran]	Noted. This issue has to be dealt in WGII and WGIII.
19441	1	1	115	55	How would water usage in different sectors change under climate change? And what would be its impacts on water cycle? [Yashar Falamarzi, Iran]	Noted. This issue has to be dealt in WGII and WGIII.
33053	1	1	200	50	The more detail discussion may be needed about big Dam construction projects effect on water cycle, in middle east and Africa (for example in turkey and its effect on surrounding countries like Iraq and Iran) [Sahar Tajbakhsh Mosalman, Iran]	Noted. This issue has to be dealt in WGII and WGIII.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33055	1	1	200	50	A discussion about advance in projects like clouds trapping or cloud seeding may be needed, That How progress in these techniques may have effects on the water cycle in arid areas during coming years . [Sahar Tajbakhsh Mosalman, Iran]	Noted. This issue has to be dealt in WGII and WGIII.
32723	1	1	200	50	The more detail discussion may be needed about big Dam construction projects effect on water cycle, in middle east and Africa (for example in turkey and its effect on surrounding countries like Iraq and Iran) [sadegh zeyaeyan, Iran]	Noted. Insufficient knowledge about that precluded a useful discussion.
32725	1	1	200	50	A discussion about advance in projects like clouds trapping or cloud seeding may be needed, That How progress in these techniques may have effects on the water cycle in arid areas during coming years . [sadegh zeyaeyan, Iran]	Noted. This issue has to be dealt in WGII and WGIII.
1291	1	1	216	7	It apperas to me that chapter 8 repeats material from other chapters such as trends in extremes, modes of variability, regional climate models, etc. I expected to see more cited works from hydrologists since this is a chapter on hydrological cycle, but I guess that many hydrological studies are very local and regional so they don't necessarily give a general answer. I also expected the chapter to include more work on new topics such as the global area of precipitation. There are some new concepts such as 'flash droughts' which could be explained. Also, the chapter sends a mixed message about tropical circulation, the Hadley and Walker circulations. I suggest that this chapter is closer coordinated with other chapters in the report to avoid repeats, a confusing structure and different messages. Also, I'd advice the authors to search to see if there is other relevant and important work that they have missed so that the chapter gives a representative picture of the status of the scientific knowledge. [Rasmus Benestad, Norway]	Thank you for your comments. A certain amount of repetition is necessary to provide context as each chapter is designed to read as a stand-alone piece. We have worked to coordinate with other chapters as much as possible and have brought in new material from contributing authors where appropriate.
12729	1	1	216	7	The Chapter 8 SOD is a great improvement upon the FOD version and it now reads much more coherently and consisely as a chapter. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Thank you very much.
93583	1	1	216		In general I find this chapter well written, well constructed and very interesting. It provides really interesting information, therefore clear conclusions while not hiding the uncertainties and what is not well known. I like the plan in the form of answers to some questions. [Jean-Louis Dufresne, France]	Thank you very much.
41543	1	22	1	22	Michael Byrne (UK / Ireland) [Michael Byrne, United Kingdom (of Great Britain and Northern Ireland)]	Noted. No comment was given.
86535	1		218		Congratulations to the authors. This is very well written. To me this is the most exciting chapter of the entire report. [Jochen Harnisch, Germany]	Noted. Thanks for the greetings.
68929	1				I hope you don’t mind, but I’ve asked CH3 to please move the regional precipitation information based on paleoclimate models and proxy data to CH8. CH3 needs to free up space to address essential comparisons between paleoclimate models and “large scale” climate indicators that were selected by CH2-CH3-CH4, where "large scale" is defined as global to continental (see CCB2.2), with a preference for global where possible. The CH8 RE is a leader in the PMIP community and knows precipitation simulations in paleo models very well. Her expertise in seems like a natural fit for the review of this topic. [Darrell Kaufman, United States of America]	Noted with thanks. In fact, there were several discussions between Ch3 and Ch8 to address overlaps and inconsistencies.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12695	2	20			It is not clear (at this stage) what drivers are being responsed to. Is this something about observations or models? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The answer is within the subsection.
53123	2	27			For the sake of outline compacity and balance in subsection length, what about merging a few subsections here and in Section 8.4, for instance "Glaciers and seasonal snow cover", "Hadley and Walker circulations"? [Hervé Douville, France]	Rejected. The present structure was retained.
64873	5	1	5	1	I would like to congratulate the Chapter team on a vastly improved, and indeed very useful, SOD! [Johannes Quaas, Germany]	Noted – thank you for the positive feedback
114859	5	1	8	2	An Executive Summary with clear key messages. [Roxana Bojariu, Romania]	Noted – thank you for the positive feedback
193	5	1	8	2	It is strange that Glaciers are not summarised in the executive summary as they're an important part of the hydrological cycle. [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Assessment about glaciers has been elevated to ES in the FGD
12697	5	3	5	8	If appropriate within an Executive Summary, you might cross-reference to the expanded discussion of distillation of multiple lines of evidence in Ch10.5.4. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Cross-reference to ch 10 included
87065	5	4	5	7	The line "This chapter…" mentions current and future changes in water cycle. But later in line 6, it again mentions future/observed terms. This looks repetitive. [Tarul Sharma, Netherlands]	Taken into account – sentence has been split into two for clarity.
59047	5	4	5	7	This sentence is too long and should be split up. For example, “This chapter assesses multiple lines of evidence to evaluate current and future changes in the water cycle. This assessment includes the physical basis for water cycle changes, observed changes in the water cycle and attribution of their causes, future projections and related key uncertainties, and the potential for abrupt change. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted – Edited as suggested
19201	5	8	5	8	The authors should define the abbreviation "SSP" [Mohamed Deyab, Egypt]	Accepted – Edited as suggested
19203	5	8	5	8	The authors should define the abbreviation "CMIP6" [Mohamed Deyab, Egypt]	Accepted – Edited as suggested
53125	5	10			Add a (3rd?) paragraph about the expected spatial and seasonal heterogeneity of regional water cycle changes despite the limitations of the DDWW paradigm over land? [Hervé Douville, France]	Taken into account: a new paragraph is introduced detailing wet/dry responses and this is merged with the modified existing paragraph
99075	5	12	5	13	Why include "virtually certain" as if there is a distincton between 99 chances out of 100 and saying "will" or better "is already driving". Or do what another chapter say "It is unequivocal that …", but there is no alternative to this happening. [Michael MacCracken, United States of America]	Accepted – Edited to read '…will drive substantial and widespread changes…'
111379	5	12	5	23	While the 2-3% C-1 may hold true for GLOBAL MEAN, it would be useful to add that more local scale changes are unlimited by energy budget contraints, with far larger (super-cc) changes possible (and observed). This opening para completely omits this point [Paul Durack, United States of America]	Reject: in this paragraph global aspects that are well understood are dealt with while regional and shorter time-scale precipitation changes are covered in later paragraphs and in the chapter

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
113135	5	12	5	44	Rather than 'Physical Basis for Water Cycle Changes' what is being discussed in some of these statements is the 'Observed Changes in Patterns'. If you want to prevent the overlap with the 'Causes of Observed Changes' part, I would avoid those statements (e.g., 'are currently partly offset'...). [Diego Miralles, Belgium]	actually, my suggestion is to merge the statements (something like "observed and understood") coming from both sect 2 and sect 3
18663	5	13	5	13	"and" should be inserted between "substantial" and "widespread" [Govindasamy Bala, India]	Accepted – Edited as suggested
129075	5	13	5	15	The sentence stating paleoclimate studies show water cycle responses to past changes in the Earth's energy budget needs more context, especially since the topic sentence of the paragraph focuses on global water cycle and thus the inference is that this sentence is focused on global as well. At glacial-interglacial timescales and longer, paleoclimate studies can be interpreted to infer global water cycle responses to changes to the Earth's energy budget. At the timescale of the Holocene, paleoclimate studies reveal regional to subcontinental water cycle responses to changes to the Earth's energy budget, over the last 1000 years, paleoclimate studies reveal regional to local water cycle responses but a causality role for changes in the Earth's energy budget is not readily discerned. Need to bound the sentence in terms of timescale and spatial scale to be informative. [Trigg Talley, United States of America]	Taken into account. The reference to paleoclimate here has been deleted in the final draft
99077	5	16	5	16	Change "are virtually certain to" to "will"--there is no doubt. [Michael MacCracken, United States of America]	Rejected. This ES statement is based on theoretical understanding
27203	5	16	5	16	Using the term evapotranspiration would be more appropriate [Eric Brun, France]	Accepted
112199	5	16			Evaporation (E) or Evapotranspiration (ET) ? E and ET are both used in this chapter. Sometimes E seems to refer to E over oceans and ET over land. [Rutger Hofste, Netherlands]	Accepted: evapotranspiration is now used to cover all aspects of water transfer from the surface to the atmosphere based on the glossary definition
96797	5	17	5	20	The statement is unclear. The explanations in section 8.2.2.2 (page 20, line 15-17) do not provide this statement. [Nicole Wilke, Germany]	Taken into account: this line has been removed since it is unclear and difficult to explain simply in the Executive Summary
96799	5	17	5	20	The statement that "expected increases are currently partly offset by rapid atmospheric adjustments to GHG and aerosol radiative forcings" somehow contradicts SPM page 28, line 19 (1-3% per °C for the SSP5-8.5 scenario). One would assume, that atmospheric adjustments to GHG and aerosol radiative forcings can occur under SSP5-8.5, too, implying that the increase of 1-3% per °C mentioned in the SPM is a very high estimate. Please explain the future increase of 1-3% per °C here. Or is the difference caused by the fact that one is the global mean and the other over land? [Nicole Wilke, Germany]	Noted. The text in question was deleted.
131549	5	17	5	20	please explain briefly (e.g. by an example) how rapid atmospheric adjustments to GHG partly offset increases in global mean E and P? [Hans Poertner and WGII TSU, Germany]	Not applicable: this sentence has been removed as it was confusing
18665	5	17	5	20	Excellent assessment of the science underpinning the global water cycle. This could be elevated to SPM headline message. This should help people who are puzzled by the lack of global mean response in precipitation in historical observations. [Govindasamy Bala, India]	Noted – thank you for the positive feedback
69213	5	17	5	21	The first sentence states that counteracting effects of GHGs and aerosols on evaporation and precipitation will diminish in relative importance in the future, while the second sentence states that anthropogenic aerosols very likely alter the water cycle. These two sentences seem to be inconsistent; and therefore, to clarify the meaning, suggest specifying which effect of aerosols alters the water cycle in the second sentence. [Kaoru Magosaki, Japan]	Noted. The text in question was deleted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4793	5	18	5	18	In what direction does this current offset take place? [Bart van den Hurk, Netherlands]	Not applicable: this sentence has been removed as it was confusing
131551	5	18	5	18	what are "rapid atmospheric adjustments" in that context? [Hans Poertner and WGII TSU, Germany]	Not applicable: this sentence has been removed as it was confusing
27205	5	18	5	19	The expression " rapid atmospheric adjustments to GHG and aerosol radiative forcings" is too complex for a Summary. The terminology and explanations need to be clarified. [Eric Brun, France]	Accepted: this sentence has been removed
53127	5	18			replace "and" by "and by"? [Hervé Douville, France]	Accepted – Edited as suggested
7565	5	19	5	19	1/1: add "by": "by aerosol radiatvie forcings" [Celine Bonfils, United States of America]	Accepted – Edited as suggested
80639	5	20	5	21	I recommend extending to: ", and their less well-understood shortwave absorption and cloud microphysical effects." You have material in section 8.2.1 and Box 8.1 that establishes the aerosol-absorption-precipitation link as a distinct and quite important mechanism. [Bjorn Samset, Norway]	Accepted – Edited as suggested
59049	5	22	5	23	"some regions". Could give some examples here and/or specify some specific attributes along with it. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted – Clarified to read '...in regions of large-scale deforestation or irrigation...'
111507	5	23	5	23	Add 8.2.3 to list of reference sections, this is where LUC and irrigation is discussed. [James Renwick, New Zealand]	Accepted – Edited as suggested
99079	5	25	5	26	An earlier chapter (see Chapter 4, page 6, lines 46-47) indicates that there are larger areas of drying over the ocean, semingly in contrast to this statement.At least make sure to cross-check. [Michael MacCracken, United States of America]	Taken into account: this statement has been removed and a statement on ocean salinity contrasts is included in later paragraphs
17163	5	25	5	26	This summary statement needs work. At a minimum I suggest rephrasing to read '...mechanisms driving reductions in continental...' [Joelle Joelle Gergis, Australia]	Accepted – Edited as suggested
88119	5	25	5	26	To me, this sentence implies that RH declines over land drive reductions (or suppress increases) in continental precipitation. While I totally agree that RH decreases, enhanced land-warming and precipitation changes over land are highly coupled, I think the causality is still an open question. In fact, I suspect it may not be possible to neatly determine a chain of causality between responses in these highly coupled variables. For example, Saint-Lu et al. (2020) showed that when a single column model (with large-scale circulation parametrized using the damped gravity wave method) was forced with increased surface temperature and reduced near-surface RH, characteristic of global warming conditions over land, it produced an increase in precipitation. That paper concluded that suppressed precipitation increases over tropical land are more likely to be driven by atmospheric teleconnections from the oceans rather than directly by local RH decreases. I suggest an alternative sentence: `There is high confidence that the precipitation response to warming over land is weaker than the response over the oceans, and is associated with enhanced land warming and continental near-surface relative humidity declines.' References: Saint-Lu, M. et al. (2020), Influences of Local and Remote Conditions on Tropical Precipitation and Its Response to Climate Change, J. Climate, 33, 4045-4063 [Robin Chadwick, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: since this statement is controversial and unclear it has been removed and the smaller rate of increase in land precipitation with warming than the ocean referred to in the earlier paragraph

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45249	5	25	5	26	The ES statement says "There is high confidence that mechanisms driving declining continental near-surface relative humidity suppress precipitation response to warming over land relative to the ocean". The consistency of this statement with the assessments in Ch.2 and Ch.3 needs to be checked. For example, it is stated in Ch.2 (page 46, lines 10-11) that "A very likely decrease in relative humidity was observed over land areas since 2000, particularly over mid-latitude regions of the Northern Hemisphere". On the other hand, it is also assessed in Ch.2 (page 48, lines 25-26) that "global land precipitation has likely increased since the middle of the 20th century (medium confidence). Further, increasing trends in precipitaiton during (1901-2018) are assessed over mid-latitude areas covering eastern North America, northern Eurasia ...(Ch.2, page 47, lines 27-29). There is an apparent inconsistency between "decrease of near-surface relative humidity and precipitation suppression over land". Additionally, Chapter 3 (pg 27, lines 51-52) says that "Owing to the limited number of studies and model biases we conclude that there is low confidence in the attribution of changes in the surface humidity". The ES statement in Ch.8 needs to consistent with the assessment in Ch.2 and Ch.3 [Krishnan Raghavan, India]	Noted: this sentence has been removed and remaining statements dealing with land precipitation are checked for consistency with Chapters 2 and 3
34659	5	25	5	28	I think you can drop the first sentence of this key message. The second sentence is a better summary and is much more clear to the reader. [Russell Vose, United States of America]	Rejected – this is an key finding from section 8.2
18667	5	25	5	28	I am not if this is a new finding after AR5. If yes, this could be highlighted in SPM. Also, this result (% change in precipitation over land per unit global warming) should be consistent with Chapter 4 where the future changes in global mean precipitation over land is shown in the ionic figure of climate change. [Govindasamy Bala, India]	Noted: this sentence has been modified to make it clear that there are comparable long term mean precipitation responses over land and ocean, consistent with Chapter 4
132211	5	25	5	31	Excellent and well drafted text on a complex topic. [Sonia Seneviratne, Switzerland]	Noted – thank you for the positive feedback
113131	5	25	5	34	I do not fully follow how these paragraphs are arranged. There should be one concept per paragraph only. Here there are at least three aspects of the hydrological cycle mixed together. Why is plant water use efficiency discussed in a paragraph whose header relates to precipitation surpression? What about you start from the begining: one paragraph dedicated to the response of (potential) evaporation; then you move to precipitation in a next paragraph, and precipitation intensity in the next. [Diego Miralles, Belgium]	Noted: this paragraph has been modified and deals with more regional aspects of precipitation and aridity change
129077	5	25	5	37	Consider rewriting the two statements to clarify relative scales so it cannot be asked how there can be a decline in near surface relative humidity along with increases in low-level moisture: ""There is high confidence that mechanisms driving declining continental near-surface relative humidity ... Well-understood increases in low-level moisture of 6-7% per C of warming..."" [Trigg Talley, United States of America]	Accepted: the first statement is now removed although continental relative humidity decline is mentioned in relation to aridification and so the 2ns statement is rewritten to emphasise that this refers to moisture holding capacity
58989	5	25	5	44	It seems like this summary could use some clarification about where overland precipitation is expected to increase versus where there might be increase in drying. My understanding is that many coastal areas and the tropics will/have experienced increases in precipitation where continental drylands are predicted to get drier (as discussed on page 217). Clarification is needed. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account: a statement is now added to emphasise that wet events become wetter and dry periods drier while regional aspects are dealt with in later paragraphs dealing with observed and projected change
53129	5	26	5	28	although the total precipitation response including the fast atmospheric adjustment to increased CO2 concentration may be stronger over land than over the ocean (cf. Table 4.3) [Hervé Douville, France]	Taken into account: it is now stated that comparable land and ocean precipitation changes are explained by a combination of the rapid response and slower response to warming that affect land and ocean differently
86413	5	26	5	28	Better to mention the period for which assessment of precipitation changes over the global land and global oceanic regions are presented. Consistency with Chapter-4 can be considered while discussing the projeced changes. [Swapna Panickal, India]	Noted: it is difficult to be specific about the time-scale but long term is generally accepted to mean and of 21st century and the statement has now been modified to note comparable long term precipitation responses are a combination of rapid responses to GHG forcing and slow responses to GHG-induced warming

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103665	5	27	5	27	In the first summary page, there is a use of "GHG forcing" and "CO2-induced wamring": perhaps use consistently GHG induced warming? [Philippe Tulkens, Belgium]	Noted: some of the statements are specific to CO2 forcing so it is necessary to use both
89003	5	27		28	Are the numbers quoted here the temperature-dependent part of the response, or the total response? If they are the temperature-dependent part of the response, that should be stated. If they are the total response, more context needs to be provided - are these the response at equilibrium? Or in a transient state, and if so, which? The answers would not be the same, because the fast response would contribute to different degrees in the different cases. [Angeline Pendergrass, United States of America]	Taken into account: the statement has now been modified to note comparable long term precipitation responses are a combination of rapid responses to GHG forcing and slow responses to GHG-induced warming
64875	5	28	5	28	is this “uncertainty range”? [Johannes Quaas, Germany]	Noted: unless otherwise stated it is the 5-95% confidence range
4795	5	28	5	28	Larger range: you mean uncertainty range? Or geographical range? Seasonal range? [Bart van den Hurk, Netherlands]	Not applicable – text removed
113129	5	28	5	31	The main driver of potential evaporation is radiation. Potential evaporation is going up, mainly due to the increase in net radiation (GHGs), rather than temperature or air dryness. [Diego Miralles, Belgium]	Noted: we take the point that radiation is the dominating driver of potential evaporation but note that vapour pressure deficit and temperature related aspects also contribute. We change warming to heating which also implies radiation.
8661	5	29	5	29	I suggest to homogenise between chapters: atmopsheric evaporative demand or atmopsheric water demand. In chapter 11 it is used the first term. [Sergio Vicente-Serrano, Spain]	Accepted: atmospheric evaporative demand is adopted
113125	5	29	5	29	drive' not 'drives' [Diego Miralles, Belgium]	Taken into account: now changed to cause
93585	5	29			replace « surface relative humidity » by « air surface relative humidity » to avoid confusion with soil humidity for non experts [Jean-Louis Dufresne, France]	Accepted: added atmospheric
113127	5	30	5	30	evapotranspiration' is already redundant enough (transpiration is evaporation). 'Land surface evapotranspiration' is double redundant. 'Evapotranspiration' is only used for land, never for oceans. Please correct throughout. [Diego Miralles, Belgium]	Rejected: evapotranspiration accounts for all transfers of water from the surface to the atmosphere according to the glossary
59051	5	31	5	31	"drying areas", shouldn't this be "arid regions" or simply "areas experiencing aridification". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable: this sentence has been removed
58987	5	31	5	34	The wording in this statement is a bit over-reaching. CO2 enrichment will only increase photosyntheic activity if other soil resources are not limiting, especially in drylands as mentioned in the previous sentence. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: it is now framed such that these effects can occur while noting that there is low confidence in how they combine
74135	5	31	5	34	While changes in soil moisture through the full column is uncertain, I'd suggest that it is possible to say that surface soils will generally dry, absent large changes in advected moisture. This is based on both modeling work ("Divergent surface and total soil moisture projections under global warming" by Berg et al. 2016 https://doi.org/10.1002/2016GL071921) and observational hydrology ("Terrestrial Evaporation and Moisture Drainage in a Warmer Climate" by Short Gianotti et al. 2020 https://doi.org/10.1029/2019GL086498). I will add suggestions for this discussion in sections 8.2.1 and 8.2.3 as well. [Daniel J. Short Gianotti, United States of America]	Noted: drying of soils in this context is relating to the combined CO2 physiological effects while declining soil moisture is implied from aridification
12699	5	31	5	34	The passage of text here is very clunky or long-winded and should be revised. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted: the line is updated but remains necessarily quite long

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64877	5	32	5	34	Despite the low confidence it would be very good to provide an idea about the sign of the change / qualitative assessment of the change [Johannes Quaas, Germany]	Rejected: it is low confidence because there is no clear sign of change
59053	5	33	5	34	Please rephrase the sentence after "but only low confidence….". For example, “but there is only low confidence in how these factors will collectively drive the regional precipitation and soil moisture changes. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
20455	5	33	5	34	"low confidence in how these factors will combine" is questionable. In IPCC reports it seems that confidence appreciations should indeed apply to a statement, not to a question mark or to an admission of lack of knowledge [philippe waldteufel, France]	Noted: a low confidence statement is necessary since there are potentially important plant-responses to CO2 that can affect the water cycle regionally but the sign is difficult to establish based on the evidence
131553	5	36	5	36	define "low-level moisture" in this context. [Hans Poertner and WGII TSU, Germany]	Taken into account: near surface is now used which is more understandable as just above the surface
103667	5	36	5	36	What is 'low-level moisture'? [Philippe Tulkens, Belgium]	Taken into account: near surface is now used which is more understandable as just above the surface
22239	5	36	5	37	This is hard to square with prior statement about RH decreasing. I assume that this should be a 6-7% increase in water holding capacity per degree C and not an actual 6-7% increase otherwise it is not possible to logically square this finding with the prior one. [Peter Thorne, Ireland]	Accepted: water holding capacity is adopted here as suggested
7567	5	36	5	38	1/1: "increases in low-level moisture of 6-7% per °C of warming explain a similar magnitude of intensification of heavy precipitation" seems to disagree with "The increase in precipitation intensity with warming can vary significantly from the mean water vapour response". To fix, this you could simply add "however": "However, the increase in precipitation intensity with warming can vary significantly from the mean water vapour response due to [...]" [Celine Bonfils, United States of America]	Noted: the sentence is adjusted now to ascribe low confidence that rates are higher for sub-daily precipitation intensification
34917	5	36	6	54	Detailed Comments by SOD Chapter – Chapter 8: Beyond the physical law that there can be 6-7% more moisture content per degree of warming, regional precipitation trends are quite speculative, even between land and sea. See general comment #12 above. [Jim O'Brien, Ireland]	Noted: this paragraph is now framed in terms of increased moisture transport into storm systems which is more robust that considering in terms of regions as suggested
112201	5	36			What is the threshold for low-level. (maybe explain for the first time to increase readibility) [Rutger Hofste, Netherlands]	Noted: this is changed to "near surface atmospheric"
34661	5	37	5	37	You can probably drop the phrase "during wet events" at the end of the sentence. [Russell Vose, United States of America]	Accepted – Rephrased to read 'heavy precipitation events'
51425	5	37	5	37	...intensification of heavy precipitation during wet events' - suggest this could be simplified to 'heavy precipitation events'? Also, could the message on flooding severity (last line of para) be brought into the first bold line? It is an important, policy-relevant message. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – Rephrased to read 'heavy precipitation events'.
12701	5	38	5	39	Perhaps a better wording here would be to state, "due to uncertainties in cloud microphysical and convective processes". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – rephrased as suggested

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12703	5	41			Change "modulate" to "alter" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – rephrased as suggested
114861	5	42	5	44	Reference to floods here is a bit beyond the scope of the chapter and should point somehow to chapter 12 and Climate Impact Drivers (CID)s. [Roxana Bojariu, Romania]	Rejected – Flooding is within the scope of material assessed in chapter 8
4797	5	42	5	44	Move "at the global scale" to the beginning of sentence [Bart van den Hurk, Netherlands]	Rejected – this sentence is clearer as written
113133	5	43	5	43	Why is floding and wet extremes here, and drought is not mentioned? You need a solid rationale and consistency on the split of chapters 8 and 11. Most readers will be wondering this while reading this summary. [Diego Miralles, Belgium]	Taken into account. This sentence now mentions both wet and dry extremes.
24541	5	43	5	44	is the flooding related confidence statement consistent with Chapter 11? [Subimal Ghosh, India]	Taken into account: the sentence is revised and is consistent with findings of Chapter 11
64879	5	44	5	44	I have a hard time to precisely understand “at the global scale”. Is this happening ubiquitously? [Johannes Quaas, Germany]	Accepted: global scale is removed
18353	5	46	5	55	Many studies suggest (e.g., Dai 2016; Dai and Zhao 2017; Dai and Bloecker 2019) that historical precipiation and other related changes (e.g., streamflow) are still likely dominated by internal climate variability, such as the IPO (Dong and Dai 2015), over most of the globe. Human influences may be evident only for certain latitude zonally-averaged changes. Relevant studies for this chapter: Dai. A., 2016: Historical and Future Changes in Streamflow and Continental Runoff: A Review. Chapter 2 of Terrestrial Water Cycle and Climate Change: Natural and Human-Induced Impacts, Geophysical Monograph 221, edited by Qiuhong Tang and Taikan Oki, AGU, John Wiley & Sons, pp. 17-37. Dai, A. and T. Zhao, 2017: Uncertainties in historical changes and future projections of drought. Part I: Estimates of historical drought changes. Climatic Change, 144, 519–533. DOI: 10.1007/s10584-016-1705-2. 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. Dong, B., and A. Dai, 2015: The influence of the Inter-decadal Pacific Oscillation on temperature and precipitation over the globe. Climate Dynamics, 45, 2667–2681. DOI 10.1007/s00382-015-2500-x. Dong, B, and A. Dai, 2017: The uncertainties and causes of the recent changes in global evapotranspiration from 1982-2010. Climate Dynamics, 49: 279–296. doi:10.1007/s00382-016-3342-x. Hegerl, G.C., and 26 Co-authors (including A. Dai), 2015: Challenges in quantifying changes in the global water cycle. Bull. Am. Met. Soc., 96, 1097–1115. doi: http://dx.doi.org/10.1175/BAMS-D-13-00212.1. [Aiguo Dai, United States of America]	Noted – However, this summary focuses on which changes are evident. The role of internal climate variability is discussed at length in the chapter and includes most of these references.
53131	5	46			could also highlight that D&A studies are still hampered by multiple observational and modelling uncertainties but that D&A can be more successful when dealing with phenomena or events than when focusing on fixed spatial domains or time intervals? [Hervé Douville, France]	Rejected – Not within the scope of material to highlight in the Executive Summary
96801	5	48	5	48	Please clarify that "human activities" means "climate-relevant activities" here. Non-climate-relevant human activity such as water management plays of course a role, too. [Nicole Wilke, Germany]	Accepted – Rephrased to read: 'There high confidence that human influences on the climate system have affected the global water cycle since pre-industrial times'
17165	5	48	5	49	This is potentially the most important statement from this chapter. Suggest rephrasing to simply read: There is high confidence that human activities have affected the global water cycle since pre-industrial times [Joelle Joelle Gergis, Australia]	Accepted – rephrased as suggested

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129079	5	48	5	49	[CONFIDENCE] Use of the term 'since pre-industrial times' in the sentence "There is growing evidence and high confidence that human activities have affected the global water cycle since pre-industrial times" is problematic since it can be interpret to be either sometime between 1750 and 1899 or post-1900. There is no evidence provided in 8.3.1.6 or 8.3.1.8 that reveals a high degree of confidence that human activities impacted the global water cycle starting in pre-industrial times (1750 to 1850-1900 [SPM-2]). It is likely human activities impacted regional to local components of the water cycle starting in pre-industrial times. It is likely human activities have impacted the global water cycle starting in the mid 20th century. Suggest sentence is revised to "There is growing evidence and high confidence that human activities have affected the global water cycle since the end of the 19th century." or even better "There is growing evidence and high confidence that human activities have affected the global water cycle since the beginning of the 20th century." [Trigg Talley, United States of America]	Accepted – Amended to read: 'There is high confidence that human activity has affected the global water cycle since the mid-20th century'.
109679	5	48	5	49	It has been known with complete confidence for some time that human activities have affected the global water cycle through a variety of mechanisms; I assume that what is supposed to be meant in this passage are changes due specifically to anthropogenic global climate change. Be more specific; a water resource scientist or engineer, and people in other allied disciplines, will raise their eyebrows at the way this passage is phrased. [Sean Fleming, United States of America]	Accepted – Rephrased to read: 'There high confidence that human influences on the climate system have affected the global water cycle since pre-industrial times'
111381	5	48	5	55	There is considerable evidence of water cycle changes over the 70% of Earth's surface covered by the global ocean (which stores 97% of Earth's water), however this evidence is omitted in this opening "Causes of observed changes" para. I wonder if in the 4 sections you currently outline "physical basis..", "causes of observed changes", "Future changes", "abrupt changes", there is a very important 5th missing "observed changes" which could provide a very high level overview of changes across realms: land, cryosphere, atmosphere, ocean? [Paul Durack, United States of America]	Taken into account – We included statements related to changes in the ocean.
51427	5	48	5	55	This paragraph opens with a statement on "anthropogenic activities" but then only covers the impact of anthropogenic emissions. Suggest that it is clarified whether the statements here refer to the imapcts of emissions only via radiative forcing or also by direct CO2 effects on evapotranspiration as discussed in section 8.3.1.4. Secondly, suggest it should also include a statement on the role of non-climate anthropogenic influences as discussed in section 8.2.3.4 and 8.3.1.7.4 [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – Amended to read: 'There is high confidence that human activity has affected the global water cycle since the mid-20th century'. Rejected other suggestions as details are provided in the relevant subsections. The key summary statement needs to be clear and concise
103669	5	48	5	55	consider merging this paragraph in text above: the whole chapter is about influences from human induced GHG on the water cylcle. [Philippe Tulkens, Belgium]	Rejected – this sentence is specifically summarising observed changes in the water cycle (not theoretical changes mentioned in the paragraph above)
12705	5	48			Change "activities have" to "activity has". Plural is not necessary here. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable – text rephrased
12707	5	49			Do we need to be concerned over which definition of pre-industrial times is being intended here? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted – Text rephrased to read 'Accepted – Amended to read: 'There is high confidence that human activity has affected the global water cycle since the mid-20th century'.
64881	5	51	5	51	to what baseline does the “enhanced” refer? [Johannes Quaas, Germany]	Not applicable – text rephrased

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7569	5	52	5	52	add "low-elevation mountain catchment" [Celine Bonfils, United States of America]	Not applicable – text rephrased
7571	5	52	5	52	add: "anthropogenic greenhouse gas emissions". I doubt that you are talking about aerosol forcing here. [Celine Bonfils, United States of America]	Accepted – Rephrased to read: 'that anthropogenic emissions of greenhouse gases'
12709	5	52	5	55	It is not immediately obvious why particular regions are singled out in this ES statement. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account – Text clarified to highlight regions with high confidence: 'There is high confidence that anthropogenic emissions of greenhouse gases have enhanced land surface drying and contributed to severe aridity and drought in dry summer climates such as the Mediterranean, South Africa, and western North America. {8.3.1.6} '
64883	5	53	5	53	Again I am unsure about the “enhanced”. Is there a natural drying trend that is enhanced? [Johannes Quaas, Germany]	Accepted – Rephrased to 'influenced'
7573	5	54	5	54	add: "anthropogenic greenhouse gas signal". I doubt that you are talking about aerosol signal here. [Celine Bonfils, United States of America]	Accepted – Rephrased to read: 'that anthropogenic emissions of greenhouse gases'
34663	5	54	5	55	Is this Mediterranean the only region for which attribution of such changes is possible? Also, at the end of the sentence, do you mean 'Mediterranean-type climates' rather than 'regions'? [Russell Vose, United States of America]	Accepted – Sentence rephrased to read: 'There is high confidence that anthropogenic emissions of greenhouse gases have influenced summer mid-latitude land surface drying and contributed to severe aridity in the Mediterranean, and medium confidence that an anthropogenic signal is contributing to increased aridity in other semi-arid regions'.
114863	5	54	5	55	Perhaps the term "aridity" has to be defined in the glossary. There is a term "arid zone" there, but I think it doesn't generly cover the characteristic of "aridity". [Roxana Bojariu, Romania]	Rejected – Definition of aridity already appears in the glossary: 'The state of a long-term climatic feature characterised by low average precipitation or available water in a region. Aridity generally arises from widespread persistent atmospheric subsidence or anticyclonic conditions, and from more localised subsidence in the lee side of mountains (adapted from Gbeckor-Kove, 1989; Türkeş, 1999).
111509	5	55	5	55	Add 8.3.1.5 and 8.3.1.7 to list of reference sections. [James Renwick, New Zealand]	Accepted – Edited as suggested
4799	5	55	5	55	"Mediterranean-like regions" sounds very Europe-centric [Bart van den Hurk, Netherlands]	Accepted – Rephrased to read 'other semi-arid regions'
18675	5		6		The Executive summary has substantially improved from FOD. Congratulations to the authors! [Govindasamy Bala, India]	Noted – thank you for the positive feedback
6885	6	1	6	5	This first half of this whole ES statement is rather confused. It starts off by referring to the water cycle (meaning precipitation, or circulation, or both?), before referring explicitly to monsoon circulations and then returning to monsoon precipitation. The non-expert reader may leave with the impression that there has been "strengthening monsoon precipitation" in the "second half of the 20th century" whereas really this applies to GHG runs only and most NH monsoons have suffered a decline. All the right ideas are there but the statement needs to be constructed in a much more bullet-proof manner in order to avoid potential misinterpretation. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The whole statement was rewritten and framed as a function of scale.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12711	6	1			Partly offset could be wholly offset, such as (arguably) in the case of the South Asian monsoon. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The qualifier "at a global scale" was added.
12713	6	2	6	5	This statement is unclear. Is not a weakened monsoon circulation expected in response to GHG, irrespective of aerosol? (e.g. page 20, lines 14-15). It is not clear from this statement on the 20th century "outcome" on the monsoons - not just a matter of offsetting, but infact a net decline for some regions, such as South Asia. In addition, irrespective of the large-scale effects of aerosol on monsoon circulations, local aerosol changes in monsoon regions may exert non-circulation related changes in rainfall due to effects on cloud microphysics. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The whole statement was rewritten and framed as a function of scale.
64885	6	3	6	3	The headline was that aerosols offset some precip effects expected due to GHG warming. So better perhaps “a relative weakening”? How was this observed? [Johannes Quaas, Germany]	Noted. The whole statement was rewritten and framed as a function of scale.
7575	6	4	6	12	1/1: In Line 4, I suggest to add "the second half of the 20th century, especially before 1975, which marks the onset of regulation of SO2 aerosol emissions by the Clean Air acts in Europe and North America". I would also add in line 9: "the recent partial recovery (after 1975)". Finally, in Line 12, I would refer to the ITCZ section and to Figure 8.11 {8.3.1, 8.3.2.1, 8.3.2.4, Box 8.1, Figure 8.11 (right column)}. It is because a southward shift of ITCZ has been detected before 1975, followed by a northward shift in ITCZ after 1975 (see one other comment, and see Figure 8.11 (right column), which is in good agreement with the monsoon changes (Figure 8.11, right column, rainall pattern), at least in Sahel. [Celine Bonfils, United States of America]	Noted. The whole statement was rewritten and framed as a function of scale.
6887	6	6			"a detectable influence" does not state whether it has been positive or negative. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The whole statement was rewritten and framed as a function of scale.
20095	6	8	6	8	Indicating a period would be better than "recent" [philippe waldteufel, France]	Not applicable – text rephrased
22241	6	8	6	12	A careful cross-check of this statement is required with chapters 6 and 7 who characterised this issue somewhat differently than is done so here. [Peter Thorne, Ireland]	Not applicable – text rephrased
6889	6	10			is "anthropogenic" needed with greenhouse gases? It is generally not necessary to state this. Also should be "an" additional contribution. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – rephrased as suggested
6891	6	10			Is there no assessed contribution to West Africa rainfall recovery from internal variability? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable – text rephrased

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89005	6	11		12	As the sentence currently reads, it seems to say that anthropogenic aerosols are largely emitted from North America and Europe, which is not true because a large fraction of aerosols are emitted from Asia. I suspect what was meant is that the transition from dimming to brightening was led by changes in aerosol emissions from North America and Europe, in which case the wording should be, ", largely due to changes in emissions from North America and Europe." [Angeline Pendergrass, United States of America]	Accepted – rephrased as suggested
6893	6	11			For the lay reader, wouldn't the dimming and brightening radiative effect not be better explained in terms of an increase then a decrease in aerosol emissions from those regions? Or I have misunderstood the science here? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The text in question was deleted.
14975	6	14	6	15	Check coherency with Chapter 2 regarding the calibrated language used in the statement for SH storm tracks shift [Juan Rivera, Argentina]	Taken into account – Confidence statement changed to medium in the FGD, for consistency.
17169	6	14	6	16	This high confidence statement in poleward shift in Southern Hemisphere storm tracks mentions the austral summer but there is no significant text supporting this provided in section 8.3.2.2. The signal is more in the austral autumn-winter so I suggest removing reference to summer. Summary statement in the Execitive Summary references Australia but no specific mention of Australia is found in 8.3.2.2. Please insert explicit reference to Australia to support high confidence in poleward migration of Southern Hemisphere storm tracks in summary statement on p 48 lines 33-40. Note that no high confidence statement is currently mentioned in section 8.3.2.2 so this needs to be checked and edited for consistency. [Joelle Joelle Gergis, Australia]	Taken into account – Confidence statement changed to medium in the FGD, for consistency with 8.3.2.2. Reference to Australia removed as the SAM/storm track shift affects all mid-latitude countries. The SAM-related shift is mostly summer and autumn (e.g. Jones et al 2016), this text has not been changed.
11103	6	14	6	16	The main sentence of this paragraph is not talking about "Causes of observed changes" as the previous two paragraph. [Wen Wang, China]	Rejected – The rest of the paragraph provides causes of these changes
111503	6	14	6	16	Rather than mention Australia specifically, generalise to say "...Southern Hemisphere mid-latitude storm tracks and associated precipitation have migrated polewards over recent decades, especially in the austral summer and autumn." [James Renwick, New Zealand]	Accepted – Rephrased to read: ' There is medium confidence that Southern Hemisphere storm tracks and associated precipitation have shifted poleward over recent decades, especially in the austral summer and autumn '.
59055	6	14	6	16	Since southern hemisphere is already mentioned is there any need to mention "austral" here. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected – State 'austral' for clarity
79433	6	14	6	20	In section 8.3.2.8 I could find much support to this statement regarding "especially in the austral summer and autumn in locations like Australia." I am not aware that in Australia there is more evidence compared to other places about the poleward migration of the storms tracks. [Alejandro Di Luca, Australia]	Accepted – Rephrased to read: ' There is medium confidence that Southern Hemisphere storm tracks and associated precipitation have shifted poleward over recent decades, especially in the austral summer and autumn'.
18669	6	14	6	20	The poleward migration of storm tracks is also discussed in Chapter 4. I hope this cross-chapter issue is taken care in this chapter. [Govindasamy Bala, India]	Noted. This statement refers only to past changes. Related statements regarding future change have been checked for consistency with Chapter 4.
99081	6	15	6	15	I don't think that the word "migrated" is the right word here, as if the storm tracks had a chance or say in this. The point to make is that the storm tracks and precipitation were pushed poleward due to poleward exapnsion of the subtropics which is in turn being caused by the warming and additional moisture leading to a stronger Hadley Cell. [Michael MacCracken, United States of America]	Accepted – Rephrased to read: ' There is medium confidence that Southern Hemisphere storm tracks and associated precipitation have shifted poleward over recent decades, especially in the austral summer and autumn'.
99083	6	16	6	17	I think there needs to be an explanation of what a positive trend in the Southern Annular Mode will mean--indicatee why the reader should care. [Michael MacCracken, United States of America]	Accepted – Rephrased to read: 'These changes are associated with a positive trend in the Southern Annular Mode related to both stratospheric ozone depletion and greenhouse gas increases, which reduces precipitation in affected regions'.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
34665	6	17	6	18	Isn't there evidence that NH storm tracks have moved poleward as well (even if the influence of anthropogenic forces isn't so clear)? [Russell Vose, United States of America]	Rejected – Statement on Northern Hemisphere storm tracks is provided in the third sentence of the paragraph
53133	6	17	6	20	also emphasize the stronger signal-to-noise ratio in summer when most models however underestimate the decreasing frequency of NH extratropical cyclones? [Hervé Douville, France]	Rejected – this detail is not suitable for an executive summary statement
20457	6	17	6	20	Same remark as on Page 5 Line 33-3' [philippe waldteufel, France]	Rejected – unclear what is being specifically requested here
131555	6	24	6	24	The acronyms "RCP" "SSP" are both explained in the glossary. Yet this paragraph includes many abreviations which makes reading difficult. It would therefore be useful to make a very brief explanation of RCP and SSP, e.g. in a side sentence or in brackets [Hans Poertner and WGII TSU, Germany]	Taken into account – reference to RCPs now removed. Acronym for SSP now appears in the first paragraph of the Executive Summary
17171	6	24	6	25	Simplify the summary statement to just say: It is very likely that precipitation will increase on average over global land over the 21st century. The rest of the paragraph and subsections provide detail [Joelle Joelle Gergis, Australia]	Accepted – rephrased as suggested
111383	6	24	6	25	Terminology check. The CMIP5/AR5-era RCP acronym stands for "Representative Concentration Pathway", with the CMIP6/AR6-era SSP short for "Shared socioeconomic pathways", both are future scenarios, with "projections" generated by using models forced by any scenario. The text "..former RCP projections and.. newly available SSP concentration scenarios" is mixing this up. There are differences between some of these scenarios regarding CO2 concentrations and CO2 emissions (which are then ingested by Earth System Models to ascertain atmospheric concentrations) [Paul Durack, United States of America]	Not applicable – text removed
51429	6	24	6	25	The use of SSPs to refer to scenarios of concentrations is confusing since it appears to ignore the uncertainty in relating emissions scenarios to concentration scenarios presented in Chapter 5. Suggest this is reconsidered here and throughout the report. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - Ch8 now complies to the general guidelines and refers to SSPs as emission scenarios although the GHG concentrations are prescribed in both GCM and ESM simulations.
79387	6	24	6	33	What are the units of increases in mean precipitation? Is that in % per degree of surface waming? Also I don't understand what these values mean 8.5 +/-7.1? [Alejandro Di Luca, Australia]	Accepted – Change by the end of the century is now specified, and the parenthetical numbers are now explained.
111511	6	24	6	33	Check the numbers quoted here against what's given in 8.4.1 and what's stated in Chapter 4. [James Renwick, New Zealand]	Accepted – The projections have been updated for consistency with Table 4.3 in Chapter 4.
18671	6	24	6	33	Why is the quantification not in terms of % change per unit global mean warming? I think expressing the changes in % per unit global mean warming will help to understand underlying fundamental science. [Govindasamy Bala, India]	Accepted - The revised ES highlights "variable regional water cycle projections across models".
45715	6	24	6	43	On the one hand it is said that precipitation over land increases but on the other hand arid areas increase. Isn't this somewhat contradictionary? [Sabine Wurzler, Germany]	Rejected – It is not a contradiction, as some arid areas are drying but total precipitation summed over all land areas still increases.
4801	6	25	6	25	Wording of ESSs is not always very strict. For instance "...precipitation will increase on average over global land" is better formulated as "...the precipitation averaged over the global land area will increase". Several similar wording issues are present in the ESS section [Bart van den Hurk, Netherlands]	Accepted – rephrased as suggested
17167	6	25	6	26	Suggest simplifying to just read: It it is very likely that precipitation will increase on average over global land over the 21st 26 century. [Joelle Joelle Gergis, Australia]	Accepted – rephrased as suggested
15527	6	26	6	27	The projected global land precipitation increases under SSP1-2.6 and SSP5-8.5 do not tally with the projections shown in Table 4.3 in Chapter 4. Please check and revise as appropriate. [SAI MING LEE, China]	Accepted – The projections have been updated for consistency with Table 4.3 in Chapter 4.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
86415	6	26	6	28	Mention the period for which projected changes are presented. Consistency between Chapter-8 and Chapter-4 in the assessment of projected changes in global precipitation and number of models used for the assessment is needed [Swapna Panickal, India]	Noted – In the revised ES, statements in ch8 and ch4 are consistent. In ch 8 we give more relevance to changes at seasonal level (instead of global annual mean as in ch 4).
89007	6	26		31	From what time period in the scenarios are the numbers included in this bullet? State near-, mid-, or long-term, or include dates. [Angeline Pendergrass, United States of America]	Accepted – The numbers are for the end of the century and this is now noted in the text.
12715	6	26			Clearly CMIP6 model numbers will be updated in time for the FGD and statements such as this modified accordingly. Likewise, for many locations later in the chapter, statements and figures pertaining to CMIP6 models will need to be updated to account for a greater number of input models. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The statistics and figures have been updated to CMIP6.
4803	6	27	6	29	In line 27 SSP5-8.5 is said to increase precip with another number than in line 29. Quite confusing. [Bart van den Hurk, Netherlands]	Noted – Line 29 is P-E, line 27 is precipitation only. Text was modified in the FGD and this phrasing has been changed.
112203	6	27			Replace but with and [Rutger Hofste, Netherlands]	Accepted – rephrased as suggested
20459	6	28	6	28	"compared to precipitation" is unclear. [philippe waldteufel, France]	Taken into account – Sentence now reads: 'Global continental runoff is projected to...'
15529	6	28	6	29	Re: the SSP1-2.6 scenario shows a much weaker increase in P-E of 1.5 +/-4.2 %, while for SSP5-8.5 shows a larger increase of 10.8 +/- 13.0 %. The projections cannot be found in the main text. Please check and revise as appropriate. [SAI MING LEE, China]	Accepted. The projection numbers are now provided in Table 8.1.
28599	6	28	6	30	The statement mentions a weaker increase in P-E, and refers to E as land "evapotranspiration". While E is defined as "evaporation" at line 16 on page 5. Does E stands for evaporation or evapotranspiration? Please make sure that the definition is clear troughout the manuscript. [Moctar Dembélé, Switzerland]	Noted – E stands for evaporation. Text has been modified in the FGD for clarity.
12717	6	28	6	31	For many of these statements, the plus/minus range could take the change well below zero. Is it fair to pose the sentence as an "increase" when the change is "1.3+/-4.8%", for example? Would a policy maker be able to interpret this? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account – the word "increase" was replaced by "change" in the FGD.
129081	6	28			Would it be more precise to refer to "projected increases or decreases" when the range is much greater than the mean? [Trigg Talley, United States of America]	Taken into account – the word "increase" was replaced by "change" in the FGD.
4805	6	29	6	30	An increase in precipitation in the first part of the sentence is suggested to lead to an increased soil moisture deficit risk, but this is not a trivial implication [Bart van den Hurk, Netherlands]	Taken into account – Sentence now reads 'It is virtually certain that evaporation will increase over the ocean, and very likely that evapotranspiration will increase over land, although this varies on a regional level'.
15531	6	30	6	31	Re: Global continental runoff is projected to increase by 1.3+/-4.8 % and 11.4+/-15.0 % for SSP1-2.6 and SSP5-8.5 respectively. The projections cannot be found in the main text. Please check and revise as appropriate. [SAI MING LEE, China]	Accepted. Numeric values of runoff projections are given in Table 8.1.
99085	6	32	6	32	I think the phrase "enhanced precipitation intensities" needs some elaboration. There have been century long trends toward a greater fraction of precipitation coming in the more intense rains and presumably these trends will continue with more and more of the rain coming in intense events that leads to a greater fraction running of, often causing high river flows, floods, erosion, etc.--and being of less value for agriculture (especially given that the incidence of low to moderate rainfalls tends to decrease)and requiring increased flood protection measures.. There is just a lot tied up in the seemingy innocuous three words, and I think elaboration is needed. [Michael MacCracken, United States of America]	Noted – Text unchanged for brevity here, fuller discussion in the text of the chapter of the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
111385	6	32	6	32	"SSP8-8.5" is likely a typo of SSP5-8.5 [Paul Durack, United States of America]	Accepted – corrected as suggested
20097	6	32	6	32	"and" should be replaced by a comma [philippe waldteufel, France]	Accepted – corrected as suggested
99087	6	35	6	36	The phrase "will occur by 2100" makes it sound as if some threshold exists and once past this the change will occur. Instead, this situation will develop through the 21st century, so I'd urge changing "occur by 2100" to "increase as the 21st century proceeds" [Michael MacCracken, United States of America]	Accepted – Text now reads 'It is very likely that shifts in the average regional and seasonal characteristics of the water cycle will occur regardless of the emissions scenario'.
18673	6	35	6	36	It may be good to highlight here that the wet regions will become wetter and dry regions will become drier on a ZONAL MEAN basis. [Govindasamy Bala, India]	Rejected – Here we aim to convey some of the regional detail.
17173	6	35	6	43	Improve grammar/phrasing to read: It is very likely that regional and seasonal changes in the water cycle will occur by 2100, with the magnitude of change increasing with the emissions scenario. Change 'robust signatures' to 'there is most confidence in'. Please add southern Australia to line 39. [Joelle Joelle Gergis, Australia]	Accepted – Text now reads 'It is very likely that shifts in the average regional and seasonal characteristics of the water cycle will occur regardless of the emissions scenario'. Inserted reference to Southern Australia also added.
51433	6	35	6	43	Thank you - it is very useful to see this discussion of where projected regional changes are robust or not. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted – thank you for the positive feedback
64887	6	36	6	36	Is this exactly linear indeed (“scaling”)? With what exact quantity of the emissions scenario? [Johannes Quaas, Germany]	Not applicable – text removed
51431	6	36	6	36	This sentence refers to emissions scenario, but the studies in 8.4.1 use concentration-driven projections which is not the same thing. Would it be more accurate to say "scaling with the level of global warming"? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable – text removed
112205	6	36			Why call it emissions scenario wheras SSP or RCP are used elsewhere? [Rutger Hofste, Netherlands]	Rejected – emission scenario is in common usage and is appropriate for simply phrasing a key summary statement
12719	6	36			Scaled to the emissions scenario? Would it not be better to state "scaled by GHG emissions"? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable – text removed
111387	6	40	6	40	"virtually certain.. TROPICAL mountain glaciers" this is likely a typo, glaciers exist outside of the tropics. Note that Ch9 includes the text "Glaciers: ..under future warming glaciers will lose mass (very high confidence)" [Paul Durack, United States of America]	Accepted – Clarified to read 'Seasonal snow cover and low-latitude and low-elevation mountain glaciers will diminish with increasing global warming levels (high confidence)'.
129083	6	40	6	40	"moutain" --> "mountain" [Trigg Talley, United States of America]	Accepted – corrected as suggested
129085	6	40	6	40	What are "tropical mountain glaciers"? How do these compare to mountain glaciers in more temperate regions? [Trigg Talley, United States of America]	Accepted – Clarified to read 'Seasonal snow cover and low-latitude and low-elevation mountain glaciers will diminish with increasing global warming levels (high confidence)'.
16111	6	40	6	40	" It is virtually certain that snow cover ...will diminish...": Annual maximum snow mass? Hemispheric extent (maximum, spring)? Duration? Or maybe there's no need to be more specific because the statement is true for all of these. [Gerhard Krinner, France]	Accepted – Clarified to read 'Seasonal snow cover and low-latitude and low-elevation mountain glaciers will diminish with increasing global warming levels (high confidence)'.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99089	6	40	6	41	I'd urge dropping "It is virtually certain that" and then at the end of the sentence add (very high confidence)--it would make the finding much clearer and get around having to indicate why a chance of less than 1 in 100 of something else happening needs to be indicated. The surrounding sentences are provided with confidence levels and this one should be too. [Michael MacCracken, United States of America]	Accepted – Rephrased to read 'Seasonal snow cover and low-latitude and low-elevation mountain glaciers will diminish with increasing global warming levels (high confidence)'.
59019	6	40	6	41	Mid-latitude glaciers are also predicted to retreat, not only tropical glaciers as mentionned in this sentence [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted – Clarified to read 'Seasonal snow cover and low-latitude and low-elevation mountain glaciers will diminish with increasing global warming levels (high confidence)'.
10157	6	41	6	43	It should be clarified what exactly is meant by "arid areas" and "drying" on these lines. From 8.4.1.8 it seems these words might be based on projected precipitation deficits and/or soil moisture deficits, but it would be better to be explicit. [Jacob Scheff, United States of America]	Accepted – Clarified to read 'However, increased evapotranspiration due to growing atmospheric water demand will dry soils in many water-limited regions, leading to pronounced aridification in semi-arid, winter rainfall-dominated regions such as the Mediterranean, western North America, south Africa, and southern Australia (high confidence)'.
59057	6	42	6	42	I request the authors to use the word "aridification" instead of "drying" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted – rephrased as suggested
7577	6	46	6	48	The way it is written might be confusing, to clarify, I would simply inverse the elements of the sentence: "In the tropics and in the summer extratropics of both hemispheres, the interannual variability of precipitation and runoff over land is projected to increase at a faster rate than changes in mean precipitation." [Celine Bonfils, United States of America]	Accepted – rephrased as suggested
17175	6	46	6	54	Remove 'on a range of timescales' from the summary statement. Mention specific regions in lines 50- 51. [Joelle Joelle Gergis, Australia]	Accepted – 'range of timescales' text removed in the FGD.
113137	6	49	6	49	Do you mean 'fewer rainy days' or 'less rainy days'? Because they do not mean the same thing and I think you actually mean the former not the latter… [Diego Miralles, Belgium]	Accepted – corrected as suggested
89127	6	49		51	These points are shown relatively clearly and concisely for CMIP5 simulations in Pendergrass et al (2017), which is not currently cited in the chapter. Pendergrass, A. G., Knutti, R., Lehner, F., Deser, C., & Sanderson, B. M. (2017). Precipitation variability increases in a warmer climate. Scientific Reports, 7(1), 17966. https://doi.org/10.1038/s41598-017-17966-y [Angeline Pendergrass, United States of America]	Accepted – thank you, paper now referenced in the text of section 8.4.1.3 in the FGD.
12721	6	49			Use fewer instead of less (or do you mean days on which less rain is received?) [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – corrected as suggested
20099	6	50	6	50	What is meant by "and"? [philippe waldteufel, France]	Accepted – corrected as suggested
22243	6	50	6	52	As written this sentence makes little sense to me. Even more strongly than what and in which areas? Without specificity this isn't really an actionable finding by policy makers. [Peter Thorne, Ireland]	Accepted – clarified to read 'Precipitation extremes are projected to increase even more strongly and in more regions than mean precipitation intensity, even where annual or seasonal mean precipitation is projected to decrease (medium confidence)'.
20101	6	54	7	1	Why is it necessary to duplicate the "high confidence" statement in this sentence? [philippe waldteufel, France]	Accepted – duplicate text removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51937	6	54	7	1	Revised text proposed: "There is (high confidence) that the seasonality of precipitation, runoff, streamflow and groundwater recharge will increase over many regions." [Richard Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account – Sentence now reads 'There is high confidence that the seasonality of precipitation, runoff, streamflow and water availability will increase with global warming in many regions, especially in the subtropical semi-arid regions and over the Amazonia.'
59021	6	54	7	2	High confidence is repeated twice in the sentence [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted – duplicate text removed
51435	6	54	7	4	It would be useful to see a remark on the expected relative importance of climate and non-climate anthropogenic influences on future streamflows here. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected – While non-climate anthropogenic influences on streamflow (e.g., water management, land use, etc.) are expected to be large, that is beyond the scope of WGI.
4815	7	1	200	50	Quite a few minor English language issues are present, careful proofreading is advised. A few are mentioned in comments below [Bart van den Hurk, Netherlands]	Noted – Text edited accordingly
79389	7	2	7	2	"will bring forward"? Not very clear what this means. Maybe "will make the timing of peak streamflow to occur earlier/sooner"? [Alejandro Di Luca, Australia]	Taken into account – Text now reads 'There is also high confidence of an earlier onset and more intensive spring snowmelt with higher peak flows at the expense of summer flows in snow dominated regions, but with medium confidence that reduced snow volume in low latitude regions will reduce the snowmelt flows'.
111513	7	4	7	4	Add Box 8.2 to list of reference sections. [James Renwick, New Zealand]	Accepted – Inserted as suggested
51947	7	4	7	4	insert: {8.3.1} to existing sections cited in parantheses [Richard Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – Inserted as suggested
6895	7	6	7	7	What is the key time horizon of relevance to this ES-statement? Are some of the conclusions still valid at the near term? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - The key time horizon of relevance is mostly the end of the 21st century (long term). Changes are not very clear for the near term.
65763	7	6	7	13	Suggest including a statement on the Australian monsoon, e.g.: "For the Australian monsoon there is low confidence in the magnitude or direction of mean rainfall change". [Kushla Munro, Australia]	Accepted – Inserted 'There is low confidence in the magnitude or direction of mean rainfall changes in the Australian monsoon'.
89129	7	7		13	Why is the North American monsoon listed first in this bullet point? It is listed in a different order in the sections. [Angeline Pendergrass, United States of America]	Rejected – the order doesn't alter the meaning of the sentence

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7579	7	8	7	8	I would add "total": "projections indicate a decrease in total precipitation" [Celine Bonfils, United States of America]	Accepted – rephrased as suggested
14977	7	9	7	10	"For the South American monsoon, the CMIP6 projections do not indicate a clear increase in precipitation during the 21st century" this sentence can be improved by the use of IPCC calibrated language [Juan Rivera, Argentina]	Accepted – Rephrased to read 'For the South American monsoon, there is no clear trend in the projected changes in total and extreme precipitation'.
22245	7	9	7	10	Do you instead mean a clear change? Lack of a clear increase leaves open the possibility of a decrease so it would be better to say no clear trend or similar surely here to avoid ambiguity? [Peter Thorne, Ireland]	Accepted – Rephrased to read 'For the South American monsoon, there is no clear trend in the projected changes in total and extreme precipitation'.
6897	7	10			With the statement "do not indicate a clear increase", why prejudice the statement towards an increase? It would be better to make this more neutral, i.e. "do not indicate a clear change in precipitation during the 21st century". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – Rephrased to read 'For the South American monsoon, there is no clear trend in the projected changes in total and extreme precipitation'.
6899	7	12	7	13	This last sentence of low confidence in projected changed to wet season onset and cessation (and thus, by deduction, the length of the west season) in many other tropical regions than the Sahel seems to directly contradict the opening sentence of this ES statement (line 7) in stating an overall extension of the length of the wet season. This needs to be thought about more carefully. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – Rephrased to read 'There is medium confidence that the monsoon season could be delayed in a warmer climate, especially in the Sahel and South America, but only low confidence in the projected changes in the wet season onset and cessation in many other tropical regions'.
113139	7	15	7	15	emission scenario' for 'emissions scenario' [Diego Miralles, Belgium]	Accepted – corrected as suggested
103671	7	15	7	16	Not clear why you want to convey with 'high confiendence' that the modeling output entails a wide range of outcomes? This weakens the statements further down in this paragraph [Philippe Tulkens, Belgium]	Accepted - The high confidence statement has been removed despite the evidence that the intermodel spread (e.g., in annual mean precipitation changes) is still large and has not much changed across multiple generations of climate models including CMIP5 vs CMIP6.
17181	7	15	7	29	I appreciate there is a slight technical difference between internal and natural variability, but as a summary statement for a policy maker audience, can we use the phrase 'natural variability' instead? Is it possible to highlight regions where the projections are most certain? [Joelle Joelle Gergis, Australia]	Taken into account. In sect 5 we used "internal climate variability", and the term is used consistently across the chapter
129087	7	15	7	29	[CONFIDENCE] The use of "high confidence" throughout this paragraph is opposite to the intended purpose of the usage. Start by rewording "For a given emissions scenario, variable model response and internal variability contribute to a substantial range in projections of water cycle changes, especially on regional scales (high confidence)" to something like "For a given emissions scenario, there is low confidence in water cycle changes on regional scales owing to variable model response and internal variability." Then recast the following sentences in a similar manner. [Trigg Talley, United States of America]	Noted. The statement has been revised including the confidence statements as agreed by IPCC rules
89009	7	15		17	Rather than "model response" and "model consensus," better wording here would be "variable response across models" and "strong consensus among models." [Angeline Pendergrass, United States of America]	Accepted - The revised ES highlights "variable regional water cycle projections across models".
17177	7	17	7	18	Amend lines 17-18 to read: Model deficiencies and unresolved small-scale processes still preclude a strong model consensus about future water cycle changes regardless of the scenario, time horizon or global warming level. [Joelle Joelle Gergis, Australia]	Rejected - It has been agreed that final chapter sections should be renamed "Final remarks" and should not focus on knowledge gaps that should be rather assessed across previous sections (mostly Section 8.5 in the case of Ch8).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
114865	7	17	7	18	Perhaps this phrase is more appropriate to be in the list of knowledge gaps than here. [Roxana Bojariu, Romania]	Rejected - It has been agreed that final chapter sections should be renamed "Final remarks" and should not focus on knowledge gaps that should be rather assessed across previous sections (mostly Section 8.5 in the case of Ch8).
87067	7	18	7	22	Needs reframing, specially from line 21. It is difficult the understand the whole sentence. [Tarul Sharma, Netherlands]	Accepted – Clarified to read 'Model response range is particularly large at the transition between wet and dry regions and seasons, and for soil moisture and freshwater reservoirs that are sensitive to small differences in precipitation or evapotranspiration changes. Poor representation of land surface processes or lack of consideration of land use change and irrigation can also lead to a variable response across models'.
6901	7	18			Remove "is" from the end of the sentence. It is not required for the text beginning "whatever the...", [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – corrected as suggested
59023	7	19	7	19	Using the word "strong" to refer to a model response range is unclear. I suggest "wide" or "large" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted – Amended to read 'large'
4807	7	19	7	19	"strong" -> "large" [Bart van den Hurk, Netherlands]	Accepted – Amended to read 'large'
12723	7	19			What is meant by a "strong" model response range? Does that mean a large spread? Better wording should be chosen. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – Amended to read 'large'
1241	7	22	7	24	The wording of the sentence "Internal climate variability strongly affects near-term water cycle responses..." is misleading since there is no evidence that the internal variability will diminish in the future. The situation is can be desribed as an externally forced signal that increases over time and will become stronger than the internal variability. The internal variability will also have a strong effect on the precipitation in the future, even in the presence of a long-term change. It's more a question about what mean level the internal variations fluctuate around and what amplitude they have. The uncertainty of the internal variability does not necessarily diminish even if it accounts for a smaller proportion of the total uncertainty (which grows over time). [Rasmus Benestad, Norway]	Taken into account - The emphasis was on the relative rather than absolute contribution of internal variability to total uncertainty in future climate. This has been clarified/simplified in the revised ES (and illustrated in revised section 8.5 by a telling Figure based on CMIP6 projections).
6903	7	22	7	24	It is good that internal variability has been considered in this ES statement. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted – thank you for the positive feedback

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89131	7	24		29	A technique that this statement seems to call for but is not addressed in the chapter is dynamical adjustment, where the contribution of internal variability to observed trends is removed from observations in order to reveal the forced response. Four studies where dynamical adjustment is applied to water cycle variables are Saffoti et al (2016), Lehner et al (2018, the results of this study are already cited in the chapter but the dynamical adjustment approach is not discussed), Guo et al (2019), and Sippel et al (2019): Guo, R., Deser, C., Terray, L., & Lehner, F. (2019). Human Influence on Winter Precipitation Trends (1921–2015) over North America and Eurasia Revealed by Dynamical Adjustment. Geophysical Research Letters, 46(6), 3426–3434. https://doi.org/10.1029/2018GL081316 Saffioti, C., Fischer, E. M., Scherrer, S. C., & Knutti, R. (2016). Reconciling observed and modeled temperature and precipitation trends over Europe by adjusting for circulation variability. Geophysical Research Letters, 43(15), 8189–8198. https://doi.org/10.1002/2016GL069802 Sippel, Sebastian, Nicolai Meinshausen, Anna Merrifield, Flavio Lehner, Angeline G. Pendergrass, Erich Fischer, Reto Knutti, et al. “Uncovering the Forced Climate Response from a Single Ensemble Member Using Statistical Learning.” Journal of Climate, May 28, 2019, JCLI-D-18-0882.1. https://doi.org/10.1175/JCLI-D-18-0882.1. Lehner, F., Deser, C., Simpson, I. R., & Terray, L. (2018). Attributing the US Southwest’s recent shift into drier conditions. Geophysical Research Letters. https://doi.org/10.1029/2018GL078312 [Angeline Pendergrass, United States of America]	Noted - Thanks for the suggestion. This technique has been assessed in Ch10 which focusses on methods and, therefore, does not deserve a specific statement in our ES (although the applications to water cycle variables are relevant within the relevant chapter sections)
12725	7	27	7	29	It should be remembered that using the agreement with long-term variability in observations is not a panacea for judging model future projections: at first order it will depend on if future forcing is of the same nature as forcing over the historical period, e.g. aerosols versus GHG in the monsoon. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted – comment, no response required
20103	7	31	7	31	"s" to be added to "change"? [philippe waldteufel, France]	Accepted – corrected as suggested
53135	7	31			Low-likelihood high-impact scenarios, including abrupt changes: such a broader subtitle could also encompass high-impact near-term regional scenarios where internal variability projects positively onto the forced response, as well as high-warming mid-to-long-term storylines as discussed for precipitation only in Section 4.8? [Hervé Douville, France]	Noted. The phrasing here has been revised in the final draft.
17179	7	33	7	34	Agreed terminology for the report is now 'low likelihood, high impact'. [Joelle Joelle Gergis, Australia]	Not applicable – text removed
79391	7	33	7	34	This is statement seems too broad and provides little to no information the way it is now. [Alejandro Di Luca, Australia]	Taken into account – Rephrased to read 'Uncertainties and past non-linear responses of hydrologic systems mean abrupt changes to the water cycle cannot be excluded'
53137	7	33	7	34	suppress "low probability" which is somehow redundant with the rest of the sentence but add a final "(low confidence)" warning? [Hervé Douville, France]	Not applicable – text removed
4809	7	34	7	35	abrupt change in what? [Bart van den Hurk, Netherlands]	Taken into account – Rephrased to read 'Uncertainties and past non-linear responses of hydrologic systems mean abrupt changes to the water cycle cannot be excluded'
88997	7	36		37	Rather than low *confidence* that abrupt changes in the water cycle will occur, should this be unlikely (page TS-6, footnotes) (with low confidence) that changes will occur? Or possibly "low likelihood, high impact" (Box SPM.1) [Angeline Pendergrass, United States of America]	Taken into account – Rephrased to read: 'It is unlikely that abrupt changes in aridification and rainfall will occur by 2100, although the possibility of abrupt events cannot be ruled out'
4811	7	37	7	37	"although ... Cannot be ruled out." This is already implied by the low confidence statement before (otherwise it would have been "virtually impossible") [Bart van den Hurk, Netherlands]	Rejected – We take the point on semantics, but we explicitly want to clearly state that abrupt changes cannot be ruled out.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58991	7	40	7	40	ITCZ needs defining. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted – Provided expansion of acronym for Intertropical Convergence Zone (ITCZ)
131557	7	40	7	40	explain "ITCZ" [Hans Poertner and WGII TSU, Germany]	Accepted – Provided expansion of acronym for Intertropical Convergence Zone (ITCZ)
6905	7	41	7	42	The use of the word "compounded" is confusing here. Normally it would mean exacerbated, but it is not clear what is being referred to. Is the "similar response of the water cycle" the southward shifting ITCZ amd thus weakened NH monsoons and strengthened SH monsoon - if so, therefore this signal would not be compounded by higher atmospheric CO2, it would be opposed. Something is wrong here. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account – Text rephrased to read '...would be influenced by the effects of higher atmospheric CO2'
12727	7	42			It is unclear what "compounded by" means in the context of this statement. Generally it means to make something that is already bad, worse. Is a stronger SH monsoon worse? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account – Text rephrased to read '...would be influenced by the effects of higher atmospheric CO2'
4813	7	43	7	44	the combination of an unlikely event with the very likely consequences of such an event is a bit strange for an Executive Summary Statement [Bart van den Hurk, Netherlands]	Rejected – This is a low likelihood, high impact scenario is of interest to a general readership so a clear statement about impacts on the water cycle is appropriate. Assessment of abrupt change was explicitly requested during the scoping stage of AR6 so this content is within the remit of this chapter
69215	7	46	7	47	In line 54-55, conditions that cause abrupt water cycle changes are clearly specified (“if solar radiation modification techniques are implemented rapidly or terminated abruptly”). Please add text on these conditions in Line 46-47. [Kaoru Magosaki, Japan]	Rejected – Adding further technical detail in the summary statement will reduce clarity of the statement. As noted by the reviewer, the implementation and termination detail is already provided further down in the same paragraph
69463	7	46	7	55	The meaning of this point is very unclear to me. What does it mean that it is "very likely" that solar radiation management "can" drive abrupt changes. Clearly if one were to immediately block 20% of the sunlight in a given region, there would be a pretty large abrupt change to the climate (including the hydrological cycle), but this is of no relevance to the future. What measures are being considered here? More generally, it does not make sense to me to put a likelihood estimate on a "can" statement. Clearly there are many things that "can" happen, given the right circumstances, but that does not make them partcularly likely over a given time horizon. From reading the relevant section, I guess the scenario considered here is rapid cessation of SRM? If that is the case, I would suggest explicitly specifiying that here. Also, the statement about the Amazon is not clear about the likliehood of abrupt change. Presumably this is being thought of as a low probability event? At present, it reads like the "very likely" in the bold sentence applies to the Amazon as well, and therefore abrupt change in the Amazon is "very likely", but the timing of this change has "low confidence". I am not sure that this is what is intended. [Martin Singh, Australia]	Taken into account. The phrasing on SRM has been modified to clarify the assessment and the text on the Amazon has been moved to a separate section in the Executive Summary.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99091	7	46	8	1	This analysis seems totally one-sided and unjustified. First, there is no indication of how much SRM is applied, it it built up slowly or done in one sharp increase, nothing about potential variations of implementation (e.g,, concetnratiions varying by latitutde),nothing about the temperature increase (or not) that would be aimed for, nothing about which scenario it is being applied to, nothing about how it might mesh with mitigation, and no statement about how the situation that results will compare without doing SRM, for which the world is being called up on to completely get rid of all fossil fuel use in a few decades, etc. And to say that all of this is "very likely" given all the uncertainties talked about with respect to the CO2-induced warmiing. The changes in the global water cycle would be back toward temperature and precipitation conditions that have been experienced in recent decades, etc. Just because model simulations have been done with very large changes to get good signal to noise does not mean that that is how SRM might be applied. I think this finding is just totaly inappropriate due to a complete lack of conctext, lack of a comparative analysis of the situation that we are headed toward without SRM (including about the sea level consequences and commitments of not limiting the temperature increase). I just see no justification for the assertive statements made here. Yes, there will be consequences, but their importance needs to be evaluated compared to the alternative, and the impacts of heading toward 3-4 C warming are very severe (wishful thinking about 1.5 C is just not a basis for such an evaluation here). [Michael MacCracken, United States of America]	Rejected. The assessment here that abrupt changes will very likely occur if SRM is implemented or stopped rapidly is rooted in the literature and consistent with the assessment in other chapters.
68435	7	46	8	2	Recommend changing solar radiation modification to solar radiation management to keep terms consistent across chapters. [Durwood Zaelke, United States of America]	Accepted – amended as suggested for consistency with other chapters
51437	7	46	8	47	Would SRM only induce abrupt changes if suddenly applied in a large way? And would a gradual ramping-up of SRM cause more gradual changes? Please clarify this here and suggest it would be clearer to discuss the abrupt changes and SRM in separate paragraphs. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The assessment here pertains specifically to rapid implementation or cessation of SRM.
87069	7	49	7	50	Can snowpack changes in watercycle be considered as abrupt? [Tarul Sharma, Netherlands]	Noted – Yes, discussion of snowpack is provided in section 8.6.2
64889	7	49	7	50	This sentence is very difficult to understand. Are these feedbacks, and thus in consequence the water cycle changes, happening or not? Where and when? what exactly? [Johannes Quaas, Germany]	Taken into account. This section of the Executive Summary has been rephrased to make the assessment clear.
64891	7	51	7	51	“can facilitate” – a probability or indeed an assessment is necessary [Johannes Quaas, Germany]	Taken into account. This section of the Executive Summary has been rephrased to make the assessment clear.
3165	7	52	7	53	In addition to Amazon deforestation and warming climate (already mentioned), the observed increase of dry season length in southern Amazon is a key factor for amazon tipping point. [Jhan Carlo Espinoza, France]	Noted – The text already states: 'Continued Amazon deforestation, combined with a warming climate…'
51439	8	1	8	1	Suggest it would be more appropriate to say the impacts of SRM "would" affect different regions etc, rather than "will", as there is no certainty that SRM will be deployed. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – corrected as suggested
79657	8	12	8	12	Could include following reference (Chaudhary etal. 2020 and 2017) Glob Change Biol. 2020; 00: 1– 15. https://doi.org/10.1111/gcb.15099; https://onlinelibrary.wiley.com/doi/abs/10.1111/gcb.15099 [Nitin Chaudhary, Sweden]	Rejected -This line # does not exist in page 8

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51441	8	25	8	26	The first line is a little obscure and complex compared to the lines that follow. It is unclear whether the key message is that near surface relative humidity is declining or that the precipitation response to warming over land is lower than over the ocean. Please make this clearer/separate these messages. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected -This line # does not exist in page 8
103673	8	36	8	38	"There is… that abrupt changes….will occur…, the possibility of abrupt changes cannot be ruled out". Redudancy: Consider revising sentence [Philippe Tulkens, Belgium]	Rejected -This line # does not exist in page 8
43761	8	40	8	40	Please add "in some latitudes" at the end of the sentence. [Francisco Tapiador, Spain]	Rejected -This line # does not exist in page 8
81133	8	46	8	48	This is a tautological statement. Suggest revision. [Mary Matthews, Azerbaijan]	Rejected -This line # does not exist in page 8
81135	8	48	8	51	Need to reference institutionalization of IWRM to enhance sustainable use [Mary Matthews, Azerbaijan]	Rejected -This line # does not exist in page 8
22255	9	1	15	26	The chapter introduction is considerably longer than any other chapter I have read so far (I am going chronologically). Several parts should not be there (see specific comments) but overall the introduction should be considerably shorter. [Peter Thorne, Ireland]	Rejected – AR6 is the first to feature a stand-alone chapter on the water cycle. Our introductory material is brief, and already significantly shortened since the FOD. This comment runs counter to our previously approved chapter structure other reviewer comments e.g. comment 12739 which states 'This has been an excellent summary of the findings from previous reports'. We have a simple difference of opinion here, so choose to not edit the text even further to retain important contextual information for this new stand-alone chapter on the water cycle. Nonetheless, we have made an effort to reduce the length of section 8.1.3.2
130537	9	5	15	26	Too weak on role of water cycle on climate system change. Line 27, page 10, Subsection title could be "…..in the climate system". [Panmao Zhai, China]	Accepted - The title of the subsection has been modified accordingly and two paragraphs have been added to emphasize the interactions of the global water cycle with the energy and carbon cycles.
65765	9	7	9	18	Suggest citing the earlier publications to emphasise the depth of knowledge. Currently, very recent publications are being cited for long held facts. [Kushla Munro, Australia]	Rejected – Note that AR6 is building off previous IPCC reports so focuses on literature published since 2013. it is also an assessment and not a review. Nonetheless, we have now added reference to Bates (2008).
130531	9	7	10	34	In line 7-8, page 9 saying "yet saline ocean water accounts for around 97% of total water availability" but in line 34 page 10 using "The ocean is the primary water reservoir on Earth (96.2%)". Why no the same number? [Panmao Zhai, China]	Accepted – changed to read 96.6% as per Figure 8.1
105545	9	8	9	9	saline gorundwater' ??? Is this saline groundwater or saline ocean? Please double cehcek! [Sanjiv Kumar, United States of America]	Rejected – Saline groundwater is correct
59089	9	8	9	10	Please add in line 9 that the remainder (saline groundwater) is 1%, even if this number is obvious (as the saline ocean water is 97% and the freshwater 2%). However, besides saline groundwater it could also be brackish surface water on land, which should also be mentionned. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted – Amended to read saline groundwater and saline lakes (1.6%) as per Figure 8.1
89013	9	8			This contradicts page 10 Line 34 which says that the ocean is the reservoir for 96.2% of water on Earth. Perhaps they can be reconciled by adding total *liquid* water availability here? [Angeline Pendergrass, United States of America]	Accepted – changed to read 96.6% as per Figure 8.1

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12731	9	9			Change to, "and the remainder "is" primarily explained by..." [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – Amended to read: '...and the remainder is primarily explained...'
7399	9	10	9	10	the proportion of glaciers/ice caps is wrong. The sentence could read: "Ice sheets, glaciers and snow pack and groundwater make up 99% of freshwater resources…" In fig 8.1. it is implied that fossil groundwater would not belong to freshwater, since it is combined with saline groundwater - yet, fossil groundwater is not neccessarily saline (but of course some is). Reference for the numbers: https://www.usgs.gov/media/images/distribution-water-and-above-earth [Nils Moosdorf, Germany]	Taken into account – corrected all figures to match Figure 8.1
27207	9	10	9	13	All this does not sum up to 100% and it is not clear what the 'this very small fraction' refers to: the 3% of easily accessible freshwater? Are the 3% part of the 96%? The way the sentence is written is confusing [Eric Brun, France]	Accepted – corrected all figures to match Figure 8.1
132069	9	12	9	15	Precision of the number 835 thousands and 205 thousands need check in because they are absent in the publication cited as a reference Abott et al. [Mourad Amara, Algeria]	Accepted – corrected all figures to match Figure 8.1
67619	9	12	9	16	Global water demands D have the unit of flux (km3/year), while the cited global freshwater amount A has the unit of km3. The units do not match. The amount can “meet the global demands” for only a limited period of time T = A/D of the order of ten years. As it currently stands, the text appears ambiguous. Suggestion: “Although the natural cycling rate of this amount is theoretically enough to meet global needs…” [Antonio Nobre, Brazil]	Accepted – Amended to read: 'Although the natural cycling rate of this amount is theoretically enough to meet global human and ecosystem needs, there are large geographical and seasonal differences that influence the availability of freshwater to meet regional demands'
6699	9	13	9	13	Is "renewable" needed or indeed appropriate here? If the rate of abstraction of groundwater is too large, groundwater will not be fully renewed. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – edited as suggested
12733	9	13			In "835 thousand" why is a combination of numbers and words used? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Editorial – format will be correct during final copyedit
7397	9	14	9	14	says "thousands km3" should be "thousand km3" [Nils Moosdorf, Germany]	Accepted – edited as suggested
13543	9	15	9	16	Global water demands and regional demands are refered to human or human and environmental needs? [Maria Amparo Martinez Arroyo, Mexico]	Accepted – Amended to read: 'Although the natural cycling rate of this amount is theoretically enough to meet global human and ecosystem needs, there are large geographical and seasonal differences that influence the availability of freshwater to meet regional demands'
59029	9	18	9	19	Following the topic sentence for this paragraph on line 18, a sentence or two about the importance of freshwater to our global ecosystems is needed. Freshwater ecosystems are considered biodiversity hotspots (Strayer et al. 2010; Dudgeon et al. 2006; Hawksworth et al. 1995) and viewed as sentinels (or canaries in the coal mine) with respect anthropogenic stress (Woodward et al. 2009; Perkins et al. 2010). A sentence or two highlighting the importance of freshwater on an ecological level would be beneficial. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected – this section is dealing with human demands on freshwater resources. Literature cited here is pre- 2013 so is outside the scope of AR6 literature assessment window

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
132075	9	18	9	19	I suggest to add this confirmed status between line 16 and line 18 as a new paragraphe : [Mourad Amara, Algeria]	Rejected – Ambiguous statement, no specific suggestion possible
13545	9	18	9	24	Beyond that water supports economic activities, it is important to highlight that water supports any type of life on earth [Maria Amparo Martinez Arroyo, Mexico]	Rejected – we agree water is important for ecosystems, but this paragraph is specifically dealing with human demands. The first sentence of the chapter (section 8.1.1.1) already states 'Water is vital to all life on Earth' so we have already acknowledged the importance of water to the biosphere
13547	9	18	9	24	It would also be relevant to indicate that these economic activities and human demands force the transfer of water between basins whose environmental effects are not yet fully known. [Maria Amparo Martinez Arroyo, Mexico]	Rejected – this statement is ambiguous. No literature cited to support suggested edit
22247	9	18	10	24	While all true this is also all the domain of WGs 2 and 3 and not WG1. To avoid accusations of scope overreach this may be best removed. This is nothing to do with the physical science basis and I doubt that there should be one and a bit pages of overreach into other WGs domains. It would be better to replace with a paragraph pointing out in what chapters of WGs 2 and 3 these issues are further discussed and pointing out that the present chapter is limited to the physical science basis. [Peter Thorne, Ireland]	Rejected – this is introductory material for a stand alone chapter on water cycle changes. Removing three sentences of contextual information will not impact the work of WG 2 or 3, but will remove the framing material needed for this chapter on physcial processes.
105723	9	19	9	21	to be added: UNESCO World Water Assessment programme, 2015: United Nations World Water Development report 2015: water for a sustainable world, UNESCO, Paris [Abou Amani, France]	Rejected – sufficient references to two major peer-reviewed review papers are already given
43753	9	20	9	21	Please add other industrial uses which are even more intensive in water use, such as paper mils and chemistry. I suggest: " (...) irrigated agricultural crops, through to industrial processes including chemical factories, paper mills, the generation of hydroelectricity and the cooling of thermoelectric power plants (Bates et al., 2008; Schewe et al., 2014). [Francisco Tapiador, Spain]	Rejected – paper mills and chemistry (?) are not cited in Schewe et al 2014 or Bates (2008). No specific literature cited to support this suggested edit
43755	9	21	9	21	Water used in cooling thermoelectric power plants can be reused. I suggest looking for a better, stronger example. [Francisco Tapiador, Spain]	Not applicable – Sentence now deleted
1243	9	21	9	22	This sentence is not clear as it stands (needs to confer the reference): "For example, power generation accounts for 88% of the water withdrawals and 36% of water consumption in the energy sector". [Rasmus Benestad, Norway]	Not applicable – Sentence now deleted
67871	9	21	9	23	There is a need to clarify the claims of high water consumption in the plant as in the following statement: "For example, power generation accounts for 88% of the water withdrawals and 36% of water consumption in the energy sector (D'Odorico et al., 2018)" Given the data The World Bank shows that the agricultural sector is 70% users of water resources as in the following link https://blogs.worldbank.org/opendata/chart-globally-70-freshwater-usedagriculture [Ruandha Agung Sugardiman, Indonesia]	Not applicable – Sentence now deleted
79393	9	21	9	23	I don't understand the sentence "For example, power generation accounts for 88% of the water withdrawals and 36% of water consumption in the energy sector" [Alejandro Di Luca, Australia]	Not applicable – Sentence now deleted
64893	9	21	9	23	What else does “energy sector” cover if not power generation? i.e. where are the other 12%? [Johannes Quaas, Germany]	Not applicable – Sentence now deleted
33051	9	22	9	22	By shifting from wet cooling to dry air cooled condensor systems in power plants, it seems that the provided data needs to be revised. [Sahar Tajbakhsh Mosalman, Iran]	Not applicable – Sentence now deleted
32721	9	22	9	22	By shifting from wet cooling to dry air cooled condensor systems in power plants, it seems that the provided data needs to be revised. [sadegh zeyaeyan, Iran]	Not applicable – Sentence now deleted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51939	9	24	9	24	Delete word: "stores" [Richard Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – edited to read 'storage'
17183	9	25	9	25	Change ' water pools' to ' water stores' [Joelle Joelle Gergis, Australia]	Accepted – edited to read 'storage'
59025	9	25	9	25	I suggest changing the word pool to more commonly used "reservoir" or "storage" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted – edited to read 'storage'
4817	9	26	9	26	"double" -> "twice the" [Bart van den Hurk, Netherlands]	Accepted – Amended to read: '...equivalent of twice the global groundwater recharge...'
40081	9	30	9	30	Define 'water scarcity' in the glossary, defveloping the definition in partnership with WGII. [TSU WGI, France]	Noted – Our definition of water scarcity already appears in the glossary of Working Group I to provide clarity for other parties
105725	9	30	9	31	to be added: UNESCO World Water Assessment programme, 2018: United Nations World Water Development report 2018: nature based solutions for water, UNESCO, Paris [Abou Amani, France]	Rejected – sufficient references to two major peer-reviewed review papers are already given
32917	9	31	9	31	add: "preservation of " environmental flows... [Tomasz Walczykiewicz, Poland]	Accepted – edited as suggested
39121	9	31	9	31	« ultimatelty » should be replaced by « ultimately » [Jean-Louis Bonne, France]	Accepted – edited as suggested
105727	9	34	9	35	to be added: UNESCO World Water Assessment programme, 2014: United Nations World Water Development report 2014: water and energy, UNESCO, Paris [Abou Amani, France]	Rejected – sufficient references to two major peer-reviewed review papers are already given
43759	9	35	9	35	Please add the following reference, which specifically addresses hydroelectricity. The sentence will read: (Tapiador et al., 2011; Schewe et al., 2014; D'Odorico et al., 2018; Djehdian et al., 2019). The reference is: Tapiador, F.J., Hou, A. Y., de Castro, M., Checa, R., Cuartero, F., and Barros, A.P. 2011. Precipitation estimates for hydroelectricity. Energy & Environmental Science, DOI:10.1039/C1EE01745D. The work is relevant to the topic and needed. [Francisco Tapiador, Spain]	Rejected – we already cite several recent review papers that cover this topic. Suggested paper was also published outside the post 2013 assessment of the literature covered by AR6
129089	9	39			"exports" to "imports"? [Trigg Talley, United States of America]	Accepted – Amended to read: '..agricultural exports or imports'
116695	9	41	9	41	The report could build on SROCC, SR15 and SRCCL for the introduction. [Valerie Masson-Delmotte, France]	Noted – Findings of special reports already appear in section 8.1.2.3
43757	9	41	9	42	Please consider adding Tapiador et al., 2016 after Jiménez Cisneros et al., 2014a. That would strenghen the sentence. The sentence would read: The impacts of climate change on society are primarily experienced through changes to the global water 42 cycle (Jiménez Cisneros et al., 2014a; Tapiador et al., 2016). The reference is: Tapiador, F.J., Behrangi, A., Haddad, Z.S., Katsanos, D., de Castro, M. 2016. Disruptions in Precipitation Cycles: Attribution to Anthropogenic Forcing. Journal of Geophysical Research (Atmospheres). Vol: 121, Pages: 2161–2177, DOI: 10.1002/2015JD023406. [Francisco Tapiador, Spain]	Rejected – this is an assessment not a review paper where we can cite long lists of literature for every sentence. The citation already provided is for the relevant IPCC AR5 chapter which contains a synthesis of the appropriate literature
112207	9	41	9	51	Water scarcity is in most cases primarily driven by socioeconoic factors rather than physical [Rutger Hofste, Netherlands]	Noted – this is implicit in the topic sentence in line 30

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
74355	9	42	9	46	I would like to suggest to add ' climate change effects on water security will present significant or higher risks to global security in the next few decade(s) (The World Climate and Security Report, 2020)' [Yulizar Yulizar, Indonesia]	Noted – we already refer to food security and economic prosperity, which are primarily the domain of WGII.
88999	9	46		48	This sentence brings up multiple concepts and connects them in a way that does not seem logical to me. One concept is the relationship between the amount of water leading to impacts - I think this would be better phrased "Having too much water can lead to flooding and having too little can lead to drought." A separate concept is how precipitation changes with warming, and two separate aspects are mentioned - its intensity and its variability. Changes in precipitation intensity are a concern for increased flooding with warming, but not for drought, which is an indication that this sentence needs untangling. Precipitation variability, though, is expected to increase with warming and that would affect both having too much and not enough precipitation (over a relevant period of time). [Angeline Pendergrass, United States of America]	Accepted – Amended sentence to read: 'Having too much or too little water can increase the likelihood of flooding or drought, as precipitation variability increases in a warming climate'.
89001	9	46		48	The two citations for this sentence are other IPCC reports. These are very broad citations, and they do not indicate to the reader where specifically to find supporting evidence, whereas citations to primary literature would be expected to. This would need citations to document the connection between too much water and flood impacts, too little water and drought impacts, changes in intensity with warming, and changes in variability with warming. One of those, increasing precipitation variability with warming in CMIP5 projections, is documented in Pendergrass et al., (2017) Pendergrass, A. G., Knutti, R., Lehner, F., Deser, C., & Sanderson, B. M. (2017). Precipitation variability increases in a warmer climate. Scientific Reports, 7(1), 17966. https://doi.org/10.1038/s41598-017-17966-y [Angeline Pendergrass, United States of America]	Rejected – Sufficient references to two IPCC synthesis reports are already given. Note that ten references by this reviewer are already cited in the chapter.
21017	9	47	8	47	likElihood [Marcelo Barreiro, Uruguay]	Accepted – edited as suggested
87071	9	47	9	47	The term "likelihood" is mis-spelled. [Tarul Sharma, Netherlands]	Accepted – edited as suggested
4819	9	47	9	47	typo in "liklihood" [Bart van den Hurk, Netherlands]	Accepted – edited as suggested
13549	9	47	9	47	Replace likelihood instead of liklihood [Maria Amparo Martinez Arroyo, Mexico]	Accepted – edited as suggested
113143	9	47	9	47	liklihood' [Diego Miralles, Belgium]	Accepted – edited as suggested
113145	9	47	9	47	Revise logic in this sentence. [Diego Miralles, Belgium]	Accepted – Amended sentence to read: 'Having too much or too little water can increase the likelihood of flooding or drought, as precipitation variability increases in a warming climate'.
103675	9	47	9	47	This is the only line in the introductoin where floods ar ementoined. The whole intro is quite biased towards 'water scarcity' whereas the summary equally focusses on floods and droughts/scarcity [Philippe Tulkens, Belgium]	Noted – Section 8.2.3.2 and 8.3.1.5, 8.4.1.5 and FAQ 8.2 all cover aspects of flooding.
64337	9	47			likelihood -> check spealling [CRISTINA Prieto, Spain]	Accepted – edited as suggested
43155	9	47			Read " the likelihood of flooding " rather than " the liklihood of flooding " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted – edited as suggested
53139	9	47			likelihood [Hervé Douville, France]	Accepted – edited as suggested
74137	9	48	9	50	If there isn't a citation here being used to justfy the value judgement of "insufficient investment" then I'd leave it as the much broader, more objective, "Water scarcity is not only driven by physical processes, but is also influenced by water management infrastructure, policy, and technology." [Daniel J. Short Gianotti, United States of America]	Accepted – Amended to read: 'Water scarcity is not only driven by physical processes, but is also influenced by water management infrastructure, policy, and technology.'

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32919	9	49	9	49	add: "or innapropriate" investment in water management… [Tomasz Walczykiewicz, Poland]	Not applicable – text no longer appears
13551	9	49	9	50	It's not only insufficient investment in water management infrastructure and technology but also some countries commonly have water use, consumption and discharge systems of one use, which are viewed as a linear production [Maria Amparo Martinez Arroyo, Mexico]	Noted – no revision necessary
53141	9	50	9	51	Rather WGII, remove and/or move into ES as an implication of Section 8.4 key findings ? [Hervé Douville, France]	Accepted – text deleted
54473	9	53	10	1	The definition of water security of UN-WATER (2013) does not enjoy international consensus, so its inclusion and mention is problematic. It is highlighted that this definition initiated the securitization of the water agenda in the institutional sphere of the United Nations Organization. By contrast, for many Member States, water is not a source of political or military insecurity but primarily a vector for cooperation between nations. As an alternative proposal, it is suggested to use the definition of water security in the Strategic Plan for the Eighth Phase (2014-2021) of the UNESCO Intergovernmental Hydrological Program, which reads as follows: "the capacity of a population to safeguard access to adequate quantities of water of acceptable quality for sustaining human and ecosystem health on a watershed basis, and to ensure efficient protection of life and property against water related hazards -- floods, landslides, land subsidence, and droughts." (PHI-UNESCO, 2012, p. 5, available: https://unesdoc.unesco.org/ark:/48223/pf0000218061). [Maria del Pilar Bueno Rubial, Argentina]	Rejected – we are using a UN definition. This is the sole reviewer to challenge its use.
105729	10	2	10	5	to reference: UNESCO, UN-Water, 2020: United Nations World Water Development report 2020: water and climate change, UNESCO, Paris, [Abou Amani, France]	Rejected – Several peer-reviewed papers are already provided to support this statement
59009	10	2	10	5	Mention not only precipitation changes as main driver but also changes in glacier runoff and snowmelt (i.e., the crucial contributions of mountaineous water resources to downstream regions) (see and cite Hock et al., (2019). Chapter 2: High Mountain Areas (Tech. Rep.). High Mountain Areas. In: IPCC Special Report on the Ocean and Cryosphere in a Chang- ing Climate) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted – edited as suggested
113147	10	3	10	3	hydoclimate' (revise this whole section, it is well written but may have a few typos that slipped through) [Diego Miralles, Belgium]	Accepted – edited as suggested
12735	10	3			Typo: replace "hydoclimate" with "hydroclimate" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – edited as suggested
109681	10	7	10	24	Relationships between water scarcity, climate change, and increasing demand can be complex and lead to worse impacts than might be expected. In particular, it should be noted in this passage that increasing water demand, due for example to population and ecnomic growth, gives nonlinear increases in the sensitivity of water scarcity to climate change; these nonlinearities will result in a potentially unexpected rapid acceleration, as time progresses, in the impacts of climate change upon freshwater availability. See Fleming, 2016, Demand modulation of water scarcity senitivities to secular climatic variation, Hydrological Sciences Journal, 61, 2849-2859. [Sean Fleming, United States of America]	Noted – this chapter is focused on physical process rather than social factors addressed in WGII. The content here is a brief contextual summary for the chapter and is not intended to be an exhaustive review of the complete literature. Instead, we cite summaries from AR5, IPCC Special Reports and a recent review paper
43775	10	9	10	9	I suggest appending the following reference after Cisneros et al., 2014a: Navarro et al., 2019. The reference is: Navarro, A.; Moreno, R.; Jimenez-Alcazar, A.; Tapiador, F. J. 2019. Coupling population dynamics with earth system models: the POPEM model. ENVIRONMENTAL SCIENCE AND POLLUTION RESEARCH. 26,2019. ISSN 0944-1344. DOI: 10.1007/s11356-017-0127-7 [Francisco Tapiador, Spain]	Rejected – The content here is a brief contextual summary for the chapter and is not intended to be an exhaustive review of the complete literature. Instead, we cite summaries from AR5, IPCC Special Reports and a recent review paper

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
74139	10	9	10	10	This is framed in the typical, "Developed world blames the developing world" manner where the finger is pointed only at population growth and not at changes in consumption per capita (which is not necessitated by these citations). I'd recommend, "Future population and per capita water use increases are expected..." [Daniel J. Short Gianotti, United States of America]	Accepted – Amended to read: 'Future population and per capita water use increases are expected…'
74141	10	9	10	11	The phrase, " that have exhibited changes related to observed warming of the climate system since the mid-20th century" is very confusing. I'm not following the point of the sentence. Consider revising or just removing that phrase. [Daniel J. Short Gianotti, United States of America]	Accepted – Clarified to read: 'Future population and per capita water use increases are expected to exacerbate pressure on global water resources and associated food production as global warming continues'
112093	10	10	10	10	exaccerbate should be exacerbate [Kosuke Yamamoto, Japan]	Accepted – edited as suggested
59031	10	14	10	16	It may be worth mentioning and introducing the notion of contaminants entering freshwater reserves as a function of glacier/ice/snow-pack melting. This is an additional threat to water security, and can pose new challenges to water treatment processes. Although it is not a non-climatic driver, it is a direct result of increasing temperatures, and in some areas, hydroclimatic changes. There is a body of literature that looks at these concerns ranging from Canadian icefields (Blais et al. 2001; Canadian Ice Core Laboratory) to the Himalayans (Sharma et al. 2015). It may be worth a mention here, and then expanded on in Chapter 9 (9.3/9.4). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected – while we agree that this is an interesting and important topic, it is beyond the scope of the chapter to deal with minor aspects of freshwater security. Our focus is on physical process associated with the global water cycle
51941	10	14	10	16	Revised text proposed: "In AR5, climate change was projected to reduce renewable surface water and groundwater resources significantly in most dry subtropical regions of the world, intensifying competition between agriculture, ecosystems, human settlements and industry for water resources (Jiménez Cisneros et al., 2014a). New observational evidence {8.3.1.7.4} challenges, however, suggested reductions in renewable groundwater resources in drylands by climate change alone." [Richard Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - Given the new evidence (which is however assessed in Section 8.3 and is not supposed to appear in the introduction) and for the sake of brevity, the whole sentence has been removed.
27209	10	16	10	16	There is only reference that is cited for many very important statements. Those statements should be supported by many more references in an IPCC report [Eric Brun, France]	Rejected – the reference provided is to an IPCC AR5 chapter which contains hundreds of references
103677	10	18	10	20	yes scarcity is a huge issue, but an additional sentence on the success of access to drinking over the last 20 years (since Johannesburg 2000) must be mentioned as well [Philippe Tulkens, Belgium]	Rejected – Access to drinking water is outside of the domain of WG1 which is focused on the physical processes. The reference to water scarcity is intended to provide brief context for our chapter, not providing an exhaustive assessment of socio-economic factors addressed in WG2
105731	10	20	10	22	to reference: UNESCO, UN-Water, 2020: United Nations World Water Development report 2020: water and climate change, UNESCO, Paris, [Abou Amani, France]	Noted – The reference to water scarcity is intended to provide brief context for our chapter, not providing an exhaustive assessment of socio-economic factors addressed in WG2. We already provide appropriate references to IPCC synthesis reports which contain thousands of peer-reviewed papers
116697	10	23	10	23	since AR5 and also SR [Valerie Masson-Delmotte, France]	Accepted – edited as suggested
51443	10	24	10	24	Please refer readers here to the forthcoming AR6 WG2 Chapter 4 on "Water" for discussion of impacts, adaptation and vulnerability in relation to changes in the water cycle (in addition to the cross-reference on page 15 line 26) [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – edited as suggested

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
113155	10	27	11	3	Please add to this section: 'The consideration of atmosheric water transfers between river watersheds (Keune and Miralles, 2019) and countries (Keys et al., 2017) is crucial for an effective global water governance and to secure freshwater availability worldwide'. Keune, J. and Miralles, D. G.: A Precipitation Recycling Network to Assess Freshwater Vulnerability: Challenging the Watershed Convention, Water Resour. Res., 52(18), 10,757–15, doi:10.1029/2019WR025310, 2019. Keys, P. W., Wang-Erlandsson, L., Gordon, L. J., Galaz, V. and Ebbesson, J.: Approaching moisture recycling governance, Global Environmental Change, 45, 15–23, doi:10.1016/j.gloenvcha.2017.04.007, 2017. [Diego Miralles, Belgium]	Rejected – This falls more in the domain of Working Group 2 not the physical processes addressed here in Working Group 1
59091	10	29	10	40	It would make sense to add a reference time or period for the mentioned numbers, e.g., that ice sheets, glaciers, snow and ice on surface and permafrost represents 2.2% of the planet's water. This number might change in future and was also different in the past. Especially as ice reservoirs, glaciers, snow cover and permafrost are diminuishing in the future... [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted – Amended to read: 'Solid terrestrial water that occurs as ice sheets, glaciers, snow and ice on the surface and permafrost currently represents 2.2% of the planet's water '
89011	10	30			"into" should be "among" [Angeline Pendergrass, United States of America]	Accepted – edited as suggested
89017	10	31		38	It's not clear to me why an entire previous IPCC report is cited 3 times in the same paragraph, without specifying where within the report each figure can be found, and without citations to any other work. [Angeline Pendergrass, United States of America]	Noted – IPCC report contains thousands of individual references related to these statements
74143	10	34	10	38	Is the precision of 96.2%, 1.8%, and 2.2% worth keeping if they add up to more than 100% (recognizing that they come from different estimation sources)? Not a huge problem, but maybe just saying "approximately 96%" would simplify things? [Daniel J. Short Gianotti, United States of America]	Accepted – all percentages amended to match Figure 8.1
89015	10	34			This contradicts page 9 line 8 which says that saline ocean water accounts for 97% of water availability. Perhaps this can be reconciled by specifying total *liquid* water availability on page 9 line 8? [Angeline Pendergrass, United States of America]	Accepted – all percentages amended to match Figure 8.2
6701	10	35	10	35	"areas covered by ice" might be better than "sections partly covered by ice". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – Amended to read: '...but also includes areas covered by ice in polar regions'
87073	10	37	10	37	The term "solid" can be changed to "condensed" as solid terrestrial water looks very vague. [Tarul Sharma, Netherlands]	Rejected – We mention water in solid form in line 30. Solid is more appropriate for a broad audience as does not lose its scientific meaning
89019	10	42			The sentence only describes the flows of water to and from the atmosphere, which we saw earlier in this section make up a small fraction of total water. Better wording would be: "The atmospheric branch of the planet's water cycle" [Angeline Pendergrass, United States of America]	Rejected – The sentence refers to evaporation and precipitation of moisture from the Earth's surface, which includes non-atmospheric elements already mentioned in lines 29-30 and Figure 8.1
113149	10	43	10	43	evapotranspiration' is not 'associated with biological processes'; 'transpiration' is the term you are looking for. [Diego Miralles, Belgium]	Accepted – Amended to read: '...including transpiration associated with biological processes'.
74145	10	43	10	46	This does not need any citations, much less two. This is standard primary school science. [Daniel J. Short Gianotti, United States of America]	Noted – recall we are writing for a diverse, non specialist audience. No changes necessary
74147	10	43	10	48	These two sentences are duplicative (and very basic -- I'm not sure you need any citations between the two of them). Both sentences make the point that transpiration supplies water to the atmosphere. I'd recommend one sentence that reads something like, "The water constituting soil moisture and river flow are supplied by precipitation, itself supplied by atmospheric water vapor, which in turn comes from the evaporation and sublimation of water from oceans, ice, the land surface, and plant transpiration." [Daniel J. Short Gianotti, United States of America]	Rejected – Suggested edit is more convoluted than the text as currently written. Recall we are writing for a diverse, non specialist audience not other scientists

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51943	10	44	10	44	Insert text as follow: "supplying soil moisture, groundwater recharge, and river flows," [Richard Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – Amended to read: 'supplying soil moisture, groundwater recharge. and river flows…'
113151	10	45	10	45	or in some areas was generated over land '. Not really: precipitation is always a mix of different sources (land/ocean evaporation). Even in regions of strong continental influece, the mean fraction of land-origin precipitation rarely exceeds 50%. [Diego Miralles, Belgium]	Noted – statement as written is correct. Recall we are writing for a diverse, non-specialist audience
6705	10	45	10	47	This sentence is confusing. I think "have all moved poleward" needs to be replaced by "have occurred". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – Amended to read: 'In terms of large-scale atmospheric circulation, the AR5 concluded that it is likely that the widening of the tropical belt, a poleward shift of storm tracks and jet streams, and a contraction of the northern polar vortex have all occurred since the 1970s'
6703	10	46	10	46	"evapotranspiration" on this line should be changed to "evaporation". "Transpiration" is covered by the following sentence. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – edited as suggested
1245	10	46	10	46	The chapter needs to add information about the global area of precipitation: E.g. "The statistical nature of precipitation depends on the ratio between the global area of evaporation, A_e, and the area of precipitation, A_p, since it does not rain everywhere and all the time. For daily precipitation, A_p has been estimated to be of the order 23-25% over the area of the Earth (Benestad, 2018; DOI: 10.1088/1748-9326/aab375). The mean precipitation intensity is expected to increase at a rate that is proportional to A_e/A_p times the global mean rate of evaporation." [Rasmus Benestad, Norway]	Noted – This is material has been addressed in Section 8.2 and 8.3 rather than in the introduction.
113153	10	47	10	47	This statement is only correct in absolute terms, but I think you wanted to highlight where transpiration matters the most for precipitation in the tropics: 'transpiration from vegetation contributes to atmospheric water vapour, especially in tropical regions'. In tropical regions, most rain comes from ocean evaporation, even if transpiration is high. Perhaps this reference can guide you: van der Ent, R. J., Wang-Erlandsson, L., Keys, P. W. and Savenije, H. H. G.: Contrasting roles of interception and transpiration in the hydrological cycle; Part 2: Moisture recycling, Earth Syst. Dynam., 5(2), 471–489, doi:10.5194/esd-5-471-2014, 2014. [Diego Miralles, Belgium]	Noted – statement as written is correct. Recall we are writing for a diverse, non-specialist audience
7581	10	48	10	48	I am lost here. keep: "the latent heat released by condensation of atmospheric water vapour" but remove sublimation of atmopsheric water vapour (sublimation is the transition of a substance directly from the solid to the gas state, without passing through the liquid state, requiring energy, the oppoite is deposition). [Celine Bonfils, United States of America]	Accepted – Amended to read: 'The latent heat released by condensation of atmospheric water vapour and evaporative processes…'
74149	10	48	10	51	I'm surprised this needs a citation, much less two, and also that it appears to have been discovered in the past decade by the references selected. [Daniel J. Short Gianotti, United States of America]	Noted – statement as written is correct. Recall we are writing for a diverse, non-specialist audience.
89021	10	49			"and evaporative processes over land" doesn't seem to fit with the rest of the sentence [Angeline Pendergrass, United States of America]	Taken into account – sentence now clarified to read 'The latent heat released by condensation of atmospheric water vapour and evaporative processes …'

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53143	10	53	10	54	Specify here that changes in ocean salinity are however mostly assessed in CH9 (rather than CH8)? [Hervé Douville, France]	Accepted – Amended to read: 'When assessing changes to the global water cycle, it is important to consider the vast reservoir of salt water stored in the world's ocean (Figure 8.1), assessed further in Chapter 9'.
89023	10	53			The sentence starts by talking about the "global water cycle," but then goes on to on talk about the surface liquid component of this. Perhaps the sentence could be changed to, "When assessing changes to the global water cycle, it is important to consider the vast reservoir of salt water stored in the world's ocean (Figure 8.1)." [Angeline Pendergrass, United States of America]	Accepted – Amended to read: 'When assessing changes to the global water cycle, it is important to consider the vast reservoir of salt water stored in the world's ocean (Figure 8.1), assessed further in Chapter 9.
93643	10	55	11	1	Delete from "along with" to "processes". After "(Stocker et al. 2013), add: Besides, cryosphere and land–ocean processes that discharge freshwater may alter stratification, influencing temperature dynamics at the coastal front (Materia et al., 2012). ref. Materia, S., Gualdi, S., Navarra, A., & Terray, L. (2012). The effect of Congo River freshwater discharge on Eastern Equatorial Atlantic climate variability. Climate dynamics, 39(9-10), 2109-2125. [Stefano Materia, Italy]	Taken into account – Amended to read: 'Movement of freshwater between the atmosphere and ocean, along with continental runoff (including ice-sheet calving)…'
20105	11	6	11	13	Figure 8.1 is beautiful and quite useful. Mind that the "total water on Earth" number at the top on the right side is erroneous. It is probably 1 380 000 [philippe waldteufel, France]	Taken into account – correct value is 1 380 thousand km3, Figure 8.1 has been corrected
93581	11	6	11	13	The ice on Fig. 8.1 is continental ice and not sea ice as shown on the figure. The depth of the ocean on the figure is totally excessive, for no apparent reason [Jean-Louis Dufresne, France]	Taken into account – Figure 8.1a is simply annotated as 'ice'. The style of the figure is to enhance interpretation, it is not drawn to scale
29041	11	8			Figure 8.1: a recently published estimate finds a value of 3 thousand km^3 of seasonal snow in the northern hemisphere which, assuming only a small contribution from the southen hemisphere, is consistent with the value in Figure 8.1 but with a much smaller uncertainty (+-1%) than quoted in the figure (+-20%) which could be reassessed. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account – seasonal snow listed as 3 +/- 15%
20461	11	9	11	9	There is no (Zhou et al., 2019b) item in the reference list [philippe waldteufel, France]	Taken into account - Zhou et al (2019) is listed in the reference list: Zhou, Y.Q., A.H. Sawyer, C.H. David, and J.S. Famiglietti, 2019: Fresh submarine groundwater discharge to the near global coast. Geophysical Research Letters, 2019GL082749, doi:10.1029/2019gl082749.
105547	11	16	13	34	why summary of AR5 and special report are here? Can a reader not read directly the AR5 or special report ? A better title can be 'kowneldge gaps indetfied in the AR5 and special reports, and how AR6 is planning to fill it or attempt to address it. [Sanjiv Kumar, United States of America]	Rejected – A summary of AR5 and special reports is specifically required for this stand alone chapter of water cycle changes which is new in AR6
22249	11	16			These precursor assessment findings would be better being placed as the opening to each substantive assessment section rather than here. This is what all other chapters in chronological sequence up to 8 have done and it is a bit jarring to suddenly have a chapter handle the issue of context setting distinctly in this manner. Separating the prior assessment from your substantive assessment also is unhelpful to the reader because the context within which your new assessment is being performed is then unclear without dashing backwards and forwards over numerous pages. This section also takes up a disproportionate amount of space as written. [Peter Thorne, Ireland]	Noted – Given AR6 is the first to have a dedicated chapter on the water cycle, our approach is to provide a very brief summary of AR5 and Special Reports in section 8.1.2, and then provide further specific detail in subsequent sub-sections of our chapter
20107	11	18	11	18	Dedicated rather than dedicating [philippe waldteufel, France]	Accepted – Amended to read: '...a chapter specifically dedicated to providing an integrated assessment of the global water cycle changes, by building on many chapters from previous reports'.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53145	11	18			dedicated [Hervé Douville, France]	Accepted – Amended to read: '...a chapter specifically dedicated to providing an integrated assessment of the global water cycle changes, by building on many chapters from previous reports'.
89025	11	18			dedicating should be dedicated [Angeline Pendergrass, United States of America]	Accepted – Amended to read: '...a chapter specifically dedicated to providing an integrated assessment of the global water cycle changes, by building on many chapters from previous reports'.
89027	11	19			comma after changes [Angeline Pendergrass, United States of America]	Accepted – Amended to read: '...a chapter specifically dedicated to providing an integrated assessment of the global water cycle changes, by building on many chapters from previous reports'.
112095	11	21	11	21	1.5C should be 1.5°C [Kosuke Yamamoto, Japan]	Editorial – will be checked again during final copyedit
21019	11	25	13	34	In view of my first comment maybe section 8.1.2.1 can be removed? Also, most of the summary included here is later mentioned in individual subsections. [Marcelo Barreiro, Uruguay]	Noted – Given AR6 is the first to have a dedicated chapter on the water cycle, our approach is to provide a very brief summary of AR5 and Special Reports in section 8.1.2, and then provide further specific detail in subsequent sub-sections of our chapter
53147	11	27			thereby emphasizing the lack of reliable global observations and the low signal to noise ratio as major obstacles for detecting earlier hydrological changes? [Hervé Douville, France]	Rejected – this is a very brief summary of AR5 findings. We are just focusing on summarising key AR5 findings here, not providing details about uncertainty estimates
87075	11	28	11	32	Even though the data is sufficient, then why is it medium confidence on whether heavy precipitation over land regions has intensified or not. [Tarul Sharma, Netherlands]	Rejected – this is a very brief summary of AR5 findings, detailed reasons for medium confidence are found in Stocker et al 2013 cited in this paragraph
22251	11	32	11	32	What are these {} pointing to? If a precursor assessment this will be highly confusing to a reader because the only context they are used in here are current chapter ES statements to tie those statements back to the underlying assessment. [Peter Thorne, Ireland]	Editorial – this punctuation points to other relevant AR6 chapters that provide extra detail
53149	11	34			although the near surface moistening has abated over land in recent years leading to a fairly widespread decrease in relative humidity (medium confidence). [Hervé Douville, France]	Rejected – this is a very brief summary of AR5 findings. We are just focusing on summarising key AR5 findings here, not providing secondary details
27211	11	36	11	36	Are we really talking about reduction and not increase? [Eric Brun, France]	Rejected – A reduction is correct. AR5 Section 2.5.3 states: 'In summary, there is medium confidence that pan evaporation continued to decline in most regions studied since AR4 related to changes in wind speed, solar radiation and humidity'.
113171	11	45	11	47	Revise grammart in this sentence. It is not the the widening, shift and contraction that moves polewards. [Diego Miralles, Belgium]	Accepted – Amended to read: 'In terms of large-scale atmospheric circulation, the AR5 concluded that it is likely that the widening of the tropical belt, a poleward shift of storm tracks and jet streams, and a contraction of the northern polar vortex have all occurred since the 1970s'
20109	11	45	11	47	"all moved poleward" should be rephrased [philippe waldteufel, France]	Accepted – Amended to read: 'In terms of large-scale atmospheric circulation, the AR5 concluded that it is likely that the widening of the tropical belt, a poleward shift of storm tracks and jet streams, and a contraction of the northern polar vortex have all occurred since the 1970s'

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12737	11	45	11	47	This sentence is not worded well. What does it mean for a widening of the tropical belt to have moved polewards? It would be better to say "have been exacerbated" which would work for all elements of the sentence. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – Amended to read: 'In terms of large-scale atmospheric circulation, the AR5 concluded that it is likely that the widening of the tropical belt, a poleward shift of storm tracks and jet streams, and a contraction of the northern polar vortex have all occurred since the 1970s'
69465	11	47	11	47	suggest "all have moved poleward" --> "all have occurred" [Martin Singh, Australia]	Accepted – Amended to read: 'In terms of large-scale atmospheric circulation, the AR5 concluded that it is likely that the widening of the tropical belt, a poleward shift of storm tracks and jet streams, and a contraction of the northern polar vortex have all occurred since the 1970s'
70981	11	50	11	50	"partly" sounds like less than half, and is then surely a gross understatement. "mainly" (meaning more than half) would be far more consistent with the literature considered in AR5. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – edited as suggested
83891	11		11		(under 8.1.2) The special IPCC reports SROCC and SRCCL are not in References. [Ulf Molau, Sweden]	Editorial – will be checked again during final copyedit
53151	12	8	12	9	may be a more logical order would be: water vapour, precipitation, surface evaporation, runoff and snowpack? [Hervé Douville, France]	Accepted – Amended to read: 'Water cycle projections in the AR5 were considered primarily in terms of water vapour, precipitation, surface evaporation, runoff, and snowpack'
7197	12	8	12	18	This section mainly refers to Collins et al 2013 work. Perhaps other studies can be cited. For example the increase in preciptiation is largest over Antarctica and Greenland. Based on Behragni and Richardson: Behrangi, A., and M. Richardson (2018), Observed High-Latitude Precipitation Amount and Pattern and CMIP5 Model Projections, Remote Sensing, 10(10), 1583. SEE FIG 6 in that paper [Ali Behrangi, United States of America]	Rejected – Collins et al 2013 is the relevant chapter in AR5 (chapter 12). Citing individual post AR5 studies in this AR5 summary is not appropriate
86417	12	9	12	10	Chapter-4 assess the global-mean precipitation very likely to increase as global surface air temperature increases over the 21st century under all five SSPs. [Swapna Panickal, India]	Noted – this is a summary of AR5 findings, not CMIP6 results reported in AR6
4821	12	10	12	10	Are these references (12ES, ...) referring to AR5? [Bart van den Hurk, Netherlands]	Editorial – this punctuation refers to relevant sections of AR5, not AR6. Will be made consistent in the final edit
22253	12	10	12	10	What are the parentheses referring to? Our chapter 12 or that of AR5? Unclear. Comment applies throughout this sub-section. [Peter Thorne, Ireland]	Editorial – this punctuation refers to relevant sections of AR5, not AR6. Will be made consistent in the final edit
16113	12	11	12	12	"the most extreme emissions scenario" - RCP2.6, or the later added RCP1.9, are also extreme scenarios, they are just on the other end of the spectrum [Gerhard Krinner, France]	Accepted – Amended to read: ' …under the under the RCP8.5 emission scenario…'
81137	12	13	12	18	Good clear explanation! This is what is needed! [Mary Matthews, Azerbaijan]	Noted – thank you
53153	12	20	12	21	may be a more logical order would be: water vapour, precipitation, surface evaporation, runoff and snowpack? [Hervé Douville, France]	Accepted – Amended to read: 'Water cycle projections in the AR5 were considered primarily in terms of water vapour, precipitation, surface evaporation, runoff, and snowpack'
3671	12	21	22	12	were the near-surface relative humidity reductions over many land areas projected to be likely increasing or decreasing, with medium confidence? [Jiafu Mao, United States of America]	Rejected – the text is clear about a reduction in near surface relative humidity: 'Near-surface relative humidity reductions over many land areas were projected to be likely, with medium confidence'
16115	12	27	12	27	As far as I remember, that was a conditional likelihood, assuming continuing global warming. [Gerhard Krinner, France]	Accepted – edited to read: 'Decreases in Northern Hemisphere snow cover were assessed as very likely with continued global warming'

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68203	12	34	12	34	Also refer to Ch2 and Ch12 that have global and regional assessements? [Guðfinna Aðalgeirsdóttir, Iceland]	Rejected – this section provides a summary of AR5 findings not AR6. Note that line 34 contains no text so this statement is ambiguous
12739	12	36	13	34	This has been an excellent summary of the findings from previous reports. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted – thank you
53155	12	38	12	39	suppress [Hervé Douville, France]	Accepted – Sentence deleted
81139	12	38	12	39	Suggest adding a summary para here that explains implications of these findings for lay person. [Mary Matthews, Azerbaijan]	Rejected – Unclear what is being requested here. We already provide a three paragraph summary of post AR5 special reports in section 8.1.2.3
11105	12	41	12	41	It's better to have a formal name of SR 1.5 reopert here, as the way for SROCC and SRCCl on page 13. [Wen Wang, China]	Accepted – edited to read: ' The Special Report on Global Warming of 1.5 °C (SR1.5)…'
130543	12	41	13	3	Please avoid overlap with Chapter 11 on extreme precipitation and dorught. [Panmao Zhai, China]	Rejected – this content is a very brief summary of material related to assessing water cycle changes covered in chapter 8. AR6 is the first report to have a stand alone chapter on the water cycle, so minor overlaps with other chapters are unavoidable. Removing content with reduce the current cohesion and stand-alone structure of our chapter. It is perfectly acceptable for minor overlaps of content in different AR6 chapters
8667	12	46	12	46	I would mention here that changes in drought in the Mediterranean are driven by observed increase in the atmospheric evaporative demand since precipitation changes are not evident in the region for the long term (see Ch. 11) [Sergio Vicente-Serrano, Spain]	Rejected – this is a summary of the special reports, not new AR6 content provided in chapter 11
112209	12	47			Not sure what the half degree rate adds. [Rutger Hofste, Netherlands]	Noted – ambiguous statement, no changes made
53157	12	49			than available? [Hervé Douville, France]	Accepted - the sentence has been splitting in two parts and the second part has been edited to read as: "Nonetheless, the differences between each warming level may require larger ensembles of simulations and more multi-model consensus than available for the SR1.5 to be discernible at the regional scale."
16117	13	5	13	19	It might be worth stating explicitly that SROCC did not assess non-polar, non-mountain snow changes (because that was beyond the scope of the report) [Gerhard Krinner, France]	Accepted – edited to clarify polar focus of SROCC: 'The Special Report on the Ocean and Cryosphere in a changing Climate (SROCC) provides a comprehensive assessment of recent and projected changes specifically in polar snow and ice over land areas that form a key component of the water cycle in high-elevation and high-latitudes areas'.
64895	13	5	13	34	In my opinion, it would be better to only refer to the appropriate Chapters in SROCC, rather than citing individual papers that SROCC assessed (i.e. drop the specific references and replace them with the reference to the SROCC assessment). Same for the two specific references in the report about SRCCL [Johannes Quaas, Germany]	Noted – We now also provide references to the complete SROCC and SRCCL, but also maintain our reference to specific papers for a maximum traceability of our assessment.
59027	13	6	13	6	I suggest removing the word "polar". The SROCC covered snow and ice in tropics, mid latitude and polar regions. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59005	13	11	13	16	Provide references for each statement, e.g., for permafrost Walvoord and Kurylyk, 2016, doi:10.2136/vzj2016.01.0010, or Lawerence et al, doi:10.1029/2005GL025080; for glaciers Shannon et al, doi:10.5194/tc-13-325-2019; for river runoff Blöschl et al, doi10.1038/s41586-019-1495-6 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account – For the sake of brevity and given the fact that the assessment of changes in the cryosphere is rather the focus of Chapter 9, we however only provide reference to full report: IPCC, 2019: IPCC Special Report on the Ocean and Cryosphere in a Changing Climate [H.-O. Pörtner, D.C. Roberts, V. Masson-Delmotte, P. Zhai, M. Tignor, E. Poloczanska, K. Mintenbeck, A. Alegría, M. Nicolai, A. Okem, J. Petzold, B. Rama, N.M. Weyer (eds.)]
87363	13	18	13	18	Caesar et al. (2018) are not showing this. This is not the appopriate reference.This statement is more coming from IPCC SROCC report, chapter 6.7 [Didier Swingedouw, France]	Accepted – changed reference to Collins et al (2019) to refer to SROCC chapter 6
113173	13	25	13	25	Correct 'twice than'. 'twice as much as' [Diego Miralles, Belgium]	Accepted – Amended to read: '...has risen nearly twice as much as global average temperature…'
20111	13	25	13	25	Add "more" after "twice" [philippe waldteufel, France]	Accepted – Amended to read: '...has risen nearly twice as much as global average temperature…'
113175	13	26	13	26	Not sure the connection between land surface temperature and dry climates has been made clear. [Diego Miralles, Belgium]	Taken into account – sentence now clarified to read '…with an increase in dry climates…'
43157	13	26			Read "leading to an increase in dry climates " rather than "leading to an in increase in dry climates " [Cyriaque Rufin Nguimalet, Central African Republic]	Taken into account – sentence now clarified to read '…with an increase in dry climates…'
113177	13	29	13	30	in THE hydrological cycle' [Diego Miralles, Belgium]	Accepted – Amended to read: ...in the hydrological cycle'
53159	13	29	13	30	The direction and magnitude of hydrological changes induced by land use change and land surface feedbacks vary with location and season (high confidence)? [Hervé Douville, France]	Accepted – Amended to read: 'The direction and magnitude of hydrological changes induced by land use change and land surface feedbacks vary with location and season (high confidence)'
131559	13	31	13	32	Not only "increased urbanisation" can intensify extreme rainfall over cities or downwind and but also normal urbanisation. Also the statement becomes very vague combining "can intensify" with a medium confidence level. The original statement in chapter 2 in the SRCCL is much clearer: "Urbanisation increases extreme rainfall events over or downwind of cities (medium confidence)" It's also unclear why there is only "medium confidence" since the effect of increased precipitation over cities has beeen studied for a long time. Liu, Jie, and Dev Niyogi. (2019) had analyzed more than 489 papers on the topic and included 85 studies in their meta-analysis. (Liu, Jie, and Dev Niyogi: Meta-analysis of urbanization impact on rainfall modification." Scientific reports vol. 9,1 7301. 13 May. 2019, doi:10.1038/s41598-019-42494-2) [Hans Poertner and WGII TSU, Germany]	Accepted – edited to read: 'Urbanisation increases extreme rainfall events over or downwind of cities (medium confidence)'. We are reported summary results from SRCCL so no extra citations are needed here
1247	13	34	13	34	Could also cite some of the findings from the Snow-Water-Ice-Permafrost in the Arctic (SWIPA) by the Arctic Monitoring and Assessment Programme (AMAP) under the Arctic Council. [Rasmus Benestad, Norway]	Noted – recall we are summarising SRCCL key findings here, not providing new AR6 assessment
105549	13	46	13	46	deeper -> comprehensive [Sanjiv Kumar, United States of America]	Accepted – Amended to read: 'This has enabled a more comprehensive evaluation…'
20113	13	48	13	48	"gases" rather than "gas", perhaps [philippe waldteufel, France]	Accepted - Amended to read: '...effects of greenhouse gases and aerosol emissions…'
103679	13	49	13	49	twentieth century: consider revising : should also including first two decades of present century [Philippe Tulkens, Belgium]	Accepted – Amended to read: 'effects of greenhouse gases and aerosol emissions since 1850.'

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12741	13	55			A cross reference could be made to Section 10.3.3 on added value. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – edited sentence to read: 'The added value of increased resolution in global or regional climate models can be also assessed more thoroughly based on dedicated model intercomparison projects (see Section 10.3.3 for further discussion)'.
111505	14	6	14	6	Remove acronym WCC, it is not used subsequently. [James Renwick, New Zealand]	Accepted – Amended to read: '...water cycle changes and to consider climate change…'
14979	14	6			The acronym for water cycle changes (WCC) was defined but not used in the chapter. I suggest to remove it [Juan Rivera, Argentina]	Accepted – Amended to read: '...water cycle changes and to consider climate change…'
53161	14	8			suppress "and evapotranspiration"? [Hervé Douville, France]	Taken into account - The revised sentence reads as: "This chapter responds to the growing need to have a focussed assessment of water cycle changes and to consider climate change from the perspective of its effects on water availability rather than only precipitation, and on freshwater resources (streamflow and soil moisture, but also recharge groundwater, wetlands and lakes).
32921	14	9	14	9	add: "recharge", wetlands… [Tomasz Walczykiewicz, Poland]	Accepted - Amended to read: '...but also recharge groundwater, wetlands and lakes'
93587	14	11	14	12	major volcanic eruptions are not part of the multiple anthropogenic drivers [Jean-Louis Dufresne, France]	Accepted – Amended to read: ' This includes not only emissions of greenhouse gases but also different species of aerosols, land and water management, and the plausible deployment of deliberate radiation modification techniques'.
53163	14	13	14	15	Rather use a passive form: Former paradigms are reconsidered/revisited, such as... This list may need to be consistent with the key findings of the chapter and link to the ES? [Hervé Douville, France]	Rejected – Active sentence construction uses fewer words and is easier for the reader to understand
129091	14	15	14	17	Incomplete sentence fragment. [Trigg Talley, United States of America]	Accepted – Amended to read: '...and the stability of geographical patterns of water cycle changes…'
64897	14	15	14	17	a verb is missing [Johannes Quaas, Germany]	Accepted – Amended to read: '...and the stability of geographical patterns of water cycle changes…'
89029	14	15		17	This sentence is a fragment and should be combined with the previous sentence [Angeline Pendergrass, United States of America]	Accepted – Amended to read: 'In particular, alterations in the physical processes associated with hydrological extremes of droughts, heavy rainfall and floods are assessed here and in Chapter 11'.
113181	14	16	14	16	At this stage I am starting to sense the level of overlap with Chapter 11 is going to be great. There should be an effort to avoid that overlap… [Diego Miralles, Belgium]	Accepted – Clarified Chapter 8's focus on the physical processes: 'In particular, alterations in the physical processes associated with hydrological extremes of droughts, heavy rainfall and floods are assessed here and in Chapter 11'.
20463	14	16	15	14	We have here 3 successive approaches for answering to the question "What is this chapter going to be about?", with the last one finally sketching the plan. Although each approach is interesting, circulating between them is a bit tedious; moreover, some repetitions cannot be avoided [philippe waldteufel, France]	Taken into account - For the sake of brevity, Sections 8.1.3.1 and 8.1.3.2 have been merged and shortened.
3673	14	18	14	18	please change “CO2” into “CO2”. [Jiafu Mao, United States of America]	Editorial – will be checked again during final copyedit

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51445	14	18	14	18	Use subscript for 2 in CO2 [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Editorial – will be checked again during final copyedit
17185	14	19	14	19	Agreed terminology for the report is now 'low likelihood, high impact'. [Joelle Joelle Gergis, Australia]	Accepted – Amended to read: '...considerable emphasis is also placed on assessing 'low likelihood, high impact' climate trajectories…'
22257	14	23	15	26	This section says several things fairly repeatedly and could be considerably shortened without any loss of meaning and this would increase accessability. [Peter Thorne, Ireland]	Taken into account - For the sake of brevity, Sections 8.1.3.1 and 8.1.3.2 have been merged and shortened.
113183	14	25	14	25	past, recent' sounds wrong to me. Equal to a hypothetical 'upcoming and future'. [Diego Miralles, Belgium]	Accepted – Amended to read: '...past, present and future water cycle changes…'
89031	14	26		28	The "fast" adjustment and "slow" temperature-dependent parts of the precipitation response are clearly separated in time for idealized forcings like we apply in abrupt CO2 increase experiments. But in the real world and forcing scenarios that emulate it, forcing changes continuously, so the adjustment to forcing evolves at the same timescale as the forcing, and this is not separated in time from the changes in temperature. Perhaps it would be better to omit "fast" and "slow" here, otherwise the context needs to be explained. [Angeline Pendergrass, United States of America]	Rejected - Even if forcing changes continuously, the response has multiple timescales.
103681	14	32	14	32	what is 'climate mediated'? [Philippe Tulkens, Belgium]	Taken into account - Parentheses content was not clear enough and has been removed for the sake of brevity.
5477	14	36	14	36	after extratropical cyclones): add "and intense water vapor transports like atmospheric rivers" [Jinwon Kim, United States of America]	Accepted – Amended to read: 'e.g. monsoons, storm tracks, tropical and extratropical cyclones, and intense water vapor transports like atmospheric rivers'
113185	14	38	14	38	I would make this clear earlier and several times. [Diego Miralles, Belgium]	Noted – Added additional reference to Chapter 11 in lines 16-17: 'In particular, alterations in the physical processes associated with hydrological extremes of droughts, heavy rainfall and floods are assessed here and in Chapter 11'
16119	14	40	14	40	Probably you are aware that some readers will interpret the word "theory" as "just a vague, unsubstantiated idea based on myth and fairy dust"… but probably this is not the place to explain what "theory" means to a scientist [Gerhard Krinner, France]	Accepted - "theory" has been replaced by "physical understanding"
12743	14	40			A cross-reference could be made to Ch10 for multiple lines of evidence (section 10.5.4). [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - This is a general IPCC strategy that is not specific to Ch10.
20465	14	43	14	44	Process understanding is indeed the key to knowledge; no scientist should disagree. Are they parts of this WG1 report where this opinion is not totally shared? The very need to emphasize the importance of process understanding raises doubts. [philippe waldteufel, France]	Rejected - What is obvious to climate scientists may be less obvious to the wider IPCC audience
51447	14	48	14	48	The sentence here says "emissions scenarios" but the chapter discusses results from concentration-driven scenarios, which are not the same thing - the uncertainties in climate-carbon cycle feedbacks discussed in chapter 5 show that a single emissions scenario can result in a range of differnt concentration pathways, and vice versa. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - "emissions" has been replaced by "concentration"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16121	14	48	14	48	Low CO2 emission scenarios need not be low aerosol scenarios, and aerosols do play an important role for precipitation changes. Should this already be mentioned here? [Gerhard Krinner, France]	Rejected - Relevant but not needed in this introduction.
107707	14	49	14	51	20 years is relatively short and period contains extreme 1997-98 El Niño [Emily Collier, Germany]	Noted – no revision necessary
12745	14	49			Change "specified otherwise" to "otherwise specified" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – Amended to read:'…unless otherwise specified…'
89033	14	51		52	I don't find "model response uncertainty" to be an intuitive descriptor of the uncertainty breakdown described in Box 4.1, in part because the word "response" is extraneous. I think a better descripition could be "uncertainty breakdown into model, scenario, and internal variability components". It's perhaps also useful to cite Hawkins and Sutton (2011) - I did not make the connection until looking at Box 4.1. [Angeline Pendergrass, United States of America]	Rejected - The wording is consistent with the framing in Ch1
103683	14	52	14	54	not entirely clear: you mean that, unformtunately, we sometimes only have 1 model to support our conclusions? Ot that the spread across different moidels is often too high to conclude with 'high confidence'? [Philippe Tulkens, Belgium]	Taken into account - The sentence was not clear enough, was not really needed and has been removed for the sake of brevity.
53165	14	52	14	54	Remove or clarify? Should it be: Storylines of conditional water cycle changes will be used to highlight high-impact scenarios where and when the forced water cycle response is still model-dependent and there is no simple alternative to the one-model-one-vote methodology ? Regional low-likelyhood high-impact hydrological scenarios could indeed deserve a stronger emphasis, beyond the abrupt changes assessed in Section 8.6. [Hervé Douville, France]	Taken into account – text now removed
53167	15	1	15	3	Rephrase? Abrupt changes (Section 8.6) only represent one category of low-likelihood high-impact scenarios and what cannot be excluded but could have high impacts or deep implications may also need to be further considered in other chapter sections? [Hervé Douville, France]	Taken into account - The revised sentence reads "Low likelihood but physically plausible high-impact scenarios are also assessed, especially the potential for abrupt climate changes (Section 8.6)." More attention has also been paid on plausible LLHI scenarios in the revised Section 8.4
17187	15	2	15	2	Agreed terminology for the report is now 'low likelihood, high impact'. [Joelle Joelle Gergis, Australia]	Accepted – Amended to read: 'We also consider low like likelihood, but physically plausible…'
12747	15	2			It is good that high impact but low likelihood, physically plausible events are being considered. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted – thank you
4823	15	5	15	6	Increases of 7% with constant RH is quite a trivial statement, and leads to confusion as to what atm vapor increase can actually be expected [Bart van den Hurk, Netherlands]	Rejected - This section (8.2) is on the physical understanding of the water cycle response and this statement is only trivial for climate scientists
81141	15	17	15	26	Recommend add this to the intro in chpt 1 as this cross cutting aspect of CC will also impact all DMs views [Mary Matthews, Azerbaijan]	Noted – Paragraph serves as a summary best placed at the end of section 8.1.3
53169	15	17			suppress the first line (redundant with a former paragraph)? [Hervé Douville, France]	Accepted - The sentence has been removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12749	15	19			It is good to be upfront about the potential overlaps explicitly, as is done here. It is not intended that chapters entirely avoid overlap since a chapter needs to exist as a coherent document in its own right. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted – thank you!
93589	15	20			is water really a major topic of mitigation policies? [Jean-Louis Dufresne, France]	Rejected - Mitigation strategies based on BECCS or afforestation heavily depend on freshwater resources
103685	15	29	15	29	Not sure this title is reflecting the content of this section: it is about the physical understanding of the global to regional drivers of water cycle changes [Philippe Tulkens, Belgium]	Noted - we decided on a narrative approach to Chapter 8 section titles which was agreed at the FOD and SOD stages so the term expected is meant to imply it is based on physical understanding
4851	15	29	15	29	General: structure of section 8.2 is very good [Bart van den Hurk, Netherlands]	Noted - thank you for your positive comment!
22259	15	29			Much of this section feels like it is proverbially putting the cart before the horse. A lot of the support for the findings arises from subsequent sections and this feels a little uncomfortable as the support for the principal findings in large parts rests upon an assessment the reader is yet to address and understand. Is it not better to build to these findings rather than start with these findings and then have the reader back engineer the support for the findings from latter sections? The tip of the pyramids were the last thing to be placed not the first after all. [Peter Thorne, Ireland]	Noted - we agreed our chapter structure as framing (8.1), physical expectations (8.2), understanidng observed changes (8.3), projections (8.4), challenges (8.5), abrupt changes (8.6) at the FOD stage and consider that this our narrative approach is distinct and compliments other chapters. Section 8.2 deals with physical expectations evidenced by fundamental phsyics underpinning simple to complex models and supported by observational evidence. This primarily focuses on idealised modelling, experiments and observations that illucidating physical processes. Of course, it is impossible and nonsensical to avoid completely evidence provided by long term observations and modelling of future changes. These are to some extent touched on in earlier chapters as well as being dealt with in leater sections but this overlap is kept to a minumum and was a consideration in further refinements to this section, the structure of which was appreciated by other reviewers.
38063	15	31	15	31	"The tight coupling between …. Forms a robust physical basis …": Land-atmopshere coupling has a seasonlity, which means that the tight LA coupling is during a specific season/episode not a general condition. This can mislead readers. Please either remove *tight* and *robust* or specify a typical season when the LA coupling is tight. [Junhee Lee, Republic of Korea]	Not applicable - this paragraph has been removed
20467	15	31	15	31	Is it possible to get some insight about the content of the indicated tight coupling? For example, according to figure 7.3, latent heat transferred to the atmosphere through evaporation amounts to about 82 W/m2, to be compared to the 340 W/m2 incoming solar energy. Is this the tight coupling the report is invocating? A reference would be welcome. [philippe waldteufel, France]	Not applicable - this paragraph has been removed
24317	15	31	15	31	"The tight coupling between …. Forms a robust physical basis …": Land-atmopshere coupling has a seasonlity, which menas that the tight LA coupling is during a specific season/episode not a general condition. This can mislead readers. Pleease either remove *tight* and *robust* or specify a typical season when the LA coupling is tight. [Jonghun Kam, Republic of Korea]	Not applicable - this paragraph has been removed
93591	15	31	15	38	I find this paragraph has little to do with the title of the section. It is more concerned with the importance of water cycle changes, that has already be addressed in previous sections. [Jean-Louis Dufresne, France]	Accepted - paragraph removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129093	15	33			"Natural climate variability and change has caused profound water cycle changes in the past based on paleoclimate and historical observations (Haug et al., 2003; Buckley et al., 2010; Pederson et al., 2014)." This is a random assortment and vague statement. How about citing Hydro2K? Or any effort to actually look at past hydro climate? [Trigg Talley, United States of America]	Not applicable - this paragraph has been removed
64899	15	35	15	35	Is there not a pertinent more recent publication? [Johannes Quaas, Germany]	Not applicable - this paragraph has been removed
81143	15	37	15	38	Expand on this please [Mary Matthews, Azerbaijan]	Not applicable - this paragraph has been removed
27213	15	38	15	38	We suggest to add "and human withdrawals" at the end of the senstence [Eric Brun, France]	Not applicable - this paragraph has been removed
131561	15	40	15	43	consider splitting this sentence into two for better readability [Hans Poertner and WGII TSU, Germany]	Accepted - this sentence has been rewritten and shortened
79395	15	43	15	44	I don't understand the sentence "Transition between energy and moisture budgets as the dominant driver occurs at spatial scales of about 4000km". [Alejandro Di Luca, Australia]	Taken into account - this sentence has been revised to emphasise that moisture transports relating to dynamical processes dominate at spatial scales smaller than about 4000km.
1249	15	43	15	44	This sentence is not very clear without consulting the reference: "Transition between energy and moisture budgets as the dominant driver occurs at spatial scales of about 4000km (Dagan et al., 2019a)". Suggest rephrasing. [Rasmus Benestad, Norway]	Accepted - this sentence has been rephrased to emphasise that moisture transports relating to dynamical processes dominate at spatial scales smaller than about 4000km.
20469	15	43	15	44	Looking at Dagan et al's article suggests that what is written here is describes rather inadequately their conclusions, in which the energy budget does not seem a prime actor. [philippe waldteufel, France]	Taken into account - this sentence has been revised to emphasise that moisture transports relating to dynamical processes dominate at spatial scales smaller than about 4000km.
69467	15	44	15	44	This was also looked at from the persepctive of the energy budget (rather than the water budget) in Jakob, C.J., Singh, M.S. & Jungandreas, L. (2019). Radiative Convective Equilibrium and Organised Convection–an observational perspective, J. Geophys. Res., doi:10.1029/2018JD030092. [Martin Singh, Australia]	Accepted - reference included and sentence modified
131563	15	44	15	44	What are the "contrasting constraints". Please explain how this statement relates to the previous argument [Hans Poertner and WGII TSU, Germany]	Accepted - it is made clear now that energy budgets constrain at global scales and moisture budgets at regional scales
81145	15	46	15	52	Well put [Mary Matthews, Azerbaijan]	Noted - thank you for the supportive comment!
113187	15	47	15	47	Again, 'evapotranspiration' is already redundant enough (considering that transpiration is an evaporation process) to introduce on top the term 'land surface evapotranspiration' ('evapotranspiration' is only used for land!). Please correct throughout. [Diego Miralles, Belgium]	Noted - changed to evaporation here but it is deemed appropriate at other points in the chapter
74151	15	47	15	49	The plant physiology is very settled here, and has been for many decaades. This sentence reads as though Milly and Dunne discovered stomata. Can it be changed to read, "which are expected from a general increase in the atmospheric evaporative demand (Scheff....) and modulated by vegetation controls on transpiration losses..."? [Daniel J. Short Gianotti, United States of America]	Accepted - suggestion is adopted in a rewritten sentence
113189	15	50	15	50	after '2018)' add 'and an higher stomatal resistance in response to the increased vapour pressure deficit (Vicente-Serrano et al., 2020)'. I wrote a Box there where this is (hopefully) cearly explained: Vicente-Serrano, S. M., Mcvicar, T. R., Miralles, D. G., Yang, Y. and Tomás-Burguera, M.: Unraveling the influence of atmospheric evaporative demand on drought and its response to climate change, WIREs Clim Change, 11(2), 1–31, doi:10.1002/wcc.632, 2020. [Diego Miralles, Belgium]	Taken into account - this reference is included but the detail is not deemed essential for the introductory discussion
103687	15	51	15	52	.. assesses advances .... ' [Philippe Tulkens, Belgium]	Noted - this is what is already written so no change is made

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116699	15		15		Mapping the interplays and complementarities with other chapters is missing. [Valerie Masson-Delmotte, France]	Rejected – this is already outlined in section 8.1.3.2 and in Figure 8.2
112211	15				Chapter Atlas sounds incorrect [Rutger Hofste, Netherlands]	Accepted – Amended to read: '…and in the Atlas…'
64339	15				i miss reference to sociohydrology, e.g. See Sivapalan et al.'s work [CRISTINA Prieto, Spain]	Rejected – outside of the scope of WG1 which deals with physical processes
93593	16	1	18	27	Very nice section except that the precise links between the text and the figure should be improved. In particular the wording should me more consistent between the text en the figure, and the text should specify the relevant panel of the figure, not only the figure number. [Jean-Louis Dufresne, France]	Taken into account - this section has been revised to make it more simple to understand. Figure 8.3 now contains a more detailed caption and panels are linked to from the text.
89053	16	1		42	Somewhere in section 8.2.1 there should be a cross reference to the overview of global heavy precipitation in section 11.4 [Angeline Pendergrass, United States of America]	Noted - this link is made in Section 8.2.3.2
27215	16	1			There is a necessity to simplify and clarify this entire subsection. Even if you know a little about water cycle this section does not help understand better, complement the knowledge. The figure 8.3 lacks clarity and explanations on the figure itself. [Eric Brun, France]	Taken into account - this section has been revised to make it more simple to understand. Figure 8.3 now contains a more detailed caption and panels are linked to from the text.
19207	16	3	16	3	The authors should add references for "Clausius-Clapeyron equation" [Mohamed Deyab, Egypt]	Taken into account - the Clausius Clapeyron equation is defined in the glossary which is now referred to
113191	16	3	16	3	Change 'which' for ', which' and clarify what 'which' reffers to. Perhaps earlier on, some terms like specfic humidity, relative humidity or VPD should be introduced. And clarify that what you refer to as 'water vapour'.is specific humidity… [Diego Miralles, Belgium]	Taken into account - this sentence is revised; specific and relative humidity are defined in the glossary but not VPD
64901	16	3	16	4	Why only “virtually certain”? is that not indeed a fact? [Johannes Quaas, Germany]	Taken into account - we add a stronger statement that water vapour will increase close to a thermodynamic rate and now refer to the likelihood statements in Chapter 2 with reference to observed water vapour changes
1251	16	3	16	15	An important constraint is the fact that it does not rain everywhere and all the time, but intermittent in selected regions (due to circulation patterns with ascending and descending air). On a daily basis, there is rain over an area A_p over earth's surface and evaporation taking place over A_e. When there is no significant accumulation of moisture in the air, then the amount evaporated over A_e will come down over area A_p. The ratio of the two areas and the rate of evaporation set the mean precipitation intencity and the frequency of rainfall (DOI: 10.1088/1748-9326/aab375). [Rasmus Benestad, Norway]	Noted - here the global scale water vapour responses are being discussed and this observational analysis covers a relatively short period that is influenced by internal climate variability and homogeneity in the satellite record so this is not considered a fundamental constraint relevant for this section
89035	16	3		4	According to the AMS glossary, the Clausius-Clapeyron equation is "The differential equation relating pressure of a substance to temperature in a system in which two phases of the substance are in equilibrium." (http://glossary.ametsoc.org/wiki/Clausius-clapeyron_equation#:~:text=Clausius%E2%80%93Clapeyron%20equation,the%20substance%20are%20in%20equilibrium.) It tells us the dependence of saturation water vapor pressure on temperature. It's just an educated guess, but I think the goal of this sentence is to say that we expect the amount of moisture to increase at roughly the rate of Clausius-Clapeyron because we expect to see changes in moisture that, to zeroth order or maybe even first order, follow constant relative humidity. This is not quite what the sentence says and I think the distinction is important. The first mention of constant RH warming I'm aware of is Arrhenius (1896). Ingram (2010) presents a nice description and Romps (2014) an analytical explanation in the tropics. Arrhenius PS. On the influence of carbonic acid in the air upon thetemperature of the ground. Lond Edinb Dubl Phil Mag J Sci.1896;41:237–76. Ingram W. A very simple model for the water vapour feedback onclimate change. Q J R Meteorol Soc. 2010;136:30–40. Romps DM. An analytical model for tropical relative humidity. JClim. 2014;27:7432–49 [Angeline Pendergrass, United States of America]	Taken into account - the Clausius Clapeyron equation is in the IPCC glossary and the sentence is revised to make the constant relative humidity assumption clearer

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53173	16	3			is a strong constraint on the atmospheric water holding capacity but also, in both observations and climate models, on the effective water vapour content (...)? [Hervé Douville, France]	Noted - the sentence has been re-framed to state that low level specific humidity increases at around 7%/oC globally assuming constant relative humidity
45251	16	3			It would be useful to briefly explain the reader what Clausius-Clapeyron equation is all about (i.e., relationship between saturation vapor pressure and temperature) before saying that atmospheric water vapor is virtually certain to increase.This is particularly helpful to non-specialists. [Krishnan Raghavan, India]	taken into account - although the Clausius Clapeyron equation is defined in the glossary some further details are added to clarify that the water vapour relative to its saturation amount increase with warming
87077	16	4	16	5	Sufficient data is available post 1970s. Why it is still "very likely" which means with high probability, that surface specific humidity has increased for the given duration. [Tarul Sharma, Netherlands]	Noted - this is the assessment of Chapter 2.
113193	16	5	16	5	are' for 'would be'. The constant RH is hypothetical. [Diego Miralles, Belgium]	Taken into account - the relative humidity assumption is emphasised now
43763	16	5	16	5	I suggest defining 'specific humidity' and 'relative humidity' or rewording the sentence to avoid likely misinterpretation by non-specialists. [Francisco Tapiador, Spain]	Taken into account - specific and relative humidity are defined in the glossary and the paragraph is revised to make it clearer to non-specialists
67577	16	5	16	6	Add citation Held and Soden 2006, Robust responses of the hydrological cycle to global warming. J. Climate, 19, 5686–5699, [Yang Tian, United States of America]	Taken into account - a reasonable suggestion and this research is covered by AR5 and AR4 which is already referenced though a mention of Held and Soden is now made in a later paragraph in relation to expectations for slower percentage increases in precipitation than water vapour content
79397	16	6	16	9	I don't get the sentence "Contrasting..." [Alejandro Di Luca, Australia]	Accepted - this sentence is revised and contrasting is removed
113195	16	6	16	9	I find this a strange justificatio. The main reason for deviating from Clausius Clapeyron is water stress at the surface preventing potential evaporation from ocurring. [Diego Miralles, Belgium]	Taken into account - this sentence is revised and the different response over land is stated with details presented later in the section
27217	16	8	16	9	It is difficult to understand this sentence. Why are sulfate aerosols and black carbon referred to here? Are the autjors saying that if there is either sulfate aerosols or black carbon in the air column, then the increase in water vapour per °C is different? [Eric Brun, France]	Accepted - it is now stated this relates to changes in aerosol experiments
26471	16	9	16	11	It should be mentioned (even if the section is about global water cycle) that the assumption of constant relative humidity in a changing climate is not valid in regional and local scales. This is also seen elsewhere than on land areas, which are mentioned here. For example, 40-year trends in ERA5 renalyses have shown that relative humidity has had regionally very variable long-term trends within the Arctic, due to changes in circulation patterns and their occurrence (Nygård et al. 2020). Nygård T., Naakka T., Vihma T. (2020): Horizontal moisture transport dominates the regional moistening patterns in the Arctic. Journal of Climate. doi: https://doi.org/10.1175/JCLI-D-19-0891.1 [Tiina Nygård, Finland]	Accepted - it is now made clear that relative humidity is not constant at the regional scale and as noted, this paragraph deals with the global scale and already mentions that relative humidity decreases over the global land, while regional changes are noted later in the section
89037	16	9		10	Is this partially or entirely a transient effect? [Angeline Pendergrass, United States of America]	Accepted - differences are related to rapid adjustments and slow responses to forcings with forcings altering the relative humidity distribution as now stated.
7427	16	15	16	15	Seems strange to refer to « (Section 8.2.1) » in Section 8.2.1. [Geremy PANTHOU, France]	Accepted - deleted (was previously in a different section)
113197	16	17	16	18	This sentence is unclear 'In contrast to the thermodynamic constraints controlling water vapour changes, global mean precipitation and evaporation are tightly linked to the atmospheric energy budget'. What do you meanby 'in contrast to the constraints?'. Phrase differently. Also: water vapour in the atmosphere IS the result of the balance of E and P, so I cannot see how a hard constraint on water vapour does not apply to the fluxes. Clarify the scale. [Diego Miralles, Belgium]	Accepted - the sentence is rephrased

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18355	16	17	16	19	Global-mean precipitation is the same as global-mean evaporation on annual and longer time scales, and global-mean evaporation is directly constrained by the surface energy balance, which is coupled to atmospheric energy balance but it also has components (e.g., surface absorbed solar radiation and heat flux into the ocean) not included in the atmospheric energy balance. Furthermore, the atmopsheric energy balance contains several other fluxes beside latent heating from precipitation process (e.g., changes in surface sensible heating flux or absorption of SW or LW radiation by the atmospheric can alter the atmospheric energy balance without changing precipitation. Thus, the atmospheric energy balance does not provide a strict constraint on preciptiation (or its response to CO2 forcing). Instead, the surface energy balance provides a direct control on global-mean evaporation and thus global-mean precipitation. To say that the global-mean P is constrained by atmospheric energy balance without mentioning the direct control of the surface energy balance on global-mean preciptiatin is very misleading. [Aiguo Dai, United States of America]	Noted - the combined role of the surface and atmospheric energy balance in determining surface evaporation and precipitation are mentioned explicitly in this paragraph
113199	16	19	16	19	What releases latent heat is condensation (and solidification, and sublimation), not precipitation. Maybe 'the latent heat release associated with precipitation'. [Diego Miralles, Belgium]	Taken into account - this sentence has been rewritten
27219	16	19	16	21	We do not understand this, nor do we understand the Figure 8.3 - Why is for example sensible heat flux that is released at the surface, combined with the latent heat released during precipitation that occurs in high altitude ... [Eric Brun, France]	Taken into account - a full description of the Figure 8.3 is now provided with links to panels in the body text
53175	16	19	16	21	This statement could be completed by the finding that first-order changes in the hydrologic cycle are mainly associated with changes in solar radiation, while those in surface temperature are mainly associated with changes in atmospheric longwave radiation (Dhara 2020, https://doi.org/10.1029/2020GL087576). Moreover, the intermodel spread in global-mean precipitation response is dominated by shortwave radiation (Fildier and Collins 2015, https://agupubs.onlinelibrary.wiley.com/doi/pdf/10.1002/2015GL065931) [Hervé Douville, France]	Rejected - assessment of the Dhara paper finds that it uses a simplistic experiment, altering shortwave optical depth and longwave optical depth to empirically deduce hydrological sensitivity from a radiative convective model. Decreasing shortwave optical depth destabilises the atmosphere relative to the surface so causes big turbulent heat flux changes while the longwave experiment does not, so heats the surface more. They compare to GCMs but these include forcing and feedback in the flux changes so the comparison is not consistent. Since this is a recently published paper that has not yet been confirmed as robust, it was not deemed essential to include this as part of the assessment here. On the other hand, the link between shortwave absorption and model-specific parametrizations are considered as part of the assessment in section 8.1.
89039	16	21		22	Another paper that I think should be cited here, with a related but subtly different approach to the surface energy budget, is Lorenz et al (2010) Lorenz, D. J., DeWeaver, E. T., & Vimont, D. J. (2010). Evaporation change and global warming: The role of net radiation and relative humidity. Journal of Geophysical Research Atmospheres, 115(20), 1–13. https://doi.org/10.1029/2010JD013949 [Angeline Pendergrass, United States of America]	Rejected - the perspective of this work, available at the time of the AR5 assessment, was not deemed to add substantial insight above that assessed here.
12751	16	22	16	24	Global average quantities can be related to local-scale impacts through pattern scaling approaches in some instances, although they are not a panacea. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted - pattern scaling approaches are not essential to the assessment in this section

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
131565	16	26	16	42	This paragraph repeats the term "response" many different times and in many different contexts. After finishing the reading, the connection remains unclear: What type of "response" "responds" to what type of "response"? The sentences eventually appear somewhat loosely connected and the core-argument gets lost [Hans Poertner and WGII TSU, Germany]	Taken into account - this paragraph is revised to reduce the usage of "response" and make the arguments clearer with respect to Figure 8.3.
89041	16	27			This would be a good place to state that the fast- and slow- terminology refer to the response to an abrupt forcing (ie idealized rather than realistic) [Angeline Pendergrass, United States of America]	Rejected - although the fast response can only be diagnosed using idealised experiments it still applies during realistic scenarios so it remains valid that the total response combines both mechanisms
12753	16	33	16	34	This doesn't seem to be the most appropriate reference for this argument and it isn't a new concept. What about something like Sutton et al., https://doi.org/10.1029/2006GL028164 ? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - the references used are deemed appropriate for the discussion of radiative adjustments discussed in this paragraph rather than land/ocean warming contrasts dealt with in the suggested reference
20471	16	41	16	41	When mentioning the global evaporation as a percentage per °C, it is a percentage of what? [philippe waldteufel, France]	Rejected - as for global precipitation, this refers to the % change relative to its mean and there is no other obvious way to calculate this so no change was made
16627	16	47	16	47	In figure 8.3 panel a, both the solid and dashed grey lines are labelled "apparent hydrological sensitivity". Presumably they should be different? [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: typo in legend has been corrected
4825	16	47	16	48	Although this figure contains interesting physical insights, it is difficult to follow due to the very incomplete figure caption and the lack of explicit references to the figure panels in the main text. [Bart van den Hurk, Netherlands]	Accepted: the caption is now completed and linked more comprehensively to the body text
64907	17	1	17	1	why “removing fast adjustment effects” here? How can these be removed? [Johannes Quaas, Germany]	Taken into account - the sentence is modified to make it clear this is the temperature-dependent component of global precipitation changes
20473	17	1	17	4	Please rewrite the sentence in such a way that hydrological sensitivity is defined clearly and in English. In this sentence, what is meant by "global mean warming"? Is that GSAT? [philippe waldteufel, France]	Accepted - the hydrological sensitivity is defined in the glossary and it is defined here in terms of GSAT as suggested
1253	17	1	18	27	The two paragraphs are difficult to understand. What is the main message here? When dealing with precipitation and response to warming, it would be natural to also discuss the change in cloud climatology? [Rasmus Benestad, Norway]	Taken into account - the paragraph is modified to make the message clearer, that hydrological sensitivity is quite well understood but the rapid adjustment component is less well understood. Although clearly linked with cloud locally there is not a simple link between cloud cover and precipitation responses at the global scale.
80641	17	2	17	2	Thanks for the citation. Samset et al. 2017 should be Samset et al. 2018. (There is no 2017 year for this journal, it started in january 2018.) [Bjorn Samset, Norway]	Accepted
87079	17	2	17	2	A submitted manuscript "Pendergrass" is cited multiple times. [Tarul Sharma, Netherlands]	Accepted - the final reference for Pendergrass, submitted is now applied
67579	17	3	17	3	Delete citation that is not published yet, delete Pendergrass, submitted [Yang Tian, United States of America]	Accepted - the final reference for Pendergrass, submitted is now applied
64909	17	6	17	6	\eta wasn’t formally introduced in the text yet [Johannes Quaas, Germany]	Rejected - it is defined in this paragraph and in the glossary

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116701	17	6	17	10	"may be overestimated"… "based the evalution of low altitude cloud changes" : please make sure to have links with ch 3 and Ch 7 on model evaluation and feedbacks. [Valerie Masson-Delmotte, France]	Accepted - Section 7.4.2 and Section 3.8.2 now linked.
64911	17	7	17	7	"overestimated" by whom or by what? [Johannes Quaas, Germany]	Taken into account - this sentence has been combined and revised to be clearer
4827	17	7	17	7	is eta overestimated, or the range of eta overestimated? [Bart van den Hurk, Netherlands]	Accepted - The magnitude is overestimated and the sentence is revised to correct this
64913	17	11	17	11	which feedbacks? [Johannes Quaas, Germany]	No longer applicable - this section of the sentence has been removed
53177	17	12	17	13	This is an important finding that may deserve a physical explanation? Does it mean that CMIP5 models tended to overestimate the low-level cloud feedbacks and is it consistent with the assessment in CH7? [Hervé Douville, France]	Taken into account - this sentence has been combined and revised to emphasise the newer results of Pendergrass and a detailed explanation is not deemed necessary. A link is made to Section 7.4.2 and 3.8.2.
45253	17	12			A recent paper by Chirag Dhara (2020): Constraining global changes in temperature and precipitation from observable changes in surface radiative heating. Geophy. Res. Lett, 47, https://doi.org/10.1029/2020GL087576 is a relevant reference for hydrological sensitivity and may be included here. [Krishnan Raghavan, India]	Rejected - assessment of the Dhara paper finds that it uses a simplistic experiment, altering shortwave optical depth and longwave optical depth to empirically deduce hydrological sensitivity from a radiative convective model. Decreasing shortwave optical depth destabilises the atmosphere relative to the surface so causes big turbulent heat flux changes while the longwave experiment does not, so heats the surface more. They compare to GCMs but these include forcing and feedback in the flux changes so the comparison is not consistent. Since this is a recently published paper that has not yet been confirmed as robust, it was not deemed essential to include this as part of the assessment here.
20115	17	13	17	13	There is a missing "in" before CMIP5, and a period probably missing before "consistent" [philippe waldteufel, France]	Taken into account - this sentence has been revised to account for this
43159	17	13			Read "CMIP5 simulations (Pendergrass, submitted). Consistent observed" rather than "CMIP5 simulations (Pendergrass, submitted) Consistent observed" [Cyriaque Rufin Nguimalet, Central African Republic]	Taken into account: the lines have been revised and a new paragraph made
53179	17	16			given the uncertain ENSO response to global warming? [Hervé Douville, France]	Taken into account - these feedbacks may not relevant for longer term climate change which is now made clear
53181	17	18	17	19	It could be then useful to add: It is the reason why it is more appropriate to define the global water cycle evolution as an intensification (or amplification) rather than an acceleration. [Hervé Douville, France]	Accepted - added: thereby drive an intensification but not acceleration of the global water cycle (Sections 8.3.1.1 and 8.4.1.1).
20117	17	21	17	22	It appears now that the "actual hydrological sensitivity" will be named "apparent hydrological sensitivity", while the quantity formerly named "hydrological sensitivity" keeps its name but is no longer the actual hydrological sensitivity. Figure 8.3 does not help, since both quantities are named "apparent hydrological sensitivities". Some mending is needed! [philippe waldteufel, France]	Accepted - Figure 8.3 is corrected!
18357	17	21	17	24	Again, precipitation has larger unforced fluctuations. Any estimates of preciptiation change over a short (<50yrs) period will likely contain stubstantinal unforced variations that are not part of the response to external forcing. Interpreting such unforced fluctions as either fast or slow response to external forcing would be misleading. The only way to get rid of such unforced changes is to average over a large number (>50) of ensemble runs and/or over a large region (e.g., the globe). Even global-mean P differs greatly among individual ensmeble runs (Dai and Bloecker 2019. https://doi.org/10.1007/s00382-018-4132-4), thus one must be cautious in interpreting the changes from a small ensemble runs. [Aiguo Dai, United States of America]	Taken into account - it is now explicitly stated that the effects of large unforced variability and complex rapid adjustments to radiative forcings make an observational constraint on apparent hydrological sensitivity difficult with reference to this paper

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
113201	17	23	17	23	adjustments to precipitation' or 'adjustments of precipitation'? [Diego Miralles, Belgium]	Accepted - changed "to" to "of"
64915	17	23	17	23	what is meant by “adjustment to precipitation”? [Johannes Quaas, Germany]	Accepted - changed "to" to "of"
4829	17	23	17	24	"Rapid atmospheric adjustments ... Aerosols." This I don't understand [Bart van den Hurk, Netherlands]	Taken into account - changed "to" to "of"
105767	17	28	17	30	The findings from Li et al (2013b) have been extended and placed in context of CMIP6 simulations by Rehfeld et al (2020, https://www.earth-syst-dynam.net/11/447/2020) [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - sentence added showing consistency in CMIP6 experiments from this reference
89043	17	28		32	Rehfeld et al., (2020) disgnose changes in global precipitation in additional LGM simulations and compare them with abrupt-4xCO2. Rehfeld, K., Hébert, R., Lora, J. M., Lofverstrom, M., & Brierley, C. M. (2020). Variability of surface climate in simulations of past and future. Earth System Dynamics, 11(2), 447–468. https://doi.org/10.5194/esd-11-447-2020 [Angeline Pendergrass, United States of America]	Accepted - sentence added showing consistency in CMIP6 experiments from this reference
69469	17	30	17	30	I tihnk this line is a bit misleading, as it suggests that large CO2 forcing somehow surpresses the fast precipitation response. I think what you mean is that with large CO2 forcing, the increase in temperature is larger, and this larger slow response (which corresponds to an increase in precipitation) dominates over the fast response (which corresponds to a decrease in precipitation). [Martin Singh, Australia]	Accepted - this sentence is simplified to avoid this confusion
53183	17	30	17	32	Is there any implication for the hydrological impact of SRM? More generally, Section 8.2 could explain why SRM is not expected to fully mitigate precipitation changes due to increased CO2 even if GSAT returns to present-day level (e.g., Bony et al. 2013) [Hervé Douville, France]	Accepted - a line is added to note the implications for SRM with a reference to Sections 8.6.3, 4.6.3.3; 6.4.7
93595	17	30			“which larger CO2 forcing”. If I understand correctly, it is not the absolute value of the CO2 forcing that is relevant here, it is the CO2 forcing value respective to the other forcings. At the LGM, the ice sheet forcing is also importance, not only the CO2, whereas the 4xCO2 experiment is only driven by the CO2 forcing. [Jean-Louis Dufresne, France]	Accepted - this sentence is simplified to avoid this confusion
113203	17	31	17	32	I would expect evaporation to be more sensitive to temperature at higher temperatures, following the Clausius-Clapeyron law. Could you clarify this point? You do not mean in % of E, right? [Diego Miralles, Belgium]	Taken into account - yes, this refers to the %/oC scaling which as the Clausius Clapeyron equation shows will be larger for lower temperatures as now made clear.
27221	17	31	17	32	Details or references are needed [Eric Brun, France]	Taken into account - it is now made clear that these points are made by the Li et al. reference
12755	17	31	17	32	How does this fit with Clausius-Clapeyron which would suggest the contrary? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - it is now made clear that these points are made by the Li et al. reference
64917	17	32	17	32	which constraints? Shouldn’t the opposite be true considering Clausius-Clapeyron? [Johannes Quaas, Germany]	Taken into account - it is now made clear that these points are made by the Li et al. reference
95885	17	34	17	35	All aerosols affect the surface energy balance, not just sulphate - I know what you mean but it is a bit confusing. [Philip Philip Stier, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - this line is modified to make it clear it is the instantaneous effect of the atmosphere or surface that is being considered

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
80643	17	34	17	47	Could you consider adding a formal assessment of our knowledge of the impact of aerosol shortwave absorption on precipitation, as a process and/or in the present atmosphere? There is quite some information in the studies you cite (and some I've added in other comments), and such an assessment would be new to this report. I'm happy to help produce one, if you don't think I'm too closely involved in the field. (I don't think one is made in Ch6 either. I see there is additional material in Box 8.1, but this also doesn't assess the process itself.) [Bjorn Samset, Norway]	Taken into account – we discuss the role of shortwave absorption on precipitation responses in Section 8.2.1 and Box 8.1
67581	17	37	17	38	Sulphate aerosols can be scanveged by clouds and form cloud condensation nuclei, the aerosol effect on precipitation seems to be more from a microphysical perspective than a ratiative forcing impact. Suggest remove the words "radiative forcing" [Yang Tian, United States of America]	Rejected - while it is of course correct that aerosols act as condensation nuclei, here it is the overall effective radiative forcing that is being considered so the response of global precipitation to the aerosol direct effect and indirect effect through cloud
12757	17	39			Why is "Black Carbon" capitalized? It wasn't earlier. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
20475	17	40	17	43	When removing anthropogenic aerosols, GSAT will increase; then, because of Claudius-Clapeyron and the general trend for relative humidity to stay about the same, specific humidity will increase, which will feed more precipitation. So here is a very simple explanation for the reported result. Is it wrong? Why? [philippe waldteufel, France]	Rejected - although increased sunlight can heat the surface and evaporate more surface water through the Clausius Clapeyron equation and increase atmospheric water content, the ability of the atmosphere to radiate an equivalent latent heating due to condensation of the water is not achievable, hence why the energy budget is a global constraint on precipitation and evaporation. An extra line is appended in the 2nd paragraph of this section to this effect.
116703	17	41	17	41	Coordination is needed with chapter 4 which also has a similar assessment (precipitation response to removal of aerosols). [Valerie Masson-Delmotte, France]	Accepted: a link to Section 4.4.4 is made which quantifies % changes whereas in this section we are looking at apparent hydrological sensitivity in %/oC.
129095	17	41			Presumably "complete removal of present day anthropogenic aerosol emissions" means "complete removal of present day anthropogenic aerosol and precursor emissions." [Trigg Talley, United States of America]	Noted - this is indeed the case as already implied
109689	17	42	17	42	In the interest of inclusivity, internationalism, geographic coverage, and fairness that ought to underpin an IPCC report, and given that Mote et al (2016) is already cited here, delete the Mote et al (2018) reference, which simply rehashes a large body of their own existing work in the western US, and replace it with Vincent et al (2015), which is a landmark integrated analysis of changes in climate and snowpack across Canada. The full reference is Vincent et al., 2015, Observed trends in Canada's climate and influence of low-frequency variability modes, Journal of Climate, 28, 4545-4560. [Sean Fleming, United States of America]	Noted. The Mote references are retained, however a reference to Vincent 2015 has been added.
109691	17	42	17	42	Some fresher choices in literature citation could stand to be included here too, as there is little new in Mote et al (2016) and especially Mote et al (2018). For example, Kurt Solander at LANL has been doing some innovative and valuable work on climate change and snow - two recent papers of his include Solander et al., 2018, Interactions between climate change and complex topography drive observed streamflow changes in the Colorado River Basin, Journal of Hydrometeorology, 19, 1637-1650; and Solander et al., 2019, Estimating hydrologic vulnerabilities to climate change using simulated historical data: A proof-of-concept for a rapid assessment algorithm in the Colorado River Basin, Journal of Hydrology: Regional Studies, 26, 100642. [Sean Fleming, United States of America]	Noted. A reference to Solander 2019 has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12759	17	43	17	45	Why is this the case? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - an explanation and additional references are added Kasoar et al., 2018; Liu et al., 2018c)
113205	17	46	17	46	Wrong grammar at 'yet is more'. [Diego Miralles, Belgium]	Accepted - corrected
4831	17	46	17	46	put comma before "yet" [Bart van den Hurk, Netherlands]	Accepted - corrected
80637	17	47	17	47	A paper just accepted in Journal of Geophysical Research: Atmosphere is relevant here: Black Carbon and Precipitation: an Energetics Perspective, Sand et al. 2020. Should be online in early view by the time you read this, or at least on its way out. Also Samset and Myhre 2015, JGRA, http://dx.doi.org/10.1002/2014JD022849, on which the new paper builds. [Bjorn Samset, Norway]	Accepted: reference added
64919	17	49	17	50	large range of what? uncertainty? [Johannes Quaas, Germany]	Accepted - uncertainty range now stated
67583	17	51	17	51	Delete citation that is not published yet, delete Pendergrass, submitted [Yang Tian, United States of America]	Taken into account - the Pendergrass published paper is cited
88121	17	51	17	53	I think this should say 'associated with' rather than `explained'. See my comment above on Chapter 8, p5, L25 [Robin Chadwick, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
69471	17	53	17	55	This gives the impression that the lower sensitivity over land is due only to the reduction in relative humiidty. But the sensivity would be lower even if the precipitation change was equal to that over ocean because land temperatures increase more than temperatures over the ocean. Moreover, the relationship between lan relative humidity and land precipitation is not straightforward; I am sure there are regions (at least in simulations) where the average surface relative humiidty decreases, but the precipitation nonetheless increases and vice versa. [Martin Singh, Australia]	Taken into account - now use "associated" and "complex" to imply a less straightforward cause and effect. The land hydrological sensitivity is relative to GSAT as now stated so the larger warming over land will not affect the value.
105553	17	54	17	54	slower warmign rate over ocean' -> I am not fully convinced that this is right cause? If this is the case; then higher warming scenario make the land wetter? Climate model simulations show collapase of the ET and temperature relataionship over land with the higher wamring (See Figure 10a in Kumar et al., 2013); this results is supported by many other studies that has analyzed the PET and ET realtionship over land (Roderick et al. 2015, Kumar et al., 2016). So, suppressed hydrolgical sensitivity can be due to limited water supply over land that decreases RH (supported by many climate studies). Kumar, Sanjiv, Paul A. Dirmeyer, Venkatesh Merwade, Timothy DelSole, Jennifer M. Adams, and Dev Niyogi. "Land use/cover change impacts in CMIP5 climate simulations: A new methodology and 21st century challenges." Journal of Geophysical Research: Atmospheres 118, no. 12 (2013): 6337-6353. Kumar, Sanjiv, Francis Zwiers, Paul A. Dirmeyer, David M. Lawrence, Rajesh Shrestha, and Arelia T. Werner. "Terrestrial contribution to the heterogeneity in hydrological changes under global warming." Water Resources Research 52, no. 4 (2016): 3127-3142. Roderick, M. L., P. Greve, and G. D. Farquhar (2015), On the assessment of aridity with changes in atmospheric CO2, Water Resour. Res., 51, 5450– 5463, doi:10.1002/2015WR017031. [Sanjiv Kumar, United States of America]	Taken into account - these sentences have been modified to reflect the importance and complexity of land surface feedbacks with reference to the suggested Kumar et al. 2016 reference and Chandan and Peltier, 2020

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89045	17	54	18	9	I think here it is important to separate the "fast" and "slow" wording between the precipitation response to an abrupt, idealized forcing and from trasnsient aspects of the warming / response to realistic forcing that varies over time: "slower warming rate" and "offset by initial increases… that rapidly warm… in the short term" since the end of the paragraph refers to the response to the historically time-varying forcing. [Angeline Pendergrass, United States of America]	Accepted - a sentence is added at the end of the paragraph to clarify this confusion
64921	17	55	18	1	which feedbacks? [Johannes Quaas, Germany]	Accepted - added involving soil-vegetation-atmosphere coupling
4833	17	55	18	1	the difference in warming between ocean and land is not a given, but is primarily caused by (or at least part of) the land atmosphere feedback that is here depicted as an "additional effect". [Bart van den Hurk, Netherlands]	Taken into account - the sentence is split now to emphasise more the role of land feedbacks
53185	18	1			more than offset and not only in the short term (cf. Table 4.3 where the globally-averaged precipitation increase is stronger over land than over the ocean for all SSPs at the end of the 21st century) [Hervé Douville, France]	Taken into account - offset is changes to counteracted, initi8al is changed to rapid responses and a final sentence linking to Chapter 4 is now made to clarify the role in determining transient climate change
45719	18	7			I seriously doubt that it is only sulphate aerosols that cause this effect. I suggest to omitt at least the word sulphate here. [Sabine Wurzler, Germany]	Accepted - sulphate removed as recommended
113207	18	11	18	12	This statement 'rapid responses to increases in GHGs… reduce global precipitation' seemingly contradicts the statement about (line 1–2) 'initial increases in precipitation explained by greater surface downwelling longwave radiation, due to CO2 increases,' [Diego Miralles, Belgium]	Accepted - this refers to global mean but I agree it is confusing and it is now made clear that the over the ocean adjustments in precipitation drive the global response
4835	18	13	18	13	"expected to be small": they are observed, so are not "expected" but "shown" I would argue [Bart van den Hurk, Netherlands]	Accepted - expected now removed
53187	18	17			and GHG concentrations stabilize before decreasing in response to mitigation actions? [Hervé Douville, France]	Taken into account - the delay in realised warming is now stated
1255	18	18	18	18	It is relevant to add information about a study of how the semi-global (between 50S and 50N, which comprises 77% of Earth total surface area) daily precipitation area, A_p, has decreased by 7% between 1998 and 2016, from 25% to 23% and a scaling factor of −17 × 10^6 km^2 per °C (DOI: 10.1088/1748-9326/aab375). The area of evaporation, A_e, is dominated by the wet surfaces provided by the oceans, and as long A_e is constant, a shrinking A_p over time means that the mean precipitation intensity increases as long as there is insignificant accumulation of moisture in the atmosphere. This has profound implications, such as greater probability for extreme precipitation in the regions where it rains, but also greater risk of droughts due to smaller area of rainfall. On a monthly time-scale, however, there is no such decline in area, which may suggest that the processes generating rain are moving over time (e.g. cyclones). [Rasmus Benestad, Norway]	Rejected - this section deals with global-scale changes so the area of precipitation is beyond the scope of the assessment. In addition the 18 years used is quite short, given the large internal climate variability, to provide fundamental insight into global precipitation area response.
81147	18	18	18	27	well done this para need to be in the exec summary [Mary Matthews, Azerbaijan]	Noted - thank you, a modified form is in the Executive summary
22261	18	19	18	20	It is disconcerting to have a principal finding so fundamentally depend upon two assessment sections yet to come within the same chapter. If these were sections in another chapter that would be another matter but these are sections within the same chapter. Is it really sensible that this finding precedes rather than follows these substantive assessments? [Peter Thorne, Ireland]	Taken into account - the links to later sections are removed. These statements are based upon robust understanding and high agreement across modelling studies though it does not make sense to avoid completely evidence provided by long term observations and modelling of future changes. These are to some extent touched on in earlier chapters as well as being dealt with in later sections but this overlap is kept to a minimum and was considered in further refinements to this section, the structure of which was appreciated by other reviewers.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
69473	18	22	18	23	What is the evidence that this reduction in precipitation over land is "driven" by changes in land relative humidity? The Berg paper finds that they are both affected by soil moisture changes, but the causailty is unclear. Suggest changing "due to declining land relative humiidty" to "associated with decreases in land relative humidity". [Martin Singh, Australia]	Accepted - associated now used
53189	18	22	18	23	Again be careful about this statement (cf. Table 4.3) [Hervé Douville, France]	Accepted - an updated line related to the transient precipitation response is now provided
129097	18	22	18	24	""It is likely that increases in multi-annual mean precipitation over land will be smaller than over the ocean due to declining near-surface relative humidity driven by increasing land-ocean thermal contrast and surface feedbacks."" This is not true by seeing Figure 4 in Fu and Feng (2014) where it is shown that the percentage changes in precipitation (scaled by the ocean-mean temperature increase) over land and ocean are the same. Citation: Fu, Q. and S. Feng, 2014: Responses of terrestrial aridity to global warming. J. Geophys. Res. 119, doi:10.1002/2014JD021608. [Trigg Talley, United States of America]	Accepted - this inconsistency was noted just after SOD submission and we now make reference to Chapter 4 and explain in more detail how the suppressed hydrological sensitivity over land is counteracted by rapid atmospheric adjustments to radiative forcings (particularly CO2 and absorbing aerosol) in this section and the summary paragraph plus the Executive summary and technical summary
27223	18	22	18	24	This has not been discussed above [Eric Brun, France]	Taken into account - this is discussed more in the section and a modified summary line
88123	18	23	18	23	Again, I think this should say 'associated with' rather than `due to'. See my comment above on Chapter 8, p5, L25 [Robin Chadwick, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - associated now used
93597	18	24			“high confidence”. Shouldn't it be “very high confidence”? Is there a scientific argument that this cannot be the case? [Jean-Louis Dufresne, France]	Rejected - high confidence is deemed sufficient given that internal variability can cause temporary alterations in the relationships
53191	18	25	18	27	What about concluding this plain language summary with: "In other words, the global water cycle is not expected to accelerate, but rather to amplify and to show enhanced variability with less rainy days but heavier precipitation events."? [Hervé Douville, France]	Rejected - there is not sufficient body text in this subsection to support an additional summary statement of this type.
89047	18	26			I don't think it's entirely clear what the causality is - most of the analysis of projections that we do is diagnostic. To address this, "driving" could be replaced by, "associated with". [Angeline Pendergrass, United States of America]	Accepted
79401	18	30	18	30	The separation between thermodynamics/dynamics is used very much but has not been introduced to the reader. Maybe you could add a few words on this at the beginning of the section and refer to the Box 11.1 where this is discussed in more detail. Your input to this Box will be very welcomed of course ;)) [Alejandro Di Luca, Australia]	Taken into account: Box 11.1 is referred to in Section 8.2.3
109683	18	32	19	48	This passage is mostly fine but should briefly but clearly provide the caveat that P-E is a very coarse representation of terrestrial hydrologic dynamics, which also have short-term to interannual dynamical relationships and lags, and depend on changes in storage associated with alpine glaciers, lakes, aquifers, and so forth; some of these freshwater stores contain water accumulated over thousands of years and have a substantial role in the terrestrial (e.g., river) hydrologic cycle, with widespread and profound impacts. Three examples that could be cited here include O'Neel et al., 2015, Icefield-to-ocean linkages across the Northern Pacific coastal temperate rainforest ecosystem, Bioscience, 65, 499-512; Moore et al., 2009, Glacier change in western North America: influences on hydrology, geomorphic hazards and water quality, Hydrological Processes, 23, 42-61; and Clarke et al., 2015, Projected deglaciation of western Canada in the twenty-first century, Nature Geoscience, 8, 372-377. [Sean Fleming, United States of America]	Taken into account: the suggestion is noted and this point is now made in the 3rd paragraph of this section (8.2.2.1)
53193	18	34			Rephrase as "Increased atmospheric moisture with global warming and its climatological horizontal transport (...)"? [Hervé Douville, France]	Taken into account: this sentence has been revised to address this suggestion

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67585	18	36	18	38	There is high confience (based on thermodyanmics, detailed modeling and observations) that this amplification is expected to be greater over oceans than over land (Roderick et al., 2014; Vallies et al. 2015), with an associated "fresh get fresher, salty get saltier" signature in ocean salinity (Durack, 2015). [Yang Tian, United States of America]	Rejected: the effects over land are dealt with in later lines and the Vallies 2015 reference could not be found on Web of Science or Google Scholar
53195	18	36	18	47	This useful paragraph could have a brief counterpart in Section 8.3.1.1 and 8.4.1.1 where changes in the oceanic water cycle have been hardly discussed. [Hervé Douville, France]	Accepted - the amplification of P-E patterns and magnitudes over ocean are discussed in 8.3.1.1 and 8.4.1.1
113209	18	37	18	37	getS'? [Diego Miralles, Belgium]	Accepted
64923	18	40	18	40	since when? [Johannes Quaas, Germany]	Taken into account: "since strengthened" changed to "supported"
4837	18	40	18	40	"since strengthened" -> "supported" [Bart van den Hurk, Netherlands]	Accepted
12761	18	41	18	44	Very long sentence. Consider revising. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: this sentence has been revised and shortened
27225	18	43	18	43	The authors do not mention the adjustment to CO2 itself but to long-wave atmospheric warming induced by CO2 , OR to changes in the land partitioning between latent and sensible heat flux. It can be very misleading to talk about adjustments to CO2 [Eric Brun, France]	Taken into account: this sentence has been revised to address this suggestion
131567	18	43	18	43	what are "rapid adjustments to CO2 increases"? i.e. What adjustes and how? [Hans Poertner and WGII TSU, Germany]	Not applicable: this reference to adjustments has been removed
70241	18	52	18	55	In Figure 8.4, the zonal-averaged change in P-E over the ocean is dependent on the latitude and it is meridionally symmetric, whereas it is not consistent over the land. Although the P-E over the land is generally positive and it is tied to runoff from land to ocean, both regional land evaporation and precipitation are critical (Figure 8.1b). There are some Figures to perform the linear trend of global precipitation and evapotranspiration based on the reanalysis data and CMIP6 models in the text. However, the changes of P-E spatial distribution over land as well as each linear trend would be helpful to understand atmospheric moisture and its horizontal transport with warming. [Seogyeong Kim, Republic of Korea]	Noted: the spatial structure of precipitation and evaporation changes are included in Section 4.
27227	19	14	19	14	LGM is defined between 21-19 ka in Chapter 2. I . There are also unconsistent definitions for the LGM time period throughout Chapter 2. This definition must be homogeneized throughout all Chapters of the AR6. [Eric Brun, France]	Accepted: now defined as 21-19 thousand years ago for consistency with Cross Chapter Box 2.1
105643	19	15	19	19	Suggest some additional nuance for this conclusion. Regional changes "can be dominated" by dynamics instead of "are dominated" (see Lowry and Morrill 2019 10.1007/s00382-018-4385-y for examples of thermodynamics being important regionally across a transect of North and South America). Also, large responses to ice sheets "can complicate comparison of these changes to future projections of the regional hydrological cycle." I make this latter suggestion because (1) despite ice sheet effects, there are other thermodynamic and dynamic processes operating during paleo time periods that might be comparable to and relevant for the future, (2) it is possible that locations more distal from ice sheets will have hydroclimate variability that is less complicated by ice sheets. Note that these suggestions will bring the conclusion more in line with those made in chapter 10 (section 10.4.1.2.7; page 86, lines 34-38) [Carrie Morrill, United States of America]	Accepted: these suggestions and the reference are added
4843	19	17	19	17	put comma before "and" [Bart van den Hurk, Netherlands]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67587	19	18	19	19	, meaning that these changes are not … [Yang Tian, United States of America]	Taken into account: this sentence has been modified
21021	19	21	19	21	Over the globe “wet gets wetter” paradigm seems to hold, maybe because of the oceans. Fig 8.4a shows the zonal mean over land, but I don’t think this is a good indicator of changes given that there are dry and wet land areas over the same latitudinal band. Where does the reduction in (P-E) between 50S-30S come from? Is this in Argentina and Chile? Has the figure been scaled? [Marcelo Barreiro, Uruguay]	Taken into account: it is now stated that the notable decline in P-E 30-50S relates to southern Chile and Argentina. The figure is also improved by adding a/b labels and more labels for latitude.
79399	19	21	19	21	I really like the idea of Figure 8.3 but it is not easy to understand the way it is right now. Do we need to include all the "temporal scales" of the changes? Can we add a temporal scale (in days for example) to the instantaneous, rapid, semi-rapid, etc? I don’t think that it is very clear to use the size of the arrow to illuistrate the size of the terms. Hydrological sensitivty needs to be defined. [Alejandro Di Luca, Australia]	Taken into account: Figure 8.3 is updated with a full descriptive caption and specific panels are referred to more in the text. Hydrological sensitivity is corrected in the figure and defined in the text and glossary
53197	19	21	19	22	This finding may deserve a specific statement in the ES. [Hervé Douville, France]	Noted: it is deemed more policy relevant to emphasise that wet and dry events will amplify rather than that wet gets wetter, dry gets drier does not apply which is less useful
12763	19	23	19	28	A very long sentence, should be revised to avoid ambiguity of meaning. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: sentence is modified
129099	19	25	19	26	""... and over land ""dryness"" or aridity is better represented by the ratio of potential evaporation (determined primarily by net radiation) to precipitation (Roderick et al., 2014; Greve and Seneviratne, 2015; Scheff and Frierson, 2015)...."" Feng and Fu (2013) and Feng and Fu (2014) should be added here. Citations: Feng, S. and Q. Fu, 2013: Expansion of global drylands under a warming climate. Atmos. Chem. Phys., 13, 10081-10094, doi: 10.5194/acp-13-10081-2013. Fu, Q. and S. Feng, 2014: Responses of terrestrial aridity to global warming. J. Geophys. Res. 119, doi:10.1002/2014JD021608. [Trigg Talley, United States of America]	Not applicable: this sentence has been simplified and no longer discusses potential evaporation. The Fu and Feng 2014 reference is included.
129101	19	26	19	28	""... although this metric is a poor proxy for aridity projections in climate models due to inadequate parametrization of potential evaporation (Greve et al., 2019)...."" It is suggested to change this sentence to ""although if this metric is an appropriate proxy for aridity projections in climate models it is still a subject of discussion (Fu et al., 2016; Dai et al., 2018; Greve et al., 2019)."" It is encouraging to notice that the spatial pattern of aridity index change (Figure 7d in Feng and Fu, 2013) matches well with surface soil moisture change as shown in Figure 8.20 (left) in this chapter. While the surface soil moisture change is a black box from ESMs, there is a clear idea about what causes the change of aridity index (Fu and Feng, 2014). Citations: Fu, Q., L. Lin, J. Huang, S. Feng, and A. Gettelman, 2016: Changes in terrestrial aridity for the period 850-2080 from the Community Earth System Model. J. Geophys. Res. Atmos., 121, doi:10.1002/2015JD024075. Dai, A., Zhao, T., and Chen, J. (2018). Climate Change and Drought: a Precipitation and Evaporation Perspective. Curr. Clim. Chang. reports 4, 301-312. Feng, S., and Q. Fu, 2013: Expansion of global drylands under a warming climate. Atmos. Chem. Phys., 13, 10081-10094, doi: 10.5194/acp-13-10081-2013. Fu, Q., and S. Feng, 2014: Responses of terrestrial aridity to global warming. J. Geophys. Res. 119, doi:10.1002/2014JD021608. [Trigg Talley, United States of America]	Not applicable: this sentence has been simplified and no longer discusses potential evaporation. The Feng and Fu reference is included.

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4845	19	27	19	27	I wouldn't argue that parameterization of potential evaporation is insuffficient, but its conceptually meaning. It defines on the amount of (atmospheric and/or land surface) feedback one allows to quantity "potential". E.g. without humidification feedback of the atmosphere potential evaporation is much larger than when accounting for a negative feedback due to atm moisture uptake [Bart van den Hurk, Netherlands]	Not applicable: this sentence has been simplified and no longer discusses potential evaporation.
74155	19	27	19	28	I'd suggest some additional information here. Maybe something along the lines of, " although this metric is a poor proxy for aridity projections in climate models due to inadequate parametrization of potential evaoration (Greve et al., 2019). Unlike over oceans, evaporation over land is constrained by moisture availability in soils, with increasing surface resistance to moisture transfer as soils dry. Land P-E..." This is pretty old news and probably doesn't need citation, but if looking for one, you could use "Soil and moisture independent estimation of stage-two evaporation from potential evaporation and albedo or surface temperature by Salvucci 1997 https://doi.org/10.1029/96WR02858 or (more recently) "Evaporation from porous surfaces into turbulent airflows: Coupling eddy characteristics with pore scale vapor diffusion" by Haghighi and Or 2013 https://doi.org/10.1002/2012WR013324. [Daniel J. Short Gianotti, United States of America]	Not applicable: this sentence has been simplified and no longer discusses potential evaporation.
24321	19	28	19	28	"potential eva*p*oration" p is missing. [Jonghun Kam, Republic of Korea]	Not applicable: this sentence has been simplified and no longer discusses potential evaporation.
24323	19	28	19	28	Either "Over land, P-E" or "Terrestrial P-E"? [Jonghun Kam, Republic of Korea]	Accepted: used "Terrestrial"
67589	19	28	19	28	potential evaporation [Yang Tian, United States of America]	Not applicable: this sentence has been simplified and no longer discusses potential evaporation.
21023	19	28	19	28	evaPoration [Marcelo Barreiro, Uruguay]	Not applicable: this sentence has been simplified and no longer discusses potential evaporation.
113211	19	28	19	28	Correct 'potential evaoration' [Diego Miralles, Belgium]	Not applicable: this sentence has been simplified and no longer discusses potential evaporation.
113213	19	28	19	28	After 'eaporation': '(Vicente-Serrano et al., 2020)'. Vicente-Serrano, S. M., Mcvicar, T. R., Miralles, D. G., Yang, Y. and Tomás-Burguera, M.: Unraveling the influence of atmospheric evaporative demand on drought and its response to climate change, WIREs Clim Change, 11(2), 1–31, doi:10.1002/wcc.632, 2020. [Diego Miralles, Belgium]	Accepted
74373	19	28	19	28	replace evapoation by evaporation [Moulay Driss HASNAOUI, Morocco]	Not applicable: this sentence has been simplified and no longer discusses potential evaporation.
32923	19	28	19	28	should be "evaporation" [Tomasz Walczykiewicz, Poland]	Not applicable: this sentence has been simplified and no longer discusses potential evaporation.
38067	19	28	19	28	"potential eva*p*oration" p is missing. [Junhee Lee, Republic of Korea]	Not applicable: this sentence has been simplified and no longer discusses potential evaporation.
38069	19	28	19	28	Either "Over land, P-E" or "Terrestrial P-E"? [Junhee Lee, Republic of Korea]	Accepted: used "Terrestrial"
74153	19	28	19	29	You don't need to make assumptions in this sentence. Change it to instead say, "Because of moisture supply constraints, land P-E is generally positive and is balanced by runoff and percolation into subsurface soils and aquifers." [Daniel J. Short Gianotti, United States of America]	Accepted: sentence modified without unnecessary assumptions
5567	19	28			correct evaoration by evaporation [Benoit Laignel, France]	Not applicable: this sentence has been simplified and no longer discusses potential evaporation.
113215	19	29	19	30	Why? 'As a result, the “wet gets wetter, dry gets drier” scaling suggests that P-E over land will become more positive (i.e. wetter) with warming'. I guess the hypothesis is that only 'wet' becomes wetter. [Diego Miralles, Belgium]	Rejected: these sentences already address the question of why positive P-E should become more positive over land and a reference to Figure 8.1 is added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
74157	19	29	19	34	Including the first of these two sentences only confuses readers. The wet gets wetter thing is not really true over land, so why try to walk through its implications? Just delete the sentence, and then say in the next sentence that WGWDGD doesn't hold over land. My suggestion is, "Local moisture supply and land surface warming drives atmospheric drying over some land regions (Figure 8.4) thereby deviating strongly from the simple “wet gets wetter, dry gets drier” scaling (Byrne and O’Gorman, 2015)." [Daniel J. Short Gianotti, United States of America]	Taken into account: thank you for the good suggestion but on reflection we decided that this did not capture the logical progression of the argument fully. We do however incorporate the link to moisture supply which we agree is useful and have fully revised these paragraphs
98461	19	31	8- 19	31	spelling mistake [Mehwish Ramzan, Pakistan]	Rejected: we did not find a spelling mistake here although it is possible the reviewer was referring to an earlier error "evaporation" which was removed
74159	19	33	19	34	The changes in P-E over land are driven by circulation changes, but also by soil moisture's control on evaporation and runoff/subsurface percolation partitioning. This in turn is driven by precipitation, and changes in precipitation intensity will change that partitioning. I suggest adding a sentence (to an already long paragraph -- may need to be split up), something like "...from the simple “wet gets wetter, dry gets drier” scaling (Byrne and O’Gorman, 2015). Changes in precipitation and humidity change the soil moisture state of the land surface, which in turn changes the partitioning between evaporation and runoff/subsurface percolation, with increased precipitation intensity increasing runoff at the expense of evaporation (cite). Decreases in P-E are partly explained..." ("Terrestrial Evaporation and Moisture Drainage in a Warmer Climate" by Short Gianotti 2020 https://doi.org/10.1029/2019GL086498.) [Daniel J. Short Gianotti, United States of America]	Taken into account: these sentences have been modified to emphasise the region and season dependent responses and now include the suggested reference to Short-Gianotti et al.
21025	19	36	19	36	reduceD [Marcelo Barreiro, Uruguay]	Accepted
3675	19	36	19	36	please change the “lead to reduce” into “lead to reduced”. [Jiafu Mao, United States of America]	Accepted
74161	19	36	19	36	reduce->reduced [Daniel J. Short Gianotti, United States of America]	Accepted
113217	19	37	19	38	It is also recognised that P-E may be negative in the tropical dry season as ground water is lost to the atmosphere and exported'. This is not just tropics and is not just groundwater. Please refer to this article, with the second half dedicated to assessing summer deficits in P-E over European catchments: Keune, J. and Miralles, D. G.: A Precipitation Recycling Network to Assess Freshwater Vulnerability: Challenging the Watershed Convention, Water Resour. Res., 52(18), 10,757–15, doi:10.1029/2019WR025310, 2019. [Diego Miralles, Belgium]	Accepted: this reference is now included in a modified sentence

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
105555	19	37	19	38	It is well known that evapotranspiration (ET) exceeds precipitation (P) during the growing season in the range of 20 to 40% [Huete et al., 2006; S. Kumar and Merwade, 2011; S. Kumar et al., 2014b; Markewitz et al., 2010; Justin Sheffield et al., 2013; Yan and Dickinson, 2014]. Amazon show offsete between wet season and forest gorwth. Climate models show higher ET than precipitation in the gowing season in North America (Sheffield et al., 2013). Overall, there are muliple line of evdeince that ET exceeds precipitation during the growing season and the extra mositure is supplied by the mosture stored from the previous season, a process that is known as soil mositure memory (Dirmeyer et al., 2016), and rememrgence (Kumar et al., 2019). Huete, A. R., K. Didan, Y. E. Shimabukuro, P. Ratana, S. R. Saleska, L. R. Hutyra, W. Z. Yang, R. R. Nemani, and R. Myneni (2006), Amazon rainforests green-up with sunlight in dry season, Geophysical Research Letters, 33(6), doi: Artn L06405 10.1029/2005gl025583. Kumar, S., and V. Merwade (2011), Evaluation of NARR and CLM3.5 outputs for surface water and energy budgets in the Mississippi River Basin, J Geophys Res-Atmos, 116. Kumar, S., D. Lawrence, P. Dirmeyer, and J. Sheffield (2014b), Less reliable water availability in the 21st century climate projections, Earth's Future, 2, 152-160, doi: doi:10.1002/2013EF000159.; Markewitz, D., S. Devine, E. A. Davidson, P. Brando, and D. C. Nepstad (2010), Soil moisture depletion under simulated drought in the Amazon: impacts on deep root uptake, New Phytol, 187(3), 592-607.; Kumar, S., Newman, M., Wang, Y., & Livneh, B. (2019). Potential reemergence of seasonal soil moisture anomalies in North America. Journal of Climate, 32(10), 2707-2734; Dirmeyer, P. A., Wu, J., Norton, H. E., Dorigo, W. A., Quiring, S. M., Ford, T. W., ... & Balsamo, G. (2016). Confronting weather and climate models with observational data from soil moisture networks over the United States. Journal of Hydrometeorology, 17(4), 1049-1067.Sheffield, J., et al. (2013), North American Climate in CMIP5 Experiments. Part I: Evaluation of Historical Simulations of Continental and Regional Climatology*, J Climate, 26(23), 9209-9245, doi: 10.1175/jcli-d-12-00592.1. [Sanjiv Kumar, United States of America]	Taken into account: this line has been modified and the references to Sheffield 2013 and Keune and Miralles added to support the statement
74163	19	38	19	38	I don't see this being a logical flow, so remove "Thus" [Daniel J. Short Gianotti, United States of America]	Accepted
113219	19	42	19	42	End with '.'. This sentence did not build up too well from the content of the paragraph. [Diego Miralles, Belgium]	Noted: the paragraph has been substantially modified and now more clearly builds to the summary
43161	19	42			Read "2019; Lan et al., 2019; Zhang and Fueglistaler, 2019)." rather than "2019; Lan et al., 2019; Zhang and Fueglistaler, 2019)" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
113221	19	44	19	44	evaporative oceans'? [Diego Miralles, Belgium]	Rejected: we consider the text is correct
113223	19	44	19	44	wet parts of circulation'? This sentence is important and needs some rethinking. [Diego Miralles, Belgium]	Accepted: changes to "high precipitation regions"
112215	19	44	19	48	If dry gets drier and wet gets wetter is no longer valid, please consider updating with a phrase the reader will remember. [Rutger Hofste, Netherlands]	Noted: the statement that there is increased contrast between wet and dry regimes is deemed appropriate and the reviewer comment is noted in the context of communicating the findings in a succinct way (e.g. more intense wet and dry events). We now include the word intensity here rather than contrast e.g. very wet or dry seasons and weather patterns will intensify in a warming climate such that wet spells become wetter and dry spells drier
113225	19	46	19	47	correct 'based' twice [Diego Miralles, Belgium]	Accepted: 2nd based removed and sentence rearranged

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129103	19	47	19	48	[CONFIDENCE] The "high confidence" regarding an increased contrast between wet and dry meteorological regimes seems like an important conclusion, but the preceding two paragraphs aren't all that supportive of it, at least over land. The conclusion seems to come out of nowhere. [Trigg Talley, United States of America]	Taken into account: the last lines of the preceding paragraph are now revised to strengthen the supporting evidence for this statement
27229	19	47	19	48	This has not really been explained above. This summary of the what we shall remember is not straightforward from reading the section [Eric Brun, France]	Accepted: the last lines of the preceding paragraph are now revised to strengthen the supporting evidence for this statement
4847	19	47	19	48	Higher contract between wet and dry regimes does imply (support) the notion "wet gets wetter, dry gets drier", doesn't it? Earlier text phrases suggest that over land this notion is not valid [Bart van den Hurk, Netherlands]	Taken into account: the change is to emphasise the intensification of wet and dry events as now stated
113227	19	48	19	48	meteorological' for 'climatological' [Diego Miralles, Belgium]	Reject: climatological implies fixed region but what is conveyed is meteorological events
26473	19	51	22	17	Polar regions are not sufficiently addressed in the section 8.2.2.2. The text adresses impacts of Arctic amplification on mid-latitude water cycle, but does not tell anything about the water cycle within the Arctic (and Antarctic). Please see for example the Arctic Freshwater synthesis (Vihma et al. 2016). Changes in large-scale circulation have affected the moisture transport within the Arctic (Nygård et al. 2020). References: (1) Vihma, T., Screen, J., Tjernström, M., Newton, B., Zhang, X., Popova, V., Deser, C., Holland, M., and Prowse, T. (2016), The atmospheric role in the Arctic water cycle: A review on processes, past and future changes, and their impacts, J. Geophys. Res. Biogeosci., 121, 586– 620, doi:10.1002/2015JG003132., (2) Nygård T., Naakka T., Vihma T. (2020): Horizontal moisture transport dominates the regional moistening patterns in the Arctic. Journal of Climate. doi: https://doi.org/10.1175/JCLI-D-19-0891.1 [Tiina Nygård, Finland]	Taken into account: this section deals with large-scale atmospheric circulation changes and their influence on the water cycle. The references provided are however added to the assessment of atmospheric moisture changes in Section 8.3.1.2 and the Vihma et al. review cited as additional evidence that process understanding of changes in circulation is not well understood
109685	19	51	22	27	Hydrologists, emergency management professionals, and water resource scientists, engineers, and managers often use coherent patterns of ocean-atmosphere variability (ENSO, PDO, NAO, etc) as convenient and reasonably effective ways of understanding and predicting hydrologic (e.g., river runoff or aquifer level) variability and its implications for flood hazards, water supplies, and so forth. It would be appropriate and useful to the broad readership of an IPCC report to briefly mention in this section, therefore, that paleoclimatic evidence has demonstrated that certain teleconnections, such as those of PDO to river flow in the northern Great Plains of North America, have shifted spatially over generational to centennial timescales (Fleming and Sauchyn, 2013, Availability, volatility, stability, and teleconnectivity changes in prairie water supply from Canadian Rocky Mountain sources over the last millennium, Water Resources Research, 49, 64-74.) These previous natural shifts are likely to continue going forward, with increasing uncertainty in their frequency and severity under global anthropogenic climate change. It also bears mentioning that climate change will modify the nature of river flow teleconnections; an example is climate change-driven glacier recession, which will affect the way that patterns like ENSO and PDO will affect river flows by changing the amount of glacial ice available for seasonal melt production (Fleming et al., 2016, Seasonal flows of international British Columbia-Alaska rivers:the nonlinear influence of ocean-atmosphere circulation patterns, Advances in Water Resources, 87, 42-55). [Sean Fleming, United States of America]	Rejected: here understanding of how large-scale aspects of atmospheric circulation respond to warming is assessed while more details of modes of atmospheric variability are assessed in 8.3-8.4.
53199	19	51			This subsection could also assess more explicitly the large-scale drivers of regional droughts (e.g., Hoerling and Kumar 2003, Kingston, 2015; Schubert et al., 2016)? Beyond the large-scale circulation features discussed in CH2-4, it could also provide a more regional perspective about the influence of weather circulation patterns on wet and dry extremes (e.g., Barlow et al. 2020 over North America, or Richardson et al. 2018 over Europe) https://doi.org/10.1007/s00382-019-04958-z https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/joc.5199 [Hervé Douville, France]	Taken into account: a detailed assessment of drivers of regional drought are considered beyond the scope of this section on large-scale circulation but the progress in linking persistent weather patterns with wet and dry hydrological extremes since AR5 is now stated with reference to Kingston et al. 2015; Barlow et al 2019 and Richardson et al. 2018
69475	19	53	19	55	Does the opening sentence of this section refer to observed changes or those projected by models? [Martin Singh, Australia]	Taken into account: the sentence is revised to make it clear this refers to changes in response to a warming climate (so can equally be historical or future projections)

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45259	19	53	22	27	Please consider shortening the section 8.2.2.2 [Krishnan Raghavan, India]	Rejected: it is not possible to reduce this section any further if the understanding of the primary large-scale circulation changes are covered across the globe. In fact Section 8.2 did not increase in length despite the addition of new assessment of recent studies which was unfortunately not the case for Section 8.3
4849	19	54	19	55	this quote between bracket is too important to put between brackets and deserves a reference, either to literature or an AR6 section [Bart van den Hurk, Netherlands]	Accepted: the sentence was updated as suggested with reference to the AR5 report as intended
53201	19	55			Could be tempered: "and can modulate or even dominate water cycle changes at the regional scale"? [Hervé Douville, France]	Accepted: now "can" is added to temper this statement
43163	20	1			Read "of freshwater " rather than "of fresh water " [Cyriaque Rufin Nguimalet, Central African Republic]	Rejected: fresh water is correct
89049	20	2			This is another instance of "fast" and "slow", which is a shorthand that needs to be explained somewhere and then probably referenced or repeated here. [Angeline Pendergrass, United States of America]	Taken into account: this sentence has been removed but a reference to Section 8.2 added for reference to fast/rapid and slow precipitation responses
53203	20	3	20	4	also Chadwick et al. (2017) as a pilot study for CFMIP [Hervé Douville, France]	Rejected: this sentence has been removed
20119	20	5	20	5	Held and Soden [philippe waldteufel, France]	Rejected: Held and Soden is quoted elsewhere as a more general reference or inferred from mention of AR5 findings so is not necessary above the more up to date and focused studies quoted here
67591	20	6	20	6	Radiative forcing [Yang Tian, United States of America]	Accepted: correction made
53205	20	8	20	9	also Liu et al. (2018) based on PDRMIP [Hervé Douville, France]	Accepted: reference added
70987	20	10	20	10	Ceppi et al. (2018, already cited in this chapter) would also be a good reference for this point, as the slow response does noticeably evolve with time [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: reference added
1257	20	14	20	15	There is evidence to the contrary of the following sentence "Long-term weakening of the tropical atmospheric overturning circulation is expected as climate warms in response to elevated CO2 (Collins et al., 2013b)" - at least in the middle troposphere (according to ERAINT): Benestad (2016; DOI: 10.1007/s00704-016-1732-y) indicates that there has been an increase in the mid-tropospheric overturning based on the ERAINT reanalysis. The paper also explaines why it makes physical sense, albeit in heuristic terms. There appears to be different trends at different heights in the atmosphere according to the ERAINT reanalysis. The measure for the atmospheric overturning in this analysis is different to the cited studies, and represents the global troposphere between 1000 and 6500 m.a.s.l. [Rasmus Benestad, Norway]	Rejected: the strengthening over the recent period since the 1990s is mentioned later in the section with regard to internal variability and therefore does not constitute long-term changes as referred to here
88125	20	20	20	20	The weakened circulation has been shown by several studies to be associated with an increased depth of convection and tropopause height, but I don’t think causality has been demonstrated. [Robin Chadwick, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: now replaced with associated to convey the lack of causality

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74375	20	22	20	22	to correct eastern in the sentence … increases in the eastern Pacific [Moulay Driss HASNAOUI, Morocco]	Accepted: correction made
67593	20	23	20	23	ENSO variability, it is too simple… [Yang Tian, United States of America]	Accepted: correction made
4853	20	27	20	27	"shift" -> "extension"? [Bart van den Hurk, Netherlands]	Accepted: correction made
45255	20	30	20	41	The paragraph "For the last glacial maximum …with a weaker Walker Circulation" introduces a sudden discontinuity to the flow of the text. [Krishnan Raghavan, India]	Taken into account: sentence rearranged to remove the discontinuity
100705	20	37	20	37	Note: I'm not sure what "Box 10" refers to here. [Matthew Kohn, United States of America]	Accepted: correction to Cross Chapter Box 2.4
20121	20	37	20	37	Would that be Cross Chapter Box 2.4? [philippe waldteufel, France]	Accepted: correction to Cross Chapter Box 2.4
22263	20	37	20	41	The box is cross-chapter box 2.4 [Peter Thorne, Ireland]	Accepted: correction to Cross Chapter Box 2.4
83525	20	39	20	39	Add here also the new SST review by McClymont et al. (submitted), which is cited in Chapter 2: McClymont, E. L., Ford, H. L., Ho, S. L., Tindall, J. C., Haywood, A. M., Alonso-Garcia, M., et al. (submitted). Lessons from a high CO2 world: an ocean view from ~ 3 million years ago. Clim. Past Discuss. (submitted). 41 doi:10.5194/cp-2019-161. [Antje H. L. Voelker, Portugal]	Accepted: reference added
129105	20	40			This would be a chance to bring in results from DeepMIP and EoMIP (coordinated experiments for Eocene/ 4xCO2). [Trigg Talley, United States of America]	Noted: a link to the paleo evidence is strengthened with reference to McClymont et al. and Cross Chapter Box 2.4 and Section 3.3.2
45257	20	43	21	4	This paragraph is rather too technical and may be simplified. [Krishnan Raghavan, India]	Taken into account: the paragraph cannot be much further simplified without the message being lost but it has been rewritten to improve the clarity
93599	20	44	20	46	I think this sentence has to be clarified, especially because the reduced temperature lapse rate (in absolute value) leads to an increase of the cooling rate in the high troposphere. [Jean-Louis Dufresne, France]	Taken into account: this sentence has been removed and the paragraph modified to convey this message
88127	20	53	20	53	SST pattern change doesn't only affect the details of the precipitation pattern response. Over the tropical oceans, it dominates the pattern of this response, e.g. Chadwick et al., 2017 (already referenced in this chapter). [Robin Chadwick, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: replaced with "can be dominated" and reference included
31461	21	1	21	2	A paper by Zhou et al. in JC 2020 (10.1175/JCLI-D-19-0922.1) suggests that over the tropical Pacific, the meridional pattern of the SST bias plays a key role in forming the rainfall bias, not only the equatorial SST bias. [Shijie Zhou, China]	Accepted: modified accordingly and reference added
129107	21	3			This would be a good opportunity to discuss *why* there could be precipitation biases in the tropical pacific. Marine stratocumulus cloud biases (attendant precipitation / olr biases / strengths?). [Trigg Talley, United States of America]	Rejected: this is beyond the scope of the discussion on expected changes in the water cycle from large-scale circulation change
12765	21	3			Change wording to, "underestimated response of tropical precipitation to warming" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: modified accordingly

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53207	21	7	21	9	although recent idealized simulations suggest that these are not the only drivers (Biasutti and Voigt, 2020)? [Hervé Douville, France]	Taken into account: the reference to Biasutti and Voigy 2019 added to convey the advanced knowledge of links between AHT and ITCZ position since AR5
41537	21	18	21	18	Suggest removing "Dynamical" as the first word in this sentence. A "dynamical understanding" is a particular type of understanding, typically based on momentum or vorticity arguments. This type of understanding is alluded to later in the sentence by using the word "dynamic". [Michael Byrne, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
67595	21	18	21	19	position, but energetic and dynamic… [Yang Tian, United States of America]	Not applicable: this sentence has been modified
64925	21	18	21	20	Is it possible to provide an assessment as to which mechanism is most relevant? [Johannes Quaas, Germany]	Taken into account: the paragraph has been re-written to emphasise that the strengthening/tightening of the ITCZ is best understood aspect of ITCZ responses to warming
7585	21	21	21	21	1/1: Also cite Bonfils et al. (submitted): "Allen et al., 2015a; Dong and Sutton, 2015a; Chung and Soden, 2017, Bonfils et al. (submitted)." This paper explains the forced components controlling the shift in ITCZ from 1850 to 2019 (Figure 8.11, right panel). [Celine Bonfils, United States of America]	Rejected: this sentence has been removed
12767	21	25			Why is global needed here? It confuses the sentence. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: global removed
129109	21	26			Citing McGee et al. (2014) coverage of PMIP2 era simulations cannot stand here alone to assert that the sum-total of all paleo knowledge is that all ITCZ variability is within 1degree. No vegetation feedbacks are underestimated in CMIP models, which alters surface albedo ... and can dramatically alter the implied energy balances attendant to ITCZ changes (see Boos and Korty, 2016, DOI: 10.1038/NGEO2833). More PMIP papers than really belong here paint a much more nuanced view, completely obliterated with this definitive statement. [Trigg Talley, United States of America]	Accepted: the explicit discussion of the McGee result is now removed and instead focus is on the distinct regional ITCZ responses (including assessment of new work by Mamalakis et al. 2021) which is a more robust aspect while a more detailed discussion of vegetation feedbacks is beyond the scope of this section
129111	21	27			Where is the reference / coverage of Hydro2k? This is literally what this whole section should be about. The volcano bit is WAY more complicated. Without spending some time about tropical versus predominantly NH or SH volcanoes (doi:10.5194/esd-7-681-2016), this is so very vague as to be not useful. [Trigg Talley, United States of America]	Taken into account: a more general statement emphasising the paleo evidence for regional changes in the ITCZ related to hemispherically asymmetric volcanic and orbital forcing is now made, including Hydro2k. A more detailed discussion is beyond the scope of this section.
67597	21	28	21	28	(…2018), and [Yang Tian, United States of America]	Not applicable: this text has been removed
12769	21	28			Why has AD been introduced here? It is not generally used alongside other years described in this chapter. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable: this text has been removed

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6907	21	30	21	32	The "(decades, >1deg latitude)" wording could be better, e.g., "(>1deg latitude over decades)" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
109399	21	34	21	37	I think that authors should devote more emphasis on recent findings about the link between monsoons and the energy framework. There is a wide literature starting from Neelin and Held 1987 to the more recent Jalihal et al., 2020. Some refs here: Neelin, J., and I. Held, 1987: Modeling tropical convergence based on the moist static energy budget. Monthly Weather Review,115 (1), 3–12.). Chou, C., and C.-A. Chen, 2010: Depth of convection and the weakening of tropical circulation in global warming.Journal of climate,23 (11), 3019–3030. Chou, C., J. Neelin, and H. Su, 2001: Ocean-atmosphere-land feedbacks in an idealized monsoon. Quarterly Journal of the Royal Meteorological Society, 127 (576), 1869–1891. Jalihal, C., Srinivasan, J., & Chakraborty, A. (2019). Modulation of Indian monsoon by water vapor and cloud feedback over the past 22,000 years. Nature Communications, 10(1), 1-8. [Roberta D'Agostino, Germany]	Rejected: a detailed discussion of pre-AR5 literature on energetic frameworks for monsoon change are beyond the scope of this brief section
72275	21	34	21	43	A post-AR5 study on future summer East Asian monsoon (Ose et al., 2020, accepted) gives a good example indicating that the future summer sea-level pressure pattern and precipitation over East Asia are determined by a subtle balance among several large-scale atmospheric responses to the land-sea warming contrast, the future tropical and sub-tropical SST patterns and the weakened vertical monsoon circulations over the Asia and Pacific region. (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]	Rejected: this section deals with physical understanding in basic monsoon responses so does not in general include new literature on projections unless they provide insight into fundamental processes
4855	21	35	21	35	If one refers to "input" and "export" of energy terms in the monsoon systems, it would be good to define the boundaries of such a system. I understand a monsoon as a land-sea contrast phenomen and thus could consider input or expert differently for the sea and land part of the system [Bart van den Hurk, Netherlands]	Rejected: since the emphasis is on understanding inputs and exports rather than quantifying magnitudes and signs, it is not necessary to define input to the monsoon region which is in any case implied
113229	21	38	21	38	is' for 'are' [Diego Miralles, Belgium]	rejected: "increases…are" is correct
6909	21	40	21	42	Does this include the possibility of decadal/internal variability? The way it is currently worded ("associated changes in SST") appears to exclude this, but otherwise this whole paragraph (line 34 to line 2 over the page) does not allow for the possibility of internal multi-decadal change arising from SST patterns or hemispheric shifts. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: "Associated" is removed so internal variability is not excluded
1259	21	50	21	51	How does the following sentence "increased convective instability due to more water vapour in a warmer world" fit with previous statements about reduced atmospheric overturning? (p. 20, L14-15) [Rasmus Benestad, Norway]	No longer applicable: this sentence has been simplified so this question no longer applies
14847	21	51	21	51	Which indication can be obtained from interglacial prior mid-Holocene, or from older warm periods? [Marie-France Loutre, Switzerland]	Taken into account: the assessment is sharpened to reflect the lack of consensus for how past responses may be interpreted in relation to future projections
116709	21	51	21	55	Please shorten and sharpen the assessment here. The notion of "past analogue" (or lack of analogue) needs to be introduced somewhere once (chapter 1?). [Valerie Masson-Delmotte, France]	Accepted: a statement is added to sharpen the assessment that a thermodynamic strengthening of monsoons is partly offset by slowing of the tropical circulation but understanding of regional circulation changes is not robust.

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129113	21	51			"Considering the mid-Holocene as a potential analogue for future global warming...." This is silly. Mid-Holocene did have orbital changes enhancing NH insolation / seasonality, BUT IT ALSO HAD DECREASES IN METHANE OF 15% PREINDUSTRIAL VALUES, which is literally the opposite of what one would want to consider as an analog for future warming. These GHG changes *cool* the tropics. Rethink this section. [Trigg Talley, United States of America]	No longer applicable: this sentence has been removed
4857	21	53	21	53	"affected" -> "underestimated" [Bart van den Hurk, Netherlands]	Not applicable: this sentence has been rewritten
67599	21	54	21	55	However, differing processes drive Northern Hemisphere monsoon intensification for the two periods, suggesting that … [Yang Tian, United States of America]	Not applicable: this sentence has been generalised
116707	21		21		Coordination with other chapter on ITCZ and monsoon is needed, to avoid assessing subsets of literature and having different formulations to reflect the same state of knowledge (esp with chapters 4, 5, 6, 7) [Valerie Masson-Delmotte, France]	Accepted: links to and consistency with 2.3.1.4.2, 10.4.2.1 and 5.1.3 are confirmed
112217	22	1	22	10	Human and other influences on the water cycle should be included. Desalination, inter-basin transfer, evaporative cooling proceses. Alternatively include: "primarily". [Rutger Hofste, Netherlands]	Rejected: this section deals with physical understanding of large-scale circulation responses and direct human influence on the water cycle is covered in Section 8.2.3.4
67601	22	4	22	7	Poleward expansion of the tropical belt is expected to drive a corresponding shift in mid-latitude storm tracks, but the driving mechanisms differ between hemispheres. Southern Hemisphere expansion is more strongly affected by GHG forcing with stratospheric ozone depletion amplifying these changes, whereas tropospheric ozone and aerosol forcing contribute to Northern Hemisphere expansion. [Yang Tian, United States of America]	Accepted: suggested splitting of sentences is applied
30693	22	4	22	8	There is now significant eveidence to indicate that, in part, the mid-latitude storm track changes themselves drive opleward expansion of the tropical belt. Make reference in this context to Rudeva et al., 2019: Midlatitude fronts and variability in the Southern Hemisphere tropical width. J. Clim., 32, 8243-8260, doi: 10.1175/JCLI-D-18-0782.1. [Ian Simmonds, Australia]	Rejected: this is not a sufficiently fundamental aspect of large-scale circulation change so is not referenced here
4859	22	4	22	8	complex sentence, suggest to break up [Bart van den Hurk, Netherlands]	Accepted: sentence split to improve readability
53209	22	5			Do you mean clearly or strongly? [Hervé Douville, France]	Taken into account: this sentence has been modified to more clearly explain
53211	22	8	22	11	The polar stratospheric vortex is another important driver of the tropospheric jet response that may also need a brief discussion (e.g., Zappa et al., 2017, Oudar et al., 2020) [Hervé Douville, France]	Rejected: the polar vortex is not deemed central to the brief discussion of large-scale drivers here
4861	22	14	22	17	Arctic amplification and jet stream are stronger in winter than summer; sure that this link is dominant in summer? [Bart van den Hurk, Netherlands]	Rejected: the seasonality is beyond the scope of the limited discussion here
99349	22	15	22	17	The discussion regarding a weaker Jet stream and early holocene precipitation is seems weak as it is based on a large databasing exercise with little detail on how reliably the proxies record precipitation, as opposed to temperature, or other properties of the environmental system. [Simon Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: these sentences are removed
27231	22	19	22	19	GHG-induced changes in water cycle is not compared to natural variability in what is written above [Eric Brun, France]	Accepted: this summary statement is revised along with text in the first paragraph of the sub-section that provides evidence for this statement
64927	22	19	22	21	What does the confidence statement refer to? is it that it is true for “most” regions? [Johannes Quaas, Germany]	Taken into account: this summary statement is revised along with text in the first paragraph of the sub-section that provides evidence for this statement
53213	22	19			Remove the comparison with natural variability which may be both scenario and region-dependent? [Hervé Douville, France]	Accepted: this summary statement is revised along with text in the first paragraph of the sub-section that provides evidence for this statement

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64929	22	22	22	22	Is a single line of evidence (“idealised models”) sufficient for such a likelihood statement? [Johannes Quaas, Germany]	Taken into account: the summary statements have been updated to reflect the body of evidence presented in the sub section
69479	22	30	24	55	A somewhat general comment about this section: It seems to lack a bit of structure; the pargraphs are very long and talk about multiple issues. There is a mixture of fairly general discussion and very specific points (about Canadian snowpacks for instance) without the overarching point being clear. I think this section would be improved by a bit more structure and a clearer identification of the main points, as at the moment I feel a bit lost. [Martin Singh, Australia]	Noted: the section has been restructured and rewritten (now 8.2.3.1 deals with cryosphere aspects)
103689	22	32	22	32	This introduction is very helpful: why not start all subsections with a few summarizing lines? [Philippe Tulkens, Belgium]	Noted: some sub-sections benefit from introductory lines but others seemed to require a more thorough introductory paragraph
20477	22	32	22	32	Please define what a regional water cycle is, and when it is possible to identify such a regime. One would imagine its kernel to be conjugated evaporation-precipitation-run off behaviours without strong dependencies on boundary conditions? Or what? [philippe waldteufel, France]	Rejected: local and regional are not precisely defined but both are at the sub-continental scale yet depend upon the process being discussed so we therefore keep these introductory lines general.
105559	22	37	25	8	Land-atmopshere interaction plays an imporant role that affect global circulation due to assymetric warming (Kumar et al., 2020; Fig. A2)or the negative feedback (Zeppetello et al., 2019; Fig. 8); therefore affect the water cycle over land (Kumar et al., 2020). Vargas Zeppetello, L. R., Battisti, D. S., & Baker, M. B. (2019). The Origin of Soil Moisture Evaporation “Regimes”. Journal of Climate, 32(20), 6939-6960.;Kumar, S., Newman, M., Lawrence, D. M., Lo, M. H., Akula, S., Lan, C. W., ... & Lombardozzi, D. (2020). The GLACE-Hydrology Experiment: Effects of Land-Atmosphere Coupling on Soil Moisture Variability and Predictability. Journal of Climate, (2020). https://journals.ametsoc.org/doi/abs/10.1175/JCLI-D-19-0598.1 [Sanjiv Kumar, United States of America]	Noted - this section has been redistributed and the more detailed discussion proposed, though useful is not deemed central to the assessment
45261	22	37	25	8	Please consider shortening the section 8.2.3.1 [Krishnan Raghavan, India]	Taken into account: this section has been re-ordered and re-written to shorten and improve the logical structure, focusing on cryosphere-related changes
129115	22	37			The recent Milly and Dunne study in Science seems relevant here (or in one of the later sections discussing snow and climate change). DOI: 10.1126/science.aay9187 [Trigg Talley, United States of America]	Accepted: reference included in assessment of cryosphere processes
27233	22	37			The organisation of this section does not facilitate the reading. The authors go from discussing precipitation then runoff or soil moisture then precipitation again, various latitudinal regions without consistency. This section would deserve a more logical flow: either by types of regions (cold, tropical, temperate for example) or by processes (precipitation, soil moisture and runoff, ...) [Eric Brun, France]	Taken into account: this section has been re-ordered and re-written to shorten and improve the logical structure. The section now deals with cryosphere-related changes in the water cycle
131569	22	39	22	39	What type of scale is large when you say "large-scale"? E.g. Is it "large-area"? [Hans Poertner and WGII TSU, Germany]	Rejected: large area is implied and this is made clearer by adding in "circulation"
4863	22	41	22	42	Unclear what you want to convey with this phrase [Bart van den Hurk, Netherlands]	Accepted: this sentence is removed and the first sentence revised top make the introduction to this sub-section clearer
51449	22	43	22	43	Typo: However [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: typo corrected on next page

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
74165	22	44	22	44	Since this section is ostensibly about land surface hydrology, maybe it's worth talking a bit about land surface hydrology before the un-marked section on glacial melting. A proposed paragraph could be something like, "The terrestrial water cycle is determined by precipitation and by the local energetic partitioning of sensible and latent heat fluxes, which are in turn determined by local moisture availability. This moisture is stored in soil, surface water, and ice reservoirs, and its evaporation to the atmosphere is controlled by moisture-state dependent surface and resistance (Salvucci & Gentine https://doi.org/10.1073/pnas.1215844110, McColl & Rigden 2020 https://doi.org/10.1029/2020GL087101), vegetation stress (Damour 2010 https://doi.org/10.1111/j.1365-3040.2010.02181.x), and coupled boundary layer processes (Santanello et al 2018 https://doi.org/10.1175/BAMS-D-17-0001.1). Soil moisture acts as a primary control on turbulent flux partitioning (Short Gianotti et al. 2019 https://doi.org/10.1029/2018WR023726) and land-atmosphere feedbacks (Berg et al. 2014 https://doi.org/10.1175/JCLI-D-13-00591.1), and determines regions dominated by moisture-limited and energy-limited latent heat fluxes." [Daniel J. Short Gianotti, United States of America]	No longer applicable: this section has been restructured and no longer includes a general land surface processes subsection so we do not include this suggested text here
81639	22	44	23	27	this text jumps a lot between high latitude permafrost, mountain glaciers and other topics, which makes it hard to follow and identify key messages [Sönke Zaehle, Germany]	Taken into account: the section has been reordered and re-written to improve the logical structure with a focus on cryosphere processes in 8.2.3.1
53215	22	44			also permafrost [Hervé Douville, France]	Accepted: permafrost added
59001	22	46	22	47	Not true that degrading permafrost will decrease water security---Arctic permafrost degradation has increased baseflow (e.g., Lamontagne-Hall et al., 2018, https://doi.org/10.1088/1748-9326/aad404) and is expected to further increase groundwater as a source of potable water (e.g., Lemieux et al., 2020, al https://doi.org/10.1007/s10040-020-02131-z). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: the suggestion that permafrost decreases water security is removed
4865	22	47	22	47	Not trivial that declines in frozen ground automatically lead to reduced water and food security [Bart van den Hurk, Netherlands]	Accepted: the suggestion that permafrost decreases water security is removed
59033	22	47	22	48	In addition to the reduction in soil ice and thermokarst lake coverage, melting permafrost can also the receiving waters' chemistry (Roberts et al. 2017; Kokelj et al. 2015) and release sequestered mercury (St. Pierre et al. 2018; Rydberg et al. 2010); thus, compounding a decrease in water and food security in Northern communities. It may be worth a slight mention here, and then expand in chapter 9 (9.5). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected: impacts beyond the water cycle are beyond the scope of this section
16123	22	47	22	48	"Permafrost degradation reduces soil ice and the extent of thermokarst lake coverage" Certainly true in the long term, but doesn't permafrost degradation first increase thermokarst formation? [Gerhard Krinner, France]	Accepted: the suggestion that thermokarst lake area is always reduced is removed
66413	22	47	22	49	Permafrost degradation can also increase thermokarst lake area, current wording makes it seem that this is always a loss of thermokarst lake area. [Charles Koven, United States of America]	Accepted: the suggestion that thermokarst lake area is always reduced is removed
16131	22	48	22	48	Thank you for referring to Chapter 9. The corresponding section is now 9.5.2 (not 9.5.3 as in the FOD). [Gerhard Krinner, France]	Accepted: update made

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66415	22	49	22	51	The first statement here (that "Soils (including permafrost) warm more slowly at depth than the near-surface atmosphere") is somewhat misleading, and the second ("that depends on the presence of surface snow cover") isn't very specific. Within at least the uppermost meter of soils, delays in the warming are not significant, and the role of snow is specifically that its loss with warming removes wintertime insulation that would otherwise warm the soil, thus where snow is lost with warming, the soil warms more slowly than the near-surface air does. The upper meter or so of soil is incredubly important for hydrologic processes so should not be ignored here. See Soong et al., (2020) https://doi.org/10.1029/2020JG005668 for discussion of each of these points. It is true that at depths of tens of meters there are delays in warming, but in that case it isn't really soil you are describing, but near-surface sediments and weathered bedrock. [Charles Koven, United States of America]	Accepted: this sentence is revised to note the insulating effect of snow and incorporate the Soong et al. and Garcia references although the seasonal complexity is not addressed here (e.g. snow loss in winter can increase soil cooling) since it is not of central importance to the key messages
81149	22	53	22	55	This is something most decision makers do not know. Please highlight this in release information [Mary Matthews, Azerbaijan]	Noted: this statement comes from Chapter 9 which assesses the relevant key messages
4867	22	54	22	54	start new sentence at "and" [Bart van den Hurk, Netherlands]	Accepted: sentence split
3365	22		4	27	It is essential to have recourse to examples of a broadening of ideas [Eduardo Erazo Acosta, Colombia]	Noted: the assessment continues to point to a lack of simple underpinning theory for how Arctic amplification on mid-latitude weather
116713	22		22		References to links between Arctic amplification and mid latitude climate need also to refer to the corresponding assessment in this report (Cross-Chapter Box 10.1:) which suggests low confidence for possible influence. [Valerie Masson-Delmotte, France]	Accepted: cross chapter box 10.1 is now correctly referenced
20479	23	1	23	6	"Linked" on Line 1? "Relating to" on line 6? In case there is a definite causality relationship in which the authors have some confidence, they should definitely spell it out. Expressions such as "linked to" or "related to" may be understood as "sorry I am unable to say in which direction the causality is flowing", or worse "I don't care about causality"... [philippe waldteufel, France]	Accepted: these statements are modified to convey the certainty ("can" and "explained by")
65767	23	3	23	5	Suggest additional reference: Arheimer, B., Lindström, G. and Donnelly, C. 2017. Regulation of snow-fed rivers affects flow regimes more than climate change. Nature Communications, 8. 10.1038/s41467-017-00092-8 [Kushla Munro, Australia]	Taken into account: this reference is now assessed in Section 8.2.3.4
65769	23	5	23	7	Suggest clarification since this is not true for all snow dominated regions (i.e. in some regions snowfall is decreasing). [Kushla Munro, Australia]	Not applicable: this statement no longer exists
111515	23	12	23	12	Add reference on New Zealand glaciers: Mackintosh, A. N., Anderson, B. M., Lorrey, A. M., Renwick, J. A., Frei, P., & Dean, S. M. (2017). Regional cooling caused recent New Zealand glacier advances in a period of global warming. Nature Communications, 8. doi:10.1038/ncomms14202 [James Renwick, New Zealand]	Accepted: reference added
81151	23	15	23	18	This needs to be highlighted for decsion makers also [Mary Matthews, Azerbaijan]	Noted: this point is also covered in Chapter 9
4869	23	16	23	16	could also use reference to Immerzeel, W.W., Lutz, A.F., Andrade, M. et al. Importance and vulnerability of the world's water towers. Nature 577, 364–369 (2020). https://doi.org/10.1038/s41586-019-1822-y [Bart van den Hurk, Netherlands]	Accepted: reference added

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26475	23	21	23	27	Loss of sea ice potentially increases evaporation, but evaporation is efficient only when the air is relatively dry. When the sea ice is diminishing, the sea area where cold, dry air masses can be formed is simultaneously diminishing, which may limit the evaporation efficiency. Large horizontal moisture transport is able to suppress local evaporation. I would add a sentense: "Large horizontal moisture transport tends to suppress local evaporation, and therefore the long-term changes in evaporation are largely controlled by horizontal moisture transport and atmospheric circulation (Nygård et al 2020)." Nygård T., Naakka T., Vihma T. (2020): Horizontal moisture transport dominates the regional moistening patterns in the Arctic. Journal of Climate. doi: https://doi.org/10.1175/JCLI-D-19-0891.1 [Tiina Nygård, Finland]	Accepted: this reference is now included in a more nuanced statement which is moved back to Section 8.3.1.2 on moisture transports
81641	23	29	23	30	be more precise here and separate the direct effects on transpiration and the indirect effect through vegetation control on interceptions and soil evaporation, rather than lumping this altogether into evapotranspiration. This would also make the link the WUE clearer, which in my view requires a brief explanation (in the lines of coupled carbon uptake and water loss, controlled by conductance, gives rise to WUE) [Sönke Zaehle, Germany]	Taken into account. We have clarified what WUE is here in this sentence.
81065	23	29	23	55	Chapter 5 section 5.4.1 dives in more detail on the evidence for increased WUE, it would be appropriate for the reader to have the link to that section if they want to learn more about it. Chapter 5 will do the same. [canadell pep, Australia]	Accepted, we now link to 5.4.1 (note that soil water aspects of 8.2.3.1 have been moved to 8.2.3.3 or deleted)
69477	23	29	24	10	This paragraph is extremely long. It would be useful to split it up a little. At the very least, I think one could divide it into a paragraph focussed on the plant physiological effect and a paragraph associated with snowpack changes. [Martin Singh, Australia]	Accepted, this paragraph has been rewritten and has been moved to 8.2.3.3
113231	23	30	23	30	You mean 'transpiration' only. Or 'transpiraton and interception loss', even better. I am thinking this may be a good reference, unfortunatelly still in review: https://www.essoar.org/doi/pdf/10.1002/essoar.10503229.1 [Diego Miralles, Belgium]	Taken into account, this sentence has been rephrased.
81643	23	30	23	35	consider making a connection to Chapter 5.4.1 [Sönke Zaehle, Germany]	Accepted, we now link to 5.4.1.
53217	23	30			through canopy interception, evapotranspiration, and radiative transfer, as well as to the root influence on soil hydraulic conductivity, thereby contributing to complex feedbacks through the surface energy, water and carbon budgets. [Hervé Douville, France]	Taken into account, this sentence has been rephrased.
109571	23	31	23	32	It seems that a change in intrinsic WUE (A/gs) is a primary response to CO2, perhaps even more so than a change in either A or gs separately. If A increases in the same proportion as iWUE then gs would not change, or if the proportional change in A is less than in iWUE then gs decreases (). Suggest rephrasing “that increasing atmospheric CO2 consistently increases water-use efficiency (WUE) through the combined enhancement of photosynthesis and stomatal regulation” [Anthony Walker, United States of America]	Accepted, modified as suggested
109573	23	31	23	32	“and increasing rates of plant growth and carbon sequestration” seems unnecessary to the point being made, and is dealt with in detail in Chapter 5. Suggest deleting. [Anthony Walker, United States of America]	Accepted, deleted.
67603	23	31	23	33	CO2 enhances photosynthesis and stomata regulation, thereby increasing water-use efficiency (WUE) and rates of plant growth. The CO2 sequestration that are regionally dependent based on detailed modelling corroborated by empirical evidence. [Yang Tian, United States of America]	Noted, the last half of sentence has been deleted

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70327	23	31	23	35	“concluded there is high confidence that increasing atmospheric CO2 enhances photosynthesis and stomata regulation thereby increasing water-use efficiency (WUE) and increasing rates of plant growth and carbon sequestration that are regionally dependent based on detailed modelling corroborated by empirical evidence (De Kauwe et al., 2013; Jones et al., 2013b; Deryng et al., 2016; Swann et al., 2016; Knauer et al., 2017; Guerrieri et al., 2019). “ This sentence is hard to understand. There are five different things listed (photosynthesis, stomatal regulation, WUE, plant growth, carbon sequestration) that are all simultaneously changing, but although they are coupled to one another and all influenced by CO2, they are not all changing directly in concert. As written it is not obvious that these are all different aspects, nor does this seem like the place to get into the details of how carbon sequestration may not change directly in relation to changes in photosynthesis. Further, this list misses calling out the direct importance of leaf area growth (not just plant growth) for altering water fluxes. We suggest that the relevant factors for water fluxes are introduced and discussed directly, rather than lumped into a long and confusing list. Plant responses to CO2 can influence land evapotranspiration through two mechanisms, which have opposite effects on evapotranspiration: (1) increased rates of photosynthesis and increases in leaf area, which increase transpiration, and (2) stomatal closure, which decreases transpiration. This may mean shifting the emphasis away from WUE. Under high CO2 WUE likely increases, but it does so as a combination of both increasing photosynthesis and a change in stomatal regulation thus alone does not determine the actual evapotranspiration flux, which is what is being discussed here. [Abigail Swann, United States of America]	Taken into account. This sentence has been rephrased to make it more clear what components contribute to WUE, following the suggestion of another reviewer
4871	23	33	23	33	move "based on detailed modelling corroborated by empirical evidence" to beginning of sentence [Bart van den Hurk, Netherlands]	Taken into account. This part of the sentence has been deleted.
113233	23	34	23	40	In this dicussion, please incorporate (main message in the title): Cheng, L., Zhang, L., Wang, Y.-P., Canadell, J. G., Chiew, F. H. S., Beringer, J., Li, L., Miralles, D. G., Piao, S. and Zhang, Y.: Recent increases in terrestrial carbon uptake at little cost to the water cycle, Nature Communications, 1–10, doi:10.1038/s41467-017-00114-5, 2017. It essentially states that effects of greening and WUE on ET balance each other, thus goes in the direction of your sentence below 'limitations on soil drying and runoff increases from increased water use efficiency as stomata contract at higher CO2 levels can be counteracted by enhanced photosynthesis over mid-latitudes'. [Diego Miralles, Belgium]	Accepted, this reference has been added
70329	23	35	23	38	This section starts by intending to say that there remains uncertainty in the magnitude of WUE response and thus if it will *completely* compensate for the effects of increased evaporation demand. However the wording used is “counterbalance losses by evapotranspiration” which appears to be a statement about photosynthesis and not water flux given that WUE is calculated as photosynthesis divided by evapotranspiration. It would be more accurate to say “increased evaporative demand” rather than “losses by evapotranspiration”. It would be helpful if the authors clearly stated that WUE is calculated as photosynthetic rate over transpiration rate, thus higher WUE does not necessarily mean lower transpiration and lower water use. [Abigail Swann, United States of America]	Taken into account, we have rephrased this sentence accordingly.

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70331	23	35	23	38	The larger issue is that this set of statements diminishes the importance of changes in evapotranspiration driven by plant processes by suggesting that these processes are not relevant unless they lead to complete counterbalancing of increases in atmospheric demand. On the contrary, these plant responses have a very large impact on eventual ET and we have high confidence that plants play a substantial role from analysing C4MIP runs which can separate the effects of plant responses to high CO2 from radiative responses (primarily increased surface temperature). There are, however, several competing processes impacting ET each with large uncertainty, such that the final expected ET flux is a balance between many uncertain factors and thus the confidence in the exact magnitude is low. Plant responses to increasing CO2 include stomatal closure, which works to reduce ET, increased photosynthesis which works to increase ET, as well as possible increased leaf area as a result of increased photosynthesis which also works to increase ET. ESMs model these processes simultaneously to find the eventual balance and resulting ET. CMIP5 and CMIP6 simulations both show that at high CO2 concentrations ET does not increase everywhere (CMIP6: Figure 8.18, CMIP5: Swann et al. 2016 Figures 2 and S2). Using C4MIP simulations we can show that this moderate change in ET happens only *because* of plant responses (Swann et al. 2016, Lemordant et al. 2018). Without the plant response ET would increase dramatically everywhere with increasing temperatures (Scheff and Frierson 2014). [Abigail Swann, United States of America]	Noted. With the shift in phrasing towards evaporative demand, it is more clear that we do not mean to imply that ET will increase everywhere.
109575	23	36	23	37	How many of the studies cited here are based on measurements and not models? I suggest a careful separation of the two and there are multiple mechanisms by which gs and iWUE water savings might be counterbalanced: e.g. leaf area increases (as described in Chapters 2 and 5), rooting depth increases with CO2 (Iversen 2010), plant competition (Farrior et al., 2015), enhanced vegetation cover (Donohue et al., 2013). None of these processes are well represented by the current CMIP models and thus the ensemble is biased towards the leaf physiology response. Iversen, C. M. (2010). Digging deeper: fine-root responses to rising atmospheric CO2 concentration in forested ecosystems. New Phytologist, 186(2), 346–357. Farrior, C. E., Rodriguez-Iturbe, I., Dybzinski, R., Levin, S. A., & Pacala, S. W. (2015). Decreased water limitation under elevated CO2 amplifies potential for forest carbon sinks. Proceedings of the National Academy of Sciences, 112(23), 7213–7218. Donohue, R. J., Roderick, M. L., McVicar, T. R., & Farquhar, G. D. (2013). Impact of CO2 fertilization on maximum foliage cover across the globe's warm, arid environments. Geophysical Research Letters, 40(12), 3031–3035. [Anthony Walker, United States of America]	Taken into account. We have clarified that it is mainly modelling studies that suggest that WUE can counterbalance evaporative demand, while observational studies cast doubt upon this. We reviewed the suggested references and added Donohue 2013; the others mainly focus on the impact on carbon sinks and thus would belong in Chapter 5.
105557	23	36	23	43	Singh et al. (2020) has shown that WUE effects can be completely counteracted by the increased plant growth or earth's greening. Singh, A., Kumar, S., Akula, S., Lawrence, D. M., & Lombardozzi, D. L. (2020). Plant growth nullifies the effect of increased water-use efficiency on streamflow under elevated CO2 in the Southeastern United States. Geophysical Research Letters, 47(4), e2019GL086940. [Sanjiv Kumar, United States of America]	Accepted, this reference has been added
7587	23	37	23	38	1/1: "However, uncertainties remain concerning whether WUE completely counterbalances losses via evapotranspiration (ET)". I do not think that any study suggests that WUE can completely counterbalances losses via ET. I suggest to replace this sentence by "(Yang et al. 2018d). However, uncertainties in the net effect of WUE on soil moisture remain, due to to the trade-off between transpiration (which reduces transpiration per leaf area) and plant growth (whcih increases the leaf area). Some studies have found [...]" [Celine Bonfils, United States of America]	Taken into account, we have rephrased this section.
3167	23	37	25	8	In tropical South America, several studies have documented modifications in the water cycle. For instance, in southern Amazon (Espinoza et al., 2019 doi: 10.1016/j.ejrh.2019.100637; Molina-Carpio et al 2017 doi: 10.1080/02626667.2016.126786; Ronchail et al 2018. doi: 10.1016/j.ejrh.2017.11.008) and over tropical Andes and Altiplano (Segura et al., 2020, doi: 10.1007/s00382-020-05132-6). Two recent review papers can be usesfully for this region (Pabon-Caicedo, 2020, doi: 10.3389/feart.2020.00061 and Espinoza et al., 2020, doi: 10.3389/feart.2020.00064) [Jhan Carlo Espinoza, France]	Taken into account: the observed water cycle changes over the Amazon with reference to the Esponoza and Segura papers are discussed in 8.3.1.2, 8.3.1.3, 8.3.2.3

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109577	23	38	23	38	“completely counterbalances losses” here seems misleading, is a complete counterbalancing expected? Plus the previous sentence states partly and the sentence goes on to suggest changes in WUE may not counterbalance these losses at all. Suggest: “can counterbalance losses” [Anthony Walker, United States of America]	Taken into account, we have rephrased this section.
113235	23	38	23	38	If you want to define the acronym 'ET', you can do this earlier on (and use it after) [Diego Miralles, Belgium]	Taken into account, we have rephrased this section.
81153	23	38	23	39	This may need to be revised. It sounds like WUE is not efficient as is? [Mary Matthews, Azerbaijan]	Taken into account, we have rephrased this section.
7589	23	40	23	43	I would add/replace some words in this sentence for clarification (see in bold) : "Reduced evaporation from vegetated surfaces can both exacerbate continental relative humidity decline and warming-induced drying above ground (Berg et al. 2016, Swann et al. 2016) while limiting the desiccation of soils at the root zone and increasing streamflow based on multiple lines of evidence (Milly and Dunne, 2016; Bonfils et al., 2017; Lemordant et al., 2018; Mankin et al., 2018; Peters et al., 2018; Yang et al., 2018d)" [Celine Bonfils, United States of America]	Taken into account, we have rephrased this section.
109579	23	42	23	43	The references cited here are the exact list cited above. Are they all necessary, and the point about models still stands. The model responses are biased towards leaf physiology responses and may over-predict water savings and thus the drying trends. As suggested by the refs cited on lines 46 and 47. This might not be limited to only water replete regions. Donohue et al (2013) show clearly that vegetation cover has increased per unit of precipitation in semi-arid regions, indicating that much of the “saved water” is used to produce additional vegetation. Yang et al., (2016) show very little change in runoff across 12 tropical river basins that are minimally influenced by human water management. Donohue, R. J., Roderick, M. L., McVicar, T. R., & Farquhar, G. D. (2013). Impact of CO2 fertilization on maximum foliage cover across the globe’s warm, arid environments. Geophysical Research Letters, 40(12), 3031–3035. Yang, Y., Donohue, R. J., McVicar, T. R., Roderick, M. L., & Beck, H. E. (2016). Long-term CO2 fertilization increases vegetation productivity and has little effect on hydrological partitioning in tropical rainforests. Journal of Geophysical Research: Biogeosciences, 121(8), 2125–2140. [Anthony Walker, United States of America]	Taken into account, we have rephrased this section and added these references
67605	23	43	23	43	However [Yang Tian, United States of America]	Accepted
21027	23	43	23	43	HowEver [Marcelo Barreiro, Uruguay]	Accepted
113237	23	43	23	43	Howver' [Diego Miralles, Belgium]	Accepted
7591	23	43	23	43	replace "Howver" by "However". [Celine Bonfils, United States of America]	Accepted
74167	23	43	23	43	Howvr -> However [Daniel J. Short Gianotti, United States of America]	Accepted
81645	23	43	23	43	spelling of However [Sönke Zaehle, Germany]	Accepted
81063	23	43	23	47	It is important to qualify all these statements based on what type of ecoregions we are discussing, eg, drier regions experiment a reduced runffo due to increased veegetation growth. See Ukkola et al. 2015. Reduced streamflow in water-stressed climates consistent with CO2 e ects on vegetation. NatureCC [canadell pep, Australia]	Taken into account, we have rephrased this section.
43165	23	43			Read "However, there is" rather than "Howver, there is" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7593	23	44	23	46	add "partially": "can be patially counteracted by enhanced photosynthesis" . For instance, in CMIP5 AMIP-type simulations of future warming (Bonfils et al. 2017), the inclusion of the physiological response to enhanced atmospheric CO2 levels decreases ET per leaf unit due to the stamatal closure, but also yields to more carbon uptake and more leaves that can intercept and transpire water. The net effect is a reduction of ET at the canopy level, and an overall conservation of the root-zone soil humidity. (see comment #45) [Celine Bonfils, United States of America]	Taken into account, we have rephrased this section.
67607	23	46	23	46	over mid-latitudes based on CMIP5 simulations (Mankin et al., 2019), and for forest species [Yang Tian, United States of America]	Taken into account, we have rephrased this section.
88129	23	47	23	50	Saint-Lu et al. (2019) provides evidence that the precipitation response to plant physiological forcing over tropical forest regions is dominated by the interaction of land-surface warming with the present-day circulation, rather than the present-day importance of moisture recycling. Reference: Saint-Lu, M., R. Chadwick, F. H. Lambert, and M. Collins, 2019: Surface warming and atmospheric circulation dominate rainfall changes over tropical rainforests under global warming.Geophys. Res. Lett.,46, 13 410–13 419 [Robin Chadwick, United Kingdom (of Great Britain and Northern Ireland)]	Noted, these sentences have been deleted from the FGD
109581	23	47	23	55	Agreed confidence is low, very low given my other comments about models being leaf-physiology response biased. Given all these references are model based is this text really necessary? [Anthony Walker, United States of America]	Accepted, this text has mostly been deleted
21029	23	49	23	49	Where in South America? Please, be more precise. [Marcelo Barreiro, Uruguay]	Noted, these sentences have been deleted from the FGD
4873	23	50	23	52	rather than emphasizing the percentile to define extreme precip, elaborate on the mechanism via which stomatal control can affect extreme precipitation in an opposite direction as mean precipitation (presumably via surface temperature effects) [Bart van den Hurk, Netherlands]	Noted, these sentences have been deleted from the FGD
27235	23	50	23	52	If it is understandable from what is above how decreased evapotarnspiration can reduce precipitation, it is not clear how it can increase heavy precipitation. Thus process explanation is missing [Eric Brun, France]	Noted, these sentences have been deleted from the FGD
116717	23		23		Coordination with chapters 6 (SLCF) and 9 (cryosphere) is needed for this section, as well as with chapter 5 (effect of CO2 on vegetation) so as to build a coherent message x chapters . [Valerie Masson-Delmotte, France]	Taken into account: links to Chapters 9 and 5 are included in this section
113239	24	1	24	1	Correct grammar [Diego Miralles, Belgium]	Taken into account, we have rephrased this section.
21031	24	1	24	2	Please revise sentence as it is not clear. [Marcelo Barreiro, Uruguay]	Taken into account, we have rephrased this section.
53219	24	2	24	4	Rather Section 8.2.2? [Hervé Douville, France]	Noted, these sentences have been deleted from the FGD
113241	24	3	24	3	drive' [Diego Miralles, Belgium]	Noted, these sentences have been deleted from the FGD
4875	24	6	24	6	Don't know what this human impact is about, quite a loose statement [Bart van den Hurk, Netherlands]	Noted, these sentences have been deleted from the FGD
74357	24	6	24	10	For me, it is confusing these two sentences. Is it related to the previous line (4-6)? Or, is it a summary from several case studies? [Yulizar Yulizar, Indonesia]	Noted, these sentences have been deleted from the FGD
109787	24	8	24	8	Part of this "low confidence" assessment stems from the large uncertainty in runoff changes. However, those might be constrainable with observations (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]	Noted, these sentences have been deleted from the FGD

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51451	24	9	24	10	Suggest stating resource management only as "use of water" is implicit and also suggest including reservoir systems. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	No longer applicable: this line has been removed
113243	24	12	24	12	moisture supplies plants and crops with water'? [Diego Miralles, Belgium]	Noted, these sentences have been deleted from the FGD
65771	24	12	24	23	Suggest clarification of point 23 (on groundwater). Suggest a discussion of the decoupling of surface and groundwater systems and changes in rainfall runoff elasticity, e.g. as seen in Australia in response to droughts. [Kushla Munro, Australia]	No longer applicable: this text has been deleted and groundwater is dealt with in 8.3.1.7.4 and 8.4.1.7.4
113245	24	15	24	15	abstraction' for 'extraction' [Diego Miralles, Belgium]	Accepted, changed
51453	24	18	24	19	Sea level rise also contaminates coastal sub-surface fresh waterwith salt (refer to WGII section)'. Please include more information about this in WGI given that it's an important physical response. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted, this text has been deleted.
12771	24	19			This reference to WGII needs to be completed formally. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted, this text has been deleted.
93649	24	21	24	21	After (Kumar et al., 2019a), add: , due to soil moisture memory effect over regions of high land-atmosphere coupling (Materia et al., 2014). This coupling varies with geographical location and season (Dirmeyer et al., 2011), and depends on soil […] Schwingschackl et al., 2018). refs. Materia, S., Borrelli, A., Bellucci, A., Alessandri, A., Di Pietro, P., Athanasiadis, P., ... & Gualdi, S. (2014). Impact of atmosphere and land surface initial conditions on seasonal forecasts of global surface temperature. Journal of Climate, 27(24), 9253-9271; Dirmeyer, P. A. (2011). The terrestrial segment of soil moisture–climate coupling. Geophysical Research Letters, 38(16). [Stefano Materia, Italy]	Noted, this text has been deleted.
131571	24	25	24	39	This paragraph contains only statements and facts without explaining relationships. Eventually the core-message/the core fact of the paragraph gets lost. [Hans Poertner and WGII TSU, Germany]	No longer applicable: this text has been deleted and groundwater is dealt with in 8.3.1.7.4 and 8.4.1.7.4

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51945	24	28	24	39	Revised text proposed: "Since the AR5, there has been progress in understanding the processes determining recharge and discharge and the response timescales of groundwater. The SRCCL notes that an increasing intensity of rainfall leads to decreased partitioning between water storage in the soil (green water) and runoff and reservoir inflow increases (blue water) (Yin et al., 2018; Eekhout et al., 2018). Satellite data has identified that surface soil moisture retains a median 14% of precipitation falling on land after three days (McColl et al., 2017). Increased soil moisture variability is also found to suppress the uptake of CO2 by the land based on Earth system climate simulations (Green et al., 2018). Further, there is increased evidence of diffuse groundwater recharge following preferential flowpaths (e.g. soil macropores, Beven and Germann, 2013) that bypass soil matrices (Kotchoni et al., 2019). Focused groundwater recharge occurring by way of leakage from surface waters (e.g. streams, ponds), ephemeral and perennial, has been shown to be a widespread process, especially in drylands. A fuller understanding of catchment processes is critical to evaluating the sensitivity of hydrological systems to climate change. The time-scales over which groundwater equilibrates to climate change recharge responses are <100 years for nearly half of the active groundwater flows globally and longer still over the most sensitive, arid regions based on combining groundwater models with hydrological datasets (Cuthbert et al., 2019a). Evidence from GRACE across the tropics and sub-tropics additionally suggests that groundwater systems in drylands are less sensitive to seasonal climate variability but vulnerable to long-term trends from which they will be slow to recover; in contrast, aquifers in humid regions are more sensitive to seasonal climate disturbances such as ENSO-related drought but relatively quick to recover (Opie et al., 2020)." [Richard Taylor, United Kingdom (of Great Britain and Northern Ireland)]	No longer applicable: this text has been deleted and groundwater is dealt with in 8.3.1.7.4 and 8.4.1.7.4
81155	24	29	24	31	Suggest clarifying green water and blue water terminology [Mary Matthews, Azerbaijan]	No longer applicable: this text has been deleted and groundwater is dealt with in 8.3.1.7.4 and 8.4.1.7.4
129117	24	30			What is meant by "decrease partitioning"? [Trigg Talley, United States of America]	No longer applicable: this text has been deleted and groundwater is dealt with in 8.3.1.7.4 and 8.4.1.7.4
113249	24	31	24	31	Clarify what depth is 'surface' here. [Diego Miralles, Belgium]	No longer applicable: this text has been deleted and groundwater is dealt with in 8.3.1.7.4 and 8.4.1.7.4
65773	24	31	24	31	Suggest clarification of the range of uncertainty in the statement "Satellite data has….a median 14 % of precipitation". This is one estimate of many. [Kushla Munro, Australia]	No longer applicable: this text has been deleted and groundwater is dealt with in 8.3.1.7.4 and 8.4.1.7.4
22265	24	38	24	39	Surely this should be accompanied by a reference to chapter 5 where further details can be found? If this is not covered by chapter 5 or disagrees with their assessment then significant edits are required for consistency. [Peter Thorne, Ireland]	No longer applicable: this text has been deleted and groundwater is dealt with in 8.3.1.7.4 and 8.4.1.7.4
12773	24	39			What does Earth system climate simulations mean? Only ESMs from CMIP5 and not standard ocean-atmosphere models? Suggest changing the sentence to refer to GCMs more generally. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted, this text has been deleted.
6913	24	42	24	44	The finding here has also been demonstrated for India (consider the study of Barton et al. 2019, https://doi.org/10.1002/qj.3538). The inference of this and the cited Taylor studies is that these processes are happening on the sub-grid scale with respect to climate models, are not currently parametrized and thus a major source of convective initiation is missing. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: This reference and inference is now included in the assessment statement (note that heavy rainfall and flooding related text has now been moved to 8.2.3.2)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116719	24	43	24	43	"based on modelling", "not well represented by simulations" : which models, link to chapter 3 for model valuation? [Valerie Masson-Delmotte, France]	Taken into account: it is now specified that only high resolution convection permitting models are able to capture this effect (note that heavy rainfall and flooding related text has now been moved to 8.2.3.2)
113251	24	44	24	44	Please add: Petrova, I., Miralles, D., van Heerwaarden, C. and Wouters, H.: Relation between Convective Rainfall Properties and Antecedent Soil Moisture Heterogeneity Conditions in North Africa, Remote Sensing, 10(6), 969–24, doi:10.3390/rs10060969, 2018. [Diego Miralles, Belgium]	Accepted: reference included in assessment (note that heavy rainfall and flooding related text has now been moved to 8.2.3.2)
6911	24	47			What is "vegetation greening state"? This wording will not be easily understood. Is it something about growing a different type of crops that have different albedo? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	No longer applicable - this text has been removed
64931	24	52	24	53	“remain below freezing” = annual maximum temperature < 0°C or what exactly? [Johannes Quaas, Germany]	Accepted: now stated remain below 0oC all year
93647	24	52	24	53	Add: There is also high confidence that warmer temperatures will increase atmospheric and vegetation water demand, leading to decreased amounts of surface and sub-surface soil moisture. Regional changes are subject to a much higher uncertainty, and may be amplified or reversed in sign. [Stefano Materia, Italy]	Taken into account, this is now stated at the beginning of the section
22267	24	52	25	8	This summary introduces things that weren't clearly stated in the preceding assessment text. For example more intense but less frequent rainfall and its consequences, were not, at least by my reading, sufficiently discussed to justify this finding. [Peter Thorne, Ireland]	Noted, this sentence has been modified
53221	24	53			except where and when? [Hervé Douville, France]	Noted, this text has been deleted.
4877	24	54	24	54	"dominate the melt": dominate over what? Unclear phrase [Bart van den Hurk, Netherlands]	Noted, this text has been deleted.
64933	24	54	24	55	if one could be more specific about the “some regions”, the statement would be much stronger and much more useful [Johannes Quaas, Germany]	Taken into account. This section has been modified accordingly.
53223	24	55			again moisture transport is rather the topic of 8.2.2? [Hervé Douville, France]	Noted, this text has been deleted.
113247	24		24		Please revise carefully these paragraphs because the quality of the writing is a bit lower here. I'll skip highlighting the typos. [Diego Miralles, Belgium]	Taken into account, we have rephrased this section.
65775	25	1	25	2	Suggest adding the following text to this sentence: 'low confidence in rainfall changes' for clarity. [Kushla Munro, Australia]	Noted, this text has been deleted.
12775	25	2	25	3	Convoluted wording in the expression, "More intense but less frequent rainfall increases the proportion of rainfall..." [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted, this text has been deleted.
51949	25	2	25	5	revised text proposed: "More intense but less frequent rainfall increases the proportion of rainfall leading to surface runoff and, in the tropics, groundwater recharge (medium confidence) with precise responses determined by catchment characteristics and surface vegetation feedbacks." [Richard Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Noted, this text has been deleted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53225	25	3			May be useful to explain why there is only medium confidence in an overall reduction in groundwater recharge? https://doi.org/10.1016/j.jhydrol.2017.09.047 https://www.sciencedirect.com/science/article/pii/S0022169415009750 https://www.pnas.org/content/114/11/2842.short [Hervé Douville, France]	No longer applicable: this text has been deleted and groundwater is dealt with in 8.3.1.7.4 and 8.4.1.7.4
70333	25	5	25	8	"There remains high confidence that increasing atmospheric CO2 enhances photosynthesis and stomata regulation thereby increasing water-use efficiency and increasing rates of plant growth and carbon sequestration that are regionally dependent " This sentence is hard to understand. There are five different things listed (photosynthesis, stomatal regulation, WUE, plant growth, carbon sequestration) that are all simultaneously changing, but although they are coupled to one another and all influenced by CO2, they are not all changing directly in concert. As written it is not obvious that these are all different aspects. Further, this list misses calling out the direct importance of leaf area growth (not just plant growth) for altering water fluxes. We suggest that the relevant factors for water fluxes are introduced and discussed directly earlier on, rather than lumped into a long and confusing list. Plant responses to CO2 can influence land evapotranspiration through two mechanisms, which have opposite effects on evapotranspiration: (1) increased rates of photosynthesis and increases in leaf area, which increase transpiration, and (2) stomatal closure, which decreases transpiration. This may mean shifting the emphasis away from WUE. Under high CO2 WUE likely increases, but it does so as a combination of both increasing photosynthesis and a change in stomatal regulation thus alone does not determine the actual evapotranspiration flux, which is what is being discussed here. [Abigail Swann, United States of America]	Taken into account, this section has been revised.
12777	25	5	25	8	This sentence is much too long and needs to be better designed to improve readability. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted, this text has been deleted.
64935	25	7	25	8	"offset" – fully, or in what proportion? [Johannes Quaas, Germany]	Noted, this text has been deleted.
79407	25	11	25	11	I think Section 8.2.3.2 is good and contains a lot of relevant information. But it seems to be too much of a literature review more than an assessment. It would be good if the text is revised so it provides explicit information supporting some statements from the ES instead of showing what has been done. A reference to Chapter 11 would be useful I think, including Box 11.1 on thermodynamic Vs. Dynamics... [Alejandro Di Luca, Australia]	Taken into account - a reference to section 11.1 is now included and the section is updated to support the key messages
34919	25	11	25	30	The SOD data shows that flooding is not directly related to precipitation, but is more so caused by topography changes and infrastructural developments. See general comment #12 above. [Jim O'Brien, Ireland]	Noted - it is clearly discussed in the 1st paragraph of this section and FAQ8.2 that flooding is not simply linked to precipitation
3169	25	11	27	18	Significant increase of extreme floods in Amazon River has been recently reported (Barichivich et al., 2018. doi:10.1126/sciadv.aat8785) using more than 100 years of information. This major change in the tropical water cycle has been atributed to increase rainfall intensity in northern Amazonia (north of 5S) related to warmer north tropical Atlantic and changes in the Hadley and Walker cell (Wang et al. 2018. https://doi.org/10.1088/1748-9326/aadbb9; Espinoza et al., 2019. doi: 10.1007/s00382-018-4462-2). This major change in the tropcal water cycle must be included in this section. [Jhan Carlo Espinoza, France]	Rejected: this section deals with the processes determining changes in heavy rainfall and flooding. Observational aspects are dealt with in 8.3.1.5 and 11.5.2

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3171	25	11	27	18	In western tropical South America, El Niño/La Niña are strongly related to major floods; for instance, in Peruvian coast (Takahashi and Martinez, 2019 Clim Dyn 52, 7389-7415; Sulca et al., 2018. doi:10.1002/joc.5185. ; Rau et al., 2016. https://doi.org/10.1002/joc.4693.; Segura et al., 2019. https://doi.org/10.1007/s00382-018-4590-8, Pabon-Caicedo et al., 2020., doi: 10.3389/feart.2020.00061). On the contrary La Niña events are frequently associated to floods in Peruvian Amazon (Espinoza et al., 2013. https:// doi.org/10.1175/jhm-d-12-0100.1). [Jhan Carlo Espinoza, France]	Accepted - a reference to the link between internal climate variability and flood events is made including reference to Takahashi and Pabon-Caicedo
22275	25	11			This section jumps around a lot with a degree of repetition as a result. It would greatly benefit from being reordered in a more logical manner starting with basic theoretical understanding and with similar text reconciled so that the same things can be told in a more coherent and shorter piece. [Peter Thorne, Ireland]	Taken into account: this section has been reordered and rewritten to improve the logical structure
129119	25	11			Surprised that this extreme future precipitation section doesn't address atmospheric rivers at all (e.g., doi: 10.1029/2017GL076968). [Trigg Talley, United States of America]	Rejected: atmospheric rivers are explicitly discussed in the 2nd paragraph
103691	25	13	25	13	Here it says 'high confidence', but at the end of the section at page 8-27 / line 8, it says 'virtually certain'. [Philippe Tulkens, Belgium]	Accepted - robust agreement is stated in paragraph 1 and the summary is changed to high confidence based on the evidence in this section and consistent with the assessment of Chapter 11 in section 11.4
104563	25	13	25	15	It would be better to cite a compelling observational evidence from Zhou and Wang (2017) that quantitatively revealed extreme precipitation always increases as the planet warms. Reference: Zhou, C., and Wang, K., (2017). Quantifying the sensitivity of precipitation to the long-term warming trend and interannual-decadal variation of surface air temperature over China. J. Clim., 30, 3687-3703. doi: 10.1175/JCLI-D-16-0702.1. [Chunlüe Zhou, United States of America]	Rejected - the key references are already supplied and further observational evidence is discussed in Section 8.3.1.3 and 11.4.2
65779	25	13	25	15	Suggest clarification. Does this statement refer to intensity or duration of heavy precipitation? We understand this to be short duration events becoming more intense. This is relevant for flash flooding, but not large scale flooding. [Kushla Munro, Australia]	Accepted - it is now stated sub-daily up to seasonal
4879	25	13	25	30	most of this paragraph has been mentioned before, isn't it? [Bart van den Hurk, Netherlands]	Noted - this paragraph has been rewritten and introduces the sub-section, bringing together the key concepts and signposting to complimentary sections, including those in Chapter 11.
113253	25	19	25	19	are' for 'is' (and 'depends') [Diego Miralles, Belgium]	Accepted
1261	25	19	25	19	If the chapter is to provide a complete picture of the knowhow, it needs to include statistical reasons for more and increased extreme precipitation amounts: an increase in the number of rainy days due to changes in the circulation. Analysis of a large number of 24-hr rain gauge data suggests that the probability of heavy rainfall, more than 50 mm/day, is mainly due to increased mean precipitation intensity but also due to more rainy days. The explanation is simple: if there are more rainy days, all with rainfall amounts described by a given probability distribution function, then the probability of seeing an extreme event is also higher. This is explained in Benestad et al. (2019; DOI 10.1088/1748-9326/ab2bb2). Neglecting to include this point will give an incomplete picture of our knowledge about trends in extreme precipitation. [Rasmus Benestad, Norway]	Noted - the physical basis for more rainy days is not apparent from this reference is not relevant to the introductory context here
51455	25	19	25	19	Does this mean antecedent conditions rather than precusor? Also, please clarify what is being referring to here when mentioning flooding. Is this purely the hazard or is it about flood risk, which involves exposure and vunerability (i.e. infrastructure and people)? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: flood hazard and antecedent conditions now removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53227	25	19	25	20	singular [Hervé Douville, France]	Accepted
1263	25	21	25	22	There is no a priori reason to expect monthly probability of being correlated with extremes because one month represents a small sample and "the law of small numbers" apply. This is well-established and basic statistical theory. One month is about 30 days, and since it does not rain every day, the effective size is substantially smaller. With small statistical samples, we expect pronounced random sampling fluctuations. Therefore it's better to analyse seasonal aggragates (90 days) or longer. [Rasmus Benestad, Norway]	Noted: the discussion of statistical aspects of monthly verses seasonal rain are beyond the scope of this section which deals with physical drivers
65777	25	23	25	24	Suggest clarification of the reference: Wasko and Nathan 2019. Wasko and Nathan misuse the word 'floods'. This paper actually shows that 'streamflow events' in Australia are decreasing due to soil moisture decreases. It actually shows that for events large enough to be called 'floods', they are increasing (and soil moisture doesn't play a role – which is cited elsewhere here). Also, 2 of the 3 papers cited refer to Australian conditions - has this been verified elsewhere in the world? For changes in flood seasonality, Arheimer et al. 2017 (Nature Communications) show expected changes in Spring flood peak from snowmelt. [Kushla Munro, Australia]	Accepted: clarified that high streamflows and flood hazard are being considered; the Arheimer reference is included
129121	25	24			The following references may be relevant: Neri, A., G. Villarini, L.J. Slater, and F. Napolitano, On the statistical attribution of the frequency of flood events across the U.S. Midwest, Advances in Water Resources, 127, 225-236, 2019 Slater, L.J., and G. Villarini, Recent trends in US flood risk, Geophysical Research Letters, 43(24), 12428-12436, 2016. [Trigg Talley, United States of America]	No longer applicable: this line was deleted
22273	25	26	25	26	You are in section 8.2.3.2 [Peter Thorne, Ireland]	Accepted: incorrect link removed
103693	25	27	25	27	Link to WGII; on 'human intervention': please rephrase like on page 8-27/lines 46-48; please add reference: Haer, T. Botzen. W.J. Aerts, J.C.J,H.. Advancing disaster policies by integrating dynamic adaptive behaviour in risk assessments using an agent-based modelling approach. ERL, https://doi.org/10.1088/1748-9326/ab0770 [Philippe Tulkens, Belgium]	Taken into account: this is rephrased as changes in flood hazard to keep to WGI remit and so the suggested reference is no longer applicable
65781	25	28	25	28	Suggest clarification as there is not agreement that floods are increasing everywhere. Suggest including more references to hydrological literature on observed trends in global flooding, e.g.: Kundzewicz et al. 2013, Svensson et al. 2006, Najibi, N and Devineni 2018, Hirsch et al. 2015. [Kushla Munro, Australia]	Noted: this is already stated in terms of increases and decreases in flood hazard with reference to examples over Europe and discussed in Section 8.3
109687	25	30	25	30	The preceding paragraph is very well-done; I suggest ending it by very briefly mentioning that flood risks are being further exacerbated by growing human setllements in flood-prone areas. [Sean Fleming, United States of America]	Rejected: the WGI remit is on the physical aspects of flood hazard which is now made clear but thank you for the nice comment
27239	25	33	25	33	This is the firts time the terminology "atmospheric rivers" appears ... it needs to be defined [Eric Brun, France]	Taken into account: the glossary is now referred to here
64601	25	34	25	34	Consider adding the relevant reference: Curry, C. L., Islam, S. U., Zwiers, F. W., & Déry, S. J. (2019). Atmospheric rivers increase future flood risk in Western Canada's largest Pacific river. Geophysical Research Letters, 46(3), 1651-1661. [Charles Curry, Canada]	Rejected: future projections including atmospheric rivers are covered in Section 8.4.2.8; here the physical drivers are introduced
20481	25	36	25	36	What is a "atmospheric river event"? [philippe waldteufel, France]	Taken into account: the reader is now referred to the glossary
129123	25	37			The following reference may be relevant: Payne, A.E., M.-E. Demory, L.R. Leung, A.M. Ramos, C.A. Shields, J.J. Rutz, N. Siler, G. Villarini, A. Hall, and F.M. Ralph, Responses and impacts of atmospheric rivers to climate change, Nature Reviews Earth & Environment, 1, 143-157, 2020. [Trigg Talley, United States of America]	Accepted: key reference added

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89055	25	38		41	Yettellla and Kay (2017) show that precipitation in extratropical cyclone composites increases at the thermodynamic rate of increase in the CESM1 Large Ensemble, contradicting the finding that precipitation in ETCs increases faster than Clausius-Clapeyron. Furthermore, it's not clear to me that the increass above Clausius-Clapeyron relative to bins fixed in the CTL period is an appropriate basis for comparison. Yettella, V., & Kay, J. E. (2017). How will precipitation change in extratropical cyclones as the planet warms? Insights from a large initial condition climate model ensemble. Climate Dynamics, 49(5–6), 1765–1781. https://doi.org/10.1007/s00382-016-3410-2 [Angeline Pendergrass, United States of America]	Taken into account: the statement regarding atmospheric rivers is now generalised to extra-tropical cyclones including atmospheric rivers, with reference to Yettella and Kay .
129125	25	40			Throughout this text the use of "Clausius Clapeyron rate" is a bit confusing, since a rate normally refers to a change with time (not with warming). [Trigg Talley, United States of America]	Rejected: this is defined earlier and in the glossary and is considered standard terminology
79403	25	41	25	41	What is the difference between a fluvial and a pluvial flooding? Maybe refer to Chaspter 11 in here too… [Alejandro Di Luca, Australia]	Taken into account: this has been changed to river flooding and flash flooding
23765	25	42	25	42	use 'rain-on-snow' for consistency (instead of rain on snow) [Annett Bartsch, Austria]	Accepted
16125	25	44	25	46	I wonder a bit about the results by O'Gorman (2014). He aggregates snowfall events by temperature bins for each grid box and day. The area where snowfall extremes increase moves further north (his figure 1b); but grid box area decreases towards the north in most AGCMs. Maybe his results would have shown a stronger extreme snowfall decrease if grid box areas had been taken into account? Or do I misunderstand this paper? [Gerhard Krinner, France]	Noted: on discussing with the author, Paul O'Gorman, it is assessed that this grid box area effect does not affect the conclusions of this study - this is now discussed in Section 8.4 instead
32925	25	48	26	43	the increased risk of flash flooding should be more strongly emphasized [Tomasz Walczykiewicz, Poland]	Rejected: this paragraph deals with longer lasting precipitation extremes while flash flooding is discussed briefly in the introductory paragraph and in the later paragraphs on short-duration precipitation events
21033	25	49	25	50	“This occurs in mid-latitudes...” is a sentence that seems to apply everywhere based only on NH examples. Please, avoid generalizing to the whole world and specify region considered. Or at least hemisphere. This comment applies to many sentences throughout the chapter. [Marcelo Barreiro, Uruguay]	Accepted: added northern mid-latitudes
67609	25	51	25	51	linked to [Yang Tian, United States of America]	Accepted.
20123	25	52	25	52	"to" missing at the beginning of the line [philippe waldteufel, France]	Accepted.
129127	25	52			The following reference may be relevant: Zhang, W., and G. Villarini, Greenhouse gasses drove the increasing trends in spring precipitation across the Central United States, Philosophical Transactions A, 2020 (in press). [Trigg Talley, United States of America]	Rejected: this attribution to greenhouse gases is not relevant to the discussion in this section
7989	25	54	25	54	Here and elsewhere, the text in this chapter was hard to read in places. A comma is needed on this line '….they occur, yet drivers…. [Anthony Lupo, United States of America]	Accepted.
129129	25	54	25	54	Here and elsewhere, the text in this chapter was hard to read in places. A comma is needed on this line "… they occur, yet drivers …" [Trigg Talley, United States of America]	Accepted.
20125	25	54	25	54	"yet drivers"? Punctuation might help. [philippe waldteufel, France]	Accepted.
14799	25	54	25	55	This statement is too general to capture the overall response of ice sheets, and associated with an odd 'medium confidence' rating but no actual citations. Suggest referring to Chapter 9 for more specific context around ice sheet and glacier trends, and reasons for these trends. [Jeremy Fyke, Canada]	Taken into account: we think this refers to p.24 which is a summary statement related to glaciers that is now made clear and backed up by body text in the section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70989	25	55	26	3	In a recent study looking at wintertime cyclone clusters over Europe in the CMIP5 models, we find no evidence for a slower transit time, and the increase in precipitation per extratropical cyclone is offset by a decreased number of cyclones within cyclone clusters, the extent of which varies strongly from northern to southern Europe. The details are in a manuscript about to be submitted (E Bevacqua, G Zappa and TG Shepherd: “Shorter cyclone clusters modulate changes in European wintertime precipitation extremes”). You may contact e.bevacqua@reading.ac.uk for the submitted version of this paper. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	No longer applicable: this text has since been removed and a corrected sentence included
129131	26	1	26	3	"Longer duration wet events will result from a weakening latitudinal temperature gradient that slows the passage of extratropical cyclones (Dwyer and O'Gorman, 2017)". Dwyer and O'Gorman do show there is a link between the zonal wind and the duration of extreme precipitation events at midlatitudes and it is fair to cite the paper for this. However, they find in model projections that the zonal wind at midlatitudes strengthens in DJF leading to decreases in duration rather than increases in duration (at least when averaged over 30 to 70 latitude). There is also a decrease in duration in JJA for the same latitude band. At higher latitudes, it is likely necessary to account for meridional propagation of storms and not just weakening of the zonal wind. [Trigg Talley, United States of America]	Accepted: this error was spotted shortly after SOD submission and has since been corrected
4881	26	2	26	2	this weakening meridional gradient is at the surface, not necessarily at higher altitudes [Bart van den Hurk, Netherlands]	Accepted: this text has been corrected and now makes it clear that upper tropospheric latitudinal temperature gradient increases
98033	26	3	26	4	The reduced global TC propagation speed finding of Kossin (2018) is a finding with only in confidence following the Lanzante (2019) and Moon et al. (2019b) critiques. The response of Kossin (2019) to those critiques points to a reduction of TC propagation speed since 1900 over U.S. land only. Even that revised finding does not rise to the level of even medium confidence in emergence of a detectable decrease (unusual compared to natural variability). [Thomas Knutson, United States of America]	Taken into account: the text is modified to emphasise the limited observational support but medium confidence based on robust understanding of tropical circulation slowing. Links to Chapter 11 are included to support the statement
12779	26	3			Change "system speed" to "propagation speed" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text now read "slower propagation speed as tropical circulation weakens"
129133	26	5	26	6	[CONFIDENCE] Single study results where a physical mechanism is not confirmed by observations should be viewed with extreme caution (see similar comment re physics described in Box 8.1), and should at minimum be accompanied by an appropriate indication of low confidence, which would be lengthy and distracting. Recommend to remove this sentence. [Trigg Talley, United States of America]	Taken into account: the text is modified to emphasise the limited observational support but medium confidence based on robust understanding of tropical circulation slowing. Links to Chapter 11 are included to support the statement
53229	26	5	26	6	but aerosol cooling was found to reduce tropical cyclone potential intensity more efficiently than GHG warming increases it (Sobel et al., 2019). [Hervé Douville, France]	Taken into account: here the local scale processes are covered and the text is modified to link to this material in Box 8.1.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98035	26	8	26	9	The increase in flooding potential from TCs comes not from projections of changes of wind speed intensity but primarily from projected increases in TC rainfall rates (Knutson et al. 2020). Secondarily there may be an effect of increased storm intensity (e.g., Liu et al. 2019) though the confidence in this is less than that for thermodynamics influence on TC rainfall rates through increases in environmental atmospheric water vapor content (the so-called “Clausius-Clapeyron” effect of about 7% per degree Celsius warming). Refs: Knutson, T., S.J. Camargo, J.C. Chan, K. Emanuel, C. Ho, J. Kossin, M. Mohapatra, M. Satoh, M. Sugi, K. Walsh, and L. Wu, 2020: Tropical Cyclones and Climate Change Assessment: Part II: Projected Response to Anthropogenic Warming. Bull. Amer. Meteor. Soc., 101, E303–E322, https://doi.org/10.1175/BAMS-D-18-0194.1 and Liu, Maofeng, Gabriel A Vecchi, J A Smith, and Thomas R Knutson, 2019: Causes of large projected increases in hurricane precipitation rates with global warming. npj Climate and Atmospheric Science, 2, 38, DOI:10.1038/s41612-019-0095-3. [Thomas Knutson, United States of America]	Taken into account: the Knutson reference is included as support for the evidence for tropical cyclone rainfall increases while a link is included to Chapter 11 with respect to the less certain aspects of strengthening
20483	26	11	26	11	See comments on page 23 lines 1 and 6 [philippe waldteufel, France]	Taken into account: now state that increased temperature gradient explains increased precipitation intensity from convective systems
20127	26	13	26	14	Please rewrite " for example in active to break phase transition over India" [philippe waldteufel, France]	Taken into account: it is not clear what needs to be re-written but we now say "including active to break…"
67611	26	14	26	14	This sentence does not make much sense [Yang Tian, United States of America]	Taken into account: the sentence is rewritten to emphasise the increased intensity of wet and dry seasons
51457	26	19	26	19	Is the text referring to "recharge" or "infiltration" here? Please refer to the comment below on the Eehout reference. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: the sentence is now split to make it clear that more intense rainfall favours runoff and reservoir recharge is favoured over infiltration into the soil
132079	26	19	26	22	uthors (Ficchi and Stephens, 2019) did their observations about El Niño–Southern Oscillation (ENSO) concerning a median change of 53 days iver East Africa. "I suggest to remove the sentence "between El Nino and La Nina, ". [Mourad Amara, Algeria]	No longer applicable: the text on the timing has been removed
89057	26	19			"climate variability" is vague here. I recommend changing it to "ENSO" or something analogous. [Angeline Pendergrass, United States of America]	Accepted: ENSO now explicitly referred to
4883	26	20	26	20	Quite unclear where this "with a median change of 53 days" refers to. Climate projections? ENSO? [Bart van den Hurk, Netherlands]	No longer applicable: this text has been removed
43765	26	20	26	22	The example is too specific, in contrast with other statements. I suggest removing the number of days. [Francisco Tapiador, Spain]	Accepted: this sentence is made less specific and shortened
23631	26	21	26	21	"El Nino and La Nina" to "El Niño and La Niña" [Deniz Bozkurt, Chile]	Not applicable: this passage has been removed and ENSO is referred to.
129135	26	24	26	26	The following reference may be relevant: Morrison, A., G. Villarini, W. Zhang, and E. Scoccimarro, Projected changes in extreme precipitation at sub-daily and daily time scales, Global and Planetary Change, 182, 1-11, 2019. [Trigg Talley, United States of America]	Rejected: the drivers of short duration precipitation are discussed here and with reference also to Section 11.4.1 so the suggested reference, though relevant is not necessary here
89059	26	26		27	A study that documents the relationship between CAPE and convective intensity in observations is Louf et al (2019) Louf, V., Jakob, C., Protat, A., Bergemann, M., & Narsey, S. (2019). The Relationship of Cloud Number and Size With Their Large-Scale Environment in Deep Tropical Convection. Geophysical Research Letters, 46(15), 9203–9212. https://doi.org/10.1029/2019GL083964 [Angeline Pendergrass, United States of America]	Accepted: this reference is included
20485	26	27	26	27	CAPE was introduced by Moncrieff and Miller (Quart. J. R. Met. Soc,1976, 102, pp 373-394) [philippe waldteufel, France]	Noted: the purpose is to highlight the link between CAPE and convective intensity using recent literature rather than identify the original idea

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
69481	26	27	26	27	This argument for CAPE increasing with warming is due to Singh, M.S. & O'Gorman, P.A. (2013). Influence of entrainment on the thermal stratification in simulations of radiative-convective equilibrium. Geophys. Res. Lett., 40, 4398-4403. [Martin Singh, Australia]	Accepted: reference added and sentence modified to note the range of evidence
87405	26	29	26	29	Additional citation is suggested for '(Hamada et al. 2015)' to '(Hamada et al. 2015, Hamada and Takayabu, 2018)' Added citation: Hamada, A, and Y. N. Takayabu, 2018: Large-scale environmental conditions related to midsummer extreme rainfall events around Japan in the TRMM region, J. Climate, 31, 6933-6945. https://doi.org/10.1175/JCLI-D-17-0632.1 [Yukari Takayabu, Japan]	Accepted: this additional reference is included
104603	26	29	26	32	A new compelling observation evidence from Zhou and Wang (2017) shows extreme precipitation increased with mean air temperature by above 10%/℃ in China (exceed the thermodynamic expectation). Please include this article in the citation in Lines 32. Reference: Zhou, C., and K. Wang, 2017: Quantifying the sensitivity of precipitation to the long-term warming trend and interannual-decadal variation of surface air temperature over China. J. Clim., 30, 3687-3703. [Chunlüe Zhou, United States of America]	Accepted: reference included and sentence modified to reflect the robust evidence and medium agreement
79405	26	30	26	30	To say "demonstrate" seems too strong in this context. [Alejandro Di Luca, Australia]	Accepted: now stated as implied from medium agreement across modelling and observational studies
69483	26	34	26	35	I think it should be made more clear what an "observational scaling" is here. At present, one might think it is an estimate of the climate change sensitivity based on trends, but that is not what is being examined in the Barbero and lenderink studies [Martin Singh, Australia]	Accepted: the discussion of scalings has been rewritten and moved to a separate paragraph which the limitation of using observed scalings as a proxy for future changes is made clearer
79409	26	34	26	37	What is the thermodynamic expectation? Scaling is used but it has not been defined before. Also, there are several issues with the traditional scaling (some that you point out) so I would be careful to say that through scaling "we corroborate that". [Alejandro Di Luca, Australia]	Not applicable: this text has been removed and the discussion of scalings separated into a new paragraph
89141	26	34		39	This topic is also addressed in Chapter 11, section 11.4.2 - it should be consistent in both places. [Angeline Pendergrass, United States of America]	Taken into account: text has been revised for consistency with 11.4 which is referred to
89061	26	34		39	The scaling of precipitation extremes in observations, when integrated over many types of synoptic states, is fundmentally different than the type of scaling we expect to find in response to global warming. This is not an approporiate piece of evidence to support statements about how extreme precipitation will change with warming, as Bao et al (2017) show, and the fundamental issues with it are not resolved by merely "improving the interpretation" basing it on dewpoint temperature. The fundamental problem is that the type of synoptic states from one temperature to another in the historical period have a different relationship between dynamical regimes than the changes from present to future will, so this statement is incorrect. The "corroboration by observed scalings" does not provide relevant evidence to climate change, and so should not be included here. It would not be clear to non-expert readers that Bao et al (2017) shows this is an approach that does not work for the climate change response. The correct way that these relationships should be interpreted is illustrated in Fig 10 of Drobinski et al (2016). Drobinski, Philippe, Nicolas Da Silva, Gérémy Panthou, Sophie Bastin, Caroline Muller, Bodo Ahrens, Marco Borga, et al. "Scaling Precipitation Extremes with Temperature in the Mediterranean: Past Climate Assessment and Projection in Anthropogenic Scenarios." Climate Dynamics, 2016, 1–21. https://doi.org/10.1007/s00382-016-3083-x. [Angeline Pendergrass, United States of America]	Accepted: the discussion of scalings has been rewritten and moved to a separate paragraph which the limitation of using observed scalings as a proxy for future changes is made clearer
69485	26	36	26	39	This does not make sense to me. If present day scalings are not relevant then why do we care about the dew point relationship? Moreover, the statemtent that scaling by dewpoint temperature reduces dependence on dynamical factors is wrong. One only gets super-CC scaling if the dynamics of the relevant preciptiation producing systems changes as a function of dew point temperature. Bao et al. showed that projected changes in precipitation extremes have nothing to do with the present day scaling of precipitation extremes with temperature or dew point temperature in the same models. This does not come through in the current reading. [Martin Singh, Australia]	Accepted: the discussion of scalings has been rewritten and moved to a separate paragraph which the limitation of using observed scalings as a proxy for future changes is made clearer

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129137	26	39			The following reference may be relevant: Zhang, W., G. Villarini, and M. Wehner, Contrasting the responses of extreme precipitation to changes in surface air and dew point temperatures, Climatic Change, 154, 257-271, 2019. [Trigg Talley, United States of America]	Accepted: reference added
51459	26	41	26	43	Eekhout et al (2018) is referenced here to support the idea that a drier surface results in reduced percolation. However, more intense rainfall would mean less precolation but this is due to the infiltration capacities being exceeded rather than being dry? This is argued similarly by Yin et al (2018). Suggest the use of this reference is revisited. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: this sentence is revised
51951	26	41	26	43	Revised text proposed: "More intense but less frequent storms (Kendon et al., 2019) are also expected to favour runoff and flash flooding (Yin et al., 2018) as well as focused groundwater recharge (Taylor et al., 2013c; Cuthbert et al., 2019b)." - NOTE: The deleted assertion on declining recharge is not based on observations but on a model in which the authors have prescribed an increase infiltration-excess runoff. That increased runoff can and will occur under the intensification of precipitation is not disputed but translating this into a pre-determined reduction in recharge, inconsistent with observations, is. Eekhout et al. (2018) argue that the increase in rainfall extremes favours “blue water” generation over “green water” - this is consistent with increased recharge, which has been observed {8.3.1.7.4}. [Richard Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: the updated text is used
65783	26	41	26	43	Suggest clarification. This statement ignores major hydrological processes: "...favour runoff and flash flooding rather than recharge since a drier surface reduces percolation from intense rain" - the main reason this occurs is because the rainfall intensity exceeds the percolation capacity of the soils, regardless of how wet or dry they are. [Kushla Munro, Australia]	Taken into account: this sentence is revised
67613	26	46	26	46	(Prein et al., 2017), and its scaling depends on time of day based on model details (Meredith et al., 2019). [Yang Tian, United States of America]	Accepted: this text is modified accordingly
4885	26	52	26	53	What is "relative humidity increases intensity"? [Bart van den Hurk, Netherlands]	Taken into account: sentence revised to make it clearer that increased relative humidity intensifies the storm
89063	26	55	27	1	In Pendergrass et al (2019) we showed that in 2/3 of CMIP5 models, there is a nonlinear response of extreme precipitation to warming that can lead to misinterpretation of the response of extreme precipitation to aerosol forcing in scenarios where aerosol forcing declines. These additional increases in extreme precipitation are not due to a sensitivity to aerosol forcing that is larger than the sensitivity to greenhouse gas forcing, but rather they are due to the nonlinear response of extreme precipitation to warming that arises from any type of forcing - they would also occur if CO2 were the forcing that drove the additional warming. Pendergrass, A. G., Coleman, D. B., Deser, C., Lehner, F., Rosenbloom, N., & Simpson, I. R. (2019). Nonlinear Response of Extreme Precipitation to Warming in CESM1. Geophysical Research Letters, 46(17–18), 10551–10560. https://doi.org/10.1029/2019GL084826 [Angeline Pendergrass, United States of America]	Noted: the non-linearity of precipitation responses is covered in Section 8.5.3
42763	27	1	45	50	I am concerned about the coherency between the informations given in Chapter 8, on the one hand, and in Chapter 12, on the other hand when it comes to regions (especially monsoon regions where the regional impact of climate change is foreseen to be strong while regional climate models are highly uncertain). It seems to me that these two chapters should refer to each other (may be Chapter 12 refering to the informations given in chapter 8 and ensuring that chapter 8 provide the information needed for chapter 12, especially concerning scales, evolution of the extremes and seasonality). In the following my remarks are focused on the West African Monsoon (WAM), but I found the same need applying to other monsoon regions as well [Thierry Lebel, France]	Noted. For the FGD Chapter 12 reviewed the drought and aridity content in Ch. 8 so there is now better cross-chapter coordination.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31487	27	2	27	4	Since the AR5, new modelling evidence shows increases in convective precipitation extremes are limited by microphysical processes involving droplet/ice fall speeds (Singh and O'Gorman, 2014; Sandvik et al., 2018; Tapiador et al., 2019). Comment: The use of simple schemes of microphysics of precipitation (MP) in some models is also key factor to explain the limitations of the simulation of extreme precipitation. Reference: Tapiador, F.J., Sánchez, J.-L., García-Ortega, E., 2019. Empirical values and assumptions in the microphysics of numerical models. Atmospheric Research 215, 214–238. https://doi.org/10.1016/j.atmosres.2018.09.010 [Andrés Navarro, Spain]	Accepted: this reference is included to note the simplicity of the microphysics schemes
89065	27	6			There is more work that has been undertaken on the topic since AR5, though Bao and Sherwood (2019) is a good start. I review recent work on the role of changing convective organization on changes in precipitation extremes in Pendergrass (2020). Pendergrass, A. G. (2020). Changing Degree of Convective Organization as a Mechanism for Dynamic Changes in Extreme Precipitation. Current Climate Change Reports, 6(2), 47–54. https://doi.org/10.1007/s40641-020-00157-9 [Angeline Pendergrass, United States of America]	Accepted: this review is included as additional, more up to date and comprehensive evidence
113255	27	10	27	10	is expected to' for 'will'. There are several of these instances in the report… it is not 'likely to be expected' but 'likely to occur'. [Diego Miralles, Belgium]	Accepted: expected to changed to will
3709	27	11	27	11	"close to 7%". Close seems a bit vague especially as the earlier pararaph says it could be three times that. Could this be made more precise? [Declan Finney, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: the supporting text has been revised and the assessment is that close to Clausius Clapeyron rates is reasonable
89067	27	11			It seems to me based on what is written here that a statement of "at least" the thermodynamic response rather than "close to" would be more accurate. [Angeline Pendergrass, United States of America]	Taken into account: the supporting text has been revised and the assessment is that close to Clausius Clapeyron rates is reasonable
51953	27	15	27	15	Revised text proposed: "… will intensify associated flooding and focused groundwater recharge." [Richard Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: text is added
33067	27	21	28	3	the role of water also in arid and semi- arid areas needs to be highlighted. [Sahar Tajbakhsh Mosalman, Iran]	Rejected. This Section is an assessment of underlying mechanisms of drought. Sections 8.3 and 8.4 provide assessments of observed and predicted trends in drought including arid regions of the world.
32737	27	21	28	3	the role of water also in arid and semi- arid areas needs to be highlighted. [sadegh zeyaeyan, Iran]	Rejected. This Section is an assessment of underlying mechanisms of drought. Sections 8.3 and 8.4 provide assessments of observed and predicted trends in drought including arid regions of the world.
19443	27	21	28	3	The role of water also in arid and semi-arid areas needs to be highlighted. [Mostafa Jafari, Iran]	Rejected. This Section is an assessment of underlying mechanisms of drought. Sections 8.3 and 8.4 provide assessments of observed and predicted trends in drought including arid regions of the world.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89071	27	21		44	Two more papers that have come out since this draft was completed that look at droughts in CMIP6 simulations are Ukkola et al (2020) and Cook et al (2020). Ukkola, A. M., Kauwe, M. G. D., Roderick, M. L., Abramowitz, G., & Pitman, A. J. (n.d.). Robust future changes in meteorological drought in CMIP6 projections despite uncertainty in precipitation. Geophysical Research Letters, n/a(n/a), e2020GL087820. https://doi.org/10.1029/2020GL087820 Cook, B. I., Mankin, J. S., Marvel, K., Williams, A. P., Smerdon, J. E., & Anchukaitis, K. J. (2020). Twenty-First Century Drought Projections in the CMIP6 Forcing Scenarios. Earth's Future, 8(6), e2019EF001461. https://doi.org/10.1029/2019EF001461 [Angeline Pendergrass, United States of America]	Noted. These references are best added to Section 8.4, which is where CMIP6 assessment takes place.
129139	27	21			In 8.2.3.3 (Drivers of drought and aridification), there are only discussions on drought (extreme event) but not at all on aridification (mean state change) although the latter is in the title. [Trigg Talley, United States of America]	Taken into account. This section has been merged with soil moisture discussions to cover drought and aridity
53231	27	21			Specify "Regional drivers" here? Remote drivers could be assessed in Section 8.2.2 (cf. former related comment). What about Land Use Change and drought? For instance, are aforestation scenarios always suitable with the available water resources or a further potential driver of drought (and mitigation failure) at the regional scale? [Hervé Douville, France]	Rejected. This section has been re-organized into drought and aridity.
40035	27	23	27	23	Consider updating the current glossary definition for drought: "A period of abnormally dry weather long enough to cause a serious hydrological imbalance. Drought is a relative term; therefore any discussion in terms of precipitation deficit must refer to the particular precipitation-related activity that is under discussion. For example, shortage of precipitation during the growing season impinges on crop production or ecosystem function in general (due to soil moisture drought, also termed agricultural drought) and during the runoff and percolation season primarily affects water supplies (hydrological drought). Storage changes in soil moisture and groundwater are also affected by increases in actual evapotranspiration in addition to reductions in precipitation. A period with an abnormal precipitation deficit is defined as a meteorological drought." [TSU WGI, France]	Noted. We have updated the text in 8.2.3.3 to match the Glossary definition.
8671	27	23	27	25	Maybe environmental droughts as another relevant drought type?: see e.g. Vicente-Serrano, S.M. Steven Quiring, Marina Peña-Gallardo, Shanshui Yuan, Fernando Domínguez-Castro. (2020) A review on environmental droughts: Increased risk under global warming? Earth Science Reviews. 201, 102953. [Sergio Vicente-Serrano, Spain]	Noted. the types of drought described in the FGD correspond to the main types identified by the cross-chapter drought working group.
51461	27	23	27	32	Suggest referencing the WGII approach to drought here. In particular, droughts that affect humans tend to be of the hydrological and agricultural drought variety. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Hydrological and agricultural drought are defined and discussed further down in this section. The Glossary definition of drought also includes hydrological and agricultural drought definitions.
129141	27	23	27	55	Drought is predominantly caused by ENSO, switching places from El Niño to La Niña, with more land drought in EN. See Trenberth, K. E., A. Dai, G. van der Schrier, P. D. Jones, J. Barichivich, K. R. Briffa, and J. Sheffield, 2014: Global warming and changes in drought. Nature Climate Change, 4, 17-22, doi:10.1038/NCLIMATE2067. [Trigg Talley, United States of America]	Rejected. ENSO is only one of many possible causes of drought. Our intention here is to broadly define drought and the basic mechanisms behind it. The role of ENSO in the water cycle is assessed elsewhere in this Chapter.
10159	27	24	27	26	Figure 8.5 should be called out here. Right now it is not mentioned in the text at all. [Jacob Scheff, United States of America]	Accepted. Figure call out has been added.
53233	27	26	27	28	and could become more common phenomena as the atmospheric water demand increases with global warming? [Hervé Douville, France]	Noted. This aspect is covered in the new text on aridity.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89069	27	26			I led a perspective that also includes an outlook for how flash droughts might change with warming, and what is needed to make progress on the problem. Pendergrass, A. G., Meehl, G. A., Pulwarty, R., Hobbins, M., Hoell, A., AghaKouchak, A., et al. (2020). Flash droughts present a new challenge for subseasonal-to-seasonal prediction. Nature Climate Change, 10(3), 191–199. https://doi.org/10.1038/s41558-020-0709-0 [Angeline Pendergrass, United States of America]	Accepted. This new paper is now referenced.
1265	27	27	27	28	It may be useful to inform the reader, since it's a fairly new concept, that the time scale of a flash flood is 30 days or a little longer. [Rasmus Benestad, Norway]	Accepted. A timescale for flash droughts (order of weeks) has been added.
6707	27	35	27	35	Should "strong winds" be mentioned as well as "High temperatures and low humidity"? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted, however this sentence has been moved and edited with the merger of the aridity text.
8669	27	35	27	37	But the effects on drought severity of the variables different to precipitation are complex and usually difficult to assess. See details in Vicente-Serrano, S.M., McVicar, T., Miralles, D., Yang, Y., Tomas-Burguera, M. (2020): Unravelling the influence of atmospheric evaporative demand on drought under climate change. WIREs Climate Change 11: e632. [Sergio Vicente-Serrano, Spain]	Taken into account. FGD revisions involve a new figure and description of drivers of drought
113257	27	37	27	37	Please add as well '(Vicente-Serrano et al., 2020)'. Vicente-Serrano, S. M., Mcvicar, T. R., Miralles, D. G., Yang, Y. and Tomás-Burguera, M.: Unraveling the influence of atmospheric evaporative demand on drought and its response to climate change, WIREs Clim Change, 11(2), 1–31, doi:10.1002/wcc.632, 2020. [Diego Miralles, Belgium]	Accepted. Reference has been added and note sentence has been moved up in text.
17189	27	39	27	39	Replace the word 'ameliorate' with a plain English alternative like improve/alleviate/dampen or similar to suit a multi-lingual audience. [Joelle Joelle Gergis, Australia]	Accepted. Changed to "alleviate"
113259	27	40	27	40	Could not you remove 'warming impacts on'? [Diego Miralles, Belgium]	Accepted. We have modified the sentence accordingly.
113261	27	40	27	40	What really depends here is what you mean by 'drought' in this statement. Under higher demand for water plants may close stomata and retain more water in land (less agricultural drought) but reduce the likelihood of (downwind) rainfall (more meteorological drought). [Diego Miralles, Belgium]	Noted. This section has been revised to reflect the direct and indirect effects of AED on plant physiology.
113263	27	40	27	40	Please add a sentence here: 'Land–atmosphere feedbacks in response to soil dessication may affect the local likelihood of rainfall (Roundy et al. 2013) and lead to the propagation of drought conditions downwind (Miralles et al., 2019)'. Roundy, J. K., Ferguson, C. R. and Wood, E. F.: Temporal Variability of Land–Atmosphere Coupling and Its Implications for Drought over the Southeast United States, J. Hydrometeor, 14(2), 622–635, doi:10.1175/JHM-D-12-090.1, 2013. Miralles DG, Gentine P, Seneviratne SI, Teuling AJ. Land-atmospheric feedbacks during droughts and heatwaves: state of the science and current challenges. Ann N Y Acad Sci. 2019;1436(1):19-35. doi:10.1111/nyas.13912, 2019. [Diego Miralles, Belgium]	Rejected. Due to space considerations we cannot add text here regarding land-atmosphere feedbacks, however further information on drought drivers may be found in Chapter 11
3677	27	40	27	43	how are they related to the drought topic for this paragraph? [Jiafu Mao, United States of America]	Rejected. We are unsure what the Reviewer is referencing - the role of plants or snowpack - but both topics are relevant here as they are drivers of drought.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129143	27	40			After ""... drought at the surface."" add ""Terrestrial aridity would change due to the changes in not only precipitation but also atmospheric evaporative demand. Noting that the precipitation change over land is controlled by the evaporation change over ocean, the terrestrial climate would become drier if the evaporative demand over land increases faster than evaporation over ocean (Sherwood and Fu, 2014; Fu and Feng, 2014). Fu and Feng (2014) showed that enhanced land warming relative to the ocean, a decrease in relative humidity over land but an increase over ocean, and part of increase in net downward surface radiation going into the deep ocean, all contribute to a drier terrestrial climate in a warming world. It is, however, still an open question about the role of stomatal resistance change in the terrestrial aridity changes in response to the CO2 increase."" Citations: Sherwood, S., and Q. Fu, 2014: A Drier Future? Science, 343 (6172), 737-739, doi: 10.1126/science.1247620. Fu, Q., and S. Feng, 2014: Responses of terrestrial aridity to global warming. J. Geophys. Res. 119, doi:10.1002/2014JD021608. [Trigg Talley, United States of America]	Noted. This section has been revised to reflect the direct and indirect effects of AED on plant physiology.
12781	27	41			The following paper may also be of relevance to the changing snowfall/rainfall ratio for the Himalayas: The impacts of climate change on the winter water cycle of the western Himalaya. K. M. R. Hunt, A. G. Turner and L. C. Shaffrey. Climate Dynamics, submitted 11 September 2019. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Specific regional information on snow and drought may be found in Chapter 12.
20487	27	46	27	46	At this stage the reader does not know about drought categories. Should he/she be referred to figure 8.5? [philippe waldteufel, France]	Noted. We have removed the text describing a fourth category to avoid confusion; the reader is referred to Figure 8.5 earlier on in this subsection so that they are familiar with categories.
4887	27	46	27	46	what is implied by "fourth category"? [Bart van den Hurk, Netherlands]	Noted, this text has been deleted.
81157	27	46	28	3	This could be very useful and interesting here to clarify a bit more. Reader left hanging. Could final draft include hyperlinks for cases such as this? [Mary Matthews, Azerbaijan]	Noted, however further discussion of these issues is better suited to WGII
22277	27	46	28	3	This is not within WG1 scope. It should thus be removed. [Peter Thorne, Ireland]	Rejected. This is an opportunity to connect this section to WGII
17191	27	47	27	47	Replace the word 'ameliorate' with a plain English alternative like improve/alleviate/dampen or similar to suit a multi-lingual audience. [Joelle Joelle Gergis, Australia]	Accepted. Changed to "alleviate"
89073	27	48			We also discuss the role of early warming systems and planning for flash droughts in Pendergrass et al (2020). Pendergrass, A. G., Meehl, G. A., Pulwarty, R., Hobbins, M., Hoell, A., AghaKouchak, A., et al. (2020). Flash droughts present a new challenge for subseasonal-to-seasonal prediction. Nature Climate Change, 10(3), 191–199. https://doi.org/10.1038/s41558-020-0709-0 [Angeline Pendergrass, United States of America]	Accepted. Reference has been added.
20489	27	54	28	2	This passage is describing what is called rebound effects, in IPCC literature and elsewhere. Frequent examples are found in WG3's report. [philippe waldteufel, France]	Noted. No changes are suggested so text is retained.
32927	27	54	28	2	statement is unclear [Tomasz Walczykiewicz, Poland]	Noted, we have removed deficits to make this sentence more clear.
116721	27		27		what about megadroughts? [Valerie Masson-Delmotte, France]	Taken into account: mega droughts are now defined
8673	28	8	28	8	I would replace the "temperature" in the figure 8.5 by the atmospheric evaporative demand, which includes also the effect of atmospheric humidity, solar radiation and wind speed (in addition to temperature) in order of contributing to drought. [Sergio Vicente-Serrano, Spain]	Noted. This figure has been completely redesigned for the final draft.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12783	28	16			The opening words would be better phrased as "Human activity influences". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
53235	28	20			Regarding LUC, may be quote the study by Song et al. (2018) based on 1982-2016 satellite data and showing that—contrary to the prevailing view that forest area has declined globally—tree cover has increased by 2.24 million km2 (+7.1% relative to the 1982 level) and that this overall net gain is the result of a net loss in the tropics being outweighed by a net gain in the extratropics. Regarding irrigation, may be quote Meier et al. (2018) showing that global and regional datasets still show a large divergence with respect to size and distribution of irrigated areas? [Hervé Douville, France]	Rejected: observed changes in forest cover is beyond the scope of this section focusing on direct drivers of water cycle change
53237	28	23	28	25	Should it be clarified here that this redistribution does not mean that the natural global river discharge is divided by two due to human activities since a substantial fraction of the redistributed water ultimately also contributes to runoff? [Hervé Douville, France]	Rejected: the statement merely compares magnitudes so does not need to be further clarified
103695	28	27	28	27	SRCCL needs to be spelled out [Philippe Tulkens, Belgium]	Noted: the SRCCL is the IPCC special report on climate change and land use which is defined elsewhere
59013	28	31	28	34	de Graaf et al., 2019 have received major critique from the surface-water-groundwater-interaction communiy for their paper (with one comment already submitted to Nature). I would refrain from citing this paper in this report. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account: the statement is weakened to "limited evidence" and made less specific since the de Graf results are questioned
53239	28	35			may be "high confidence" given the "can contribute" (e.g., Aral Sea)? [Hervé Douville, France]	Rejected: confidence statements are reserved for the summary statements
12785	28	36	28	37	If irrigation can both aggravate or alleviate severe local weather then why is it medium confidence? If it can alleaviate something, then how is it contributing to scarcity? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: this sentence has been modified and the confidence statement is reserved for the summary
131573	28	37	28	38	What do you mean by "large-scale" extraction of water? i.e. what scale do you use to measure extraction? It can be lare-area (km²), lare-volume (liter), etc... [Hans Poertner and WGII TSU, Germany]	Accepted: changes to widespread since this is a general statement noting the drying of inland seas
131575	28	37	28	38	What is the baseline/ reference value of the classifier "large" in "large-scale extractions"? E.g. "large" could be defined as any value above the standard deviation. Or against some other reference that is choosen to differentiate "high" from "low". [Hans Poertner and WGII TSU, Germany]	Accepted: changes to widespread since this is a general statement noting the drying of inland seas
32929	28	39	28	39	after:are shrinking add sentence: "It affects also local climate changes". [Tomasz Walczykiewicz, Poland]	Rejected: evidence for the climate effect is not provided and so the statement focuses only on the link between water extraction and shrinking inland seas
132077	28	43	28	50	This is low evidence, many contradictory example exist and could be found everywhere. Authors in their references suggest idea about microclimate and extreme precipitation and this statment should not generalized. Jiang et al. (2016) wrote about possible link between precipitation and urban extent in China [Mourad Amara, Algeria]	Noted: we use the assessment from the SRCCL and find no reason to change this assessment of medium confidence
113265	28	44	28	44	And evaporation (not just stability and turbulence) [Diego Miralles, Belgium]	Accepted.
113267	28	48	28	48	alters' [Diego Miralles, Belgium]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27241	28	49	28	50	This paper, even published, suffers severe inconsistencies .... for example how can vegetation grow where solar farms have been implemented. So we do not think it is wise to report such an extreme and inconsistent study in this report [Eric Brun, France]	Taken into account: a further modelling study is included and the statement is weakened to "limited modelling evidence"
64937	28	49	28	50	“doubled precipitation” – at which scale? was this from local recycling or large-scale transport? [Johannes Quaas, Germany]	Taken into account: this statement is weakened to "increase" precipitation based on "limited modelling evidence" from 2 studies
113269	28	53	28	53	reducing evapotranspiration is altering the surface energy balance [Diego Miralles, Belgium]	Accepted: this sentence is rearranged to make this point
12787	28	54	28	55	In what way has this been found? Sensitivity testing of a model or some proportion of the observed rainfall decline? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: it is now made clear this is modelling evidence which is now cited correctly
113271	28	55	28	55	Not sure 'ET' needed to be defined if it is never used [Diego Miralles, Belgium]	Accepted: the definition of ET is removed since it is not used
113273	29	8	29	8	It is not the 'model-simulated' that 'drives'. [Diego Miralles, Belgium]	Accepted: sentence modified to remove this ambiguity
22279	29	8	29	10	How many of these modelling studies are new since AR5 and thus constitute new knowledge? Were older, potentially less accurate models deprecated? There seems to be a considerable risk of issues proverbially under the hood in this study and it is cited without further support which must yield low confidence potentially? [Peter Thorne, Ireland]	Taken into account: it is now emphasised that most of these studies are pre-AR5 but the 90% confidence range remains useful for the assessment and backed up my more recent experiments including CMIP6.
17193	29	22	27	22	Suggest using the word 'extraction' instead of 'abstraction' for a diverse, multi-lingual audience [Joelle Joelle Gergis, Australia]	Accepted: we agree extraction is less abstract than abstraction
69217	29	22	29	27	Large-scale forest fires occur frequently not only in the Amazon but also in other regions, and could lead to severe deforestation. Taking this into consideration please add text on the impacts of forest fires and deforestation on the water cycle (e.g. Line 22-27) in the Executive Summary. [Kaoru Magosaki, Japan]	Reject: the effect of forest fires on deforestation is beyond the scope of this chapter which deals with water cycle changes
53241	29	22	29	27	What about aforestion as planned in high mitigation scenarios? [Hervé Douville, France]	Rejected: there is not sufficient evidence to provide a summary statement with regard to afforestation affects on the water cycle
27243	29	24	29	24	We do not agree with the 'likely' statement as the decrease in precipitation is not "local" in case of large-scale deforestation. Moreover this is true only with unrealistic scales of deforestation, and rather homogeneous deforestation. So we do think the statement is not correct [Eric Brun, France]	Taken into account: we now state regional rather than local but based on the modelling evidence it is likely that large-scale deforestation reduces precipitation and low confidence in more limited deforestation
81159	29	26	29	27	Again, this is a critical point for DMs to know and understand. This should include an information available on % of at risk population. And where .please. [Mary Matthews, Azerbaijan]	Reject: although we agree this is important information, this section deals with processes that underpin expected changes and not detailed projections
89161	29	29	30	44	This section should cross reference the discussion of ET and potential ET in Chapter 11, section 11.6, and specifically 11.6.1.2, which also discusses energy and water limited regimes [Angeline Pendergrass, United States of America]	Rejected. Aerosol effects on evaporation are not discussed in section 11.6.1.2.
95887	29	32	30	31	The summary of aerosol radiative effects on precipitation misses some recent insights gained into the mechanisms. In an (already cited) study, Dagan et al. (2019b) show that the limited ability of the atmopshere to diverge dry/moist static energy at mid-latitudes due to Coriolis effects explains the contrasting response of precipitation to absorbing aerosol in tropics and mid-latitudes. In the tropics, absorption can drive thermaly direct circulations with local preciptiation enhancement (and of course still a decrease in the global mean), which is not possible in the mid-latitudes, leading to local precipitation suppression. This is consistent with CMIP style simulations (c.f. Roeckner et al., 2006). This explanation is not captured in the current summary provided in the box. [Philip Philip Stier, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The limited space in the box did not allow the inclusion of this description.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53243	29	32			Check consistency with Section 6.3.1.3. Also quote Liu et al. (Scientific Reports, 2019) suggesting that the aerosol effect on convective clouds shifts from invigoration to suppression with increasing aerosol optical depth, as well as Dagan et al. (GRL, 2019) showing that aerosols have contrasting effects on tropical vs extratropical precipitation? [Hervé Douville, France]	Accepted. The discussion and references were added.
20129	29	39	29	39	Comma after "radiation" [philippe waldteufel, France]	Accepted.
64939	29	42	29	46	Are these two references sufficient to assess this with “high confidence”? Wild (2012) did not do a formal detection-attributions study. [Johannes Quaas, Germany]	Accepted. Two supporting references for the attribution were added (Undorf et al., 2018a and 2018b)
27245	29	43	29	43	The changes in solar radiation at surface are attributable with high confidence to anthropogenic aerosol emissions, but is there really "high confidence" in the attribution of the decreasing precipitation trends during 1950s-1980s to anthropogenic aerosol emissions? Have detectable decreasing precipitation trends during 1950s-1980s really been observed on many regions of NH? Are there references? Additionally, for example Boé (2016) shows that the response of precipitation over Europe to anthropogenic aerosols is extremely uncertain in CMIP5 models . 2016 - Boé J.: Modulation of the summer hydrological cycle evolution over western Europe by anthropogenic aerosols and soil-atmosphere interactions. Geophysical Research Letters 43, 7678-7685 [Eric Brun, France]	Rejected. The change in northern hemisphere precipitation was mostly driven by the lower latitudes. The changes in precipitation are now shown in new Box 8.1 Figure 1.
129145	29	46			Check figure callout. [Trigg Talley, United States of America]	Accepted. The call was changed to new Fig 1 of Box 8.1.
7621	29	49	29	49	add: {section 3.3.1.1} [Celine Bonfils, United States of America]	Accepted.
6915	29	49	29	51	Polson et al. (2014) should probably be cited here too, given the CMIP5 evidence (https://doi.org/10.1002/2014GL060811 "Decrease monsoon precipitation in the Northern Hemisphere due to anthropogenic aerosol") [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The reference was added.
83871	29	50	29	50	How do dimming over NH weakens the monsoon flow? [Ajaya Mohan Ravindran, United Arab Emirates]	Taken into account. The dimming is reducing the land surface heating, as mentioned already in section 3.3.1.1, which is now referenced here.
116723	29		29		This box could be a x chapter box including also ch 3, 6, 7. [Valerie Masson-Delmotte, France]	Noted: a cross-chapter discussion decided to retain this as a Chapter 8 box
116725	29		29		Coordination is needed with chapters 4 and 5 on land use (deforestation), and vegetation-climate interactions related to possible abrupt change (the same literature is assessd in x chapters). [Valerie Masson-Delmotte, France]	Noted: Chapters 4 and 5 do not specifically deal with the influence of deforestation on precipitation. Abrupt changes are beyond the scope of this section on direct human effects (e.g. deforestation, urbanisation, irrigation, etc)
20491	30	2	30	4	Figure B8.1-1 in its present state is useless when attempting to understand the features mentioned in this paragraph. [philippe waldteufel, France]	Taken into account. We retain just the panels of GPCC, ALL, GHG, and A for Figure 1 of Box 8.1.
12789	30	14	30	15	What is meant by agreement of the ensemble? Is this all members of the ensemble or a fraction such as 2/3? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The figure was completely changed.

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129147	30	25	30	31	What is supported by observations versus modeling, and what are factors aside from aerosol playing a role? [Trigg Talley, United States of America]	Taken into account. The increased moist static energy along with larger convective inhibition due to aerosol absorption is widely observed and simulated. A reference to a paper that both observe and simulates the effect of aerosols on more severe convective storms was added. The "elevated heat pump hypothesis" has an observational support. The limited space does not allow this kind of elaboration.
68205	30	27	30	27	Isn't it Ch12 (not Ch10) that provides detailed regional studies [Guðfinna Aðalgeirsdóttir, Iceland]	Noted. The comment does not seem to pertain to the text.
4891	30	27	30	31	Very unclear what this "release of unstability" mechanism is about, and which part is indiced by aerosols, which by topography. [Bart van den Hurk, Netherlands]	Accepted. The added words "aerosol induced instability" should clarify the sentence.
6917	30	28	30	30	The sentence should be specific to black carbon/soot here. Air pollution aerosols such as sulphate do not contribute to this mechanism. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The word "Absorbing" was added to describe the aerosols.
64941	30	30	30	31	I doubt even medium confidence. Already a while ago we have shown that the aerosol effect is insufficient to drive such a mechanism, based on satellite observations (Kuhlmann and Quaas, Atmos Chem Phys 2010 doi doi:10.5194/acp-10-4673-2010) [Johannes Quaas, Germany]	Taken into account. The study questioning the EHP had many inaccuracies, as demonstrated by Bill Lau in responding to an enquiry with respect to that. Nevertheless, the confidence level was downgraded to "medium".
64943	30	34	30	42	Is it meaningful to give a hypothesis such a weight here by explaining this in a full figure? If the Chapter 8 authors believe this is a key mechanism driving substantial changes in the hydrological cycle, they should provide data-based evidence, rather than such a sketch. [Johannes Quaas, Germany]	Rejected. The illustration describes the basis for all the paragraph in which it is referenced, and supported by all the references there.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129149	30	37	31	6	[CONFIDENCE] Recommend key paragraph revisions to consider emerging evidence of major uncertainties, as follows: ""Cloud droplets nucleate on pre-existing aerosols particles which act as cloud condensation nuclei (CCN). Anthropogenic aerosols add CCN that produce clouds with more numerous and smaller droplets, slower to coalesce into raindrops and to freeze into ice hydrometeors at temperatures below 0°C. Adding CCN suppresses light rainfall from shallow and short-lived clouds, but could be compensated by heavier rainfall from deep clouds. Adding aerosols to clouds in extremely clean air invigorates them at low levels by more efficient vapor condensation on the added drop surfaces (Koren et al., 2014; Fan et al., 2018a). Clouds forming in more polluted air masses (hence with more numerous and smaller drops) may need to grow deeper to initiate rain (Freud and Rosenfeld, 2012; Konwar et al., 2012; Braga et al., 2017). In that case, delaying rain initiation to greater heights transports more cloud water above the 0°C altitude and leads to invigoration of mixed phase precipitation and the resultant hail and cloud electrification (Rosenfeld et al., 2008a; Thornton et al., 2017). However, recent in situ measurements, modeling, and laboratory studies are increasingly revealing major gaps in the understanding of convective microphysics (e.g., Leroy et al., 2017; Stanford et al., 2017; Ackerman et al., 2015; Fridlind et al., 2017; Lauber et al., 2018; Korolev et al., 2020). There is therefore low confidence in modeling study results until the multiplication mechanisms that appear to dominate ice formation in widespread continental and marine deep convection are better established. More generally, the microphysical aerosol-related processes may often compensate or buffer each other (Stevens and Feingold, 2009). For example, suppressed warm rain enhances mixed phase precipitation. Therefore, despite the potentially large aerosol impacts on the precipitation forming processes, the net outcome of aerosol microphysical effects on precipitation amount has generally low confidence, especially when evaluated with respect to the background of high natural variability in precipitation (Tao et al., 2012).”" Added citations: Ackerman, A. S., A. M. Fridlind, A. Grandin, F. Dezitter, M. Weber, J. W. Strapp, and A. V. Korolev, 2015: High ice water content at low radar reflectivity near deep convection - Part 2: Evaluation	Taken into account. The main suggested change was adding qualifications with respect to knowledge gaps of the effects of INP. It was addressed by adding at the ending of the paragraph on INP the following text: "There are still major gaps in understanding the effects of INP mainly on deep convective clouds (Stanford et al., 2017)(Korolev et al., 2020)."
43167	30	40			Read "deep clouds with heavy precipitation (Wang et al., 2013)." rather than "deep clouds with heavy precipitation. From (Wang et al., 2013)." [Cyriaque Rufin Nguimalet, Central African Republic]	Noted. The text was changed.
20131	30	45	31	13	It is suggested that this passage in Box 8.1 and subsection 7.3.3.2 should be better harmonised. Even though Chapter 7 is aimed at the estimation of forcings, it includes a sizable discussion of cloud microphysics issues, close to the topics addressed in Box 8.1. [philippe waldteufel, France]	Accepted. Coordination was done.
95891	30	47	31	6	This paragraph presents hypotheses as facts. There exists considerable controversy regarding potential aerosol invigoration mechanisms of deep convective clouds, which are not captured by this summary. There is evidence for warm phase invigoration to be efficient but other work cited here as fact (e.g. Fan et al., 2018a) is subject to considerable controversy in the community, with recent accepted publications (Grabowski and Morrison, 2020, https://journals.ametsoc.org/doi/pdf/10.1175/JAS-D-20-0012.1) disputing the mechanisms and methodology. Likewise, invigoration via latent heat of freezing may exist - but it does not materialise in the ongoing ACPC deep convection modelling intercomparison study (van den Heever et al. in prep), bringing together a wide range of state of the art models, including the one used in Fan et al above. This section should be iterated to provide a more balanced view of current scientific consensus. [Philip Philip Stier, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The paper of Grabowsli is now shown to be erroneous in many ways (Fan and Khain, submitted), and should be ignored. The referenced papers, including Fan et al., 2018, do show that some of the invigoration comes through latent heat of additional freezing and deposition. The paper in preparation of van den Heever does not consider latent heating of added vapor deposition. Referencing the added latent heat of deposition was added. The paper of Van den Heever is only for one case, which cannot override all other previous simulations. Furthermore, this paper is still in preparation.

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95889	30	48	30	49	CCN only add to cloud droplet numbers if updrafts can generate sufficient supersaturation (CCN limited regimes). [Philip Philip Stier, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The added CCN was qualified to add cloud droplets compared to a pristine background.
9879	30	52	30	52	Fan et al., (2018a) and 2018b cited in this chapter (Chapter 8) are the same article (https://science.sciencemag.org/content/359/6374/411). So they should be changed to Fan et al., (2018) throughout the chapter. [Jiwen Fan, United States of America]	Accepted. Corrected.
64945	30	57	30	57	is the “medium confidence” based on the single study? [Johannes Quaas, Germany]	Accepted. Two additional references were added in supporting the assessment, out of many more that are available and not included.
4893	31	1	31	1	"flooding from them": from whom? [Bart van den Hurk, Netherlands]	Accepted. Added "form the outer rain bands".
45729	31	1	31	6	This is speculation that aerosol microphysical effects often buffer themselves. It might be the case for some deep convective clouds, but it won't work in all cases or for warm clouds. It is also contradictionary to what you are saying on p. 30 l. 28-31 [Sabine Wurzler, Germany]	Rejected. It is written that the aerosol effects often buffer each other, but not always. This is well established.
20133	31	3	31	3	Should not the reader be explained what is meant by "warm rain"? [philippe waldteufel, France]	Accepted: The term "warm rain" was replaced with rain by drop coalescence.
129151	31	8	31	13	Recommend to replace paragraph with a summary of more recent findings: ""It has been estimated that greater than 50% of Earth's precipitation is initiated by ice nucleating particles (INPs) [Vali et al., 2015] forming ice crystals impacting the hydrological cycle and climate (Lau and Wu, 2003; Mulmenstadt et al., 2015; Storelvmo, 2017). The prediction of atmospheric ice nucleation is challenging due to the fact that (i) different ice nucleation pathways from INP exist (Vali et al., 2015), (ii) INP represent a small fraction of atmospheric aerosol particles on the order of 10-1 to 10-5 (DeMott et al., 2010; Kanji et al., 2017), and (iii) the difference in ice formation rates of INP types stems from the individually different physicochemical particle properties such as composition and morphology (Cziczo et al., 2017; Frohlich-Nowoisky et al., 2016; Hoose and Mohler, 2012; Kanji et al., 2017; Knopf et al., 2018; Murray et al., 2012). Furthermore, subsequent secondary ice formation processes may commonly dominate ice crystal number concentrations in a manner that remains extremely poorly understood (Field et al., 2017; Lauber et al., 2018; Korolev et al., 2020). While it has been recognized that mineral dust particles play an important role in cloud glaciation (Cziczo et al., 2017; Kanji et al., 2017), also biological particles [Frohlich-Nowoisky et al., 2016], particles from the marine environment (DeMott et al., 2016; Knopf et al., 2011; Wilson et al., 2015; McCluskey et al., 2018), and organic aerosol acting as INPs can contribute as sources of ice crystal formation (Knopf et al., 2018; Zhao et al., 2019). Overall the confidence of predicting ice crystal number concentration from aerosol particles remains low. Cloud seeding with IFN such as AgI has been used for enhancing snowfall from orographic supercooled clouds with medium confidence of success (French et al., 2018; Friedrich et al., 2020; Rauber et al., 2019)."" References: Cziczo, D. J., L. A. Ladino, Y. Boose, Z. A. Kanji, P. Kupiszewski, S. Lance, S. Mertes, and H. Wex (2017), Measurements of Ice Nucleating Particles and Ice Residuals, in Ice Formation and Evolution in Clouds and Precipitation: Measurement and Modeling Challenges, edited, pp. 8.1-8.13, American Meteorological Society, doi:10.1175/AMSMONOGRAPHS-D-16-0008.1. DeMott, P. J., et al. (2016), Sea spray aerosol as a unique source of ice nucleating particles, P. Natl. Acad. Sci., 113(21), 5797-5803, doi:10.1073/pnas.1514034112.	Taken into account. The main suggested changes appertain to uncertainties with respect to the effects of INP on precipitation. This qualification was added to the text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
78729	31	8	31	13	A lot of new research has been done in the past decade on what is called IFN herein. This part of the text rather reads like it's from the AR5. First of all, these are ice nucleation particles (INP), and they are called like this in other chapters of AR6, so a change is needed (I checked the whole chapter, and these particles are not mentioned elsewhere, so these changes here are not influencing anything else). Then there is new knowledge and new review papers (the three new citations below are all reviews). I suggest an alternative text for this paragraph, based on the current text, but with amendments and corrections: “Ice nucleating particles (INP) aerosols initiate ice precipitation from persistent supercooled water clouds that have too small cloud droplets for efficient warm rain, or expedite mixed phase precipitation in short lived supercooled rain clouds (Creamean et al., 2013). INP received much research in recent years (Kanji et al., 2017; Coluzza et al., 2017), including parameterizations to better describe atmospheric INP (DeMott et al., 2010). Most INP are desert and soil dust particles and particles of biogenic origin, with the latter being more rare but important at temperatures above about -15°C (Murray et al., 2012). Cloud seeding with IFN such as AgI has been used for enhancing snowfall from orographic supercooled clouds with medium confidence of success (French et al., 2018; Rauber et al., 2019).” - three new citations: 1) Coluzza, I., J. Creamean, M. J. Rossi, H. Wex, P. A. Alpert, V. Bianco, Y. Boose, C. Dellago, L. Felgitsch, J. Fröhlich-Nowoisky, H. Herrmann, S. Jungblut, Z. A. Kanji, G. Menzl, B. Moffett, C. Moritz, A. Mutzel, U. Pöschel, M. Schauperl, J. Scheel, E. Stopelli, F. Stratmann, H. Grothe, and D. G. Schmale, III (2017), Perspectives on the future of ice nucleation research: Research needs and unanswered questions identified from two international workshops, Atmosphere, 8(138), doi:10.3390/atmos8080138. 2) Kanji, Z. A., L. A. Ladino, H. Wex, Y. Boose, M. Kohn, D. Cziczo, and M. Krämer (2017), Chapter 1: Overview of Ice Nucleating Particles, in Ice Formation and Evolution in Clouds and Precipitation: Measurement and Modeling Challenges, edited, Meteor. Monogr., doi:10.1175/AMSMONOGRAPHS-D-16-0006.1. 3) Murray, B. J., D. O'Sullivan, J. D. Atkinson, and M. E. Webb (2012), Ice nucleation by particles immersed in supercooled cloud droplets, Chem. Soc. Rev., 41, 6519-6554,	Taken into account. The term IFN was change to INP, and part of the suggested references and text were incorporated into the text.
45727	31	8	31	13	I am missing the role of biological particles, such as pollen or bacteria here. [Sabine Wurzler, Germany]	Taken into account. Biological particles are now discussed, but there is insufficient room for accounting for their sources, which go beyond only bacteria and pollen.
9875	31	10	31	11	I would add a sentence about the new progress, i.e., "Most IFN are dust and soil particles, with a lesser role for air pollution aerosols (DeMott et al., 2010). Both in-situ observations and modeling with sophisticated bin microphysics suggested that dust particles from long-range transport effectively modified snow precipitation over the Sierra Nevada in California (Creamean et al., 2013; Fan et al. 2014; 2017)." The two references are: Fan, J., L. R. Leung, P. J. DeMott, et al. (2014), Aerosol Impacts on California Winter Clouds and Precipitation during CalWater 2011: Local Pollution versus Long-Range Transported Dust, Atmos. Chem. Phys., 14, 81-101, 2014. Fan. J., L. R. Leung, D. Rosenfeld, and P. J. DeMott (2017), Effects of cloud condensation nuclei and ice nucleating particles on precipitation processes and supercooled liquid in mixed-phase orographic clouds Atmos. Chem. Phys., 17, 1017-1035. http://www.atmos-chem-phys.net/17/1017/2017/doi:10.5194/acp-17-1017-2017. [Jiwen Fan, United States of America]	Taken into account. The effect of INP from long range transport on snow in the Sierra Nevada was incorporated, along with some of the references.
53245	31	18			This comprehensive section could be easier to read if each subsection had a similar framework and addressed the same sequence of questions: Do we have reliable observations? Do we have reliable models? What is the observed multi-decadal variabilty? Is it captured by the all-forcing historical simulations? What are the dominant drivers? What are the implications for the water cycle when it is about changes in large-scale circulation? [Hervé Douville, France]	Noted with thanks.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
103697	31	20	31	22	In Section 8.2, observed and modeling data was also used to assess changes in the water cycle. Perhaps state more clearly in the intro of 8.2 what the objective is, and in 8.3 how it differs from 8.2. Section 8.2 is about the mechnisms that drive the water cycle, and 8.3 about the observations on changes. This could mean some of the statement in 8.2 could be transferred to 8.3 [Philippe Tulkens, Belgium]	Taken into account. Thanks. In the FGD, we have clearly explained in the beginning the objective of section 8.3. "This section focuses on the evaluation and attribution of past and recent water cycle changes using observational datasets, theoretical understanding and model simulations. Paleoclimate records and historical observations provide evidence for past water cycle changes caused both by natural variability and human activities (Haug et al., 2003; Buckley et al., 2010; Pederson et al., 2014). Key elements of the observed water cycle changes are assessed in this section, including flux and storage variations across the atmosphere, the continents and to a lesser extent the ocean and cryosphere, as well as related changes in large-scale atmospheric circulation and modes of variability. Particular emphasis is placed on assessing changes across regions and seasons (Box 8.2)".
17195	31	22	31	29	Unclear as written. It's important to frame this section as clearly as possible. Sugges rephrasing: Changes in seasonal and annual averages, variability and extremes are assessed across the atmosphere, land, ocean and cryosphere…Particular emphasis is placed on assessing changes in seasonality (Box 8.2). Theoretical understanding and model simulations of observations and palaeoclimate records are used to evaluate and attribute observed changes in the water cycle. Futher model evaluation and assessment of the human influence on water cycle variability can be found in Chapter 3. Chapter 9 provides a comprehensive assessment of observed changes in snow cover and glaciers. Chapter 11 provides an assessment of observed changes in heavy precipitation, floods and droughts. Chapter 10 provides detailed regional case studies of observed water cycle changes. [Joelle Joelle Gergis, Australia]	Accepted. This text is rephrased to improve clarity of this section.
53247	31	24			Although the model fit-for-purpose is also assessed in Section 8.5.1, it may be here relevant to emphasize that observed trends cannot be simulated if models are strongly biased or miss important processes, which may need a focused model evaluation in each subsection. [Hervé Douville, France]	Noted. The model fit-for-purpose belongs to 8.5.1. The focus of the introductory part of this section is to ensure clarity and continuity of the text (please see earlier comment).
34921	31	32	31	42	The SOD admits that the warming of 1890-1940 (due then to predominantly natural causes) caused the US Dust Bowl era, Indian Monsoon failures and Australian droughts. It is also interesting that the rate of warming in that period was very similar to the current warming rate, for a then much-lower GHG level. [Jim O'Brien, Ireland]	Noted with thanks.
53249	31	34	31	42	Distribute this material into the following subsections? Rather explains how this section builds on and adds value to CH2 and CH3 and possibly refers to the Essential Climate Variables relevant to the water cycle (https://gcos.wmo.int/en/essential-climate-variables/). [Hervé Douville, France]	Noted. Text was rephrased
79411	31	34	31	42	So, is there any evidence that some period had more significant hydrological changes than another? After reading this paragraph I have the impression that all periods have hydrological changes somewhere which is I guess what we would expect. [Alejandro Di Luca, Australia]	Noted. Editorial change is suggested, see also 53249
17197	31	36	31	36	New paper on dust bowl should be cited: Cowan, T., Undorf, S., Hegerl, G. C., Harrington, L. J. and Otto, F. E. L. (2020). Present-day greenhouse gases could cause more frequent and longer Dust Bowl heatwaves. Nature Climate Change: DOI: 10.1038/s41558-020-0771-7. [Joelle Joelle Gergis, Australia]	Accepted - Reference was added
81067	31	36	31	36	"Dust Bowl" should be capitalized [Benjamin Cook, United States of America]	Accepted - Done

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5569	31	40	31	42	There is a severe sahel multi-decadal drought, but also in the Sout of the mediteranean zone, particulrly in the Maghreb. Cf ref biblio : 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; Nouaceur et al., 2013 PhysioGeo [Benoit Laignel, France]	Accepted - These references were included.
17199	31	41	31	42	To improve regional coverage, please include reference to the very severe Millennium Drought that impacted south-eastern Australia from 1997-2009. Suggest editing to read: Extreme hydrological changes were also observed throughout the second half of the 20th century, for example the severe Sahel multi-decadal drought started in the 1960s and the Millennium Drought that affected south-eastern Australia from 1997–2009 (Cai et al 2014). [Joelle Joelle Gergis, Australia]	Accepted - This text was rephrased and the reference was included.
111389	31	45	31	52	"..flux of fresh water between ocean and atmosphere.. Influences ocean surface salinity" The E-P fluxes interact with the ocean through the ocean surface "lid", however E-P changes lead to subsurface changes to salinity as well. I would remove the "surface" qualifier. In addition, E is never directly measured, rather fluxes are directly measured in very few locations across the global ocean and then E calculated using bulk formula. The difficulty with trying to measure E-P (and E particularly) is it is very state dependent, and occurs on very local scales. Some further expansion of this opening para would be useful to highlight these points [Paul Durack, United States of America]	Accepted - "surface" was removed from the text, short explanation of the problems with direct measures of P-E was added in section 8.3.1.1
4895	31	49	31	49	What is "enhanced oceanic pattern"? [Bart van den Hurk, Netherlands]	Noted - "Enhanced" was replaced by "an amplified" to explain the effect of increasing salinity in the regions with high salinity and freshening in the regions with low salinity, which was described in AR5.
129153	31	49			"The AR5 presented robust evidence of enhanced oceanic pattern in P-E..." This sentence seem incomplete. Robust evidence of what pattern? [Trigg Talley, United States of America]	Noted - Pattern is the increasing salinity in the regions with high salinity and freshening in the regions with low salinity, which was described in AR5. "Enhanced" was replaced by "an amplified".
129155	31	49			Does "enhanced oceanic pattern in P-E" mean an enhanced regional contrast? [Trigg Talley, United States of America]	Noted - Enhanced was replaced by "an amplified" to explain the effect of increasing salinity in the regions with high salinity and freshening in the regions with low salinity, which was described in AR5.
17201	31	51	31	51	Unclear as written Rephrase: ...resulting in the conclusion that these changes are very likely the result of anthropogenic forcing. [Joelle Joelle Gergis, Australia]	Accepted - This sentence was edit according to reviewer suggestion and now it written as "This pattern is consistent with theoretical understanding of human induced changes in the water cycle, resulting in the conclusion that these changes are very likely the result of anthropogenic forcing (Chapter 9)."
129157	31	52			"... resulting in the conclusion that it is very likely ..." This seems unclear. What is "it" referring to? [Trigg Talley, United States of America]	Accepted - It was removed and now this sentence is "This pattern is consistent with theoretical understanding of human induced changes in the water cycle, resulting in the conclusion that these changes are very likely the result of anthropogenic forcing (Chapter 9)."
116727	31		31		is this the first assessment of cloud seeding in an IPCC context? (please check for WGII) [Valerie Masson-Delmotte, France]	Noted. This mention is not meant to evaluate cloud seeding in a mitigation context, but rather as an evidence for the effects of ice nuclei aerosols.
1267	32	1	32	5	Since it's related to P-E, it may be relevant to state that the area of precipitation has diminished by 7% between 1998 and 2016 and over the region 50S-50N, based on TRMM satellite measurements (DOI: 10.1088/1748-9326/aab375). [Rasmus Benestad, Norway]	Accepted - This is mentioned in section 8.3.1.3, which deals with precipitation

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129159	32	1	32	10	Drought is predominantly caused by ENSO, switching places from El Niño to La Niña, with more land drought in EN. See Trenberth, K. E., A. Dai, G. van der Schrier, P. D. Jones, J. Barichivich, K. R. Briffa, and J. Sheffield, 2014: Global warming and changes in drought. Nature Climate Change, 4, 17-22, doi:10.1038/NCLIMATE2067. [Trigg Talley, United States of America]	Accepted - Thank you. Reference at Trenberth et al., 2014 was added
53251	32	4			shift comma? [Hervé Douville, France]	Accepted - Thank you! Comma was shifted. Now it stays after "respectively".
64949	32	8	32	8	To understand the relevance of 20 % cases that show significance of any kind, it would be good to know the significance level [Johannes Quaas, Germany]	Rejected - On asking the author if the trends were statistically significant at the 95% level they responded that they believe that's correct which was not as definitive as I would have hoped and we consider that the current text is adequate and so does not need to be modified.
113275	32	10	32	10	Evaporation is missing ere. Please rephrase as 'Global land runoff, evaporation and precipitation variations correlate significantly to ENSO' Cite: Miralles, D. G., van den Berg, M. J., Gash, J. H., Parinussa, R. M., De Jeu, R. A. M., Beck, H. E., Holmes, T. R. H., Jiménez, C., Verhoest, N. E. C., Dorigo, W. A., Teuling, A. J. and Dolman, A. J.: El Niño–La Niña cycle and recent trends in continental evaporation, Nature Climate Change, 4(1), 1–5, doi:10.1038/nclimate2068, 2013. [Diego Miralles, Belgium]	Accepted - The reference to evaporation was included
28983	32	10			P-E changes over land also correlate with decadal ENSO variability based on simulations and observations (Liu & Allan 2013 ERL doi.org/10.1088/1748-9326/8/3/034002) so this can be added to further back up the summary statement made in the next paragraph [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Reference to Liu and Allan, 2013 was added "Continental P-E estimated from reanalyses and land-surface models driven by meteorological data (Kumar et al., 2019b) reveal interannual variations that are linked to ENSO (Robertson et al., 2014, Trenberth et al., 2014, Liu and Allan, 2013). " In the summary paragraph this statement formulated as ". It is however likely that P-E variations over the global land did not reveal a major anthropogenic influence since the late 1970s but were dominated by internal climate variability, mostly linked to teleconnections of the ENSO and Pacific Decadal Oscillation (PDO) (medium confidence). "
43169	32	16			Read " (including 3 month running mean), updated from Robertson et al. (2016)." rather than " (including 3 month running mean). Updated from Robertson et al., (2016)." [Cyriaque Rufin Nguimalet, Central African Republic]	This figure is dropped in the FGD.
20493	32	22	32	28	Please remember that the purpose of a summary consists of presenting the main points of a text. By definition, everything included in a summary should find its origin in the text. This is not the case here for example for the PDO. Moreover, the unexpected attack against assimilation techniques will be felt as shocking by many readers. [philippe waldteufel, France]	Accepted. PDO was removed from the summary since it was not mentioned in the text and to diminish the "attack against assimilation techniques" "preclude" was replaced by "limit" as was suggested in review comment 17203.
17203	32	23	32	26	Phrasing needs work. Also note there are now more decadal Pacific modes, not just PDO (see chapter 2). Rephrase: In summary, observational uncertainties and current data assimilation techniques limit the clear assessment of recent trends in P-E over land. It is likely that P-E variations over the global land do not show a major anthropogenic influence since the late 1970s but were dominated by internal climate variability, mostly linked to teleconnections of the ENSO and Pacific Decadal Variability (PDV) (medium confidence). [Joelle Joelle Gergis, Australia]	Accepted - Summary was rephrased

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21035	32	26	32	27	The statement “In the tropics...” is consistent with the “wet gets wetter, dry get drier” paradigm, which in a previous section you said it does not hold (see my comment above).Or am I missing something? Please, make sure there is consistency accros the chapter and with other chapters. [Marcelo Barreiro, Uruguay]	Noted - The mechanism for dry gets drier relating to amplification of P-E patterns does not hold over land but it is explained in Section 8.2 that reduced P-E over land is related to other factors including circulation change, decreasing relative humidity in response to land-ocean warming contrasts and vegetation feedbacks which have been more clearly articulated now in Section 8.2.
53253	32	28			The oceanic pattern is supported by observed changes in sea surface salinity which have been attributed to the concommittent human-induced global warming (high confidence, see Chapter 3). While the land pattern has not been so far attributed formally, it is partly consistent with the P-E projections (note that the revised Section 8.4.1.1 could support - or not - this statement) and thus more likely than not partly due to increasing concentrations of greenhouse gases? [Hervé Douville, France]	Rejected. The combination of "more likely than not" and "partially due to" results in a statement that, while perhaps justifiable, is both complicated and low information, and is not analysed in the previous literature. As such, it does not appear worthwhile to spend additional space supporting and discussing it.
3173	32	31	33	41	A major chanhe in water vapour tranport in tropical South America has been recently reported (Barichivich et al., 2018 doi:10.1126/sciadv.aat8785.; Wang et al., 2018. https://doi.org/10.1088/1748-9326/aadbb9; Espinoza et al., 2019. https ://doi.org/10.1007/s00382-018-4462-2) involving intensificantion in the Hadley/Walker cells an increasing of convective activity in northern Amazon basin. Over this region rainfall increased in about 17% since the 1980s (Espinoza et al., 2019. https ://doi.org/10.1007/s00382-018-4462-2). This intensification of regional Hadley cell is also related to changes in moisture advection toward the tropical Andes (Segura et al., 2020. doi: 10.1007/s00382-020-05132-6). [Jhan Carlo Espinoza, France]	Accepted: these references are included in a new discussion of regional atmospheric circulation related moisture transport changes
80301	32	33	32	35	This paragraph is already mentioned in CH2 (page 45, lines 32 to 34) [Paola Arias, Colombia]	Noted: the message is consistent and relevant to both chapters; Section 2.3.1.2 is now referred to in the next paragraph
79413	32	34	32	35	I don't get this: "but with medium confidence of abatement in near-surface moistening trends over land associated with reduced relative humidity since the late 1990s." [Alejandro Di Luca, Australia]	Accepted: abatement is replaced by end of the moistening trend (abatement were the words chosen in AR5)
113277	32	37	32	37	Before RH decreases were mentioned and here the water vapour changes are referred to as consistent with Clausius-Clapeyron [Diego Miralles, Belgium]	Rejected: this refers to the global-scale where total water vapour increases close to the Clausius Clapeyron rate
17205	32	47	32	47	The Southern Hemisphere is a big place! Please specify a region. [Joelle Joelle Gergis, Australia]	Accepted: specific regions are now stated
45263	32	47			It is mentioned in Ch.2 (page 46, lines 10-11) that "A very likely decrease in relative humidity was observed over land areas since 2000, particularly over mid-latitude regions of the Northern Hemisphere". Please check consistency with Ch.2. [Krishnan Raghavan, India]	Noted: this section is consistent with Chapter 2
53255	32	49			historical simulations (what about GMMIP?) [Hervé Douville, France]	Rejected: the monsoons model intercomparison project does not provide additional evidence that is relevant here
21037	32	51	32	51	Shouldn’t 0.4-0.8 be negative numbers? [Marcelo Barreiro, Uruguay]	Accepted: corrected to negative
7623	32	51	32	51	minus 0.4-0.8? Check if there is a missing sign. [Celine Bonfils, United States of America]	Accepted: corrected to negative
43171	32	51			Read " (-0.05 to -0.25 %/decade compared with an observed rate of 0.4-0.8 " rather than " (-0.05 to -0.25 %/decade compared with an observed rate of 0.4-0.8 " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
113279	32	52	32	53	Why not an underestimation of soil moisture contraints on evaporation? [Diego Miralles, Belgium]	Taken into account: the sentence has been simplified since the cause of the discrepancy does not have agreement in the literature
53257	32	52	32	54	These are not the only two possible explanations. The lack of stomatal closure effect in many CMIP5 models could also contribute (e.g., Douville et al., revised), as well as potential issues with in situ observations at least at the regional scale (e.g., Freychet et al., 2019). [Hervé Douville, France]	Accepted: these references are added and the sentence simplified

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8685	33	1	33	1	Blunden & Arndt 2017 reference is relatively vague - could the relevant section in this BAMS State of the Climate be cited instead of the whole report? [Robert Dunn, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: a new reference (Chung et al. 2014) is added but we retain Blunden and Arndt since while we agree the relevant section by John et al. contains the relevant information, the reference is correct and it is not normal to reference sections of publications
20135	33	4	33	6	Commas before "thereby" and after "events" are recommended [philippe waldteufel, France]	Taken into account: the sentence is now split to improve readability
53259	33	4			Expectations are assessed in Section 8.2. What about assessing here trends in reanalyses (including ERA5) and CMIP6 historical (+DAMIP and GMMIP?) simulations (may be using global maps or only zonal means in meridional transport)? [Hervé Douville, France]	Taken into account: the reference to Section 8.2.2 is useful in underpinning the more limited evidence of moisture transport changes and now more discussion of changes are included
79415	33	7	33	8	It is not clear to me how radiosonde data can be used to calculate convergece of humidity… [Alejandro Di Luca, Australia]	Rejected: the publication constructs moisture transport based on an albeit sparse coverage of the entire Arctic by radiosonde data
6709	33	9	33	9	"instrumented" should be replaced by "instrumental". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
26477	33	11	33	13	I would add: ", whereas the trends in moisture transport in the Arctic agree well between reanalyses and radiosonde observations (Nygård at al 2020). However, the modern renanalyses do not agree on the regional patterns of the Arctic moistening (Rinke et al 2019)." References: (1) Nygård T., Naakka T., Vihma T. (2020): Horizontal moisture transport dominates the regional moistening patterns in the Arctic. Journal of Climate. doi: https://doi.org/10.1175/JCLI-D-19-0891.1 (2) Rinke, A., B. Segger, S. Crewell, M. Maturilli, T. Naakka, T. Nygård, T. Vihma, F. Alshawaf, G. Dick, J. Wickert, and J. Keller, 2019: Trends of Vertically Integrated Water Vapor over the Arctic during 1979–2016: Consistent Moistening All Over?. J. Climate, 32, 6097–6116, https://doi.org/10.1175/JCLI-D-19-0092.1 [Tiina Nygård, Finland]	Rejected-The references fail to support the argument. There is no comparison between reanalyses and radiosonde observations in Nygård at al. (2020). According to Rinke et al. (2019), different reanalyses show consistency in the spatial pattern of the Arctic moistening.
53261	33	12	33	13	and opposite but non-significant trends making the detection of changes in the Arctic water cycle still ambiguous despite the polar amplification of global warming. [Hervé Douville, France]	Accepted-The relevant statements have been added.
79417	33	18	33	34	What have we learned about "Observed water cycle changes" based on the improvements described in this paragraph? [Alejandro Di Luca, Australia]	Taken into account - This paragraph sentence has been removed
4179	33	18	33	34	This paragraph mentioned the importance of the trace of water vapor path. Zhong et al. (2018) developed a dynamical recycling model based on time-backward Lagrangian moisture tracking to quantify the moisture resources over Arctic region and centre Asia. They found that the extenal water vapor plays more vital role on the BKS moisture than local water vapor. I think this method is very meaningful and easy to practice. In this case, I recommend the auther to refer relevant works to make scientists know and apply this method. (Zhong et al. 2018, "Local and external moisture sources for the Arctic warming over the Barents-Kara Seas"; Hua et al. 2017, "Decadal Transition of Moisture Sources and Transport in Northwestern China During Summer From 1982 to 2010".) [Wenqi Zhang, China]	Not applicable: this sentence has been removed
20137	33	20	33	20	In this list, while some items are indeed missions (GOSAT, Sentinel), other are instruments, parts of the payload of a multi-instrument mission (SCIAMACHY on ENVISAT, IASI on METOP). Therefore is it more appropriate to write "missions and instruments" rather than "missions". [philippe waldteufel, France]	Not applicable: this sentence has been removed

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53263	33	21			but not on simulated trends given the too short records? (then the rest of the paragraph could be further reduced or removed?) [Hervé Douville, France]	Not applicable: this sentence has been removed
4897	33	26	33	29	One could cite Sutanto, S.J., B. van den Hurk, G. Hoffman, J. Wenninger, P. Dirmeyer, S. Seneviratne, T. Roeckmann, K. Trenberth en E. Blyth, A perspective on different approaches to determine the contribution of transpiration to the surface moisture fluxes; Hydrology and Earth System Sciences, 2014, 11, 2583-2612, doi:10.5194/hessd-11-2583-2014. [Bart van den Hurk, Netherlands]	Not applicable: this sentence has been removed
113281	33	27	33	27	main sources' add 'and sinks' [Diego Miralles, Belgium]	Not applicable: this sentence has been removed
80303	33	28	33	34	More geographical balance should be considered [Paola Arias, Colombia]	Not applicable: this sentence has been removed
113283	33	31	33	34	If you do not mind, please cite for the East and/or West Sahel: Miralles, D. G., Nieto, R., McDowell, N. G., Dorigo, W. A., Verhoest, N. E., Liu, Y. Y., Teuling, A. J., Dolman, A. J., Good, S. P. and Gimeno, L.: Contribution of water-limited ecoregions to their own supply of rainfall, Environmental Research Letters, 11(12), 1–12, doi:10.1088/1748-9326/11/12/124007, 2016. [Diego Miralles, Belgium]	Not applicable: this sentence has been removed
21039	33	34	33	34	To complement the references I would add following "… (Keys et al 2014)." the sentence: "Also, the main sources for southeastern South America are the recycling, central-eastern shore of Brazil together with the surrounding Atlantic ocean, and the southwestern Atlantic (Martin-Gomez et al 2016, J. Climate, DOI: 10.1175/JCLI-D-15-0803.1). [Marcelo Barreiro, Uruguay]	Not applicable: this sentence has been removed
27237	33	34	33	34	Is this general? Please give the name of the humid region if it is specific to that particular region [Eric Brun, France]	Not applicable: this sentence has been removed
14983	33	36	33	37	Check coherency with Chapter 2 regarding the calibrated language used in the statement for water vapour changes [Juan Rivera, Argentina]	Noted: the consistency with Chapter 2 is confirmed
53265	33	36	33	40	Split this long sentence in two parts and link to CH7 and a positive water vapour feedback as far as the first part is concerned? [Hervé Douville, France]	Taken into account: the sentence has been rewritten and split but the link to Chapter 7 is retained in the body text
20139	33	36	33	40	One cannot disagree with this conclusion. The problem facing the reader is that, under various shapes, this conclusion is being repeated many times over the SOD. Is it possible to control this stuttering syndrome? This question will of course be included in a comment concerning the entire report. Specifically, it seems perfectly legitimate to deal with this matter in chapter 8. Therefore, Chapter 8 authors are encouraged to propose why this matter ought then not to be dealt with elsewhere… [philippe waldteufel, France]	Noted: we consider that overlap with other chapters is acceptable but it is necessary to ensure consistency
17207	33	36	33	41	Phrasing needs work, very long sentence. Suggest: In summary, consistent with the AR5, it is very likely that water vapour has increased globally and throughout the troposphere since the 1970s. Based on extended observations and increased mechanistic understanding that relative humidity has decreased over many land regions, there is now high confidence that atmospheric moisture content is increasing, but continental air is becoming less saturated. There is medium confidence that moisture transport is increasing with warming which will influence regional precipitation. [Joelle Joelle Gergis, Australia]	Accepted
113285	33	36	33	41	I agree with 'that atmospheric moisture content is increasing but continental air is becoming less saturated.' butneeds to be reconciled with the statement of water vapour changes are referred to as consistent with Clausius-Clapeyron mentioned above (page 32). [Diego Miralles, Belgium]	Accepted: the reference to a sub-Clausius Clapeyron rate of increase in moisture is now stated
26479	33	40	22	41	could be "…implications for evaporation and precipitation", as moisture transport affects surface evaporation. [Tiina Nygård, Finland]	Accepted-Text revised
7625	33	44	33	44	sse comment #14 about including Marvel and Bonfils 2013. The difficulity is that chapter 3 also has a section fosuing on the D&A of rainfall changes. [Celine Bonfils, United States of America]	Taken into account. We have addressed this in the FGD by coordinating with Ch.3 regarding D & A of precipitation changes (see 8.3.1.3, page 35)
80305	33	46	33	48	This is mentioned in CH2 [Paola Arias, Colombia]	Noted with thanks.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1269	33	48	33	48	An increase in precipitation is either due to more rainy days, more intense rainfall on average or a combination of both. It's worth stating this fact since is matters for both ecosystems and society whether the increase is due to higher intensity or due to more rainy days. The mean precipitation is the product of the wet-day frequency and the wet-day mean precipitation and the total precipitation is the product of the number of days and the mean precipitation. Also there are different processes causing the wet-day frequency to increase (circulation changes) and increased mean intensity (thermodynamics and cloud properties/convection). The decomposition of causes for heavy precipitation is explained in DOI 10.1088/1748-9326/ab2bb2. [Rasmus Benestad, Norway]	Noted. This is important point and the following sentence and corresponding reference were added to the text: "Benestad et al. (2019) found general increase in the probability of precipitation exceeding 50 mm/day for the period 1961-2018 in Europe and USA which is due to increase in the intensity of the rain. "
53267	33	51	33	52	Also quote, here or later, Adler et al. (2017) since GPCP remains one of the most widely used global dataset. Note also that several products allow us to distinguish between convective versus stratiform precipitation, which may exhibit different trends (e.g., Ye et al., 2017). The changing nature of precipitation could therefore also deserve a specific assessment. [Hervé Douville, France]	Taken into account. The Adler et al. (2017) reference on GPCP is mentioned here. Distinction between the changes in convective and stratiform precipitation is also discussed (e.g., Ye et al. 2017).
65785	33	51	33	53	Suggest including modelling uncertainties for rainfall, which are larger. See, e.g. Beck et al. 2017 https://doi.org/10.5194/hess-21-6201-2017 [Kushla Munro, Australia]	Taken into account. Thanks for the suggestion. However, we could not include this reference for the following reasons. The Beck et al. (2017) paper provides an evaluation of 22 gridded daily precipitation (P) datasets for the period 2000–2016, rather than "model uncertainties" for rainfall. Moreover, the dataset covers a relatively short period 2000-2016 which poses difficulties in inferring long-term precipitation variations / changes.
82297	33	52	33	52	I propose to change "Roca, 2019" into "Roca et al., 2019". Ref.: Roca, R., Alexander, L. V., Potter, G., Bador, M., Jucá, R., Contractor, S., Bosilovich, M. G., and Cloché, S.: FROGS: a daily 1° × 1° gridded precipitation database of rain gauge, satellite and reanalysis products, Earth Syst. Sci. Data, 11, 1017–1035, https://doi.org/10.5194/essd-11-1017-2019, 2019. [Schröder Marc, Germany]	Accepted - Reference was changed in the text and added in the reference list
43767	33	52	33	52	Please add Tapiador et al., 2017 after 'model evaluation'. It is a published and relevant paper on that topic. The reference is: Tapiador, F.J., Navarro, A., Levizzani, V., García-Ortega, E., Huffman, G.J., Kidd, C., Kucera, P.A., Kummerow, C.D., Masunaga, H., Petersen, W.A., Roca, R., Sánchez, J.-L., Tao, W.-K., Turk, F.J. 2017. Global precipitation measurements for validating climate models. Atmospheric Research. Vol. 197, 1-20, DOI: 10.1016/j.atmosres.2017.06.021 [Francisco Tapiador, Spain]	Taken into account. Tapiador et al. 2016 reference is now included
79419	33	52	33	53	Why refering to model evaluation when the chapter is about observed changes? [Alejandro Di Luca, Australia]	Agreed. Modelling evaluation is considered in Section 8.5
79431	33	52	33	53	"Neu et al. (2013) demonstrated consistent upward trends in the total number of boreal-winter cyclones " but a few lines below it says "Trend estimates of the total number of cyclones over the Northern Hemisphere during 1979-2016 reveal a large spread across the reanalysis products". Maybe it would be good to make explicit that Neu only used one single reanalysis dataset. [Alejandro Di Luca, Australia]	Noted/accepted. Unlikely the page number is correct, nevertheless, reference to Neu is subordinated with additional references analysing the spread of different reanalyses.
31489	33	53	33	53	Since the AR5, there have been updates of several major satellite, surface, reanalysis and merged data sets of precipitation (e.g., Roca, 2019) yet observational uncertainties still remain an issue for model evaluation, at least for extreme precipitation (Tapiador et al. 2017, Tapiador et al. 2019, Bador et al., submitted). References: Tapiador, F.J., Navarro, A., Levizzani, V., García-Ortega, E., Huffman, G.J., Kidd, C., Kucera, P.A., Kummerow, C.D., Masunaga, H., Petersen, W.A., Roca, R., Sánchez, J.-L., Tao, W.-K., Turk, F.J., 2017. Global precipitation measurements for validating climate models. Atmospheric Research 197, 1–20. https://doi.org/10.1016/j.atmosres.2017.06.021 Tapiador, F.J., Roca, R., Dewitte, B., Petersen, W., Zhang, F., 2019. Is Precipitation a Good Metric for Model Performance? Bull. Amer. Meteor. Soc. 100, 223–233. https://doi.org/10.1175/BAMS-D-17-0218.1 [Andrés Navarro, Spain]	Accepted - 2 refences were added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
86419	33	54	34	4	Observed changes in tropical precipitaion since 1979 are shown to be consistent with increase in moisture transport with warming while the variability since 1950 are associated with decadal changes in El Niño. Please clarify these statements, and the periods mentioned, especially why the decadal changes in El Nino since 1950's have contribution to tropical precipitation variability [Swapna Panickal, India]	Accepted - This has been rephrased for a clearer message.
1271	33	54	34	4	Isn't the statement "observed increases in precipitation in convergence zones and decreases" somewhat inconsistent with the notion of reduced overturning in the atmosphere [Rasmus Benestad, Norway]	Taken into account: it is now added that the reduced circulation partly offset this response
116729	33		33		Nice reference to water isotopes but only from satellites, while there is also new knowledge (but on shorter time scales) emerging from in situ water vapour data (see work from HC Steen Larsen for instance) and diagnoses of moisture origin in atmospheric backtrajectories as well as water tagging in different types of models (see recent work by H Sodemann for instance). [Valerie Masson-Delmotte, France]	Taken into account-Reference to in situ water isotope observations and moisture tracking technologies has been added .
79421	34	1	34	3	Not very clear as most of the warming is observed after 1950 so are the precipitation changes consistent with natural variability or with warming? [Alejandro Di Luca, Australia]	Taken into account: more description is now provided to show that trends are dominated by the warming but there is substantial variability about the trend
113287	34	2	34	2	Also consistent with the dry-gets-dry wet-gets-wet hypothesis [Diego Miralles, Belgium]	Accepted: it is now stated that these trends are wet regimes wetter, dry regimes drier
53269	34	3	34	4	May be replace and expand by quoting three more papers: Despite expectations that changes in regional precipitation should have already occured as a result of human influence on climate, compelling evidence of anthropogenic fingerprints on regional precipitation is generally obscured by observational and modelling uncertainties and is likely to remain so using current methods for years to come (Sarajini et al., 2016). CMIP6 models however predict a strengthening contrast between wet and dry regions in the tropics and subtropics, which is better captured by tracking rainfall change each month in the wettest and driest third of the 30°S-30°N domain and well supported by reanalyses (Schurer et al., 2020). Such a methodology is less affected by model biases and provides further evidence that rainfall has increased in wet regions, but slighly decreased in dry regions over recent decades (Fig. 3.13). This enhanced contrast is attributable to both anthropogenic and natural forcings, although the observed trends are statistically larger than the model responses. Moreover, CMIP5 models also indicate that anthropogenic forcings have resulted in decreased uniformity in annual precipitation amount and intensity at global and continental scales (Konapala et al., 2017). [Hervé Douville, France]	Thank you. The three references (Sarojini et al., 2016, Schurer et al., 2020, Konapala et al., 2017) are included in the FGD and the text is modified suitably.
58937	34	3	34	4	It could be useful to add some reference here and/or level of confidence of this statement. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. References are included here. Done.
98037	34	3	34	4	This is statement on precipitation trends at regional scales need more reference to a timescale for the trends. As shown by Knutson and Zeng (2018, their Figs. 3, 4, and 5 analysis of land-based precipitation trends since 1901, 1951, and 1981) the dominance of internal variability over trends is particularly true for relatively shorter period trends (e.g., 1981-2010). Ref: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637, https://doi.org/10.1175/JCLI-D-17-0672.1. [Thomas Knutson, United States of America]	Taken into account. Thank you. In the FGD, timescale for the precipitation trends is provided in the caption Figure 8.7
20143	34	4	34	11	Assuming that the purpose of figure 8.7 is to compare various historical simulation options with observations on land surfaces plots a) and b), plots c to h would be considerably easier to read if ocean areas were blanked. [philippe waldteufel, France]	Noted. In the FGD, observed precipitation trends are also shown both over land and ocean for the more recent period (1985-2014). For this purpose, we retain precipitation trends from the CMIP6-DAMIP experiments both over land and ocean.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32107	34	6	35	28	Relevant literature: Dyrrdal, A.V., Isaksen, K., Hygen, H.O., and Meyer, N.K., 2012: Changes in meteorological variables that can trigger natural hazards in Norway. Climate Research, 55: 153–165, https://doi.org/10.3354/cr01125. Suggested text: "In Dyrrdal et al. (2012) the intensity of daily annual maxima was shown to have increased in major parts of Norway during 1968-2010." [Dyrrdal Anita, Norway]	In the FGD, we have cut down much of the material on daily heavy precipitation / extremes; as this is being discussed in Chapter 11.
32109	34	6	35	28	Relevant literature: Myhre, G., Alterskjær, K., Stjern, C. W., Hodnebrog, Ø., Marelle, L., Samset, B. H., Sillmann, J., Schaller, N., Fischer, E., Schulz, M., and Stohl, A.: Frequency of extreme precipitation increases extensively with event rareness under global warming, Sci Rep, 9, 2019. Suggested text: "Myhre et al. (2019) showed that the frequency of heavy precipitation events are the main reason for an increase in total precipitation, and that the increase of intensity is less significant." [Dyrrdal Anita, Norway]	Rejected. The assessment of extremes is done in ch 11 and not in ch 8. The suggested reference is already included in the assessment in ch 11
32111	34	6	35	28	Relevant literaure: Benestad, R. E., Parding, K. M., Erlandsen, H. B., and Mezghani, A.: A simple equation to study changes in rainfall statistics, Environmental Research Letters, 14, 2019. Suggested text: "Benestad et al. (2019) indicated that the main cause of a general increase in the probability that precipitation exceeds 50 mm/day has been a boost in the intensity of the rain. They found positive trends over the period 1961–2018 at most locations with observations longer than 50 years in Europe and the USA." [Dyrrdal Anita, Norway]	Not applicable. This text was removed.
89075	34	6		24	A new paper addressing the historical record of Sahel rainfall and the contributions from various forcing agents is Marvel, K., Biasutti, M., & Bonfils, C. (2020). Fingerprints of external forcing agents on Sahel rainfall: aerosols, greenhouse gases, and model-observation discrepancies. Environmental Research Letters. https://doi.org/10.1088/1748-9326/ab858e [Angeline Pendergrass, United States of America]	Noted.
20141	34	7	34	8	Decreases are, or decrease is [philippe waldteufel, France]	Accepted and corrected. Thanks.
113291	34	7	34	11	I would add 'and were intensified by land feedbacks (Zeng, 2003)'. Please cite Zeng, N.: Drought in the Sahel, Science, 302(5647), 999–1000, doi:10.1126/science.1090849, 2003. [Diego Miralles, Belgium]	Accepted.
27247	34	7	34	14	We suggest to cite Chapter 10 4.1.2.1, specifically on this question. [Eric Brun, France]	Rejected. Subsection 4.1.2.1 in Chapter 10 does not exist.
98039	34	7	34	14	While there is a significant negative trend in Sahel region rainfall over 1951-2010 (due to Sahel drought related decreases mentioned from the 1960s -1980s), CMIP5 models did not simulate the negative precipitation trend consistently. In fact, Knutson and Zeng (2018, Fig. 4) show that a 10-model mean CMIP5 model trend over this period was positive in historical runs (and significantly different from observations), indicating the challenge that simulating multidecadal variability poses for models in this region. Ref: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637, https://doi.org/10.1175/JCLI-D-17-0672.1. [Thomas Knutson, United States of America]	Thank you. Taken into account. The challenge in simulating the multidecadal variability of rainfall is included in the FGD.
113289	34	8	34	8	is' for 'are' [Diego Miralles, Belgium]	Accepted and corrected. Thanks.
1273	34	12	34	12	It's fine to state '29-43 mm/yr per decade' but most readers don't know if this is a big deal. Perhaps also state X%/year per decade? The same goes for other figures in this paragraph. [Rasmus Benestad, Norway]	Accepted - The %/decade values computed by the author and now added as requested.
53271	34	13			What is the exact period used for estimating the trends in this figure? Results also suggest that the recent decrease in the NH aerosol loadings has also contributed to the recovery, in agreement with the quoted papers. [Hervé Douville, France]	Thank you. The exact period for calculating the trends in Figure 8.7 is included in the FGD. The recovery is consistent with reduction in NH anthropogenic aerosol loading as reported by several studies

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4899	34	14	34	17	In AR5 some discussion was devoted to the contrast between increasing precipitation in CMIP5 trends and decreasing precipitation/more droughts in observations. Are the observed patterns of drought frequency consistent with CMIP5 or CMIP6 projections? [Bart van den Hurk, Netherlands]	Taken into account - Recent research indicates that the discrepancy in rainfall trends over East Africa between simulations and observations can be explained by internal variability. This is now made more clear by changing and correcting the text
3713	34	17	34	19	Wainwright et al. (2019) investigates the long rains (March-May), this sentence is referring to the short rains. I recommend moving it to the group of citations at the end of the previous sentence. [Declan Finney, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
81069	34	20	34	20	Recent work suggests an anthropogenic contribution to precipitation deficitis during the recent Cape Zero drought in South Africa (https://iopscience.iop.org/article/10.1088/1748-9326/aae9f9/meta). Additionally, a comprehensive analysis of precipitatiion trends in Mediterranean climate regions finds a significant anthropogenic contribution to precipitation drying in this region (https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-18-0472.1) [Benjamin Cook, United States of America]	Taken into account. The paper about the South African drought does not discuss precipitation trends. The mention of the trends in the Mediterranean climate was added.
12791	34	23			Does the changing data coverage refer to spatial or temporal effects or both? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - Text is updated to "explained by the effect of changing spatial coverage of the data over time"
98041	34	24	34	24	The Maidment et al. 2015 study cited here focused on the relatively recent period (1983-2010) and compared with specified SST runs, but not with historical runs designed to isolate the anthropogenic and natural forcing responses. In contrast, an analysis of longer term (1901-2010) GPCC annual precipitation trends (Knutson and Zeng 2018; Fig. 3) over Africa shows detectable decreases in precipitation. (In these regions the decreasing trends are significantly different from natural variability as simulated by CMIP5 models forced natural forcing plus accounting for internal variability). The African regions with detectable decreases in precipitation include parts of northeast Africa/Mediterranean region (extending from Egypt to Syria), and also in the Sudan region and parts of northern tropical Africa. A CMIP5 historical run 10-model ensemble (including both anthropogenic and natural forcing) simulates decreased precipitation over most of these regions, suggesting that anthropogenic forcing contributed to the detectable decreases in those regions. Southern Africa does not exhibit a lot of detectable precipitation trends in the GPCC data: Knutson and Zeng (2018, Figs. 3-5) find little evidence for detectable trends there on any timescale except for a small region along the southwest coast. Ref: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637, https://doi.org/10.1175/JCLI-D-17-0672.1. [Thomas Knutson, United States of America]	Thank you. Taken into account in the FGD.
81071	34	26	34	26	Over Chile, evidence that climate change has contributed to the recent Chilean "meagadrought" (https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/joc.6219). Also, see Seager et al paper from above documenting anthropogenic drying in Mediterranean climate regions, including Chile. [Benjamin Cook, United States of America]	Accepted - These references were included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
5571	34	26	34	27	There are informations about the precipitations in several regions of Africa, but nothing for the North Africa, why ? There is a lot of studies in the Norhern part of Africa. [Benoit Laignel, France]	Noted. Ch 8 is not supposed to assess the changes in precipitation over each specific region (for this you need to refer to regional chapters, i.e. ch 12 and Atlas having specific sections for Africa, including North Africa, Sect. 12.4.1 and Atlas.4.2). Also, west Africa and the Sahel are assessed as a specific case study in ch 10 (sect 10.4.1.2.1). Ch 8 is supposed to deal with the processes at the base of the changes in the water cycle and some regions are considered as examples to describe and assess the main processes at work, but there is no systematic treatment of each region of the world.
3175	34	26	34	28	Note however that over the Peruvian-Bolivian Altiplano a significant rainfall increase has been detected (from observation data and from satellite based estimatios) since the 1980s (Segura et al., 2020doi: 10.1007/s00382-020-05132-6) [Jhan Carlo Espinoza, France]	Accepted - This reference was included.
3177	34	26	34	33	For a more comprehensive view of rainfall trend over the Andes, please include information reviewed in Pabon-Caicedo et al 2020. doi: 10.3389/feart.2020.00061 [Jhan Carlo Espinoza, France]	Accepted
98043	34	29	34	33	In contrast to the statement about unclear roles and anthropogenic and natural forcing for the southeast South America precipitation increases, the CMIP5 regional precipitation trend analysis of Knutson and Zeng (2018, Fig. 3) find that for 1901-2010, the southeastern South America positive trends are detectable and have emerged from the background of natural (forced plus internal) climate variability. Furthermore, these positive trends are in the same direction (though stronger than) the historical forced trends in CMIP5 models (i.e, based on comparing All-Forcing runs with Natural Forcing only runs). These results imply that these regional positive trends over southeastern South America are detectable and at least partly attributable to anthropogenic forcing (medium confidence). A small region of detectable anthropogenic decrease was inferred along the coast of central Chile. Ref: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637, https://doi.org/10.1175/JCLI-D-17-0672.1. [Thomas Knutson, United States of America]	Accepted - This statement was rephrased.
58939	34	30	34	31	A recent paper of Boisier et al (2018) may be added here. Reference: Boisier, J.P., Alvarez-Garretón, C., Cordero, R.R., Damiani, A., Gallardo, L., Garreaud, R.D., Lambert, F., Ramallo, C., Rojas, M. and Rondanelli, R., 2018. Anthropogenic drying in central-southern Chile evidenced by long-term observations and climate model simulations. Elem Sci Anth, 6(1), p.74. DOI: http://doi.org/10.1525/elementa.328 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - This reference was included.
4901	34	33	34	33	Also a reference to Atlas.5.5.2 would be appropriate here [Bart van den Hurk, Netherlands]	Accepted
21041	34	33	34	33	Please add following references: Barreiro, M., N. Diaz, M. Renom, 2014: Role of the global oceans and land-atmosphere interaction on summertime interdecadal variability over northern Argentina Clim. Dyn., doi:10.1007/s00382-014-2088-6 // Martin-Gomez V., M. Barreiro, 2015: Analysis of ocean's influence on spring time rainfall variability over southeastern South America during the 20th century Int. J. Climatology, 36, 1344-1358. doi:10.1002/joc.4428 [Marcelo Barreiro, Uruguay]	Accepted - These references were included.

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58945	34	33	34	37	Haghtalab et al (2018) could be included here. In this study they analyze the spatial patterns of precipitation and its trends and link them to land use and cover changes (such as deforestation) and also to global scale forcings. Reference: Haghtalab, N.; Moore, N.;Heerspink, B.P.; Hyndman, D.W. (2020): Evaluating spatial patterns in precipitation trends across the Amazon basin driven by land cover and global scale forcings. Theoretical and Applied Climatology, 140:411–427. DOI: https://doi.org/10.1007/s00704-019-03085-3. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - This reference was included.
3179	34	33	34	37	Considering the 1980-2018 period the rainfall increasing over the northern Amazon is estimated in 17% (for the wet period), while rainfall diminution in southern Amazon is estimated in 18% (dry season) [Espinoza et al., 2018a]. Therefore, I am not agree with "small decrease" as indicated here. [Jhan Carlo Espinoza, France]	Accepted
3181	34	33	34	37	Note that these changes in regional rainfall in the Amazon basin and related to intensifications in Hadley and Walker cells have been reproduced in modeling studies (e.g. Wang et al., 2018. https://doi.org/10.1088/1748-9326/aadbb9) [Jhan Carlo Espinoza, France]	Noted - Wang et al. (2018) was referenced.
3183	34	33	34	37	Several studies have documented significant changes in runoff regime into the Amazon basin using observed data. Unfortunately, these studies are not reported here. See for instance: Espinoza et al (2014 doi:10.1088/1748-9326/9/12/124007); Ovando et al (2016. J. Hydrol. Regul. Stud. 5, 293–308.); Molina-Carpio et al (2017 10.1080/02626667.2016.126786.) and Espinoza et al (2019. doi: 10.3389/feart.2020.00064.) for Bolivian Amazon; Espinoza et al (2011. doi:10.1029/2011GL047862.), Espinoza et al., (2013. doi: 10.1175/JHM-D-12-0100.1) and Lavado et al (2012. DOI:10.1080/02626667.2012.672985) for the Peruvian Amazon. For a regional view of changes in streamflow in Amazon basin see Espinoza et al (2009. Journal of Hydrology, 375, 297-311) Marengo and Espinoza (2016. doi:10.1002/joc.4420.); Wongchuig-Correa et al (2017. doi: 10.1016/j.jhydrol.2017.04.019.) among many others. [Jhan Carlo Espinoza, France]	Noted - This section deals with observed changes in precipitation. These references were considered in section 8.3.1.5
3185	34	33	34	37	What about extreme flooding related to Coastal El Niño in the western South America (e.g. Takahashi and Martinez., 2018. Clim Dyn 52, 7389-7415.)? [Jhan Carlo Espinoza, France]	Noted - This section deals with observed changes in precipitation. This is considered in section 8.3.1.5
53273	34	35	34	37	based on raw (no bias adjustment) and currently available (not all models have been included) CMIP6 model outputs? [Hervé Douville, France]	Accepted - This is clarified
24325	34	39	34	40	The reference and the temporal coverage of observed precipitation date (1930-2004) are quite old. Recent published reference: Guo, R., Deser, C., Terray, L., & Lehner, F. (2019). Human influence on winter precipitation trends (1921–2015) over North America and Eurasia revealed by dynamical adjustment. Geophysical Research Letters, 46(6), 3426-3434. [Jonghun Kam, Republic of Korea]	Accepted - Reference was added
38071	34	39	34	40	The reference and the temporal coverage of observed precipitation date (1930-2004) are quite old. Recent published reference: Guo, R., Deser, C., Terray, L., & Lehner, F. (2019). Human influence on winter precipitation trends (1921–2015) over North America and Eurasia revealed by dynamical adjustment. Geophysical Research Letters, 46(6), 3426-3434. [Junhee Lee, Republic of Korea]	Accepted - Reference was added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98045	34	39	34	40	The Kumar et al. (2013) study is actually mischaracterized here. See their Fig. 13b. They find significant positive trends (1930-2004) over the central and northeastern US, not the western U.S. More importantly, Knutson and Zeng (2018, Fig. 3) show that these strong increases in precipitation over the north-central and northeastern U.S. and south-central and southeastern Canada have emerged from the background of natural variability and are thus detectable, based on 1901-2010 trends. These trends are in the same direction as, but stronger than, the simulated CMIP5 model trends in runs that include anthropogenic forcing. These results strongly suggest that these increasing precipitation trends are both detectable and attributable, at least in part, to anthropogenic forcing with at least medium confidence. On the other hand, the strong positive precipitation trends in the south-central U.S. are significantly different from natural variability but unfortunately, the CMIP5 model historical runs simulate the wrong sign of precipitation trend in this region (decreasing precipitation) so this regional feature over the south-central U.S. remains unexplained. Knutson and Zeng (2018, Figs. 3-5) find little evidence for any detectable decreases in precipitation over the U.S., Mexico, or Central America in GPCC data (assessed with CMIP5 models) except for a small region of the southwest U.S./northern Mexico during 1981-2010 (seasonal trends). As an aside, the Pacific Northwest and southwest Canada are also interesting cases: there are prominent decreasing trends there in recent observations (1981-2010), but those are not found to be detectable (unusual compared to natural variability), and they don't hold up over longer periods like 1901-2010. (Southwest Canada still has a negative trend over 1951-2010 but it is not detectable, and is a regional trend feature not produced in the CMIP5 historical runs for that period.) Ref: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637, https://doi.org/10.1175/JCLI-D-17-0672.1. See also the supplemental material of this paper for seasonal analyses. [Thomas Knutson, United States of America]	Accepted: the Knutson & Zeng paper is now included in the assessment
7199	34	39	34	55	it should be noted that these trends in precipitation can vary with the period considered in the trend analysis. this issue should be clearly mentioned for completeness of the discussion . See the reference below (supplementary figure ; FIG S7) : Golian, S., M. Javadian, and A. Behrangi (2019), On the use of satellite, gauge, and reanalysis precipitation products for drought studies, Environmental Research Letters, 14(7), 075005, doi: 10.1088/1748-9326/ab2203. [Ali Behrangi, United States of America]	Not applicable. This text was removed.
58941	34	39	34	55	The two previous paragraphs provided consistent regional description of Africa and South America, respectively. In the present paragraph a mixture of regions are described (China, Russia, Australia, India and North America). I found it a bit messy when I read the paragraph. I suggest to reorder the text based on the region described. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted: the paragraphs reflect the available evidence so it was deemed appropriate to maintain their balance
7991	34	40	34	40	North America should be capitalized. Same line "central" should have a small "c". This needs to consistent throughout the chapter. [Anthony Lupo, United States of America]	Accepted - Done
129161	34	40	34	40	North America should be capitalized. Same line "central" should have a small "c". This needs to consistent throughout the chapter. [Trigg Talley, United States of America]	Accepted - Done

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98047	34	43	34	46	An assessment of long-term (1901-2010 or 1951-2010) precipitation trends for Europe is as follows, based on Knutson and Zeng (2018), comparing GPCC (Fig. 3-5) or CRU (Fig. 8) observations with a CMIP5 10-model ensemble. Detectable anthropogenic increases in annual precipitation were found for much of Scandinavia and northwest Russia, and parts of northwest Europe/UK and Iceland. A smaller region of detectable anthropogenic decrease was inferred for parts of southern and southeastern Europe near the Mediterranean. Elsewhere in central Europe the trends were not distinguishable from natural variability. Ref: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637, https://doi.org/10.1175/JCLI-D-17-0672.1. [Thomas Knutson, United States of America]	Accepted - The part of the statement for the observations and reference were added as "Precipitation trends since 1979 over Europe derived from station data do not demonstrate any regular pattern, with differences in trend values across datasets (Zolina et al., 2014; Nguyen et al., 2018b) though longer records since 1910 show increases for much of Scandinavia, northwest Russia, and parts of northwest Europe/UK and Iceland (Knutson and Zeng, 2018), records since 1930 show increases over western Russia (Kumar et al., 2013). "
86421	34	49	34	51	How confident are the role of aerosol induced precipitation changes over north India, especially study by Jin and wang (2017) has reported a revival in Indian summer monsoon since 2002. What is the period for which linear trends are computed in Fig.8.7 [Swapna Panickal, India]	Taken into account. This sentence is modified in the FGD.
98049	34	51	34	55	An assessment of long-term (1901-2010 or 1951-2010) precipitation trends for the Australia region is as follows, based on Knutson and Zeng (2018), comparing GPCC (Fig. 3-5) or CRU (Fig. 8) observations with a CMIP5 10-model ensemble. Detectable anthropogenic increases in annual precipitation were found for much of northern Australia (1901-2010; 1951-2010; and 1981-2010) and a small region along the south-central coast of Australia (1901-2010 only). Small regions of detectable anthropogenic decrease was inferred for parts of southwestern Australia and Tasmania. Elsewhere in eastern Australia and parts of western Australia the trends were not distinguishable from natural variability. Ref: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637, https://doi.org/10.1175/JCLI-D-17-0672.1. [Thomas Knutson, United States of America]	Accepted - This reference was included.
17209	34	51	34	55	Not quite right, and missing SW Australian trends. Please edit to read: Rainfall over northern Australia has increased in all seasons since the 1930s (Kumar et al., 2013). In particular, there has been increases observed in the northwest in summer since 1979 (Figure 8.7) and during the tropical wet season (October– April) since the 1970s. In contrast, there has been a decline in rainfall over southern Australia related to changes in the intensification and position of the sub-tropical ridge (Bureau of Meteorology and CSIRO, 2018). In south-eastern Australia, there has been a decline of around 11% in the April–October rainfall since 1999 (Bureau of Meteorology and CSIRO, 2018). The drying trend over southwest Western Australia is particularly evident between May–July where rainfall declines of around 20% have been observed since 1970. Since 1999, this reduction has increased to around 26% (Bureau of Meteorology and CSIRO, 2018). [Joelle Joelle Gergis, Australia]	Taken into account. This paragraph was rewritten
5573	34		35		There are no informations about the precipitations evolution in the Mediteranean zone and Europe, why ? There exists studies about the relationships between the precipitation varibility and the climatic fluctuations (NAO, ENSO…). [Benoit Laignel, France]	Noted. Ch 8 is not supposed to assess the changes in precipitation over each specific region (for this you need to refer to regional chapters, i.e. ch 12 and Atlas having specific sections for Europe and the Mediterranean, Sect. 12.4.5 and Atlas.5.6). Ch 8 is supposed to deal with the processes at the base of the changes in the water cycle and some regions are considered as examples to describe and assess the main processes at work, but there is no systematic treatment of each region of the world.
7605	35	6	35	6	Dates of coverage for the trends in Figure 8.7 should be specified. [Celine Bonfils, United States of America]	Taken into account in the FGD (Figure 8.7). Trends for two periods (1901-1984) and (1985-2014) are presented.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58981	35	6	35	9	Specify time period for trend calculations in the figure caption. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account in the FGD (Figure 8.7). Trends for two periods (1901-1984) and (1985-2014) are presented.
27249	35	14	35	14	Is the "general increase" significant? [Eric Brun, France]	Not applicable: this sentence has been replaced with a link to the Chapter 11 assessment
22283	35	14	36	15	This seems like it will cross-cut pretty much 1:1 with chapter 11. I am not sure that it is wise to cover a topic so explicitly twice and would urge discussion with chapter 11 to resolve so it only appears once in the report as a whole. [Peter Thorne, Ireland]	Noted. The section is focused on mean precipitation changes (including frequency and intensity) and the assessment of precipitation extremes is in Chapter 11
74359	35	16	35	16	This 10 mm threshold might be valid for a certain region, but not for the whole part in the world [Yulizar Yulizar, Indonesia]	Noted: this is now presented as an example rather than a general finding
53275	35	18			Figure 8.8 may not be the most suitable illustration given the poor-man data assimilation in the 20th century reanalyses. What about replacing this figure by Fig. 5 from the review paper by Alexander (2016) or an update of this figure based on GHCN daily precipitation? [Hervé Douville, France]	Accepted/noted. Generally agree that the present 8.8 is not ideal. Fig.8.8 is replaced by daily precipitation intensity (SDII), and possibly the mean number of dry days.
58993	35	24	35	28	This discussion of freezing rain events seems a bit out of place in a discussion of heavy precipitation events. Are the authors implying that freezing rain is considered a heavy precipitation event? If so, why and how is this supported by the data presented? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected: freezing rain is considered as part of the precipitation section and fits best at the more extreme end of the scale so we keep this line here
7607	35	33	35	33	Dates of coverage for the trends in Figure 8.8 should be specified. [Celine Bonfils, United States of America]	This figure is dropped in the FGD.
43173	35	33			Read "changes in heavy precipitation days (R10mm) (Donat et al., 2016a). " rather than "changes in heavy precipitation days (R10mm). From (Donat et al., 2016a). " [Cyriaque Rufin Nguimalet, Central African Republic]	This figure is dropped in the FGD.
43769	35	38	35	38	Suggest replacing "spatial heterogeneity" by "spatial variability", which is the term widely used in the precipitation literature. [Francisco Tapiador, Spain]	Accepted: used variation
53277	35	38	35	41	Cut the sentence and complete the paragraph by quoting Du et al. (2019)? For instance: "Detection of observed global changes in precipitation extremes is also confounded by internal variability, as well as by spatial heterogeneity and sensitivity to shifts in atmospheric circulation (refs). Yet, the signal-to-noise ratio is improved by... (refs). Moreover, observed global-mean annual-maximum precipitation is 50% stronger for persistent extremes than daily extremes, thereby suggesting the need to focus on complete events with variable duration in D&A studies (Du et al., 2019). [Hervé Douville, France]	Accepted
58959	35	38	35	41	Is this sentence referring to changes in precipitation extremes? I.e. should the sentence begin: "Observational detection of changes in precipitation extremes …"? The final clause also seems a little ambiguous "... while signal-to-noise ratio is improved by analyzing intensity distributions independent of region" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account: this sentence has been modified
108945	35	41	35	41	Cross-link to chapter 11. It would be useful to add a statement that heavy rainfall has increased over the majority of land regions and a land fraction that is larger than expected by chance, e.g. Westra, S., Alexander, L. V., & Zwiers, F. W. (2013). Global increasing trends in annual maximum daily precipitation. Journal of climate, 26(11), 3904-3918. and Fischer, E. M., and R. Knutti (2014), Detection of spatially aggregated changes in temperature and precipitation extremes, Geophysical Research Letters, 41(2), 547-554. [Erich Fischer, Switzerland]	Accepted: a link to Chapter 11 is now added and the Fischer/Knutti 2014 reference is included in the later assessment of whether CMIP5 simulations underestimate the precipitation response
7609	35	41	35	41	repetition needs to be to be fixed. [Celine Bonfils, United States of America]	Accepted: repetition removed

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58961	35	41	35	46	The Fujibe (2013) reference focuses more on (approximate) Clausius Clapeyron (CC) scaling of short duration (i.e. 10-minute and 1-hour) precipitation, rather than daily extremes. Fujibe (2013: 131) notes only in passing that if you look at the long-term (i.e. 100 years considered in a different study - doi:10.2151/jmsj.84.1033) it "implies a CC-like increase of extreme daily precipitation corresponding to long-term warming, although it may not be detectable from data for a few decades". I.e. "extremes in P_day [daily precipitation] show no significant dependence on temperature" over the 1951-2010 period analysed in Fujibe (2013: 131). Therefore it may be better to remove this reference in relation to this point. It is also a little confusing currently, because the following sentence on changes within/outside the range of natural variability only pertains to the Guerrerio et al. (2018) reference, not the Fujibe (2013) reference as well, which is how it reads currently. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: this first reference to Fujibe is removed and the reference added to the sentence on sub-daily extremes
58963	35	46	35	49	Could specify that the Donat et al. (2016b) reference refers to global wet and dry regions to make it clear after the nationally/regionally focused studies discussed in the preceding sentences. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
43175	35	46			Read " Donat et al. (2016b) identify " rather than " Donat et al., (2016b) identify " [Cyriaque Rufin Nguimalet, Central African Republic]	Rejected: the reference format is determined by the agreed Mendeley format
53279	35	49	35	53	or a poorly constrained response of the convective scheme (see also Section 8.5.1). [Hervé Douville, France]	Accepted
89077	35	49		53	It is also relevant to mention here the skill of the models for observed precipitation extremes. This is documented for CMIP6 simulations in a new paper, Akinsola et al (2020). Akinsanola, A. A., Kooperman, G. J., Pendergrass, A. G., Hannah, W. M., & Reed, K. A. (2020). Seasonal representation of extreme precipitation indices over the United States in CMIP6 present-day simulations. Environmental Research Letters. https://doi.org/10.1088/1748-9326/ab92c1 [Angeline Pendergrass, United States of America]	Accepted: reference added as an additional possible cause
12793	35	51	35	52	Are regimes and circulation patterns not the same thing? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: now combined
53281	36	1	36	2	You may want to add that: "Recent satellite observations also show a common occurrence of intense convective systems at high latitudes where the greatest surface warming has been occuring (Houze et al., 2019), although the record is too short for detection-attribution studies. [Hervé Douville, France]	Rejected: the 5-year period is considered too short to assess observed changes
83863	36	9	36	10	Reference Missing: Goswami eta l. , 2006, Increasing Trend of Extreme Rain Events Over India in a Warming Environment, Science, doi: 10.1126/science.1132027 [Ajaya Mohan Ravindran, United Arab Emirates]	Rejected: this is dealt with in the AR4/AR5 assessment which is referred to
83865	36	9	36	10	Reference Missing: Ajayamohan et al., 2010, Increasing trend of synoptic activity and its relationship with extreme rain events over central India, Journal of climate, doi: 10.1175/2009JCL2918.1 [Ajaya Mohan Ravindran, United Arab Emirates]	Rejected: this is dealt with in the AR5 assessment which is referred to
16697	36	9	36	15	regarding the spatio-temporal changes of precipitation in China, a recent study by Su et al. (2020, DOI: 10.3390/atmos11030303) also shows the Increasing intensity of heavy rainfall but suppressed low intensity rainfall in eastern China from 1961 to 2016, accompanied with increasing amount of aerosols. Moreover, it shows the increasing clear day time frequency and decreasing cloudy time frequency in almost all regions except Northwest China. [Chuanfeng Zhao, China]	Accepted: reference added

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9877	36	12	36	15	I changed the sentence and added an additional sentence to reflect the research status better: "Precipitation suppression through aerosol microphysical effects is seen in South America and the southeast Atlantic, associated with local biomass burning (Andreae et al., 2004; Costantino and Bréon, 2010), and in industrial regions in Australia (Hewson et al., 2013; Heinzeller et al., 2016). However, precipitation enhancement through aerosol microphysical effects is also seen in many regions such as Amazon, Southern U.S., India, and Korea associated with anthropogenic aerosols from cities (Fan et al., 2018, Carrio et al., 2010, Sarangi et al., 2018; Lee et al., 2018)." References: Fan., J. , D. Rosenfeld, Y. Zhang, S.E. Giangrande, Z. Li, L.A.T. Machado, et al. (2018). "Substantial Convection and Precipitation Enhancements by Ultrafine Aerosol Particles." Science, 359, pp. 411-418, DOI: 10.1126/science.aan8461. Lee, S. S., Kim, B. G., Li, Z. Q., Choi, Y. S., Jung, C. H., Um, J., Mok, J., and Seo, K. H.: Aerosol as a potential factor to control the increasing torrential rain events in urban areas over the last decades, Atmos Chem Phys, 18, 12531-12550, doi: 10.5194/acp-18-12531-2018, 2018. Sarangi, C., Tripathi, S. N., Qian, Y., Kumar, S., and Leung, L. R.: Aerosol and Urban Land Use Effect on Rainfall Around Cities in Indo-Gangetic Basin From Observations and Cloud Resolving Model Simulations, J Geophys Res-Atmos, 123, 3645-3667, doi:10.1002/2017jd028004, 2018. Carrio, G. G., Cotton, W. R., and Cheng, W. Y. Y.: Urban growth and aerosol effects on convection over Houston Part I: The August 2000 case, Atmos Res, 96, 560-574, doi:10.1016/j.atmosres.2010.01.005, 2010. [Jiwen Fan, United States of America]	Accepted. The text was adopted with slight modifications.
64951	36	14	36	14	the two studies cited are not detection-attribution studies that would allow for the conclusion presented. [Johannes Quaas, Germany]	Rejected. The study of Andreae et al., 2004 is an aircraft campaign that does document the microphysical processes leading to the attribution.
98051	36	17	36	18	Based on Wan et al. (2015) and Knutson and Zeng (2018) you could say there is medium or even high confidence for detectable anthropogenic increasing in precipitation in mid- and sub-polar latitudes of both hemispheres over land regions over the past half century. Refs: Wan, H., Zhang, X., Zwiers, F., and Min, S.-K. (2015). Attributing northern high-latitude precipitation change over the 28 period 1966--2005 to human influence. Clim. Dyn. 45, 1713–1726. doi:10.1007/s00382-014-2423-y. Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637, https://doi.org/10.1175/JCLI-D-17-0672.1. [Thomas Knutson, United States of America]	Taken in to account. "There is medium confidence that such an increase is partly due to anthropogenic forcing." was added to the text.
53283	36	17	36	18	with a detectable human influence on the high latitude precipitation of the northern hemisphere (see also Section 3.3.2). [Hervé Douville, France]	Accepted
21043	36	17	36	18	The statement “there is medium-confidence in the increase in men precipitation...” is not clear where it comes from. Observations in the SH in mid and subpolar latitudes are scarce in the beginning and middle of the century. Also, which season is this statement referring to? There is some evidence that rainfall over the southern ocean has increased during austral summer due to the shift of the SAM, but to my understanding it is not possible to say anything about other seasons. [Marcelo Barreiro, Uruguay]	Accepted. The confidence language was specified for these two seasons and hemispheres. For the NH medium stayed and for the SH for summer season the confidence level changed to low.
17211	36	17	36	26	These summary statements need to be as clear as possible. Clarity and phrasing needs work. Specifically mentioning regions is more helpful than phrases like 'mid and sub polar latitudes'. Remember our audience is a non-specilaist, policy maker. Statements about changes in observed precipitation are very important to the broader community. [Joelle Joelle Gergis, Australia]	Accepted: example regions are now stated
112219	36	18			Both hemispheres is confusing as it might indicate western - eastern instead of northerns - southern [Rutger Hofste, Netherlands]	Taken into account: regions are now specified

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7627	36	19	36	19	add: is linked with greenhouse gas forcing, and a reduction of anthropogenic aerosol emissions. [Celine Bonfils, United States of America]	Accepted.
53285	36	19			is linked to the increasing GHG forcing and the recent decrease in the NH aerosol forcing? [Hervé Douville, France]	Accepted.
28985	36	19			The summary statement "Warming resulting from greenhouse gas forcing has also driven an increase in wet season rainfall in the tropics since 1979 associated with increased moisture transport (medium confidence)." does not seem to be supported by the body text so either this or the body text should be updated. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: this is based on a number of papers referred to in the 2nd paragraph of the section
98053	36	21	36	22	Re: "There is high confidence that a decrease of summer precipitation over northern India and eastern China is linked with aerosol forcing." See the summary paragraph for East Asian monsoon (p. 8-51, lines 31-36). This summary is inconsistent with the above claim of high confidence linkage. Similarly, the discussion for the South Asian monsoon (p. 8-50) doesn't really support the above claim of high confidence either, as it seems difficult to disentangle natural variability from (expected) modeled responses to aerosol forcing. See also the summary statement on p. 8-89 of the draft: "In summary, there is still low confidence in the simulated influence of the aerosol microphysical effects on future precipitation changes." Given all of these uncertainties I would suggest medium confidence until there is a clearly detectable and attributable signal in observations. [Thomas Knutson, United States of America]	Taken into account. The text has been suitably modified in the FGD.
98055	36	22	36	24	Re: "Over most regions internal climate variability is found to dominate multi-decadal trends in regional precipitation (high confidence)." I would say this is most true for relatively short period trends like 1981-2010 or the satellite era. Knutson and Zeng (2018, Fig. 5) find that for land regions 78% of the area of adequate coverage has trends that are non-detectable (not distinguishable from simulated natural variability). However, if one looks at trends over a longer period (1901-2010, their Fig. 3) the fraction of land area with adequate data coverage for trends that has non-detectable trends is down to 58%. So you could mention here that over the century scale there is evidence for detectable anthropogenic increases in precipitation over much of the mid- to high latitude land regions, and some evidence for detectable anthropogenic decreases in precipitation around the Mediterranean region and parts of northern tropical Africa. Ref: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637, https://doi.org/10.1175/JCLI-D-17-0672.1. [Thomas Knutson, United States of America]	Accepted. Thank you. The text has been suitably modified in the FGD.
53287	36	22	36	24	Over most (...) is still found (...), which makes the detection of human influence all the more difficult that it is also obscured by observational and modelling uncertainties (high confidence). Yet, … [Hervé Douville, France]	Taken into account. Thank you. This text in 8.3.1.3 is suitably modified in the FGD as follows: "In summary, regional changes in precipitation amounts can be obscured by the contrasting responses to GHG and aerosol forcings across much of the 20th century and can be thus dominated by internal variability at decadal to multi-decadal timescales (high confidence)." Also it is mentioned in the FGD (page 37, line 26) that "Section 8.3.2.4 assesses monsoon precipitation changes in detail."
20145	36	24	36	26	This looks like a built-in repetition between chapters 8 and 11 [philippe waldteufel, France]	Noted: the statement has been updated for consistency with Chapter 11 but is backed up by evidence in the text as well as referral to Chapter 11

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28975	36	24			Summary statement on heavy precipitation may need checking against Sections 11.4.2 and 11.4.4 (very likely vs likely and high confidence vs likely) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The trends in heavy and extreme precipitation are not the same thing.
103699	36	25	36	25	Why 1951? Perhaps weaken to mid 20th century? [Philippe Tulkens, Belgium]	Accepted
12795	36	26			Can or should you be more specific? Why not say anthropogenic greenhouse emissions rather than mixing up with aerosol etc., if that is the dominant factor? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: there is also aerosol influence (for example relating to cloud microphysics) so for consistency with Chapter 11 we retain anthropogenic
113293	36	29	36	29	Again, 'evapotranspiration' is only used for land. Please correct. [Diego Miralles, Belgium]	Accepted - Land surface ET has been replaced by ET for the sake of brevity
89079	36	29	37	44	Two recent studies on land surface ET that aren't included here are Padrón, R. S., Gudmundsson, L., Greve, P., & Seneviratne, S. I. (2017). Large-Scale Controls of the Surface Water Balance Over Land: Insights From a Systematic Review and Meta-Analysis. Water Resources Research, 53(11), 9659–9678. https://doi.org/10.1002/2017WR021215 Padrón, R. S., Gudmundsson, L., Michel, D., & Seneviratne, S. I. (2020). Terrestrial water loss at night: global relevance from observations and climate models. Hydrology and Earth System Sciences, 24(2), 793–807. https://doi.org/10.5194/hess-24-793-2020 [Angeline Pendergrass, United States of America]	Taken into account - The first reference have been quoted in the revised introduction of this subsection. Although relevant, the second one is less essential for our assessment and has not been included for the sake of brevity.
113295	36	32	36	33	No sublimation? Sorry for the self-promotion but please have a quick look at: https://www.essoar.org/doi/pdf/10.1002/essoar.10503229.1 [Diego Miralles, Belgium]	Rejected. Sublimation is not explicitly mentioned as we think that it is a minor component of ET at the global, and even regional scale.
53289	36	33			transpiration (typically 60% of global mean land surface ET but up to 70% of ET in tropical rainforests, Schlesinger and Jasechko, 2014) [Hervé Douville, France]	Accepted. However, there are uncertainties in the contribution of Transpiration to ET, which depends on methods used with an average contribution around 0.6. The text was changed to "transpiration (around 60% of global mean land surface ET but up to 70% of ET in tropical rainforests, Schlesinger and Jasechko 2014; Wei et al 2017, Lian et al 2018, Li et al 2019)
28987	36	33			I think the sentence can be removed since it is not vital to the assessment: "Modelled ET is usually estimated by applying an empirical soil moisture stress on potential ET as estimated from the surface energy budget". I assume potential ET on the next line is defined in the glossary? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - We think it is important to highlight the potential hiatus between the complexity of the multiple drivers of recent ET trends, as inferred from more or less direct observational estimates, and the relatively empirical treatment of ET in global climate models. Potential evaporation is defined in the SOD glossary.
4907	36	35	36	35	drop "only": it is suggestive [Bart van den Hurk, Netherlands]	Accepted. Update in the text
103701	36	36	36	36	Sure AR5 said 'declined in most continental areas'? This really needs a thorough explanation, whether AR6 overrules AR5 [Philippe Tulkens, Belgium]	Taken into account - We do confirm the AR5 CH2 key findings but the revised paragraph now aims at reconciling the contrasted (medium confidence) trends in pan evaporation vs actual ET by emphasizing the possible contribution of internal climate variability to pan evaporation interdecadal variability and of vegetation greening to the recent increase in actual ET.

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113297	36	36	36	36	Please define 'potential ET' somewhere early on… [Diego Miralles, Belgium]	Take into account. A reference to the glossary is add in the text.
113303	36	36	36	36	Miralles et al. (2016) did not show trends. Change for the above Miralles et al. (2014). You may also add: Brutsaert, W.: Global land surface evaporation trend during the past half century: Corroboration by Clausius-Clapeyron scaling, Advances in Water Resources, 106, 3–5, doi:10.1016/j.advwatres.2016.08.014, 2017. Cheng, L., et al.: Recent increases in terrestrial carbon uptake at little cost to the water cycle, Nature Communications, 1–10, doi:10.1038/s41467-017-00114-5, 2017. [Diego Miralles, Belgium]	Taken into account - Miralles (2016) has been replaced by Miralles (2014). The study by Cheng et al. (2017) has been quoted in the revised subsection to emphasize the role of increasing water use efficiency.
113299	36	37	36	37	Add citation after 'humidity'. Maybe McVicar, T. R. and Roderick, M. L.: Atmospheric science: Winds of change, Nature Publishing Group, 3(11), 747–748, doi:10.1038/ngeo1002, 2010. [Diego Miralles, Belgium]	Rejected - The emphasis is put on post-AR5 publications.
12797	36	37			Change the beginning of the sentence to, "In contrast, on a global scale, ..." [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not Applicable - The whole paragraph has been rephrased.
113301	36	40	36	40	Actually I am quite certain that the study that showed that the decline reported by Jung et al. was due to ENSO is this one: Miralles, D. G., van den Berg, M. J., Gash, J. H., Parinussa, R. M., De Jeu, R. A. M., Beck, H. E., Holmes, T. R. H., Jiménez, C., Verhoest, N. E. C., Dorigo, W. A., Teuling, A. J. and Dolman, A. J.: El Niño–La Niña cycle and recent trends in continental evaporation, Nature Climate Change, 4(1), 1–5, doi:10.1038/nclimate2068, 2013. [Diego Miralles, Belgium]	Accepted - The suggested reference has been quoted in the revised subsection.
28989	36	40			remove "presumably" since there is evidence it has; instead a confidence statement could be made. It would be useful to add in post-AR research e.g. Zhang et al. (2016) Sci. Rep. doi:10.1038/srep19124. Also Stephens et al. (2018) https://doi.org/10.1029/2018GL079332 find declines in ET reversed in mid-2000s due to vapour pressure deficits dominating over wind speed changes (stilling) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The statement made in Jung et al. (2010) remains speculative. Note that the suggested references have been quoted in the revised subsection and that confidence statements are only used in the final summary of the assessment.
53291	36	43	36	44	Figure 8.9 deserves further discussion and is not best quoted here. [Hervé Douville, France]	Accepted. This figure is no longer referenced here.
113305	36	45	36	49	Please do not call it 'reconstruction'. We do not reconstruct, we 'estimate' with process-based models or machine learning. [Diego Miralles, Belgium]	Accepted - "reconstructions" has been replaced by "estimates"
4911	36	47	36	47	strange to use mass units tonnes; suggest to use mm [Bart van den Hurk, Netherlands]	Not Applicable - The range has been suppressed but a confidence interval has been added, in mm/year/decade.
58965	36	47	36	48	Would it be better just to use one set of units in the sentence (i.e. perhaps converting the range quoted in the first clause to mm year-1 decade-1 if it is not already stated in Zeng et al. (2018b))? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not Applicable - The range has been suppressed but a confidence interval has been added, in mm/year/decade.
53293	36	48	36	51	According to a recent study (Zhan et al., 2019) based on the comparison between ET and PET, the increase of global permanent surface water area (cf. Section 8.3.1.7.3) between 1984-1999 and 2000-2015 may have caused a 30.4 ± 15.5 km3/yr (convert in mm/day?) increase in global ET, a change which is of comparable magnitude to that of ET change assuming stationary surface water areas and may therefore also contribute to strong uncertainties in ET reconstructions. [Hervé Douville, France]	Taken into account - This has been included
113307	36	50	36	51	I dissagree with this statement. In Miralles et al. 2016 we showed that intrception loos is the main source of dissagreement, not transpiration (despite the latter being a bigger chunck of ET). [Diego Miralles, Belgium]	Taken into account - Both transpiration and interception loss have been emphasized as sources of disagreement in the revised sentence.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
113309	36	53	36	54	Rephrase as: 'The decline in global ET recorded from 1998 to 2008 (Jung et al., 2010) and reported in the AR5 was shown to be an episodic phenomenon associated to ENSO variability (Miralles et al. 2014)'. The reference to Zhang is not correct. [Diego Miralles, Belgium]	Rejected - Both Miralles et al. (2014) and Zhang et al. (2015) are relevant references here.
113311	36	54	37	4	Who is 'they' referring to? If you mean Zhang et al., that paper is a study of the greeness effects on ET trends, and it is posterior to the Nature Climate Change paper by Miralles et al. that revealed the ENSO impact on the evaporation decline you mention. I would strongly encourage to rephrase these statements – please see abstract of Miralles et al. (2014). https://www.nature.com/articles/nclimate2068 Moreover, it is not just ENSO. See: Martens, B., Waegeman, W., Dorigo, W. A., Verhoest, N. E. C. and Miralles, D. G.: Terrestrial evaporation response to modes of climate variability, Nature PJ Climate and Atmospheric Science, 1(1), 1–7, doi:10.1038/s41612-018-0053-5, 2018. [Diego Miralles, Belgium]	Taken into account - We refer to Zhang et al. (2015) which emphasizes the vegetation greening effect on ET but also the role of ENSO variability (cf. end of their abstract). As suggested, we also quote Martens et al. In the revised subsection to further emphasize the role of internal variability in ET multidecadal variability.
74377	36	45 and 46	36	45 and 46	for the expression "last decade" need to precise which periode [Moulay Driss HASNAOUI, Morocco]	Accepted - "over the past three decades" has been replaced by "between the early 1980s and the early 2010s"
113313	37	6	37	6	so far only one attempt'. While I very much like the study by Douville et al., I believe that others have also tried to attribute these trends; perhaps less 'formally'. From the top of my head I know these: Laîné, A., Nakamura, H., Nishii, K. et al. A diagnostic study of future evaporation changes projected in CMIP5 climate models. Clim Dyn 42, 2745–2761 (2014). https://doi.org/10.1007/s00382-014-2087-7 Zhang, Y., Peña-Arancibia, J., McVicar, T. et al. Multi-decadal trends in global terrestrial evapotranspiration and its components. Sci Rep 6, 19124 (2016). https://doi.org/10.1038/srep19124 Berg, A. and J. Sheffield, 2019: Evapotranspiration Partitioning in CMIP5 Models: Uncertainties and Future Projections. J. Climate, 32, 2653–2671, https://doi.org/10.1175/JCLI-D-18-0583.1 [Diego Miralles, Belgium]	Taken into account - While we maintain that Douville et al. (2013) is so far the only formal attribution study (i.e., based on optimal fingerprints), we now also quote Dong and Dai (2017) that further support their finding of a human influence on recent (rather than projected) ET trends. Note however that the suggested references are not relevant to the D&A topic.
39903	37	6	37	10	“medium confidence” , but only based on one paper [TSU WGI, France]	Accepted - The confidence statement has been removed from the sentence.
7611	37	9	37	9	lack of precision for "recent ET variations". Insterad, repalce by: "the recent rise in ET in both the northern high latitudfes and midlatitudes since 1960 could not be understood" [Celine Bonfils, United States of America]	Accepted - although "recent" has been replaced by "post-1960" and "since 1960" has been removed for the sake of brevity.
64953	37	9	37	10	is one study enough for medium confidence? (even if it is the CLA) [Johannes Quaas, Germany]	Accepted - although "recent" has been replaced by "post-1960" and "since 1960" has been removed for the sake of brevity.
116731	37	10	37	14	I do not understand why "threat" is used specifically for this aspect and not others (IPCC usually refers to reasons for concern eg for human security). Please integrate with Ch 5 for CO2 physiological effects ; there is literature related to greening effects too (eg. Forzieri et al). The reference (Douville et al 2012) should be 2013. [Valerie Masson-Delmotte, France]	Taken into account - "threat" may be a too emotive wording and has been removed from the revised draft, the reference has been updated (2013 instead of 2012) and more emphasis has been put on the greening effect in the next paragraph.
28991	37	10			"dimming decades" is unclear so could be explicit e.g. "end of aerosol-related declines in surface sunlight" and could cite e.g. Schwarz et al. (2020) Nature Geosci. https://doi.org/10.1038/s41561-019-0528-y; "possible threat posed by enhanced GHG" sounds emotive and could be replaced with "influence of increasing GHG" [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - The first part of the sentence has been modified accordingly and the suggested reference has been added. Yet, the end of the sentence now reads as "and highlighted the potential negative influence of increasing GHG concentration and related global warming on freshwater resources if there is no parallel increase in precipitation at the regional scale" to emphasize that increasing ET can be a threat for regional water resources.
7613	37	14	37	14	Optional: you could add at the end: "While the omission of these factors facilitates the attribution of ET changes to anthropogenic (GHG and aerosols) influences, it also prevents the complete attribution of changes measured in-situ." [Celine Bonfils, United States of America]	Taken into account - The last part of the proposed sentence has been introduced in the revised paragraph.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58983	37	19	37	22	Specify time period for trend calculations in the figure caption. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account in the FGD (Figure 8.8). Trends for two periods (1901-1984) and (1985-2014) are presented.
53295	37	19			As for Figure 8.8, the caption of Figure 8.9 should specifc the selected period. [Hervé Douville, France]	Taken into account in the FGD (Figure 8.8). Trends for two periods (1901-1984) and (1985-2014) are presented.
113315	37	20	37	20	Please add the reference to the 'observational dataset'. Miralles, D. G., Holmes, T. R. H., De Jeu, R. A. M., Gash, J. H., Meesters, A. G. C. A. and Dolman, A. J.: Global land-surface evaporation estimated from satellite-based observations, Hydrol. Earth Syst. Sci., 15(2), 453–469, doi:10.5194/hess-15-453-2011, 2011. Maybe add 'GLEAM' as well, because it is in the figure and not in the caption (so readers may not know whee the 'observational dataset' comes in teh figures) [Diego Miralles, Belgium]	Noted. The LMIP (1901-1984) and GLADS (1985-2014) are products considered in Figure 8.8 (FGD).
113317	37	27	37	28	The references to Good et al. and Jasechkoet al. feel already old considereing how much it has happened in this topic in recent years. In addition Jasechko's paper has been disputed even by Scott Jasechko himself. I would replace for: Wei, Z., Yoshimura, K., Wang, L., Miralles, D. G., Jasechko, S. and Lee, X.: Revisiting the contribution of transpiration to global terrestrial evapotranspiration, Geophys. Res. Lett., 165(2), 549–10, doi:10.1002/2016GL072235, 2017. And: Stoy, et al.: Reviews and syntheses: Turning the challenges of partitioning ecosystem evaporation and transpiration into opportunities, Biogeosciences, 16(19), 3747–3775, doi:10.5194/bg-16-3747-2019, 2019. [Diego Miralles, Belgium]	Taken into account - The former reference to Jasechko et al. (2013) has been replaced by Wei et al. (2018)
113321	37	27	37	40	Please add also 'recently a new study has found evidence of an increased physiological control over ET (Forzieri et al., 2020)'. Honestly, this is not self-promotion; they are high-impact journal papers that should be acknowledged… Forzieri, G., Miralles, D. G., Ciais, P., Alkama, R., Ryu, Y., Duveiller, G., Zhang, K., Robertson, E., Kautz, M., Martens, B., Jiang, C., Arneth, A., Georgievski, G., Li, W., Ceccherini, G., Anthoni, P., Lawrence, P., Wiltshire, A., Pongratz, J., Piao, S., Sitch, S., Goll, D. S., Arora, V. K., Lienert, S., Lombardozzi, D., Kato, E., Nabel, J. E. M. S., Tian, H., Friedlingstein, P. and Cescatti, A.: Increased control of vegetation on global terrestrial energy fluxes, Nature Climate Change, 1–22, doi:10.1038/s41558-020-0717-0, 2020. [Diego Miralles, Belgium]	Accepted - This recent reference has been quoted and summarized in the revised paragraph.
12799	37	29	37	30	"recent decades" and "over these last decades". Both are not needed in the sentence, so remove the latter. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - "over these last decades" has been removed
4913	37	34	37	34	hard to believe that 12 mm change is 55% of total ET. Is this correct? [Bart van den Hurk, Netherlands]	Taken into account - The 55% contribution refers to the total increase in global ET. This has been clarified in the revised sentence.
113319	37	34	37	34	This discussion misses the findings of this article, as condensed in its title. I think this was actually the first to look at the competting effects of WUE increase versus greening on ET: Cheng, L., Zhang, L., Wang, Y.-P., Canadell, J. G., Chiew, F. H. S., Beringer, J., Li, L., Miralles, D. G., Piao, S. and Zhang, Y.: Recent increases in terrestrial carbon uptake at little cost to the water cycle, Nature Communications, 1–10, doi:10.1038/s41467-017-00114-5, 2017. [Diego Miralles, Belgium]	Accepted - This reference has been quoted in the revised subsection.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
113323	37	42	37	44	This is an extremely negative and dismissively short take-home message, that in addition does not reflect the content of the previous paragraphs nor the literture published in the last decade. If you extract the titles from 10 of the high-impact papers above cited I cannot believe you can condense those into 'we do not know'. Mentioning the uncertainties is fine, but please include, at the very least (a) overall positive trends in general agrement with C-C in energy limited climates, (b) strong climate variability mostly due to ENSO, (c) an increasing role of vegetation physiology. [Diego Miralles, Belgium]	Accepted. The assessment statements here have been modified accordingly.
7615	37	43	37	44	Optional: you could add at the end; "uncertain role of plant physiology and greening. The contributing roles of land use, land cover, GHG vs AA through time (Figure 8.9) needs further investigation." [Celine Bonfils, United States of America]	Accepted - The final summary of the assessment has been modified accordingly.
53297	37	44			given the possible offset between increased water use efficiency and enhanced photosynthesis? [Hervé Douville, France]	Noted. This assessment text has been revised.
17213	37	47	37	47	Floods not floodings [Joelle Joelle Gergis, Australia]	Accepted.
53299	37	47			Nothing about Africa where blue water resources are however vital. May be quote the review paper by Roudier et al. (2014)? The section could also start by explaining why river discharge is a key variable of the water cycle, for instance as follows: "River-discharge measurements have essential direct applications for water management and related services, including irrigation and flood protection. The flow of freshwater from rivers into the oceans also needs to be monitored because it reduces ocean salinity, and changes in flow may thereby influence the thermohaline circulation. Detecting and attributing possible changes in runoff, streamflow and floods is therefore crucial to help identify and adapt to some of the most significant potential effects of climate change." [Hervé Douville, France]	Rejected. The review paper of Roudier et al. (assuming HESS, 18, 2789–2801, 2014) focuses on potential future changes not observed changes, which are the focus of this section. Also, this focuses directly on the observed changes and relies on the earlier parts of the chapter to described the water cycle.
22285	37	49	38	4	This opening paragraph should also note the chapter 2 global assessment finding. [Peter Thorne, Ireland]	Accepted. Chapters 2 and 3 are now mentioned.
28993	37	49	38	9	Since AR5 there have been advances in understanding how small trends in streamflow over the USA can be explained through modelling as a cobination of counteracting effects of enhanced plant growth and Increased water-use efficiency driven by increased CO2 levels e.g. Singh et al. (2020) GRL https://doi.org/10.1029/2019GL086940 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. As Chapter 11 provides a comprehensive assessment of flooding, the discussion of flooding in this section now just summarizes their assessment. No additional material is provided, for the sake of consistency.
12801	37	55			The wording here is confusing. Suggest replacing "or permanent glacier" to "or melt of permanent glacier". (But then this would result in increased streamflow, would it not? So I'm not quite sure of the point being made. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not Applicable - The whole paragraph has been rephrased.
65787	38	1	38	3	Suggest clarification. Suggest adding the following statement: "In Australia, significant downward trends in mean annual streamflow are observed in the southwest and southeast of the country but increases in the monsoonal north (Zhang et al. 2016 https://doi.org/10.5194/hess-20-3947-2016), increases in low frequency floods in the east, but decreases in higher frequency floods (Wasko and Nathan 2018)" [Kushla Munro, Australia]	Not applicable. As chapter 11 provides a comprehensive assessment of flooding, the discussion of flooding in this section now just summarizes their assessment. No additional material is provided, for the sake of consistency.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45571	38	6	38	13	Trends in mean annual streamflow (1961-2012) over Western Europe show a strong climate-driven North-South divide, with large decreases in the Iberian peninsula which are heighten by direct anthropogenic influences (Vicente-Serrano et al., 2019). Vicente-Serrano, S., Hannaford, J., Murphy, C., Peña-Gallardo, M., Lorenzo-Lacruz, J., Domínguez-Castro, F., López Moreno, J. I., Beguería, S., Noguear, I., Harrigan, S., Vidal, J.-P. (2019) Climate, irrigation, and land-cover change explain streamflow trends in countries bordering the Northeast Atlantic. Geophysical Research Letters, 46, 10821-10833, https://doi.org/10.1029/2019GL084084 [Jean-Philippe Vidal, France]	Accepted. The reference has been added.
58971	38	10	38	10	I think the reference should be Blöschl et al. (2019). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not Applicable - The whole paragraph has been rephrased.
4915	38	10	38	13	what is the role of increased irrigation and other non-CO2 antropogenic drivers? [Bart van den Hurk, Netherlands]	Not applicable. This text has now been replaced by a summary of Chapter 11's assessment of flooding, which is more comprehensive.
8663	38	10	38	13	A more plausible explanation of the reduction of streamflow in South Europe are the land cover changes and water demand by irrigated lands better than possible anthropogenic influences. In the absence of substantial changes in precipitation the increase of the atmsopheric evaporative demand cannot explain solely the decrease in streamflow without considering changes in land cover/water demand: Vicente-Serrano, S.M., Marina Peña-Gallardo, Jamie Hannaford , Conor Murphy , Jorge Lorenzo-Lacruz , Fernando Dominguez-Castro , Juan López-Moreno , Santiago Begueria , Ivan Noguera , Shaun Harrigan , Jean-Philippe Vidal. (2019) Climate, irrigation and land-cover change explain streamflow trends in Western Europe. Geophysical Research Letters, 46, 10,821–10,833. Teuling AJ, de Badts E, Jansen FA, Fuchs R, Buitink J, van Dijke AJ, Sterling S. 2019. Climate change, re-/afforestation, and urbanisation impacts on evapotranspiration and streamflow in Europe. Hydrology and Earth System Sciences. DOI: 10.5194/hess-2018-634. Vicente-Serrano, S.M., Zabalza-Martínez, J., Borràs, G., López-Moreno, J.I., Pla, E., Pascual, D., Savé, R., Biel, C., Funes, I., Azorin-Molina, C., Sanchez-Lorenzo, A., Martín-Hernández, N., Peña-Gallardo, M., Alonso-González, E., Tomas-Burguera, M., El Kenawy, A. (2017) Extreme hydrological events and the influence of reservoirs in a highly regulated river basin of northeastern Spain. Journal of Hydrology:Regional Studies 12: 13-32. Vicente-Serrano, S.M., Zabalza-Martínez, J., Borrás, G., López-Moreno, J.I., Pla, E., Pascual, D., Savé, R., Biel, C., Funes, I., Martín-Hernández, N., Peña-Gallardo, M., Beguería, S., Tomás-Burguera, M., (2017) Effect of reservoirs and water demand on streamflow and river regimes in a heavily regulated river basin of Northeast Spain. Catena. 149: 727-741. García-Ruiz, J.M., López-Moreno, J.I., Vicente-Serrano, S.M., Lasanta, T., Beguería, S. (2011): Mediterranean water resources in a global change scenario. Earth Sciences Review 105, 121-139. Note that precipitation decrease in South Europe is not evident: Vicente-Serrano, S.M., Domínguez-Castro, F., Murphy, C., Hannaford, J., Reig, F., Peña-Angulo, D., Tramblay, Y., Trigo, R.M., MacDonald, N., Luna, M.Y., McCarthy, M., Van der Schrier, G., Turco, M., Camuffo, D., Noguera, I., El Kenawy, A., García-Herrera, R., Becherini, F., della Valle, A. Long-term variability	Not applicable. This text has been removed.
116733	38	10	38	14	"altough models… " (which types of models?) Please also refer to SROCC for impacts of glacier shrinkage (chapter 2 has clear statements on this). [Valerie Masson-Delmotte, France]	Not applicable / Accepted. Not applicable: the "although models …" text has been removed. Accepted: the SROCC assessment on glacier shrinkage is now included.
39901	38	10			"it is likely that..."without confidence language [TSU WGI, France]	Not applicable. This text has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96803	38	11	38	11	Please clarify what is meant with "this pattern" here. If "models significantly underestimate the response" that is reflected in observations by "a small and insignificant increase in the mean streamflow", there would not be much support for the statement that it is "likely that these changes were caused by anthropogenic influence". [Nicole Wilke, Germany]	Not applicable. This text has been removed.
12803	38	11			Change to "accounting for the increase in..." [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This text has been removed.
58943	38	13	38	17	Results from Barichivich et al 2018 could be included here. This study assesses the recent intensification of flooding over the Amazon and associates these to a strengthened Walker circulation. Reference: Jonathan Barichivich, Emanuel Gloor, Philippe Peylin, Roel J. W. Brienen, Jochen Schöngart, Jhan Carlo Espinoza, Kanhu C. Pattnayak. Recent intensification of Amazon flooding extremes driven by strengthened Walker circulation. Science Advances, 2018; 4 (9): eaat8785 DOI: 10.1126/sciadv.aat8785 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. As chapter 11 provides a comprehensive assessment of flooding, the discussion of flooding in this section now just summarizes their assessment. No additional material is provided, for the sake of consistency.
58947	38	13	38	17	Rivera et al (2017) also studied observed decrease in the river runoff over central Andes in Argentina and the relationship to the Pacific ocean conditions. Results of this paper (and references therein could be added for further discussion here. Another interesting study to take into account is Burger et al (2019) in which the glacier contribution to runoff over Andean catchment is studied with special focus on local scale processes (snow avalanche, debris thickness, amonth others). References: Rivera, J.A.; Penalba, O.C.; Villalba, R., Araneo, D.C. (2017) Spatio-Temporal Patterns of the 2010–2015 Extreme Hydrological Drought across the Central Andes, Argentina. Water, 9(9), 652; https://doi.org/10.3390/w9090652. Burger, F.; Ayala, A.; Farias, D.; Shaw, T.E.; MacDonell, S.; Brock, B.; McPhee, J.; Pellicciotti, F. (2019) Interannual variability in glacier contribution to runoff from a high-elevation Andean catchment: understanding the role of debris cover in glacier hydrology. Hydrol. Process. 33, 214–229. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	No longer applicable. The section now primarily summarizes the assessments in chapters 2 and 3.
3187	38	15	38	17	Segura et al (2020) is a better reference for this point (doi: 10.1007/s00382-020-05132-6) [Jhan Carlo Espinoza, France]	Not applicable. This text has now been replaced by a summary of Chapter 11's assessment of flooding, which is more comprehensive.
98057	38	20	38	22	This sentence does not make sense to me. Is it referring to two different regions? [Thomas Knutson, United States of America]	Not applicable. This text has been removed.
20495	38	20	38	22	What Gudmundsson et al report is different (please read carefully the second paragraph of their conclusions) [philippe waldteufel, France]	Not applicable. This text has been removed.
129163	38	24	38	28	This section summarizes Berghuijs et al. (2017) for trends in 30-year floods in four regions of the world in the last 30 years. There is a study on major-flood trends (25-100 year floods) that should be discussed here (Hodgkins et al., 2017) which is based on data from 1200 relatively natural basins in North America and Europe. It shows that there are not more temporal trends than expected due to chance in the number of major floods over the last 50-75 years, and that the major floods for some regions in both North America and Europe are related to the Atlantic Multidecadal Oscillation. Citation: Hodgkins, G.A., Whitfield, P.H., Burn, D.H., Hannaford, J., Renard, B., Stahl, K., Fleig, A.K., Madsen, H., Mediero, L., Korhonen, J. and Murphy, C., 2017, Climate-driven variability in the occurrence of major floods across North America and Europe: Journal of Hydrology, v. 552, p. 704-717, http://dx.doi.org/10.1016/j.jhydrol.2017.07.027. [Trigg Talley, United States of America]	Not applicable. As chapter 11 provides a comprehensive assessment of flooding, the discussion of flooding in this section now just summarizes their assessment. No additional material is provided, for the sake of consistency.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12805	38	26	38	27	Would the general reader understand what is meant by "near-natural"? I'm guessing it refers to catchments where there is no human interuption to the flow, such as dams or barrages. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This text has been removed.
12807	38	28			This term GHM has not yet been defined. Later definitions of the acronym (p86, p90, p92) should be removed. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This text has been removed.
12809	38	31			Something wrong with the referencing style here. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Editorial – copyedit to be completed prior to publication
43177	38	31			Read "conditions (Graversen et al., 2014; Lange et al., 2019, submitted)." rather than "conditions, (Graversen et al., 2014) Lange et al. (2019; submitted)." [Cyriaque Rufin Nguimalet, Central African Republic]	Editorial – copyedit to be completed prior to publication
129165	38	33	38	35	[PRECISION] Yin et al. (2018) is referenced for trends in runoff extremes. However, in that article, extreme runoff is apparently defined as any runoff that has been separated from baseflow using baseflow separation. This is not extreme runoff. This is all runoff, which will be mostly small events. The term extreme is misleading here and is subject to misinterpretation by readers. This article shouldn't be used to show changes in extreme runoff/streamflow. [Trigg Talley, United States of America]	Not applicable. As chapter 11 provides a comprehensive assessment of flooding, the discussion of flooding in this section now just summarizes their assessment. No additional material is provided, for the sake of consistency.
96805	38	34	38	34	Most readers will not be familiar with the Clausius-Clapeyron relation. Please rephrase or add some words on the meaning with respect to runoff extremes. [Nicole Wilke, Germany]	Not applicable. This text has been removed.
2179	38	35	38	35	Please note that results opposing Yin et al (2018) were found in the following manuscript which precedes Yin et al (2018): Wasko, C., Sharma, A., 2017. Global assessment of flood and storm extremes with increased temperatures. Sci. Rep. 7, 7945. https://doi.org/10.1038/s41598-017-08481-1. Wasko and Sharma (2017) found predominantely negative sensitivties of flow with temperature for the 99th percentile across the world but positive sensitivities for more extreme events. The difference between studies has been attributed to the fact that Yin et al (2018) only analysed data up to a peak point temperature gauranteeing a positive sensitivity and only analysed runoff which is misleading as flooding could still be decreasing at this scale due to other factors such as changing antecedent conditions. See also the following which comments on the methodology of Yin et al (2018): Wasko, C., Sharma, A., Lettenmaier, D.P., 2019. Increases in temperature do not translate to increased flooding. Nat. Commun. 10, 5676. https://doi.org/10.1038/s41467-019-13612-5. [Conrad Wasko, Australia]	Not applicable. As chapter 11 provides a comprehensive assessment of flooding, the related discussion in this section is now just a summary of their discussion, and the text the reviewer comment refers to is no longer included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129167	38	35	38	37	[PRECISION] This section discusses how human watershed changes may affect flood flows but doesn't give any references. There is a highly relevant paper that could be discussed here (Hodgkins et al., 2019). It looks at the difference in peak-flow trends for >2500 catchments across the U.S., showing that different results are seen for relatively natural basins, urbanized basins, and basins with high amounts of reservoir regulation. Citation: Hodgkins, G.A., Dudley, R.W., Archfield, S.A., and Renard, B., 2019, Effects of climate, regulation, and urbanization on historical flood trends in the United States: Journal of Hydrology, v. 573, p. 697-709, https://doi.org/10.1016/j.jhydrol.2019.03.102. [Trigg Talley, United States of America]	Not applicable. As chapter 11 provides a comprehensive assessment of flooding, the discussion of flooding in this section now just summarizes their assessment. No additional material is provided, for the sake of consistency.
103703	38	37	38	38	Did Kundezewicz make a reference to anthropogenic influences to explain the year to year variability, or did he also refer to climate variability? [Philippe Tulkens, Belgium]	Not applicable. This text has been removed.
58967	38	40	38	43	Does the "significant decrease in natural (without dam) streamflow … because of glacier mass loss" pertain to a decrease in seasonal (e.g. summer) streamflow or annual streamflow? Specifying this would clarify whether the conclusion is primarily about a shift in seasonality or a decrease in annual mean streamflow (or equivalently annual total volume) - or a combination of the two possibilities. The Jiménez Cisneros et al. (2014a) (AR5) reference cited in the first paragraph of the section (8.3.1.5) (and supporting the conclusion here) considers observed changes in streamflow in snow/glacier-influenced basins only briefly. It seems to me that this reference makes the point that summer streamflow has decreased in observations where snow storage decline has occurred, but I wonder if there are any more recent references to consider to help refine this conclusion. (On an editorial note it would seem reasonable just refer to "streamflow" and not "streamflow values".) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not Applicable - The whole paragraph has been rephrased.
22287	38	40	38	46	Nowhere in the preceding text as far as I can tell is there anything that supports this contention. I would expect a significant discussion of this in the preceding text that would justify such a high confidence finding. The only portion of this finding which I feel the preceding text directly can justify is that on lines 46-49. It is critical that the assessment summary findings be underpinned by the preceding text and assessment summaries cannot be used to introduce new material. [Peter Thorne, Ireland]	Accepted. The summary has been rewritten to follow directly from the text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
109695	38	40	38	46	I'm really happy to see this passage here - I had to fight for something like this when I was an external reviewer for AR5, and even so any mention of these effects barely appeared in that report! That said, some references for this concept of peak water in glacier-fed rivers, and indeed for the entire idea that the impacts of climate change on glaciers affects downstream hydrologic cycles, would be very useful here - these facts have only very recently gained widespread recognition and acceptance in the watershed hydrology and climate science communities, yet are crucial for understanding the effects of climate change in many mountain regions worldwide. By the same token, as currently written this passage reflects a glaciologist's perspective, which is fine as far as it goes, but to meaningfully capture the hydrologic cycle implications (as per the topic of Chapter 8) of these glacier changes, a water resource science and engineering perspective and corresponding citations are also required. Specifically, adding something like the following would greatly improve the general credibility and relevance of this section: "While it was long recognized that glacial meltwater can be variable, with some potential to influence river flows, the first specific empirical evidence that climate change can deeply affect river basin-scale hydrologic cycles specifically by altering glacial melt production was provided in a paired-catchment study by Fleming and Clarke (2003) near the Yukon-Alaska border, which considered rivers with and without glaciers in a single hydroclimatic region experiencing uniform global anthropogenic climate changes. The concept of "peak water" was first suggested in a theoretical glacier modeling study in the Alps by Braun et al (2000); it was first hypothesized as a possible general principle by Jansson et al (2003); it was independently rediscovered and refined, and the first observational data-based empirical proof clearly demonstrating it at watershed scales was assembled, by Moore et al (2009) working in western Canada; and the first study to combine data and models to track the actual progression of it was Baraer et al (2012) working in the Andes. It has only recently become a common theme in understanding hydrologic cycle impacts of glacier change (Huss and Hock 2018). Moreover, the way that glaciers modify river runoff responses to intermediate-term variability, such as ENSO or PDO, change as glaciers retreat under longer-term global anthropogenic climate change (Fleming et al 2016). More broadly, to meaningfully capture the hydrologic cycle implications of	Not applicable. This text has been removed.
28995	38	40		49	The substantial discussion of extreme/high streamflow and flooding is not refelected in the summary which mostly mentions cryosphere-related aspects that are discussed relatively little [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The summary has been rewritten to follow directly from the text.
112221	38	40			I don't see how the summary is derived from the main body of text. The human withdrawal part is not captured but named as a key driver. [Rutger Hofste, Netherlands]	Accepted. The summary has been rewritten to follow directly from the text.
64955	38	41	38	42	can one specify which regions are meant by “some regions” [Johannes Quaas, Germany]	Not applicable. This text has been removed.
64957	38	43	38	43	Is it possible to quantify “many”? (e.g. as a percentage?) [Johannes Quaas, Germany]	Not applicable. This text has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58969	38	43	38	46	It is not clear to me that this point on increasing streamflow in some glacier runoff-dominated basins is substantiated by any evidence earlier in the section (it does not seem to be discussed or references given). The second sentence ("When this short-term increase ...") also appears more to do with the future (i.e. projections) than the observed water cycle changes that define this section of the chapter. Again, any statement of this nature probably needs to specify whether we are talking about changes in seasonality or annual mean/volume. Indeed, singling out glacier mass loss can sometimes give a misleading characterisation of the basin water balance and flow changes, especially if changes in the amount and (particularly) the fate of precipitation through altered runoff generation mechanisms are neglected. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not Applicable - The whole paragraph has been rephrased.
20497	38	45	38	46	Is this strong feature of the long-term climate system present in ESM projections? [philippe waldteufel, France]	Not applicable. This text has been removed.
4917	38	46	38	46	disappear -> disappears [Bart van den Hurk, Netherlands]	Not applicable. This text has been removed.
53301	38	46	38	49	In spite of increasing global mean precipitation over land, there is still low confidence in the sign of the overall human influence on annual mean global runoff, and medium confidence that (...). Observational and modelling uncertainties are still major obstacles for the detection of global runoff changes, despite growing evidence at the regional scale. [Hervé Douville, France]	Accepted. A discussion of the factors leading to uncertainty has been added.
21045	38	47	38	47	Chapter 3, page 3-28 says “medium confidence that antropogenic CC has altered local and regional streamflow”. Please coordinate and revise. [Marcelo Barreiro, Uruguay]	Accepted. Chapter 3's assessment has been incorporated.
22289	38	54	39	5	This paragraph should at least make a nod when discussing use of in-situ to calibrate satellite of the scale mis-match in that soil moisture likely varies considerably over very small scales and measurements following disturbance may not be representative? [Peter Thorne, Ireland]	Not applicable. The sentence has been removed.
74169	39	18	39	18	To be fair to microwave satellite soil moisture, it is correlated with deeper column water storage ["Landscape Water Storage and Subsurface Correlation From Satellite Surface Soil Moisture and Precipitation Observations" by Short Gianotti et al 2019 https://doi.org/10.1029/2019WR025332]. Could change to say, "A limitation of microwave remote sensing is that it provides estimates of only the surface soil moisture, although this is correlated with deeper column water storage." [Daniel J. Short Gianotti, United States of America]	Accepted - The text is changed accordingly
12811	39	18			What is "top" defined as what in terms of cm of soil depth for measuring moisture? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - "top" has been replaced by "near-surface" already defined as typically 0-10 cm in the previous paragraph
39905	39	18			“over the years 2000 to 2011-15”, it is confusing [TSU WGI, France]	Accepted - "2011-15" has been replaced with "2011"
12813	39	24			What is meant by this, "water withdrawn"? Would water abstraction be better? [If kept, change withdrawn to withdrawal.] [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - "withdrawn" was replaced by "withdrawal"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
81073	39	27	39	27	Soil moisture in the GLEAM product is not solely a calculated from a land surface model forced by meteorological observations. It directly assimilar ESA satellite based soil moisture directly into the surface layer. Other processes in the land surface model (e.g., vegetation stress) are also directly constrained by observations. GLEAM is therefore materially different from offline models like GLDAS. [Benjamin Cook, United States of America]	Not applicable - The sentence has been removed.
108105	39	28	39	28	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]	Accepted
113325	39	30	39	30	This paper is much more inclusive and specific than Balsamo et al. (2015), although still in discussions: Beck, H. E., van Dijk, A., Resources, D. M. W.2013: Global patterns in base flow index and recession based on streamflow observations from 3394 catchments, Wiley Online Library, doi:10.1002/2013WR013918. [Diego Miralles, Belgium]	Noted, but the paper has not been accepted so far.
113327	39	32	39	32	Why is and land-atmosphere coupling detrimental for the observational uncertainty? [Diego Miralles, Belgium]	Rejected - Because such "observational products" are based on off-line land surface models as explained earlier. The sentence has not been modified.
12815	39	32			Does this mean the lack of land-atmosphere coupling, or something else? Perhaps this sentence could be expanded. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This text has been removed.
22291	39	34	39	34	Despite their inherent limitations is too strong and anyway the overall ESM assessment is the charge of chapter 3 and not chapter 8 so at a minimum this needs to be recast as limitations in hydrological cycle and not more broadly. Text as it stands could be used to discredit swathes of report and absolutely must be changed accordingly. [Peter Thorne, Ireland]	Rejected - Fit-for-purpose is also in the scope of Ch8 (for water cycle processes), not only Ch3, and this part of the sentence refers to Section 8.5.1 (as explicitly stated via the parenthesis). Nevertheless, the statement is now associated with supporting references that have been shifted from the final summary of the section to this paragraph.
22293	39	34	39	49	This is a synthesis and assessment not a literature review so this needs to be better synthesised. [Peter Thorne, Ireland]	Noted. The text is being revised to better fit the goal of assessment
12817	39	40			No need for capitalisation of detection and attribution [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
7595	39	41	39	49	1/1: Now that I read the entire chapter 8, I strongly recommend to move this entire part to section 8.3.1.8 ("aridity and drought"). Indeed, the detectable anthropogenic trend during the first half of the century employs PDSI, which is a variable mainly based on PET. While some comparison between PDSI and SM could be made, PDSI is not a SM measurement, and I do not want to give the false impression that a anthropogenic SM signal (from GLEAM or MERRA-2) has been detected in Marvel et al. (2019). This would be wrong. In this section, you could just replace this section by : "Marvel et al. (2019) identified a possible externally-forced spatial signal of semi-global summertime surface and root-zone SM changes in CMIP5 models, but these simulated changes are NOT detectable above the background of internal climate variability in GLEAM or MERRA-2 soil moisture data over the observational period (1981-2017)." The other results will be discussed in section 8.3.1.8. [Celine Bonfils, United States of America]	Accepted. This text was reorganized accordingly in the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7597	39	47	39	49	Similarly, Bonfils et al. (submitted) should be moved in ection 8.3.1.8 ("aridity and drought"). The aridity index (CMI) is not a SM index. See comment #36. [Celine Bonfils, United States of America]	Accepted. This text was reorganized accordingly in the FGD.
27251	39	49	39	49	Should not Figure 8.11 be introduced here (i.e. before Figure 8.10)? [Eric Brun, France]	Rejected - Fig. 8.11 is discussed in Section 8.3.1.8 in the revised Section 8.3.1
129169	39	49			Figure 8.11 is not particularly instructive; the message must require reading the full paper. At the very least, (a) and (b) should be defined in the caption. [Trigg Talley, United States of America]	Accepted. This figure was replotted in the IPCC style in the FGD.
98059	39	51	39	54	The high confidence in this conclusion is not justified. First the statement is vague, making it difficult to objective evaluate: exactly what metric is the claim of detectable anthropogenic influence being made for, and what regions specifically? There is such a mix of variables and regions in the discussion that it's hard to link the claim up to anything concrete. The discussion above is focused on derived reanalysis measures (which is quite a level of abstraction from the real world soil moisture), off-line hydrologic model calculations, and PDSI-based analyses. The reliability of D&A claims based on off-line hydrologic models and PDSI-based work is questionable based on the critical analysis of such methods by Milly and Dunne (2016). The Marvel et al. D&A study (which has the empirical PDSI issue) also has a very peculiar D&A result that it is only during 1900-1949 that a robust GHG forced change is robustly detected. This is counter to the accelerated global warming rate seen from 1950 on, and leads me to question the overall reliability of the detection/attribution claim even for 1900-1949. The study is based on a highly derived view of drought as obtained from tree rings via the PDSI machinery—all higher levels of abstraction compared to actual soil moisture content. So I cannot conclude that the high confidence statement is justified. [Thomas Knutson, United States of America]	Taken into account - "high confidence" has been replaced by "medium confidence" and "multiple line of evidence" has been replaced by "multiple lines of evidence (including multiple metrics strongly related to soil moisture)"
21047	39	53	39	53	Delete "in" [Marcelo Barreiro, Uruguay]	Accepted - Thanks
113329	39	53	39	53	in in' [Diego Miralles, Belgium]	Accepted - Thanks
28997	39	53			Is high confidence in declining upper soil moisture warrented given the discussion above and the limitations of the observing system? Note also "in in" [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - "high confidence" has been replaced by "medium confidence"
43179	39	53			Read "of human activity in enhancing the dry or " rather than "of human activity in in enhancing the dry or " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted - Thanks
78859	40	1	41	21	There is wide overlapping in the information provided on changes in snow cover here and in Chapter 3. I suggest to avoid redundancy and overlapping in the two chapters and focus on effects of changes in snow cover on wate cycle. [MONICA TOLOTTI, Italy]	Noted - Overlaps have been removed
53303	40	3			What about merging glaciers and seasonal snow cover in a single subsection which should link to both CH9 and CH3 (Sections 3.4.2 and 3.4.3.1) and could first remind the reader about the relevance of the cryosphere to the water cycle, for instance as: "Glacier changes provide independent and reliable evidence of climate change. Glacier melting impacts the regional water cycle, but also increases local hazard risks (CH12) as well as the global mean sea level (CH9). Snow cover on the ground plays a significant role in the climate system due to its high albedo and heat insulation, but also contributes to the seasonality of soil moisture and runoff in the high latitudes." The hydrological consequences of earlier snowmelt on streamflow and floods could be however shifted to Section 8.3.1.5. [Hervé Douville, France]	Rejected: It is more appropriate to separate glaciers from snow cover in two separate subsections, 8.3.7.1.1 and 8. 3.7.1.2, since they are two distinct components of the cryosphere, even though both serve as fresh reservoirs. The contribution to basin hydrologic cycle (streamflow) by glaciers is only significant in heavily glacierized river basins, and river basins dominated by seasonal snow cover may or may not have large glaciers, which could take centuries or thousands of years to build up.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17215	40	4	40	4	Shrunk not shrinked [Joelle Joelle Gergis, Australia]	Accepted: Line 4 of Page 40 is revised to "most glaciers worldwide have shrunk over recent decades."
3979	40	4	40	4	shrunk (not shrinked) [Sabine Baumann, Germany]	Accepted: Line 4 of Page 40 is revised to "most glaciers worldwide have shrunk over recent decades."
185	40	4	40	4	"shrinked" is not a word. "have shrunken", "shrank" or "receded". [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Line 4 of Page 40 is revised to "most glaciers worldwide have shrunk over recent decades."
130533	40	4	40	6	Would the use of "almost ALL glaciers…" be too strong? In fact, there a few large glaciers in Tibet Plateau increased.In addition, shall we call "mass budget" or "mass balance"? [Panmao Zhai, China]	Accepted: Line 4 of Page 40 is revised to "most glaciers worldwide have shrunk over recent decades."
58949	40	4	40	21	Some recent articles published recently as part of the Special Issue of Front. Earth Sci. titled "Connecting Mountain Hydroclimate Through the American Cordilleras" may be included here. As an example I can suggest one study mapping the ice mass loss in Alaska, and another reviewing the current state and changes of the Andean cryosphere: 1) High Resolution Mapping of Ice Mass Loss in the Gulf of Alaska From Constrained Forward Modeling of GRACE Data by Doumbia, C., Castellazzi, P., Rousseau, A. N, and Amaya, M. (2019). Front. Earth Sci. 7:360. doi: 10.3389/feart.2019.00360. 2) A review of the current state and recent changes of the Andean cryosphere by M. Masiokas , A. Rabatel , A. Rivera , L. Ruiz , P. Pitte , J.L. Ceballos , G. Barcaza, A. Soruco, F. BownFront. Earth Sci., doi: 10.3389/feart.2020.00099 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected: No objection to add another reference or two about glacier shrinkage if the number of words is not an issue. On the other hand, since we have already cited quite a few recent papers about glacier mass loss (Marzeion et al., 2018; Watson et al., 2015; Zemp et al, 2019), there is no need to add more references at this stage, unless these references contain additional information about glacier mass loss. We should only cite a few key references in this AR6 report.
12819	40	4			Change shrinked to shrunk for the correct past participle. As in the 1980s film, Honey I Shrunk The Kids. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Line 4 of Page 40 is revised to "most glaciers worldwide have shrunk over recent decades."
89379	40	7	40	8	How did you get this number from the Zemp data? Is it just the cumulative mass change (-9625±7975 Gt) divided by the time period? [Robert McNabb, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Zemp et al. (2019) used an extrapolation of glaciological and geodetic observations to show that worldwide glaciers contributed 27 ± 22 millimetres to global mean sea-level rise (SLR) from 1961 to 2016. Given melting of about 360 Gt of ice will result in about 1 mm of SLR, this means that global glacier mass loss cumulated to about 171.9 +/- 142.4 Gt/yr over the period 1961 to 2016, e.g., 27 X 360/56 = 173 Gt/yr
4919	40	8	40	9	important to add a time range, as the relative contributions to SLR from Greenland is changing rapidly [Bart van den Hurk, Netherlands]	No longer applicable. Sentence has been removed from the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
187	40	10	40	10	Also Patagonia (SAN) is a major contributor (Zemp et al. 2019), greater than Alaska in terms of sea level contributions and with a more negative mass balance. See also Braun et al. 2019 (Nature Climate Change). [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Re-phrased Line 17-18 of Page 40 to, "The rate of total mass loss of glaciers in South America is estimated to be 19.4 ±18 0.6 Gt/yr based on surface elevation changes over the years 2000 to 2011–15 (Braun et al., 2019), which include the North and South Patagonian Icefields of South America". Glasser et al. (2011) estimated that the North Patagonian Icefield (NPI) of South America has lost 103±20.7 Gt of ice (0.0018±0.0004 mm yr−1) since 1870 and the South Patagonian Icefield (SPI) has lost 503±101.1 Gt of ice (0.0034±0.0007 mm yr−1) since 1650, which are about an order of magnitude lower than estimates of melting over recent years. Glasser et al., 2011, Global sea-level contribution from the Patagonian Icefields since the Little Ice Age maximum, Nature Geoscience, DOI: 10.1038/NGEO1122. However, it is unnecessary to cite Glasser et al. (2011) since we are more concerned with recent glacier mass loss.
64959	40	10	40	11	“would” – under which circumstances? [Johannes Quaas, Germany]	Accepted: Re-phrase Lines 9-11 of Page 40 to "glacierized regions such as Alaska, North Arctic Canada, and the Greenland periphery dominate the present glacier mass loss and are expected to continue contributing to sea-level rise beyond 2100"
341	40	11	40	12	It would be relevant to cite the recent study by Shean et al. (2020) that support the results of Brun et al. (2017) a way to illustrate the convergence between different estimates. Shean, D. E., Bhushan, S., Montesano, P., Rounce, D. R., Arendt, A. and Osmanoglu, B.: A Systematic, Regional Assessment of High Mountain Asia Glacier Mass Balance, Frontiers in Earth Science, 7, 363, doi:10.3389/feart.2019.00363, 2020. [Etienne Berthier, France]	Rejected: Another reference about glacier shrinkage could be added if the number of words is not an issue. However, since we have already cited quite a few recent papers about glacier mass loss (Marzeion et al., 2018; Watson et al., 2015; Zemp et al, 2019), we should avoid adding more references at this stage, unless these references contain additional information about glacier mass loss. We should only cite a few key references in this AR6 report.
27253	40	11	40	15	A very recent paper (Shean et al, 2020) confirms and extends the findings of Brun et al (2017): the mass loss of HMA glaciers between 2000 and 2018 is now -19.0 +/-2.5 Gt/yr - worth citing it - ref Shean DE and 5 others (2020) A systematic, regional assessment of high mountain Asia glacier mass balance. Frontiers in Earth Science. doi: 10.3389/feart.2019.00363 [Eric Brun, France]	Accepted - The reference was included
189	40	14	40	14	"Nyainqeunanghla" "kunlun" - needs context. These placenames won't be familiar to people. Where are they? [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Re-phrase Lines 13-15 of page 40 to "The most negative region-wide changes were found in Nyainqentanglha of the Tibet region of China with -4.0+/-1.5 Gt/yr, while glaciers in Kunlun of the northern Tibetan Plateau slightly gained mass at a rate of 1.4 +/- 0.8 Gt/yr".
3981	40	15	40	15	Maurer et al. (2019) [Sabine Baumann, Germany]	Accepted: revise Line 15 of Page 40: (Maurer et al., 2019) to Maurer et al. (2019)
12821	40	15			incorrect reference formatting [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - The reference was corrected.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3983	40	16	40	16	acceleration of mass loss: should this be expressed in negativ or positive numbers (it is negative in the text)? [Sabine Baumann, Germany]	Accepted: It is better to express in negative than positive numbers, since it should be clear that negative number represents mass loss.
88471	40	16	40	18	Other studies suggest higher mass loss in the area, for example Dussailant, Nat Geo, 2019 (-22.9+/-5.9 Gt yr-1 for 2000-2018) and Wouters et al, Frontiers, 2019 (-30.3+/-11Gt yr-1 for 2002-2016). [Bert Wouters, Netherlands]	Taken into acount. Assessment include these papers however we note that Dussailant et al. (2019) estimated the total mass change over 2000-2018 of −22.9 ± 5.9 Gt yr^{-1} was for the Andean glaciers, not for the Himalayan glaciers.
343	40	18	40	18	Same as above but this time to cite Dussaillant et al. that found values in agreement with Braun et al. 2019 for the entire Andes but studied a longer period (2000-2018). Dussaillant, I., Berthier, E., Brun, F., Masiokas, M., Hugonnet, R., Favier, V., Rabatel, A., Pitte, P. and Ruiz, L.: Two decades of glacier mass loss along the Andes, Nature Geoscience, 12(10), 802–808, doi:10.1038/s41561-019-0432-5, 2019. [Etienne Berthier, France]	Accepted: Re-phrase Lines 17-18 of Page 40 to "the rate of mass loss of glaciers in South America is estimated to be -19.4 ±18 0.6 Gt/yr based on surface elevation changes over the years 2000 to 2011–15 (Braun et al., 2019), and at −22.9 ± 5.9 Gt yr^{-1} over 2000-2018 by Dussailant et al. (2019)."
345	40	18	40	21	Thwaites glacier is part of the Antarctic Ice Sheet and should not be treated in the “glaciers” section but in the “ice sheet” section. If there is no “ice sheet” section then the title of the present section could be changed. But then it would strange to only mention one outlet glacier (Thwaites) here. [Etienne Berthier, France]	Rejected: Outlet glaciers of Greenland and the Antarctic have been regarded as glaciers instead as parts of the ice sheets. Ice discharge is primarily via a small number of ice streams and major outlet glaciers that calve into the surrounding oceans, directly in Greenland or mainly from ice shelves in Antarctica. For example, Moon, T. and Joughin, I. 2008. Changes in ice front position on Greenland's outlet glaciers from 1992 to 2007. J. Geophys. Res., 113, F02022, doi:10.1029/2007JF000927. Allen, C. et al. 1997. Airborne radio echo sounding of outlet glaciers in Greenland. Int. J. Re. Sens. 18(14): 3103-8.
88473	40	18	40	21	Previous text in this section only discusses mountain glaciers outside of the main ice sheets (Greenland/Antarctica). Thwaites Glacier is just one of the many glaciers on the ice sheets that exhibit a complex behaviour. Either remove this sentence or provide a more elaborate discussion of the ice sheets [Bert Wouters, Netherlands]	Rejected: Section 8.3.1.7.1 discusses glaciers across the world, including outlet glaciers of Greenland and Antarctic, even though outlet glaciers don't quite "qualify" as freshwater reservoirs or mountain glaciers as other glaciers. It is beyond the scope of this sub-section to discuss the two ice sheets.
191	40	19	40	19	Citing Thwaites Glacier isn't appropriate here because it's an ice stream of Antarctica, whilst this section is about glaciers. It is also not a freshwater reserve (it doesn't provide freshwater to downstream catchments). It's an important driver of sea level rise, but this is not the right section for it. [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: Ice streams tend to be smaller, can be ephemeral and bounded by slower moving ice while outlet glaciers tend to be larger and bounded by rock walls that flow through deep glacially eroded troughs (Chapter 4, Barry and Gan, 2011, Global Cryosphere, Past, Present and Future, Cambridge U Press). Section 8.3.1.7.1 discusses glaciers across the world, including outlet glaciers of Greenland and Antarctic, even though outlet glaciers don't quite "qualify" as freshwater reservoirs or mountain glaciers as other glaciers.
347	40	23	40	26	Regarding attribution studies, Marzeion et al. (2014) could be cited here I think (as in chapter 3). Marzeion, B., Cogley, J. G., Richter, K. and Parkes, D.: Attribution of global glacier mass loss to anthropogenic and natural causes, Science, 345(6199), 919–921, doi:10.1126/science.1254702, 2014. [Etienne Berthier, France]	Accepted: Re-phrase Line 25 of Page 40 to "forcing despite the lack of formal attribution studies such as Marzeion et al. (2014). See Ch3.4.3.1 and 3.5.3.2 for attribution of glacier changes."
68207	40	23	40	26	see also Ch3.4.3.1 and 3.5.3.2 for attribtuion of glacier changes [Guðfinna Aðalgeirsdóttir, Iceland]	Accepted: Re-phrasing Line 25 of Page 40 to "forcing despite the lack of formal attribution studies such as Marzeion et al. (2014). See Ch3.4.3.1 and 3.5.3.2 for attribution of glacier changes."

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21049	40	25	40	25	Please in the sentence “More details...” add also that section 3.4.3.1 in chapter 3 includes more details. [Marcelo Barreiro, Uruguay]	Accepted - The reference to CH3 was included
16129	40	26	40	26	Thank you for referring to Chapter 9. The corresponding section is now 9.5.1 (not 9.5.2 as in the FOD). [Gerhard Krinner, France]	Accepted: Re-phrase Lines 25-26 of Page 40 to "More details on this topic can be found in Chapter 9 (Section 9.5.1)."
14987	40	29	41	28	There is no mention to the changes in the Southern Hemisphere snow cover extent. If there is no information regarding this aspect, it should be clarified in the section. [Juan Rivera, Argentina]	Accepted: There are very few studies conducted about snow cover in Southern Hemisphere where snow cover is very limited compared to Northern Hemisphere. Add the following sentence in Line 28 of Page 41, "Saavedra et al. (2018) estimated the snow cover duration between 2000 and 2016 from Moderate Resolution Imaging Spectroradiometer (MODIS) satellite sensors in Andes. They found a large area (34,370 km2) with persistent snow cover over 29-36◦ S experienced 2–5 fewer days of snow year−1. Snow loss was more pronounced on the east side of the Andes, and the snowline elevation has increased at 10–30 m year−1 south of 29–30◦ S" Saavedra, F. A., Kampf, S. K., Fassnacht, S. R., and Sibold, J. S., 2018, Changes in Andes snow cover from MODIS data, 2000–2016, Cryosphere, 12, 1027–1046, doi.org/10.5194/tc-12-1027-2018
28999	40	29			Is snow depth dealt with also? Some papers to consider e.g. Fontrodona Bach et al. (2018) GRL find an average decrease in mean snow depth over Europe around -12%/decade since 1951, accelerating after the 1980s; Zeng et al. (2018) GRL: Observations indicate reduced annual maximum snow mass and shorter snow seasons since 1982 over parts of the USA with variability explained by temperature and accumulated winter precipitation; Siler et al. (2018) GRL argue natural changes in atmospheric circulation have offset most western-US snowpack loss relating to global warming since the 1980s; Snow Mass: Pulliainen et al. (2020) Nature https://doi.org/10.1038/s41586-020-2258-0: a recent reassessment of satellite data shows a seasonal snow mass decrease of 46 Gt/decade (4%/decade) across North America but negligible trends for Eurasia, with increases over East Siberia compensated by decreases over the Baltic region [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Snow depth is discussed in Lines 13-16 of Page 41, "Most studies show negative trends in snow depth and snow duration over past decades in the mountain cryosphere of Europe (Beniston et al., 2018a), with less pronounced changes at high elevations (Terzago et al., 2013). Considerable spatiotemporal variability of snow depth across the Eurasian Continent has been observed during 1966-2012 (Zhong et al., 2018)." Add the following sentence at the end of Lines 13-16, "A recent reassessment of satellite data shows a seasonal snow mass decrease of 46 Gt/decade (4%/decade) across North America but negligible for Eurasia, with increases over East Siberia compensated by decreases over the Baltic region (Pulliainen et al., 2020)", and re-phrase Line 23 of Page 41, "In summary, a decline in the Northern Hemisphere springtime snow cover, snow depth and snow duration has been observed,..." Pulliainen et al. (2020) Nature https://doi.org/10.1038/s41586-020-2258-0
53305	40	31			Should'nt changes in snowfall be assessed in the precipitation rather than snow cover subsection? [Hervé Douville, France]	Rejected: Snowfall is briefly discussed in Sub-section 8.3.1.7.2 on Snow cover because snowfall affects snow cover, snow depth and snow duration.
29001	40	31			A link back to Figure 8.1 and total wetland water volume (40 000 km3) would be useful. Is all the background necessary/policy relevant as the observed water cycle changes are the focus here? Discussion on stores could be removed with brief rererence to Section 1/Figure 8.1.Note also the total wetland area looks like 12.106 km2 rather than 12x10^6 km2 which matches Davidson et al. (2018) but the upper bound is Aires et al. (2017) quoted in Aires et al. (2018). (similar error on next page) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted- A link to Figure 8.1 was included. Any reference to impacts has been removed.
22295	40	33	40	35	Impacts are the domain of WG2 and should not be included in WG1. [Peter Thorne, Ireland]	Accepted - Impacts have been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12823	40	40			Change "in the context of" to "representing" [or, "ice core proxies of"] [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Re-phrase Line 40 of Page 40 to, "Antarctica during the late twentieth century was found from ice core proxies of the past 300 years"
17217	40	45	40	47	References needed to support this statement. [Joelle Joelle Gergis, Australia]	Accepted: A reference is added at the end of Lines 46-47 of Page 40, "a shift from snowmelt-dominated to rainfall-dominated flow regimes in some regions with consistent changes towards earlier timing of the flood peak (e.g., Bonsol et al., 2019)." Bonsal, B.R., Peters, D.L., Seglenieks, F., Rivera, A., and Berg, A. (2019): Changes in freshwater availability across Canada; Chapter 6 in Canada's Changing Climate Report, (ed.) E. Bush and D.S. Lemmen; Government of Canada, Ottawa, Ontario, p. 261–342
116735	40		40		Please refer to the assessment on glaciers in SROCC and ch 9 (avoid duplication) [Valerie Masson-Delmotte, France]	Taken into account: In 8.3.1.7, we merely summarize the observational and model evidence of glaciers, snow cover, wetland and lakes which are key components of freshwater reservoirs. However, we have also referred to more details given in Chapter 9 and SROCC (2019), as explained in our response to Comment ID 83893.
83893	40		41		(for 8.3..1.7.1 & 8.34.1.7.2). Address and cite relevant SROCC chapters and include in References (as e.g. in Ch9) [Ulf Molau, Sweden]	Accepted: rephrased Line 25-26 of Page 40 to "More details on this topic can be found in Chapter 9 (Section 9.5.2), and in SPM, Chapters 1 and 2 of IPCC (2019)." and rephrased Lines 27-28 of Page 41 to "A more comprehensive assessment of observed changes in seasonal snow cover can be found in Chapter 9 (Section 9.5.4), and SPM, Chapters 1 & 2 of IPCC (2019)." IPCC, 2019: Summary for Policymakers. In: IPCC Special Report on the Ocean and Cryosphere in a Changing Climate [H.-O. Pörtner, D.C. Roberts, V. Masson-Delmotte, P. Zhai, M. Tignor, E. Poloczanska, K. Mintenbeck, M. Nicolai, A. Okem, J. Petzold, B. Rama, N. Weyer (eds.)].
17219	41	1	41	10	Several grammar and editorial issues e.g. citation in line 1, line 6 just say In Canada there has been extensive declines… [Joelle Joelle Gergis, Australia]	Accepted: Revise Line 1 of Page 41 to "ablation rates showed decreasing trends." and Line 6 of Page 41 to "For Canada, there have been extensive decreasing snow depths and snow cover duration"
43181	41	1			Read "Santolaria Otin and Zolina (2019, submitted)" rather than "Santolaria Otin and Zolina (2019, submitted, Clim. Dyn)" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted: Revised Line 1 of Page 41 to "Santolaria Otin and Zolina (2019, submitted)" Updated to Santolaria-Otín, M. and O. Zolina, 2020: Evaluation of snow cover and snow water equivalent in the continental Arctic 51 in CMIP5 models. Climate Dynamics, 55(11), 2993–3016, doi:10.1007/s00382-020-05434-9.
12825	41	10			Should "is probably to decrease" say "probably due to a decrease"? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Revise Line 10 of Page 41 to, "streamflow, which will probably decrease in catchments that experience significant reductions in the fraction of precipitation falling as snow because of a warmer climate"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129171	41	18			What does "negative trends in recent years" mean? A trend must be defined over, say, decades. This sounds more like decadal-scale variability. [Trigg Talley, United States of America]	Accepted: Revise Line 17-18 of Page 41 to, "However, there were positive trends of maximum snow depth and SWE in higher and colder parts of the Fennoscandian Mountains, although overall it turns out to be negative trends in the 1978-2012 study period (Kivinen and Rasmus, 2015)."
22297	41	23	41	24	The justification for this attribution statement is lacking in the preceding text. The substantive assessment text must directly support the conclusion. [Peter Thorne, Ireland]	Accepted. The summary has been rewritten to follow directly from the text.
21051	41	23	41	24	Chapter 3, page 3-39 has a similar statement but with “very likely”. Please coordinate. [Marcelo Barreiro, Uruguay]	Accepted. The Chapter 3 assessment has now been included.
17221	41	23	41	28	This summary statement needs work. Suggest: In summary, a decline in spring Northern Hemisphere snow cover has been observed since the early satellite records from the late 1960s and has been formally attributed to a human influence (high confidence). There is low confidence in the main drivers of the snow cover variability on regional scales during autumn and winter due to high seasonal variability, although various regions exhibit a shortening of the snow cover season which is consistent with the observed warming. A more comprehensive assessment of observed changes in seasonal snow cover can be found in Chapter 9 (Section 9.5.4). [Joelle Joelle Gergis, Australia]	Accepted - This summary has been rephrased
58973	41	25	41	25	Does "snow cover evolution" refer to changes / trends? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: Revise Lines 25-26 of Page 41 to "There is only low confidence in the main drivers of snow cover changes at the regional scale and during the fall and winter seasons"
20499	41	27	41	28	While there is no section 9.5.4 in this SOD, section 9.5.3 exists and is relevant! About one third of references quoted in 8.3.1.7.2 are found again quoted in Chapter 9. While admittedly passages in both chapters differ, it is not very easy to perceive how and why choices are operated. [philippe waldteufel, France]	Response: Section 8.3.1.7.2 discusses observed seasonal snow cover changes primarily of Northern Hemisphere obtained from satellite data since the late 1960s over about 1,000 words. Even though the focus of Section 9.5.3 is also seasonal snow cover, the discussion is about 3 times as extensive (about 3000 words) covering snow cover extent and duration, snow mass and depth, model evaluation, and projections. We expect some common references cited between both sections.
16127	41	28	41	28	Thank you for referring to Chapter 9. The corresponding section is now 9.5.3 (not 9.5.4 as in the FOD). [Gerhard Krinner, France]	Accepted: Revise Line 28 of Page 41 to "of observed changes in seasonal snow cover can be found in Chapter 9 (Section 9.5.3)."
22299	41	34	41	34	This should have a link to chapter 5, surely, where a substantive assessment is carried out. [Peter Thorne, Ireland]	Taken into account. Included link to chapter 5 {5.2.2}.
29169	41	35	41	35	Add a reference to Zhang et al. : (e.g. Saunois et al. 2016, Zhang et al., 2017). Corresponding reference: Zhang, Z., Zimmermann, N. E., Stenke, A., Li, X., Hodson, E. L., Zhu, G., ... & Poulter, B. (2017). Emerging role of wetland methane emissions in driving 21st century climate change. Proceedings of the National Academy of Sciences, 114(36), 9647-9652. [Catherine Prigent, France]	Thank you. This reference is added.
13537	41	40		41	Although the efforts by the countries seek to integrate inventories with a solid base trough the quantification of the surface and volume of inlands waters to best cover the dynamic rate of variation to make possible the change detection. It is necessary taht each contry increases and allocates greater interest and financial resources to expedite the systematization of inlands water inventories. [Maria Amparo Martinez Arroyo, Mexico]	Noted. This comment is related to mitigation and beyond the scope of this section.
29171	41	47	41	47	replace 'merging' by 'combining'. [Catherine Prigent, France]	Accepted. The modification is included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29175	41	49	41	50	Change 'Inter-comparison studies suggest a good consistency between the different estimates (Aires et al., 2018; Davidson et al., 2018)' with 'Inter-comparison studies try to reconcile the different estimates (Hu et al., 2017; Aires et al., 2018; Davidson et al., 2018)' [Catherine Prigent, France]	Accepted. The modification is included.
4921	41	52	41	52	adjust exponential notation [Bart van den Hurk, Netherlands]	Taken into account. Typo is corrected. Million is used for the exponential 10^6.
74379	41	52	41	52	for the expression "Most recent multisattelite" Need to precise which satellite [Moulay Driss HASNAOUI, Morocco]	Taken into account. Several satellites are used to provide the full time series. Difficult to name them all. We have mentioned the types of observations: 'Most recent multi-satellite (visible, infrared, and microwaves) products…'
22301	41	52	41	55	I assume you mean 12106-14106km squared. And 3106 km squared. Why the decimal place? Why the undue precision given the tilde assigning an approximation? Why not 12-14k km2 and 3k km2 which would be more justifiable? I really do not understand what these numbers are or their justification as presently written so would suggest edits for clarity. [Peter Thorne, Ireland]	Taken into account. Typo is corrected. The numbers should read 12x10^6, 14x10^6, 3x10^6 (with 10^6 meaning 1000000). Million is used for the exponential 10^6.
13539	41	52		53	Just as the estimated surfaced has been mentioned, a current estimate of the volume of inland waters could also be included in a worldwide estimated quantity. [Maria Amparo Martinez Arroyo, Mexico]	Noted. To my knowledge, direct quantification of inland water volume and related changes at the global scale is not available with enough accuracy to be reported here.
13541	42	2		3	Use the next text to complete the sentence: "However the Ramsar sites are not evenly distributed in many contries. Although there is a great effort by country regions to account for freshwater areas, the processes to decree new areas can be slow due to administrative issues and a lack research about to the possible and new Ramsar sites. [Maria Amparo Martinez Arroyo, Mexico]	Taken into account. This comment is related to mitigation and not in the scope of this section. The text is suitably modified.
29173	42	7	42	7	reference to be changed: Prigent et al., 2020. Prigent, C., Jimenez, C., & Bousquet, P. (2020). Satellite-derived global surface water extent and dynamics over the last 25 years (GIEMS-2). Journal of Geophysical Research: Atmospheres, 125, e2019JD030711. https://doi.org/10.1029/2019JD030711 [Catherine Prigent, France]	The Prigent et al. 2020 reference is included.
12827	42	7	42	8	Works in preparation or to be submitted should not have been included in the SOD. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The Prigent et al. 2020 paper is published. Reference: Prigent, C., Jimenez, C., & Bousquet, P. (2020). Satellite-derived global surface water extent and dynamics over the last 25 years (GIEMS-2). Journal of Geophysical Research: Atmospheres, 125, e2019JD030711. https://doi.org/10.1029/2019JD030711
29003	42	10			"man-made" --> "human-made" or "artificial" [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Corrected as 'human-made'
103705	42	11	42	13	Only natural water bodies areas have disappeared or also human made water bodies? [Philippe Tulkens, Belgium]	Noted. Satellite imagery over the last 30 years tends to show that the surface water has increased globally (not decreased), with the loss of water bodies in some regions, compensated by the creation of water bodies (mostly human-made reservoirs) , in other parts of the globe.
22303	42	14	42	14	Again, what is this number and is the precision justified? [Peter Thorne, Ireland]	Taken into account. Typo is corrected. Million is used for the exponential 10^6.
4923	42	14	42	14	adjust exponential notation [Bart van den Hurk, Netherlands]	Taken into account. Typo is corrected. Million is used for the exponential 10^6.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64961	42	19	42	20	Why is there only “high confidence”? is this not a known fact? [Johannes Quaas, Germany]	Noted. Global assessment of dams and water reservoirs is not trivial, with development of new dams and drying of others. There is a high confidence of increased human-made water surfaces, but it is not obvious. It is not just a known fact.
22305	42	20	42	22	I did not get this message clearly from the underlying assessment text that preceded this summary. If this is the finding the text needs to be revised to far better support it. [Peter Thorne, Ireland]	Taken into account. With the ambiguities suppressed on the provided surface extent (see comments above from the same reviewer and the corresponding responses), the message should be now much clearer.
17223	42	25	42	26	Grammar – groundwater not groundwaters. Amed to read: Water cycle changes affect groundwater [Joelle Joelle Gergis, Australia]	Corrected
17225	42	25	43	40	Grammar is poor in this section. Needs to be carefully edited by a native English speaker for clarity. [Joelle Joelle Gergis, Australia]	Accepted. Entire section has been rewritten by a native speaker.
5575	42	25	43	40	There are no informations about the groundwater evolutiion in the Mediterranean zone, why ? The piezometric level and the groudwater resource in this area decreases dramatically. [Benoit Laignel, France]	Accepted. Text has been revised and accounts for recent reductions in groundwater levels in the southern Mediterranean coastal areas,
51955	42	25	43	40	Replace all text in this section with this: "Water cycle changes directly affect groundwater through regional changes in the frequency and intensity of precipitation, and meltwater regimes from glaciers and seasonal snow cover that alter the magnitude and timing of groundwater recharge. Attribution of changes in groundwater storage, observed locally through piezometry (Taylor et al., 2013b) or at regional scales (> 100 000 km2) estimated from GRACE satellite measurements (Rodell et al., 2018) (Fig 8.10), is often complicated by non-climate influences on terrestrial water budgets that include land-use change (Favreau et al., 2009) and human withdrawals. As the world’s largest distributed store of freshwater (Taylor et al., 2013b), groundwater is estimated to supply between a quarter and a third of the world’s annual freshwater withdrawals to meet agricultural, industrial and domestic demands (Döll et al., 2012; Wada et al., 2014; Hanasaki et al., 2018).. Following a global review of groundwater and climate change by Taylor et al. (2013b) evidence from piezometric observations of an association between heavy or statistically extreme precipitation and groundwater recharge has continued to grow, especially in tropical (Asoka et al., 2018; Kotchoni et al., 2019; Cuthbert et al., 2019) and sub-tropical regions (Meixner et al., 2016a). Stable-isotope ratios of O and H at 15 sites across the tropics trace groundwater recharge to intensive monthly rainfall, commonly exceeding the ~70th intensity decile (Jasechko and Taylor, 2015). Further, heavy rainfall recharging groundwater resources is often associated with large-scale controls on climate variability such as El Niño Southern Oscillation and Pacific Decadal Oscillation (Taylor et al., 2013c; Kuss and Gurdak, 2014; Asoka et al., 2017; Cuthbert et al., 2019; Kolusu et al., 2019). Shamsudduha and Taylor (2020) additionally show that increases in groundwater storage estimated from GRACE for 37 of the world’s large-scale aquifer systems from 2002 to 2016 commonly result from episodic recharge associated with extreme (>90th percentile) annual precipitation. Rising global freshwater withdrawals, primarily associated with the expansion of irrigated agriculture in drylands, have led to groundwater depletion that has been computed globally range for the year 2000 from ~100 and ~300 km3 yr-1 from hydrological models and volumetric-	Accepted with thanks.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53307	42	26	42	30	Assess the reliability of the available observations and models before moving on to the attribution and, first, remind the reader about why groundwaters are an important component of the water cycle: "Groundwater counts in average for one third of the freshwater consumed by humans, but at some parts of the world, this percentage can reach up to 100%. Climate change affects groundwater recharge rates through changes in precipitation and evapotranspiration. However, attributing observed groundwater change to climate change is difficult because of the influence of land-use change and groundwater abstraction. Climate change can also affect groundwater through saltwater intrusion in coastal aquifers as sea level rises (cf. CH9)." [Hervé Douville, France]	Accepted. Revised text clearly addresses both the importance of groundwater withdrawals to humanity, evidence of changes in groundwater from observations and models, and the risk of seawater intrusion from excessive pumping.
22307	42	32	42	39	How is attribution complicated? Human abstraction is as unambiguous as it gets. If you mean attribution of climate change related changes in groundwater then state this explicitly and clarify intent in this paragraph. But that humans are responsible (the attribution problem) is really clear for groundwater, particularly fossil groundwater. The paragraph could also better delineate between modern and fossil groundwater. [Peter Thorne, Ireland]	Accepted. Revised text clearly articulates the link between human withdrawals and groundwater depletion.
112223	42	36			Arent't ice and glaciers the largest store? [Rutger Hofste, Netherlands]	Accepted. The revised text argues that groundwater is the world's largest distributed store of freshwater; freshwater storage from ice is greater (~double) but resides almost entirely in the ice sheets of Antarctica and Greenland, remote from human habitation.
129173	42	37	42	39	""... groundwaters supply substantial proportions of the estimated water used for ..."" Construction of sentence is awkward (not estimated water). Consider striking ""estimated"". ""They are therefore narrowly intertwined ..."" Also somewhat awkward. Consider ""They are therefore interconnected ..."" [Trigg Talley, United States of America]	Accepted and resolved clearly in revised text.
53309	42	38	42	39	May be add that: "Global total natural (not accounting for human withdrawals) groundwater storage was found to decrease over the past 5–7 decades with modeled rates ranging from 0.01 to 2.18 mm year−1 due to both natural climate variability and climate change (Liu et al., 2019)." [Hervé Douville, France]	Taken into account. Estimated declining trends in groundwater storage, independent of human withdrawals, are very small, generally less than 5 mm year-1, relative to those estimated associated with human withdrawals, generally > 20 mm year-1 (Herbert and Döll, 2019. Water Resour. Res. 55, 4760–4784).
112225	42	41			Consider adding other studies such as https://www.geosci-model-dev.net/11/2429/2018/ instead of just Döll et al., 2012 [Rutger Hofste, Netherlands]	Taken into account. It does not make a material difference to percentage values quoted – section 7.2.4 of Müller Schmied et al. (2021) Geosci. Model Dev. 14, 1037–1079 states: “The general patterns of potential net abstractions (Fig. 15a and b) are consistent with the earlier assessment of Döll et al. (2012).”
129175	42	41			"... large groundwater footprint ..." What is a groundwater footprint? Also, authors might want to cite the following for a large scale investigation of groundwater depletion in the United States: http://pubs.usgs.gov/sir/2013/5079 [Trigg Talley, United States of America]	Accepted. Revised text clearly articulates what is meant by use of the word "footprint" as this relates to the use of GRACE satellite data which records changes in terrestrial water storage at a scale of between 100 000 and 200 000 square kilometres; groundwater depletion in the continental US in the California Central Valley and High Plains aquifers occurs below this "footprint" as deduced from piezometry.
38353	42	45	42	47	This section lists the data on groundwater depletion from 2000 to 2010, which comes to a conclusion of the rise in depletion rates in India (23%), United States (31%), and China (102%). The conclusion, which does not reflect the groundwater depletion, is misleading. It is suggested that data be supplemented or the absolute value of the groundwater depletion be substituted, in order to increase readability. [Yaming LIU, China]	Accepted. The revised text provides estimates of depletion that derive updated analyses.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58975	42	46	42	46	Presumably the km3 figures quoted here should really be rates, i.e. km3 year-1, as in the remainder of the paragraph. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The revised text clearly specifies depletion in cubic kilometres is an annual rate.
58977	42	48	42	54	There is a mixture of mm year-1 and cm year-1 units used and it may be faster to interpret if just one or the other is retained. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The revised text uses volumetric units more consistently.
129177	42	51	42	52	""… was estimated to …"" might be better worded as ""… was estimated as …"" ""… consistent with ground monitoring well observations …"" should be ""… consistent with groundwater monitoring well observations …"" [Trigg Talley, United States of America]	Accepted. Revised text makes these distinctions clear.
20501	42	52	42	55	The repetition of 106 km2 within all 3 area figures casts some doubt. Doubt increases after checking that, for example, the estimated area for lake Baikal is 31722 km2. Would a factor 1000 be missing in these numbers? [philippe waldteufel, France]	Accepted. The specifically quoted figure is no longer used in this section.
116737	42		42		There are aspects on wetlands and lakes in SRCCL and SROCC which could be used here too. [Valerie Masson-Delmotte, France]	Accepted - The SRCCL and SROCC have been checked, to verify that the current section is compatible with these reports. For instance, the loss of natural wetland is discussed in section 4.3.1 of SRCCL (process of land degradation), based on inventories (e.g., Davidson et al.) that are also considered in the current section.
129179	43	1	43	13	Is this paragraph about research on the association between heavy precipitation and groundwater recharge (like the lead sentence), about global recharge estimates, or about regional recharge estimates? Line 1 is difficult to understand (evidences?). [Trigg Talley, United States of America]	Accepted. The revised discusses each of these issues in separate paragraphs.
12829	43	1			Incorrect reference formatting. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and addressed.
43183	43	1			Read " by Taylor et al. (2013b) " rather than " by (Taylor et al., 2013b) " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted - Done
4925	43	2	43	3	Unclear sentence. In what direction is the association between heavy precipitation and recharge changing? [Bart van den Hurk, Netherlands]	Accepted. The revised text clarifies the direction of change.
113331	43	10	43	10	Jasechko and Taylor, (2015)' for 'Jasechko and Taylor (2015),' [Diego Miralles, Belgium]	Accepted - Done
22309	43	11	43	11	Surely there are only 10 deciles so its either the 7th decile or the 70th percentile? [Peter Thorne, Ireland]	Accepted. Revised text is"70th percentile".
4927	43	11	43	13	But earlier evidence showed that increase in fraction of rainfall as heavy precip is increasing runoff as well, so contradicting rainfall availability for groundwater recharge [Bart van den Hurk, Netherlands]	Accepted and addressed. It is logical query but there are two challenges to the deduction: (1) increased runoff does not necessarily come at the cost of reduced recharge (e.g. focused recharge); and (2) infiltration rates overlying many aquifers are observed to be much more rapid than conventional, pore-matrix flow conceptualisations defined by the Darcy-Richards equation (e.g. Beven and German, 2013; Water Resources Research, Vol. 49, 3071–3092.)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58951	43	15	43	17	An interesting global assessment of the contribution of nonrenewable groundwater abstraction to irrigation (Wada et al 2012) could be included here. Reference: Wada, Y., L. P. H. van Beek, and M. F. P. Bierkens (2012), Nonsustainable groundwater sustaining irrigation: A global assessment, Water Resour. Res., 48, W00L06, doi:10.1029/2011WR010562 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Indeed, the revised text refers to an even more recent study (Bierkens, M. F. P., and Wada, Y., 2019. Non-renewable groundwater use and groundwater depletion: a review. Environmental Research Letters, Vol. 14, 063002.)
129181	43	15	43	17	This short paragraph seems out of place, with little to no evidence for the opening conclusion. What does the pumping rate in the Central Valley during 2006-10 have to do with the first sentence? Was this during a drought? Was this higher than during other times? [Trigg Talley, United States of America]	Accepted. The revised text contextualises well the discussion of groundwater depletion in California.
59015	43	19	43	24	Refer to most recent literature review from Hayashi, M. (2019). Alpine Hydrogeology: The Critical Role of Ground- water in Sourcing the Headwaters of the World. Groundwater. doi: 10.1111/gwat.12965 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference included in discussion of climate change impacts on recharge supplied ice and snow meltwaters.
109693	43	19	43	26	Note also, however, that in regions with heavy glacial ice cover where climate change has increased ice melt fluxes, groundwater recharge and river baseflow have increased, which is further reinforced by permafrost melting (Fleming, 2006, Impacts of climatic trends upon groundwater resources, aquifer-stream interactions and aquatic habitat availability in glacierized watersheds, Yukon Territory, Canada, In: Glacier Science and Environmental Change (ed. P.G. Knight), Blackwell, UK, pp. 151-152.) [Sean Fleming, United States of America]	Accepted. The revised text details well the impact of climate change on changing glacial ice and snow cover on groundwater recharge, citing papers published from 2009 to 2018.
43185	43	21			Read "to an earlier spring melt, and (2) low groundwater levels" rather than "to an earlier spring melt (2) low groundwater levels" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted and addressed in the revised text.
58995	43	24	43	25	This sentence needs a citation. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text has been thoroughly revised with reference to consulted studies.
59003	43	24	43	26	Give suitable references regarding coupling of sw and gw, also mention the shift from a sw-dominated system to a groundwater-dominated system (Lamontagne-Halle et al., 2018; Lemieux et al., 2020). L. 26: give more recent references (e.g., Walvoord and Kurylyk, 2016, doi:10.2136/vzj2016.01.0010) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text has been revised with reference to these consulted studies.
5577	43	29	43	31	The sea water intrusion into coastal aquifer depends of coastal topography, recharge and goruwater abstraction, not only, but also of the sea level rise. It results of the combining of several factors, continental and marine. [Benoit Laignel, France]	Accepted. Text has been thoroughly revised and accounts for these factors.
103707	43	32	43	35	This study claims SLR and reduced fresh watyer runoff has a larger influence on salt intrusion than cyclones: that https://www.mdpi.com/2225-1154/7/5/69 [Philippe Tulkens, Belgium]	Accepted. Text revised.
22311	43	32	43	35	What is the peer reviewed basis for this? [Peter Thorne, Ireland]	Accepted. Text revised and rooted in peer review.
20503	43	37	43	40	Since groundwaters are assessed by IPCC to have been depleting over decades, one wonders how much time it would take at present net depletion rate for groundwaters to disappear! From Figure 8.1, the estimated stock is about 630 000 km3 and recharge (13000/yr)) is larger than discharge (4500/yr). In this chapter 8 12000-15000 figures are quoted for recharge, but no total discharge figure is proposed and the beautiful GRACE Figure 8.10 does not help. What is the reader to conclude? [philippe waldteufel, France]	Taken into account. The text and concluding statements of this section have been thoroughly revised. The assertion that global-scale depletion has been occurring for decades is no longer argued. Indeed, groundwater depletion is largely confined to groundwater withdrawals for irrigation in some of world's most productive agricultural regions challenged; such depletion is more localised than earlier studies have argued.
53311	43	37	43	40	and to persistent model deficiencies in simulating present-day groundwater recharge processes and equilibrium? [Hervé Douville, France]	Accepted. This argument has been incorporated into the revised text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59017	43	37	43	40	Please do not use across the globe. In permafrost regions, for instance, groundwater recharge has increased (e.g., Walvoord and Kurylyk, 2016, doi:10.2136/vzj2016.01.0010) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The text and concluding statements of this section have been thoroughly revised. The assertion that reductions in recharge are global is no longer argued.
22313	43	39	43	39	Attribution of these changes directly to climate change is still challenging … [Peter Thorne, Ireland]	Accepted. However, the balance of evidence no longer challenges the intensification of precipitation to warming but whether or not the magnitude of this intensification follows the Clausius-Clapeyron relation.
4929	43	39	43	39	I assume the word "increased" is missing in this sentence [Bart van den Hurk, Netherlands]	Accepted. This point made clear in the revised text.
58979	43	39	43	39	Would "net groundwater recharge" or "increases in groundwater storage" be better than "groundwater recharge" here to convey the time scales / aggregate picture implied by this conclusion? Also, parts of India, China and North Africa could be considered subtropical, so there is a little bit of conflict with the general nature of the preceding sentence. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. This point made clear in the revised text.
129183	43	39			"… groundwater recharge has been observed in tropical ..." Do authors mean "… has been observed to increase ..."? [Trigg Talley, United States of America]	Accepted. This point made clear in the revised text.
129185	43	45			Figure 8.10 is discussed after Figure 8.11. [Trigg Talley, United States of America]	Thank you. This problem is resolved in the FGD
113333	44	1	44	1	Correct use of m-dash and hyphen for time ranges in this section. Use n-dash consistently. [Diego Miralles, Belgium]	Accepted - Fixed.
20505	44	1	45	42	Is there a reason why "aridity" is mentioned in the title of this subsection? The word "aridity" never appears in the text. [philippe waldteufel, France]	Noted. The soil moisture and drought sections were combined in the FGD and soil moisture is related to aridity.
132225	44	1	45	42	It is confusing to have this short text on observed changes in aridity and drought without any reference to chapter 11 which includes the main assessment on drought. A better coordination with chapter 11 is required. This text should be either fully coordinated with chapter 11 or removed. Based on pre-LAM discussions, we suggest to have a cross-chapter team working on the drought assessment, coordinated by chapter 11 and including contributors from chapters 12 and 8 (and possibly also the atlas). [Sonia Seneviratne, Switzerland]	Taken into account. All drought/aridity assessments have been cross-checked with Chapter 11 for the final draft.
113347	44	1	45	42	I see no explicit mention of European droughts (2003, 2006, 2010, 2015, 2018…) in this section. In terms of frequency of large anomalies, Europe has been a hotspot for droughts over the past two decades. And I do not mean the Mediterranean but rather central Europe. There is plenty of literature to cite on the topic... [Diego Miralles, Belgium]	Noted. Detailed regional assessments may be found in Ch. 11 and 12.
22317	44	1			I found this section 8.3.1.8 confusing. In particular there seemed to be gross inconsistencies in findings of droughts in N. America that were not squared between earlier and later paragraphs. The section also does not follow standard drought type nomenclature as outlined earlier in the chapter. It would be better to consider meteorological, agricultural and hydrological droughts and do so using those terms in turn and call out explicitly the differences where they arise between them for given regions e.g. N. America and discuss why. [Peter Thorne, Ireland]	Noted. The North American text was edited for clarity and consistency in the FGD.
129187	44	1			In 8.3.1.8 (Aridity and Drought), there are only discussions on drought (extreme event) but not at all on aridity (mean state change) although the latter is in the title. There are many papers since AR5 on the terrestrial aridity change which have been totally ignored in this report. [Trigg Talley, United States of America]	Noted. The soil moisture and drought sections were combined in later drafts including the FGD

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10161	44	3	45	42	There is another long evaluation of this exact same question (attribution of recent drought trends) in Chapter 11 (Section 11.6.4), that is framed quite differently and, unfortunately comes to rather different conclusions than this section. Ideally, these two sections should be harmonized and written together so the report is not self-contradictory. I strongly prefer the framework in Section 11.6.4, that makes it clear what exact physical quantity is meant by "drought" in each study (as different authors actually mean quite different physical metrics by the word "drought".) [Jacob Scheff, United States of America]	Taken into account. For the final draft, the text has been harmonized with Chapter 11 through cross-chapter coordination.
42765	44	5	44	7	"The science of detection and attribution has progressed considerably since then, especially in the area of extreme or singular event attribution (Trenberth et al., 2015; Easterling et al., 2016; Stott et al., 2016)";. This statement is obviously true, but it should appear beforehand, because the detection challenge does not hold for aridity and drought only; it is a fairly general challenge when analysing the various components of the water cycle. The editors could thus consider to add a specific Challenges sub-section to 8.1 (introduction) dealing more specifically with the issues of detection and attribution from a water cycle perspective; alternatively it could appear as a conclusion of section 8.2, which details all the physical processes involved in linking atmospheric warming and water cycle changes [Thierry Lebel, France]	Noted, however detection/attribution has in particular advanced the assessment of drought which is why it is explicitly mentioned here.
22315	44	5	44	12	This aspect is covered in depth in chapter 11. This text should be shortened accordingly with a forward throw instead given to the relevant chapter 11 section to avoid inviting the reader to play spot the difference? [Peter Thorne, Ireland]	Noted. The final FGD text was altered to align with chapter 11, in consultation with the authors of that chapter.
10163	44	11	44	12	The Griffin and Anchukaitis (2014) study is suspect because its finding of "unprecedented" recent drought in California was based not on a physical variable like precipitation or tree growth, but on an offline calculation of the PDSI from NOAA that used Thornthwaite PET, which is known to be far too sensitive to greenhouse warming. More recent studies on the same question, using updated versions of the same offline approach and better methods like Penman-Monteith or direct soil moisture modeling, do not find that this event was unprecedented (Williams et al 2015 already cited; Williams et al 2020 Science doi.org/10.1126/science.aaz9600) though they do find that it was unusual. Furthermore, even those more recent studies do not attempt to account for the reductions in ET and PET due to plant physiological effects (e.g. Yang et al. 2018d) which may make the recent event even less "unusual" by canceling even more of the warming->PET effect that made it appear so unusual. (All of these studies agree that the 2012-2014 event was not quite so unusual in terms of precipitation alone - but that's not clear from the current text.) [Jacob Scheff, United States of America]	Noted. Phrasing has been changed to "unusual" rather than "unprecedented", as unusual was confirmed by Williams et al. 2015. Williams et al. reference has been added here.
7599	44	13	44	13	1/2: Before discussing the regional results, the global results from Marvel et al. (2019) and Bonfils et al. (submitted) should be discussed here. You could add: "Marvel et al. (2019) identified externally-forced spatial patterns of semi-global summertime changes in Palmer Drought Severity Index (PDSI) in CMIP5 models. Using this information with instrumental, modeled, and tree-ring reconstructions, they determined that an anthropogenic trend is detectable in the first half of the twentieth century, but not over the more recent decades (1981-2017)." A discussion for Bonfils et al. (submitted) which is currently missing can then be included right after (see next comment). [Celine Bonfils, United States of America]	Accepted. This text was reorganized accordingly in the FGD.

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7601	44	13	44	13	2/2: Here is the discussion of Bonfils et al. (submitted) and of Figure 8.11 (left column) that should be included here, or in section 8.3.1.3 since it discussed both the changes in rainfall and aridity": "Bonfils et al. (submitted) investigated the simultaneous changes in precipitation, temperature and continental aridity in CMIP5 historical simulations and found, as a main signal, quasi-global human-induced changes characterized by a global warming, an amplification of rainfall wet-dry latitudinal patterns, and a progressive continental aridification (Figure 8.11, left column). These anthropogenic changes are statistically detectable in reanalyses over the 1950-2014 period with high confidence (extremely likely; S/N>1.96). These observed changes are cannot be explained by climate noise alone, but are largely driven by multi-decadal increases in greenhouse gas emissions, partially masked by the effect of anthropogenic aerosols." Note: Bonfils et al. (submitted) is not discussed in section 3.3.2.1 (section on precipitation), no in section 8.3.1.3 ("Precipitation"). [Celine Bonfils, United States of America]	Accepted. This text was reorganized accordingly in the FGD.
8675	44	14	44	24	The comment on precipitation trends it is inconsistent with recent studies that do not show long-term precipitation trends in the region, so I would not give high confidence to the precipitation changes in the Mediterranean. See Ch. 11 (page 80 1-6). There are also regional long-term studies that do not find long-term trends over the period stated in Italy, Spain, Portugal and the East of the Mediterranean (Brunetti et al., 2006; Camuffo et al., 2013; Esteban-Parra et al., 1998; Reiser and Kutiel, 2011; González-Rouco et al., 2001; Paulo et al., 2016; Prohom et al., 2016) but also precipitation reconstructions show similar signal (Hanel et al., 2018) . There is an increase in the severity of droughts but this is based on the increased trend of the atmopsheric evaporative demand (e.g. Vicente-Serrano et al. 2014 Env Res Lett; Stagge et al. 2017 Sci Reports) but not based on changes in precipitation. This comment is also valid for P74-50. Hanel M, Rakovec O, Markonis Y, Máca P, Samaniego L, Kyselý J, Kumar R. 2018. Revisiting the recent European droughts from a long-term perspective. Scientific Reports 8(1). DOI: 10.1038/s41598-018-27464-4. Paulo A, Martins D, Pereira LS. 2016. Influence of Precipitation Changes on the SPI and Related Drought Severity. An Analysis Using Long-Term Data Series. Water Resources Management, 5737–5757. González-Rouco JF, Jiménez JL, Quesada V, Valero F. 2001. Quality control and homogeneity of precipitation data in the southwest of Europe. Journal of Climate 14(5): 964–978. DOI: 10.1175/1520-0442(2001)014<0964:QCAHOP>2.0.CO;2. Reiser H, Kutiel H. 2011. Rainfall uncertainty in the Mediterranean: Time series, uncertainty, and extreme events. Theoretical and Applied Climatology 104(3–4): 357–375. Esteban-Parra MJ, Rodrigo FS, Castro-Diez Y. 1998. Spatial and temporal patterns of precipitation in Spain for the period 1880-1992. International Journal of Climatology 18(14): 1557–1574.Camuffo D, Bertolin C, Diodato N, Cocheo C, Barriendos M, Dominguez-Castro F, Garnier E, Alcoforado MJ, Nunes MF. 2013. Western Mediterranean precipitation over the last 300 years from instrumental observations. Climatic Change 117(1–2): 85–101. DOI: 10.1007/s10584-012-0539-9. Prohom M, Barriendos M, Sanchez-Lorenzo A. 2016. Reconstruction and homogenization of the	Taken into accound. Literaute has been reassessed in coordination with chapter 11.
98061	44	16	44	16	Add reference to Knutson and Zeng (2018) here. This is another study which supports the Hoerling et al. conclusion for detectable anthropogenically forced precipitation declines in the Mediterranean region. Ref: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637, https://doi.org/10.1175/JCLI-D-17-0672.1. [Thomas Knutson, United States of America]	Noted. This study is more appropriate to include in the preceding paragraph about global trends in aridity.
116739	44	19	45	19	I suggest to avoid using the word "demonstrate" in the assessment of evidence (the demonstration depends on the robustness of the evidence etc) [Valerie Masson-Delmotte, France]	Accepted. Word changed to "indicate"

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129189	44	21			After ""... (Cook et al., 2016a)."" add ""A study based on the Community Earth System Model showed that the terrestrial aridity change from preindustrial to present day (1950-2005) is four times of that from the Little Ice Age (1550-1850) to the Medieval Warm Period (950-1250) (Fu et al., 2016)."" Citation: Fu, Q., L. Lin, J. Huang, S. Feng, and A. Gettelman, 2016: Changes in terrestrial aridity for the period 850-2080 from the Community Earth System Model. J. Geophys. Res. Atmos., 121, doi:10.1002/2015JD024075. [Trigg Talley, United States of America]	Noted. This study is more appropriate to include in the preceding paragraph about global trends in aridity.
53313	44	23	44	24	Please, check the consistency with CH3 [Hervé Douville, France]	Noted. This assessment matches the other chapters in the FGD
96807	44	25	44	25	Please mention which region in North America has a Mediterranean climate (California?) or delete exception. [Nicole Wilke, Germany]	Noted. this sentence has been revised.
98063	44	25	44	28	Recommend to replace/update/correct the existing sentence with the following: "In the southern hemisphere subtropical land regions, this includes a small region within the West Cape region of South Africa, where human influences increased the likelihood of the 2015—2017 drought by a factor of three (Otto et al., 2018); southwest and parts of southern Australia, where precipitation declines have been largely attributed to anthropogenic changes in greenhouse gases and ozone (Delworth and Zeng, 2014); and parts of New Zealand affected by the 2013 drought where anthropogenic influence via circulation changes was found (Harrington et al. 2014). In these regions, as well as parts of other southern subtropical regions--including parts of southeast Australia/Tasmania, Chile, and a small region of New Zealand--detectable anthropogenic decreasing precipitation signals have been reported (Knutson and Zeng 2018)." Refs: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637 https://doi.org/10.1175/JCLI-D-17-0672.1. LJ Harrington, S Rosier, SM Dean, S Stuart, A Scahill, 2014: The role of anthropogenic climate change in the 2013 drought over North Island, New Zealand. Bull. Am. Meteorol. Soc 95, S45-S48. [Thomas Knutson, United States of America]	Accepted. This sentence has been modified accordingly.
17227	44	27	44	28	Break sentence at line 27 between African and Australian content. Amend Australian content to read: In southern Australia, especially in the south-west, rainfall declines have been largely attributed to anthropogenic increases in greenhouse gases and reductions in stratospheric ozone, the intensification of the sub tropical ridge, and poleward expansion of the Hadley Cell circulation (Delworth and Zeng, 2014, Cai et al 2014, Timbal and Drosdowsky, 2013). [Joelle Joelle Gergis, Australia]	Noted. This sentence has already been revised in accordance with review comment 98063. Additional suggested references have been added.
29965	44	28	44	30	The Sahel, one of the regions most affected by drought, is overlooked in this section: it has been shown that the 20th century drought conditions in western Sahel have clearly emerged from natural variability in the context of the past 1600 years (Carré et al., Clim. Dyn. 2019). https://doi.org/10.1007/s00382-018-4311-3 [Matthieu Carré, France]	Noted. A detailed assessment of changes in the Sahel may be found in Ch. 11 and 12 in the FGD.
64963	44	30	44	30	To which statement does the confidence statement refer? [Johannes Quaas, Germany]	Rejected. The sentences following this confidence statement present the needed evidence.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58953	44	30	44	38	The following paper could be also cited within these lines. Bellprat et al (2016) have presented an application of event attribution for dry and wet rainy seasons over Southern Africa and Southern South America. Particularly for South Africa they found an increase in risk of anomalously dry austral summer seasons and an increase in risk of anomalously wet seasons attributable to anthropogenic influence on climate. Meanwhile, for Southern South America, they were not able to make a robust attribution statement for the analyzed extreme events. Reference: Bellprat O., Lott F.C., Gulizia C., Parker H.R., Pampuch L.A., Pinto I., Ciavarella A., Stott P.A. (2015). Unusual past dry and wet rainy seasons over Southern Africa and South America from a climate perspective. Weather and Climate Extremes, 9 , pp. 36-46. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This belongs in the Ch. 11 assessment of extreme events.
21053	44	31	44	32	“South America” should be replaced by “northeast Brazil”. If you individualize California within the US, you should also be more precise wrt locations in other parts of the globe. [Marcelo Barreiro, Uruguay]	Accepted, the sentence has been changed.
98065	44	35	44	36	Change to: “This may be a signature of anthropogenic forcing (low confidence) but cannot yet be distinguished from natural variability at least over parts of this region (Hoell et al., 2017b; Philip et al., 2018), although Knutson and Zeng (2018) report detectable anthropogenic decreases in annual precipitation over at least the Sudan region of eastern Africa for 1901-2010.” Refs: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637 https://doi.org/10.1175/JCLI-D-17-0672.1 [Thomas Knutson, United States of America]	Noted. The sentence has been modified accordingly.
6919	44	40			Wording should be should be "during the post-1950s period" or "since the 1950s". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted-Text revised
53315	44	42	44	43	What about the drivers of this Indian Ocean warming (e.g., Hoerling and Kumar 2003)? [Hervé Douville, France]	Rejected. This warming could be partly attributed to anthropogenic forcing, but that assessment belongs in Chapter 9.
21055	44	45	44	45	“Pacific northwest” may not be clear to many people. Could you be more specific? Maybe northwestern US or western Canada? [Marcelo Barreiro, Uruguay]	Accepted, changed to north-western US
33047	45	1	45	55	Explain the reason for the change in the forecast of future rainfall reduction to increase precipitation in new IPCC reports. [Sahar Tajbakhsh Mosalman, Iran]	Rejected. Unclear what the reviewer is suggesting.
32717	45	1	45	55	Explain the reason for the change in the forecast of future rainfall reduction to increase precipitation in new IPCC reports. [sadegh zeyaeyan, Iran]	Rejected. Unclear what the reviewer is suggesting.
113335	45	6	45	6	'climate change contribution' I think you mean 'anthropogenic contribution' here. [Diego Miralles, Belgium]	Accepted, change made.
113339	45	10	45	10	When refering to 'soil moisture drought' and 'runoff drought' bear in mind that 'agricultural drought' and 'hydrological drought' have already been defined earlier on. [Diego Miralles, Belgium]	Accepted, we have changed this to refer to agricultural and hydrological drought.
12831	45	10	45	36	The written style is very nice for this section. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted, thank you.

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72245	45	10	45	36	Discussion is missing and should include: Batibeniz et al 2020: Doubling of U.S. Population Exposure to Climate Extremes by 2050. Earth's Future. 23 March 2020 https://doi.org/10.1029/2019EF001421 [Hunter Cutting, United States of America]	Rejected. This paper concerns future projections, which belongs in Section 8.4. This paper might also be better assessed by Ch. 11, the extremes chapter.
72247	45	10	45	36	Discussion is missing and should include: Milley and Dunne, 2020: Colorado River flow dwindles as warming-driven loss of reflective snow energizes evaporation. Science 13 Mar 2020: Vol. 367, Issue 6483, pp. 1252-1255 DOI: 10.1126/science.aay9187 [Hunter Cutting, United States of America]	Accepted, this reference has been added
72249	45	10	45	36	Discussion is missing and should include studies that detect drying trends in the Western United States and attribute those trends to anthropogenic warming, including: Martin et al 2020: Increased drought severity tracks warming in the United States' largest river basin. PNAS May 26, 2020 117 (21) 11328-11336; first published May 11, 2020 https://doi.org/10.1073/pnas.1916208117 [Hunter Cutting, United States of America]	Accepted, we have added this reference
72251	45	10	45	36	Discussion is missing and should include: Park Williams 2020: Large contribution from anthropogenic warming to an emerging North American megadrought. Science 17 Apr 2020: Vol. 368, Issue 6488, pp. 314-318. DOI: 10.1126/science.aaz9600 [Hunter Cutting, United States of America]	Accepted, this reference has been added
72253	45	10	45	36	Discussion is missing and should include: Berg and Hall, 2017: Anthropogenic warming impacts on California snowpack during drought. Geophysical Research Letters. https://doi.org/10.1002/2016GL072104 [Hunter Cutting, United States of America]	Rejected, this study is discussed in the previous paragraph.
72255	45	10	45	36	Discussion is missing and should include: Williams et al 2020: QUANTIFYING HUMAN-INDUCED TEMPERATURE IMPACTS ON THE 2018 UNITED STATES FOUR CORNERS HYDROLOGIC AND AGRO-PASTORAL DROUGHT. Bulletin of the American Meteorological Society. January 2002 DOI:10.1175/BAMS-D-19-0187.1 [Hunter Cutting, United States of America]	Rejected. In Ch. 8 we are charged with giving an overview of regional findings. Specific regional case studies are the purview of Ch. 12.
72257	45	10	45	36	Shaleene B. Chavarria and David S. Gutzler,2018: Observed Changes in Climate and Streamflow in the Upper Rio Grande Basin. Journal of the American Water Resources Association First published: 06 March 2018 https://doi.org/10.1111/1752-1688.12640 [Hunter Cutting, United States of America]	Rejected, we have removed this text here in favour of developing a more international assessment. This reference may be considered in Ch. 12.
72259	45	10	45	36	Discussion is missing and should include: Overpeck and Udall, 2020: Climate change and the aridification of North America. PNAS June 2, 2020 117 (22) 11856-11858; first published May 19, 2020 https://doi.org/10.1073/pnas.2006323117 [Hunter Cutting, United States of America]	Noted, the streamflow component of this text was reorganized in the FGD.
53317	45	10	45	36	May deserve a broader assessment (not only ver the US) [Hervé Douville, France]	Noted. More detailed regional assessments can be found in Ch. 11 and 12 in the FGD.
113337	45	11	45	11	A recent review on the role of evaporative demand on drought: Vicente-Serrano, S. M., Mcvicar, T. R., Miralles, D. G., Yang, Y. and Tomás-Burguera, M.: Unraveling the influence of atmospheric evaporative demand on drought and its response to climate change, WIREs Clim Change, 11(2), 1–31, doi:10.1002/wcc.632, 2020. [Diego Miralles, Belgium]	Rejected-Nice review on the atmospheric evaporative demand, but the focus of this paragraph is on the fact of drought change under global warming and its attribution
113341	45	14	45	16	I would say the underlying processes are part of the drought, not a reason not to trust the diagnostics. Also,the main problem of runoff as 'natural drought' diagnostic is management of river discharge. [Diego Miralles, Belgium]	Noted. However it is important here to clarify why we give a medium (vs high) confidence here in this section so we retain this sentence.
10165	45	15	45	16	This is an important caveat and should be expanded a bit / made more explicit, and some basic references added. [Jacob Scheff, United States of America]	Rejected. The uncertainties in plant physiological processes are assessed elsewhere in the Chapter (Section 8.2.3.3). However references and links to this section are now provided in the FGD
53319	45	15			also, and may be first of all, the VPD control on potential evapotranspiration (Peng et al., 2018)? [Hervé Douville, France]	Noted. This is implicit in the reference here to vegetation processes.
109785	45	16	45	16	There are also model biases in runoff sensitivity that affect our ability to detect and attribute changes (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, we now mention this with this reference
113343	45	19	45	19	driest anomaly' for 'most negative anomaly'? [Diego Miralles, Belgium]	Accepted, changed to "lowest"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98067	45	22	45	29	The following study should also be cited here and considered elsewhere in this chapter: Milly, P C., and Krista A Dunne, March 2020: Colorado River flow dwindles as warming-driven loss of reflective snow energizes evaporation. Science, 367(6483), DOI:10.1126/science.aay9187. This study identifies a different physical process for the Colorado River flow declines than is currently discussed here. [Thomas Knutson, United States of America]	Accepted, this reference has been added
29967	45	38	45	39	Sahel should be included in that list [Matthieu Carré, France]	Noted. A detailed assessment of changes in the Sahel may be found in Ch. 11 and 12 in the FGD.
72261	45	38	45	39	Liklihood statement here should be changed. Given the 8 additional references provided here (comments #36 to #43), it is extremly likely (not very likely) that the frequency and severity of drought has increased in Western North America, drought defined as hydrological. [Hunter Cutting, United States of America]	Noted, likelihood statements removed in consulation with Ch 11 in the FGD, but high confidence is assessed.
8677	45	38	45	40	I agree that drought severity has increased in the recent decades in regions of the Mediterranean, but this is not caused by the precipitation decrease but consequence of increased atmospheric evaporative demand given increased temperature and reduced relative humidity. See e.g.: Stagge JH, Kingston DG, Tallaksen LM, Hannah DM. 2017. Observed drought indices show increasing divergence across Europe. Scientific Reports 7(1). DOI: 10.1038/s41598-017-14283-2. but precipitation changes cannot be attributed to antrophogenic forcing as suggested since precipitation is not decreasing for the long term (the decrease between 60s and 2000s was consequence of the variability in the North Atlantic Oscillation). Thus, for the long term (1850-2018) precipitation has increased at least in the W Mediterranean region: Vicente-Serrano, S.M., Domínguez-Castro, F., Murphy, C., Hannaford, J., Reig, F., Peña-Angulo, D., Tramblay, Y., Trigo, R.M., MacDonald, N., Luna, M.Y., McCarthy, M., Van der Schrier, G., Turco, M., Camuffo, D., Noguera, I., El Kenawy, A., García-Herrera, R., Becherini, F., della Valle, A. Long-term variability and trends in meteorological droughts in Europe (1851-2018). International Journal of Climatology. Under review. [Sergio Vicente-Serrano, Spain]	Noted, the text was updated in the FGD to reflect the importance of atmospheric evaporative demand.
51465	45	38	45	42	These overall findings (and the related text) are inconsistent with the findings in Chapter 11 on drought. The executive summary of Chapter 11 [p.11-8 L14-18] (and related underlying sections) find that there is "High confidence that precipitation deficits have increased since the 20th century in west Africa, central Africa and southern Africa... there is medium confidence that some regions show more frequent hydrological drought (e.g. southern Africa, southern Northern America, the Mediterranean region)". These inconsistencies have also found their way into the SPM. Similar comments have been made in the SPM and Chapter 11. Would it be possible to work with Chapter 11 and SPM authors to ensure cross-referencing and consistency across chapter findings please? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We are coordinating with Ch. 11 to ensure that our confidence statements are similar across chapters.
5479	45	38	45	42	Anthropogenic warming will increase stream flow in late winter-early spring at the expense of summer runoff. Considering the effects of anthropogenic warming on snow (Section 8.1.3.7.2) and the warm-season stream flow and the importance of warm season runoff in determining water resources in western North America, the link between anthropogenic warming and (warm-season) droughts in western North America may be of higher confidence than stated here. [Jinwon Kim, United States of America]	Rejected-No quantitative results were found to support such arguments.
113345	45	38	45	42	This is ill-phrased: 'drought increased in A, B, C. In A with high confidence, in D?..., in C..., other regions'. The region 'D' needs to be in the first sentence. Moreover 'other regions' can only refer to 'B' now. [Diego Miralles, Belgium]	Taken into account-The region 'D' has been added in the first sentence. The 'other regions' refers to 'B' but also other regions.
7629	45	38	45	42	This summary includes one sentence about the formally detectable global changes in aridity found in Marvel et al (2019) over the 1900-1950 period, and in Bonfils et al. (submitted) over the 1950-2014 period. [Celine Bonfils, United States of America]	Accepted. Statements concerning global-scale trends in drought were added to the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17229	45	39	45	41	Please change both instances of 'Australia' to 'southern Australia' in lines 39 and 41. [Joelle Joelle Gergis, Australia]	Accepted, fixed.
98069	45	39	45	42	Re: “These changes can be at least partly attributed…parts of South Africa, and parts of southwest and southern Australia…” Comment: you may consider adding parts of Chile, Tasmania, and New Zealand to your list of medium confidence areas for increasing drought risk, based on the precipitation trend analyses in Knutson and Zeng (2018) and Harrington et al. (2014, for North Island of New Zealand). These additional regions pass model-based detection/attribution tests and importantly also appear to fit within a broad pattern of such long-term (century-scale) decreases in precipitation at similar latitudes across the southern hemisphere land regions. Also, these features seem to lie within the broad patterns of the set of decreasing precipitation regions in these southern hemisphere latitudes simulated in climate models (regions which extend out over the oceans in models), as shown for example in Fig. 8.2 (c,d). Refs: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637 https://doi.org/10.1175/JCLI-D-17-0672.1. LJ Harrington, S Rosier, SM Dean, S Stuart, A Scahill, 2014: The role of anthropogenic climate change in the 2013 drought over North Island, New Zealand. Bull. Am. Meteorol. Soc 95, S45-S48. [Thomas Knutson, United States of America]	Noted, Chile is discussed in the FGD; more detailed regional assessments are relegated to Ch. 11 and Ch. 12
129191	45	39			Change in South Africa is attributed to anthropogenic warming while not being listed as a region where drought worsening has occurred? The section on internal variability doesn't cover some of the insights from large-ensembles, which suggest the intensification of ENSO teleconnections to key tropical land regions. Citation: Fasullo, J.T., B. Otto-Bliesner, S. Stevenson, 2018; ENSO Teleconnections to Temperature, Precipitation and Wildfire in a Warming Climate, Geo. Res. Lett., doi: 10.1029/2018GL079022. [Trigg Talley, United States of America]	Noted, we have made the list of the regions consistent. The assessment of the role of ENSO however is outside the scope of this particular subsection.
72263	45	41	45	42	Confidence statement here should be changed. Given the 8 additional references provided here (comments #36 to #43), there is high confidence in an anthropogenic contribution to the frequency and severity of drought in Western North America, drought defined as hydrological. [Hunter Cutting, United States of America]	Noted, asssessment in FGD is high confidence
105551	45	43	13	44	transition ….a domninant driver of what????? [Sanjiv Kumar, United States of America]	Taken into account, this section has been revised.
29969	45	47	47	11	It should be mentionned in this section that a reconstruction of humidity in Sahel based on mollusk stable isotopes showed a strong increase in aridity since the early 19th century that supports the narrowing trend of the tropical rainbelt. The contrast between dry MCA and humid LIA conditions is not consistent with hydroclimate in the Sahel being related to latitudinal shifts of the ITCZ (Carré et al., Clim.Dyn. 2019). [Matthieu Carré, France]	Accepted - The reference was included.
53321	45	47			Could also emphasize the model difficulty to simulate the observed ITCZ features (double ITCZ syndrome) [Hervé Douville, France]	Accepted - We now emphasized the current biases in CMIP5/CMIP6 models.
113349	45	49	45	49	I find this confussing: 'the tropical belt has widened' then the 'ITCZ has narrowed'. Pressumably this 'widening of the tropical belt' refers to the poleward drift of the Hadley Cells. If it does, I am not sure what the next section refers to (8.3.2.2). Also, by 'tropical rain belt' (not 'belts'?) in the title, I understand the belt of convective rain at the ITCZ, not the tropics in general. [Diego Miralles, Belgium]	Accepted. The text reads now: "the tropical belt as delimited by the Hadley circulation has widened…"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17231	45	49	46	42	Please improve palaeoclimate citations for this section to provide a long term perspective on recently observed changes. Also highlights significant advances in tropical/Southern Hemisphere palaeoclimatology since AR5. Examples include: 1) Griffiths, M. L., Kimbrough, A. K., Gagan, M. K., Drysdale, R. N., Cole, J. E., Johnson, K. R., Zhao, J.-X., Cook, B. I., Hellstrom, J. C. and Hantoro, W. S. (2016). Western Pacific hydroclimate linked to global climate variability over the past two millennia. Nature Communications 7: DOI: 10.1038/ncomms11719. 2) Denniston, R. F., Ummenhofer, C. C., Wanamaker, A. D., Lachniet, M. S., Villarini, G., Asmerom, Y., Polyak, V. J., Passaro, K. J., Cugley, J., Woods, D. and Humphreys, W. F. (2016). Expansion and Contraction of the Indo-Pacific Tropical Rain Belt over the Last Three Millennia. Scientific Reports 6: 34485. 3) Maupin, C. R., Partin, J. W., Shen, C. C., Quinn, T. M., Lin, K., Taylor, F. W., Banner, J. L., Thirumalai, K. and Sinclair, D. J. (2014). Persistent decadal-scale rainfall variability in the tropical South Pacific Convergence Zone through the past six centuries. Clim. Past 10 (4): 1319-1332. 4) Partin, J. W., Quinn, T. M., Shen, C.-C., Emile-Geay, J., Taylor, F. W., Maupin, C. R., Lin, K., Jackson, C. S., Banner, J. L., Sinclair, D. J. and Huh, C.-A. (2013). Multidecadal rainfall variability in South Pacific Convergence Zone as revealed by stalagmite geochemistry. Geology 41 (11): 1143-1146. [Joelle Joelle Gergis, Australia]	Accepted - These references were included.
14849	46	4	46	8	The structure of the sentenc is not clear. Or is there an unfortunate 'cut-and-paste'? Or is it the punctuation? [Marie-France Loutre, Switzerland]	Noted - This corresponds to two different sentences. Punctuation was corrected.
21057	46	6	46	6	The “,” before Byrne should be a “.”, right? [Marcelo Barreiro, Uruguay]	Noted - This corresponds to two different sentences. Punctuation was corrected.
4931	46	6	46	6	Somewhere in this sentence a period (.) is missing [Bart van den Hurk, Netherlands]	Noted - This corresponds to two different sentences. Punctuation was corrected.
12833	46	6			Is this supposed to be two separate sentences? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted - This corresponds to two different sentences. Punctuation was corrected.
43187	46	6			Read " ITCZ location consistent with Zhou et al. (2011). Byrne et al. (2018) from " rather than " ITCZ location consistent with Zhou et al. (2011), Byrne et al. (2018) from " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
14475	46	9		10	Physical reasons for ITCZ narrowing have been recently discussed in Stuecker, M. F., A. Timmermann, F.-F. Jin, C. Proistosescu, S. M. Kang, D. Kim, K.-S. Yun, E.-S. Chung, J.-E. Chu, C. M. Bitz, K. C. Armour, and M. Hayashi (2020): Strong remote control of future equatorial warming by off-equatorial forcing, Nature Climate Change, doi:10.1038/s41558-019-0667-6 [Malte Stuecker, United States of America]	Not applicable – text removed
23477	46	13	46	17	please consider the following study which mention the connection between sea surface salinity and the terrestrial precipitation during ENSO/IOD events. "Near-Surface Salinity Reveals the Oceanic Sources of Moisture for Australian Precipitation Through Atmospheric Moisture Transport" "https://doi.org/10.1175/JCLI-D-19-0579.1" [Saurabh Rathore, Australia]	Noted - This section addresses changes in the ITCZ, not atmospheric moisture transport. This reference is considered in section 8.3.1.2

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
571	46	19	46	26	To support the paragraph from line 19 to line 26, Utida et al. (2019) study could be cited. They show that the north-south displacement if the ITCZ during the last 2,300 in the Atlantic Basin were due to changes in Northern Hemisphere climate and also to expansion/contraction of the tropical rainbelt.These changes were related to variations in sea surface temperature and southeast trade winds in the tropical South Atlantic Ocean. This study also finds a dry Little Ice Age in Northeastern Brazil. Full reference: Utida, G. et al. (2019). Tropical South Atlantic influence on Northeastern Brazil precipitation and ITCZ displacement during the past 2300 years. Scientific Reports 9:1698. [Luciana Figueiredo Prado, Brazil]	Accepted - This reference was included.
5481	46	19	46	26	The two paragraphs, 19-26 and 28-42, may be combined into a single paragraph. These two paragraphs are on the effects of anthropogenic aerosols on tropical rainfall and the land-part of ITCZ based on two different observational data sets. Combining these two paragraphs will result in a more compact description on the effects of anthropogenic aerosols on tropical rainfall variations with the ITCZ as a special case. [Jinwon Kim, United States of America]	Accepted - These paragraphs were merged.
22321	46	19	46	42	These two paragraphs are repetetive and should likely be merged. [Peter Thorne, Ireland]	Accepted - These paragraphs were merged.
64965	46	22	44	26	The Wild (2012) study is not an attribution study. Where does the statement come from? [Johannes Quaas, Germany]	Taken into account - The attribution was added by model simulations which are now discussed and referenced.
7603	46	22	46	25	A brief description of Figure 8.11 (right column) and Bonfils et al. (submitted) could be incorporated around the discussion of the Wild (2012) results, or after line 42 of page 46. You can add something like: "By investigating the simultaneous changes in precipitation, temperature and continental aridity in CMIP5 historical simulations, Bonfils et al. (submitted) found a secondary signal (Figure 8.11, right column) characterized by a robust interhemispheric temperature contrast {section 3.3.1.1} a latitudinal shift in ITCZ (in accord with the theory of cross-equatorial energy transport; section 8.2.2.2), and changes in aridity in Sahel {section 8.3.1.3}. These forced changes are statistically detectable in reanalyses over the 1950-2014 period at the 95% level confidence". I think this could be sufficent sincee some material is described in Box 8.1. Eventually add "they include a southward shift in ITCZ prior 1975 triggered by an aerosol-driven cooling in the Northern Hemisphere, followed by a northward shift in ITCZ after 1975 caused by a reduction of sulfur dioxide emissions and the greenhouse gases-induced warming of Northern Hemisphere landmasses {Box 8.1)." [Celine Bonfils, United States of America]	Accepted - The suggested text was incorporated.
22319	46	22	46	26	This text should link to the relevant sections in chapters 6 and 7 where the reader may go for further information. [Peter Thorne, Ireland]	To be done after first revised Chapters 6 and 7 are available by mid-August.
29005	46	23			This is not entirely consistent with 8.3.1.3 and 8.2.2.2 since Dong & Sutton (2015) [duplicated in references] argue that greenhouse gas radiative forcings dominate ITCZ recovery over the Africa region. A regional perspective on the ITCZ shifts is probably necessary. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Section 8.3.1.3 does recognize the contribution of the aerosol forcing (among other factors) to the recovery of the Sahel precipitation.
5579	46	37	43	38	There is a high confidence that groudwaters have been depleting across the globe… in India, US,… , not only, but also in Mediterranean zone [Benoit Laignel, France]	Not applicable - This text is not longer included
7617	46	40	46	40	2 references could be added: Chiang, J., C. Chang, and M. Wehner (2013), Long-Term Behavior of the Atlantic Interhemispheric SST Gradient in the CMIP5 Historical Simulations, Journal of Climate, 26(21), 8628-8640, doi:10.1175/JCLI-D-12-00487.1; Iles, C., and G. Hegerl (2014), The global precipitation response to volcanic eruptions in the CMIP5 models, Environmental Research Letters, 9(10), doi:10.1088/1748-9326/9/10/104012. [Celine Bonfils, United States of America]	Accepted - Chiang and Chang (2013) is already referenced. Iles and Hegerl (2014) was added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12835	46	41			Here and elsewhere: ensure English use of spellings such as behaviour, vapour etc. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted - We verified English spelling across the chapter.
39077	46	42	46	42	As it reads now, it is not immediate to understand the discussion on ITCZ changes with Fig. 8.11. Perhaps some more detail could be given in the text. [Federico Serva, Italy]	Taken into account in the FGD. Details are given in 8.3.1.6 (page 40).
129193	46	47			Another Figure 8.11? [Trigg Talley, United States of America]	Thanking for you for pointing out the figure numbering problem. This is now rectified in the FGD.
113351	47	6	47	11	This summary is missing some reference to the tropical rain belt(s) in the title of the section. I still find this unclear... [Diego Miralles, Belgium]	Noted - We modified the paragraph mentioning the tropical rain belts.
53323	47	8			first of all to improve the ITCZ simulation in coupled models? [Hervé Douville, France]	Noted - We modified the paragraph mentioning the need to improve the ITCZ simulation in GCMs.
29009	47	14			Chemke & Polvani (2019) Nature Geosci. argue recent strengthening of northern Hadley Circulation in reanalyses is spurious and continued weakening of the tropical Hadley circulation is an expected consequence of rising CO2 concentrations. This section could be more focused on water cycle aspects of the Hadley Cell if mechanistic understanding is discussed in other chapters e.g. Chapter 2, Section 2.3.1.4.1 which can also be checked for consistency with summary "In summary, there has been a very likely widening of the Hadley Circulation since the 1980s, although there is only medium confidence in the extent of the changes. This has been accompanied by a strengthening of the Hadley Circulation, particularly in the northern hemisphere (medium confidence). There is low confidence in the estimation of trends in the strength of the Walker circulation, which are highly time period dependent. Trends since 1980 are consistent with a strengthening that resembles a La Niña-like Walker circulation and a westward shift of the Walker circulation (medium confidence).") [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - We focused this section on the hydrological aspects of the observed changes in the Hadley circulation.
22323	47	16	47	19	This opening paragraph should allude to the findings made by precursor chapters - mainly chapters 2 and 3, in part to avoid overt repetition. Coordination of this section with chapters 2 and 3 is required. In particular the attribution findings of chapter 3 are never mentioned and I fear the assessment findings reached also differ. [Peter Thorne, Ireland]	Noted - We checked consistency with CH2 and 3. Also, mention to CH3 assessment was included.
12837	47	18	47	19	Change the wording from "but..." to "with large uncertainties in the magnitude of this shift" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - This sentence was rephrased as suggested.
1275	47	21	47	33	It may be useful to explain to readers unfamiliar with tropical meteorology how the Hadley cell and the ITCZ are connected. Also, it is important to note that reanalyses do not provide reliable trend information as they over time incorporate increasingly improved observational network in their data assimilation. [Rasmus Benestad, Norway]	Noted - The paragraph already states the discrepancies among reanalyses and between reanalyses and models. Also, the connection between the ITCZ and the HC/tropical belt is mentioned previously in the chapter, in section 8.2.2.2
80309	47	23	47	23	The reference should be in brackets [Paola Arias, Colombia]	Accepted - The reference was corrected.
21059	47	23	47	24	In the previous subsection it is said that no shift in the Pacific and Atlantic ITCZ are observed (p 46, l 28). The sentence “...consistently with the observed southward shift of the ITCZ...” is not consistent with that statement. [Marcelo Barreiro, Uruguay]	Accepted - The statement in previous subsection was modified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12839	47	23			Reference not properly included in sentence. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - The reference was corrected.
29007	47	24			I could not find reference of the ITCZ southward shift in Greve et al. (2014). [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - This text is not longer included
12841	47	25			What is meant by expansion of the subtropical highs? Does it mean that they get wider in diameter, or do their centres move poleward? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted - "Expanded" means that they occupied a larger area, and thus their diameter increased.
12843	47	29			Remove "recently" since there is a "recent" later in the sentence. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - This text is not longer included
113353	47	35	47	35	Why is the widening of the tropical belt discussed here if it was the first thing mentioned in the previous section (whose title listed the 'tropical ran belts')? For those of us that are not experts in this topic this may feel hard to grasp unless the structure is more clear. [Diego Miralles, Belgium]	Noted - Here the widening of the tropical belt is applied to the understanding of the changes in the hydrological cycle. The previous mention substantiated that the widening actually occurred and its possible causes.
6921	47	38	47	41	What direction is the monsoon rain band shift over China: north or south? From the rest of the statement, it is probably south. And is this the Meiyu front or just a way to describe the monsoon in general? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - A description was added how aerosol-induced weakening of the east Asian monsoon coupled with the enhanced moisture flux to southern China leads to drying the north and wetting the south and centre.
17233	47	47	47	47	Insert reference to Cai et al (2012) as it is a key paper and deals with Africa, Australia and South America. Cai, W., Cowan, T. and Thatcher, M. (2012). Rainfall reductions over Southern Hemisphere semi-arid regions: the role of subtropical dry zone expansion. Scientific Reports 2: 702. [Joelle Joelle Gergis, Australia]	Accepted - The reference was included.
80307	47	49	47	52	This is mentioned in CH2 (page 49, lines 33 to 35) [Paola Arias, Colombia]	Noted
6923	47	54			Is this cause or effect? One would assume that intensification of the hydrological cycle would entail both a strengthening of the monsoon and of the subtropical high pressure zones. Or has the strengthened subtropics led to greater ventilation, weakening the monsoon following the arguments of page 21? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - Text has been removed since it discusses projections and not historical changes

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88131	48	3	48	3	Poleward expansion of the Hadley cells and subtropical precipitation declines are both occurring, but recent modelling evidence (e.g. He and Soden, 2017, cited here) suggests that subtropical precipitation declines are a response to direct CO2 radiative forcing over land, not the Hadley cell expansion. [Robin Chadwick, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - This has been clarified in the text.
17235	48	4	48	4	Insert reference to Timbal and Drosdowsky (2013) for southern Australia [Joelle Joelle Gergis, Australia]	Accepted - This reference was included.
12845	48	13			Remove capitalisation from "Southern Edge" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
20507	48	17	48	31	All this is not without interest, but one would expect to find it in a chapter named "circulation" or something similar. While admittedly the outline requests to address circulation, the features discussed here appear associated more with general energy transfer issues than to water cycle [philippe waldteufel, France]	Accepted - We now provide an assessment related with the impacts of HC changes in the water cycle to avoid overlaps with CH3.
1277	48	24	48	24	The points made here may be clearer for most readers if the text explains exactly what rapid circulation adjustments to CO2 forcing is, without having to read Ceppi et al (2018b). [Rasmus Benestad, Norway]	Not applicable - This text is not longer included
21061	48	31	48	31	Please include a link to 3.3.3.1. [Marcelo Barreiro, Uruguay]	Accepted
22325	48	33	48	40	I am unclear how this assessment chimes with that in chapter 3 - but regardless there is a real risk of redundancy here and a discussion is clearly required with chapter 3 to resolve. Hadley Circulation is more clearly in scope of the chapter 2-3-4 collective than chapter 8. As is walker circulation. Chapter 8 should be considering solely the hydrological cycle aspects as per its charge. Also, almost certain is not recognised uncertainty language construct. Use virtually certain but only if you mean it and ensure that characterisation is consistent with chapter 2. [Peter Thorne, Ireland]	Accepted - We now provide an assessment related with the impacts of HC changes in the water cycle to avoid overlaps with CH3. Uncertainty language has been calibrated according to the IPCC guidelines.
17237	48	33	48	40	The executive summary states for chapter 8 states that: 'There is high confidence that Southern Hemisphere storm tracks and associated precipitation have migrated polewards over recent decades, especially in the austral summer and autumn in locations like Australia'. However, no similar statement in made here in section 8.3.2.2. Need to include a specific statement to support the exective summary statement, especially as it a 'high confidence' statement. Summary statement also needs work, phrasing and clarity needs to be improved. [Joelle Joelle Gergis, Australia]	Accepted - The statement was rephrased to support the ES
53325	48	34			do you mean virtually certain? [Hervé Douville, France]	Noted - We replaced this with "high confidence"
39079	48	37	48	37	I have not found a discussion of aerosol forcing before in this section 8.3.2.2, perhaps this should be expanded. [Federico Serva, Italy]	Noted - This chapter is focused on water cycle changes. A comprehensive assessment of historical aerosol forcings is available in Ch2 (Section 2.2.6)
88133	48	43	49	3	Why only discuss trends since the 1980s? The evidence from longer-term observed trends is a bit contradictory, but shouldn't it still be discussed? [Robin Chadwick, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - We included discussion on changes before the 1980s

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17239	48	43	49	3	Section 8.2.2.3 on the Walker circulation is really short to warrant its own sub-section. Suggest incorporating elsewhere or expand the assessment. Surely changes in walker circulation are important for ENSO variability etc so I think it needs more work. [Joelle Joelle Gergis, Australia]	Accepted - We expanded this section considering the impacts on the water cycle.
14981	48	43	49	3	The Walker circulation section seems like a mix between Chapters 2 and 3, with results from observations and models. Which are the implications of these changes in the regional water cycle? Maybe the answer can be found in section 8.2.2.2 (29 pages before), so I wonder if the Walker circulation assessment can be improved by organizing it in a different way. [Juan Rivera, Argentina]	Noted - We focused this section on the hydrological impacts of the observed changes in the Walker circulation
53327	48	43			Could be further develop, starting with the AR5 conclusions and linking to the Pacific variability contribution to the recent global warming hiatus (XC Box3.1). Analyses of Walker circulation trends in AMIP-type simulations could be also assessed (e.g., Yim et al., 2017) and emphasize the possible influence of land surface warming. [Hervé Douville, France]	Accepted - These topics were included in this section.
29011	48	43			This is brief and does not place the result in the context of the expected long term decline of the Walker circulation expected on energetic and thermodynamic grounds (8.2.2.2) nor the hydroclimatic implications. It should also be checked with 2.3.1.4.1 which summarises "There is low confidence in the estimation of trends in the strength of the Walker circulation, which are highly time period dependent. Trends since 1980 are consistent with a strengthening that resembles a La Niña-like Walker circulation and a westward shift of the Walker circulation (medium confidence)." [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - We linked with the expected long-term decline of the Walker circulation based on the discussion in section 8.2.2.2. We checked inconsistencies/overlaps with CH2. Also, we included the impacts on water cycle.
113355	48	45	48	46	There are apparently inconsistent messages regarding the Walker circulation. While section 8.2.2.2 reports a weakening, here it is stated that there is consistent evidence of a strengthening. Please revise and clarify. Also I am not sure why these two sections read so different while meant to be reporting on the same issue. Perrhaps a figure with time series would help. [Diego Miralles, Belgium]	Noted - Despite the expected long-term decline of the Walker circulation based on energetic and thermodynamic aspects, as discussed in section 8.2.2.2, there is contrasting evidence regarding the variability of the Walker circulation strength. As mentioned in section 8.3.2.3, different studies indicate a weakening of the Walker circulation since the 1950s while many others consistently indicate a strengthening since the 1980s. Therefore, changes in the strength of the Walker circulation are highly dependent on the time period considered, as assessed in Chapter 2 (section 2.3.4.1.1). We highlighted this in the text.
1279	48	45	48	46	Perhaps relate how a strengthening of the Walker circulation fits in the relation to a modelled decreased atmospheric overturning (p. 20, L14-15 " The slowdown can occur in both the Hadley and Walker circulations, but in most climate models occurs preferentially in the Walker circulation")? To me, there seems to be a mixed picture between the simulated and observed systems. [Rasmus Benestad, Norway]	Noted - Despite the expected long-term decline of the Walker circulation based on energetic and thermodynamic aspects, as discussed in section 8.2.2.2, there is contrasting evidence regarding the variability of the Walker circulation strength. As mentioned in section 8.3.2.3, different studies indicate a weakening of the Walker circulation since the 1950s while many others consistently indicate a strengthening since the 1980s. Therefore, changes in the strength of the Walker circulation are highly dependent on the time period considered, as assessed in Chapter 2 (section 2.3.4.1.1). We highlighted this in the text, indicating the role of natural variability.
22331	48	45	48	54	This material was largely assessed already in chapters 2 and 3. Why is chapter 8 repeating and inviting the reader to play spot the difference. Better surely to start from the assessed basis arising from chapters 2 and 3 and point the readers there? There is also a very substantive discussion in chapters 4 and 7 around the walker circulation and implications but I don't see refernce there. Given that this is the sole paragraph of assessment text on walker circulation there is a real question around value of retention? [Peter Thorne, Ireland]	Noted - We focused this section on the hydrological impacts of the observed changes in the Walker circulation. We included references to CH2, 3, and 7. CH4 is not referenced in this subsection since it deals with projections, which is developed in section 8.4.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6711	48	51	48	51	"hiatus period" should be changed to "slowdown" to be consistent with the terminology used in Chapter 3. Earlier in the same line "recent" could be replaced by "early 21st century", as the word recent means different things to different people. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
22329	48	51	48	51	Reckoned is not a scientific term suitable for use in an IPCC report. [Peter Thorne, Ireland]	Accepted – Amended to read: '…is thought to be a major driver…'
88135	48	51	48	51	I suggest 'thought' instead of 'reckoned' [Robin Chadwick, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – Amended to read: '…is thought to be a major driver…'
12847	49	1			Replace "from 1980s" with either "since the 1980s" or "since 1980" depending on your intended meaning. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – Amended to read: '…since the 1980s…'
112227	49	1			Are there any regions in the world that currently not have a monsoon but will under a changing climate? [Rutger Hofste, Netherlands]	Noted - The atmospheric general circulation over the extratropics and polar regions is very different from the circulation and moist processes over the tropical monsoon areas. Therefore it is very unlikely that extratropics and polar regions will experience a monsoon regime due to climate change (at least by the end of 2100). However, if there is a slight poleward shift of monsoonal circulation due to climate change, then some fringe areas, which are presently not under monsoon regime, could experience monsoon type of climate in the future.
21063	49	3	49	3	Please include a link to 3.3.3.1. [Marcelo Barreiro, Uruguay]	Accepted
85049	49	6	49	6	This is a nicely composed section. Discussing each "monsoon" area separately in this Assessment is a welcome feature. [venkatachalam ramaswamy, United States of America]	Thanks for the feedback
80311	49	6	49	6	CH2 mentions South African monsoon (page 52, line 43). This monsoon is not assessed in CH8 [Paola Arias, Colombia]	Noted. The South African monsoon "exists" only in the framework of the global monsoon definition, and it does not have dedicated literature independently from the global monsoon one. According to this, it is considered in fig 8.12 and 8.22. Specific explanation is given in the introductory paragraph and in the figure captions.
6957	49	6			Generally this historical/present-day monsoon section looks in much better shape than the monsoon coverage in the FOD. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Thanks for the feedback

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19209	49	8	49	9	It is very good if the authors add the percentages for using freshwater for agriculture, water resources, industry, transportation and various socio-economic activities. [Mohamed Deyab, Egypt]	Noted with thanks.
22333	49	8	49	9	Mission creep into WG2 impacts territory [Peter Thorne, Ireland]	Noted
22335	49	10	49	12	It would be advisable to recap what the assessment findings for these two sections were here as the jump off point for your own assessment. [Peter Thorne, Ireland]	Noted with thanks.
86423	49	13	49	15	Nice representation of regional monsoon domains shown in Figure 8.12. Chapter-4 assessed projected changes in precipitaion and circulation over the global monsoon domain following AR5 assessment. Some discussion on these aspects can be included either in Chapter-4 or Chapter-8. [Swapna Panickal, India]	Thanks for the feedback. Future projected changes in regional monsoons are assessed in Section 4 with links to Ch.4.
6925	49	14			Change "monsoons" to "monsoon". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
6927	49	18	49	38	The authors are to be congratulated for producing a figure describing domains which represent objective assessments as well as encompassing the variety of literature that examines monsoons on palaeoclimate, historical, present and future climate monsoons. Note that the observational symbols are extremely small on panel (b). The caption omits to mention the period covered or how many members are used for each experiment. Presumably it is the historical experiments? Could CMIP5 results also be included? Finally, the caption states that the domains have been defined somehow based on literature. Will this literature be defined in some sort of technical annex? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Thank you. The figure has been further improved and modified in the FGD (Figure 8.11). Information about the ensemble members, literature, monsoon regions, etc .. are provided in Annex V and Annex VII (Glossary).
76779	49	20	49	35	Figure 8.12 the definition of the North American Monsoon does not agree with the very vast literature on the topic, nor is not consistent with the area assessed in this chapter nor in the Atlas. Can you please re-defined the area or provide a solid justification of why/how you define this area. Also please note the in the central and southern regions of Mexico there is not a monsoon dynamics, even if there is a maximum of ppt, is not driven by a monsoon dynamics, it is more related to the shift of the ITCZ and/or tropical systems. [Ruth Cerezo Mota, Mexico]	Taken into account in the FGD. The definition of North American Monsoon in AR6 is provided in Annex V.
70243	49	25	49	26	In the Figure caption, the seasons used to define the South American (EqSAmer) and South African (SAfri) monsoons are not shown clearly in the caption and text. In addition, the authors do not address anything about EqSAmer in the text. [Seogyeong Kim, Republic of Korea]	The rationale for defining the regional monsoons in AR6 is provided in Annex V. The region over South Africa, Central America and equatorial South America have a strong seasonality in precipitation but their qualification as monsoons is a subject of discussion. In the assessment of the regional monsoons in Sections 8.3.2.4 and 8.4.2.4, these regions are not considered as distinct regional monsoons, but they are discussed in Box 8.2 that is dedicated to changes in water cycle seasonality. The domains of the regional monsoons in AR6 are defined based on published literature and expert judgement, and accounting for the fact that the climatological summer monsoon rainy season varies across the individual monsoon regions.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70245	49	27	49	31	What is the reason separate the North American monsoon (NAMS) in AR5 to North American monsoon (NAmerM) and EqSAmer in AR6? [Seogyeong Kim, Republic of Korea]	Taken into account in the FGD. The North American Monsoon definition in AR5 was based on the concept of global monsoon. The rationale for defining the regional monsoons (including the North American Monsoon) is provided in Annex V.
70247	49	32	49	36	The analysis period for the results based on CMIP6 models and reference period in CRU and GPCP are absent. Besides, the colors of stippling (Figure 8.12a) and block representing the SAfri and EqSAmer (Figure 8.12b) look like the same. It would be nice to discern the color to exhibit each monsoon. [Seogyeong Kim, Republic of Korea]	Taken into accounting the FGD. The analysis period (1951-2014) is now mentioned in the FGD (Figure 8.11).
40071	49	41	49	41	Define in the glossary as a subterm of 'monsoon' [TSU WGI, France]	Accepted. The sub-term of monsoon 'South and Southeast Asian monsoon' is defined in the glossary
87531	49	41	49	56	The remote sensing has been used to study Monsoon Energetic over Bay of Bengal were studied by analyzing the Cloud and Monsoon Depression fields during Summer Monsoon using TIROS-N & DMSP (Defense Meteorological Satellite Pictures), Satellite imageries. The 700 mb kinematic features e.g. vergence, vorticity, Pressure perturbations and vertical velocities for a few selected phases appeared in the Bay of Bengal during 27 June – 06 Aug'79.; within the 10o x 10o grid-box (i.e. 15 – 25o N and 85 – 95o E) were computed. The Time Series curves were plotted for the Cloud fields. Also, the kinematic features of a typical Disturbed Phases (05 – 07 Jul 79) were studied in a LaGrange an frame by using Drop -Wind- Sonder data of U.S. research Aircraft. The data profiles u and v were drawn to identify the evolutionary features of severe Thunderstorm Cell associated with Monsoon Depression as well as to evaluate the few optimum values of the Disturbed Phases maxima in terms of Percentage Cloud Coverage; Life time and movement (direction and speed) of the Depressions along with the optimum values of kinematic features over the Bay of Bengal before they affect Indian region. Goswami VK (1979) studied the Tele-connection of SH Cyclonic Storm with the unusual rainfall over South peninsula of India during Northeast Monsoon. Nitta, Saha, Subrahmanyyam , Kung, et.al (1981): analyzed MONEX-79 Data sets in several ways to study the Monsoon Depressions & role of Barotropic instability to forecast summer rainfall. Very little has been addressed to the Cloud Cluster behavior over the summer MONEX domain. A good number of publications are available in literature on the characteristics of Cloud Clusters over the Atlantic & Pacific (Suomi, Martin and Sikdar 1972,1980, 1981) and other parts of tropics (Young and Sikdar, 1977, Knox and Gary 1973 and House, et. al. 1981). Goswami VK, Sikdar, Martin (1984) at University of Wisconsin, USA by using DMSP & TIROS_N (VR & IR) imageries, FGGE Level III-B,MONEX-79 ,made the pioneer studies of the summer MONEX Cloud Cluster's characteristics, viz. the distribution, size, intensity, lifetime and trajectories over the summer MONEX domain (0 – 30o N & 70 – 120o E) and related their distribution to the synoptic scale weather disturbances. It was found that lifetime of these clusters increases with size and intensity. The intense and very large clusters were found to be	Noted with thanks. All the suggested papers were published before 1984. Note that relevant papers published after 2013 (AR5) have priority for citation
6929	49	43			Remove "in South and Southeast Asia" from this line since it is already mentioned at the beginning of the sentence. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29013	49	44		47	The following is not central so could be removed "Areas in the northwestern Himalayas also receive substantial precipitation during winter and early spring in association with extra tropical synoptic-scale disturbances, also referred as western disturbances (Madhura et al., 2014; Cannon et al., 2015; Dimri et al., 2015; Hunt et al., 2018; Krishnan et al., 2018)". In general, the discussion of regional monsoons could be usefully condensed with a focus on the material that is policy relevant and backs up the summary paragraphs. Notably the S American and Australasian monsoon sections contain lengthy discussion but very brief summary statements. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	taken into account. In the FGD, the text on winter and early spring precipitation over Northwestern Himalayas has been dropped from 8.3.2.4.1. The section on Australian and Maritime Continent monsoon is shortened in the FGD. The length of the South American Monsoon section in the FGD is nearly the same as in the SOD. There is policy relevant focus of regional monsoons in the FGD.
6931	49	46			"also" is not required and can be removed' also change "referred as" to "referred to as". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and corrected. Thanks.
20509	49	50	56	15	According to Figure 8.12, the simulated precipitations seem on the high side for southern hemisphere monsoons. [philippe waldteufel, France]	Noted. This part of the assessment has been revised as Fig. 8.12 (8.11 in the FGD) has been updated using more available CMIP6 models outputs
6933	49	50			replace |"like" with "such as" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and corrected. Thanks.
116741	49	53	49	53	I think that "solar insolation" means orbital (astronomical) forcing here? [Valerie Masson-Delmotte, France]	Taken into account. Yes. This refers to solar orbital forcing which is now included. Thanks.
6935	49	55	50	2	Note that such long term negative trends in Indian monsoon rainfall are not supported by long-term rain gauge records which find no appreciable trend (e.g. since the early 1800s, Sontakke et al. https://dx.doi.org/10.1177/0959683608095576 The Holocene: Instrumental period rainfall series of the Indian region (AD 1813-2005): Revised reconstruction, update and analysis, or instead the recent work of Knutson and Zeng 2018 who noted no conclusive trend over the 1901-2010 period: Model assessment of observed precipitation trends over land regions: Detectable human influences and possible low bias in model trends. J. Clim. doi:10.1175/JCLI-D-17-0672.1). [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Thank you. This point is now included.
83867	50	4	50	7	Several references are missing here, Only papers of the authors cited especially lead author's. For e.g. a very well known and highly cited paper like Turner and Annamalai (2012) is missing. Reference: Turner and Annamalai, 2012, Climate change and south Asian summer monsoon, Nature Climate Change, doi: 10.1038/nclimate1495 [Ajaya Mohan Ravindran, United Arab Emirates]	Turner and Annamalai (2012) is now cited. Note that relevant papers published after 2013 (AR5) have priority for citation
72055	50	4	50	13	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]	Taken into account. Samanta et al (2020) is cited in the FGD (page 50, line 53) in the context the observed declining trend of monsoon rainfall.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6943	50	8	51	36	Could or should this subsection on the East Asian monsoon also assess changes (if there have been any) to the Meiyu-Baiu front? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account- Projections on the Meiyu-Baiu rain belt have been added.
58997	50	9	50	9	post-1950 what? Incomplete sentence which potential useful information omitted. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The sentence is corrected.
12849	50	10	50	13	Should the recovery also reference the groundwater increase in the post-2002 period shown clearly for much of peninsular India in Figure 8.10? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	The ground water changes (Fig.8.10) cannot be attributed to this apparent monsoon recovery. There is no robust scientific attribution for this.
29015	50	11		13	Unless a published record is available I suggest removing this link to a website. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The website link is removed. A new reference is included.
6937	50	12	50	13	I don't think it is appropriate to cite a webpage in an IPCC report. Clicking this link resulted in "Error 404, page not found". The IPCC make assessments of published, peer-reviewed literature. If such literature does not exist to support this claim, then the claim should be removed. To my knowledge, and as stated elsewhere on the MOL webpage, "These charts are based on area-weighted mean using the real-time reports of 53 stations over India". 53 is a very small sample from which to base such a claim, given that Indian rainfall gridded datasets (even the 1-degree Rajeevan product) contain ~2000 gauges quality controlled back to the 1950s. (See also a later comment, that your figure of groundwater changes supports increasing rainfall.) [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The website link is removed. A new reference is included.
6713	50	12	50	13	It is unusual to see a reference to a website in the body of an IPCC Assessment Report. Many website references could have been used in other chapters to point to the latest results of monitoring activities, but were not. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The website link is removed. A new reference is included.
85045	50	19	50	20	The NH anthropogenic aerosol forcing dominance playing a large role in the regional monsoon weakening during the late 20th C is correctly recognized. However, there was probably also a small role due to the volcanic aerosols from the two major eruptons. [venkatachalam ramaswamy, United States of America]	Accepted. This point about the influence of volcanic aerosols is now included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29017	50	22		23	Box 8.1 is already referenced on this line so this sentence could be removed "A summary of the physical processes and mechanisms of monsoon precipitation changes induced by aerosols is given in Box 8.1." [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This modification is included.
6939	50	25			Large spread is not the only issue here; for example the Polson et al. 2014 work cited above shows that anthropogenic aerosols in CMIP5 yield a negative forcing on the monsoon in the late-20th century, yet they do not explain the magnitude of the decline. One suggestion is that consideration of the IPO (negative to positive phase transition) is necessary in order to explain the full magnitude of the change, e.g. as in the Salzmann works but also in the recent large ensemble modelling study of Huang et al. (2020), The Recent Decline and Recovery of Indian Summer Monsoon Rainfall: Relative Roles of External Forcing and Internal Variability. https://doi.org/10.1175/JCLI-D-19-0833.1 . Note that a subsequent phase change of the IPO (positive to negative phase) can be used to help explain the subsequent recovery of the Indian monsoon rainfall as also mentioned in the Huang study. A MMM from CMIP experiments cannot capture such forcing, by design. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Thank you. Based on the reviewer's comment, the phase change of the Pacific interdecadal variability is mentioned in the FGD (page 51, lines 18-19) as follows: "including variations linked to phase changes of the Pacific interdecadal variability (Section AVI.2.6) (Huang et al., 2020b)"
85047	50	27	50	27	See also Bollasina, M., Y. Ming, V Ramaswamy, M. D. Schwarzkopf, and V. Naik, 2014: Contribution of Local and Remote Anthropogenic Aerosols to the 20th century Weakening of the South Asian Monsoon. Geophysical Research Letters, 41(2), DOI:10.1002/2013GL058183 [venkatachalam ramaswamy, United States of America]	Thank you. The Bollasina et al. 2014 reference is included.
83859	50	28	50	30	None of the cited studies tells us how the rapid warming trend of equatorial Indian Ocean contributes to the negative trend in monsoon precipitation ina conclusive manner. A conclusive statement like this in the IPCC report based on these two studies sahll be avoided. BTW, as per section 10.6.3.5 this is still a hypothesis. [Ajaya Mohan Ravindran, United Arab Emirates]	Rejected. One of the papers Krishnan et al. 2016 showed the effect of the equatorial IO SST warming on the Indian monsoon precipitation using model simulation experiments.
6941	50	32	50	38	This paragraph seems rather standalone and missing in context. What also of the poorly simulated diurnal cycle of convection in parametrized models, ubiquitous in the tropics? There is also the issue of some means of convective initiation caused by mesoscale gradients in soil moisture, as observed in India but which will not be represented in parametrized models (e.g. Barton et al., 2019: https://doi.org/10.1002/qj.3538 A case study of land-atmosphere coupling during monsoon onset in northern India.) [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Challenges in representing the diurnal cycle of convection and land-atmosphere coupling are discussed.
83869	50	40	50	40	weakening of SAsiaM is unclear. Need to clearly mention whether it is precipitation or circulation [Ajaya Mohan Ravindran, United Arab Emirates]	Taken into account in the FGD (page 50, lines 50-53). Weakening of SAsiaM is seen both in precipitation and circulation (see page 50, FGD) - "A significant decline in summer monsoon precipitation is observed over India since the mid-20th century, which is accompanied by a weakening of the large-scale monsoon circulation (Mishra et al. et al., 2012; Abish et al., 2013; Krishnan et al., 2013, 2016; Saha et al., 2014; Roxy et al., 2015; Guhathakurta et al. et al., 2017; Samanta et al. et al., 2020)"
29333	50	40	50	42	Why does the final statement in this sentence, on the observational evidence for a drying trend in the second half of the 20th century, lack a confidence assessment? The cold epochs and tree ring statements are given with "high confidence". There seems to be enough evidence stated of the observed drying trend on lines 4-10 that some form of confidence statement can be made. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Confidence statement for the observational evidence of drying trend in the second half of the 20th century is included

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98071	50	42	50	45	This statement should be classified as “medium confidence”, not “high confidence”. It is basically a model result about expected changes (assuming the models are correct) with at present little observational support in the form of detectable changes in observed precipitation changes that can be attributed to various forcings. [Thomas Knutson, United States of America]	Rejected. High confidence is retained. This is supported by the attribution of observed monsoon precipitation changes based on the CMIP6 models (Fig.8.11).
22337	50	42	50	45	I did not see sufficient supporting evidence in the underlying assessment text to justify this finding. There needs to be a clearer case made in the underlying text to justify this assessment finding or the finding will need to be modified accordingly. [Peter Thorne, Ireland]	Taken into account. The sentence is modified in the FGD.
40073	50	48	50	48	Define in the glossary as a subterm of 'monsoon' [TSU WGI, France]	Accepted - This definition is presented in the Glossary: The East Asian monsoon (EAsiaM) is the seasonal reversal in wind and precipitation occurring over East Asia during the boreal summer (from June to September). The EAsiaM region cover a continental area roughly bounded by 110-130E and 20-40N, including mainland China, Japan, the Korean peninsula, Taiwan, Hong Kong and Macau. Differently from the other monsoon it extends quite northward, out of the tropical bounds, and it is largely influenced also by subtropical systems, like the Western North Pacific subtropical high, and by disturbances from the mid-latitudes. The EAsiaM has a strong winter component with opposite characteristics and with documented consequences for the climate of the region, specifically named as East Asian winter monsoon.
78749	50	48	51	36	This section overlaps with Chapter 10 (10.4.1.2.2.) in describing the historical changes of East Asian Monsoon. However, only the weakening trend is emphaasized here while in Chapter 10 both the weakening and recovering of the monsoon are presented. [jian li, China]	Taken into account. The assessment of the East Asian summer monsoon has been revised incorporating and summarizing the assessment done in the SOD of ch 10.
130535	50	48	51	36	The content here overlaps with Chapter 10 on section 10.4.1.2.2 in p75 but inconsistent in conclusion. [Panmao Zhai, China]	Not applicable. Assessment of East Asian summer monsoon is kept in ch 8 and removed from ch 10. Two assessment have been merged in the FGD
6945	50	50			Change "evidences" to "evidence". The word evidence encompasses plural concepts. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been revised as suggested
6947	50	51			There is potential ambiguity over the term "monsoon rain belts". Do you mean the Meiyu fron or simply the northward extent of the monsoon? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account-Here refers to the northern extent of the East Asian monsoon, which has been clarified in the text.
22339	50	52	50	52	Delete 'warm' in front of holocene for consistency across report [Peter Thorne, Ireland]	Accepted-"warm Holocene" has been changed into "mid-Holocene".

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6949	50	53			Change "northerly" to "northward". Northerly means from the north. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted-Text revised.
116743	50		55		Coordination x chapters is needed for monsoon to avoid duplication and sharpen the overall findings (what is observed, outcome of detection and attribution, large scale drivers, response to different types of forcing incl volcanic, aerosol, GHG, model evaluation & projections) [Valerie Masson-Delmotte, France]	Accepted. We have coordinated with Chapters 2 and 3 (and also 4) to avoid duplication and sharpened the overall findings. Thanks a lot.
6951	51	6			Does "jet stream" refer to the "subtropical westerly jet stream or the polar jet stream which may also affect northern East Asia? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted- The subtropical jet stream and polar jet stream are two conceptual jet streams which are driven by the thermodynamic and dynamical processes respectively. In the real wind field, the two jet streams often merge as one strong jet stream, which is identified as the zonal wind maximum. Here, the East Asian westerly jet just refers to the upper-troposphere zonal wind maximum in East Asia, where features strong seasonality. During summer, it shifts towards north of 45N, exerting significant influence on precipitation in northern East Asia.
78751	51	10	51	11	The drivers of monsoon changes are different with that shown in Chapter 10. [jian li, China]	Not applicable. Assessment of East Asian summer monsoon is kept in ch 8 and removed from ch 10. The two assessments have been merged in the FGD
6953	51	10			Change "are identified" to "have been identified" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted-Text revised.
6955	51	14			The word "Apparently" could be read as a value judgement on the paper in question. Was that a word used by its authors? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted - The word "apparently" is no longer used
78753	51	18	51	19	The drivers of monsoon changes are different with that shown in Chapter 10. [jian li, China]	Not applicable. Assessment of East Asian summer monsoon is kept in ch 8 and removed from ch 10. The two assessments have been merged in the FGD
130539	51	19	51	21	Very uncertain on aerosol influnce on wither precipitation in east Asia. Sugget to drop "Anthropogenic aerosol-induced cooling". [Panmao Zhai, China]	Taken into account. The assessment here has been focused on the summer monsoon only.
6959	51	23	51	28	This discussion seems out of place given that interannual variability and teleconnections have not been discussed either for the EASM as a whole or the preceding SAsiaM. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The paragraph has been removed in the revised text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6961	51	23	51	28	Does this WNP monsoon have a land component or is it purely oceanic? Is it relevant to policymakers? Could it be illustrated by a dotted line as a subregion of the EASM for Figure 8.12? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The paragraph has been removed in the revised text.
6963	51	34			Change "aerosols increase" to "aerosol increase". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been revised as suggested
6965	51	35	51	36	The sentence here is a good cross-reference to chapter 10. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thank you
85051	51	39	51	39	Missed seeing discussion of precipitation changes in the southern part of Africa, with biomass burning emissions of aerosols affecting the precipitation. There are quite a few investigations suggesting sensitivity to aerosols, both scattering and absorbing types. Example: Randles, C A., and V Ramaswamy, 2010: Direct and semi-direct impacts of absorbing biomass burning aerosol on the climate of southern Africa: a Geophysical Fluid Dynamics Laboratory GCM sensitivity study. Atmospheric Chemistry and Physics, 10(20), DOI:10.5194/acp-10-9819-2010. [venkatachalam ramaswamy, United States of America]	Noted with thanks.
88909	51	39	51	39	Change title to 'African monsoons' since this section rightly includes the East African monsoon [Dave Rowell, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. East African rainy seasons are assessed. Whether this can be called "monsoon" is a subject of debate.
40075	51	39	51	39	Define in the glossary as a subterm of 'monsoon' [TSU WGI, France]	Accepted. The West American monsoon (WAfriM) is a reversal wind system that dominates the West Africa region and characterized by advance of low-level moist south westerlies from the Atlantic (May to September) and hot and dry north easterlies (harmattan), with the rainfall maximum located south of the confluence of these streams. Annual cycle of monsoon rainfall exhibits three distinct phases: the initiation phase (March-May), the high rain period (June-August), and the southward retreat of the rain belt (September-October) leading to bimodal cycle over coastal region unimodal cycle over Sahel. During the onset phase of the WAfriM, precipitation maximum remains along the Guinean coast near 4°N until late June or early July. At that time, the rainfall maximum shifts abruptly into the southern Sahel, near 12°N, known as the West African monsoon jump.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7435	51	39	52	45	This section concerns the West African Monsoon. It mainly focuses on the Sahel region. I suggest to consider the possibility to add a paragraph on the southern part of West Africa (between Sahel and Guinea gulf). Maybe this study could help : Recent Trends in the Daily Rainfall Regime in Southern West Africa. Francis Nkrumah, Théo Vischel, Geremy Panthou, Nana Ama Browne Klutse, David C Adukpo, Arona Diedhiou. 2019. Atmosphere [Geremy PANTHOU, France]	Accepted. The following text is add : "Over the Southern West Africa region, a regime of wetter conditions has been observed since the mid-1990s, mainly due to the both increase in mean intensities of wet days and occurrences of extreme events. This is more pronounced in the coastal and inland regions than the Sudan region. Coastal region experiences increase in extreme rainfall since 1990, while Sudan regions observed an increase between 1970 and 1990, followed by a stabilization through to the current years (Nkrumah et al., 2019). Positive trend in rainfall was observed during the second rainfall season (September-November) in recent years as a result of more intense and more frequent rainfall event, while more intense but less frequent rainfall events is associated to insignificant trend during the April-July rainfall season (Sanogo et al., 2015; Bichet and Diedhiou, 2018; Nkrumah et al., 2019). "
29971	51	39	52	45	The multicentennial scale perspective provided by Sahel paleoclimate reconstructions suggest that the wet conditions of the 50s, the drought of the 70-80s and the recent recovery of rainfall is related to strong multidecadal natural variability that masks a long-term centennial trend of aridification (Carré et al., 2019; Gallego et al., 2015 https://doi.org/10.1002/qj.2601), that is difficult to detect in instrumental data. It is consistent with the late 20th century Sahel drought being attributed "half" to climate change. The mollusk shell isotopic record from Senegal indicates that Sahel drought has emerged from natural variability which supports anthropogenic forcing, and that it is not related to a soutward shift of ITCZ. The section and section conclusion should be revised to take these insights into account. [Matthieu Carré, France]	Noted. The assessment has been revised taking into account the suggested references combined with the check of consistency among the report for aridity and drought in the different regions
53329	51	39			Also quote the recent synthesis by Biasutti (2019)? [Hervé Douville, France]	Accepted. Biasutti (2019) is cited
113357	51	40	51	41	The West African Monsoon (WAfriM) region experienced the wettest decade (1950s and early 1960s) in the twentieth century' This can be better phrased. [Diego Miralles, Belgium]	Accepted. Sentence has been rephrased. Thank you.
105733	51	40	51	43	Justin Sheffield; Eric Wood; Nathaniel Chaney; Kaiyu Guan; Sara Sadri; Xing Yuan; Luke Olang; Abou Amani; Abdou Ali; Siegfried Demuth; Laban Ogallo, 2013: A Drought Monitoring and Forecasting System for Sub-Sahara African Water Resources and Food Security, BAMS-D-12-00124 [Abou Amani, France]	Accepted.
42767	51	41	51	43	"with a deficit in annual rainfall falling to 60% of the long-term century mean (Ali and Lebel, 2009; Nicholson, 2013; Descroix et al., 2015)"; this percentage was not the same everywhere. In fact it was rather constant in absolute value around 200mm, thus ranging from 60% in the North of the Sahel to 25-30% in the South (LeBarbé et al., 2002 and Lebel et al., 2003 provide comprehensive information on this) Refs: Le Barbé, L., T. Lebel, and D. Tapsoba, 2002. Rainfall variability in West Africa during the years 1950-1990. J. Climate, 15(2), 187-202 Lebel, T., Diedhiou, A., and Laurent, H. (2003). Seasonal cycle and interannual variability of the Sahelian rainfall at hydrological scales. J. Geophys. Res. 108, 8389. doi:10.1029/2001JD001580 (which apperared in the first version of this chapter) [Thierry Lebel, France]	Taken into account. This point is mentioned. It may be noted that relevant papers published after 2013 (AR5) have priority for citation. This point is considered while citing older papers if absolutely necessary.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42769	51	43	51	44	The long decline in annual rainfall is related to a decrease of rain occurrence over the Sahel (Frappart et al., 2009; Bodian et al., 2016) Why choosing these two particular references, while previous work based on comprehensive data sets identified this important characteristic of the drought at both regional (LeBarbé and Lebel, 1997) and local scale (Balme et al., 2006). Note that those two references appeared in the first version of Chapter 8, and I see no reason for them to disappear in this revision. Please explain [Thierry Lebel, France]	Taken into account. This section doesn't deal with characteristics of drought, which is assessed in Chapter 11. Relevant papers published after 2013 (AR5) have priority for citation. Most of the references suggested by the reviewer are much before 2013 (e.g., Le Barbé et al. 2002, Lebel et al. 2003, Lebel and Ali, 2009, ...). Yet, we have included some of them, if found to be relevant to this section.
42771	51	43	51	44	Following on the previous comment, I would redraft the sentence as follows: "The long decline in annual rainfall in the 1970s-2000s over the Sahel was mainly related to a decrease in rainfall occurrence (Lebarbé and Lebel, 1997; Frappart et al., 2009; Bodian et al., 2016), even though the interannual variability pattern is more complex as revealed by Balme et al. (2006). The decrease of rainfall occurrence also appears responsible for a 20-year (1970-1990) rainfall deficit the Soudano-Guinean sub-region of West-Africa (LeBarbé et al., 2002), as well as for a decrease of summer rains over parts of Ethiopia and south Sudan which have linkages to WAfriM (Nicholson, 2017). Ref: LeBarbé, L., and Lebel, T. (1997). Rainfall climatology of the HAPEX-Sahel region during the years 1950–1990. J. Hydrol. 188–189, 43–73. doi:10.1016/S0022-1694(96)03154-X. Le Barbé, L., T. Lebel, and D. Tapsoba, 2002. Rainfall variability in West Africa during the years 1950-1990. J. Climate, 15(2), 187-202 [Thierry Lebel, France]	Taken into account. It may be noted that relevant papers published after 2013 (AR5) have priority for citation. This point is considered while citing older papers if absolutely necessary.
6967	51	43	51	44	Change "over Sahel" to "over the Sahel" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
53331	51	43			replace long by multidecadal? [Hervé Douville, France]	Accepted
42779	51	45	51	45	Ref should read: (LeBarbé et al., 2002) [Thierry Lebel, France]	Accepted
6969	51	45	51	46	Note that these countries are outside of the WAM domain shown in Figure 8.12. (I offer no opinion either way on this.) [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted
6973	51	46	51	48	I don't understand this point. Why is interannual variability of the WAM important for its long-term declining trend? This point needs better explanation. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Agreed that the short text on the interannual variability is not relevant here and hence it is deleted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6971	51	46			Change "Decrease of rainfall occurrences" to "The decreased rainfall occurrence" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and modified.
93653	51	47	51	47	Replace WAM with WAfriM [Stefano Materia, Italy]	Accepted and modified. Thanks.
42775	51	50	51	53	Rather than Ali and Lebel (2009), the proper (or complementary) reference should be Lebel and Ali (2009), where three 20-year periods (namely 1950-1969; 1970-1989; 1990-2007) were analysed and compared and where it was shown that the interannual and decadal variability of the annual rainfall was not homogeneous over the entire Sahel. It also showed that the annual rainfall recovery was stronger in the East than in the West of the region, coherently with the long term trend anticipated in CMIP5 simulations. Lebel, T. and Ali, A., 2009. Recent trends in the Central and Western Sahel rainfall regime (1990 - 2007). J. Hydrol., 375(1-2), 52-64 [Thierry Lebel, France]	Taken into account. This point is included.
42773	51	50	51	55	It would be worth mentioning the shift in seasonality of the Sahelian rainfall reported in Lebel and Ali (2009) and in Nicholson (2013), since it has important consequences on the flow regimes of the Sahelian rivers and on agriculture yields. There is little agreement on whether this important shift will prevail in the future, but it is certainly one of the most significant present change in the rainfall regime of the region and should absolutely be mentioned Ref: Lebel, T. and Ali, A., 2009. Recent trends in the Central and Western Sahel rainfall regime (1990 - 2007). J. Hydrol., 375(1-2), 52-64 Nicholson (2013) already referenced [Thierry Lebel, France]	Accepted. This point is included with relevant references.
6975	51	50			Remove apostrophe from 1990's [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and modified.
17921	52	1	52	3	The issue of the role of SSTs in Sahel rainfall could be explained with greater clarity. If the report wishes to separate interannual and longer time scales, then Giannini et al. (2003 in Science) and Bader and Latif (2003, in Geophys Res Lett) attribute the abrupt shift from wet 1950s and 60s to dry 1970s and 80s to warming of the Indian Ocean. Giannini et al. 2013 reconcile the role of tropical oceans warming, of which the Indian Ocean is a manifestation, with variations in North Atlantic SSTs essentially exploiting the same argument given by Vecchi et al. (2008, in Science) about the influence of the relative warming of the North Atlanic on hurricanes. [Alessandra Giannini, France]	Taken into account. This point has been included. Thank you.
7433	52	3	52	4	« Increased frequency of extreme rainfall events impacts high flow occurrences of the large Sahelian rivers (e.g., Wilcox et al., 2018). » The increasing flooding events detected in this study also concerns the small to meso-scale watersheds. [Geremy PANTHOU, France]	Accepted. The suggested details have been included in the revised text
22341	52	3	52	6	Is this not the domain of chapter 11? Has consistency been checked? Do they assess the same thing? If so is this wise? [Peter Thorne, Ireland]	Noted. The assessment is consistent with ch 11. As it is just a sentence to complement an argument it can stay also in ch 8.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7431	52	8	52	8	Citation Wilcox et al. 2018 refer to 2 references. This one is not in the referecnes list : «Trends in hydrological extremes in the Senegal and Niger Rivers » Catherine Wilcox, Théo Vischel, Gérémy Panthou, Ansoumana Bodian, Juliette Blanchet, Luc Descroix, Guillaume Quantin, Claire Cassé, Bachir Tanimoun, Soungalo Kone Journal of Hydrology [Geremy PANTHOU, France]	Accepted. The reference has been corrected as suggested
22343	52	13	52	13	In cities and rural areas is superfluous text here. [Peter Thorne, Ireland]	Accepted. Suggested text has been removed
6977	52	14	52	15	The meaning of this sentence is not clear, particularly compounded (made worse?) and what is the role of LULC change? Could the words "leading to" before "increased runoff" improve the legibility? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text has been revised as suggested. "Compounded" is used with its meaning of "made worse".
42777	52	17	52	32	The possible role of continental surfaces as studied by Eltahir and Gong (1996), Zheng and Eltahir (1998), Philippon and Fontaine (2002), among others, is not mentioned here. Even though the dominant role of the unique SST pattern in the last part of the 20th century is the privileged explanation of the big drought, continental surfaces certainly modulate it both in space (ie Sahel versus Sudano-Guinena zone) and interannually (probably also modulating the decadal variability). Refs: Boone, A. A., Xue, Y., De Sales, F., Comer, R. E., Hagos, S. M., Mahanama, S., … Mechoso, C. R. (2016). The regional impact of land-use landcover change (LULCC) over West Africa from an ensemble of global climate models under the auspices of the WAMME2 project. Climate Dynamics, 47(11), 3547–3573. Eltahir, E., C Gong, 1996. Dynamics of wet and dry years in West Africa. J. Climate, 9, 1030-1042. Philippon, N., B Fontaine, 2002. The relationship between the Sahelian and previous 2nd Guinean rainy seasons: a monsoon regulation by soil wetness. Annales Geophysicae, 20, 575-582 Zheng, X., E. Eltahir, E., 1998. The role of vegetation in the dynamics of West African monsoon. J. Climate, 11, 2078-2096 [Thierry Lebel, France]	Noted. Because of space limitations we have not ben able to include these details in the assessment. The suggested reference has been included in the Annex dedicated to the regional monsoons description, including the west African monsoon
7631	52	19	52	19	cite also Bonfils et al. (submitted) and Figure 8.11. [Celine Bonfils, United States of America]	Not applicable. Text has been removed
6979	52	20			For the text "Sahel drought (1968-1995/1998)", the authors need to state the year range in a more appropriate manner such as "mid-to-late 1990s" if necessary. The policy maker or other interested reader cannot be expected to have to make up their own mind as to whether the trend lasted until 1995 or 1998. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Thank you. This sentence is rephrased to improve clarity.
6981	52	22			The words, "leading to regional precipitation changes" are unnecessary since the sentence already starts with the mention of the Sahel drought. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Thank you. We have corrected this sentence.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6983	52	23			Change "combined effects" to "combined effect" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and modified. Thanks.
6985	52	24	52	29	This passage of text seems to miss the key finding of the cited Giannini and Kaplan (2019) study, in which the subsequent aerosol removal led to SST warming over the North Atlantic, shifting the ITCZ further northward and strengthening the West African monsoon. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Thank you. We have included this key point in the revised text.
7633	52	29	52	29	optional: cite Bonfils et al. (submitted). [Celine Bonfils, United States of America]	Noted.
69487	52	29	52	31	This statement seems overly broad: is it meant to be refering to the Sahel, or all monsoons? If all monsoons, why is it tacked on to the end of a paragraph specifically about West Africa? Also, one could be a bit ore specific about the evidence for this statement. Is it primarily attribution studies using climate models? One could also use projections as evidence. But all of this evidence is contingent on reasonable representations of the processes within models. [Martin Singh, Australia]	Accepted with thanks. We had moved this broad statement in the beginning of 8.3.2.4
6987	52	29	52	32	The final 4 lines of this paragraph seem to be general rather than specific to the West African monsoon. Could they be raised to a general level within 8.3.2.4? (Although I appreciate that there is currently no such material other than the definition of the monsoon domains.) [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted with thanks.
6989	52	34	52	36	Can this be related (and thus cross-referenced) to ITCZ or Hadley Circulation changes described elsewhere in Chapter 8? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This paragraph has been removed and reallocated elsewhere (i.e. in Box 8.2 and in the regional chapters (where relevant)
98073	52	34	52	38	Since this section on West African Monsoon seems to be the only place where East African tropical precipitation trends are discussed, I would add here that: Knutson and Zeng (2018, Fig. 3) find evidence for a detectable anthropogenic decrease in annual rainfall over 1901-2010 in parts of northeast tropical Africa, including the Sudan region. Ref: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637, https://doi.org/10.1175/JCLI-D-17-0672.1. [Thomas Knutson, United States of America]	Rejected. In the FGD, the text on East African precipitation trends has been dropped from Section 8.3.2.4.3 on West African Monsoon.
42781	52	34	52	38	While rainfall variability over East Africa is of course highly relevant, it is strange that it is treated in a sub-section entitled "West African Monsoon". The same comment applies to section 8.4.2.4.3. In both cases the "mother" section is titled "Monsoons", but in fact 8.3.2.4.3 and 8.4.2.4.3 rather deal with "African rainfall raher than specifically with rainfall changes in the sole WAM [Thierry Lebel, France]	Taken into account. This paragraph have been removed from here and re-allocated elsewhere, including Box 8.2 and regional chapters

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29019	52	34		38	East Africa is not part of the West Africa monsoon. Also this is discussed elsewhere e.g. 8.3.1.3 and Box 8.2 so these lines can be replaced with a link to these. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This paragraph have been removed from here and re-allocated elsewhere, including Box 8.2 and regional chapters
88911	52	42	52	43	Change to "with more intense extreme events resulting from the combined effects of increasing GHG and decreasing European and North American anthropogenic aerosols forcing". ie. change "alongside" to "with",because I think there's no evidence the change in seasonal mean is due to anything other than the change in extremes. And I think worth clarifying that decreasing - ratehr than increasing - aerosols are important (alongside increasing GHGs). [Dave Rowell, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text changed as suggested.
53333	52	42	52	44	dominated by the combined effets of GHG and anthropogenic aerosol forcings (medium confidence), while natural Atlantic Ocean SST variations may have also contributed. (Land use change has rather been suggested as a potential driver of the Sahelian drought)? [Hervé Douville, France]	Taken into account. Land use changes and moisture changes have been included in the assessment in the core paragraphs of section 8.3.2.4.3.
6991	52	43			Change "aerosols forcing" to "aerosol forcing" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text changed as suggested.
4933	52	48	53	34	The monsoon sections are very informative. In this NAmMonsoon section I would have expected some info on the great dust bowl in the '30s [Bart van den Hurk, Netherlands]	Taken into account. A more extensive assessment has been done, however the dust bowl is only discussed in section 8.6.2.3
7015	52	48	53	34	Has no detection and attribution work been done to assess the role of GHG and aerosol in single-forcing CMIP5 (or 6) historical experiments for the North American monsoon? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. CMIP5 model assessments is included in the assessment of the west african monsoon.
100707	52	52	52	52	Note: It's only the ~1585 drought that exceeded the drought of 2000-2018. I would rewrite this statement somewhat to take that into account. See Williams et al. (2020; Science) [Matthew Kohn, United States of America]	Taken into account, this section has been revised and a more thorough assessment was done
6993	52	52			How are droughts and "failures" quantified? E.g. for the South Asia monsoon a drought is declared when rainfall falls 10% or more below average (and this is stated in the text). Failure is rather an emotive word. Does failure here really mean complete failure of the rains, i.e. 0%? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	taken into account, text was reworded
6995	52	53			Change "any of the instrumental period" to "any during the instrumental period" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted-Done

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6997	53	1			Remove the word "forced" as it confuses the sentence, and the sentence can be understood better without it. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted-Done
6999	53	3			Wouldn't this be more accessible language if "lake sedimentary record" were used instead of "lacustrine"? Is some important detail lost by simplifying the language? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted-Done
83527	53	6	53	6	To be conform with the definition used for LGM in Chapter 2 and Annex II it should say here 21,000 - 19,000 years ago. I also recommend to spell out yr as years because it easier to understand for non-expert readers and frees you from the issues (to be solved) of the use of yr/kyr/Myr vs. a/ka/Ma that should be homogenized throughout AR6 WG I as I pointed out especially for Chapters 2 and 5. [Antje H. L. Voelker, Portugal]	done
100709	53	6	53	6	Note: Could state the implications of this for moisture. For example, during cold Heinrich events, groundwater discharge in the SW substantially increased. See Springer et al. (2018; USGS Prof. paper #1839) [Matthew Kohn, United States of America]	Taken into account, this section has been revised and a more thorough assessment was done, including more references
7001	53	6	53	10	Not clear if these sentences pertain to palaeoproxy evidence or model experiments or both. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, we clarify now whether is model or proxy
7003	53	10	53	12	These sentences mention winter. Is this relevant to the summer monsoon or are you treating the region (and the other monsoon regions holistically)? The winter regime was not routinely discussed for other regions. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, text was reviewed and reworded
7005	53	12	53	14	Does this sentence return to summer or does it follow the winter introduced in the previous sentence? It is not stated. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, text was reviewed
7007	53	16	53	17	Does the sentence here imply a northward shift of the monsoon? If so it is perhaps worth stating (and may be relevant for a later comment on this paragraph). [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, text was reviewed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53335	53	20	53	23	A dense network of 59 rain gauges located in southeastern Arizona suggests that the intensification of monsoon subdaily rainfall intensities had already started in the mid-1970s (Demaria et al. 2019), as expected by a stronger global warming signature for subdaily rather than daily or monthly precipitation accumulation. [Hervé Douville, France]	Taken into account, reference was included
7009	53	20			Perhaps the words "Over the United States" are unnecessary for understanding the sentence. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	taken into account. Paragraph was reworded
7011	53	24			Remove "the" from "that increase in the NAmerM rainfall" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Done
7013	53	25	53	27	Is this sentence consistent mechanistically with the northward shift implied on lines 16-17? Could the sentences be connected together? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	taken in account. Paragraph was reworded
7017	53	31			Change "evidences" to "evidence". The word evidence encompasses plural concepts. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	done
40077	53	37	53	37	Define in the glossary as a subterm of 'monsoon' [TSU WGI, France]	Accepted - The South American monsoon (SAmerM) is a regional circulation characterized by the inflow of low-level winds from the Atlantic to South America in association with the development of surface pressure gradients during austral summer (December-February). This monsoon is characterized by: (i) a latent heat release due to deep convection in the Amazon, (ii) an upper troposphere circulation over the Bolivian Altiplano (a.k.a the Bolivian High), (iii) the development of the South Atlantic Convergence Zone, (iv) the strengthening of the South American low-level jet, among other features. The SAmerM onset is typically observed during austral spring, exhibiting its mature phase in austral summer and retreat phase in austral fall.
117291	53	37	54	44	For the disussion in this section, I think the confidence statement on delayed onsets of the SAMS could be stronger (high confidence?). Only 1 study is mentioned to come to the opposite conclusion, but by now it is also clear that that study can not be reproduced and results depeded on the region used to define the SAMS.Also, this conclusion is in line with theoretical understanding and future projections (see Seth et al, 2013: CMIP5 Projected Changes in the Annual Cycle of Precipitation in Monsoon Regions [Maisa Rojas, Chile]	Accepted - We now state "high confidence" for the SAMS onset delay

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
109401	53	37			Authors might want to refer for South American monsoon to this paper that is currently under review in JCLIM: Contrasting Southern Hemisphere monsoon response: midHolocene orbital forcing versus future greenhouse-gas induced global warming by D'Agostino et al. Basically it is an analogue study of the Northern Hemisphere counterpart that as been already cited in this chapter. The study is pretty new and offers a detailed description of Southern Hemisphere monsoons in past and future climates and their relationship with the shift of center of actions (main updraft) of local Hadley and Walker circulations. [Roberta D'Agostino, Germany]	Accepted - This reference was included.
53337	53	37			May be first quote the difficulty for global climate models to capture the features of the present-day SAmerM and its natural variability? [Hervé Douville, France]	Accepted - This was mentioned at the beginning of the subsection
29973	53	41	53	41	The lake sediment record published by Bird et al. (PNAS 2011, www.pnas.org/cgi/doi/10.1073/pnas.1003719108) should be cited here. [Matthieu Carré, France]	Accepted -This reference was included.
117293	53	42	53	44	There seems to be a missing object in this sentence. What about the warm periods? [Maisa Rojas, Chile]	Accepted - This sentence was completed indicating that the SAMS was stronger during LIA than during warmer periods, according to CMIP5/PMIP3 simulations.
7021	53	42	53	44	I'm not sure of the relevance of this sentence. It is not made clear why the warmer periods such as the MCA are important for the SAmerM, particularly since the subsequent sentence, "In accordance with…" talks about different periods such as the mid-Holocene. Both sentences need revising, and if they really are linked then "In accordance with" should be changed to "In consequence" or "Following" or something better. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - This sentence was completed indicating that the SAMS was stronger during LIA than during warmer periods, according to CMIP5/PMIP3 simulations.
83529	53	45	53	45	Similar comment to the one above: either use 6 ka -as used in Chapter 2 for a date/fixed point in time- but even better and more conform with the style used in this Chapter and Chapter 1 and most of Chapter 7, just write it out as 6000 years ago. [Antje H. L. Voelker, Portugal]	Accepted - We now use 6000 years ago.
22345	53	45	53	45	Should reference cross-chapter box 2.1 for definition of MH rather than assigning your own definition here. [Peter Thorne, Ireland]	Accepted - A reference to Cross-Chapter Box 2.1 was used.
29975	53	47	53	47	Mollier-Vogel et al., QSR 2013, and Bird et al., EPSL 2011, should be cited along with Prado et al., 2013a [Matthieu Carré, France]	Accepted - These references were included.
83531	53	49	53	49	You could refer here also to the speleothem study of Stríkis, N.M., Cruz, F.W., Barreto, E.A.S., Naughton, F., Vuille, M., Cheng, H., Voelker, A.H.L., Zhang, H., Karmann, I., Edwards, R.L., Auler, A.S., Santos, R.V., Sales, H.R., 2018. South American monsoon response to iceberg discharge in the North Atlantic. Proceedings of the National Academy of Sciences, 115 (15) 3788-3793, doi 10.1073/pnas.1717784115. and may be add as other paleo-archive evidence the marine sediment based study of Campos, M.C., Chiessi, C.M., Prange, M., Mulitza, S., Kuhnert, H., Paul, A., Venancio, I.M., Albuquerque, A.L.S., Cruz, F.W., Bahr, A., 2019. A new mechanism for millennial scale positive precipitation anomalies over tropical South America. Quaternary Science Reviews 225, 105990, doi: https://doi.org/10.1016/j.quascirev.2019.105990. [Antje H. L. Voelker, Portugal]	Accepted - These references were included.
29977	53	55	53	55	"However" seems an odd link since there is no contradiction between this sentence (increase of monsoon and vegetation in the late Holocene) wit hthe previous results (reduced mid-Holocene monsoon) [Matthieu Carré, France]	Accepted - "However" was removed from the sentence.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7023	54	2			I don't understand this. What are paleo-equilibrium lines of glacier? The wording needs to be changed to make the sentence understandable. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - "Paleo-equilibrium lines" was replaced with "equilibrium lines"
7025	54	3			Change "slowdown of AMOC" to "AMOC slowdown" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
7027	54	4			Insert "the" before "Tropical". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
7029	54	7			Perhaps replace "in the last decades" with "recent decades". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
84029	54	15	54	17	Chagas and Chaffe 2018 (DOI: 10.1029/2018WR022947) provides evidences of changes in observed rainfall in south Brazil. [Marco Tulio Cabral, Brazil]	Accepted - This reference was included.
7031	54	20			The expression "with changes in Pacific SST" does not make clear whether the changes are an internal mode or externally forced. More detail is needed. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - This has been clarified. The increased WDF in western Amazonia is related with warmer Pacific SSTs and a stronger Walker cell.
7033	54	25			Insert "the" before "late 1990s". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
7035	54	27			Regarding the "warming and cooling of the tropical Atlantic and eastern Pacific", do these relate to the AMV/AMO and PDV/IPO? Since these modes are discussed extensively in the report, the links should be made explicit if they exist. Such work has been assessed for the SESA region on page 80 of Chapter 10, for example. Cross references with the attribution case study in that chapter could be made. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - This was clarified in the text: "... which has been linked to a strengthening of the Walker circulation over the Pacific and Atlantic Oceans due to strong anomalous tropical Atlantic warming (partly AMV/AMO-related) and eastern Pacific cooling (partly PDV/IPO-related) "

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3191	54	28	54	29	In relation to Hadley Cell intensification, Segura et al (2020. doi: 10.1007/s00382-020-05132-6) documented a change in precipitation-driving processes over the southern tropical Andes/western Amazon, including increasing summer precipitation over the Altiplano since 1982. The study documented that since the 2000s rainfall variability over the Altiplano is no longer related to intensity of the Bolivian High. This is a major change in the rainfall related atmospheric process, which have implications for Paleoclimate and future rainfall projection studies. [Jhan Carlo Espinoza, France]	Noted - This reference was included.
19211	54	30	54	30	What the authors mean by the following sentence " Positive and significant (negative but not significant) trends" [Mohamed Deyab, Egypt]	Noted - For clarification, this sentence has been rephrased to "Positive and significant trends in precipitation were observed over south-eastern Brazil during 1902-2005 while negative but not significant were detected in central Brazil"
22347	54	30	54	37	Assessment needs to go on to interpret causes if it can and this then needs to be reflected in revisions to the assessment summary. [Peter Thorne, Ireland]	Noted -We now include a sentence related with D&A for the South American monsoon.
3189	54	32	54	34	Please, modify this sentence as follow: "The Peruvian Amazon does not reveal significant changes in mean annual rainfall during 1965–2007, although significant rainfall and runoff diminution has been observed during the dry season (Lavado et al., 2013, Espinoza et al., 2011 doi:10.1029/2011GL047862; Ronchail et al., 2018, 10.1016/j.ejrh.2017.11.008)." [Jhan Carlo Espinoza, France]	Accepted - The sentence has been rephrased as suggested.
7037	54	34	54	37	Have no D&A experiments been analysed for the South American monsoon, for GHG- and aer-only forcing? Some GHG attribution for SESA is discussed for example on page 81 of chapter 10 in the attribution case study. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted - We now include a sentence related with D&A for the South American monsoon.
5483	54	39	54	41	ARs also affect East Asia during warm season. The following statement and references may be added to the end of this paragraph: "ARs also affect East Asia (EA) strongly during the period from late spring to summer (Kamae et al. 2017a,b; Kim et al. 2020). [Jinwon Kim, United States of America]	Accepted
7019	54	39			Remove apostrophe from 1970's [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
40079	54	44	54	44	Define in the glossary as a subterm of 'monsoon' [TSU WGI, France]	Accepted - A definition for this monsoon was included in the Glossary: The Australian-Maritime Continent monsoon occurs during austral summer, with the large-scale shift of the Inter-Tropical Convergence Zone into the Southern Hemisphere. Over northern Australia, the monsoon season generally lasts from December to March and is associated with inflow of moist west to north-westerly winds, producing convection and heavy precipitation. Over the Maritime Continent, the main rainy season south of the equator is centred on December to February with north-westerly monsoon flow at low levels. The islands north of about 1- 2°S have a weaker seasonal cycle.
109403	54	44			Same comment as South American monsoon. Please refer to D'Agostino et al. (under review). [Roberta D'Agostino, Germany]	Accepted - The reference was included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98075	55	1	55	5	Add at the end: “or anthropogenic forcing in general (Knutson and Zeng 2018).” Note that Knutson and Zeng (2018) also find detectable increases in north-central Australian rainfall for the period 1901-2010, which they inferred was partly attributable to anthropogenic forcing, so the increasing precipitation trend behavior there is apparently not just limited to the past 50 years, nor is it fully explainable by natural variability as simulated by CMIP5 models. Refs: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637 https://doi.org/10.1175/JCLI-D-17-0672.1. [Thomas Knutson, United States of America]	Taken into account – Knutson and Zeng study which compares only NAT versus ALL forcings so no specific attribution to trends in aerosols is possible. Text has been clarified to read: 'Other studies find that model simulations that include anthropogenic aerosols (Rotstayn et al., 2007; Rotstayn et al., 2012; Dey et al., 2018) are better able to capture observed Australian monsoon rainfall trends than simulations with natural or greenhouse gas forcing only'.
7041	55	1	55	5	This doesn't really read like an assessment, particularly the part of the sentence, "others finding a possible contribution from". What is the view of the LA team on whether this is a viable finding? Can the contribution from aerosol be quantified? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - text revised to clarify that there is currently no consensus on the cause of trends in Australian monsoon rainfall. The studies which find a contribution from anthropogenic aerosols are based on historical single forcing model simulations, which provide a better match with observations when aerosols are included versus natural or GHG forcing only. Therefore it is not possible to directly quantify the influence of aerosols from these studies.
7039	55	3			Natural climate variability is named here but the driver is not stated. What mode has it been attributed to (PDV, AMV) or is it unexplained? We should not prejudice that a policymaker is interested in a long-term/multi-decadal trend only if an anthropogenic driver can be identified. It is just as useful to know if blame can be apportioned to a specific mode, since this assists with understanding how it might change in the future. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	
88479	55	5	55	5	This recent study might be helpful to mention at the end of this sentence, including text added such as "and more intense convective rainfall from thunderstorms (Dowdy 2020)." Reference: Dowdy, A.J., 2020. Climatology of thunderstorms, convective rainfall and dry lightning environments in Australia. Climate Dynamics, 54(5), 3041-3052, https://doi.org/10.1007/s00382-020-05167-9 [Andrew Dowdy, Australia]	Accepted - Amended to add "There is also a trend towards more intense convective rainfall from thunderstorms over northern Australia (Dowdy 2020)" after discussion of mean monsoon rainfall trends.
7043	55	19			Remove the words "the period" from the sentence as they are unnecessary. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - amended to remove "the period".
7045	55	21	55	22	If the TBO mode has already been defined/described in the AR6 then give a cross-reference to it. If not then you should cite an appropriate paper from G. Meehl's back catalogue on the subject. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Amended to add citation: Meehl, G. A., 1994: Coupled land–ocean–atmosphere processes and South Asian monsoon variability. Science, 266, 263–267, https://doi.org/10.1126/science.266.5183.263.
22349	55	21	55	25	Given that this is the first mention of the TBO and that it is relatively speaking esoteric (I had to look it up) then more detail on what it is and why it matters is required here than is presently given. Should it be introduced earlier than this? And given it apparently matters for other regional monsoons why is it not mentioned in their assessments? The regional monsoon assessments need to hang together better. [Peter Thorne, Ireland]	Noted - TBO is defined in its first use and is included in the Glossary
112229	55	27			After covering each individual monsoon, it might be helpful to have another section explaining the key differences and similarities between these monsoons. [Rutger Hofste, Netherlands]	Noted - A comprehensive summary of regional monsoons is presented in the Executive Summary

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7047	55	28	55	29	Change "anthropogenic forced" to "anthropogenic forcing". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – Amended to read: '…natural variability of anthropogenic forcing'
12855	55	32	56	25	Is there any literature on the contribution of TCs to transport of moisture in the atmosphere to different latitude bands that needs to be assessed? (Or to the contrary, i.e. on the transport of moisture away from certain regions by TCs?). How does this perform in models and does resolution make a difference etc.? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. There is literature on transport of moisture by Tropical Cyclones, for e.g., over East Asia. This is now included in the revised version. Thank you.
87533	55	32	56	27	"The predicted global average annual loss associated with the wind and storm surge of TropicalCyclones (known Hurricane over Atlantic regions), earthquakes, tsunamis and floods is US$314 billion. As the intensity and timely onset of Monsoon rains have a great influence on the agriculture and aviation of many populous nations of South-East Asia.; so the case is of Hurricanes causing devastation over the Se-Atlantic & Caribbean Oceanic regions comprising American & Caribbean Nations. August- September of year 2017 were the most devastating months in the history of American & Caribbean nations inflicted by mainly three storms namely 'Hurricane Harvey' ,'Hurricane Irma' & 'Hurricane Maria. Its imperative to know how and why these Storms form, intensify and what are the morphological and thermodynamic properties responsible for the energetic and movement." As regards, Northern Hemisphere, though the dense network of stations is available in mid-latitude but, the tropics still face the paucity of data, specially over the Oceans and high mountain terrain .Though, the scarcity has been alleviated by Satellite imagery and by several regional scale observational programs but, very little has been done with the Percentage Cloud Coverage, observed Cloud Peaks, tropical Cloud Clusters and their relation with Pressure perturbations in the main Southwesterly flow in relation with tropical cyclones(Hurricanes). The qualitative and quantitative estimation of the precipitation involving time and space variation constitute an important aspect oflong, medium and short range forecasting on different scales viz. Global, macro, synoptic, meso and micro. Although, any specific event in any given area dependent only partially on the local structure of the atmosphere inside the region but, the major central comes from the broad scale situations existing over very long areas. The broad scale situation is most clearly understood most readily by the current aloft as well as when viewed in their hemispheric setting. With this end in view; the remote sensing has been used to study the Energetics of Tropical Cyclones (known Hurricane over Atlantic regions), over South-SE-Asian & SE-Atlantic regions by studying the Cloud clusters & mesoscale convective systems during Monsoon & the Autumn (Jul-Oct) using Satellite imageries, and cloud computing for real-time analysis.	Noted with thanks.
37743	55	32	57	28	The EPTG is less in warm periods so the decline in frequency of cyclones and hgurricanes is evidence of global warming and an increase in frequency and severity can be expected in ice ages (Cf also work of Pro H Lamb-founder Hadley Centre, England)) [Howard Brady, Australia]	Noted with thanks.
53339	55	32			May be remind the reader that the current-generation global climate models are still unable to simulate the most intense cyclones given their limited resolution? [Hervé Douville, France]	Taken into account. Thank you. The FGD mentions that limited agreement across models and data heterogenity provide low confidence in assessing detectable changes in tropical cyclones globally (pages 56-57).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43771	55	35	55	35	Please add Hristova-Veleva et al., 2020 at the end of the first sentence. The reference is: Hristova-Veleva, S.M., P.P. Li, B. Knosp, Q. Vu, F.J. Turk, W.L. Poulsen, Z. Haddad, B. Lambrigtsen, B.W. Stiles1, T. Shen, N. Niamsuwan, S. Tanelli, O. Sy, E. Seo, H. Su, D.G. Vane, Y. Chao, P.S. Callahan, R.S. Dunbar, M. Montgomery, M. Boothe, V. Tallapragada, S. Trahan, A.J. Wimmers, R. Holz, J.S. Reid, F. Marks, T. Vukicevic, S. Bhalachandran, H. Leighton, S. Gopalakrishnan, A. Navarro, and F.J. Tapiador: An Eye on the Storm: Integrating a Wealth of Data for Quickly Advancing the Physical Understanding and Forecasting of Tropical Cyclones. Bull. Amer. Meteor. Soc., in press, https://doi.org/10.1175/BAMS-D-19-0020.1 [Francisco Tapiador, Spain]	Accepted. Thank you. This reference is included.
22351	55	35	55	41	These findings were related principally to the intensity aspects not to the hydrological aspects and thus are out of scope. To avoid conflict with chapter 11 you may need to remove or alter this? Regardless you should focus only on the hydrological cycle relevant findings. [Peter Thorne, Ireland]	Taken into account. Thank you. This part is altered
113359	55	37	55	37	Isn't the North Atlantic extra-tropical (sect. 8.3.2.8)? [Diego Miralles, Belgium]	Thank you. Taken into account. The text on tropical cyclone over the North Atlantic has been dropped in the FGD.
53341	55	46			There is also a better understanding of tropical cyclone sensitivity to both GHG and aerosols (Sobel et al., 2019) and medium confidence that (...) [Hervé Douville, France]	Thank you. This point on the better understanding of the TC response to anthropogenic forcing is included.
20147	55	50	56	15	Page 55 lines 50-51: spelling and grammar to be checked. Page 56 lines 13-15: poor English [philippe waldteufel, France]	Thank you. The grammatical mistakes are corrected and the sentences are improved.
12851	55	50			Coterminous is a rather esoteric word. Could contiguous be used instead? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Thank you. This is amended.
12853	55	51			Something wrong with the grammar here; I don't understand the intended meaning of "have become more probably" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Thank you. The grammatical mistakes are corrected.
116747	55		55		SROCC chapter 6 could be used as a starting point. Coordination with ch 11 is needed to avoid duplication for cyclones. [Valerie Masson-Delmotte, France]	Taken into account. SROCC is used as a starting point for the tropical cyclone section in chapter 11, which this section is well cooridinated with.
109405	55				I expected some information about the South African monsoon or East African Monsoon. Is there any? I suggest to include a brief section about it. [Roberta D'Agostino, Germany]	Noted. Observed (and projected) changes in tropical regions with a strong seasonality like South Africa and East Africa, not classified as "canonical" monsoon regions, are assessed in Box8.2.
24203	56	1	56	6	The study by Zhang et al. (2018, Urbanization exacerbated the rainfall and flooding caused by hurricane Harvey in Houston. Nature 563, 384–388. doi:10.1038/s41586-018-0676-z.), which is discussed and cited in Ch. 11, should probably be cited and discussed in this paragraph. [Rhawn Denniston, United States of America]	Thank you. The Zhang et al. (2018) study is cited.
65083	56	1	56	7	I am concerned that the assessment of TC translation speed change is different in this chapter than that in Chapter 11. There the assessment is "medium confidence" that TCs have slowed, with "low confidence" of a global signal due to data heterogenity , but here in Chapter 8, the assessment is "low confidence" of TC slowdown, based in part based on one of the same global studies. [Laurie Agel, United States of America]	Taken into account. Thank you. This problem is resolved in the FGD by ensuring consistency in the assessments between 8.3.2.5 and 11.7.1 with regard to the confidence levels for the observed changes in TC translation speed over the U.S. (medium confidence) and global signal (low confidence).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29021	56	4		9	Check for consistency with 11.7.1.2 ("There is medium confidence that TC translation speed has slowed detectably over the U.S. since 1900, but low confidence for a global signal because of the potential for data heterogeneity. There is low confidence in the cause of the slowdown in any region due to a lack of robust agreement among models that simulate TCs, although the slowdown is consistent with theory and modelling studies that indicate a general slowing of atmospheric circulation with warming") and 8.2.3.5 ("a weakening tropical circulation that reduces tropical cyclone system speed (Kossin, 2018; Lanzante, 2019; Moon et al., 2019b) thus amplifying thermodynamic intensification of rainfall (Chauvin et al., 2017) though observational evidence is weak (Chan, 2019)." [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Thank you. In the FGD, it is ensured that the assessment in 8.3.2.5 is consistent with 11.7.1 for confidence levels regarding slow down of TC translation speed over the U.S. (medium confidence) and global signal (low confidence).
29023	56	11		15	The lines on aerosol seem more appropriate to mechanisms (e.g. Section 2/Box 8.1) so could be removed since the observational evidence is of the mechanism rather than changes which is the focus here [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. In the FGD, we have dropped the lines on the invigoration of peripheral rain bands by ingestion of aerosols in tropical cyclones.
22353	56	13	56	15	Tropical cyclone intensity is not in the scope of the chapter and this should be covered in chapter 11 instead. [Peter Thorne, Ireland]	Taken into account. As suggested by the reviewer, the text on tropical cyclone intensity i.e., "storm's maximum wind speed up" has been dropped in the Chapter 8 FGD.
22355	56	17	56	19	Intensity of tropical cyclones is not in chapter scope and should be covered in chapter 11 instead. [Peter Thorne, Ireland]	Taken into account. As suggested by the reviewer, the text on tropical cyclone intensity i.e., "there is a poleward migration of ..." has been dropped in the Chapter 8 FGD.
17241	56	17	56	19	Insert reference to Sharmila and Walsh (2018) for poleward shift of Southern Hemisphere TCs. Sharmila, S. and Walsh, K. J. E. (2018). Recent poleward shift of tropical cyclone formation linked to Hadley cell expansion. Nature Climate Change 8: 730–736 [Joelle Joelle Gergis, Australia]	Accepted. The suggested reference for the poleward shift of the SH TC is included.
53343	56	21	56	22	Rephrase, clarify and further expand consistently with CH11? For instance: In agreement with theoretical understanding, it is likely that the proportion of stronger TCs has increased globally over the past 40 years. It is very likely that the average location where tropical cyclones reach their maximum intensity poleward of their current climatology and/or move at a slower speed in some areas since the early 20th century, with potential implications for related precipitation accumulation and flood risk. [Hervé Douville, France]	Taken into account. Thank you. This sentence is dropped in the FGD following the comment No. 22355
103709	56	21	56	25	perhaps make a refernce to windspeed observations in others Chapters? [Philippe Tulkens, Belgium]	Taken into account. Reference to windspeed changes in other chapters is included. Thank you.
89081	56	22		23	A statement about water vapor increase does not belong in the section on tropical cyclones, but elsewhere. [Angeline Pendergrass, United States of America]	Noted. The reference is being made to water vapour changes associated with TCs.
65789	56	28	57	18	Suggest including a discussion of the mechanism of quasi-resonant wave amplification (QRA) (Petoukhov et al. 2013, 2016) and its role in extreme weather events (Mann et al. 2018, Kornhuber et al. 2019, 2020). In boreal summer, QRA was shown to several heat and rainfall extremes (Kornhuber, K. et al. 2018). Mann et al. (2018) found that in CMIP5 QRA events are likely to increase by ~50% this century under business-as-usual carbon emissions, with some predicting tripling of QRA. [Kushla Munro, Australia]	Accepted. A discussion of QRA has been added, although focused on the relevance to observed changes, the focus of this subsection, rather than on future behaviour.
22359	56	28			The section 8.3.2.6 is written entirely about dynamics. There is no assessment whatsoever of hydrological cycle impacts. Arguably the section is thereby out of scope. If retained the section needs to be considerably refocussed to concentrate upon the hydrological cycle implications so that it is seen as in scope. [Peter Thorne, Ireland]	Accepted. The material has been refocused on the hydrological impacts, although material on the changes in the stationary waves, themselves, has been retained as it is needed to consider the hydrologic impacts.
53345	56	28			What about the model ability to simulate the present-day stationary waves, their seasonality and variability? [Hervé Douville, France]	Accepted. A discussion of model ability to simulate stationary waves has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
103711	56	30	56	31	Stationary waves not only results from land-sea interaction and topography. Internal Rossby wave dynamics create quasi-stationary waves (see work going back to Gustav Rossby in 1950s) [Philippe Tulkens, Belgium]	Accepted. The statement has been broadened.
29025	56	30		46	Intro of stationary waves reads like a text book and can in my opinion be condensed to a line or two. Moreover it is not clear what the implications for the water cycle changes are. This section could be combined with blocking which seems well summarised. There is also the possibility to link to 8.2.2.2. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The introduction has been shortened and the implications for the water cycle have been highlighted.
22357	56	41	56	43	This has been covered in some earlier chapters already and yet there is no reference to this. Also, this characterisation needs to be cross-checked against the more substantive assessment considered therein. From memory the main assessment was in chapter 7 but it was also covered potentially in chapters 3 and 4. [Peter Thorne, Ireland]	Accepted. Cross-chapter Box 10.1 summarizes the influence of the Arctic, and is now referred to as the main source for assessment of the Arctic influence on stationary waves.
70991	56	41	56	50	This discussion misses an important mechanism. In both hemispheres, there is an important influence of the stratospheric polar vortex. In the NH, even if the vortex variations are axisymmetric, the tropospheric response is not (Hitchcock and Simpson 2014 doi: 10.1175/JAS-D-14-0012.1), and has first-order implications for winter hydroclimate changes (Zappa and Shepherd 2017, already cited in this chapter). In the SH the variations are more zonally symmetric, but still not entirely so, and there are interesting regional variations in summer hydroclimate (Mindlin et al. 2020 doi: 10.1007/s00382-020-05234-1). [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The mechanism is potentially important for future water cycle changes but none of the references provide evidence directly relating to observed changes, the focus of this section.
103713	57	1	57	2	:"....increased over the North Atlantic region (Overland et al., 2015), possibly as a result of 1 weakening of theNorth Atlantic storm track and transfer of energy to the mean flow and stationary waves": Make clear that this is about winter circulation. [Philippe Tulkens, Belgium]	Not applicable. The text is no longer included.
12857	57	8			Here and several places elsewhere: replace "Artic" with "Arctic" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
22361	57	13	57	18	And the relevance to the hydrological cycle and thus the chapter charge is what? This needs to be rewritten to give a hydrological cycle implication and the preceding text needs modifying accordingly. [Peter Thorne, Ireland]	Accepted. The section has been rewritten to more clearly convey the relevance to the water cycle. However, please note that this is one of several subjects that is known to be of importance to the water cycle and so needs to be included in the chapter but where the literature specifically on the water cycle impacts is limited.
21065	57	21	57	48	In this section about atmospheric blocking there is no mention about blocking in the SH. Please, be clear in sentences that you are talking about the NH (e.g. lines 31 and 34 and summary). Also, link to 2.3.1.4.3 and 3.3.3.3. [Marcelo Barreiro, Uruguay]	Accepted - Northern Hemisphere was added in several places through the text of this section. Links to Chapter 2 and 3 were added.
22363	57	21			As written this section adds little if anything to existing segments on blocking in chapters 2 and 3. Worse still it does not cite them or start from those findings but rather performs its own substantive assessment. This must be addressed in FGD. Either delete this section or starting from the findings of chapters 2 and 3 proceed to undertake additional assessment necessary to support hydrological cycle aspects. What is not advisable is to repeat assessments already undertaken in earlier chapters. It simply invites readers to play spot the difference to the potential detriment of the report as a whole. [Peter Thorne, Ireland]	Rejected - This section considers the assessment of the changes in the atmospheric blockings for the Northern Hemisphere with the focus on the influence of blocking regime to precipitation which is not presented in Ch.2 and 3. Ch3 is mostly focused on SH blocking and Ch2 provides regional examples of blockings in the Northern and Southern Hemispheres. General conclusion is the same “no robust trend in atmospheric blockings has been detected “. Corresponding references to Ch2 and 3 are given.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53347	57	21			Again attribution also needs reliable models. May be add a sentence about the model evaluation and the possible progress made by CMIP6 models? [Hervé Douville, France]	Rejected - Assessment of model evaluation is given in Section 8.4.2.7." CMIP6 multi-model means show substantial decreases in blocking activity over Greenland and the North Pacific for SSP7.0 and SSP8.5." Also I didn't find any peer-review literature about the progress in describing of blocking in CMIP6.
80313	57	23	57	28	This text could be removed if needed [Paola Arias, Colombia]	Rejected. This introductory paragraph is important to make a link between blocking and precipitation.
113361	57	28	57	28	and heatwaves' (e.g., Miralles, D. G., Teuling, A. J., van Heerwaarden, C. C. and Vilà-Guerau de Arellano, J.: Mega-heatwave temperatures due to combined soil desiccation and atmospheric heat accumulation, Nature Geosci, doi:10.1038/ngeo2141, 2014) [Diego Miralles, Belgium]	Accepted - "and heatwaves" and corresponding reference were added in the text
103715	57	30	57	42	This section is a bit confusing: Different mechanisms and different seasons are mixed together. These should be clearly differentiated (blocking mechanisms are dependent on season). Also important literature is missing, eg Pfahl et al, Nat Geo, 2015; Kornhuber et al, J Clim, 2017; Kornhuber et al, ERL, 2019; [Philippe Tulkens, Belgium]	Noted - This paragraph is organizing as the description of different mechanisms, which are different for different seasons and this explicitly mentioned in the first half of this paragraph and the second half of this paragraph gives the description of the sensitivity of results to the choose of blocking indexes. Suggested references were added.
205	57	36	57	38	This is a critical point that runs the risk of being obscured by the vocabulary of the sub-field; "blocking frequencies" and "blocking index" make sense in papers on the subject but may obscure the larger point about attribution and correlation to those outside the subfield. [Patrick Orenstein, United States of America]	Accepted. Frequency was replace by occurrence and now this written as "...linked to an increase in the occurrence of blocking".
4175	57	39	57	42	This paragraph elaborated some of mechanisms about the blocking variability under external forcings. At the theoretical level, a simple but significant work by Luo et al.(2019, JAS, "A Nonlinear Theory of Atmospheric Blocking: A Potential Vorticity Gradient View") presented a dynamics explanation of the Arcitic warming modulating the blocking activity in the view of PV gradient. This study think the Arctic warming can weaken the meridional gradient of potential vorticity, which means the blocking system has weak dispersion, hence the blocking can maintain a long lifespan. I think it's meaningful for publics and researchers to understand the linkage of the blocking variance and Arctic warming and this theory is based on rigorous mathematical foundation. [Wenqi Zhang, China]	Accepted. The paragraph describing the mechanism is written as following and reference is included: "Also, a weakening of the zonal wind, eddy kinetic energy and amplitude of Rossby waves in summer in the Northern Hemisphere (Coumou et al., 2015)(Coumou et al., 2015, Kornhuber et al., 2017, 2019) as well as an increased waviness of the jet stream associated with Arctic warming (Francis and Vavrus, 2015, Pfal et al., 2015, Luo et al., 2019)."
100859	57	40	57	42	Davini and D'Andrea 2016, 2020 and Shiemann et al 2017 could be added to the reference list. Also Davini and D'Andrea 2020 show that models underestimate summertime Greenland blocking everywhere over the Northern Hemisphere. Davini, P., and F. D'Andrea, 2016: Northern hemisphere atmospheric blocking representation in global climate models: Twenty years of improvements? Journal of Climate, 29 (24), 8823– 8840. Schiemann, R., and Coauthors, 2017: The resolution sensitivity of Northern Hemisphere blocking in four 25-km atmospheric global circulation models. Journal of Climate, 30 (1), 337–358. [Corti Susanna, Italy]	Accepted - Such sentence and corresponding reference were added in the text: "However, over the Pacific Ocean it was found large improvements in the models simulation of blocking for the last 20 years (Davini and D'Andrea, 2016)."

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
100857	57	44	57	44	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 (Hanna et al. 2018; Davini and D'Andrea 2020 J Clim under revision) that show instead a decrease in frequency. 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. The difference between Northenr hemisphere blocking trends and variability in summer and winter should be mentioned. In particular the signal over Greenland in summer (increase of frequency) has been found signicant at 5% 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. [Corti Susanna, Italy]	Accepted - Statement about Greenland blocking and corresponding references were added.
65791	57	44	57	48	Suggest including a reference to atmospheric blocking in the Southern Hemisphere, e.g. Patterson et al. 2019 which showed a decrease in blocking frequency in CMIP5 models with future anthropogenic forcing, particularly in the Australia-New Zealand sector). [Kushla Munro, Australia]	Noted. Information about blocking in the Southern Hemisphere can be found in Chapter 2 (Section 2.3.1.4.3) and in Chapter 3 (Section 3.3.3.3) but the reference was added in the description of models simulation of blocking: "However, over the Pacific Ocean it was found large improvements in the models simulation of blocking for the last 20 years (Davini and D'Andrea, 2016, Patterson et al., 2019)"
64967	57	45	57	45	is a confidence statement for "no clear link" appropriate? [Johannes Quaas, Germany]	Accepted - Statement about "no clear link with Arctic amplification" was remove .
103717	57	45	57	48	This lack of signal despite an overall decline in the projected occurrence of blockings (Section 8.4.2.6) may be the result of the strong internal variability and/or of the competing effects of low-level Artic amplification and upper-level tropical amplification on the equator-to-pole temperature gradient (medium confidence).: This is speculative and does not deserve the level "medium confidence". Its arguably more likely that it has to do with model biases. [Philippe Tulkens, Belgium]	Noted. "May be" was replaced by "likely".
64969	57	45	57	48	"may be" and "medium confidence" seems to be contradictory [Johannes Quaas, Germany]	Accepted. "May be" was replaced by "likely"
17243	57	51	57	51	To improve Southern Hemispehre coverage, please insert sentence on extratoprical cyclones from Australia: Dowdy, A. J., Pepler, A., Di Luca, A., Cavicchia, L., Mills, G., Evans, J. P., Louis, S., McInnes, K. L. and Walsh, K. (2019). Review of Australian east coast low pressure systems and associated extremes. Climate Dynamics 53 (7): 4887-4910. Another paper worth considering is Dowdy, A. J. and Catto, J. L. (2017). Extreme weather caused by concurrent cyclone, front and thunderstorm occurrences. Scientific Reports 7: 40359. [Joelle Joelle Gergis, Australia]	Accepted. Reference to Dowdy et al 2019 was added.
11107	57	51	57	51	In Section 8.3.2.8, it not appropriate to have only Section 8.3.2.8.1, but no 8.3.2.8.2. [Wen Wang, China]	Noted - We now include section 8.3.2.8.2
65793	57	51	59	43	Suggest clarification on how ETC frequency or intensity is expected to change (see, e.g., Zappa et al. 2013, Chang 2017, 2018). [Kushla Munro, Australia]	Accepted. Changes in frequency and intencity of ETC is discussed in FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
20511	57	51	59	43	Excepting the poleward shift of storm tracks, it does not seem possible to say much about what is called here "cyclone activity" (what is it?) Do we have adequate indices? The reader can only be stricken by the fact that, in this discussion, no mention is made of what is felt as most important for the man in the street: winds and precipitation [philippe waldteufel, France]	Noted - Thank you for this important question, indeed, the chapter is lacking more precise definition of the cyclone activity. In many places, we have added such terms as cyclone numbers, cyclone depth, cyclone deepening rate, linear trends of the number of cyclones, the precise quantitative measure of the poleward deflection and made the chapter more clear.
12859	57	51			There is potential for citing this paper: An Overview of the Extratropical Storm Tracks in CMIP6 Historical Simulations by Matthew D. K. Priestley, Duncan Ackerley, Jennifer L. Catto, Kevin I. Hodges, Ruth E. McDonald, and Robert W. Lee https://journals.ametsoc.org/doi/abs/10.1175/JCLI-D-19-0928.1 [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Citation is added, thank you.
22369	57	51			This section has almost nothing to do with the hydrological cycle and is massively redundant with assessments already performed in chapters 2 and 3. This is hugely problematic because it invites readers to play spot the difference. It should start from chapter 2 and 3 findings and then focus upon adding additional detail necessary to understand the hydrological cycle changes. Changes in general circulation are the charge of chapters 2 through 4. The ARs subsection is in scope and should be retained. The rest should be replaced with pointers to the assessments already performed by chapters 2 and 3 and any material not included therein merged with the existing texts of those chapters. [Peter Thorne, Ireland]	Accepted. Synergy and consistency with Ch2 and 3 were provided.
79423	57	53	57	55	Hi Olga. A bunch of comments on ETCs that I hope might help to improve the section. Please, let me know if anything is unclear or if I can help with something. " low confidence in long-term changes in the intensity", and frequency too? Also, I guess the just say "reanalyses" instead of "reanalysis 20CR". Surface pressure instead of pressure? SREX was published before AR5 so it could not confirm it right? [Alejandro Di Luca, Australia]	Noted - (1) By the low confidence in the long-term changes we mean here the uncertainties among reanalyses, we have rephrased it according to the new estimates of the seasonal number of cyclones of different intensities. (2) done (3) by pressure we mean the gradients of the sea level pressure, which is typically used we the cyclone activity assessments. We have now replaced " pressure" with "sea level pressure" (4) right
22365	57	53	58	4	These aspects were substantively assessed in chapters 2 and 3. Where are the cross-references to these? Why does the current chapter not start from the basis of these antecedent assessments? [Peter Thorne, Ireland]	Noted. Indeed these aspects are considered in CH2 and CH3, but this was a chapter decision to have this also here. This particular comment need to be discussed in a view of #22369 pointing to the whole content of the sub-section
69491	57	53	59	31	I think this section should lead with the difficulties in determining trends in ETC from ranalysis rather than beginning with a laundry list of what the trends are, and then returning to the question of whether the trends are real or not. [Martin Singh, Australia]	Not applicable. Text was consideraly re-written and structure of this section was changed.
116749	57		57		On blocking, consistency is needed across chapters (2, 4, 10) and 8 (on detection, understanding, projections, confidence levels). [Valerie Masson-Delmotte, France]	Not applicable. The fact that blocking has its own separate section is now mentioned in the introductory paragraph.
79427	58	3	58	4	"except for the ERA20C reanalysis". Maybe add ERA5? [Alejandro Di Luca, Australia]	Accepted - Yes, ERA5 was added, thank you.
79425	58	6	58	7	So, if incosistensies in reanalyses data remain, where the progress has been made? Also, the key question here is whether progress has been made in quantifying trends over the past century instead of charaterising better the climatology of ETCs, right? [Alejandro Di Luca, Australia]	Noted - The progress mostly has been made into the assessment of the uncertainties among different datasets and methodologies. Here we demonstrate the common signals in extratropical cyclone activity (number of cyclones of different intensities and displacement of the common path of cyclone trajectories)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29027	58	7			can remove "The Intercomparison of Mid-Latitude Storm Diagnostics project based on ERA-Interim" which is not needed. This section is long and contains quite a few pre-AR5 references so could benefit from being condensed to what is the assessment (number, intensity, hemispheric differences; guided by the summary statements) with references supporting this with consensus or otherwise supporting the confidence. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Thank you for this comment. The IMILAST project is of the achievements of the last decade and important project, that allowed to clarify the uncertainties in diagnosis of the extratropical cyclones derived from different tracking methodologies. Thus, we decided to keep this reference
12861	58	13	58	14	This sentence seems to be missing something and doesn't work in a standalone manner. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. "With" was replaced by "using".
79429	58	13	58	14	Not sure I understand the sentence: "Grieger et al., (2018) with ERA-Interim and ERA5 showing weak upward trends contrasting the other reanalyses." [Alejandro Di Luca, Australia]	Accepted - This sentence was rewritten.
43189	58	13		14	Read "Grieger et al., (2018) with ERA-Interim and ERA5 shown weak upward trends " rather than "Grieger et al., (2018) with ERA-Interim and ERA5 showing weak upward trends " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted - Done.
12863	58	15			The Tilinina (to be submitted) study should not have been included here since it cannot be assessed (see also several instances later on this and the following pages). [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Thank you, we have changed the status of the paper to "submitted"
53349	58	18	58	19	Another recent study (Chang et al., 2018) however indicates a decreased NH summer cyclone activity from 1979 to 2014, which was underestimated by most CMIP5 models but has caused a decrease in cloud cover and higher maximum surface temperatures, thereby contributing to increase the atmospheric water demand. [Hervé Douville, France]	Accepted. Reference was added
69493	58	18	58	28	There is no mention of the fact that the different reanalyses give vastly different climatological cyclone numbers in the Southern Hemisphere (Fig. 8.13). Surely this is important for assessing the likelihood that the trends are real. [Martin Singh, Australia]	Noted - Indeed, the spread among reanalyses in Southern Hemisphere is stronger than in Northern Hemisphere. We have added a sentence stating this fact -"It is important to notice, that overall uncertainty in estimates on the seasonal number of cyclones among different reanalyses in stronger over the Southern Hemisphere, than over the Northern Hemisphere, this statement holds for the number of deep cyclones (<980 hPa)".

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21067	58	20	58	24	This sentence should be rewritten as Wang et al (2016) published before Tilinina et al 2019. In fact, Reboita et al (2015) found this first (Clim. Dynamics 45, 1929-1944). Also, Reboita et al (2015) found that there are different regional trends depending on the ocean basin considered. [Marcelo Barreiro, Uruguay]	Noted - we agreed our chapter structure as framing (8.1), physical expectations (8.2), understanding observed changes (8.3), projections (8.4), challenges (8.5), abrupt changes (8.6) at the FOD stage and consider that this our narrative approach is distinct and complements other chapters. Section 8.2 deals with physical expectations evidenced by fundamental physics underpinning simple to complex models and supported by observational evidence. This primarily focuses on idealised modelling, experiments and observations that elucidating physical processes. Of course, it is impossible and nonsensical to avoid completely evidence provided by long term observations and modelling of future changes. These are to some extent touched on in earlier chapters as well as being dealt with in later sections but this overlap is kept to a minimum and was a consideration in further refinements to this section, the structure of which was appreciated by other reviewers.
129195	58	20			Multiple mentions of "Tilinia et al. 2019 (to be submitted)" in this section are worrisome. Update the paper's status. [Trigg Talley, United States of America]	Noted - Thank you. We have updated the status of the paper, now it is Tilinina et al., 2020, submitted.
43191	58	25			Read "Grieger et al. (2018)" rather than "Grieger et al., (2018)" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted - Done, thank you.
12865	58	26			Missing degree symbol. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Done, thank you.
43193	58	50			Read " (Krueger et al., 2013; Wang et al., 2013b; Varino et al., 2019)." rather than " (Wang et al., 2013b; Varino et al., 2019) (Krueger et al. 2013)." [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted - Done, thank you.
69489	58	51	58	52	demosntrated --> demonstrated [Martin Singh, Australia]	Accepted - Fixed
12867	58	53			Change "hinting on" to "hinting at" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Done, thank you.
237	58	58	6	28	Avoid to cite studies which have not been submitted. The two parapgraphs rely too much on a single study which has not even been submitted for review (Tilinina et al. 2019 to be submitted). This is not good scientific practise [Sebastian Schemm, Switzerland]	Noted - Thank you. We have updated the status of the paper, now it is Tilinina et al., 2020, submitted.
239	58	58	6	28	The increase in the number of storms is in Chapter 2, page 53, line 37, attributed to Chang and Yau, 2016; Wang et al., 2016, while here it is Tilinina et al. 2019 (to be submitted) is referenced. This could confuse the reader. [Sebastian Schemm, Switzerland]	Noted - Thank you. We have updated the status of the paper, now it is Tilinina et al., 2020, submitted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
243	58	58	43	44	This sentence about the number of cyclones and fronts is erroneous in several ways. (a) The citation of Schemm et al. (2016) is incorrect, it is Schemm et al. (2017; doi:10.1002/2016GL071451); (b) The citation of Schemm et al. (2017) is also missing in the list of references; (c) Schemm et al. (2017) argue that the trend in the number of extremly strong fronts is not due a general increase in the total number of fronts (p. 554: "... the annual numbers of analyzed fronts and front grid points ... are not a consequence of an increase in the number of fronts."), but it is rather an increase in the strengths of the strongest fronts. . Please correct all three aspects (a,b, and c). I suggest to disconnet the two sentence and not cite a study which is not yet submitted. [Sebastian Schemm, Switzerland]	Accepted - Thank you for this comment. (a) fixed, (b) fixed, (c) corrected.
12869	59	4			There is no Tilinina et al. (2019) in the reference list. Does this refer to the "in preparation paper" listed earlier? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted - We have updated the status of the paper, now it is Tilinina et al., 2020, submitted.
43195	59	5		6	Read "this signal was found by Wang et al. (2017a)." rather than "this signal was found by (Wang et al., 2017a)." [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted - Fixed
43197	59	12			Read "revealed by Wang et al. (2016c), " rather than "revealed by Wang et al., (2016c), " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted - Fixed
43199	59	12			Read "with Grise et al. (2014)" rather than "with Grise et al., (2014)" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted - Fixed
12871	59	13			What is meant by a coordinated shift? Consistent? In which case the earlier part of the sentence might also need to be changed. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - We edited this sentence, now it is written as - "coordinated with the shift of the storm tracks displacement in the other atmospheric fields"
43201	59	17			Read "Allen et al. (2010) noted no" rather than "Allen et al., (2010) noted no" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted - Fixed
53351	59	23	59	31	Nice to read about the fit-for-purpose of global climate models here. Yet, what about harmonizing across the subsections and start with observational and modelling uncertainties before assessing the observed changes and their attribution? [Hervé Douville, France]	Accepted. Text in FGD was considerably changed but due to the logic of this section the sentence about uncertainties was added at the end of this section.
14989	59	33	59	43	Check coherency with Chapter 2 regarding the calibrated language used in the statement for the changes in number and intensity of extratropical cyclones [Juan Rivera, Argentina]	Noted/accepted. In both chapters the low confidence is given for this statement.
53353	59	34	59	35	Such a decrease is also found in summer but is underestimated by many models raising questions about the underlying drivers and/or the model reliability. [Hervé Douville, France]	Accepted. Model results can be mentioned, however reference is needed. You may quote the following study https://doi.org/10.1002/2016GL068172 although the role of internal variability should not be underestimated doi:10.1038/ngeo2752 (Hervé)
14991	59	46	60	28	The assessment of this section is mostly based on literature of atmospheric rivers in North America. I believe that new literature should be included to have a broader picture of AR changes. Few examples here: https://link.springer.com/content/pdf/10.1007/s00382-019-05099-z.pdf; https://agupubs.onlinelibrary.wiley.com/doi/pdf/10.1002/2016GL070634; https://onlinelibrary.wiley.com/doi/epdf/10.1002/hyp.10982; https://journals.ametsoc.org/doi/pdf/10.1175/JHM-D-18-0006.1 [Juan Rivera, Argentina]	Noted - References for other regions have been included. Two of the references suggested by the reviewer relate to projections which are not treated in this section.

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41133	59	47	59	47	Update to be consistent with the glossary definition by changing to "Long, narrow (up to a few hundred km wide), shallow (up to a few km deep) and transient corridors,,," [TSU WGI, France]	Accepted
7201	59	47	60	7	Towards the end of this paragraph, it is important to note that our estimation of precipitation rate from AR can have large uncertainties, especialy as AR hits topographically complex coastal regions (Behrangi et al. 2016). This can cause uncertainties in quantifying AR related precipitation. Behrangi, A., B. Guan, P. J. Neiman, M. Schreier, and B. Lambrigtsen (2016), On the Quantification of Atmospheric Rivers Precipitation from Space: Composite Assessments and Case Studies over the Eastern North Pacific Ocean and the Western United States, Journal of Hydrometeorology, 17(1), 369-382, doi: 10.1175/jhm-d-15-0061. [Ali Behrangi, United States of America]	Accepted
80315	59	47	60	7	This text could be removed if needed [Paola Arias, Colombia]	Noted
28971	59	47			This should focus on changes in ARs with only a brief 1-2 lines of background. It also misses the Atlantic perspective e.g. Lavers et al. (2012) JGR doi:10.1029/2012JD018027; Lavers & Villiarini (2015) J. Hydrol. 522, 382–390; Gimeno, L. et al. Annu. Rev. Environ. Resour. 41, 117–141 (2016); Ramos et al. (2016) GRL doi:10.1002/2016GL070634; Ummenhofer et al. (2017) GRL doi:10.1002/2017GL074188 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The introductory material has been shortened and information about regions outside the US west coast has been added, referencing an example global study for brevity.
29029	59	47			specify "km in width" to avoid abmiguity [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
29031	59	50			while ETC drive moisture transport, a recent analysis questions the specific role of ARs: Dacre et al. (2019) J. Hydromet http://doi.org/10.1175/JHM-D-18-0175.1 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. A caveat has been added with this reference.
39123	59	52	59	52	parenthesis problem with : (IWV, e.g., Neiman et al., (2008) [Jean-Louis Bonne, France]	Not applicable. Paragraph has been removed in the FGD
43203	59	52			Read "(IWV, e.g., Neiman et al., (2008))." rather than "(IWV, e.g., Neiman et al., (2008)." [Cyriaque Rufin Nguimalet, Central African Republic]	Not applicable. Paragraph has been removed in the FGD
37531	59	55	59	55	The Kingston and MeMecking (2015) paper that is cited here does not refer to atmospheric rivers by name, although the events analysed in this paper are likely associated with atmospheric rivers. A more appropriate citation to support the text in this location would be Kingston et al. (2016). Full citation: Kingston, D., Lavers, D. A., & Hannah, D. M. (2016). Floods in the Southern Alps of New Zealand: The importance of atmospheric rivers. Hydrological Processes, 30(26), 5063-5070. doi: 10.1002/hyp.10982 [Daniel Kingston, New Zealand]	Not applicable. Paragraph has been removed in the FGD
43205	59	55	60	1	Read "(e.g., Kingston and McMecking, 2015; Sodemann and Stohl, 2013) as" rather than "(e.g., Kingston and McMecking, 2015)(Sodemann and Stohl, 2013) as" [Cyriaque Rufin Nguimalet, Central African Republic]	Not applicable. Paragraph has been removed in the FGD

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241	59	59	3	3	The poleward deflection of NH storm tracks is again referenced with different literature as in Chapter 2. Similar to my comment above, Chapter 8 pays only attension to Tilinina et al. (2013,2019), rather than to the more detailed citations given in Chapter 2, page 53, lines 48 to 55. This should be made consistent or it confuses the reader. Suggestion: Why not simply say that AR5 reports a poleward shift in storm tracks, rather than poinint to a single study? [Sebastian Schemm, Switzerland]	Done. The text was considerably re-written and consistency with ch.2 was provided
39129	60	1	60	1	ARs impact in regions outside the US west coast could be more detailled. Over Greenland, evolution of ARs activity impacting the ice sheet surface melt has been shown in the last decade: « Mattingly, K. S., Mote, T. L., & Fettweis, X. (2018). Atmospheric river impacts on Greenland Ice Sheet surface mass balance. Journal of Geophysical Research: Atmospheres, 123, 8538–8560. https://doi.org/10.1029/2018JD028714 ». This publication could also be cited as a complement to the studies on polar regions (page 60 line 1). [Jean-Louis Bonne, France]	Accepted. Information about regions outside the US west coast has been added. Given the wide range of relevant regions and studies and the need for brevity, a global study is used as the example reference.
23633	60	1	60	2	Bozkurt et al. (2018) also highlighted the role of atmospheric rivers in warming/melt events in Antarctica. Bozkurt, D., Rondanelli, R., Marín, J., Garreaud, R., 2018. Foehn event triggered by an atmospheric river underlies record-setting temperature along continental Antarctica. Journal of Geophysical Research-Atmospheres, 123(8) 3871-3892, https://doi.org/10.1002/2017JD027796. [Deniz Bozkurt, Chile]	Accepted - The reference was included.
43207	60	1		2	Read "(Gorodetskaya et al., 2014; Komatsu et al., 2018)." rather than "(Gorodetskaya and co-authors, 2014; Komatsu et al., 2018)." [Cyriaque Rufin Nguimalet, Central African Republic]	Editorial – copyedit to be completed prior to publication
7993	60	4	60	4	There's an extra 'p' in 'orthogonal' that shouldn't be there. [Anthony Lupo, United States of America]	Editorial – copyedit to be completed prior to publication
69495	60	4	60	4	orthopgonal --> orthogonal [Martin Singh, Australia]	Editorial – copyedit to be completed prior to publication
129197	60	4	60	4	There's an extra 'p' in 'orthogonal' that shouldn't be there. [Trigg Talley, United States of America]	Editorial – copyedit to be completed prior to publication
20149	60	4	60	4	Typo on "orthogonal" [philippe waldteufel, France]	Editorial – copyedit to be completed prior to publication
39125	60	4	60	4	« orthopgonal » should be replaced by « orthogonally » [Jean-Louis Bonne, France]	Editorial – copyedit to be completed prior to publication
43209	60	4		5	Read "(e.g., Ralph and Dettinger, 2011; Guirguis et al., 2018). " rather than "(e.g., Ralph and Dettinger, 2011) (Guirguis et al., 2018). " [Cyriaque Rufin Nguimalet, Central African Republic]	Editorial – copyedit to be completed prior to publication
12873	60	4			Correct "orthopgonal" to "orthogonal". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Editorial – copyedit to be completed prior to publication
64603	60	6	60	6	Consider adding the relevant reference: Curry, C. L., Islam, S. U., Zwiers, F. W., & Déry, S. J. (2019). Atmospheric rivers increase future flood risk in Western Canada's largest Pacific river. Geophysical Research Letters, 46(3), 1651-1661. [Charles Curry, Canada]	Rejected -- that study is about projections while this section is about observed trends.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58955	60	6	60	7	Viale et al (2018) quantifies the impact of atmospheric rivers (ARs) on precipitation in southern South America and could be included here or elsewhere within the section 8.3.2.8.1. Reference: Viale, M., R. Valenzuela, R. D. Garreaud, and F. M. Ralph, 2018: Impacts of atmospheric rivers on precipitation in southern South America. J. Hydrometeor., 19, 1671–1687, https://doi.org/10.1175/JHM-D-18-0006.1. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - The reference was included.
29033	60	6			There is discussion on AR effects in 8.2.3.2; 8.3.1.2 while they are discussed as part of ETCs in Chapter 11.7.2 which is my preference since the assessment summary is quite thin here [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted -- this section attempts to provide new material.
43211	60	6			Read "(Guan et al., 2018; Konrad and Dettinger, 2017; Kim et al., 2013)." rather than "(Guan et al., 2018; Konrad and Dettinger, 2017) (Kim et al., 2013)." [Cyriaque Rufin Nguimalet, Central African Republic]	Editorial – copyedit to be completed prior to publication
58957	60	7	60	7	The citation of Corringham et al (2018) could be updated to the following one: Corringham, T.W.; Ralph, F.M.; Gershunov, A.; Cayan, D.R; Talbot , C.A. (2019) Atmospheric rivers drive flood damages in the western United States. Science Advances, 5(12), eaax4631. DOI: 10.1126/sciadv.aax4631 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This sentence has been removed, as it is more relevant to Working Group 2.
22371	60	7	60	7	Straying into WG2 territory. [Peter Thorne, Ireland]	Accepted. This sentence has been removed.
5485	60	7	60	7	ARs also affect other regions, most notably the mid-latitudes. For example East Asia is affected strongly by ARs in warm season. The following statement and references may be added to the end of this paragraph: "ARs also affect other regions in the globe, most noticeably the mid-latituide coastal regions, with large seasonal variations. ARs affect East Asia (EA) strongly during the period from late spring to summer (Kamae et al. 2017a,b; Kim et al. 2020). The frequency of AR occurrences as well as the fraction of the AR-related precipitation in EA in the period is comparable to those in the Pacific coast of the North America during winter. Similar to the west coast of extratropical continents during winter, ARs are closely related to warm-season extreme precipitation events in EA as over 70% of summerrainfall events exceeding 100 mm/day in EA are related to ARs (Kim et al. 2020)." [Jinwon Kim, United States of America]	Accepted. Information about regions outside the US west coast has been added. Given the wide range of relevant regions and studies and the need for brevity, a global study is used as the example reference.
17245	60	9	60	9	Palaeocliamte records not paleoproxies [Joelle Joelle Gergis, Australia]	Not applicable. Paragraph has been removed in the FGD
12875	60	9	60	10	Why were landfalls shifted southward in the LGM? The sentence could be expanded to explain the reasoning. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Paragraph has been removed in the FGD
22375	60	11	60	23	I recall studies, I think by a Spanish group, looking at ARs in 20th Century renalaysis products. It would be worth revisiting whether there are published analyses using the 20th century reanalysis products that could be assessed here. I'm writing these offline so can't search for the papers right now. [Peter Thorne, Ireland]	Noted. We have found and read what appears to be the relevant paper (Brands et al. 2017, https://doi.org/10.1007/s00382-016-3095-6). However, it doesn't analyse trends so we have not included it.
64971	60	25	60	25	one should at least state the sign of the trend [Johannes Quaas, Germany]	Accepted - The sign of the trend has been included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98077	60	25	60	28	The summary statement is too vague, as it is not clear what is meant by “AR activity”. It would also be helpful to address in this section on ARs whether observed changes are any different from what would be expected if there is just an increase in water vapor content in the atmosphere (i.e., at ~7% per deg C warming). [Thomas Knutson, United States of America]	Rejected. "AR activity" is the same phrase used in the supporting literature. In terms of addressing whether observed changes are different from what would be expected solely due to water vapor increase, we are limited to assessing the existing literature and could find no studies analysing this for the historical period. We have added a discussion of observed warming in ARs.
53355	60	25			positive trend or strengthening? [Hervé Douville, France]	Noted - This was clarified in the text
17247	60	31	60	36	Suggest: Modes of climate variability and regional teleconnections as sub-heading title. This text contains grammatical issues, rework. [Joelle Joelle Gergis, Australia]	Accepted. Title of the subsection has been changed as suggested and the subsection had been carefully edited.
86425	60	33	60	34	Modes of variability are also assessed in Chapter-4 [Swapna Panickal, India]	Noted. We know that the modes of variability are assessed also in ch 4 for the projections, and in fact in our section 8.4 the assessment starts from what concluded in ch 4
12877	60	33	60	36	But what type/time scale of mode will be discussed? Perhaps some signposts can be given to this. There is nothing on the AMV/PDV for example, so it should be clear to the reader not to expect this. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have specified in the introductory paragraph that here the modes at interannual and seasonal timescale are considered
14851	60	39			The impact of ENSO is discussed in this sectio. Where are discussed the processes that impact ENSO (e.g. volcanism)? [Marie-France Loutre, Switzerland]	Noted. Ch 8 is not the place for that assessment. Something on that topic can be found in ch 3 (in terms of anthropogenic influence and model evaluation), in ch 4 (in terms of projections) and in ch 7 (in terms of climate sensitivity)
70271	60	40	60	42	I do not believe that this statement is entirely conistent with what is presented in CH2 and CH3, I am particularly focusing on the reference to "relative to the early 20th century (high confidence)." Please see lines 4-10 of page 87 of the SOD version of CH2. [Shayne McGregor, Australia]	Accepted. The confidence statement has been corrected to be consistent with Ch2
12879	60	40			Here and throughout, Nino (and Nina) needs to be properly accented. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Here and where needed the text has been revised as suggested.
23479	60	45	60	46	Recent study highlight the role of sea surface salinity in the regional precipitation during ENSO/IOD events. "Near-Surface Salinity Reveals the Oceanic Sources of Moisture for Australian Precipitation Through Atmospheric Moisture Transport" "https://doi.org/10.1175/JCLI-D-19-0579.1" [Saurabh Rathore, Australia]	Accepted. The suggested references have been inserted in the revised text as teleconnection of ENSO related to moisture transport for Australia.
72057	60	45	60	55	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]	Noted. The suggested reference has been included in section 8.3.2.4.1 (South and Southeast Asian monsoon), where the teleconnection is briefly assessed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
69497	60	45	61	20	I am a bit confused about this section. Presumably it is about trends in climate modes, but the majority of the descrption is about the teleconnections of the modes to the water cycle in general. Moreover, the discussion focusses on very specific regions without giving much a of a context of larger-scale patterns. Why is the first mention about a small region in the Middle East? Surely the largest impacts of ENSO are in the Pacific, over the maritime continent and tropical South America. If the more general discussion of modes is kept, it should start with a summary of the broader patterns before digging down into the nitty gritty. On the other hand, if the discussion of the teleconnnections is left to the Annex, this section should focus solely on observed changes in ENSO connections to the water cycle. [Martin Singh, Australia]	Noted. This section in chapter 8 is dedicated to the teleconnections of the modes of variability pertinent to water cycle processes. So processes are at the base of the assessment and then regions are used as "examples" of how the processes work.
69499	60	53	60	55	I don't understand what is meant by "the associated precipitation deficits… remain large". Does this mean there is no observed trend in the magnitude of the precipitation anomalies associated with ENSO, but there is a trend towards anomalies further northeasteard? Please clarify. [Martin Singh, Australia]	Taken into account. The text has been rephrased. The point is that multi-year La Nina episodes exacerbates droughts in southern United States, as they extend toward east mostly during the second year.
109807	61	3	61	3	You could reference a new paper that looks at the relationship between ENSO and frequency and intensity of sub-daily precipitation globally - Li, X-F., Blenkinsop, S., Barbero, R., Yu, J., Lewis, E., Lenderink, G., Guerreiro, S., Chan, S., Li, Y., Ali, H., Villalobos Herrera, R., Kendon, E., Fowler, H.J. 2020. Global Distribution of the Intensity and Frequency of Hourly Precipitation and their Responses to ENSO. Climate Dynamics, DOI: 10.1007/s00382-020-05258-7. [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This aspect of teleconnection easily relates to extreme precipitation. Ch 11 more than Ch8 is supposed to assess this.
3193	61	4	61	6	Please include the review paper from Marengo and Espinoza (2016. doi:10.1002/joc.4420.) regarding ENSO impacts on the Amazon Basin [Jhan Carlo Espinoza, France]	Not applicable. The paragraph has been largely reduced and a specific reference to the Amazon Basin has been removed
17249	61	5	61	8	What does TWS refer to here? Avoid acronyms, clearly state what you mean. Also include reference to the Murray Darling Basin in Australia (the nation's food bowl). Reference: Gallant, A. J. E., Kiem, A. S., Verdon-Kidd, D. C., Stone, R. C. and Karoly, D. J. (2012). Understanding hydroclimate processes in the Murray-Darling Basin for natural resources management. Hydrology and Earth System Science 16 (7): 2049-2068. [Joelle Joelle Gergis, Australia]	Taken into account. TWS is removed and instead full meaning (terrestrial water storage) is used. The suggested reference has not been included in the revised text as we avoided to mention specific basins.
53357	61	8			Link to the related teleconnection figure in CH3? [Hervé Douville, France]	Accepted. A reference to fig. 3.37 for precipitation over the globe has been inserted in the revised text.
12881	61	14	61	40	This page is very heavy on the use of the bracket structure to indicate alternatives. The authors should consider whether this is the best way to make the text readable. For example, see Alan Robock's commentary on this at: https://eos.org/opinions/parentheses-are-are-not-for-references-and-clarification-saving-space. (There are also many spelling and grammar mistakes on this page.) [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text has been revised and the use of brackets has been avoided as much as possible. Thanks for the suggested paper
21069	61	20	61	20	To complement the references on southern South America please include: " Precipitation in southeastern South America is influenced by ENSO, the IOD and the tropical north Atlantic and the interaction among these signals, which modulate the northerly transport of moisture, has varied over the 20th century inducing different anomalies in different decades (Martin-Gomez and Barreiro 2015, Int. J. Climatol, doi:10.1002/joc.4228; Martin-Gomez et al 2016, J. Climate doi:10.1175/JCLI-D-15-0803.1)" [Marcelo Barreiro, Uruguay]	Accepted. The suggested references have been inserted in the revised text as teleconnection of ENSO related to moisture transport.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17251	61	22	61	40	This section is important and need to be clearer. Avoid unusual acronyms (IOB?) and bracketed sentences whereever possible. Interaction of IOD and ENSO should be cited in this section, with particular reference to combined positive IOD and El Nino events causing severe drought in Australian region to improve geographic diveristy of the text. 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., Shen, C.-C., Cheng, H., Edwards, R. L. and Heslop, D. (2020). Coupling of Indo-Pacific climate variability over the last millennium. Nature: https://doi.org/10.1038/s41586-020-2084-4. This IOD review paper might also be helpful: Abram, N. J., Hargreaves, J. A., Wright, N. M., Thirumalai, K., Ummenhofer, C. C. and England, M. H. (2020). Palaeoclimate perspectives on the Indian Ocean Dipole. Quaternary Science Reviews 237: 106302. [Joelle Joelle Gergis, Australia]	Taken into account. Unused acronyms have been removed in the revised text. The suggested reference for the co-occurrence of El Nino and IOD has been included in the assessment.
108073	61	29	61	29	IOD instead of IOB [Emily Collier, Germany]	Rejected. The sentence actually refer to the Indian Ocean Basin mode and the acronym for it (as listed in the Annex IV on the Modes of Variability) is IOB.
3717	61	29	61	29	A recent review review brings together the complex interactions between the IOD and ENSO. It is not simply a one-way effect. https://science.sciencemag.org/content/363/6430/eaav4236 [Declan Finney, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Actually the acronym is IOB and stands for Indian Ocean Basin mode, it is not a mistake for IOD.
69501	61	30	61	31	What is meant by "east equatorial Indian Ocean GHG increases"? Is this meant to be increases in GHGs in this region specifically? [Martin Singh, Australia]	Accepted. A comma was missing between "Indian Ocean" and "GHG". The text has been revised
69503	61	31	61	40	There are a number of attribution statements here that are stated as facts. Presumably they are based on models of some sort. I think one needs to make this explicit. E.g., GCM simulations only reproduce X when aerosols are included. [Martin Singh, Australia]	Accepted. The text has been revised and assessment remain has focused on the teleconnection processes of IOD. Attribution statements have been removed as part of the assessment done in ch 3
107709	61	33	61	40	With regards to East Africa, we also found co-occuring positive IOD & ENSO events were associated with the most anomalous moisture and precipitation on the glaciers on Kilimanjaro. However, we only considered a 12-year period. Collier, E., Mölg, T., and Sauter, T.: Recent atmospheric variability at Kibo Summit, Kilimanjaro, and its relation to climate mode activity, J. Clim, 31, 3875–3891, doi:10.1175/JCLI-D-17-0551.1, 2018. [Emily Collier, Germany]	Rejected. The study is focused on a too short period of analysis
53359	61	37	61	40	Prior research has shown that dry conditions tend to persist in the Sahel when El Niño develops, but recent ENSO events highlight that the Sahelian precipitation response also depends on the whole tropical SST background (Pomposi et al., 2020 [Hervé Douville, France]	Accepted. The suggested reference has been included in the assessment
4935	61	40	61	40	although the notion of impact of floods on infectuous disease is very relevant, it is a bit an outlier statement (as there are many more relevant impacts of floods; to be assessed in WG-II) [Bart van den Hurk, Netherlands]	Accepted. Statement has been removed.
22377	61	40	61	40	Impacts is the domain of WG2. [Peter Thorne, Ireland]	Accepted. Statement has been removed.
23635	61	42	61	44	A compound event produced a heat wave in southern South America, flooding in the hyperarid region of the Atacama desert, and record melting and warmth in the Antarctic Peninsula during the March 2015. Rondanelli et al. (2019) showed that the origin of these seemingly disconnected extreme events can be traced to a Rossby wave response to the strongest Madden-Julian Oscillation (MJO) on record in the tropical central Pacific. Rondanelli, R., Hatchett, B., Rutllant, J., Bozkurt, D., Garreaud, R., 2019. Strongest MJO on record triggers extreme Atacama rainfall and warmth in Antarctica. Geophysical Research Letters, 46(6), 3482-3491, https://doi.org/10.1029/2018GL081475. [Deniz Bozkurt, Chile]	Rejected. Extremes are assessed in ch 11.

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72067	61	42	62	21	One recent study showed the changes in the Indo-Pacific warm pool and the MJO are related to increased rainfall over southeast Asia, northern Australia, Southwest Africa and the Amazon, and drying over the west coast of the United States and Ecuador. It should be mentioned in this context --Roxy, M. K., Dasgupta, P., McPhaden, M. J., Suematsu, T., Zhang, C., & Kim, D. (2019). Twofold expansion of the Indo-Pacific warm pool warps the MJO life cycle. Nature, 575(7784), 647-651. [Samanta Dhrubajyoti, Singapore]	Accepted. The suggested reference has been included in the assessment
22379	61	44	61	47	The evidence basis for this assertion is not sufficiently articulated to justify a confidence statement. Either more detail on the evidence supporting this is required or use of confidence language requires reconsideration. [Peter Thorne, Ireland]	Accepted. More literature has been included in the assessment supporting the evidence statement
4937	61	45	61	45	"number of events": you mean, events linked to MJO? [Bart van den Hurk, Netherlands]	Taken into account. Sentence reworded as "intensity and frequency of the MJO"
69505	61	46	61	47	I think this couldbe expanded upon; My undrestanding is that there is some conflict between measures of the MJO associated with precipitation and measures associated with kinetic energy or strength of circulations (see studies by Wolding et al.). [Martin Singh, Australia]	Taken into account. Issues of possible conflicts in measuring the MJO is briefly considered in the assessment.
22381	61	53	61	53	rectification makes no sense. Do you mean linkages? [Peter Thorne, Ireland]	Not applicable. The sentence has been removed from the revised text
84031	62	6	62	10	There are a lot of studies about the influence of MJO on SACZ. See Carvalho et al., 2004 (DOI: 10.1175/1520-0442(2004)017<0088:TSACZI>2.0.CO;2); Grimm and Tedeschi, 2009 (DOI: 10.1175/2008JCLI2429.1); Grimm, 2011 DOI: 10.1007/s00477-010-0420-1); Shimizu and Ambrizzi, 2016 (DOI: 10.1007/s00704-015-1421-2); Shimizu et al., 2017 (DOI: 10.1002/joc.4893); Rodrigues and Woollings, 2017 (DOI: 10.1175/JCLI-D-16-0493.1) [Marco Tulio Cabral, Brazil]	Taken into account. Some of the suggested references relevant for this assessment post-AR5 have been included in the revised text.
3195	62	6	62	15	Recent studies have documented the influence of MJO in tropical South America: Mayta et al (2018 https:// doi.org/10.1002/joc.5810., Mayta etal 2020. doi: 10.1007/s00382-020-05202-9.) Recalde-Coronel et al (2020.https://doi.org/10.1007/s00382-019-05107-2) [Jhan Carlo Espinoza, France]	Taken into account. Some of the suggested references have been included in the assessment
4939	62	9	62	9	typo in "intercations" [Bart van den Hurk, Netherlands]	Accepted. Typo has been corrected
22383	62	17	62	18	What actionable information does this provide a policy maker? My feeling is it would make more sense to drop this opening sentence and recast the next sentence as an opening statement accordingly. [Peter Thorne, Ireland]	Accepted. The summary statement has been completely rewritten.
1281	62	17	62	21	The phrasing of the paragraph is strange, starting with a dismissal of a negative result. Why not just say something like there some evidence of an increased amplitude of both ENSO...? [Rasmus Benestad, Norway]	Accepted. The summary paragraph has been completely rewritten
29035	62	17			I think we should present evidence for what has changed and why, not what there isn't evidence for so this line could be removed "there is no evidence of a dampening of tropical variability and related hydrological events over recent decades (high confidence).". Further, given the thin summary assessment I recommend that this subsection is substantially condensed to what is policy relevant in relation to water cycle change [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The summary paragraph has been completely rewritten
70269	62	18	62	19	The confidence statement "Instead, there some evidence of an increased amplitude of both ENSO and MJO precipitation variability over recent decades (low confidence)." does not accurately represent the ENSO changes reported in CH2, 2.4.2. It is also unclear if a summary statement is required since only a summary of other chapters findings appear to be presented. [Shayne McGregor, Australia]	Accepted. The summary paragraph has been completely rewritten
43213	62	18			Read "Instead, there are some evidence of an increased" rather than "Instead, there some evidence of an increased" [Cyriaque Rufin Nguimalet, Central African Republic]	Not applicable. The summary has been completely rewritten
17253	62	19	62	21	Worth checking this statement with Chapter 2's ENSO section to make sure they align [Joelle Joelle Gergis, Australia]	Taken into account. Summary has been rewritten with information coming from here, as there is no need to repeat what concluded in ch 2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
5581	62	24	63	30	It is underlined that exists links between the precipiations varibility and the NAO fluctuations in Europe. There exists other works about these links in other regions. Cf ref biblio : 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; Nouaceur et al., 2013 PhysioGeo. There exist also relationships between the hydrological varibility (streamflow and groundwater)) and the NAO and ENSO fluctuations in Europe, Mediterranea, North America... Cf the publications above and Massei et al., 2011International journal of climatology; Rossi et al., 2011 Global Planetray Change; Fritier et al., 2012 CR Geoscience; Chevalier et al., 2014 Hydological Sc. J.; Laignel et al., 2010 IAHS publ, Massei et al., 2017 Journal of Hydrology...and other... [Benoit Laignel, France]	Taken into account. In the revised text we have included a short sentence about the influence of NAO on groundwater and streamflow over Europe and Mediterranean countries, including some of the suggested references.
22385	62	25	62	35	This opening segment should also characterise the relevant findings from chapters 2 and 3 which should form the jump off point for your further assessment here. Maybe most easily achieved by integrating the first few sentences of the next paragraph into this opening paragraph. [Peter Thorne, Ireland]	Accepted. The introductory paragraphs for the extra-tropical modes have been revised to start directly from the assessment done in ch 2 and ch 3
4941	62	28	62	28	"has been largely offset": quite unclear formulation: recent changes are of opposite sign as the 1950-1990 trend? [Bart van den Hurk, Netherlands]	Not applicable. Paragraph has been removed in the FGD
70993	62	34	62	35	This sentence is ambiguous to me. I would normally interpret "competing" as meaning acting in opposite directions. Up until about 2000, when ozone was declining, the two drivers were acting on the summertime SAM in the same direction, and many studies have shown that the ozone effect far outweighed the GHG effect. Since 2000, ozone has been recovering, but the recovery will be much slower than the decline, so that to a first approximation, the two effects will largely offset each other. In that sense, the competition between them will intensify rather than weaken. I suggest rewording for clarity. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Paragraph has been removed in the FGD
53361	62	34	62	35	Suppress or clarify. Ozone depletion and GHG increases both contributed to the positive trend in the SAM (cf. CH3) and did not compete during the 20th century. [Hervé Douville, France]	Not applicable. Paragraph has been removed in the FGD
4943	62	35	62	35	How do I interpret the phrase "the two effects are less compensating"? [Bart van den Hurk, Netherlands]	Not applicable. Paragraph has been removed in the FGD
12883	62	45			Change "reduction of" to "reduced" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text changed as suggested.
4945	62	47	62	49	A link to Indian Summer Monsoon rainfall has been documented by Di Capua et al https://www.earth-syst-dynam.net/11/17/2020/ [Bart van den Hurk, Netherlands]	Accepted. The suggested reference has been included in the assessment
65795	63	1	63	22	Suggest including a discussion on SAM influence on rainfall in Australia and the projected changes. For example, see: - Lim, E.-P., H. H. Hendon, J. M. Arblaster, F. Delage, H. Nguyen, S.-K. Min, and M. C. Wheeler (2016), The impact of the Southern Annular Mode on future changes in Southern Hemisphere rainfall, Geophys. Res. Lett., 43,7160–7167, doi:10.1002/2016GL069453. - Hendon, H. H., D. W. J. Thompson, and M. C. Wheeler (2007), Australian rainfall and surface temperature variations associated with the Southern Hemisphere annular mode, J. Clim., 20, 2452–2467. - Hendon, H. H., E.-P. Lim, and H. Ngyuen (2014a), Variations of subtropical precipitation and circulation associated with the Southern Annular Mode, J. Clim., 27, 3446–3460. - Meneghini, B., I. Simmonds, and I. Smith (2007), Association between Australian rainfall and the Southern Annular Mode, Int. J. Climatol., 27, 109–121. [Kushla Munro, Australia]	Rejected. This subsection is not about projections but it is about past changes
22387	63	12	63	12	Why are these controversial? Use of controversial by IPCC carries considerable weight. Are you sure you wish to characterise as such? [Peter Thorne, Ireland]	Accepted. The word controversial has been removed and the sentence rephrased

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30695	63	12	63	13	Add Grieger, J., G. C. Leckebusch, C. C. Raible, I. Rudeva and I. Simmonds, 2018: Subantarctic cyclones identified by 14 tracking methods, and their role for moisture transports into the continent. Tellus, 70A, 1454808, doi: 10.1080/16000870.2018.1454808 to references for this topic. [Ian Simmonds, Australia]	Accepted. The suggested reference has been included in the assessment
12885	63	12			Change "relates SAM" to "relate the SAM" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text changed as suggested.
7049	63	12			The expression "findings relates" is a combination of singular and plural. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text changed as "findings relate the …"
22389	63	20	63	22	Impacts are the domain of WG2 and should not be included in WG1. [Peter Thorne, Ireland]	Accepted. Sentence has been removed
53363	63	24	63	30	To what extent are you assessing here changes in modes of variability or how changes in the mean state project onto these modes of variability? Would it be possible to coordinate with CH2/3/4 in order to propose a common framework for analysing and interpreting such changes? [Hervé Douville, France]	Accepted. Summary statements have been rewritten avoiding repetitions with ch 2 and ch 3
22391	63	24	63	30	This finding is redundant with those in chapters 2 and 3 and has very little to do with the hydrological cycle and is poorly justified by the prior text. The finding should be recast to focius upon the hydrological cycle impacts of any changes in the extratropical modes and leave the assessment of mode changes and their causes to chapters 2 and 3. [Peter Thorne, Ireland]	Accepted. Summary statements have been rewritten avoiding repetitions with ch 2 and ch 3
53365	63	33			This section is probably the most eagerly awaited by many readers. It could be even more comprehensive, better link to Figures (including from CH4), include a synthetic schematic of projected circulation changes relevant to the water cycle, and conclude each subsection with clear key messages to be elevated in the ES. The brief introduction could also explains how this section builds on and adds value to CH4. [Hervé Douville, France]	Thank you. The chapter now has a schematic as you outline and we have linked to Chapter 4 and other chapters where appropriate.
28279	63	33			Section 4: For projected changes in long-term mean P–ET (equivalent to runoff) see Padrón, R. S., Gudmundsson, L., & Seneviratne, S. I. (2019). Observational constraints reduce likelihood of extreme changes in multidecadal land water availability. Geophysical Research Letters, 46, 736–744. https://doi.org/10.1029/2018GL080521 [Ryan Padrón, Switzerland]	Rejected. Changes in runoff and P-ET covered via other literature. This paper is cited in Chapter 11.
17255	63	37	63	37	Suggest rephrasing to read: For ease of comparsion, this section on model projections mirrors the structure of Section 8.3 on observations (or similar). [Joelle Joelle Gergis, Australia]	Noted, sentence has been removed in FGD.
131577	63	37	63	37	Even though you described the manner of organisation in the previous section a small reminder would be useful here. To avoid to much repetition a few reminding words/ a side sentence would be sufficient. [Hans Poertner and WGII TSU, Germany]	Noted, sentence has been removed.
53367	63	42	63	48	While all available projections have not been considered, this section makes substantial use of the latest-generation SSP1-2.6, SSP2-4.5 and SSP5-8.5 scenarios that encompass a wide range of plausible emissions. Other reasons for the possible lack of confidence in water cycle projections are assessed in Section 8.5. [Hervé Douville, France]	Noted. This paragraph has been substantially reworded, with reference to both CMIP5 and CMIP6 simulations, and cross-reference to the Atlas.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6715	63	43	63	43	"have not yet necessarily emerged" is rather awkward wording. Some projected changes may never emerge, as the projections may be wrong: in some cases it may not be natural variability that is inhibitting the signal from emerging. The wording of the sentence needs to be amended to allow for this possibility. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This paragraph has been substantially reworded in the FGD.
59129	63	51	63	51	It is not logic that the names of Section 8.3.1.1 and 8.4.1.1 differ. Plese be consistent. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Agreed. The two subsections are now titled "Global water cycle intensity and P-E over land and oceans."
111391	63	51	64	36	"Consideration of water cycle intensity.." a key point to note is the response in water limited (e.g. land) and unlimited realms (e.g. ocean/cryosphere). The work of Greve et al 2014 doi: 10.1038/ngeo2247 for e.g. reports that over land the "wet get wetter, dry get drier" is only valid for ~11% of the global land area, with ~10% showing the inverse, ~80% unidentified. Another point to note is that over land, if you don't have measurements you don't have any information as there is no memory once soil moisture reaches 0. As a water unlimited realm, the ocean captures signals both freshwater loss (enhanced salinity) and gain (freshening), with this change signal "integrated" over time and leading to coherent change patterns expressed when assessed over multi-decades. It is this difference which needs to be highlighted when referencing "interpretations [of water cycle intensity]" [Paul Durack, United States of America]	Thanks. This subsection has been extensively reworked and cross-referenced, with a more detailed discussion of land/ocean contrasts.
129199	63	51	64	56	[PRECISION] The term "intensity" is grossly misused here and by some authors. Intensity refers to the rate of rainfall. In dealing with P-E it should refer to "amplification" not intensity. Moreover, all models have major errors in precipitation amount, distribution, intensity, and frequency (too often, not enough). The Cheng et al. (submitted from Chapter 2) has a nice analysis of total salinity from 0 to 2000m depth that shows clearly the fresh get fresher and salty get saltier aspects of change. [Trigg Talley, United States of America]	Thanks. This subsection has been extensively reworked. The term "intensity" has been retained but is explained in more detail.
59111	63	53	64	36	Also changes in the cryosphere should be mentioned in this context. As more of the solid cryospheric storages melts, more water in liquid and gas form is available, which also has an impact on the water cycle intensity (even if this is still a very small proportion). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This subsection has been revised to cover P-E over land and oceans as well as water cycle intensity. We agree that cryosphere melt does contribute in a small way but is not appropriate to include in this overview discussion. Cryosphere changes are discussed elsewhere and are largely the domain of chapter 9.
53369	64	11	64	12	Do you refer to global land surface evaporation? Please, be more speciific. A brief assessment of the global ocean P-E projections, consistent with CH4 and 9 would be also useful for a more complete picture of changes in the global water cycle intensity. [Hervé Douville, France]	Noted, thanks. This subsection has been extensively reworked and cross-referenced, with a more detailed discussion.
113363	64	13	64	13	It may be better to use the same colorbar as for precipitation in the evaporation maps. Blue being little evaporation is not very intuitive. [Diego Miralles, Belgium]	Rejected. Decreased ET implies more surface moisture which fits with the blue shading.
4953	64	14	64	14	I don't see a robust decrease of evaporation from fig 8.18 (and I don't understand it either) [Bart van den Hurk, Netherlands]	Noted, discrepancy in wording corrected in the FGD.
22405	64	14	64	14	robust decreases seems at odds with the later sub-sections which imply robust increases? [Peter Thorne, Ireland]	Noted, discrepancy in wording corrected in FGD.
82315	64	14	64	14	It is stated that evapotranspiration exhibits "robust decreases". When looking at Fig. 8.18 the impression is that ET increases on average. If I am not misinterpreting the figure please change "decreases" into "increases". [Schröder Marc, Germany]	Noted, discrepancy in wording corrected.
113365	64	14	64	14	decreases? This contradicts the figure and everything said above. [Diego Miralles, Belgium]	Noted, discrepancy in wording corrected.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59113	64	14	64	14	Maybe I got it wrong. But in my understanding, it is no logic, that you write that the global evapotranspiration shows robust decrease. Figure 8.1.8 shows a clear increase. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted, discrepancy in wording corrected.
27255	64	17			Figure 8.14 is barely discussed. Is it really useful? [Eric Brun, France]	Taken into account. This figure on future changes in P-E is assessed in the FGD (Figure. 8.13)
29037	64	32			nice intro! Can shorten ", discussed further in the next section," to "(Section 8.4.1.2)" [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Thanks. This subsection has been extensively reworked in the FGD.
1283	64	34	64	36	I fail to understand the logics of this sentence and why it's even in this report. Has there been a pressure to make global water cycle intensity a policy-relevant metric? And why does different responses over different region to a global response make it a non policy-relevant metric? Isn't it quite the opposite if some regions get too much rainfall and others too little? [Rasmus Benestad, Norway]	Noted. This subsection has been extensively reworked and that sentence removed in the FGD.
70999	64	35	64	35	Zappa et al. (2020), which is not in the reference list, is now published: doi: 10.1073/pnas.1911015117). [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Thanks, reference included.
59093	64	35	64	35	Zappa et al., 2020 is missing in the reference list [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted, now included.
1285	64	35	64	36	Does not the observed evolution, discussed earlier, suggest otherwise to "Both theory (Held and Soden, 2006a) and models (Vecchi et al., 2006) suggest a slowdown of the tropical circulation ...". The message in this chapter on the Hadley cell and the Walker circulation seems to me to be mixed. Is the picture consistent? [Rasmus Benestad, Norway]	Noted. This subsection has been extensively reworked and made more consistent in the FGD.
4955	64	35	64	36	The statement on policy relevance is out of context. One might argue that patterns of intensification are policy relevant, or regional intensification is not policy relevant (as far too coarse scale). [Bart van den Hurk, Netherlands]	Noted. This subsection has been extensively reworked and that sentence removed in the FGD.
59131	64	38	64	38	It is not logic that the names of Section 8.3.1.2 and 8.4.1.2 differ. Plese be consistent. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The section name of 8.4.1.2 has been changed to match the parallel section, 8.3.1.2.
22395	64	40	65	3	There is a lot of use of confidence language in this paragraph but without any substantive assessment text to back it up. Presumably the evidence follows in the subsections it is introducing but it ends up feeling like the cart is being put before the horse rather than vice-versa as a result. I would suggest reconsideration accordingly with less frequent use of confidence language and let instead the section summaries which follow the substantive assessment text do the lifting vis-a-vis confidence and uncertainty language [Peter Thorne, Ireland]	Accepted. The section has been rewritten to provide the evidence first and then conclude with a summary statement with confidence language.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129201	64	40	65	3	This reference is the only one that has provided an observational estimate of water vapor feedback: Trenberth, K. E., Y. Zhang, J. T. Fasullo, and S. Taguchi, 2015: Climate variability and relationships between top-of-atmosphere radiation and temperatures on Earth. J. Geophys. Res., 120, 3642-3659, Doi: 10.1002/2014JD022887. [Trigg Talley, United States of America]	Not applicable. The text has been revised to provide less background information, which is covered earlier in the chapter. While the reference is certainly important, the focus of this section is projected changes, not observed changes.
29039	64	40			can reference back to Figure 8.1. Although the atmosphere is the smallest globally encompassing store, Figure 8.1 includes smaller individual stores such as rivers, reservoirs, seasonal snow and biological water so the statement could be made more precise by saying "the atmosphere only contains 0.05% of the global fresh water (Fig. 8.1)". The following sentence didn't make sense to me and can be replaced by "Atmospheric water vapour is currently increasing with planetary warming (Section 2.3.1.3 and 8.3.1.2) as understood from well understood thermodynamic processes (8.2)." Note that summary statements are missing from 8.4 sub-sections. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Not Applicable (text suggestion) / accepted (summary statement). The background discussion of water vapour has been removed, as it is amply discussed in previous sections of the chapter and the focus here is on projections. The section has been rewritten to have a separate summary paragraph.
113367	64	41	64	41	This may be a good reference after 'week' Sodemann, H.: Beyond Turnover Time: Constraining the Lifetime Distribution of Water Vapor from Simple and Complex Approaches, Journal of Atmospheric Sciences, 77(2), 413–433, doi:10.1175/JAS-D-18-0336.1, 2020. [Diego Miralles, Belgium]	Not applicable. The sentence has been deleted, as background discussion of water vapour is already covered earlier in the chapter.
69509	64	41	64	42	The most common reference to relative humidity in 8.2 and 8.3 is with regard to declining near-surface RH over land, so I think it is not obvious here what is being refered to as the "constant relative humidity hypothesis". [Martin Singh, Australia]	Not applicable. The sentence has been deleted.
59095	64	45	64	45	Do you mean Held and Soden, 2006a or 2006b? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Held and Soden 2006a and 2006b are the same paper that was inadvertently listed twice in the references.
43215	64	45			Read " (Held and Soden, 2006; Watterson et al., submitted) " rather than " (Held and Soden, 2006; Watterson et al submitted) " [Cyriaque Rufin Nguimalet, Central African Republic]	Editorial – copyedit to be completed prior to publication.
6717	64	46	64	46	It might be better to omit "near-surface" here, because the net decline in the relative humidity of surface air over land seen from observations implies that the 6-7% increase in atmospheric water content is not happening there, on average. See also lines 52 to 54, which states that climate models project "a robust and substantial decrease over land (high confidence)" for near-surface relative humidity. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. "Near surface" has been removed.
22393	64	46	64	49	This is discussed and assessed substantively in chapter 7. It woud surely make sense to cross-reference to the relevant section here? [Peter Thorne, Ireland]	Accepted. Chapter 7 is now referenced.
64973	64	47	64	48	why only “high confidence”? is this not a known fact? [Johannes Quaas, Germany]	Noted. That water vapor is a GHG is certainly a fact. That increases in precipitable water are a positive feedback is a near certainty but, due to the complex processes and dynamics involved, not a fact.
113375	64	48	64	48	Why is this 'high confidence' not 'certain'? [Diego Miralles, Belgium]	Noted. That water vapor is a GHG is certain. That increases in precipitable water are a positive feedback is a near certainty but, due to the complex processes and dynamics involved, not a fact.
64975	64	52	64	53	what leads the conclusion about “possible overestimation” for future changes over the ocean? [Johannes Quaas, Germany]	Not applicable. The sentence has been rewritten.
113369	64	55	64	55	Involving vegetation 'and soil oisture'. [Diego Miralles, Belgium]	Accepted. Phrase added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29043	64	56	65	4	The final statement of this sub-section seems to repeat the penultimate one. Also there is no Section 8.2.1.2 (8.2 or 8.2.2.1 is correct) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text has been rewritten and the subsection number corrected.
116753	64		64		with virtual certainty => it is virtually certain that [Valerie Masson-Delmotte, France]	Thanks, corrected.
113371	65	1	65	1	Not necessarily 'amplified'. I would say 'regulated'/'modulated'. There are also important negative feedbacks. A very obvious one: evaporation is surpressed by evaporation as it increases near-surface humidity. [Diego Miralles, Belgium]	Accepted. "Amplified" has been changed to "modulated."
113373	65	2	65	2	such as' for 'involving'. [Diego Miralles, Belgium]	Rejected. Soil moisture and plant stomatal changes are intended as important examples, not the only factors.
43217	65	2		3	Read " (Berg et al., 2017; Douville et al., 2020). " rather than " (Berg et al., 2017) (Douville et al, 2020). " [Cyriaque Rufin Nguimalet, Central African Republic]	Editorial – copyedit to be completed prior to publication.
59097	65	3	65	3	Douville et al., 2020 is missing in the reference list [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The reference has been added.
98079	65	6	66	17	You could add to the discussion that confidence in future projections of regional precipitation change is relatively higher (lower) when climate model historical run precipitation changes are shown to be consistent (inconsistent) with observed historical changes. While Fig. 8.7 could be a start on doing such an analysis, more work is needed there to actually do the comparison and to account for internal variability effects. This has already been done in Knutson and Zeng (2018, Fig. 3-5) and their consistency / inconsistency results can be used as one metric to inform confidence in future projections that are done with those same models. Refs: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637 https://doi.org/10.1175/JCLI-D-17-0672.1. [Thomas Knutson, United States of America]	Noted. The reference and associated discussion appears in section 5 of the FGD.
53371	65	6			A final summary of the key findings is missing [Hervé Douville, France]	Noted, summary added in FGD.
129203	65	8	65	18	This seems to not take into account the role of aerosols which seem to have reduced precipitation in some areas, and so the global mean has not increased. [Trigg Talley, United States of America]	Noted. This subsection has been reorganised. It does not mention aerosol effects directly as they are covered elsewhere in the relevant regional discussions.
88137	65	8	66	17	As I commented in the previous draft, This subsection on 'Global and regional precipitation' tries to cover too much and so is not comprehensive or very useful, particularly on regional precipitation change. Each paragraph seems to cover a different topic, and the sub-section as a whole doesn't link together very well. I suggest clarifying the focus of this sub-section, or removing it altogether. If the authors wish to cover regional precipitation change in detail in this chapter then it will require more space than this. [Robin Chadwick, United Kingdom (of Great Britain and Northern Ireland)]	Thanks. The whole of 8.4.1.3 has been reworked and combined into one, hopefully making it more coherent in the FGD.
53373	65	10			also refer to CH4 including Fig. 4.1b? [Hervé Douville, France]	Noted. Text extensively revised and cross-references to Chapter 4 added but not a reference to this specific figure.
43219	65	10			Read "CMIP6 will be included for " rather than "CMIP6. Will be included for " [Cyriaque Rufin Nguimalet, Central African Republic]	Noted, text extensively revised in FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
20513	65	11	65	15	Together with congratulations about scenarios exhibiting consistencies and responses being understood, is it possible to obtain comments about the salient features depicted on Figure 8.15 concerning the spatial repartition of changes, as well as their dependency upon season? [philippe waldteufel, France]	Noted, text extensively revised in FGD.
43221	65	11			Read "sensitivity). There is a relatively robust " rather than "sensitivity) There is a relatively robust " [Cyriaque Rufin Nguimalet, Central African Republic]	Noted, text changed in FGD.
70997	65	14	65	18	These statements need nuancing. They only apply in a broad sense. Especially over land, there are many regions where models show inconsistent responses, and which are not constrained by simple energetic arguments (if they were, they would not be so inconsistent). Also, for many Mediterranean-type regions subject to water stress, there is a sensitivity to SST pattern evolution in a warming climate; the rapid adjustment is only a small part of the story there (see Zappa et al. 2020 doi: 10.1073/pnas.1911015117). [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted, text extensively revised and these statements removed.
29045	65	15			8.2.1.1 --> 8.2.1 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - Text removed in the FGD.
96809	65	21	65	31	What about the systematic error of the used CMIP6 models? Regarding the CMIP5 models the systematic error (for the RCP8.5 scenario) in widespread areas was comparable with or exceeds the magnitude of the climate change signal. [Nicole Wilke, Germany]	Taken into account - Systematic errors and model fitness-for-purpose are briefly discussed in Section 8.5.1.1. The assessment of systematic errors is generally based on historical simulations rather than on scenarios and also relies on accurate observations. Systematic errors in simulated precipitation are assessed in Ch3 (Fig.3.10), the reason why Ch8 rather focuses on the evaluation of P-E against ERA5 reanalysis (Fig. 8.24).
22397	65	21	65	31	Chapter 4 includes a near identical figure. I presume we don't need both and it would be better to cross-reference? [Peter Thorne, Ireland]	Taken into account – This figure has been changed to show the four seasons change for a medium scenario (SSP2-4.5).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70995	65	23	65	29	We are submitting a manuscript (G Zappa, E Bevacqua and TG Shepherd: “The real mean signal to noise of multi-model climate change projections”) to Int. J. Climatol. in which we propose a new methodology to diagnose the robustness and magnitude of future projected changes from multi-model ensembles. Rather than basing the stippling and hatching of spatial maps on the signal to noise of the multi-model mean response - as it is standard in the IPCC, and in this chapter - our proposed approach evaluates the mean forced signal-to-noise of the individual model responses. This enables us to make statements on regions where a large future change compared to year-to-year variability is plausible, regardless of whether the mean signal is robust across the ensemble. While previously proposed alternative approaches were also able to discriminate between regions with a small response from those with large uncertainty, this method has the benefit of being as simple, and with as few free parameters, as the standard IPCC approach, while explicitly providing information that is relevant for risk assessment, i.e. the potential for a large change. For mean precipitation changes, we find that the majority (58% in surface area) of the unmarked regions and part (18%) of the hatched regions from the AR5 hid climate change responses that are on average large compared to the year-to-year variability. Based on the newer CMIP6 ensemble, we identify that a considerable risk of large annual-mean precipitation changes, despite the lack of a robust projection, exists over 21% of the global land area, mostly including Central America, Northern South America (including the Amazon), Central and West Africa (including parts of the Sahel) and the Maritime continent. You may contact g.zappa@isac.cnr.it for the submitted version of this paper. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted - The stippling/hatching strategy proposed in the submitted study is relevant and interesting. Nonetheless, a simpler method with hatching only has been eventually used in Chapter 8 for mapping multi-model ensemble mean changes. Model uncertainties have been highlighted in Section 8.5 using more explicit techniques such as the fractional contribution to total uncertainty (Fig.8.23) or the 5-95% confidence interval (Fig.8.26).
113377	65	34	65	34	We just read that the Walker and Hadley circulation are strengthening, what is meant here (and in othre instances) by 'a slowdown of the tropical circulation' I cannot tell if this is a relevant inconsistency, but even if it is a misunderstanding please phrase clearly and consistently throughout. [Diego Miralles, Belgium]	Noted, text removed.
85053	65	34	65	35	Presumably this is the situation when LLGHGs are a very dominant forcing and anthropogenic aerosols are negligible. As long as significant aerosol emissions are present, the tropical circulation pattern (especially the meridional circulation) may not be completely under the control of the LLGHG forcing. Further, sustained stratospheric loadings of aerosols could also prevent a LLGHG control of the tropical circulation. [venkatachalam ramaswamy, United States of America]	Noted, text removed. Aerosol effects discussed elsewhere.
89083	65	34		36	This sentence doesn't seem logical to me. The arguments for the slowdown of tropical circulation made by Held and Soden (2006) is that the projected rate of increase of moisture is greater than precipitation. Precipitation frequency is not relevant to this argument. This is also a strange statement to made about changes in precipitation frequency, and it's not the central point of Trenberth (2011). [Angeline Pendergrass, United States of America]	Noted, text removed.
64977	65	35	65	36	In my understanding, it is not precipitation frequency, but integral rate, together with the atmospheric water content, that determine the lifetime. [Johannes Quaas, Germany]	Noted. This subsection has been extensively revised and this sentence removed.
29047	65	35		38	it's not clear why "as precipitation frequency is not projected to increase at the global scale" follows and could be removed. On the next line, attributed to what effect? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This subsection has been extensively revised and this sentence removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
88139	65	36	65	38	Which effect is being discussed here, the direct CO2 effect? The previous sentence was discussing the weakening of the circulation in general, not just the direct CO2 effect. [Robin Chadwick, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This subsection has been extensively revised and this sentence removed.
4957	65	38	65	38	What is "this effect" referring to? [Bart van den Hurk, Netherlands]	Noted. This subsection has been extensively revised and this sentence removed in the FGD.
88141	65	43	65	43	Despite what? [Robin Chadwick, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text removed.
69511	65	43	65	43	Despite what? The previous sentence is about precipitation shifts, which is not obviously in opposition to the statement about shortened wet seasons. [Martin Singh, Australia]	Noted, text removed.
72283	65	43	65	48	A post-AR5 analysis of CMIP5 projections by Horinouchi et al (2019) showed that the northward migration of early summer East Asian rainband such as the Baiu is delayed along with that of the mid-latitude westerly jet in the future and the associated vertical motion. Ose (2019a and 2019b) indicated that the tropospheric vertical motion anomaly induced by the horizontal heat advection is a possible dynamical mechanism to explain the future monthly and regional precipitation pattern in the mid-latitudes of summer East Asia in addition to the thermodynamic increase in moisture. These results have not been necessarily recognized enough as a possible mechanism for the future dynamical precipitation change in the mid-latitudes of summer East Asia. (Reference) Horinouchi T., S. Matsumura, T. Ose, and Y. Takayabu (2019) Jet-precipitation relation and future change of the Mei-Yu-Baiu rainband and subtropical jet in CMIP5 coupled GCM simulations. J. Climate, 32, 2247-2259. DOI:10.1175/JCLI-D-18-0426.1 Ose, T., 2019a: Characteristics of Future Changes in Summertime East Asian Monthly Precipitation in MRI-AGCM Global Warming Experiments. J. Meteor. Soc. Japan, 97, 317-335, doi:10.2151/jmsj.2019-018. 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. This text has been removed in a rewrite of the subsection. Monsoon changes are discussed elsewhere, and two of the references included.
29049	65	43			Also Southern Africa (Dunning et al. 2018 J. Cim) by around 5-10 days decline in length with a smaller decrease in west Africa [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted, reference and discussion included in Box 8.2 and elsewhere in the FGD.
59099	65	45	65	45	Saeed et al., 2018 is missing in the reference list [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Citation removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59101	65	46	65	47	Lee and Wang, 2014 is missing in the reference list [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Citation removed.
71001	65	47	65	48	This misunderstands Ceppi et al. (2018). There the term "fast" was used to include both the rapid adjustment and the fast SST response (up to 10 years). There was no separate attribution between the two. In Zappa et al. (2020: doi: 10.1073/pnas.1911015117) this response was explicitly split into its two components. Fig. 2D,E of that paper shows that in the extratropics, the storm track response is mainly driven by the SST response, not the rapid adjustment (and the precipitation changes go along with that). [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This text has been removed in a rewrite of the subsection. Related discussion in sections 8.2.2.2 and 8.5.3.1.
89085	65	50			To me, "anticipate" implies that the emissions scenarios have a reason to expect a decline in aerosol emissions. Ultimately this will be determined by decisions people make. A better description would be they "assume" a decrease in anthropogenic aerosol emissions. [Angeline Pendergrass, United States of America]	Noted. This text has been removed.
22399	65	52	65	52	Probably offset the implies that there has been no change yet earlier sections plus chapter 2 support the presence of trends so it would surely be better to say partially offset rather than probably offset for intra-chapter and whole-of-report consistency? [Peter Thorne, Ireland]	Noted. This text has been removed in a rewrite of the subsection.
85055	65	52	65	53	Levy et al. demonstrate how the drawdown of aerosol effects, ari+aci, intensifies the global hydrologic cycle, with pronounced increase of precipitation due to both aerosol decrease and continued LLGHG increase in the east Asian region. The investigation also offers a contrast between the 20th and 21st centuries. (Levy II, H., L W Horowitz, M D Schwarzkopf, Y Ming, J-C Golaz, V Naik, and V Ramaswamy, 2013: The Roles of Aerosol Direct and Indirect Effects in Past and Future Climate Change. Journal of Geophysical Research: Atmospheres, 118, DOI:10.1002/jgrd.50192.) [venkatachalam ramaswamy, United States of America]	Noted. This text has been removed in a rewrite of the subsection.
29051	65	53			also Wilcox et al. (2020) ACPD https://doi.org/10.5194/acp-2019-1188, in review but may be accepted in time. The following paragraph quoting Richardson et al. seems to follow from this and the Mediterranean line seems out of place. There is lots of discussion of aerosol but less relating to the regional responses seen in Figure 8.15 which reflect also thermodynamic and circulation responses. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Thanks, reference included and text reworked in the FGD.
74381	65	9 to 11	65	9 to 11	to add) for … after sensitivity). [Moulay Driss HASNAOUI, Morocco]	Noted, text modified.
89087	66	3		7	This paragraph should be edited for internal logical flow. The first sentence introduces a method but no findings. The second sentence is set up to contradic the first ("However,"), but there's nothing to contradict. [Angeline Pendergrass, United States of America]	Noted, text extensively revised.
43223	66	3			Read "slow precipitation response framework, Richardson et al. (2018c) used a simple " rather than "slow precipitation response framework, (Richardson et al., 2018c) used a simple " [Cyriaque Rufin Nguimalet, Central African Republic]	Noted, text extensively revised in FGD.
53375	66	4			remove however? [Hervé Douville, France]	Noted, text extensively revised in FGD.
53377	66	6	66	7	albeit depending on regions (e.g., Kusunoki et al. 2020) but whatever the emission scenario is since future emissions will make substantial differences mostly in the second half of the 21st century (Fig. 4.1.b). [Hervé Douville, France]	Noted, text extensively revised in FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6719	66	9	66	10	A rewrite is needed here, as global-mean precipitation does not have a value particularly over a region. "an increase global mean precipitation, particularly in East and South Asia" could be changed to "an increase in precipitation in the global mean and particularly over East and South Asia." [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted, text extensively revised in the FGD.
43225	66	11			Read " Westervelt et al. (2018) explored " rather than " Westervelt et al., (2018) explored " [Cyriaque Rufin Nguimalet, Central African Republic]	Noted, text extensively revised in FGD.
89089	66	13		14	Over what time period? [Angeline Pendergrass, United States of America]	Noted, text extensively revised.
22401	66	14	66	17	Should cross-reference not be made to relevant sections of chapters 6 and 7 here? [Peter Thorne, Ireland]	Noted, text extensively revised. Aerosol discussion moved elsewhere in FGD, with cross-referencing.
72193	66	31	66	43	a definition of seasonality should be given [Joanna Wibig, Poland]	Taken into account - In line with the IPCC Glossary which does not provide a definition, we feel that seasonality is a widely understood concept whose quantification is however not trivial. Rather than providing a unique and narrow definition, we have just added the following preliminary sentence at the beginning of the Box: "The seasonal dependence of precipitation and water resources is a key feature of regional climate, especially in the tropics where precipitation are usually concentrated within one or two rainy seasons."
29053	66	34			The early period is affected by large aerosol forcing and circulation responses have been shown to smear out the contrast between weat and dry regimes including seasons e.g. Kumar et al. (2015) GRL doi:10.1002/2015GL066858 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - The following sentence has been added: "Such a contrast may be explained by a larger aerosol forcing in the middle of the 20th century, which has been shown to smear out the contrast between wet and dry regimes including seasons (Kumar et al., 2015)."
59103	66	35	66	35	Do you mean Chou et al., 2013a or 2013b? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted - We mean Chou, C., Chiang, J. C. H., Lan, C. W., Chung, C. H., Liao, Y. C., and Lee, C. J. (2013). Increase in the range between wet and dry season precipitation. Nat. Geosci. 6, 263–267. doi:10.1038/ngeo1744. All references will be double-checked before the FGD submission
22403	66	36	66	36	This is ambiguous. Do you mean that two satellite datasets disagree with each other or do you mean that two satellite datasets agree in showing contrasting trends between the two regions? A redraft is required either way to clarify here. [Peter Thorne, Ireland]	Taken into account - This ambiguity has been clarified by starting the sentence as follows: "Two satellite datasets revealed contrasting regional trends since 1979, ..."
53379	66	38	66	40	A recent study (Tan et al., 2020) moreover highlights large differences in precipitation seasonality between seven global precipitation datasets, with no region showing a consistent, statistically significant, positive or negative trend over the last three decades. [Hervé Douville, France]	Accepted - This relevant study has been quoted in the revised paragraph.
31491	66	40	66	43	However, there is growing evidence that contrasts between wet and dry regimes, that include seasonality, have increased since the 1980s (Liu and, Allan, 2013; Polson et al., 2013; Murray-Tortarolo et al., 2016; Tapiador et al. 2016, Polson and Hegerl, 2017a; Barkhordarian et al., 2018; Lan et al., 2019). Reference: Tapiador, F.J., Behrangi, A., Haddad, Z.S., Katsanos, D., de Castro, M. 2016. Disruptions in Precipitation Cycles: Attribution to Anthropogenic Forcing. Journal of Geophysical Research (Atmospheres). Vol: 121, Pages: 2161–2177,https://doi.org/10.1002/2015JD023406 [Andrés Navarro, Spain]	Accepted - This relevant study has been quoted in the revised paragraph.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17257	66	41	66	43	This contrast is also observed in Australia. Please include reference to Australalia's climate change projection report: CSIRO and Bureau of Meteorology (2015). Climate Change in Australia Information for Australia's Natural Resource Management Regions: Technical Report, CSIRO and Bureau of Meteorology, Australia. Perhaps also saying since the late 20th century as 1980 is very specific. Obviously it varies a little across regions. [Joelle Joelle Gergis, Australia]	Rejected - Could you please suggest a more specific paper quoted in this report? Note that we have quoted Gellego et al.(2017) in the revised Box, as suggested by a later review comment.
29059	66	41			This statement can be backed up for example Dunning et al. (2017) ERL 10.1088/1748-9326/aa869e for west Africa [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The suggested study focuses on CMIP5 model deficiencies rather than observed trends
88961	66	42	66	42	A reference to Schurer et al 2020 could be added here https://doi.org/10.1088/1748-9326/ab83ab. This article is an update on Polson and Hegerl 2017a which uses CMIP6 models and attributes changes to antropogenic and natural forcings separately. A figure from this paper is currently being using in chapter 3. [Schurer Andrew, United Arab Emirates]	Accepted - although the focus is more on spatial than seasonal contrasts
65797	66	45	67	2	Suggest including some examples from Australia, e.g.: - Moise et al 2019 https://doi.org/10.1002/joc.6334 - Gallego et al 2017 https://www.nature.com/articles/s41598-017-16414-1 - Sharmila et al https://www.nature.com/articles/s41598-020-61482-5.epdf, and others. [Kushla Munro, Australia]	Taken into account - Gallego et al.(2017) has been quoted to include one example from Australia
20515	66	45	67	2	European readers will expect to find among these examples the Mediterranean zone, introduced above (8.3.1.8) in relation with droughts; this is a case where the wet season is becoming shorter. [philippe waldteufel, France]	Rejected - Could you please suggest a specific paper that supports this statement?
64979	66	45	67	2	I have trouble seeing the big picture in these reports about timing changes. Can one structure this better to make a clear picture emerge? [Johannes Quaas, Germany]	Rejected - The somehow blurred picture reflects the available literature and the fact that internal variability still obscures the long-term changes in seasonality (as emphasized in the last sentence of the paragraph)
7053	66	46	66	47	This sentence is meaningless as currently worded. With "throughout" it is akin to saying that "every year had a delayed onset", or in politician-speak, "every student should be above average". Is it meant to say something instead about a trend? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - The revised sentence reads as follows: "A later monsoon onset trend was reported throughout India from 1901 to 2013"
42783	66	48	66	49	In the African Sahel, rainfall is more intermittent and concentrated in the late rainy season since the 1990s (Biasutti, 2019).This is not exactly what is written in this paper by Biasuti: she states "the core rainy season is a time when ascent is always possible—so an increase in global specific humidity will intensify rainfall". This does not mean that the end of the rainy season may not end earlier, as analysed in Lebel and Ali (2009). Lebel and Ali (2009) and Biasuti (2019) converge in stating that the wetting starting in the 2000s was mostly concentrated during the peak of the rainy season and characterised by a larger intermettency as compared to the 1950s and 1960s. A possible rephrasing could thus be: "In the African Sahel, rainfall is more intermittent (Panthou et al., 2014) and concentrated in the peak of the rainy season since the 2000s (Biasutti, 2019; Lebel and Ali, 2009). [Thierry Lebel, France]	Taken into account - Thanks for the clarification and suggestion. Only the most recent (post-AR5) studies have been however quoted in the revised sentence.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29055	66	49			Over southern Africa, an observed earlier onset and later cessation is in contrast to simulated historical and projected shortening of the wet season (Dunning et al. 2018 J. Clim) with recent increases in rainfall linked with Pacific internal variability (Maidment et al. 2015 GRL). Another way of writing this paragraph is to briefly list where seasonality has increased, decreased or shifted and then make an assessment that observed changes are primarily explained by internal variability (medium or high confidence) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - The reference about southern Africa has been included. Although appealing, the proposed revised structure has not been implemented since the paragraph is focused on the timing (onset and cessation) and duration of wet seasons rather than on the amplitude of the annual cycle.
7055	66	50	66	51	As in the case above above: do you mean to say an increasing trend thereof? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - "increasing" has been replaced "increasingly" in the revised sentence
59105	66	51	66	52	Do you mean Arias et al., 2015a or 2015b? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - The reference was corrected (Arias et al.; 2015b)
7057	66	52			What are the "contrasting seasonal changes"? Be specific. Are they in contrast to the early onset trend of the previous sentence (i.e. a later onset trend), or varying results among different studies? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - The contrasting changes refer to a delayed onset of the South American monsoon. This was clarified
116755	66		66		Integration x chapters (4, 6, 7) on the issue of the response to aerosol reductions is needed. I think that these aspects are not sufficiently clearly addressed in the TS/SPM : aerosol RF is stabilizing and its geographical pattern has changed; discernable effects; future effects in the case of further reductions (see also chapter 6 and discussions of contrasted SSP7 scenarios for SLCF). [Valerie Masson-Delmotte, France]	Taken into account. While preparing the FGD, we have coordinated with Chapter 6 in assessing the influence of aerosols in climate.
29057	67	5			A shorter snow season is also observed for the USA (Zeng et al. 2018) [GRL https://doi.org/10.1029/2018GL079621] [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Thanks for the relevant reference.
4959	67	22	68	34	Would be good to base entire box on CMIP6 [Bart van den Hurk, Netherlands]	Rejected - not enough available publications to support this request.
53381	67	30	67	31	A regional model study also suggests enhanced rainfall seasonality over the western Maritime Continent (Kang et al., 2018). [Hervé Douville, France]	Noted. The text was changed.
59107	67	33	66	33	Pascale et al., 2016 is missing in the reference list [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - Reference added.
64981	67	35	67	35	“a case study” or rather two? [Johannes Quaas, Germany]	Taken into account - "A case-study for" has been replaced by "Projections over"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17259	67	37	67	37	Hope et al (2015) is missing from the reference list. Is this the one? P Hope, D Abbs, J Bhend, F Chiew, J Church, M Ekström, D Kirono, A Lenton, C Lucas, K McInnes, A Moise, D Monselesan, F Mpelasoka, B Timbal, L Webb, P Whetton (2015). Climate change in Australia projections for Australia's natural resource management regions. CSIRO and Bureau of Meteorology, Canberra, ACT, Australia. [Joelle Joelle Gergis, Australia]	Taken into account - Reference added. Hope, P., Grose, M. R., Timbal B., Dowdy, A. J., Bhend, J., Katzfey, J. J., Bedin, T., Wilson, L., and Whetton, P. H. (2015). Seasonal and regional signature of the projected southern Australian rainfall reduction. Australian Meteorological and Oceanographic Journal, 65, 54–71, https://doi.org/10.22499/2.6501.005.
59109	67	37	67	37	Hope et al., 2015 is missing in the reference list [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - Reference added
7059	67	39	67	41	Why does the Hasson et al. result for a delayed South Asian monsoon rainfall onset in CMIP5 contradict the AR5 report of earlier onset given on line 25? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	The contradictory sentence is dropped in the FGD.
29061	67	39			A shorter wet season by 5-10 days is projected for southern Africa (Dunning et al. 2018 J. Clim). [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Thanks for the relevant reference.
113379	67	41	67	41	latter'? [Diego Miralles, Belgium]	Accepted
29063	67	51			earlier snow melt is projected e.g. by 30 days at the end of the 21st century in RCP4.5 for the Sierra Nevada (Sun et al. 2019) GRL https://doi.org/10.1029/2018GL080362. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Thanks for the relevant reference.
129205	67	53			What is the difference between an ESM and a climate model? [Trigg Talley, United States of America]	Noted - Please refer to the Glossary
12887	68	3			Change "to improve" to "for improving" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
64983	68	5	68	5	"partly support" = in the other parts, contradict these findings? [Johannes Quaas, Germany]	Taken into account - The sentence has been revised and suggests that the lack of full agreement may be due to the use of a specific metric to assess projected changes in seasonality in Fig. 1

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29065	68	6			not clear what "relative seasonality" means. The caption for Box 8.2 Figure 1 needs to define what is being shown in (a) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - The definition is provided in the revised caption.
17261	68	7	68	7	What do you mean by 'southern strom tracks'? Please provide specific regions. [Joelle Joelle Gergis, Australia]	Taken into account - We meant Southern Hemisphere extratropical storm tracks
7061	68	11			Isn't the "little change" of monsoon rainfall seasonality described here in contradiction to the "projected increase in rainfall seasonality" noted since the AR5 on line 27 of page 67? If this contradiction does exist between CMIP5 and CMIP6 results, it should be highlighted. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - As specified at the beginning of the sentence, this paradox is partly explained by the high value of the seasonality index in present-day climate so that the relative increase is weaker.
20517	68	14	68	20	According to the Investopedia page (https://www.investopedia.com/terms/s/seasonality.asp), seasonality "is a characteristic of a time series in which the data experiences regular and predictable changes that recur every calendar year". Whether this definition is accepted or not (if not, it would be nice to include the IPCC definition in the glossary), it is surprizing that when investigating seasonality no effort is made to describe the results with the help of harmonic analysis or similar, more sophisticated tools. [philippe waldteufel, France]	Rejected - We do not plan to add a definition of seasonality in the IPCC Glossary since there are multiple technical definitions which all suggest the seasonal dependence of the variable of interest, as can be understood by the wide IPCC audience.
20519	68	25	68	31	In line with the previous comment, this figure B8.2- 1 illustrates that one should certainly begin by applying this kind of analysis to numerical simulations. In addition however, the figure shows that in this report seasonality is not what the Internet says (see previous comment), but follows (Walsh and Lawler, 1981), probably selecting what they call Seasonality Index. It is recommended that the formula defining this index be included in the legend of the figure [philippe waldteufel, France]	Accepted - The definition is provided in the revised caption.
12889	68	25	69	31	For this and all other figure descriptions, how many members have been used? All available or a single member? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Ensemble sizes included where relevant in FGD.
70249	68	26	68	27	The “(a) 1995-2014 reference” is absent in Figure caption and the other captions are not matched with Figure. [Seogyeong Kim, Republic of Korea]	Accepted - The caption has been completed and corrected.
20521	68	39	69	7	This section is not satisfactory; needs restructuring and rewriting. In case of mentioning CAPE, please consider comment on page 26 line 27 [philippe waldteufel, France]	Accepted. The section has been completely re-written. CAPE is no longer mentioned.
53383	68	39			could deserve at least two separate paragraph and a brief discussion of how convective precipitation is defined in global climate models with parametrized convection? [Hervé Douville, France]	No longer applicable. The section has been completely re-written and convective precipitation is no longer discussed.
64985	68	40	68	50	I don’t see clearly what key message is learned from this paragraph. [Johannes Quaas, Germany]	Accepted. The section has been completely re-written.
89091	68	41			Section 8.2.2.1.2 is no longer present so this reference should be deleted. Section 8.5.1.1.1 is potentially relevant and could be mentioned, though. [Angeline Pendergrass, United States of America]	No longer applicable. The section has been completely re-written and this sentence is no longer present.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29067	68	41			8.2.2.1.2). --> 8.2.3.2 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	No longer applicable. The section has been completely re-written and this sentence is no longer present.
113381	68	43	68	43	what two elements does 'between' refer to here? I am guessing you may mean 'a mismatch in convective precipitation between high-resolution observations (Houze, 1997) and climate model simulations'. [Diego Miralles, Belgium]	No longer applicable. The section has been completely re-written and this sentence is no longer present.
89093	68	45		50	Since this draft was completed, more studies have looked at the effects of convective precipitation and its role in precipitation and its change. Louf et al (2019) is potentially relevant and focuses on Australia. Pendergrass (2020) reviews studies on the role of changing convective organization on extreme precipitation change, and some studies discussed therein are potentially relevant here. Louf, V., Jakob, C., Protat, A., Bergemann, M., & Narsey, S. (2019). The Relationship of Cloud Number and Size With Their Large-Scale Environment in Deep Tropical Convection. Geophysical Research Letters, 46(15), 9203–9212. https://doi.org/10.1029/2019GL083964 Pendergrass, A. G. (2020). Changing Degree of Convective Organization as a Mechanism for Dynamic Changes in Extreme Precipitation. Current Climate Change Reports, 6(2), 47–54. https://doi.org/10.1007/s40641-020-00157-9 [Angeline Pendergrass, United States of America]	No longer applicable. The section has been completely re-written and convective precipitation is no longer discussed.
88143	68	48	68	48	An alternative description of why CAPE increases under warming was proposed by Singh et al. Increasing potential for intense tropical and subtropical thunderstorms under global warming, PNAS, 2017 114 (44) 11657-11662 [Robin Chadwick, United Kingdom (of Great Britain and Northern Ireland)]	No longer applicable. The section has been completely re-written and CAPE is no longer discussed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
69513	68	48	68	49	Has anyone made the argument that increased CAPE with warming should lead to MORE convective precipitation? I don't think it is made in any of the papers cited here. Moreover, I don't think it is correct; the amount of convection tends to be not very sensitive to CAPE, but rather depends on large-scale convergence. See for example, in observations: Davies, L.; Jakob, C.; May, P.; Kumar, V. V. & Xie, S. Relationships between the large-scale atmosphere and the small-scale convective state for Darwin, Australia J. Geophys. Res.: Atmos., Wiley Online Library, 2013, 118, 11534-11545 and in idealised modelling/theory Loriaux, J. M.; Lenderink, G.; De Roode, S. R. & Siebesma, A. P. Understanding Convective Extreme Precipitation Scaling Using Observations and an Entraining Plume Model J. Atmos. Sci, 2013, 70, 3641-3655 Singh, M.S., Warren, R.A. & Jakob, C.J. (2019). A steady-state model for the relationship between humidity, instability, and precipitation in the tropics, J. Adv. Model. Earth Syst., 11, doi:10.1029/2019MS001686. Also, if the statement about CAPE is kept in, I would suggest different citations: Studies which have shown projected increases in CAPE over large regions of the globe include: Sobel, A. H. & Camargo, S. J. Projected future seasonal changes in tropical summer climate J. Climate, 2011, 24, 473-487, S Singh, M. S.; Kuang, Z.; Maloney, E. D.; Hannah, W. M. & Wolding, B. O. Increasing potential for intense tropical and subtropical thunderstorms under global warming Proc. Nat. Acad. Sci. USA., 2017, 114, 11657 - 11662.	No longer applicable. The section has been completely re-written and CAPE is no longer discussed.
12891	68	55			Change "it is theorized" to "it has been theorized" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	No longer applicable. This phrase is no longer included.
16133	69	4	69	5	"Another shows that..." This sentence reads like logically linked to the preceding one, but it only refers to mean quantities, while the preceding sentence talks abot heavy snowfall events. That's a bit confusing. [Gerhard Krinner, France]	No longer applicable. The section has been completely re-written.
132223	69	10	70	7	Given that there is a full chapter on "Weather and climate extremes" it is not very useful to have this topic being treated here. In particular, it does not seem to be well referring to chapter 11. There is only a short mention to Section 11.4 on heavy precipitation, but the text also mentions "consecutive dry days" and the right section to refer to this is Section 11.6 but is not mentioned here. Please coordinate this text well with chapter 11 or remove. [Sonia Seneviratne, Switzerland]	Accepted. The section has been re-written and now no-longer has subsections.
29069	69	10			there is some overlap between 8.4.1.3.2 and 8.4.1.3.3 which could probably be combined [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Section 11.4 is now summarized.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28977	69	11			could summarise 11.4 when discussing extreme precipitation e.g. "Over almost all land regions, it is very likely that extreme precipitation will be more intense and more frequent in a warmer world with intensities increasing close to the 7% per oC thermodynamic increases in low level moisture but with large differences in the increase regionally." [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The entire section has been re-written.
4961	69	14	69	14	It was not clear that fig 8.16 shows interannual variability (caption doesn't specify the kind of variability plotted) [Bart van den Hurk, Netherlands]	Accepted. Caption now specifies CMIP6.
27257	69	14	69	15	Figure 8.16 shows the results for CMIP6, not CMIP5 [Eric Brun, France]	No longer applicable. The sentence is no longer included.
21071	69	14	69	20	Please indicate in the caption of fig 8.16 which latitudes limit the tropics (30S-30N?) and whether SH extratropics are also included. [Marcelo Barreiro, Uruguay]	Accepted. The legend now specifies "year-to-year" variability.
89095	69	14		20	The paragraph needs to be rewritten. Fig 8.16 only slows land mean values averaged over the tropics and extratropics, and does not show the months discussed in the paragraph (which sound like they should be in the figure). Instead these are probably discussed in the papers that are cited here. [Angeline Pendergrass, United States of America]	Accepted (SH extratropics) and rejected (definition of tropics). The SH component is now explained. As the standard definition for the "tropics" is used throughout, it is not explicitly defined in the figure captions.
17263	69	15	69	15	Do you mean Figure 8.15? [Joelle Joelle Gergis, Australia]	No longer applicable. The sentence is no longer included.
12893	69	17			Change "variability increase" to "the increase in variability" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The Berthou et al. (2019) reference has been added.
70251	69	26	69	28	The regions and seasons represented in the Figure caption are not matched with Figure 8.16. The caption “extra-tropical land in (a) summer and (b) winter” would be changed to (c) and (d). In addition, the “tropical land in (c) JJA and (d) DJF” needs to modify as the caption of (a) and (b). They should be rearranged. [Seogyeong Kim, Republic of Korea]	Accepted – done
12895	69	28			Why does the nomenclature identifying seasons change half way through the caption? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account : winter here means a combination of DJF for Northern hemisphere and JJA for southern hemisphere ; this has been clarified in the caption and figure
132219	69	37	69	37	This text mentions the "intensification of the global water cycle", which I would argue is a wrong concept in the literature. I noted that the chapter 8 ES did not mention this notion, but it would be an important point to clarify in this chapter, and possibly also in the TS and SPM. [Sonia Seneviratne, Switzerland]	Accepted. The text has been rewritten for consistency and now specifically mentions a rate close to 7% per 1°C of global warming
88913	69	37	69	47	This increase in rain intensity and longer dry spell is also really well illustrated by analysis of a convection-permitting simulation over Africa, eg. Kendon et al. (2019), Berthou et al. (2019). Berthou, S., Kendon, E.J., Roberts, M., Rowell, D.P., Tucker, S. and Stratton, R., 2019: Larger future intensification of rainfall in West Africa in a convection-permitting model. Geophys. Res. Lett., 46, 13299-13307 [Dave Rowell, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The concept and the associated complexities is discussed in Section 8.2.1 and Section 8.3.1.1.
129207	69	37	69	47	[PRECISION] This section is out of synch with SPM-29, C.5.3, line 40-41 (based on Chapter 11, Section 4) which states the magnitude of extreme precipitation is projected to increase by approximately 7% per 1°C warming. [Trigg Talley, United States of America]	Accepted. This phrasing is now used.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6721	69	39	69	41	Is the text contradictory here? The first sentence refers to "fewer but potentially stronger events" and the second to an increase "in the frequency of extreme precipitation events". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text has been rewritten for greater clarity.
89099	69	39		40	We provided a quantative description of how the precipitation frequency and amount distributions change with warming in Pendergrass and Hartmann (2014), and developed a model showing how this can relate to changes in moisture and circulation in Pendergrass and Gerber (2016). Pendergrass, A. G., and D. L. Hartmann. "Changes in the Distribution of Rain Frequency and Intensity in Response to Global Warming." J. Clim. 27 (2014). https://doi.org/10.1175/JCLI-D-14-00183.1. Pendergrass, Angeline G., and Edwin P. E.P. Gerber. "The Rain Is Askew: Two Idealized Models Relating Vertical Velocity and Precipitation Distributions in a Warming World." Journal of Climate 29, no. 18 (2016): 6445–62. https://doi.org/10.1175/JCLI-D-16-0097.1. [Angeline Pendergrass, United States of America]	Accepted. The section has been re-written to be more complementary to the material in Chapter 11, and to refer it.
89097	69	40		42	A related finding is that accumulation of the heaviest events will increase with warming Neelin, J. D., Sahany, S., Stechmann, S. N., & Bernstein, D. N. (2017). Global warming precipitation accumulation increases above the current-climate cutoff scale. Proceedings of the National Academy of Sciences of the United States of America, 114(6), 1258–1263. https://doi.org/10.1073/pnas.1615333114 [Angeline Pendergrass, United States of America]	Accepted. The Neelin et al. (2017) reference has been added.
21073	69	46	69	46	increAses [Marcelo Barreiro, Uruguay]	Accepted. Corrected.
113383	69	46	69	46	increses'. [Diego Miralles, Belgium]	Accepted. Corrected.
129209	69	46			After ""... the number of dry days increases in several regions."" add the following sentences: ""Na et al. (2020) showed that discrepancy exists in the projected nonprecipitation day changes between CMIP6 model and those from a global cloud resolving model (NICAM). The latter shows that nonprecipitation days will increase over most of low and middle latitudes (see their Figure 11a)."" Citation: Na, Y., Q. Fu, and C. Kodama, 2020: Precipitation Probability and Its Future Changes From a Global Cloud-Resolving Model and CMIP6 Simulations. J. Geophys. Res. Atmos., 125, 5, doi:10.1029/2019JD031926. [Trigg Talley, United States of America]	Rejected. While this is an interesting paper, for space reasons and because comparing CMIP6 results to results in cloud resolving models is at an early stage, we have not included this level of discussion.
129211	69	47			This section (and some later sections) don't include summary paragraphs. Is this intentional? [Trigg Talley, United States of America]	Accepted. A summary section has been added.
113385	70	8	70	8	Land surface evapotranspiration', ET is only land surface. [Diego Miralles, Belgium]	Noted, changed to Evapotranspiration.
89163	70	8	71	3	This section should cross reference the discussion of ET and potential ET in Chapter 11, section 11.6, and specifically 11.6.1.2 and 11.6.2.2 among others [Angeline Pendergrass, United States of America]	Noted. We are coordinating with Ch. 11 to improve the assessment
59121	70	8	71	3	It is not logic, that evaporation over oceans is not mentionned in a specific section or in a joint section with evapotranspiration over the land surface.. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted, changed to Evapotranspiration.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
113387	70	10	70	16	This discussion on the projected changes reads a bit odd. In reality, the projected changes do resemble precipitation changes, but are not necessarily the causal result of precipitation changes. Why? In energy-limited regions, ET is mainly driven by energy (temperature, radiation). In water-limited regions, ET is driven mainly by P changes (see e.g. Miralles et al., 2011). However, because in energy-limited region, P changes are also largely driven by energy changes, the trends in P and ET still agree. Therefore: in water-limited, ET and P agree because changes in P drive ET, and in energy limited they agree because both are driven by temperature. As a result the projected trends in P and E agree pretty much everywhere. And needless to say, part of the P changes are caused by ET. Miralles, D. G., De Jeu, R. A. M., Gash, J. H., Holmes, T. R. H. and Dolman, A. J.: Magnitude and variability of land evaporation and its components at the global scale, Hydrol. Earth Syst. Sci., 15(3), 967–981, doi:10.5194/hess-15-967-2011, 2011. [Diego Miralles, Belgium]	Noted. These sentences have been rephrased so that this is less confusing.
23317	70	11	70	16	The following paper can also suport the drivers of evapotranspiration: Zeng, Z., et al. (2018). "Impact of Earth greening on the terrestrial water cycle." Journal of Climate; Zeng, Z., et al. (2018). "Response of terrestrial evapotranspiration to Earth's greening." Current Opinion in Environmental Sustainability 33: 9-25; Zeng, Z., et al. (2016). "Responses of land evapotranspiration to Earth's greening in CMIP5 Earth System Models." Environmental Research Letters 11(10): 104006. [Zhenzhong Zeng, China]	Noted. Since these studies concern historical changes, they are cited in the Observed Changes section of this Chapter (8.3.1.6).
70335	70	14	70	16	This section goes on to discuss neither irrigation or land use change. The section does discuss how plant responses to a changing climate do impact water availability on land, thus we suggest that the above sentence be modified to explicitly list "plant physiological responses". One such possible wording is "At the regional scale, plant physiological responses in increasing atmospheric CO2, irrigation, and land use change may also represent major drivers of future changes in evapotranspiration. [Abigail Swann, United States of America]	Noted. Since irrigation and land use change is not assessed here we removed this sentence.
27259	70	15	70	16	References are needed [Eric Brun, France]	Noted. This sentence has been removed.
6723	70	18	70	18	Is it really "evapotranspiration" or just plain "evaporation"? If it is "evapotranspiration" the vegetation has to be specified in the computation of it, as well as meteorological data. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This sentence has been removed/modified to clarify that we are talking here about atmospheric demand, which can be quantified in a number of different ways.
113389	70	18	70	20	They can also mean other things. Recent reviews on the different meanings of 'potential evaporation' and 'atmospheric demand' are found here: Vicente-Serrano, S. M., Mcvicar, T. R., Miralles, D. G., Yang, Y. and Tomás-Burguera, M.: Unraveling the influence of atmospheric evaporative demand on drought and its response to climate change, WIREs Clim Change, 11(2), 1–31, doi:10.1002/wcc.632, 2020. and here: Maes, W. H., Gentine, P., Verhoest, N. E. C. and Miralles, D. G.: Potential evaporation at eddy-covariance sites across the globe, Hydrol. Earth Syst. Sci., 23(2), 925–948, doi:10.5194/hess-23-925-2019, 2019. [Diego Miralles, Belgium]	Noted. We now reference the Vicente-Serrano paper here and refrain from giving a specific definition of AED as it can be assessed in a number of ways
113391	70	20	70	20	high-emission scenario' [Diego Miralles, Belgium]	Noted. This has been rephrased
113393	70	21	70	21	Reference out of brackets [Diego Miralles, Belgium]	Accepted, this has been fixed/rephrased
29071	70	21			also Greve and Seneviratne (2015) GRL 10.1002/2015GL064127 who further show significant increases in aridity (approximated from P minus a PET proxy) affect only 16% of all land areas in end of century RCP8.5 projections. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Greve and Seneviratne (2015) GRL is included in the FGD (page 19, line 54).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43227	70	21			Read "models, Scheff and Frierson (2014) found a robust increase " rather than "models, (Scheff and Frierson, 2014) found a robust increase " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted, this has been fixed/rephrased
113395	70	22	70	22	constant-relative humidity warming? No idea what thtmay mean... [Diego Miralles, Belgium]	Noted. This has been rephrased
113397	70	22	70	22	Cof' [Diego Miralles, Belgium]	Accepted, this has been fixed/rephrased
43229	70	22			Read "(5-6% per °C of local warming " rather than "(5-6% per °Cof local warming " [Cyriaque Rufin Nguimalet, Central African Republic]	Noted. This has been rephrased
113399	70	23	70	23	where' for 'when' [Diego Miralles, Belgium]	Accepted, this has been fixed/rephrased
70337	70	26	70	27	We disagree with this statement. Analysis of other CMIP5 and CMIP6 runs does not support an increase in land ET with medium confidence, nor is this seen in Figure 8.18 in this chapter. The paper cited analyzes RCP 4.5 only, which would have smaller plant physiological responses than scenarios with higher CO2 concentrations. In addition to Figure 8.18 in this chapter, Swann et al. 2016 shows ET changes for 1pctCO2 runs (Figure 1F, Figure 2A, Supplemental Figure 2F) and finds little change in ET over mid and low-latitude land in CMIP5. Zarakas et al. (in revision, preprint doi:10.31223/osf.io/emgxb) shows ET changes for 1pctCO2 runs in CMIP6 (Supplemental Figure S7), with similar findings to Swann et al. 2016. In particular, land ET decreases in many regions (e.g. the Amazon, Central America, Southern Africa, Southern Europe) and there is poor model agreement on the sign of ET changes over much of the land surface. [Abigail Swann, United States of America]	Rejected. Figure 8.18 has been updated with more models. It does show a general decrease over land in three scenarios, supporting our medium confidence assessment. Plant effects are assessed separately since there is some uncertainty in whether these processes are adequately captured by models
6725	70	27	70	27	This is the third time the acronym ET has been defined in this chapter. The first is on page 23, line 38 and the second on page 36 line 31. The first one should be sufficient. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, this has been fixed/rephrased
29077	70	27			can a 90% confidence range be estimated instead? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. There is no summary confidence interval in this paper but the statement has been modified to "most land areas" recognizing that there are some regions where ET does not increase.
113401	70	30	70	30	I do not understand the logic at 'However, regional changes in ET are not only governed by changes in precipitation,' You were talking about temperature as driver, not precipitation, in the previous sentence. [Diego Miralles, Belgium]	Accepted, this has been fixed/rephrased

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70339	70	32	70	37	This juxtaposition suggests that the plant responses play an unimportant role if they do not fully compensate for increased evaporative demand. Advances in assessment of expected drought and aridity in the future now find that it is, in fact, these large plant responses pushing the system towards less ET which counteract large increases in the demand by the atmosphere for ET flux resulting in relatively little total change in ET (Roderick et al. 2015, Milly and Dunne 2016, Swann et al. 2016, Bonfils et al. 2017, Lemordant et al. 2018, Berg and Sheffield 2018, Scheff 2018, Swann 2018). Thus the narrative would be more correctly framed as follows: "Plant photosynthesis and growth responses to increasing CO2 (growing season length, leaf area) work in combination with higher evaporative demand to increase evapotranspiration (Frank et al., 2015; Mankin et al., 2017, 2018, 2019; Guerrieri et al., 2019) (Section 8.3.1.4); however, plant physiological responses to increasing CO2 (stomatal closure) counteract the drivers leading to higher ET, resulting in moderate change in evapotranspiration over land as CO2 increases (Milly and Dunne, 2016; Swann et al. 2016; Lemordant et al., 2018)" [Abigail Swann, United States of America]	Noted. These sentences have been rephrased, however the report-wide assessment of plant physiological impacts is that there is currently low confidence that they can counteract increases in ET due to higher AED.
74383	70	33	70	33	to replace an with a in the expression … to a decrease … [Moulay Driss HASNAOUI, Morocco]	Accepted, this has been fixed/rephrased
93651	70	33	70	33	Replace "an decrease" with "a decrease" [Stefano Materia, Italy]	Accepted, this has been fixed/rephrased
29073	70	33			Yang et al. (2018) Nature Clim.: Evapotransporation increases driven by vapour pressure deficit in a warmer world are almost entirely offset by increased water use efficiency in simulations that account for plant stomatal response to elevated CO2 levels, countering the argument that warming leads to drying. Section 8.2.3.1 could also be referred to here (but see it is at the end) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The Yang study has been added.
74385	70	35	70	35	to add index to … leaf area index. [Moulay Driss HASNAOUI, Morocco]	Accepted, this has been fixed/rephrased
113403	70	36	70	36	This are probably the only explicit papers looking at the competing effects of 'greeening' and water use efficiency on ET trends: Cheng, L.,et al.: Recent increases in terrestrial carbon uptake at little cost to the water cycle, Nature Communications, 1–10, doi:10.1038/s41467-017-00114-5, 2017. Forzieri, G., Miralles, D. G., Ciais, P., Alkama, R., Ryu, Y., Duveiller, G., Zhang, K., Robertson, E., Kautz, M., Martens, B., Jiang, C., Arneth, A., Georgievski, G., Li, W., Ceccherini, G., Anthoni, P., Lawrence, P., Wiltshire, A., Pongratz, J., Piao, S., Sitch, S., Goll, D. S., Arora, V. K., Lienert, S., Lombardozzi, D., Kato, E., Nabel, J. E. M. S., Tian, H., Friedlingstein, P. and Cescatti, A.: Increased control of vegetation on global terrestrial energy fluxes, Nature Climate Change, 1–22, doi:10.1038/s41558-020-0717-0, 2020. [Diego Miralles, Belgium]	Noted. These studies concern observed changes so are mentioned in Section 8.3 of this chapter
113405	70	36	70	36	An effect that I am missing here is the stress caused by VPD increases on transpiration (via stomatal regulation). The reference to Vicente-Serrano etal. (2020) is good in that sense. Maybe another reference: Zhou, S., Williams, A. P., Berg, A. M., Cook, B. I., Zhang, Y., Hagemann, S., Lorenz, R., Seneviratne, S. I. and Gentine, P.: Land-atmosphere feedbacks exacerbate concurrent soil drought and atmospheric aridity, Proceedings of the National Academy of Sciences, 6, 201904955–6, doi:10.1073/pnas.1904955116, 2019. [Diego Miralles, Belgium]	Accepted, these references have added to emphasize the impact of atmospheric demand/VPD

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70341	70	37	70	39	The confidence that vegetation plays a role is actually very high, while the confidence in the net magnitude of that response combined with other responses is low. Without vegetation responses to increasing CO2, the ET would look more like the Potential ET (i.e. very large increases, Scheff and Frierson 2014), but vegetation responses to CO2 dramatically alter the resulting ET (Swann et al. 2016 Figure 1F). The resulting ET change is thus much closer to zero although confidence in the exact final net magnitude is low. We suggest the following wording for the second sentence: “Thus there is high confidence that vegetation modulates future projections of evapotranspiration, but low confidence in the net magnitude of change when combined with other factors influencing evapotranspiration. [Abigail Swann, United States of America]	Noted. See also assessment of this issue in Sections 8.2 and 8.3. In 8.2 we state there is high confidence that WUE will increase, however how much this will effect ET in the future is low confidence. our assessment here is consistent with the rest of the chapter.
4965	70	38	70	38	Is this "low confidence" justified? We do know about increase in WUE, about options of vegetation to adjust to changes in growing season length etc. [Bart van den Hurk, Netherlands]	Noted. The low confidence is justified given conflicting studies.
74387	70	38	70	38	to put capital letter after . Like . There is … [Moulay Driss HASNAOUI, Morocco]	Accepted, this has been fixed/rephrased
93645	70	38	70	38	Add reference: Peano et al., 2019. ref. Peano, D., Materia, S., Collalti, A., Alessandri, A., Anav, A., Bombelli, A., & Gualdi, S. (2019). Global variability of simulated and observed vegetation growing season. Journal of Geophysical Research: Biogeosciences, 124(11), 3569-3587. [Stefano Materia, Italy]	Accepted, reference added
113407	70	38	70	38	), for). [Diego Miralles, Belgium]	Accepted, this has been fixed/rephrased
43231	70	38			Read " insufficient (Franks et al., 2017; Peters et al., 2018). There is thus low" rather than " insufficient (Franks et al., 2017; Peters et al., 2018), There is thus low" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted, this has been fixed/rephrased
12897	70	41			Here and elsehere: "similar ... to" not "simlar ... as" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Fixed
70343	70	45	70	45	Is there a reference for this? Is it due to drier soils or drier soils in combination with stomatal closure/increased water use efficiency? [Abigail Swann, United States of America]	Noted. Not all models provide WUE output and so this is not assessed in the Chapter. However comparison with the soil moisture assessment suggests that drying soils is a likely cause. We know reference that figure here.
4969	70	46	70	47	This statement is not true in (Eastern) Asia [Bart van den Hurk, Netherlands]	Noted. This statement has been deleted.
113409	70	48	70	48	...counterbalancing the effects of a higher atmospheric demand on ET' [Diego Miralles, Belgium]	Noted. This statement has been deleted.
70345	70	50	70	51	Why is the confidence stated here different from line 26 on the same page? Further, we disagree that increases in evapotranspiration over land are a robust feature in future projections, nor is this seen in Figure 8.18 in this chapter. In addition to Figure 8.18 in this chapter, Swann et al. 2016 shows ET changes for 1pctCO2 runs (Figure 1F, Figure 2A, Supplemental Figure 2F) and finds little change in ET over mid and low-latitude land in CMIP5. Zarakas et al. (in revision preprint doi:10.31223/osf.io/emgxb) shows ET changes for 1pctCO2 runs in CMIP6 (Supplemental Figure S7), with similar findings to Swann et al. 2016. In particular, land ET decreases in many regions (e.g. the Amazon, Central America, Southern Africa, Southern Europe) and there is poor model agreement on the sign of ET changes over much of the land surface. If this statement refers to changes in land evaporation rather than land evapotranspiration, we think that this statement should be edited to clarify this distinction. [Abigail Swann, United States of America]	Noted. We have changed the confidence statement to match line 26, keeping "medium confidence" based on CMIP6 results but acknowledging that plant physiology contributes to lingering uncertainty.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
113413	70	50	70	55	Essentially it increases everywhere but in water-limited regions. In this summary it is not clear. [Diego Miralles, Belgium]	Accepted, this has been added to the summary
112231	70	50			Not sure if robust in a statistical manner or used colloquially. Probabaly the former [Rutger Hofste, Netherlands]	Accepted, this has been fixed/rephrased
29075	70	50			Increases over the ocean do not seem to be mentioned in the section body so perhaps could be with reference to Figure 8.18 to substantiate this summary statement [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, fixed.
113411	70	52	70	52	What does 'seasonal ET' mean here? What season? OR do you man 'seasonality of ET'? Clarify. [Diego Miralles, Belgium]	Accepted, this has been fixed/rephrased
70347	70	54	70	54	We disagree that increases in evapotranspiration over land are a robust feature in future projections, nor is this seen in Figure 8.18 in this chapter. In addition to Figure 8.18 in this chapter, Swann et al. 2016 shows ET changes for 1pctCO2 runs (Figure 1F, Figure 2A, Supplemental Figure 2F) and finds little change in ET over mid and low-latitude land in CMIP5. Zarakas et al. (in revision preprint doi:10.31223/osf.io/emgxb) shows ET changes for 1pctCO2 runs in CMIP6 (Supplemental Figure S7), with similar findings to Swann et al. 2016. In particular, land ET decreases in many regions (e.g. the Amazon, Central America, Southern Africa, Southern Europe) and there is poor model agreement on the sign of ET changes over much of the land surface. [Abigail Swann, United States of America]	Noted. As stated above, our medium confidence statement is supported by Figure 8.18 however we have the caveat that this increase is over "most land areas" and notably not in dryland areas.
116757	70		70		integration with ch 5 needed here (CO2 effect on plant physiology). [Valerie Masson-Delmotte, France]	Noted, the text in the FGD is consistent with other chapters, but unsure what the reviewer is referring to on the mentioned figure.
70349	71	1	71	3	We believe the statement “ET is projected to increase [...] in most areas with no major change in seasonal precipitation” is supported primarily by a comparison of figures 8.15 and 8.18. However, this comparison is not discussed earlier in this section. Additionally, given that land ET does not robustly increase in projections, the second half of this sentence needs to be re-written. We suggest “Increased atmospheric demand for water (i.e. increased PET) acts to increase ET (high confidence), while vegetation responses to global warming and enhanced atmospheric CO2 concentration generally decrease ET (medium confidence). The net ET change that results from the combination of these two processes varies regionally and across models. ET is projected to increase at the expense of runoff and water availability in most areas with no major change in seasonal precipitation (medium confidence).” [Abigail Swann, United States of America]	Noted. These sentences have been removed/revised.
29081	71	5			Could link to FAQ8.2 and Section 8.2.3.2 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, links to Section 8.2 and also the cross-chapter box were added in the FGD version
17265	71	21	71	21	Remove 'For runoff' at the start of this sentence [Joelle Joelle Gergis, Australia]	Accepted.
113417	71	23	71	23	Still referring to AR5? Clarify. [Diego Miralles, Belgium]	Accepted. This is referring to WGII and that has been clarified.
17267	71	25	71	26	What does 'fractional change' actually mean? Simplify and clarify [Joelle Joelle Gergis, Australia]	Not applicable. That sentence has been deleted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12899	71	28	71	29	South Asia should also be capitalized for consistency with Southeast Asia [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Note also that a copyedit will be completed prior to publication.
6727	71	35	71	36	Change "mean global temperature" to "global-mean temperature". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Editorial – copyedit to be completed prior to publication
53385	71	35	71	36	More emphasis could be put on the recent quoted study by Lehner et al. (2019) which highlights that CMIP5 models display a large spread of runoff sensitivity for present-day climate, which projects onto runoff changes under global warming but may be partly constrained with observations. [Hervé Douville, France]	Accepted. The spread in sensitivity is now mentioned.
12901	71	35			Why not "global-mean"? What does "global-scale mean" imply that is different? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The phrase has been changed to just 'global mean.'
17269	71	39	71	39	Amend to read 'southern Australia' [Joelle Joelle Gergis, Australia]	Accepted.
96811	71	43	71	43	A bit exotic that CMIP6 SSP3-7.0 is shown here. In many other sections SSP5-8.5 is displayed. Please be consistent. [Nicole Wilke, Germany]	Accepted. SSP1-2.6, SSP2-4.5, and SSP5-8.5 are now consistently shown.
59125	71	43	71	43	Verb is missing in this sentence - meaning of the sentence is unclear. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The sentence has been edited.
4971	71	43	71	50	Language in this section misses some words. Also I cannot confirm the likely increase in runoff in figure 8.20d, it looks like an overall reduction in runoff [Bart van den Hurk, Netherlands]	Accepted. The section has been edited. The global mean of the runoff changes is difficult to estimate by eye, both because of the spatial complexity and because the changes are shown in percent.
12903	71	43			This doesn't appear to be a full sentence. Something is missing, e.g. how the runoff has changed as depicted in Figure 8.20. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. A comparison to AR5 is now provided.
129213	71	43			Figure 8.20 is referred to before Figure 8.19. [Trigg Talley, United States of America]	Accepted. The original Figs 19 and 20 have been reordered.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29079	71	44			Guerrieri et al. (2019) PNAS 10.1073/pnas.1905912116: Recent increases in forest water use efficiency dominated by enhanced photosynthesis with reduced stomata conductance only important for species experiencing moisture limitation based on a 30 year tree ring record [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The reference and finding have been added.
6729	71	45	71	45	"be" should be inserted after "to". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Note also that a copyedit will be completed prior to publication.
12905	71	45			Change the wording to, "to be somewhat non-linear" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Note also that a copyedit will be completed prior to publication.
24327	71	48	71	48	"within *the* models that [Jonghun Kam, Republic of Korea]	Accepted. Note also that a copyedit will be completed prior to publication.
113419	71	48	71	48	Correct 'that models' [Diego Miralles, Belgium]	Accepted. Note also that a copyedit will be completed prior to publication.
38073	71	48	71	48	"within *the* models that [Junhee Lee, Republic of Korea]	Accepted. Note also that a copyedit will be completed prior to publication.
12907	71	48			Change "hemosphere" to "hemisphere" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Note also that a copyedit will be completed prior to publication.
53387	71	49	71	50	Rather in a final summary and split the paragraph when you move to streamflow. [Hervé Douville, France]	Accepted. A new paragraph has been added for streamflow and the summaries have been put together in a separate paragraph at the end of the section.
96813	71	53	53	43	"as widely" as what? [Nicole Wilke, Germany]	Accepted. The sentence has been edited to clarify: not as widely explored as variables directly included in the climate models
65799	71	53	71	55	Suggest considering whether Australia be dealt with explicitly in this section. [Kushla Munro, Australia]	Rejected. The listed areas are those highlighted in the Doll (2018) reference.
74389	71	54	71	54	in place of "," to decrease put "and" to decrease because of the end of the sentence. [Moulay Driss HASNAOUI, Morocco]	Accepted. Note also that a copyedit will be completed prior to publication.
74391	72	1	72	1	how inondated lands can be exposed to decreased low flows? [Moulay Driss HASNAOUI, Morocco]	Rejected. It is not clear how the question replies to the text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
20151	72	5	72	5	Since we have here a summary of what is presented in chapter 11, external readers may limit themselves to chapter 11, while chapter 11 writers are the best experts to review the summary given in chapter 8… [philippe waldteufel, France]	Noted. The summary here has been provided to the chapter 11 authors for review.
109697	72	5	72	14	This section is good but it could stand to be a little clearer about the basic framework. Flood risks are increasing, and will continue to do so, due to three major factors: climate change-induced increases in precipitation intensity; urbanization, with associated changes in impermeable area and destruction of natural drainage systems; and increased numbers of people living in flood-prone areas, such as floodplains. [Sean Fleming, United States of America]	Accepted. We have added a note that water resources management and other human-controlled factors are beyond the scope of Working Group I. Those factors, including urbanization and development in floodplain, are certainly important but are in the remit of Working Group II.
53389	72	5			May be however quote here the recent study by Arnell et al. (2019) which provides global numbers for 50-yr return period river flood increases at +1.5 and +4°C GWL respectively? [Hervé Douville, France]	Rejected. The assessment of flooding is done by Chapter 11 and it would be inconsistent to modify it here.
96815	72	7	72	7	"basin characteristics" --> "basin and river characteristics" [Nicole Wilke, Germany]	Accepted.
113421	72	16	72	16	add 'fluvial and pluvial' [Diego Miralles, Belgium]	Accepted.
16135	72	21	72	24	"There are not sufficient studies yet for a confident projection… but… can be expected…" - does this mean that there is low confidence in these findings? Can this be written explicitly in calibrated language? [Gerhard Krinner, France]	No longer applicable. This sentence is no longer included.
89159	72	29		47	This section should cross reference the discussion of soil moisture in Chapter 11, section 11.6, subsections 11.6.1.3 and 11.6.2.3 and others [Angeline Pendergrass, United States of America]	Accepted, references to Ch. 11 have been added in the revised draft.
65801	72	31	72	47	Suggest including Australian conditions, which are currently not mentioned. For example, see: Vogel et al 2017 (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016GL071235) which includes northern Australia as one study areas. [Kushla Munro, Australia]	Noted. This study discusses the contribution of soil moisture to extremes, so is best assessed in Ch. 11. However we now discuss the projected decline in southwestern Australia.
17271	72	31	72	47	Soil moisture is projected to decline in southern Australia, please incorporate this into this section. Reference: I90 [Joelle Joelle Gergis, Australia]	Accepted, southern Australia is noted. This is seen in the CMIP6 projections.
129215	72	38			The reference to Figure 8.21 doesn't seem right. [Trigg Talley, United States of America]	Accepted, fixed.
104419	72	39	72	40	Ukkola et al., accepted, GRL 2020 (DOI: 10.1175/JCLI-D-16-0863.1) also report better agreement in CMIP6 drying in Amazonia. [Luke Parsons, United States of America]	Noted, this study concerns drought duration and intensity so is discussed further down in this section.
104421	72	40	72	40	Parsons et al., submitted- now submitted to Earth's Future, under review under new title. [Luke Parsons, United States of America]	Accepted, fixed.
12909	72	40			"Parsons et al., submitted" has not been included in the reference list and therefore cannot be assessed by a reviewer. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, fixed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98081	72	45	72	46	Re the high confidence in projected decreases in soil moisture in South Africa, while models seem to agree on projected decrease in precipitation in southern Africa, observed trends and climate change detection analysis is indicating less than high confidence (Knutson and Zeng 2018). In terms of detectable anthropogenic influence on precipitation, only a small region along the west coast of southern Africa has a detectable decrease since 1901, with most trends in southern Africa being non-detectable. Similarly for 1951-2010 most observed trends in the region are non-detectable, though models and observations agree on the general sign (negative) of trends particularly over the eastern part of southern Africa. But to 1981-2010, most precipitation trends in the southern subtropical Africa region are positive (but not detectable) in observations, yet negative in model historical runs, suggesting strong internal variability in the region. So I think that high confidence is not justified for the projected soil moisture decline in South Africa, except for those regions like along the western coast of southern Africa where a detectable negative trend in precipitation has apparently already emerged. Refs: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637 https://doi.org/10.1175/JCLI-D-17-0672.1. [Thomas Knutson, United States of America]	Noted. The observed trends in precipitation in south Africa are discussed in Section 8.3 where it is noted that the precipitation trends do not merit high confidence but only medium confidence. However the CMIP6 models simulate a robust drying in southern Africa in the future, which merits high confidence.
53391	72	46			Note the recent study by Parson (2020) showing a more dramatic and widespread drying across Amazonia in CMIP6 compared to CMIP5 models. [Hervé Douville, France]	Accepted, fixed.
24139	73	9	73	9	Why is permafrost not treated? [Wilfried Haeberli, Switzerland]	Noted. Permafrost is assessed in Chapter 9, Section 9.5.2
113423	73	11	73	11	Some of these sections no longer have an 'lun summary' last paragraph. I would check and homogenize this. [Diego Miralles, Belgium]	Noted. All paragraphs now conclude with the key finding.
3041	73	11	73	39	This section seems to be largely based on one single work (Huss and Hock, 2018). Here, the literature has probably more to offer. The comment is of particular relevance since Chapter 9 defers to this Chapter (and this Section) for all what is related to hydrological changes due to glacier retreat. Probably, one or the other Chapter needs to provide more details. [Daniel Farinotti, Switzerland]	Agreed. In rewriting this section many more references have been added and more information on hydrological changes is now provided.
14801	73	11	73	40	For this entire section: enlist a Lead Author from Chapter 9 to re-write, consistent with extensive Chapter 9 content on glacier and ice sheet changes. This section is lacking critical details (for example, mention of trends to the largest glacier/ice sheet reservoir of all, Antarctica) and consistent cross-referencing to Chapter 9 content which speaks much more authoritatively to the topic of changes to ice sheets/glaciers. Ultimately, this section should become a short summary of Chapter 9 findings. [Jeremy Fyke, Canada]	Agreed. This section has been rewritten by LAs from Ch.9 and now summaries the relevant aspects from that chapter.
22409	73	11			Section lacks both an opening statement of prior knowledge and a closing summary assessment presently. [Peter Thorne, Ireland]	Agreed. SROCC/AR5 intro has been added and closing summary paragraph also added.
109699	73	12	73	39	At least some brief mention of the tremendous glacial ice fields that span continguous areas of Alaska, British Columbia, and Yukon is warranted alongside the discussion of Himalayan glaciers, as they are of comparable size. Projected changes in the former will have significant changes in sea level, coastal ocean dynamics, and biogeochemical fluxes to the coastal ocean and terrestrial and ocean ecosystems. For a recent review and synthesis of these geophysical processes and ecological implications, see O'Neel et al., 2015, Icefield-to-ocean linkages across the Northern Pacific coastal temperate rainforest ecosystem, Bioscience, 65, 499-512. [Sean Fleming, United States of America]	Agreed. New text describing these areas has been added.
68209	73	15	73	15	suggest to delete "glaciation" before "models" it is sufficient to write surface mass balance model [Guðfinna Aðalgeirsdóttir, Iceland]	Noted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17273	73	17	73	21	Phrasing needs work. Suggest: According to the Hindu Kush Himalaya Assessment report (Wester et al., 2019), two thirds of glaciers in the Himalayas will experience pronounced volume losses by 2100 if global emissions are not sharply reduced. Even if the goal of limiting global warming to 1.5oC is achieved, glacier melt will still increase by 2100. This has major implications for water resources in a region considered the world's 'Third Pole'", providing streamflow to ten of the world's most important river systems that nearly two billion people rely on. [Joelle Joelle Gergis, Australia]	Noted. There isn't enough space for this amount of text, but the key points have been added.
179	73	17	73	21	See also Immerzeel et al. (2020, Nature), who argue that the Himalaya are highly vulnerable to changes in glacier extent and calculate the number of people that would be affected by water shortages due to declining glacier cover. [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	Agreed, reference added.
29083	73	17		21	Can also refer to Pritchard (2019) Nature doi:10.1038/s41586-019-1240-1 to substantiate some of these statements and suggest removing "According to the Hindu Kush Himalaya Assessment report" which is not needed. A summary statement is needed for this subsection [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. There isn't enough space for this amount of text, but the key points have been added.
64987	73	21	73	21	"accelerate by 2100" – until then or at that date? [Johannes Quaas, Germany]	Noted. Text rewritten.
17275	73	23	73	23	Synthesis study looking a palaeoclimate and future projections from the Andes should be incorporated: Neukom, R., Rohrer, M., Calanca, P., Salzmann, N., Huggel, C., Acuña, D., Christie, D. and Morales, M. (2015). Facing unprecedented drying of the Central Andes? Precipitation variability over the period AD 1000–2100. Environmental Research Letters 10 (8): 084017. [Joelle Joelle Gergis, Australia]	Noted. Other references added for this region. Palaeoglaciological changes are dealt with in Ch. 9
68211	73	23	73	39	note that the GlacierMIP phase2 (see Marzeion et al 2020 https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2019EF001470) provides newer projections for all 19 RGI regions that informes the projections in Ch9, suggest to use that reference also here [Guðfinna Aðalgeirsdóttir, Iceland]	Agreed, reference added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
109701	73	23	73	39	This is a great section, but it requires one important correction. From past experience, we know that after publication, this kind of IPCC material will be taken by readers and applied to all sorts of environmental science and management issues, so it needs to be clear on something: the kinds of studies presently summarized in this section are for the most part very large-scale studies making significant simplifications of watershed hydrology, and results for a given glacier-fed basin can only be determined using detailed, site-specific coupled models of climate, glaciers, and river hydrology. This is **not** a subtle point: some of the simplifications made by the kind of large-scale glaciology studies currently cited in this section have led to demonstrably wrong conclusions. For example, the otherwise excellent and ground-breaking study by Clarke et al (2015) concluded that peak water has not yet been reached in the large, transboundary (Canada-US) Columbia River Basin, whereas the water resource science and engineering community knows - from both rigorous statistical analysis of long-term historical observations, and rigorous watershed-specific joint climate change-glaciological-water resource science modeling studies - that peak water here occured long ago and the glacier-fed component of river flow is declining. It is therefore important to provide readers with a brief caveat that local-scale impacts can be considerably more complex, and to provide a few references to studies which have developed specific modeling methodologies to quantify those effects. Returning to the Columbia River example, (1) historical reductions in glacial river flows were demonstrated by Fleming and Weber, 2012, Detection of long-term change in hydroelectric reservoir inflows: bridging theory and practise, Journal of Hydrology, 470, 36-54; and (2) rigorous projections of future conditions were provided by Jost et al., 2012, Quantifying the contribution of glacier runoff to streamflow in the upper Columbia River Basin, Canada, Hydrology and Earth System Sciences, 16, 849-860. [Sean Fleming, United States of America]	Noted. This is a very detailed point and we have attempted to capture the essence of the suggestion in the new text, but limited space prevents a full discussion of catchment-scale differences in glacier hydrology.
183	73	23	73	39	Immerzeel et al. (2020) highlight how glaciers, snowcover and lakes in global mountain regions provide water in each downstream catchment, regulating water supply, and quantify how this will be affected by climate change. They also create a "supply index" and a "demand index" to show which mountain catchments are most important for downstream populations, and which are most threatened by climate change, population and GDP change and political tension. [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Reference has been added but there is insufficient space for this level of socio-economic detail.
3043	73	23	73	39	It seems opportune to include a sentence pointing at the significant uncertainties that the poorly-constrained present-day glacier ice volume has on such long-term projections of freshwater availability. Farinotti et al. (2019), for example, showed that by the end of the century, the decrease in water contribution in summer from all ca. 96,000 glaciers in High Mountain Asia can vary between -15% and -24% depending on the glacier ice thickness distribution assumed at present. Reference: Farinotti et al. (2019). A consensus estimate for the ice thickness distribution of all glaciers on Earth. Nature Geoscience. https://doi.org/10.1038/s41561-019-0300-3 [Daniel Farinotti, Switzerland]	Noted. This is a useful point but too complex to be conveyed within space limitations. Furthermore, the emphasis of this section is more on the timing of glacier mass loss, rather than volume specifically, so these uncertainties may be less of an issue.
3985	73	26	73	26	was projected [Sabine Baumann, Germany]	Agreed. Text modified
74393	73	26	73	26	To separate was from projected [Moulay Driss HASNAOUI, Morocco]	Agreed. Text modified
181	73	26	73	26	typo - " basins was projected" [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	Agreed. Text modified

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12911	73	26			Missing space in "wasprojected" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Agreed. Text modified
43233	73	26			Read "basins was projected to decrease by 43 " rather than "basins wasprojected to decrease by 43" [Cyriaque Rufin Nguimalet, Central African Republic]	Agreed. Text modified
4973	73	27	73	27	What is driving an increase in glacier extent in order to arrive at a maximum in the early 20th century? This peak is not really clear to me [Bart van den Hurk, Netherlands]	Noted. Text rewritten.
2855	73	32	73	32	The sentence needs to be rephrased because Radic et al. paper is older. It should be: "Such results support previous findings by Radic et al. (2014) who…" [Antoine RABATEL, France]	Noted. Text rewritten.
349	73	32	73	32	Radic et al. (2014) should be updated with Hock et al. (2019) regarding projection of glacier mass loss and the statement adjusted to the 2019 article content. Hock, R., Bliss, A., Marzeion, B., Giesen, R. H., Hirabayashi, Y., Huss, M., Radic, V. and Slangen, A. B. A.: GlacierMIP - A model intercomparison of global-scale glacier mass-balance models and projections, J. Glaciol., 65(251), 453–467, doi:10.1017/jog.2019.22, 2019. [Etienne Berthier, France]	Noted. Both citations are included.
3987	73	32	73	32	Radic et al., (2014) [Sabine Baumann, Germany]	Noted.
12913	73	32			Reference has not been included properly in the sentence. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
43235	73	32			Read "Such results are supported by Radić et al. (2014) who found " rather than "Such results are supported by (Radić et al., 2014) who found " [Cyriaque Rufin Nguimalet, Central African Republic]	Noted.
351	73	34	73	34	The first author is “Clarke” not “Clark” [Etienne Berthier, France]	Yes, it should be Clarke, not Clark. This is corrected in the FGD.
59123	73	34	73	34	Clark et al., 2015 is missing in the reference list [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Yes, the reference was missing in the SOD and is now being included in the FGD. Clarke et al., 2015, Projected deglaciation of western Canada in the twenty-first century, Nature Geosciences, DOI: 10.1038/NGEO2407
54535	73	37	73	39	I will encourage the authors to look at the recent IMBIE publication (https://doi.org/10.1038/s41586-019-1855-2) and refer to this rather than the Nick et al, 2013 [Kristian Kjelden, Denmark]	Agreed, reference added.
59127	73	37	73	39	In these two sentneces losses of the Greenland Ice Sheet are shortly mentioned. Why are in this context in this chapter no information given on other ice sheets (e.g. Antarctica) or at least links to the relevant chapters? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Agreed. Both ice sheets are now discussed.
54533	73	38	73	38	Delete the abbreviation GIS - it is only used this one time [Kristian Kjelden, Denmark]	Noted. Text rewritten.
353	73	38	73	38	This very brief statement about the Greenland ice sheet quoting an old reference seems out of context here. [Etienne Berthier, France]	Agreed. Both ice sheets are now discussed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22415	73	42	73	54	Is it appropriate to have a section which essentially entirely points elsewhere for the assessment? It is fine and right for the section to point to the chapter 9 assessment but should the broader hydrological cycle impacts then be assessed more thoroughly here? [Peter Thorne, Ireland]	Taken into account. As noted by the reviewer, the intent here is to briefly summarize the extensive consideration in chapter 9. For the sake of simplicity, the broader hydrological impacts are discussed in the sections corresponding to the impacted variable. This is now noted explicitly.
29085	73	42			This is very short - could it be combined with glaciers into a cryosphere section. An example of new research could be given e.g. Sun et al. (2019) GRL 10.1029/2018GL080362: Projections for the Sierra Nevada show a 30+-12% mean reduction in snowpack and 30 days earlier spring melt under the RCP4.5 scenario. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The intent here is just to briefly summarize the extensive consideration in chapter 9. Merging with the glaciers section would be complicated at this stage in the process.
22411	73	43	73	43	Sentence makes no sense as written. Suspect should remove 'that NH'? [Peter Thorne, Ireland]	Accepted. The sentence has been edited to move "NH" later in the sentence where it is more clear.
17277	73	43	73	54	This section is far too shallow to be a useful assessment. Please have another go at summarising specific key findings from chapter 9 here. [Joelle Joelle Gergis, Australia]	Rejected. The intent here is just to briefly summarize the extensive consideration in chapter 9 and direct the reader to the relevant section for more information. The two assessments from the summary paragraph of section 9.5.3.3 are included here.
12915	73	43			Something is wrong with the grammar here. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence has been edited.
12917	73	48			What is the purpose of this sentence, "As discussed further in Chapter 9." Does it refer to the previous sentences or the following sentences? This section does not look like it has been proof read. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence has been edited (a period should have been a comma) to clarify that the reference to chapter 9 applies to the rest of the paragraph.
16137	73	50	73	51	Ch 9 assessment is indeed that virtually certain that future Northern Hemisphere snow cover extent and duration will continue to decrease, but note that this is contingent on continuing global warming (SOD Ch 9, p.82, l.3) (one may of course argue that there is little doubt that this condition will be verified). [Gerhard Krinner, France]	Accepted. We have added the phrase "as global climate continues to warm" to signal this.
23637	73	51	73	54	Regarding the Southern Hemisphere, Bozkurt et al. (2018) showed that even under the low emission scenario, the Andes (central-southern Chile) snowpack is projected to decrease by 35–45% by mid-century. In more snowmelt-dominated areas, the projected hydrological changes under RCP8.5 go together with more loss in the snowpack (75–85%) and a temporal shift in the center timing of runoff to earlier dates (up to 5 weeks by the end of the century). Furthermore, Bozkurt et al. (2018) highlighted that the role of evapotranspiration change is more important towards the end of the year in the annual cycle climatology and mainly depends on water and soil moisture availability. Bozkurt, D., Rojas, M., Boisier, J.P., Valdivieso, J., 2018. Projected hydroclimate changes over Andean basins in central Chile from downscaled CMIP5 models under the low and high emission scenarios. Climatic Change, 150, 131-147, doi:10.1007/s10584-018-2246-7. [Deniz Bozkurt, Chile]	Noted. However, for brevity and consistency, the intent of this section is just to briefly summarize the detailed consideration in chapter 9, rather than to elaborate on it.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22413	73	53	73	54	Is it appropriate to make this as a fact based statement or should it be virtually certain as the prior finding it is conditioned upon is. [Peter Thorne, Ireland]	Accepted. We have deleted "the" at the beginning of the statements, so it becomes a general factual statement about any snow changes, whatever they may be.
74395	73	37 and 38	73	37 and 38	(GIS) in Greenland Ice Sheet (GIS) may be confused with the Commonly known Geographical Information System (GIS) usually used by many scientists. [Moulay Driss HASNAOUI, Morocco]	Noted. Text rewritten.
29089	74	1			additional reference to consider: Wang et al. (2018) Nature Geosci https://doi.org/10.1038/s41561-018-0114-8.: Evaporation from lakes is projected to increase due to reduced ice cover and reduced longwave radiation loss due to slower surface warming than surrounding land. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference added.
12919	74	2			Sentences should follow each other without new lines being started. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
74397	74	3	74	3	The SRCCL is in double [Moulay Driss HASNAOUI, Morocco]	Accepted.
29087	74	3			The sentence "The SRCCL SRCCL and SROCC provided some discussion of wetlands projections and uncertainties." is redundant and can be removed since this is discussed next. Some typos can also be corrected. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The sentence is providing specific information about what is covered in SRCCL and SROCC (just wetlands). Some typos have been corrected. Additionally, there will be a copyedit completed prior to publication.
43237	74	3			Read "The SRCCL and SROCC provided some discussion of wetlands projections " rather than "The SRCCL SRCCL and SROCC provided some discussion of wetlands projections " [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted.
43239	74	6			Read " projecting substantial decreases (Spencer et al., 2016) and " rather than " projecting substantial decreases ((Spencer et al., 2016) and " [Cyriaque Rufin Nguimalet, Central African Republic]	Editorial – copyedit to be completed prior to publication
74399	74	7	74	7	missed (in Schurch et al., 2018) to make it like (Schurch et al., 2018). [Moulay Driss HASNAOUI, Morocco]	Editorial – copyedit to be completed prior to publication
43241	74	7			Read "possible increases (Schuerch et al., 2018). The SROCC discussed " rather than "possible increases (Schuerch et al., 2018) The SROCC discussed " [Cyriaque Rufin Nguimalet, Central African Republic]	Editorial – copyedit to be completed prior to publication
12921	74	13			The mid to high latitudes covers such a range of environments/biomes as to be a little meaningless. Prairie would fall within mid-to-high-latitudes, for example. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The description of the latitudinal range, while broad, is taken directly from the referenced paper.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
113425	74	17	74	19	In multiple instances in the report, there is a space lacking after or before brackets.Here are two examples. I won't note here all of them. Also, sometimes references are used in brackets when should be outside brackets. I hope this can be check through in the final version. [Diego Miralles, Belgium]	Editorial – copyedit to be completed prior to publication
43243	74	17			Read " inland wetlands (Junk et al., 2013; Moomaw et al., 2018). ' rather than " inland wetlands(Junk et al., 2013; Moomaw et al., 2018). " [Cyriaque Rufin Nguimalet, Central African Republic]	Editorial – copyedit to be completed prior to publication
74401	74	19	74	19	to separate (Sharma et al., 2019) from and [Moulay Driss HASNAOUI, Morocco]	Editorial – copyedit to be completed prior to publication
29179	74	19	74	19	Add a sentence: To assess the impact of wetland change in the methane emissin, Zhang et al (2017) estimated the evolution of wetland extent under climate change and cncluded on an increase of the boreal wetlands, due to thawing during the cold season. Corresponding reference: Zhang, Z., Zimmermann, N. E., Stenke, A., Li, X., Hodson, E. L., Zhu, G., ... & Poulter, B. (2017). Emerging role of wetland methane emissions in driving 21st century climate change. Proceedings of the National Academy of Sciences, 114(36), 9647-9652. [Catherine Prigent, France]	Rejected. While the paper is interesting, its focus - on the impacts of wetlands change on methane emissions - is outside the scope of this section, which focuses on the projected changes in the wetlands, themselves.
12923	74	19			Missing space in "2019)and" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Editorial – copyedit to be completed prior to publication
43245	74	19			Read "and ice (Sharma et al., 2019) and, for many lakes" rather than "and ice (Sharma et al., 2019)and, for many lakes" [Cyriaque Rufin Nguimalet, Central African Republic]	Editorial – copyedit to be completed prior to publication
24141	74	20	74	21	This is a strange statement. Examples of projections from modeling of glacier-bed overdeepenings are, for instance: Linsbauer et al. (2016) for the Himalayas, Colonia et al. (2017) for the Peruvian Andes, or Magnin et al. (2020)for the Mont Blanc region. References: (1) Linsbauer, A., Frey, H., Haeberli, W., Machguth, H., Azam, M.F., Allen, S., 2016. Modelling glacier-bed overdeepenings and possible future lakes for the glaciers in the Himalaya–Karakoram region. Annals of Glaciology 57(71). doi:10.3189/2016AoG71A627. (2) Colonia, D., Torres, J., Haeberli, W., Schauwecker, S., Braendle, E., Giraldez, C., Cochachin, A., 2017. Compiling an inventory of glacier-bed overdeepenings and potential new lakes in de-glaciating areas of the peruvian andes: approach, first results, and perspectives for adaptation to climate change. Water 9, 336, http:// dx.doi.org/10.3390/w9050336. (3) Magnin, F., Haeberli, W., Linsbauer, A., Deline, P., Ravanel, L., 2020. Estimating glacier-bed overdeepenings as possible sites of future lakes in the de-glaciating Mont Blanc massif (Western European Alps). Geomorphology 350, 106913. doi.org/10.1016/j.geomorph.2019.106913 [Wilfried Haeberli, Switzerland]	Accepted. The references have been added. However, given the caveats in the studies, they are framed as evidence in support of increases rather than confident projections.
22417	74	23	74	27	Is there enough meat on the bones here to support these statements? In particular statements about lake temperature and stability seem to be reliant upon a single study which would all else being equal imply low confidence. What is it that traceably can support such high confidence statements here? Are there more studies that could be cited to improve the underlying assessment here and provide better support to these summary findings? [Peter Thorne, Ireland]	Partially accepted. There are both multiple studies and physical expectations for lake temperature and ice changes, so we have retained "high confidence" but provided more explanation. We have changed the confidence assessment to "low" for mixing, as there is only a single study to date and a less direct physical basis.
74403	74	24	74	24	to join in and to like … saltwater intusion into … [Moulay Driss HASNAOUI, Morocco]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19213	74	30	74	30	Can the authors add a figure for groundwater distribution across the world? [Mohamed Deyab, Egypt]	Rejected. We are short on space and do not have a preferred figure on groundwater projections. A background figure on the global distribution of groundwater amounts, while informative, would be out of place in this section, which focuses only on projections.
113427	74	30	74	30	I certainly prefer 'groundwater' but note that earlier on 'groundwaterS' was used. [Diego Miralles, Belgium]	Noted. Groundwater, as used here, is the preferred term.
4975	74	31	74	39	This section is quite unclear. Sentence structure makes it difficult to read, but also I don't know what "focused recharge" (line 35) is, and also in what direction this "disproportional dependence on heavy rainfall" is pointing. Too little or too much recharge under heavy precip conditions? [Bart van den Hurk, Netherlands]	Accepted. The section has been revised and shortened for clarity. Focused recharge (recharge through geographically localized mechanisms such as perennial streams) is no longer specifically mentioned.
129217	74	33			"… projected to reduce renewable groundwater resources significantly ..." As 'projected' is already used in the sentence, it might read better as "..., resulting in significantly reduced renewable groundwater resources in most ..." [Trigg Talley, United States of America]	Accepted. The sentence has been revised.
51957	74	37	74	39	Replace text with this: "… such as ENSO (Cuthbert et al., 2019b; Kolusu et al., 2019), the future trends of which remain uncertain (Brown et al., 2020)." [Richard Taylor, United Kingdom (of Great Britain and Northern Ireland)]	No longer applicable. This sentence is no longer included.
74407	74	45	74	45	to put capital after ; like …; These are … [Moulay Driss HASNAOUI, Morocco]	Editorial – copyedit to be completed prior to publication.
129219	74	54			The amount of groundwater recharge from irrigation is also (and perhaps most importantly) dependent upon irrigation method (think drip versus flooding). [Trigg Talley, United States of America]	No longer applicable. This discussion is no longer included.
74405	74	32 and 33	74	32 and 33	projected in double [Moulay Driss HASNAOUI, Morocco]	Accepted. The sentence has been revised.
74409	74	52 to 55	74	52 to 55	for example for drip irrigation is not the case because there is no reharge at all. [Moulay Driss HASNAOUI, Morocco]	No longer applicable. This discussion is no longer included.
113429	75	1	75	6	Three examples of citations that should be without parenthesis. [Diego Miralles, Belgium]	Editorial – copyedit to be completed prior to publication.
12925	75	1	75	9	Reference not properly written in to the sentence. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Editorial – copyedit to be completed prior to publication.
12927	75	1	75	9	What is the overall assessment of these findings? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. A summary statement is now provided.
43247	75	1			Read "Zaveri et al. (2016) reported an increase in irrigated " rather than "(Zaveri et al., 2016) reported an increase in irrigated " [Cyriaque Rufin Nguimalet, Central African Republic]	Editorial – copyedit to be completed prior to publication.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
74411	75	5	75	5	when starting the sentence by any reference usually you don't put "(" at the begining like ". Crosbie et al., (2013) …" in order to keep the same format [Moulay Driss HASNAOUI, Morocco]	Editorial – copyedit to be completed prior to publication.
43249	75	5			Read " Crosbie et al. (2013) reported a " rather than " (Crosbie et al., 2013) reported a " [Cyriaque Rufin Nguimalet, Central African Republic]	Editorial – copyedit to be completed prior to publication.
17279	75	6	75	6	Which part of Australia? Please specify. Also note there are referencing formatting issues in this paragraph. [Joelle Joelle Gergis, Australia]	No longer applicable. This sentence is no longer included.
74413	75	6	75	6	when starting the sentence by any reference usually you don't put "(" at the begining like ". Wada and Bierkens, (2014), …" in order to keep the same format [Moulay Driss HASNAOUI, Morocco]	Editorial – copyedit to be completed prior to publication.
43251	75	6		7	Read " Wada and Bierkens (2014) estimated abstraction" rather than " (Wada and Bierkens, 2014) estimated abstraction" [Cyriaque Rufin Nguimalet, Central African Republic]	Editorial – copyedit to be completed prior to publication.
22419	75	9	75	9	This section lacks a closing assessment summary. [Peter Thorne, Ireland]	Accepted. A summary statement is now provided.
113431	75	12	75	12	Please clarify the type of drought it is referred to in each statement it is used. [Diego Miralles, Belgium]	Noted. Here we focus on large-scale projections without dividing them into types of drought. Detailed assessment of types of drought may be found in Chapter 11.
59137	75	12	76	27	This section is well-written. However, it is compared to the other chapters quite long. I would suggest to shorten or even move passages, which should be more prominent in other sections even they are of course highly relevant for aridity and droughts. This is for example the case for p.75 l. 36-45, which refers more to sections considering precipitation and seasonal snow cover. Moreover, this is the case for p.75, l.55 to p.76, l.13, which refers more to the section on soil moisture. Regarding the last paragraph, p.76 l.14-27, this is an interesting and important aspect and would even be a own subchapter (e.g. plant water), but is maybe also not at the right place under this section. Another idea would be to add the latter aspect in the title of this section. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. In the revised draft we have combined soil moisture and drought together to create a more compact assessment. We have removed references to snow as this is assessed elsewhere. We have shortened the reference to plant physiology as this is discussed in more detail in several other locations in the chapter.
33069	75	12	77	2	the role of water also in arid and semi- arid areas needs to be highlighted. [Sahar Tajbakhsh Mosalman, Iran]	Accepted. We have combined soil moisture and drought together for a more compact assessment and removed references to snow. We have coordinated with Ch. 11 to minimize overlap and ensure consistency. A summary has been added.
32739	75	12	77	2	the role of water also in arid and semi- arid areas needs to be highlighted. [sadegh zeyaeyan, Iran]	Noted. Many of these references can be found in the evapotranspiration assessment as well as elsewhere in the chapter. A summary has been added.
19445	75	12	77	2	The role of water also in arid and semi-arid areas needs to be highlighted. [Mostafa Jafari, Iran]	Noted. Semi-arid areas are featured prominently in this section.
113439	75	12	77	5	This section feels like a repetition of what has already discussed in the runoff, precipitation, soil moisture and snow sections. In addition it overlaps with Chapter 11. I would suggest to condese to avoid that overlap, and to rephrase to make sure that every statement deals explicitely with drought or aridity. Do a summary by the end. [Diego Miralles, Belgium]	Accepted, summary added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29091	75	12			nice subsection; additional references to possibly consider: Lickley & Solomon (2018) ERL doi:10.1088/1748-9326/aae013: based on CMIP5 simulations, 3 billion people are expected to experience 25% increases in aridity under a high emissions scenario by the end of the century. Bonfils et al. (2017) J. Clim doi:10.1175/JCLI-D-17-0005.1: aridity increases in ~70% of regions where aridity sensitive to ENSO, but only 40% when aridity indicator for soil moisture used due to physiological effects for enhanced CO2 Scheff and Frierson (2015) J. Clim doi:10.1175/JCLI-D-14-00480.1.: increased subtropical aridity from increased PET due to increased net radiation but large discrepancies between models Greve and Seneviratne (2015) GRL 10.1002/2015GL064127 find significant increases in aridity (approximated from P minus a PET proxy) affect only 16% of all land areas in end of century RCP8.5 projections. A summary statement would be useful. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Semi-arid areas are featured prominently in this section.
69515	75	14	76	27	I am a bit confused as to how the evdience outlined in this section fits in with Yang et al. (2018). They seemed to show a pretty robust increase in effective evaporative resistance across CMIP5 models, which would imply, in these models at least, the WUE effect dominates over the LAI effect. Perhaps I have misunderstood this study, but I think it would be useful to put this in context within this section. Yang, Y., Roderick, M.L., Zhang, S. et al. Hydrologic implications of vegetation response to elevated CO2 in climate projections. Nature Clim Change 9, 44–48 (2019). https://doi.org/10.1038/s41558-018-0361-0 [Martin Singh, Australia]	Noted. Semi-arid areas are featured prominently in this section.
74415	75	22	75	22	we need to add "in" after increases like "… increases in these areas" [Moulay Driss HASNAOUI, Morocco]	Accepted, fixed.
12929	75	22			Inconsistent section referencing style compared to the rest of the chapter. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We have shortened the discussion of plant physiology here because it is covered in the assessment of evapotranspiration, 8.4.1.4. This study is mentioned there.
43253	75	22			Read "precipitation increases in these areas. " rather than "precipitation increases these areas. " [Cyriaque Rufin Nguimalet, Central African Republic]	Noted, this sentence has been deleted.
7063	75	24	75	26	It would also be relevant to cite Wang et al. (2020) BAMS Monsoon Climate Change Assessment here, with respect to CMIP6 results for the monsoons. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, major differences from CMIP5 are now described.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89101	75	24		34	Cook et al (2020) is out now. Another paper documenting CMIP6 drought projections that might have some different interpretations is Ukkola et al (2020). Ukkola, A. M., Kauwe, M. G. D., Roderick, M. L., Abramowitz, G., & Pitman, A. J. (n.d.). Robust future changes in meteorological drought in CMIP6 projections despite uncertainty in precipitation. Geophysical Research Letters, n/a(n/a), e2020GL087820. https://doi.org/10.1029/2020GL087820 Cook, B. I., Mankin, J. S., Marvel, K., Williams, A. P., Smerdon, J. E., & Anchukaitis, K. J. (2020). Twenty-First Century Drought Projections in the CMIP6 Forcing Scenarios. Earth's Future, 8(6), e2019EF001461. https://doi.org/10.1029/2019EF001461 [Angeline Pendergrass, United States of America]	Noted, this sentence has been deleted.
12931	75	24			Suggest changing "congruent" to "consistent" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, both of these studies are assessed in this section.
53393	75	24			although there may be a few exceptions like Amazonia (Parsons 2020)? [Hervé Douville, France]	Rejected. Congruent is the preferred descriptor here.
113433	75	27	75	27	drying' for 'decline' [Diego Miralles, Belgium]	Noted. monsoons are assessed in a separate section. We have removed mention of monsoons here.
129221	75	27	75	29	Feng and Fu (2013) should be cited here: Feng, S., and Q. Fu, 2013: Expansion of global drylands under a warming climate. Atmos. Chem. Phys., 13, 10081-10094, doi: 10.5194/acp-13-10081-2013. [Trigg Talley, United States of America]	Noted, this sentence has been modified.
22421	75	31	75	50	What is the basis that supports these likelihood statements? No papers or other evidence is sprcifically cited to support these findings. Are they solely expert judgement? That would seem dangerous. You need to be substantially clearer what the basis that supports such findings is. [Peter Thorne, Ireland]	Noted. This is an analysis of CMIP5 models so it is referenced in our review of CMIP5 results at the beginning of the section.
98083	75	32	75	34	I would characterize the regions of very likely increases in precipitation differently than is done here, based on the regional detection/attribution/consistency analysis of historical precipitation trends vs. CMIP5 models (1901-2010) as shown in Knutson and Zeng (2018, Fig. 3 and related seasonal figures). Then I would drop the "major monsoon regions (e.g., India, Southeast Asia) from the list (little evidence for detectable anthropogenic influence since 1901 or 1951), while you could add the north-central and northeastern U.S., southern Canada, southeast South America, and parts of Scandinavia and northern Eurasia and Iceland, and northern Australia Those are regions where (in addition to the high northern latitudes) there are already detectable positive century-scale precipitation trends that are consistent (at least in sign) with expected trends over the same period from the CMIP5 model ensemble. That is some basis of high confidence in future projected increases (and a reason for more moderate confidence levels for the monsoon region projected increases). Refs: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637 https://doi.org/10.1175/JCLI-D-17-0672.1. [Thomas Knutson, United States of America]	Accepted, this sentence has been deleted
53395	75	32			relative increases? [Hervé Douville, France]	Accepted, this sentence has been deleted
113435	75	34	75	34	correct 'Asia.)' [Diego Miralles, Belgium]	Accepted, this sentence has been deleted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7065	75	34			Remove fullstop after "South Asia" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, this sentence has been deleted
98085	75	36	75	38	Regions with projected precipitation declines over land can be relatively localized features, as noted, and it makes sense to list here those various areas where there are projected to be decreases. However, I would only categorize as “very likely decreases” those regions where there is already demonstrated to be detectable anthropogenic decrease. That is, the very likely decrease regions are those that already have observed decreasing trends that are highly unusual compared to simulated natural variability, and where climate model historical runs show a decreasing sign of change in response to anthropogenic forcing). The existing list on lines 36-38 does not accomplish this. It has missing regions, and in some cases, erroneously included regions (like the central U.S and Mexico and the Pacific Northwest) where there is not yet a clear detectable anthropogenic decreasing trend. In fact the central U.S. has a large detectable increasing trend in precipitation, not decreasing trend. Mexico and the Pacific Northwest are interesting cases because they do show decreases in both observations and models for the recent period 1981-2010. However those negative trends are not robust as they do not hold up over longer time periods (1951-2010 or 1901-2010), except for southwest Canada (1951-2010); and they are not assessed as detectable for 1981-2010 except for a small subset of grids in the southwest US (seasonal trends). So 1981-2010 observed trends appear dominated by internal variability, and it thus makes more sense to examine longer term trends over land, like 1901-2010 where a greater fraction of trends are detectable. Based on Knutson and Zeng’s (2018, Fig. 3) analysis for 1901-2010 regional precipitation trends over land, the following list can be used for detectable annual-mean decreases with at least some anthropogenic contribution: Large regions: the Mediterranean region and parts of northern tropical Africa including the Sudan. Smaller land regions across the southern subtropics: a small region along the west coast of southern Africa, southwest Australia, Tasmania, and parts of central Chile. There are some other scattered small regions which are not so well organized spatially but should be reported as well so planners in those regions are at least made aware of documented detectable decreases. These include Sri Lanka, Bahamas, Falklands, and some small regions within Indonesia, Phillippines, Japan, the Caribbean, northern India, and eastern China. For 1951-2010 trends, we can add to the above list parts of Madagascar, Brazil, southeast Australia, New Zealand, and	Accepted, this sentence has been deleted
17281	75	36	75	38	Amend to read 'southern Australia'. Also suggest droppping Euro-centric phrase of 'Mediterranean climate' and just list specific regions in their own right [Joelle Joelle Gergis, Australia]	Noted. This section has been revised and regions where robust declines in soil moisture are discussed. Observational changes are assessed in Section 8.3
53397	75	36			also over Amazonia where a majority of CMIP6 models agree that average precipitation will decline over the entire basin thereby leading to a dramatic increase in hot drought risk across this region. [Hervé Douville, France]	Accepted, changed
113437	75	37	75	37	remove quotations from ‘Mediterranean-climate’ [Diego Miralles, Belgium]	Accepted, changed
16139	75	39	75	45	Not clear what the literature or other evidence is that underlies these assessments - several "virtually certain" are in this paragraph without any cited literature (or is that all from Cook et al?). The preceding and to the following paragraph partly give a similar impression, at least at first look. [Gerhard Krinner, France]	Accepted, changed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8665	75	45	75	47	This is a great point, which is observed in S Europe. Although some influence of the atmospheric evaporative demand is observed, mostly in summer season (Vicente-Serrano, S.M., Juan-I. Lopez–Moreno, Santiago Beguería, Jorge Lorenzo–Lacruz, Arturo Sanchez–Lorenzo, José M. García–Ruiz, Cesar Azorin–Molina, Jesús Revuelto, Ricardo Trigo, Fatima Coelho, Francisco Espejo. (2014) Evidence of increasing drought severity caused by temperature rise in southern Europe. Environmental Research Letters. 9, 044001. doi:10.1088/1748-9326/9/4/044001). The sensitivity of streamflow to the atmospheric evaporative demand is small in comparison to the sensitivity of streamflow to precipitation (Vicente-Serrano, S.M., Marina Peña-Gallardo, Jamie Hannaford , Conor Murphy , Jorge Lorenzo-Lacruz , Fernando Dominguez-Castro , Juan López-Moreno , Santiago Begueria , Ivan Noguera , Shaun Harrigan , Jean-Philippe Vidal. (2019) Climate, irrigation and land-cover change explain streamflow trends in Western Europe. Geophysical Research Letters, 46, 10,821–10,833.) [Sergio Vicente-Serrano, Spain]	Noted, these statements on snow have been removed as they are assessed elsewhere.
53399	75	47	75	53	May be add that: "The global average chance of an agricultural drought was found to increase from 9% in 1981-2010 to 24% at +1.5°C and 61% at +4°C above preindutrial level (Arnell et al., 2019, although there is considerable uncertainty around these central estimates and impacts and risks vary substantially across regions." [Hervé Douville, France]	Noted, this section has been revised.
112233	75	48			And recharge (through irrigation) [Rutger Hofste, Netherlands]	Noted, projections in specific drought types may be found in Ch. 11
74417	75	50	75	50	to suppress "," before "and" like "and …." [Moulay Driss HASNAOUI, Morocco]	Noted, this sentence has been deleted.
116759	75		75		missing summary statements for some sections. [Valerie Masson-Delmotte, France]	Accepted. A summary statement is now provided.
129223	76	2			Fu et al. (2016) should be cited here: Fu, Q., L. Lin, J. Huang, S. Feng, and A. Gettelman, 2016: Changes in terrestrial aridity for the period 850-2080 from the Community Earth System Model. J. Geophys. Res. Atmos., 121, doi:10.1002/2015JD024075. [Trigg Talley, United States of America]	Noted, we now have a cross-chapter box on the carbon-water nexus.
129225	76	3			Figure 8.21 does not show soil moisture trajectories; it instead shows PDSI, which is arguably an inappropriate substitute. [Trigg Talley, United States of America]	Rejected. Here we are referring to regional changes in PDSI reconstructed from the CESM LME as done in Cook 2014. Fu 2016 is a different aridity index and doesn't compare to tree rings.
29093	76	7			See also Kumar et al. (2019a) J.Clim regarding carry over moisture [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted, we have clarified that the figure shows PDSI. Paleoclimate reconstructions of drought are calculated in PDSI units, which is physically related to soil moisture.
59133	76	10	76	10	Do you mean Cook et al., 2015a or 2015b? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, reference added
53401	76	14	76	15	as well as other drivers and processes such as the VPD evolution and its influence on both surface evaporation and vegetation transpiration (Peng et al. 2018)? [Hervé Douville, France]	Noted, this sentence has been deleted.
53403	76	17	76	18	rather the opposite? Also quote Yang et al. (2018) in NCC? [Hervé Douville, France]	Noted, this sentence has been deleted.
70351	76	18	76	18	The phrase “enhance evaporative drying” is unclear. Does this mean more evaporation and a drier land surface? Or less evaporation (wetter soils) and a drier near surface air mass? We suggest “leave more water in the soil” or “lead to less evapotranspiration”. [Abigail Swann, United States of America]	Fixed, but this sentence has been removed for brevity

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8679	76	18	76	18	I think it should be the opposite since higher WUE would reduce (not enhance) evaporative drying. In any case, the role of CO2 on WUE shows uncertainties and different role as a function of the drought type, the drought metric and the soil moisture conditions (see also Ch 11 assessment and also more details in Vicente-Serrano, S.M., McVicar, T., Miralles, D., Yang, Y., Tomas-Burguera, M. (2020): WIREs Climate Change 11: e632 and references therein). See also Vicente-Serrano, S.M. et al. Global characterization of hydrological and meteorological droughts under future climate change: The importance of time-scales, vegetation-CO2 feedbacks and changes to distribution functions. Int. J. Climatol. https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/joc.6350 and Brodribb et al., Science 368, 261–266 (2020). [Sergio Vicente-Serrano, Spain]	Noted, this sentence has been deleted.
10167	76	18			Was "enhance" here supposed to be "diminish" ? Higher WUE alone = less evaporation & less drying of the soil, because of stomatal closure. Or am I not understanding the intended purpose of the sentence. [Jacob Scheff, United States of America]	Noted, yes these have the opposite effect. Yang 2018 is discussed in 8.4.1.4
129227	76	18			"enhance" to "decrease"? [Trigg Talley, United States of America]	Fixed, but this sentence has been removed for brevity
10169	76	19			Lemordant et al. (2018) could also be cited for this, in addition to Swann et al. (2016). [Jacob Scheff, United States of America]	Fixed, but this sentence has been removed for brevity
12933	76	21			Change "rasises" to "raises" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted, these studies are discussed in 8.4.1.4
43255	76	21			Read "raises the total evaporative surface " rather than "rasises the total evaporative surface " [Cyriaque Rufin Nguimalet, Central African Republic]	Noted, this sentence has been deleted.
70353	76	24	76	24	Some individual models may show this, but most models project that increases in LAI, despite being large, do not dominate the response on larger spatial scales for CMIP5 and CMIP6 projections (Swann et al. 2016, Zarakas et al. in review, preprint doi:10.31223/osf.io/emgxb). In most CMIP6 models, LAI increases in the tropics are not large enough to counteract CO2-driven reductions in stomatal conductance, leading to a net decrease in tropical land evapotranspiration (Zarakas et al. in review; Figure 6c). Additionally, Kooperman et al. (2018) show that in CESM1-BGC, plant responses to CO2 can drive regional increases in soil moisture and streamflow, especially extreme runoff intensity. [Abigail Swann, United States of America]	Noted, this sentence has been deleted.
59135	76	24	76	24	Do you mean Ukkolaet al., 2016a or 2016b? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted, these studies are discussed in 8.4.1.4
7619	76	24	76	25	The discussion between the impact of plant water use efficiency (WUE) and leaf area index (LAI) is also discussed in Bonfils et al. (2017) shows opposite results: In CMIP5 AMIP-type simulations of future warming, the inclusion of the physiological response to enhanced atmospheric CO2 levels decreases ET per leaf unit due to the stomatal closure, but also yields to more carbon uptake and more leaves that can intercept and transpire water. In this study, one of the main conclusion is that the net effect is a reduction of ET at the canopy level, whcih prevents the dessication of the deeper soil. [Celine Bonfils, United States of America]	Noted, this sentence has been deleted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53405	76	26	76	27	May be add: "In the meantime, empirical observational constraints can be used to refine the projections but suggest contrasted results in terms of projected drying or changes in extreme water availabilty (e.g., Douville and Plazzotta, 2017; Padron et al., 2019)." [Hervé Douville, France]	Noted, this sentence has been deleted.
87673	76	32	76	36	In Figure 8.20, distributions of VPD (a) and WUD (b) are not displayed, while (c) and (d) are shown correctly. Please check the figure. [Kenji Tanaka, Japan]	Taken into account in the FGD (Figure 8.19).
12935	76	34			Reference not properly written in to the sentence. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account in the FGD.
12937	76	43			Here and in several other captions, change "At left", "At right" etc. to "On the left...", "On the right..." or better check on IPCC style, which probably requires proper referencing of figure sub-panels. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Panels are referenced by letter.
12939	76	53			Why not include CMIP5 in addition to CMIP6 for comparison where space allows? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. CMIP6 LME results were not yet available by the IPCC cut-off deadline.
116761	76		76		integration with ch 5 needed here (CO2 effect on plant physiology; aridity and fire). [Valerie Masson-Delmotte, France]	Noted, this sentence has been modified.
113441	77	2	77	2	Merge this sentence with the text in the section, rather than leaving it as a single-line paragraph. [Diego Miralles, Belgium]	Accepted, summary added
22423	77	2	77	2	This section has no summary statement [Peter Thorne, Ireland]	Accepted, this sentence has been incorporated farther up in the text
111393	77	5	86	14	"Projected changes.." this section omits changes over 71% of the Earth's surface, the ocean. In AR5 Ch11 and 12 annual mean surface salinity change was presented, it would be useful to cover all realms, including the ocean, you cover this very briefly in 8.6.1.1 (P101) [Paul Durack, United States of America]	Noted. This section focuses on particular phenomena and regions, e.g. The ITCZ and the monsoons. The water cycle over the oceans is covered in section 8.4.1.
59141	77	9	77	10	Do you mean Byrne et al., 2018a or 2018b? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted-Both Byrne et al. (2018a) and Byrne et al. (2018b) point to the same reference as listed below. The citation has been checked and revised. Byrne, M. P., Pendergrass, A. G., Rapp, A. D., and Wodzicki, K. R. (2018). Response of the Intertropical Convergence Zone to Climate Change: Location , Width and Strength Precipitation climatology. Curr. Clim. Chang. Reports 4, 355–370. doi:10.1007/s40641-018-0110-5."

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113443	77	9	77	16	Once again, I am not sure how to reconcile this weakening with the strengthenning of the Hadley and Walker circulation mentioned several times in this chapter. This topic should be much clearer. [Diego Miralles, Belgium]	Taken into account- I didn't see the projected strengthening of the Hadley cell and Walker circulation in this chapter or peer-reviewed papers so far. Do you mean the intensified ascent in the deep tropics? If so, I'd like to clarify that it only occurs in a limited region with descending anomalies outsides. To identify the mechanisms associated with precipitation response to climate change, the total precipitation change is often partitioned into changes linked with thermodynamic (such as increase in atmospheric moisture) and dynamical (such as weakened upward motion) processes. Thus, the circulation weakening reconciles the thermodynamic increase in water vapor with a smaller increase in total precipitation. More explanations has been given in text.
29095	77	10			8.2.1.3 --> 8.2.2.2 and throughout; a policy-relevant summary assessment statement would be useful in these succinct large-scale circulation sections [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected-Compared with the large-scale circulation, policy decision more depends on variations in the surface temperature and precipitation. It might be more appropriate to present the policy-relevant statements in Section 8.4.1.
41539	77	18	77	23	There are two robust zonal-mean ITCZ responses in CMIP5 models (Byrne et al 2018): [1] A narrowing of the ascent region and [2] an intensification of ascent within the core of the ITCZ. These responses have collectively been termed the "deep-tropics squeeze" by Lau & Kim (2015). The narrowing is mentioned in this paragraph but not the strengthening in the ITCZ core. I think the strengthening should be discussed in this paragraph because it it one of the few robust tropical circulations changes found across models, it has some observational support as discussed elsewhere in the chapter, there is theory emerging to understand it (e.g. Su et al 2019), and it will likely be included in the schematic diagram of circulation responses currently being produced for this chapter. I thinik it would also be good to mention that the magnitudes of changes in ITCZ width and strength across CMIP5 models are not independent and in fact are strongly correlated (Byrne et al 2018). This suggests a robust physical connection between these ITCZ responses that is thought to be related to the longwave cloud effect (Su et al 2019). [Michael Byrne, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account-The robust response of ITCZ in its width and strength (Byrne et al., 2018; Lau and Kim, 2015), their anti-correlation, and changes in the tropical high cloud fraction (Su et al., 2019) have been added in this section.
95893	77	18	77	23	Witout having looked into this myself I am suprrised by the lack of a change of the ITCZ position. CMIP6 (hist-aer minus piControl) gives a clear shift of the ITCZ due to aerosols, so I would expect some of this to revert when cleaning up in future projections? [Philip Philip Stier, United Kingdom (of Great Britain and Northern Ireland)]	Noted-Despite the model tested shift of the ITCZ due to aerosol forcing, we cannot expect the same or opposite changes in future projections. One lies in the limited influence of aerosol in future projection if compared with that of the greenhouse gases. Another consideration is the variation of aerosol emissions over time and between regions. The global contrasting trends in aerosol emissions could help to explain the no significant changes in the zonal mean ITCZ position.

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89105	77	21		23	The statement that the total area of tropical ascent is not projected to change directly contradicts the findings of Byrne and Schneider (2016b) and Su et al (2017) that the zonal mean is projected to narrow. Looking at the methods sections and figures of each paper, I cannot find any indication that either of these analyses is restricted to the ocean. The reason Johnson and Xie (2010) find no change in the area of ascent is that they use a fixed threshold of 2 mm/d to define regions of ascent. It is not appropriate to use a fixed precipitation threshold to determine the changes in ascent area with warming, when we know that precipitation rates are expected to increase overall, and more so when it is raining. Decreases in ascent area by measures of circulation, like convergence, streamfunction, or vertical velocity, would show decreases that would be offset for a fixed precipitation threshold by the increase in precipitation and its intensity. It is not clear to me what evidence in Muller and O'Gorman (2011) and Chadwick et al., (2013) support the statement that the total area of tropical ascent is not projected to change, but I am not aware of any evidence other than the incorrect assertion for this statement other than the incorrect assertion by Johnson and Xie (2010), so this assertion should be removed. [Angeline Pendergrass, United States of America]	Accepted-The relevant statements have been removed.
89103	77	22			The relevant paper is not Byrne and Schneider (2016), but Byrne and Schneider (2016b), "Narrowing of the ITCZ in a warming climate:..." [Angeline Pendergrass, United States of America]	Not applicable-Texts no longer exist.
88145	77	23	77	23	I think the Muller & O'Gorman and Chadwick et al. references have been left here by mistake [Robin Chadwick, United Kingdom (of Great Britain and Northern Ireland)]	Accepted-The relevant statements and references have been removed.
74419	77	23	77	23	replace …. , 2013)(Johnson … by …. , 2013 ; Johnson … [Moulay Driss HASNAOUI, Morocco]	Not applicable-Texts no longer exist.
43257	77	23			Read "(Johnson and Xie, 2010; Muller and O'Gorman, 2011; Chadwick et al., 2013). " rather than "(Muller and O'Gorman, 2011; Chadwick et al., 2013)(Johnson and Xie, 2010). " [Cyriaque Rufin Nguimalet, Central African Republic]	Not applicable-Texts no longer exist.
59143	77	28	77	28	Xie et al., 2010 is missing in the reference list [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account- Reference has been added.
59145	77	28	77	28	Huang et al., 2013 is missing in the reference list [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account- Reference has been added.
59147	77	30	77	30	Do you mean Kooperman et al., 2018a or 2018b? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account-Here refers to Kooperman, G. J., Chen, Y., Hoffman, F. M., Koven, C. D., Lindsay, K., Pritchard, M. S., et al. (2018a). Forest 38 response to rising CO2 drives zonally asymmetric rainfall change over tropical land. Nature. Clim. Chang. 8, 434–39 440. doi:10.1038/s41558-018-0144-7. Citation has been revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
88147	77	30	77	33	Why only pick out the north African response in this section? [Robin Chadwick, United Kingdom (of Great Britain and Northern Ireland)]	Noted-Precipitation over northern Africa experiences opposite changes since the 1980s, and there are peer-reviewed papers in support of its relation with ITCZ shift. We would also consider including other regions if new evidence emerges.
59149	77	32	77	32	Do you mean Dong and Sutton 2015a or 2015b? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account- Dong and Sutton 2015a and 2015b point to the same reference. Dong, B., and Sutton, R. (2015). Dominant role of greenhouse-gas forcing in the recovery of Sahel rainfall. Nature. Clim. Chang. 5, 757–760. doi:10.1038/nclimate2664. Citation has been revised.
22425	77	33	77	33	This section has no summary statement [Peter Thorne, Ireland]	Taken into account-A summary has been given.
17697	77	33	77	34	Section 8.4.2 could be significantly enhanced by the addition of a reference to projected change of large-scale phenomena in southern Africa. As stated in the executive summary, southern Africa is a region with medium to high confidence of projected drying, and so the chapter would benefit from describing the associated projected changes in large-scale circulation. This would bring section 8.4.2's coverage of southern Africa in line with the other regions of projected drying. A proposed addition may be, at line 34 of page 8-77: "In southern Africa, the projected delay in the wet season onset (Dunning et al 2018) is associated with a circulation-based northward shift in the tropical rain band (Lazenby et al 2018). This shift is related to an increased persistence and a slowdown in the seasonal southward progression of the Congo Air Boundary (CAB) in between September and December (Howard and Washington 2020). The behaviour of the CAB is closely linked to that of the Angola heat low (Howard and Washington 2019), which is projected to intensify as a response to greenhouse gas forcing (Munday and Washington 2019)." I would be happy to provide more details. References: •Dunning, C. M., Black, E. C. and Allan, R. P. (2018), `Later wet seasons with more intense rainfall over Africa under future climate change', Journal of Climate 31, 9719–9738, https://doi.org/10.1175/JCLI-D-18-0102.1 •Howard, E and Washington, R (2019) Drylines in southern Africa: rediscovering the Congo Air Boundary. Journal of Climate 32 8223-8241. https://doi.org/10.1175/JCLI-D-19-0437.1 •Howard, E and Washington, R. (2020) Tracing future spring and summer drying in southern Africa to tropical lows and the Congo Air Boundary. (Accepted April 2020). https://doi.org/10.1175/JCLI-D-19-0755.1 •Lazenby, M. J., Todd, M. C., Chadwick, R. S. and Wang, Y. (2018), `Future precipitation projections over central and Southern Africa and the adjacent Indian Ocean: What causes the changes and the uncertainty?', Journal of Climate 31, 4807-4826. https://doi.org/10.1175/JCLI-D-17-0311.1	Taken into account-Thanks for bringing these references to our attention. We have abstracted the statement you proposed and added it into this paragraph.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24421	77	38	78	21	The reference (Zhou B. T., Shi Y., Xu Y.. CMIP5 simulated change in the intensity of the Hadley and Walker circulations from the perspective of velocity potential. Adv. Atmos. Sci., 2016, 33(7): 808-818, doi: 10.1007/s00376-016-5216-x) needs to be cited.This paper assessed the simulations of CMIP5 models in the intensity of Hadely and Walker circulations and projected their changes toward the end of this century under RCP4.5 and RCP8.5 scenarios from the perspective of 200 hPa velocity potential. The MME projects a weakening of winter Hadley circulation and Walker circulation by the end of the 21st century, with larger changes under RCP8.5 than RCP4.5. For summer Hadley circulation, the MME shows little change under RCP4.5 and a weakening under RCP8.5 [Zhou Botao, China]	Taken into account-The reference has been cited.
12297	77	43	77	46	"The expansion of the Hadley cell and northeastward shift of the northern hemisphere storm tracks are associated with distinct drying in the southern semi-arid part of the Mediterranean and slight wetting tendencies to the north, in central Europe in 21st century projections". Recent research demonstrates that this statement might be partially true in boreal winter but not in boreal summer (Brogli et al. 2019a). During summer thermodynamic changes, most importantly the land-ocean warming contrast, are more important drivers for the Mediterranean precipitation. In fact, the local Hadley cell does not expand polewards during summer in the Mediterranean region in CMIP5 models analyzed in Brogli et al. (2019b), consistent with more general studies (e.g. D'Agostino et al. (2017)). The two references not in the chapter list are Brogli, R., Sørland, S. L., Kröner, N., & Schär, C. (2019a). Causes of future Mediterranean precipitation decline depend on the season. Environ. Res. Lett., 14, 114017. https://doi.org/10.1088/1748-9326/ab4438 and Brogli, R., Kröner, N., Sørland, S. L., Lüthi, D., & Schär, C. (2019). The Role of Hadley Circulation and Lapse-Rate Changes for the Future European Summer Climate. J. Climate, 32, 385–404. https://doi.org/10.1175/JCLI-D-18-0431.1 [Roman Brogli, Switzerland]	Taken into account-Thanks for your comments. We have clearly stated that the projected precipitation reduction in subtropical arid regions could be dominated by land-sea thermal contrast, the direct radiative effect of greenhouse gases, and other factors, not limited to the poleward shift of the Hadley circulation.
59151	77	52	77	52	Frierson et al., 2007 is missing in the reference list [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account-The reference has been added.
28973	77				8.4.2: summary statements would be beneficial after each subsection [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted, consistent with chapter assessment elsewhere, plant effects are assigned low confidence
21075	78	1	78	1	To my understanding He and Soden (2015b) say that most of the weakening of the Hadley cell is caused by the mean SST warming. Please check. [Marcelo Barreiro, Uruguay]	Taken into account- According to He and Soden (2015), the mean SST warming dominates the mean weakening of the tropical circulation. In terms of the spatial pattern of weakening, the mean SST warming, SST warming patterns and direct CO2 forcing are all important. We have added the role of mean SST warming and direct CO2 forcing in the revision.
71003	78	1	78	6	In the three Mediterranean climates of the Mediterranean itself, California and Chile, the precipitation (and P-E) changes in both seasons will evolve with time due to changing contributions from the rapid adjustment, the fast SST response, and the slow SST response (Zappa et al. 2020 doi: 10.1073/pnas.1911015117). The role of the different timescales of the SST response is being underemphasized in this chapter. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account- The role of time-evolving SST pattern in the Mediterranean hydrological climate has been highlighted in this paragraph.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12299	78	3	78	4	Basically the same comment as above, research specific to the (European) Mediterranean region shows that the land-sea contrast is a very important driver for the precipitation decline in summer, while dynamic changes (including the Hadley cell) are of primary importance in winter. [Roman Brogli, Switzerland]	Taken into account-Statements on the modulation of land-sea thermal contrast in the Mediterranean summer precipitation has been added.
12941	78	4			replace "is complicated" by "it is complicated" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable-Texts no longer exist.
22427	78	6	78	6	This section has no summary statement [Peter Thorne, Ireland]	Taken into account-A summary has been given.
22429	78	9			Changes in the walker circulation and implications are discussed in some depth in chapters 4 and 7. These should be cross-referenced and a review made for consistency. Section also lacks a summary (amuch more minor issue) [Peter Thorne, Ireland]	Taken in account. Consistency between chapters was verified. A more comprehensive assessment was done and it has a summary now
53407	78	9			A surprisingly brief assessment given the major influence on the tropical water cycle and its variability, as well as on possible teleconnections with the extratropical circulation. [Hervé Douville, France]	Taken in account. A more comprehensive assessment was done
103719	78	11	78	11	: "The Walker circulation is projected to weaken..". The models indeed project this but this is likely due to systematic biases in zonal SST gradients over the Pacific. See eg Seager et al Nature, 2020 [Philippe Tulkens, Belgium]	references were reviewed
17283	78	11	78	21	Changes in the Walker circulation has direct relevance to features like ENSO. Surely more than one paragraph on projected changes is needed? What did AR5 report? What is new in AR6? [Joelle Joelle Gergis, Australia]	taken into account, a more comprehensive assessment was done
12943	78	14			Missing space in "cooling(Knutson" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	done
21077	78	15	78	15	To my understanding He and Soden (2015b) say that the pattern of SST warming does not weaken the Walker circulation despite a reduction in the zonal SST gradient (that is, an El Niño-like pattern). Please check. [Marcelo Barreiro, Uruguay]	taken in account, this subsection was reviewed to provide a more comprehensive assessment
103721	78	15	78	16	Same thing. The reduction in zonal SST gradients under GHG forcing is likely due to model bias mentioned above [Philippe Tulkens, Belgium]	Taken into account, this section has been revised and a more thorough assessment was done
103723	78	17	78	18	However, it is uncertain whether projected changes in equatorial SST gradients are consistent with observed trends. Rephrase: observed trends and modeled trends in SST gradients are in opposite directions. Its unlikely that this can be explained by natural variability. And more likely due to model biases in cold tongue (see again Seager et al and papers cited in there) [Philippe Tulkens, Belgium]	this section was rewritten and updated to included a more detailed assessment
12945	78	20	78	21	Doesn't internal variability have a role to play in Walker circulation changes that may make past trend to future projection comparisons unwise? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	taken into account, a more comprehensive assessment was done

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12947	78	20	78	21	It is not clear what is meant by rainband shifts in the zonal direction. What major rainbands are oriented north/south and are translated east/west? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	taken into account, this subsection was thoroughly reviewed, more bibliography included and a more extensive assessment was done
39969	78	23			Assessment results lack of confidence language in the section 8.4.2.4 [TSU WGI, France]	now all the assessment are written with levels of confidence
109407	78	24			Also in this section: is there any reason why South African monsoon is not mentioned anywhere? [Roberta D'Agostino, Germany]	Precipitation changes in the South African region (Fig.8.11 and Fig.8.22) are assessed in Box 8.2 Changes in water cycle seasonality. Whether this region qualifies as a monsoon region remains a topic of debate.
22433	78	24			Given the import of monsoon changes to society these short segments feel unduly compressed. There is a strong case for spending much longer on changes in the regional monsoons and this will support WG2 assessment of impacts. There needs to be a more in-depth assessment. I know from AR5 approval that parties will want this information on start date, end date, intensity, variability etc. to be pulled through. To do this these segments need to be considerably expanded to support a strong assessment in this area. [Peter Thorne, Ireland]	Thank you. This is implemented in the FGD.
53409	78	24			May deserve a final summary ? [Hervé Douville, France]	Thank you. Summaries are provided for all the monsoon regions.
69517	78	26	78	27	This sentence does not make sense. Thermodynamic increases in moisture convergence are based on the simplification that the circulation does not change under warming. Decreases in the tropical circulation then modify this thermodynamic response (primarily by wekening it). [Martin Singh, Australia]	Accepted. The sentence is suitably modified.
20523	78	26	78	31	A feature that emerges when looking at the cleverly structured figure 8.22 is the fact that the behaviour of "seasonal mean precipitation" is associated with LONGITUDE. Consider the sequence NAmer, EqSA, SAmer, WAfri, SAfri, SAsia, Easia, AusMC [philippe waldteufel, France]	Thank you. In Fig.8.22, the monsoon regions are shown starting from the American region and extending eastward into East Asia. To be consistent with the observed monsoon changes Fig.8.11, the regional monsoons are assessed starting from SAsiaM.
7067	78	27			This sentence is a bit confusing. It suggests that the increase in moisture convergence is associated with (which a reader may interpret as caused by) the weakened tropical circulation, rather than despite it. I would replace "associated with" by "despite". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Thank you. As suggested 'associated with' is replaced by 'despite'.
29097	78	27			this does not seem correct: the weakening circulation offsets thermodynamic increases in monsoon intensity (also 8.2.1.3 --> 8.2.2.2) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken in account. The sentence is modified to avoid confusion
7069	78	28			Remove "from" as it is unnecessary for understanding the sentence. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Done

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7071	78	34	78	39	The figure is well designed and very informative. The caption should include more details on: the base period chosen and the definition of the standard near, mid and long-term periods, as well as number of members per model that were considered. Hopefully there will be more CMIP6 models included by the time of the FGD. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account in the FGD. In the FGD, 24 CMIP6 models are considered (Figure 8.22). The near, mid and long-term periods are provided in the figure caption.
80645	78	42	79	30	Differences in aerosol emissions between the SSP scenarios will likely affect the SA and EA Monsoons. See e.g. Wilcox et al. 2020, ACP, https://doi.org/10.5194/acp-2019-1188 (in Discussion, but with positive reviews and should be available well in advance of the revised deadline.) [Bjorn Samset, Norway]	Taken into account. This point concerning the differences in the aerosol emission scenarios across the SSPs is included in the FGD. The Wilcox et al. 2020 paper is cited.
53411	78	42			Could be more comprehensive, for instance quote the CMIP5 studies by Sooraj et al. (2015, 2016) on both the monsoon circulation and the precipitation intraseasonal variability? [Hervé Douville, France]	Taken into account. Additional references based on the CMIP5 models, including Sooraj et al. 2015, are included in the FGD.
29099	78	42			The section title is inconsistent with the corresponding Section 8.3.2.4.1 which seems to contain many sections that could be combined and condensed [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	taken in account; this section (and the rest o the monsoons) are consistent with the ones in section 8.3
7073	78	46			Is this a correct use of the calibrated uncertainty language? There is no quantification here and therefore a likelihood statement cannot be given. Furthermore, a confidence statement cannot be given either, since a single run of a single model is only a single piece of evidence and therefore nothing can be said about the level of agreement. Therefore no calibrated language can be used to describe this decrease, unless I have misunderstood the intended meaning here. Why has a confidence or likelihood statement not been given instead for the earlier part of the sentence governed by "most studies"? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have dropped the likelihood statement and modified the sentence. Also, high confidence is assigned for the earlier sentence which is based on "most studies".
7075	78	49	78	50	Is this sentence related to a single model or many models etc.? CMIP5? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	This is based on multiple CMIP5 models and also CMIP3 models. This is included in the FGD.
7077	78	49			Hyphenate "low-level" in "low level jet". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Done.
7079	78	51			"eventhough" is two words so replace with "even though". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Done

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7081	78	54			I understand how the poleward shift would contribute to this, but how would the weakened genesis do so? Perhaps the sentence needs to be rephrased to avoid confusion. In addition, it's not really clear what a weakened "genesis distribution" is? Is that higher pressure in the monsoon trough? Or is the PDF of monsoon depression vorticity somehow of reduced amplitude? Not clear from the wording. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This sentence is rephrased in the FGD.
74421	78	41 and 44	78	41 and 44	need to put and define abreviation "SASiaM" for "South Asian Monsoon" here before the following citation also according to lign 6 in page 8-79. [Moulay Driss HASNAOUI, Morocco]	Thank you. Done.
98087	79	2	79	2	"3% increase (high confidence)" the high confidence seems out of place here. Also the relatively small change will be very difficult to detect in the data. [Thomas Knutson, United States of America]	Accepted. Confidence level is dropped.
45533	79	6	79	8	A work from Chen et al. (2020 GRL) based on multi-scenarios projection of 19 CMIP6 models reveals the projected changes of global monsoon summer precipitation and also has similar results. The robust increases of precipitation are seen over South Asia and East Asia monsoon region in the long-term projection across different scenarios. This results are similar with that in Figure 8.22 and here as well as Section 8.4.2.4.2. You may check it: Chen, Z., Zhou, T., Zhang L. et al. (2020). Global land monsoon precipitation changes in CMIP6 projections, Geophys. Res. Lett. [Wenqi Zhang, China]	We have included the Chen et al. 2020 reference in the FGD.
7085	79	6	79	10	For near-term interactions it would be worth citing the near-term influence of internal variability, e.g. as suggested by Huang et al. (2020): DOI: 10.1126/sciadv.aay6546 South Asian summer monsoon projections constrained by the interdecadal Pacific oscillation. This is based on the findings from a single-model large ensemble study. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Thanks. The Huang et al. 2020 paper on the influence of internal variability on the near-term projections is mentioned in the FGD.
7089	79	6	79	10	I trust that further CMIP6 results will be considered in the FGD. An example of the different near/mid-term projections that may emerge for South Asia given the potentially different future air pollution policies (and thus in the SSP experiment design) is that of Wilcox et al. (2019): "Accelerated increases in global and Asian summer monsoon precipitation from future aerosol reductions", https://doi.org/10.5194/acp-2019-1188. The large diversity in aerosols emissions pathways over Asia gives rise to very different sulphate loadings over India and China. Depending on the SSP chosen, there may be very different trajectories for the monsoon. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	We have included results from the CMIP6 models in the FGD. Also the Wilcox et al. 2020 paper is cited in the FGD.
7083	79	6			Change "show increase" to "show an increase". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Done
83861	79	8	79	10	Fig.2 of Ha et al. 2020 shows that the median of MME on duration of monsoon is still close to zero indicating low confidence in this statement. Besides there is considerable varaibility amiong the 16 models especially on retreat. [Ajaya Mohan Ravindran, United Arab Emirates]	Thank you. The large spread across the models in the projected late retreat is mentioned in the FGD.
7087	79	9			In the phrase "at least in the SSP2-4.5" remove "the" since "SSP2-4.5" is not followed by a noun. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Done

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22431	79	10	79	10	This section has no summary statement [Peter Thorne, Ireland]	Summary statement is included in the FGD.
74423	79	13	79	13	to put abreviation with "East Asian Monsoon" (EAM) [Moulay Driss HASNAOUI, Morocco]	Thank you. Done.
72271	79	13	79	30	A post-AR5 analysis of CMIP5 projections by Endo et al. (2018) showed the opposite roles of warming over land and ocean in the future summer monsoon circulation. The warming over the ocean weakens the future summer monsoon circulation over East Asian ocean whereas the warming over the continent strengthens the future summer monsoon circulation over East Asian continent at surface. The future summer monsoon over East Asian ocean is quite different from that over East Asian continent. Japan, Korea and the surrounded region are strongly influenced by the summer monsoon over East Asian ocean. Therefore, the information on the future summer monsoon over East Asian ocean is necessary in the section to some extent , at least, about the weakened Pacific High in the future. (Reference) Endo, H., A. Kitoh, and H. Ueda, 2018: A unique feature of the Asian summer monsoon response to global warming: the role of different land-sea thermal contrast change between the lower and upper troposphere. SOLA, 14, 57-63, DOI:10.2151/sola.2018-010. [Tomoaki Ose, Japan]	The Endo et al. 2018 paper is discussed in the FGD 8.4.2.4.2
72273	79	13	79	30	As commonly shown in Figure 4.27 in AR6 and Figure 12.22 in AR5, increase in JJA mean precipitation is projected in the northern East Asia consistently among the CMIP models. However, future monthly precipitation during JJA tends to be influenced by atmospheric circulation changes. For example, a post-AR5 analysis of CMIP5 projections by Horinouchi et al (2019) showed that the northward migration of early summer East Asian rainband such as the Baiu is delayed along with that of the mid-latitude westerly jet in the future and the associated vertical motion. Ose (2019a and 2019b) indicated that the weakened summer mosoon circulations over the northern East Asian ocean induce tropospheric downward motions and tend to make relatively large uncertainty in future monthly precipitaion increase over the northern East Asian ocean. Japan, Korea and the surrounded region are strongly influenced by the summer monsoon over East Asian ocean. Besides, seasonal migratioin of precipitation displays distincitive features from month to month in summer East Asia. Therefore, the information on the future summer monthly precipitation is expected in the section to some extent, for example, about the effect of the weakened monsoon circulation over the northern East Asian ocean on delaying northward migration of early summer East Asian rainband and making relatively large uncertainty in future monthly precipitaion increase over East Asian ocean. (Reference) Horinouchi T., S. Matsumura, T. Ose, and Y. Takayabu (2019) Jet-precipitation relation and future change of the Mei-Yu-Baiu rainband and subtropical jet in CMIP5 coupled GCM simulations. J. Climate, 32, 2247-2259. DOI:10.1175/JCLI-D-18-0426.1 Ose, T., 2019a: Characteristics of Future Changes in Summertime East Asian Monthly Precipitation in MRI-AGCM Global Warming Experiments. J. Meteor. Soc. Japan, 97, 317-335, doi:10.2151/jmsj.2019-018. 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]	The Horinouchi et al. (2019) and Ose (2019) are discussed in the FGD 8.4.2.4.2

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7103	79	13	79	30	Nothing has been mentioned for EAsiaM about the near-term projections or potential interaction with modes of variability at that time horizon. In addition, is there any literature available yet to be assessed on the role of considering different aerosol pathways in SSP for the future EAsiaM? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Uncertainties in the near-term projections of the EAsiaM due to internal climate variability and uncertainties in the aerosol emission trajectories are mentioned in the FGD.
74425	79	16	79	16	word monsoon after abrevaition EAM is not necessary … (monsoon to be sppressed) [Moulay Driss HASNAOUI, Morocco]	Thank you. Done.
7091	79	16			"EAM" should be "EAsiaM" in accordance with your terminology [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Thank you. Done.
7093	79	18			Change "by intensified land-sea thermal contrast" to "by an intensified land-sea thermal contrast" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Thank you. Done.
7095	79	20			Change "in CMIP5 RCP8.5 scenarios" to "in the CMIP5 RCP8.5 scenario" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Thank you. Done.
87675	79	23	79	23	1.5°C rather than 1.5° [Kenji Tanaka, Japan]	Done
7097	79	23			Change "implies" to "implied" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken in account
7099	79	25			Is the 50% increase in the frequency or intensity? This always needs to be stated when describing extremes. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	It is 50% increase in the frequency of persistent extremes. This is included in the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
72269	79	28	79	30	As commonly shown in Figure 4.28 in AR6 and Figure 12.18 in AR5, negative sea-level pressure anomaly beyond the standard deviation is cleary projected in the northern East Asian ocean in JJA. This anomaly represents a weak northward migration of the summer Pacific High in the future, meaning the weakened summer climatological circulation over the northern East Asian ocean. Japan, Korea and the surrounded region are strongly influenced by the summer monsoon over East Asian ocean. The description of 'East Asia with intensified circulation' in summary is clearly inconsistent with the above mentioned future change in the Pacific High. [Tomoaki Ose, Japan]	Thank you. This assessment is modified in the FGD.
7101	79	28			Change "post AR5 CMIP5 studies" to "post-AR5 CMIP5 studies" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Done
88915	79	33	79	33	Again, change title to 'African monsoons' since this section also rightly includes the East African monsoon [Dave Rowell, United Kingdom (of Great Britain and Northern Ireland)]	the West African monsoon is the one that was assessed
7109	79	33	79	53	This section on future rainfall change for WAfriM may also want to consider the possibility for opposing storylines that are physically credible to emerge, e.g. in the study of Dosio et al. (2020) Environmental Research Letters, A tale of two futures: contrasting scenarios of future precipitation for West Africa from an ensemble of Regional Climate Models. https://doi.org/10.1088/1748-9326/ab7fde [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	this subsection was rewritten, more literature assessed
42785	79	34	79	37	Later wet seasons with more intense rainfall have been confirmed more recently and attributed to intensification of the Sahara heat low. See remark above: later wet seasons are foreseen in CMIP5 simulations but do not correspond to what is currently observed. A possible rephrasing could thus be: A change in seasonality (Biasutti, 2019; Lebel and Ali, 2009) with more intense rainfall (Panthou et al., 2014) has been observed since the 2000s and attributed to intensification of the Sahara heat low (Dunning et al., 2018a). [Thierry Lebel, France]	taken in account, this subsection was rewritten to consider for the comments of the reviewers and to include a more detailed assessment
59153	79	38	79	38	John et al., 2014 is missing in the reference list [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	taken in account the list of references was verified
80317	79	39	79	39	WAfriM was previously defined in line 34 [Paola Arias, Colombia]	done, and corrected
88917	79	40	79	40	Sorry, another parochial comment, but a clear illustration of this east-west contrast In Rowell et al. (2016). Rowell, D.P., Senior, C.A., Vellinga, M. and Graham, R.J., 2016: Can climate projection uncertainty be constrained over Africa using metrics of contemporary performance? (+ eSupplement) Climatic Change, 134, 621-633 [Dave Rowell, United Kingdom (of Great Britain and Northern Ireland)]	taken in account; a more detailed assessment was done and more literature reviewed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
88919	79	41	79	42	Need to be clear that this possible sensitivity to the convection parameterization is model dependent; Berthou et al find no sensitivity for seasonal mean rain (but find strong sensitivity for extreme rain events). Berthou, S., Kendon, E.J., Roberts, M., Rowell, D.P., Tucker, S. and Stratton, R., 2019: Larger future intensification of rainfall in West Africa in a convection-permitting model. Geophys. Res. Lett., 46, 13299-13307 [Dave Rowell, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - text was rewritten to make a clear statement
7105	79	41			Is it the magnitude or pattern of the rainfall change that is sensitive to the choice of convective parametrization, or both? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	this has been reviewed
88921	79	43	79	43	Start new paragraph for East Africa [Dave Rowell, United Kingdom (of Great Britain and Northern Ireland)]	done
88925	79	43	79	46	Drivers outside the African region are also critical; Rowell and Chadwick (2018). [Dave Rowell, United Kingdom (of Great Britain and Northern Ireland)]	taken in account; a more detailed assessment was done and more literature reviewed
7107	79	43	79	52	These regions are no doubt important and millions of people live there, but just to point out that they lie outside the WAfriM domain depicted in Figure 8.12. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	reviewer is correct, text was rewritten to consider for the region as depicted in figure 8.11
88923	79	46	79	47	Change to "not consistent with the recent decreasing trend observed (Hoell et al., 2017, Rowell et al. 2015) which could be explained by internal variability or aerosol forcing (Rowell et al. 2015)" [Dave Rowell, United Kingdom (of Great Britain and Northern Ireland)]	taken in account; a more detailed assessment was done and more literature reviewed
45535	79	46	79	51	I agree with your statement that recent decreasing trend could be explained by internal variability. A work published in GLR recently indicates that the uncertainty of monsoon rainfall caused by internal variability would maintain in the near-term projection (FigS7 in the paper). In the long-term projection, even if the SST pattern is certain, the uncertainty from model structure may not be reduced (FigS6 in the paper). You may check it: Chen, Z., Zhou, T., Zhang L. et al. (2020). Global land monsoon precipitation changes in CMIP6 projections, Geophys. Res. Lett. [Wenqi Zhang, China]	done

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59155	79	47	79	47	Do you mean Hoell et al., 2017a or 2017b? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	this subsection was rewritten, more literature assessed, in the final version of this subsection neither paper was included
22435	79	49	79	50	This is unclear as written and needs to be revised for clarity as to what you mean here. [Peter Thorne, Ireland]	taken in account, this subsection was rewritten to include more and recent references
88927	79	52	79	52	Need citations for these projected seasonality changes: Dunning et al. and Wainwright et al. papers (already in reference list). [Dave Rowell, United Kingdom (of Great Britain and Northern Ireland)]	taken in account, this subsection was rewritten to include more and recent references
22437	79	53	79	53	This section has no summary statement [Peter Thorne, Ireland]	it has now
7111	80	3			Change "eventhough" to "even though" since it is two separate words. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	done
74427	80	5	80	5	NAMS is to be defined here at least [Moulay Driss HASNAOUI, Morocco]	done
7113	80	8			Is it the presence of the whole Gulf of California that cannot be simulated due to coarse resolution or something specific about it (e.g. the SST distribution therein). [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	taken in account, a more detailed and comprehensive assessment was done for the final version
74429	80	9	80	9	NAM is to be defined here at least [Moulay Driss HASNAOUI, Morocco]	done
7115	80	9	80	10	As in the earlier comment, does this refer to intensity or frequency of extremes or both? Also the wording is a little clunky. Better, "an increase of extreme precipitation events". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	taken in account, a more detailed and comprehensive assessment was done for the final version
7117	80	11			Change "is at odd" to "is at odds". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	done

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4977	80	15	80	17	A decrease in the NamerM is one of the most striking features in fig 8.22, yet the section discussing it deals with (earlier) projections mainly pointing at small or positive changes. The short comment on CMIP6 requires a bit more substance to have confidence in this declining trend shown in fig 8.22 [Bart van den Hurk, Netherlands]	taken in account, a more detailed and comprehensive assessment, including CMIP6 literature, was done for the final version
129229	80	15			"most simulations are model-dependant (sp)" needs clarification. [Trigg Talley, United States of America]	taken in account, text was reviewed for clarity
7119	80	16			Change "the CMIP6 models show decrease of the NAmerM precipitation" to "the CMIP6 models shows decreases of NAmerM precipitation". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	done
22441	80	17	80	17	This section has no summary statement [Peter Thorne, Ireland]	now it has
115569	80	17	80	18	North American Monsoon 8.4.2.4.4 It is important to remember that many factors determine the timing and intensity of the North American Monsoon. Among these the location of the hawaiian High and the behavior of ENSO plus the PDO Cycle are important. It may not be easy to have high confidence in the modeling of these interactions so the change from the AR5 Conclusion to the AR6 Conclusion about the North American Monsoon should be considered as the best estimate at this point in time. [Sigmund Silber, United States of America]	taken in account, this subsection was reviewed to provide a more comprehensive assessment
22443	80	20	80	27	This is insufficient detail to constitute actionable information by a policymaker and needs to be expanded with additional details given. [Peter Thorne, Ireland]	taken in account, this subsection was reviewed to provide a more comprehensive assessment
74431	80	22	80	22	"the" before season is in double tu suppress one like "… of the season …." [Moulay Driss HASNAOUI, Morocco]	done
74433	80	22	80	22	to add that to this expression like … indicate that the SAmerM … [Moulay Driss HASNAOUI, Morocco]	taken in account, and corrected
59161	80	22	80	25	Please add in which direction the onset and end of the SAmerM season shifts instead of only stating that the SAmerM would very likely experience changes in its life cycle. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	taken in account, this subsection was reviewed to provide a more comprehensive assessment
74435	80	23	80	23	to suppress "(" before "onset" like "onset ..." [Moulay Driss HASNAOUI, Morocco]	done
7121	80	23			Change "experienced" to "experience" since the sentence is in the future conditional tense. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	done
43259	80	23			Read " changes in its life cycle, onset and end of the season" rather than " changes in its life cycle (onset and end of the season" [Cyriaque Rufin Nguimalet, Central African Republic]	done
74437	80	24	80	24	RCP's scenarios [Moulay Driss HASNAOUI, Morocco]	done

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98089	80	30	81	7	Could mention here that a detectable anthropogenic influence in northern Australia precipitation has already been identified for 1901-2010, 1951-2010, and 1981-2010 trends (Knutson and Zeng, 2018, Fig. 3-5). This increases the likelihood of further increases. It also provides a test of climate model historical runs: Do models capture the detectable observed increase over the historical periods? If not, why should we believe their projections for the coming decades? Refs: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637 https://doi.org/10.1175/JCLI-D-17-0672.1. [Thomas Knutson, United States of America]	Rejected – This material does not fit in this projections section given that no studies have specifically looked at ability of models to simulate trends in context of their projections, except Dey et al. (2019) which focused on the role of aerosols vs GHG. If anything, it belongs in the observed trends section, but not here in the future projections discussion. No changes made.
7123	80	31			Start the sentence with "The" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – edited as suggested
74439	80	33	80	33	to check if correct according to the meaning of the expression " present day regional climate project little"? [Moulay Driss HASNAOUI, Morocco]	Taken into account – Sentence clarified to read 'Models that perform better at simulating present day regional climate project little change or an increase in Australian monsoon rainfall'
17285	80	34	80	34	Add reference to CSIRO and Bureau of Meteorology (2015). Climate Change in Australia Information for Australia's Natural Resource Management Regions: Technical Report, CSIRO and Bureau of Meteorology, Australia. Also note that it is Brown et al 2017 (as listed in the refernces) not Brown et al 2016. Change all instances in lines 34-38. [Joelle Joelle Gergis, Australia]	Accepted – Citation to CSIRO and BoM (2015) added here and to the reference list. The correct reference here is actually Brown et al (2016):Brown, J. R., Moise, A. F., Colman, R., and Zhang, H. (2016). Will a warmer world mean a wetter or drier Australian monsoon? J. Clim. 29, 4577–4596. doi:10.1175/JCLI-D-15-0695.1. Now corrected in the text and reference list
65803	80	34	80	35	Suggest rephrase for clarity: 'Rainfall changes are correlated with the extent of warming in the western tropical Pacific in CMIP5 models'. [Kushla Munro, Australia]	Accepted – edited as suggested
65805	80	36	80	36	Suggest rephrase for clarity: '… but inter-model differences are ALSO related to modelled large-scale zonal mean precipitation response.' [Kushla Munro, Australia]	Accepted – edited as suggested
65807	80	36	80	36	Suggest rephrase for clarity: ... zonal mean response in both CMIP5 and CMIP6 ensembles.' (Narsey et al, accepted in GRL). [Kushla Munro, Australia]	Accepted – edited as suggested
74441	80	36	80	36	To check if it isn't published about (Nasey et al. Submitted) [Moulay Driss HASNAOUI, Morocco]	Taken into account – Narsey et al (2020) updated in the text and reference list
74443	80	43	80	43	to close by ")" in "(e.g. (…., 2015))". [Moulay Driss HASNAOUI, Morocco]	Accepted – edited as suggested
7125	80	43			Does the favoured northward shift of precipitation therefore imply a weakening of the Australian monsoon? If so, state it explicitly. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected – This was not investigated in Rotstayn et al. (2015), who only looked at zonal mean. Older studies found a reduction in Australian monsoon precipitation but these were based on models with large biases, e.g. CSIRO model, so are considered unreliable. See discussion in AR5. No changes made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17287	80	44	80	44	This section should be updated to reflect CMIP6 analysis of Australian monsoon recently published by Narsey et al 2020): Narsey, S. Y., Brown, J. R., Colman, R., Delage, F., Power, S., Moise, A. and Zhang, H. (2020). Climate change projections for the Australian monsoon from CMIP6 models. Geophysical Research Letters n/a (n/a): e2019GL086816. Also note there are formatting issues with the referencing in this section. [Joelle Joelle Gergis, Australia]	Taken into account – extra sentence added a sentence to the first paragraph summarising the results of Narsey (2020) study: 'An updated assessment using CMIP6 models found a reduced range of projections for Australian monsoon rainfall, but continued disagreement on the sign of change (Narsey et al., 2020)'.
59157	80	46	80	46	Josephine et al., 2017 is missing in the reference list [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account – Corrected formatting error to now read Brown et al (2017).
74445	80	46	80	46	one more "," to suppress from "(Josephine et al., 2017)" [Moulay Driss HASNAOUI, Morocco]	Taken into account – Corrected formatting error to now read Brown et al (2017).
7127	80	46			Not in reference list. I'm guessing this should be Brown et al. (i.e., Jo[sephine] Brown). [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account – Corrected formatting error to now read Brown et al (2017).
74447	80	48	80	48	To check if it isn't published about Ref. Smith et al. Submitted [Moulay Driss HASNAOUI, Morocco]	Taken into account – Lead author changed to Moise et al (2019) paper now published: Moise, A., Smith, I., Brown, J. R., Colman, R., and Narsey, S. (2020). Observed and projected intra-seasonal variability of Australian monsoon rainfall. Int. J. Climatol. 40, 2310–2327. Citation updated in the text and reference list
59159	80	52	80	52	Do you mean Zhang et al., 2013a or 2013b? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account – Citation is Zhang et al (2013), duplication removed from reference list
74449	80	52	80	52	to suppress "(" at the begining of expression like "Zhang et al., (2013)" [Moulay Driss HASNAOUI, Morocco]	Taken into account – formatting error corrected
7129	80	52	80	55	Citation style is wrong in 2 places in this paragraph. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account – formatting error corrected
43261	80	52			Read "Zhang et al. (2013) examined changes in" rather than "(Zhang et al., 2013) examined changes in" [Cyriaque Rufin Nguimalet, Central African Republic]	Taken into account – formatting error corrected

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
76861	81	1	81	6	Worth noting the following findings for the Maritime Continent region from the 2nd National Climate Change Study of Singapore: (1) The region-wide patterns of changes in precipitation in the northeast monsoon season (NDJ) show projected increases over Borneo, Sumatra, Java, and adjacent sea areas. (2) During the southwest monsoon season (JJA) there is a north-south pattern to the projected rainfall changes with some northern parts getting wet and the southern parts of SEA, and the southern part of the South China Sea, showing drying. Reference: Marzin, C., R. Rahmat, D. Berni, L. Bricheno, E. Buonomo, D. Calvert, H. Cannaby, S. Chan, M. Chattopadhyay, W. K. Cheong, et al. (2015) Singapore's Second National Climate Change Study – Phase 1 [Sandeep Sahany, Singapore]	Taken into account. Discrepancies among models in terms of projected precipitation changes over the region are included in the assessment with some recent references about potential understanding on the origin of the differences.
74451	81	2	81	2	abreviation AusMCM to put may be in the title of page 80 lign 30 [Moulay Driss HASNAOUI, Morocco]	Accepted – Amended to read 'The AR5 concluded that projected changes in Australian and Maritime Continent Monsoon (AusMCM) rainfall and seasonality are uncertain in CMIP5 models'
74453	81	5	81	5	abreviation AusMCM to put only instead of the full name. [Moulay Driss HASNAOUI, Morocco]	Rejected – Australian and/or Maritime Continent are generally addressed in separate studies, use of the AusMCM here is incorrect.
7131	81	5			Change "northern and eastern part" to "northern and eastern parts"; next change "has" to "have" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – edited as suggested
65809	81	7	81	7	Suggest rephrase for clarity: '… across all the scenarios, although they do not agree over northern Australia.' [Kushla Munro, Australia]	Accepted – edited as suggested
22445	81	7	81	7	This section has no summary statement [Peter Thorne, Ireland]	Taken into account – Summary statement now added: ' In summary, the CMIP6 projections show an increase of the AusMCM precipitation across all the scenarios, although there is low agreement on the direction of change over northern Australia (Figure 8.22). There is a projected increase in rainfall variability for the Australian monsoon, with increased intensity of rainfall during the active or 'burst' phase (medium confidence)'.
7133	81	7			Perhaps the AusMCM acronym should be introduced again in this paragraph, since it has not bee used since the earlier historical climate section. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account – The acronym is now used in the first line of this section which addresses the reviewer's concerns.
98091	81	10	81	23	A more recent WMO expert team assessment of TC projections for global warming is just out and can be used to update this section (along with updates from Ch. 11): Refs: Knutson, T., S.J. Camargo, J.C. Chan, K. Emanuel, C. Ho, J. Kossin, M. Mohapatra, M. Satoh, M. Sugi, K. Walsh, and L. Wu, 2020: Tropical Cyclones and Climate Change Assessment: Part II: Projected Response to Anthropogenic Warming. Bull. Amer. Meteor. Soc., 101, E303–E322, https://doi.org/10.1175/BAMS-D-18-0194.1 [Thomas Knutson, United States of America]	Taken into account. Section 8.4.2.5 has been substantially revised starting from the assessment done in section 11.7.1.5, and then completing in terms of implications for water cycle changes aspects available in literature.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22447	81	10	81	23	This is so short as to not be meaningful. The text that is there also makes limited sense in a number of places. The piece should be rewritten starting from the chapter 11 finding on TCs generally and then expanded to a consideration of the water cycle components. The findings should exclusively consider the implications for the water cycle to avoid overt overlap with chapter 11. [Peter Thorne, Ireland]	Taken into account. Section 8.4.2.5 has been substantially revised starting from the assessment done in section 11.7.1.5, and then completing in terms of implications for water cycle changes aspects available in literature.
28979	81	10			correct based on 11.7.1 summary "There is high confidence that average tropical cyclone rain-rates will increase with warming, and there is medium confidence that the peak rain-rates will increase at greater than the Clausius-Clapeyron scaling rate of 7% per °C of warming in some regions due to increased low-level moisture convergence caused by regional increases in wind intensity." The wind field perhaps does not need to be mentioned, only the water cycle aspect [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. A summary taken from section 11.7.1.5 has been included, without mentioning conclusions related to the wind as less pertinent to water cycle changes.
53413	81	10			a few typos and more studies could be quoted to support the conclusion about increased heavy precipitation associated to TC (e.g., Chauvin et al. 2017). The final summary could be more consistent with CH11 and specify that there is medium confidence that the peak TC rainfall intensity could in some regions increase at a greater rate than the Clausius-Clapeyron scaling of 7% per °C of warming due to increased low-level moisture convergence. [Hervé Douville, France]	Taken into account. The assessment is now consistent with (and starts from) the conclusions from ch 11. Few more studies (including Chauvin et al 2017) have been included to support the conclusion on precipitation changes related to tropical cyclones projections
17289	81	12	81	23	Many typos and grammatical errors in this section. Also the assessment here is too brief to be useful. Suggest expanding given how important tropical cyclones are for many regions of the world. For example, tropical cyclones are projected to move into heavily populated areas south of 25S in Australia e.g. CSIRO and Bureau of Meteorology (2015). Climate Change in Australia Information for Australia's Natural Resource Management Regions: Technical Report, CSIRO and Bureau of Meteorology, Australia. At a bare minimum, a better summary of chapter 11 results are needed. [Joelle Joelle Gergis, Australia]	Taken into account. The assessment is now consistent with (and starts from) the conclusions from ch 11. Few more studies (including technical report from CSIRO)) have been included to support the conclusion on precipitation changes related to tropical cyclones projections
74459	81	13	81	13	WC is to specify [Moulay Driss HASNAOUI, Morocco]	Not applicable. We have removed the abbreviation from the section
12949	81	13			Spell out "WC" as water cycle. This acronym has not been used anywhere else and thus minimal space is saved. It should be consigned to the lavatory. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The paragraph has been substantially changed. However the abbreviation WC is not used anymore.
7437	81	15	81	15	« In. » iss repeated. Please remove one. [Geremy PANTHOU, France]	Not applicable. The paragraph has been completely rewritten
7995	81	15	81	15	There's an In. at the start of the line that should be removed. [Anthony Lupo, United States of America]	Not applicable. The paragraph has been completely rewritten
129231	81	15	81	15	There's an In. at the start of the line that should be removed. [Trigg Talley, United States of America]	Not applicable. The paragraph has been completely rewritten
74461	81	15	81	15	"in" in double in "in the IPCC …" [Moulay Driss HASNAOUI, Morocco]	Not applicable. The paragraph has been completely rewritten

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6731	81	15	81	18	The initial "In. " needs removing and "f are dependent" needs to be turned into something understandable. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The paragraph has been completely rewritten
12951	81	15			Remove one superfluous "In". Add "the" before "consensus". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The paragraph has been completely rewritten
7987	81	18	81	18	is 'f' = frequency? Also, here and elsewhere make sure references are listed chronologically. [Anthony Lupo, United States of America]	Noted. "f" was for frequency. However the paragraph has been totally rewritten and the symbol removed.
129233	81	18	81	18	Is 'f' = frequency? Also, here and elsewhere, make sure references in this chapter are listed chronologically. [Trigg Talley, United States of America]	Noted. "f" was for frequency. However the paragraph has been totally rewritten and the symbol removed.
74463	81	18	81	18	and f are ? to check if appropriate [Moulay Driss HASNAOUI, Morocco]	Noted. "f" was for frequency. However the paragraph has been totally rewritten and the symbol removed.
12953	81	18			"and f are" ? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. "f" was for frequencies. However the paragraph has been totally rewritten and the symbol removed
43263	81	18			Read "and fare dependent on the different TC " rather than "and f are dependent on the different TC " [Cyriaque Rufin Nguimalet, Central African Republic]	Not applicable. The sentence has been rewritten
7439	81	21	81	21	Typo « heaevy » [Geremy PANTHOU, France]	Accepted. Typo has been corrected
113445	81	21	81	21	heaevy' [Diego Miralles, Belgium]	Accepted. Typo has been corrected
74465	81	21	81	21	to correct heaevy by heavy [Moulay Driss HASNAOUI, Morocco]	Accepted. Typo has been corrected
4979	81	21	81	22	typos in "heaevy" and "ocurrance" [Bart van den Hurk, Netherlands]	Accepted. Typos have been corrected for the words kept in the rewritten sentence
20153	81	21	81	23	One spelling error per line [philippe waldteufel, France]	Noted. The paragraph has been completely rewritten and typos checked.
107711	81	21	81	23	Mention trends in LMI and translation speed? [Emily Collier, Germany]	Rejected. This is part of the assessment done in ch 11
64989	81	21	81	23	looks like this was particularly quickly drafted, four typos in a sentence… [Johannes Quaas, Germany]	Accepted. Typos have been corrected for the words kept in the rewritten sentence

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12955	81	21			Change "heaevy" to "heavy". This section has perhaps not been proof read as there are several spelling/grammar errors ("ocurrance...", "extend of the TC wind...") [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Typos have been corrected for the words kept in the rewritten sentence
12957	81	21			Change "associated to" to "associated with" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The paragraph has been completely rewritten
5583	81	21			Correct incease of heaevy precipitation by increase of heavy… [Benoit Laignel, France]	Accepted. Typos have been corrected for the words kept in the rewritten sentence
74467	81	22	81	22	of occurrence of the TC [Moulay Driss HASNAOUI, Morocco]	Not applicable. The paragraph has been completely rewritten
43265	81	22			Read "the frequency of ocurrence" rather than "the frequency of ocurrance" [Cyriaque Rufin Nguimalet, Central African Republic]	Not applicable. The paragraph has been completely rewritten
12959	81	22			Change "occurance" to "occurrence" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The paragraph has been completely rewritten
12961	81	23			Change "extend of" to "extent of" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The paragraph has been completely rewritten
12967	81	26	81	41	Is there any literature to be assessed on how the silk road pattern (SRP) or circumglobal teleconnection (CGT), which are examples of stationary waves, change with warming? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. A discussion of CGT projections has been added; references for projections of the SRP were not found.
29101	81	26			Stationary waves is brief and not distinct from Blocking (which needs a policy-relevant summary assessment) so these sections could be merged here and in 8.3 also. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted (content) / Rejected (merge). The discussion of stationary waves and their water cycle implications has been extended, and a summary statement has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12963	81	33	81	34	I'm not sure how to interpret this as a reader. Wet-gets-wetter, dry-gets-drier implies an increased amplitude, so are you implying that the wave amplitude increases? And what is being outweighed? The description here needs to be made much clearer. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The relevant text has been removed.
74469	81	34	81	34	add "n" to "souther" to become the "southern hemisphere …" [Moulay Driss HASNAOUI, Morocco]	Editorial – copyedit to be completed prior to publication
12965	81	35			By "wetting southerlies" do you mean "moist southerlies"? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The relevant text has been removed.
43267	81	37			Read "Mediterranean" rather than "Mediterranenan" [Cyriaque Rufin Nguimalet, Central African Republic]	Editorial – copyedit to be completed prior to publication
59163	81	39	81	39	Maybe I got it wrong. But in my understanding, it is no logic, that you write that the global evapotranspiration shows robust decrease. Figure 8.1.8 shows a clear increase. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Evapotranspiration is not discussed in this subsection.
17291	81	44	81	44	To improve regional coverage, please consider results of Marshall, A. G., Hudson, D., Hendon, H. H., Pook, M. J., Alves, O. and Wheeler, M. C. (2014). Simulation and prediction of blocking in the Australian region and its influence on intra-seasonal rainfall in POAMA-2. Climate Dynamics 42 (11): 3271-3288. [Joelle Joelle Gergis, Australia]	Not applicable. Blocking projections are now assessed in Chapter 4 and only summarized here.
74457	81	10 and 12	81	10 and 12	to put the abrevaition TC in "Tropical Cyclones" (TC) [Moulay Driss HASNAOUI, Morocco]	Accepted. The acronym has been included in the introductive paragraph
74455	81	3 and 5	81	3 and 5	to complete "… Maritime Continent" by "monsoon" like "… Maritime Continent monsoon" [Moulay Driss HASNAOUI, Morocco]	Taken into account – edited as suggested
4177	82	2	82	8	There is a set of simple but meaningful theoretical researches by Luo and cooperators to shed light on the blocking response to climate change. According to their studies, meridional gradient of potential vorticity is a vital factor to influence the blocking behaviour, and the PV gradient can be modulated by external forcing (SST, sea ice and so on), which can be regarded as the background condition of blocking events. hence, I think it's helpful for researchers and publics to comprehend the linkage between Arctic change and atmospheric circulation behaviour and it is desirable mentioned here. (recommend to refer: Chen et al. 2019, "Winter Midlatitude Cold Anomalies Linked to North Atlantic Sea Ice and SST Anomalies: The Pivotal Role of the Potential Vorticity Gradient".) [Wenqi Zhang, China]	Not applicable. Blocking projections are now assessed in Chapter 4 and only summarized here.
22449	82	3	82	8	Chapter 3 also looked at this issue and yet is not referenced here and there is a risk of redundant and diverging assessments as a result. It would be good to cross-reference and also is necessary to undertake a cross-check with chapter 3 for consistency. [Peter Thorne, Ireland]	Accepted. Chapter 3 is now referred to.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
100861	82	9	82	9	The improvement of blocking frequency with resolution has been shown in Davini et al. 2017 as well. Also, on the same note, 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). 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. [Corti Susanna, Italy]	Not applicable. Blocking assessment is now done in Chapters 3 and 4 and only summarized here.
100863	82	9	82	12	– I would say that in the case of boreal winter blocking the decrease of blocking frequencies projected for the end of the XXI century for the most extreme scenarios is quite robust and it should be taken in due consideration. Davini and D'Andrea 2020 found that: 1) The models showing the largest decrease in blocking frequency in boreal winter (especially over Greenland and Europe) are those showing the smallest deficit in blocking frequency during the historical period (i.e. the least is the bias the largest is the decrease under extreme future scenarios). (see their figure 7) 2) The negative trend in blocking frequencies exhibited by the models for boreal winter over the period 1950-2100 is significant at 5% confidence. (figure 9) So the signal appears robust enough to consider for an higher confidence level (medium?). Of course this is for boreal winter. For summer the situation is more complex and the models fail to reproduce the observed trend over Greenland, for example. [Corti Susanna, Italy]	Not applicable. Blocking assessment is now done in Chapters 3 and 4 and only summarized here.
21079	82	10	82	11	Please change to SSP3-7.0 and SSP5-8.5 [Marcelo Barreiro, Uruguay]	Accepted.
29107	82	17	83	29	A policy relevant summary paragraph is required and from this it would be useful to condense this section so that it is focused on supporting the summary statements. Note that 8.4.2.6.4 no longer exists. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted, summary statements added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11259	82	17	83	37	Discussions in this section seem to be largely focused on model consensus (or multi-model mean), but do not really address one important issue that has become apparent after AR5: that uncertainties (i.e. model-to-model difference) in projected precipitation changes in many mid-latitude regions can be explained to a large degree by uncertainties in projected storm track or ETC changes. Multiple studies (Zappa et al. 2015; Chang 2013; Chang et al. 2015; and Osburn et al 2018) have shown strong relationships between model projected precipitation change in many regions and model projected change in storm track activity near that region. In my opinion this is an important issue for continuing research to understand the model-to-model differences in projected storm track changes that should be discussed in this section related to how projected storm track changes might impact the hydrological cycle in the future. References: Zappa, G., Hawcroft, M. K., Shaffrey, L., Black, E., and Brayshaw, D. J. (2015). Extratropical cyclones and the projected decline of winter Mediterranean precipitation in the CMIP5 models. Clim. Dyn. doi:10.1007/s00382-014-2426-8. Chang, E.K.M, 2013: CMIP5 projection of significant reduction in extratropical cyclone activity over North America. J. Climate, 26, 9903-9922, doi:10.1175/JCLI-D-13-00209.1. Chang, E.K.M., C. Zheng, P. Lanigan, A.M.W. Yau, and J.D. Neelin, 2015: Significant modulation of variability and projected change in California winter precipitation by extratropical cyclone activity, Geophys. Res. Lett., 42, 5983-5991, doi: 10.1002/2015GL064424. Osburn, L., K. Keay, and J. L. Catto, 2018: Projected change in wintertme precipitation in California using projected changes in extratropical cyclone activity. J. Climate, 31, 3451-3466. [Edmund Kar-Man Chang, United States of America]	Noted, text modified to discuss uncertainties, first of your references was added to the FGD. Further discussion is in section 8.5.
5589	82	17	84	37	The results of CMIP6 are not clear : Are the results the same of the AR5 report : extratropicazl storms wer expected to decrease in the N hemisphere and precipitation associated with thses storms was projected to increase or not ? [Benoit Laignel, France]	Text extensively revised, summary statement added in the FGD.
28981	82	17			Missing summary - check for consistency with 11.7 "high confidence that average and maximum ETC rain-rates will increase with warming, mostly due to increases in atmospheric water vapour." [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted, text reworked and this passage removed in the FGD.
22455	82	17			Similarly to my comment in section 8.3 the dynamical aspects of storm tracks are in the charge of 2-3-4 and not 8. Chapter 8 should take these findings and then expand to discuss implications for the hydrological cycle. Again, similarly, the atmospheric rivers part is novel and should be retained and expanded upon. But chapter 8 should not be redundantly repeating aspects assessed in prior chapters as it invites readers to play spot the difference. It is really only p.83 ln 29-37 that is broadly in scope here and the precursor analysis should be from the prior chapters with the reader referenced there for further information. [Peter Thorne, Ireland]	Noted. Atmospheric rivers now its own sub-section in the FGD. Some dynamical discussion has been retained as it is necessary to make the text coherent.
53415	82	21			First explain that reanalysis data suggest that extratropical cyclone precipitation scales with the product of cyclone intensity (as measured by near-surface wind speed) and atmospheric moisture content (Pfahl and Sprenger 2016)? [Hervé Douville, France]	Noted, and included in discussion.
79435	82	23	82	23	Maybe say that latent heat influence ETCs but drives seems a little too much. [Alejandro Di Luca, Australia]	Noted, text modified.
69521	82	23	82	23	Latent heating does not drive extratropical storms. In some situations, latent heating might contribute to their amplification, but even that depends on details. [Martin Singh, Australia]	Noted, text modified.

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29103	82	24			this could be substantiated by refering to Zhang et al. (2019) GRL who find enhanced latent heat release through atmospheric rivers can invigorate the parent storm [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted, text modified and Zhang et al (2019) reference included in FGD.
129235	82	27			Increase in an increase? [Trigg Talley, United States of America]	Noted, text modified.
22451	82	29	82	30	As presently formulated it is hard to tell from where this confidence arises. Is it the papers mentioned previously or something else? Better traceability to support this finding is required. [Peter Thorne, Ireland]	Noted, text modified and sevral references added in the FGD.
71005	82	30	82	32	We are submitting a manuscript (E Bevacqua, G Zappa and TG Shepherd: “Shorter cyclone clusters modulate changes in European wintertime precipitation extremes”) which shows across the CMIP5 models that the accumulated precipitation in wintertime cyclone clusters generally increases across Europe, although by less than the mean precipitation per cyclone, because of a decrease in the overall number of cyclones within clusters. This dynamical modulation of the accumulated precipitation varies substantially between northern and southern Europe. You may contact e.bevacqua@reading.ac.uk for the submitted version of this paper. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text in this subsection has been extensively modified and further discussion of changes related to ETC frequency added. However this point related to European precipitation has not been explicitly included.
22453	82	34	82	40	As written this is solely about dynamics. What is the relevance to the hydrological cycle? Where is the reference to what chapter 4 assessed if it did so? [Peter Thorne, Ireland]	Noted, references included in the text in the FGD. We have not included a cross-reference to Chapter 4, section 4.5.1.6.
12969	82	34			change "souther" to "southern" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted, fixed.
53417	82	34			southern [Hervé Douville, France]	Noted, fixed.
5585	82	34			Correct souter hemisphere by southern [Benoit Laignel, France]	Noted, fixed.
74471	82	35	82	35	replace "than" by "do" in "… likely do not to shift" [Moulay Driss HASNAOUI, Morocco]	Rejected, phrase is part of a standard confidence statement.
39131	82	39	82	39	Why use a bulled point here instead of a new subsection, similar to 8.3.2.8.1 ? [Jean-Louis Bonne, France]	Noted, Atmospheric River now changed to be a separate subsection int he FGD.
53419	82	39	82	40	and was shown to be the result of both a shift in cyclogenesis and an enhanced latitudinal displacement of individual cyclones (Tamarin-Brodsky and Kaspi 2017). [Hervé Douville, France]	Thanks, added.
79437	82	42	82	42	"uncertainty of the projection" of the intensity or frequency of ETCs? Also, competing factors for what? The number of ETCs? [Alejandro Di Luca, Australia]	Noted, text changed.
129237	82	54			recommend "investigated" rather than "explored". [Trigg Talley, United States of America]	Noted, text changed.
59165	82	55	88	55	Do you mean Zappa and Shepherd 2017a or 2017b? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Now only one Zappa and Shepherd (2017) referenced.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116763	82		84		check ch4 and help integrate results for TS. Missing summary sections. [Valerie Masson-Delmotte, France]	Section 8.4.2.8 revised and split into two sub-sections, with summary statements. Checked for consistency with Chapter 4.
59167	83	1	83	1	Do you mean Wang et al. 2017a, 2017b, 2017c or 2017d? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Wang J. Et al (2017) is what's referred to. Agree the citation is unclear.
43269	83	2			Read "between their influences " rather than "between the influences " [Cyriaque Rufin Nguimalet, Central African Republic]	Thanks - fixed.
74473	83	4	83	4	suppress "(" at the begining of "Ciasto et al. (2016) " [Moulay Driss HASNAOUI, Morocco]	Fixed.
43271	83	4			Read "further examined by Ciasto et al. (2016)," rather than "further examined by (Ciasto et al., 2016)," [Cyriaque Rufin Nguimalet, Central African Republic]	Thanks - fixed.
12971	83	4			Reference needs to be properly included in the sentence. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted, citation text fixed in FGD.
71009	83	6	83	9	Mindlin et al. (2020 doi: 10.1007/s00382-020-05234-1) has examined storm track and related precipitation changes for the SH, and finds an interesting tug of war between the effects of GHG increases on tropical upper tropospheric warming and on stratospheric polar vortex breakdown delay, especially for summer. Although both act in the same way on the SAM, they act in opposite ways on regional precipitation, because of zonally asymmetric effects on storm tracks. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted, reference added and text modified.
1287	83	9	83	10	Exactly how are atmospheric rivers associated with extratropical cyclones? [Rasmus Benestad, Norway]	Text removed in the FGD.
12973	83	9	83	11	The sentence discussion atmospheric rivers and refers to an earlier section, yet there is a heading and associated paragraph on atmospheric rivers to follow in this section. In light of the later paragraph, is this sentence even necessary? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - Text removed.
79439	83	10	83	10	"Atmospheric rivers, which are associated with extratropical cyclones, have been the focus of considerable research, especially with regards to extreme precipitation; this is discussed in Section 8.4.2.6.4." This could be moved to the Atmospheric rivers section below maybe? [Alejandro Di Luca, Australia]	Not applicable - Text removed.
39127	83	11	83	11	The reference to section 8.4.2.6.4 is wrong. Should be 8.3.2.8.1 [Jean-Louis Bonne, France]	Not applicable - Text removed in FGD.
74475	83	11	83	11	to suppress section 8.4.2.6.4 because it dosn't exist in the chapter 8 [Moulay Driss HASNAOUI, Morocco]	Not applicable - Text removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89107	83	11			Section 8.4.2.6.4 no longer exists in this draft, so the reference should be removed. [Angeline Pendergrass, United States of America]	Not applicable - Text removed.
129239	83	11			Actually, atmospheric rivers are discussed a few paragraphs down. [Trigg Talley, United States of America]	Not applicable - Text removed.
69523	83	13	83	13	Not sure if thise one sentence paragraph is meant to be here, but it reads as a bit of a non sequitor. [Martin Singh, Australia]	Noted, text modified.
17293	83	13	83	37	This section is very light. Include regional information. For example mid-latitude storm track projected to shift poleward and the westerlies are projected to strengthen over Australia. Decline in extra tropical cyclones projected for southern Australia and east coast lows in eastern Australia. CSIRO and Bureau of Meteorology (2015). Climate Change in Australia Information for Australia's Natural Resource Management Regions: Technical Report, CSIRO and Bureau of Meteorology, Australia. [Joelle Joelle Gergis, Australia]	Noted. Extra regional information added to FGD, including CSIRO/BoM reference.
89109	83	13		14	This single sentence should not constitute an entire paragraph; it should be integrated somewhere. [Angeline Pendergrass, United States of America]	Noted, text modified and this sentence integrated into a longer paragraph.
53421	83	16	83	27	Model evaluation could appear earlier in the assessment. You may also quote Booth et al. (2018) showing that the fraction of rainfall generated by the convection scheme in simulated extratropical cyclones is highly model- and resolution-dependent, which may be a source of uncertainty regarding their precipitation response to anthropogenic forcings. The last statement in line 27 could specify if the conclusion is based on a single or multi-model study. It could be added that the response may be even more sensitive to the representation of convective processes (cf. CH11 and Section 8.5.1), but that global climate simulations that explicitly resolve fine-scale and frontal features of extratropical cyclones also show a precipitation increase that scales with Clausius-Clapeyron (Kodama et al., 2019). [Hervé Douville, France]	Noted, text modified and Booth et al. (2018) reference added.
17295	83	17	83	21	Change 'boreal fall' to austral spring as we are talking about impacts influencing people in Southern Hemisphere regions. Things like this make the report more accessible to people from outside of the Northern Hemisphere. This sentence is also far too long and unclear. Please rephrase for clarity. Also incorporate new results from [Joelle Joelle Gergis, Australia]	Rejected. Comment seems out of place.
43273	83	19			Read "of the biases in storm" rather than "of the bias in storm" [Cyriaque Rufin Nguimalet, Central African Republic]	Noted. Text modified.
4981	83	20	83	20	typo in "genration" [Bart van den Hurk, Netherlands]	Noted. Text modified in the FGD.
74477	83	20	83	20	word "genertion" to correct in the expression like "… CMIP6 generation" [Moulay Driss HASNAOUI, Morocco]	Noted. Text modified.
12975	83	20			Change "genertion" to "generation" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text modified.
5587	83	20			Correct The CMIP6 genertion by … generation [Benoit Laignel, France]	Noted. Text modified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89111	83	27			The simulations presented in Willison et al (2015) are limited area simulations with boundary conditions from a pseudo-global warming configuration, where circulation is fixed and only surface temperature change to simulate the warming response. These types of simulations don't have an adequate representation of the hydrologic cycle, because they don't simulate global energy balance, and thus lack important feedbacks on precipitation change (Prein and Pendergrass 2019, Thackeray et al 2018). Interpretations of these simulations are not appropriate or sufficient evidence to make statements about sensitivity of precipitation to warming. Prein, A. F., & Pendergrass, A. G. (2019). Can we Constrain Uncertainty in Hydrologic Cycle Projections? Geophysical Research Letters, 2018GL081529. https://doi.org/10.1029/2018GL081529 [Angeline Pendergrass, United States of America]	Thanks, text changed and citation removed.
4983	83	29	83	30	most of the text before is about the inability to generate credible projections of changes in storm tracks, yet this phrase assumes these projections do show changes in storm tracks. Bring in balance with the evidence level [Bart van den Hurk, Netherlands]	Noted. Text modified for balance in the FGD.
113447	83	33	83	33	don't' [Diego Miralles, Belgium]	Thanks, fixed.
71007	83	35	83	37	Zappa et al. (2020 doi: 10.1073/pnas.1911015117) looks also at P-E changes in the Mediterranean, California and Chile, which are directly linked to storm track changes. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Thanks, text changed and citation added.
40849	83	39			Subsection: Atmospheric Rivers. But only one subsection in section 8.4.2.8 [TSU WGI, France]	Noted, now a stand-alone subsection.
12977	83	39			The nature of this heading type is not clear. See also earlier comment. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted, now a stand-alone subsection in FGD.
12979	83	43	83	47	This statement contains incorrect use of likelihood language. For the last part regarding the Iberian peninsula, since confidence is low then a likelihood statement must not be given. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted, thanks. Text modified in FGD.
29105	83	51			also Lavers et al. (2013) ERL doi:10.1088/1748-9326/8/3/034010 for NW Europe [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Thanks, reference added in FGD.
59169	83	52	83	52	Do you mean Espinoza et al., 2018a or 2018b? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Reference was to Espinoza, Waliser et al (2018), i.e. 2018b

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
74483	83	52	83	52	replace "one" by "on" [Moulay Driss HASNAOUI, Morocco]	Rejected. "One" is correct.
12981	83	53			This sentence seems to repeat the message of the first sentence in this paragraph; one or other could be removed to save space. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - Text removed.
74479	83	21 and	83	21 and	To check if it isn't published for Bracegirdle et al. Submitted [Moulay Driss HASNAOUI, Morocco]	Now published, citation and reference updated.
74481	83	21 and	83	21 and	To check if it isn't published for Harvey et al. Submitted [Moulay Driss HASNAOUI, Morocco]	Now published, citation and reference updated.
12983	84	4			West Coast does not need to be capitalized [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted, fixed.
74487	84	19	84	19	to add the word "precipitation" to the expression "The annual total" precipitation …. [Moulay Driss HASNAOUI, Morocco]	Noted, fixed.
79441	84	29	84	29	I thnik it would be useful to have a summary paragraph at the end as in 8.3.2.8 highlihgting the main results of storm tracks, ETCs and ARs [Alejandro Di Luca, Australia]	Done, thanks.
43275	84	34			Read "From Gershunov et al. (2019)." rather than "From (Gershunov et al., 2019)." [Cyriaque Rufin Nguimalet, Central African Republic]	This figure is dropped in the FGD.
12985	84	34			Reference not included properly in sentence. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	This figure is dropped in the FGD.
129241	84	36			"Real-5" isn't defined. [Trigg Talley, United States of America]	This figure is dropped in the FGD.
17297	84	49	85	46	Include new CMIP6 results on ENSO and IOD: Grose, M., Narsey S, Delage FP, Dowdy AJ, Bador M, Boschat G, Chung C, Kajtar JB, Rauniyar S, Freund MB, Lyu K, Rashid H, Zhang X, Wales S, Trenham C, Holbrook NJ, Cowan T, Alexander L, Arblaster JM and Power, S. (2020). Insights from CMIP6 for Australia's future climate. Earth and Space Science Open Archive: https://doi.org/10.1002/essoar.10501525.1. Overall I feel this section still needs work as these tropical moes are extremely important for rainfall variability in many regions. [Joelle Joelle Gergis, Australia]	Rejected. The suggested reference is not about projected changes in the teleconnections of ENSO and IOD. In the suggested paper the teleconnections are considered in the historical period, while the projections do not include any conclusions on changes in teleconnections

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12987	84	54	84	55	Is this statement correct, namely that an ENSO response is expected under weak forcing but not under strong forcing? It seems counterintuitive. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The statement has been modified consistently with the assessment provided in ch 4. In fact the amplitude of ENSO variability is not projected to robustly change in the 21st century
89113	84	54		55	One relevant paper underscores the large disagreement among models in projections of ENSO strength and variability with warming: Kohyama et al (2018). Kohyama, T., Hartmann, D. L., & Battisti, D. S. (2018). Weakening of Nonlinear ENSO Under Global Warming. Geophysical Research Letters, 45(16), 8557–8567. https://doi.org/10.1029/2018GL079085 [Angeline Pendergrass, United States of America]	Rejected. This section is not about projected changes for ENSO but implications on water cycle changes. The suggested reference is not needed for this section.
4985	84	55	84	55	"low" scenarios in terms of what? Emissions/temperature or mitigation effort? [Bart van den Hurk, Netherlands]	Not applicable. The sentence has been rewritten
22457	84	55	84	55	What do you mean by low and high scenarios? [Peter Thorne, Ireland]	Not applicable. The sentence has been rewritten
74485	84	2,3,25	84	2,3,25	IVT may be is to define here [Moulay Driss HASNAOUI, Morocco]	Acronym removed.
70303	85	1	85	2	This sentence could be interpreted as ENSOs teleconnected rainfall veriability everywhere, which is not entirely correct, see Perry et al. 2019 as cited in this poaragraph. [Shayne McGregor, Australia]	Taken into account. We have revised the sentence, consistently with the references included
53423	85	1	85	2	Even if El Niño events do not increase in intensity, the pervasive long-term warming trend means that the atmospheric drought impact of each strong El Niño will become more severe in many parts of the tropics which will experience enhanced VPD conditions with each new strong El Niño event (Rifai et al. 2019). Also quote Endris et al. 2018 about the enhanced teleconnection with East African rainfall? [Hervé Douville, France]	Taken into account. The suggested references have been included in the assessment
103725	85	1	85	11	A bit confusing section. Please make a clear distinction between: (1) The more-pronounced global rainfall/drought response from ENSO in a warmer world. This is thermodynamically driven (wet-get-wetter an dry-get-drier) and therefore has medium/high confidence; (2) Dynamical changes to the ENSO cycle itself. There is evidence that under GHG forcing there will be more extreme El Nino events (low/medium confidence). eg Cai et al NCC, 2015 [Philippe Tulkens, Belgium]	Taken into account. We have revised the assessment of ENSO teleconnections
22461	85	12	85	12	not supposed' is not particularly rigorous scientific language. Not projected to? [Peter Thorne, Ireland]	Not applicable. The summary statement has been totally rewritten
4987	85	17	85	21	Very complex sentence, don't understand fully what is stated here [Bart van den Hurk, Netherlands]	Accepted. The sentence has been rewritten and split to have clearer and simpler concepts highlighted
86427	85	17	85	23	IOD also has strong links to Indian summer monsoon rainfall. [Swapna Panickal, India]	Taken into account. We have included a short sentence and reference to the influence of IOD on monsoon rainfall
43277	85	18			Read "in boreal fall" rather than "in boreall fall" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. Typo has been corrected
12989	85	19			It is not clear what the strict link is from this sentence, which implies some form of correlation. Do good models project larger changes, for example? This sentence should be revised. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The point is that most models' tend to have a positive IOD-like mean state and this influences the teleconnections and the projections. We have clarified better this in the revised text

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29109	85	22			mainly? Is there anything else other than model uncertainty and variability for a given scenario? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Actually this comment refer to page 86 of the SOD, i.e. the introductory paragraph of section 8.5. It has been rewritten, and now the sources o uncertainties are explicitly listed
12991	85	25			Change "Bijerknes" to "Bjerknes" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Typo has been corrected
70305	85	43	85	44	Consider adding "Pacific" so this sentence reads "ENSO'S influence on Pacific precipitation will strengthen" [Shayne McGregor, Australia]	Accepted. The sentence has been changed as suggested
21081	85	44	85	44	Should say "… influence on TROPICAL precipitation...", right? [Marcelo Barreiro, Uruguay]	Taken into account. In fact we have modified the sentence specifying "precipitation over the Pacific Ocean".
70307	85	46	85	46	Consider replacing "precipitation projections in regions influenced by their teleconnections" with "projected precipitation teleconnection changes" [Shayne McGregor, Australia]	Accepted. We have modified the sentence as suggested
53425	85	46			near-term? [Hervé Douville, France]	Noted. Yes, it is in the near term. We have adjusted the text accordingly
4989	85	50	85	51	Also this sentence is confusing. If internal variability is large, differences between projections are generatlly more difficult to demonstrate [Bart van den Hurk, Netherlands]	Not applicable. The sentence has been removed as the assessment starts from the conclusions done in chapter 4
22459	85	50	85	54	The key findings from chapter 4 should perhaps be related here? Perhaps easiest to achieve by moving the start of next paragraph up? [Peter Thorne, Ireland]	Taken into account. The assessment here starts directly from the conclusions drawn in Chapter 4
12993	85	51			The grammar does not make sense. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The sentence has been removed
74489	85	52	85	52	to add an "n" to "souther" in the expression "the positive Southern Annular Mode (SAM)" [Moulay Driss HASNAOUI, Morocco]	Not applicable. The sentence has been removed
12995	85	52			Change "Souther" to "Southern" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The sentence has been removed
103727	86	1	86	12	line 1-10: Boreal summer modes (SNAO, circumglobal waves, storm tracks...) are not discussed at all but very important from an climate impact point of view. Discuss work by: Overland et al, GRL 2012; Wang et al, JGR, 2013/2015; Coumou et al, Nat Comm, 2018; Kornhuber et al, ERL, 2019; Lehmann et al, ERL, 2014; line 12: "teleconnections are not supposed to change in the future", weird wording [Philippe Tulkens, Belgium]	Rejected. The suggested references are not about projections of teleconnections, that is instead the topic of this section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17299	86	1	86	12	Include new CMIP6 results on Southern Annular Mode: Grose, M., Narsey S, Delage FP, Dowdy AJ, Bador M, Boschat G, Chung C, Kajtar JB, Rauniyar S, Freund MB, Lyu K, Rashid H, Zhang X, Wales S, Trenham C, Holbrook NJ, Cowan T, Alexander L, Arblaster JM and Power, S. (2020). Insights from CMIP6 for Australia's future climate. Earth and Space Science Open Archive: https://doi.org/10.1002/essoar.10501525.1. Also note there are several grammatical issues with this section. The summary needs work need to rephrase 'teleconnections are not supposed to change'. Do you mean are not projected to chage? [Joelle Joelle Gergis, Australia]	Rejected. The suggested reference is not about projected changes in the teleconnections of ENSO and IOD. In the suggested paper the teleconnections are considered in the historical period, while the projections do not include any conclusions on changes in the teleconnections
71011	86	2	86	3	This is not correct (and is not what Section 4.3.3.1 says), moreover it contradicts what is said immediately below on lines 8-10. The positive SAM trend is expected to weaken under all scenarios, but the SAM itself would only weaken under the weakest forcing scenarios. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence has been changed, correctly reporting the conclusions from chapter 4, consistently with the other sentence just below
65811	86	2	86	10	Suggest clarification. The text states that SAM is said to 'weaken under all scenarios because of stratospheric ozone recovery', and, at the same time 'over the Southern Hemisphere, a robust positive trend projected for the SAM'. [Kushla Munro, Australia]	Taken into account. The text has been changed. The statements from chapter 4 are reported correctly, and they are consistent with other sentences below in the text
12997	86	3			Does "increased boreal winter NAO" mean higher amplitude or frequency, or more persistence in a particular phase? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Both increased amplitude and increased frequency are important. The text has been modified accordingly
71013	86	8	86	10	The story is more complicated than this (Mindlin et al. 2020 doi: 10.1007/s00382-020-05234-1): even for a positive SAM change, the regions of drying and wetting depend on the storylines of circulation change in the SH, and exhibit a complex regional dependence. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text has been revised and the suggested reference has been included in the assessment
71473	86	8			Parts of your section 8.5 overlap with parts of Section 10.3 in Chapter 3. I suggest to discuss and resolve these issues (what should be merged, and into which Chapter, what should be kept separate?). These overlaps arise mainly in subsections 8.5.1.1, 8.5.1.2 and 8.5.2.1. [Douglas Maraun, Austria]	Taken into account. Section 8.5 has been revised and inconsistencies and overlaps with ch 10 have been adjusted and/or removed
113449	86	12	86	12	supossed' for 'expected' [Diego Miralles, Belgium]	Accepted. The term "supposed" has been changed with "expected"
69525	86	12	86	12	Not sure what "teleconections are not supposed to change" means. I would suggest being more specific along the lines of "CMIP5/6 models do not predict large changes in teleconnections…" [Martin Singh, Australia]	Taken into account. The sentence has been changed including the suggested text
129243	86	12	86	14	[CONFIDENCE] Recommend to consistently use "low confidence" to indicate that uncertainties are large. For instance, reword to "In summary, teleconnections are not supposed to change in the future but the uncertainties related to the projections of both tropical and extra-tropical modes may imply low confidence in the projected changes of hydrological properties associated to those teleconnections." [Trigg Talley, United States of America]	Taken into account. We have revised the sentence. We know that there are uncertainties in the projections of the main modes variability because of internal climate variability, and this definitely cascade into uncertainties in projections of related teleconnections
89115	86	12			Perhaps "expected" is better than "supposed" here - if something is "not supposed to change" that means it shouldn't change, I think this is a language issue. [Angeline Pendergrass, United States of America]	Accepted. The term "supposed" has been changed with "expected"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89117	86	13		14	Does it make sense to assign high confidence to a statement of large uncertainty? [Angeline Pendergrass, United States of America]	Taken into account. We have revised the sentence. We know that there are uncertainties in the projections of the main modes variability because of internal climate variability, and this definitely cascade into uncertainties in projections of related tele
66931	86	17	86	31	I think it would be helpful for the introduction of section 8.5 to have a clear statement of purpose / topic sentenc. [Mathew Barlow, United States of America]	Rejected - The title of the section is explicit about the purpose and does not need to be paraphrased for the sake of concision.
66933	86	17	86	31	At the end of the introduction paragraph for section 8.5, I think it would be useful to address how we can have so many condifent statements throughout the chapter despite all the limitations discussed in this section. This section has a trendomous amount of excellent material but (to me) does not yet feel fully connected with the rest of the chapter. [Mathew Barlow, United States of America]	Taken into account - While it is rather the focus of Section 8.4 to assess the confidence in climate change projections (and the focus of section 8.5 to explain why the confidence may remain limited), the introductory paragraph has been adjusted to explain the content, also in relation to section 8.4
22465	86	17			Such a long and detailed exposition of all the ills in the model world risks undermining considerably the assessment. Also, a reader may reasonably ask why pick on the models so? It is hardly as if the observations that are available are themselves without issue? I have concerns over the messaging aspect of having what is wrong with models quite so bluntly placed and fear that it undermines the substantive assessment that precedes it (both 8.3 because it calls into open question the attribution aspects and very specifically 8.4). It would be better to integrate it in but that comes back then to my fundamental point about chapter 8 structure being a considerable outlier in the structures adopted in SOD by the rest of the chapter 5-9 family of process-oriented chapters. Fundamentally this section should be directly informing the assessment statements being made in 8.4 and that requires it to be integrated with 8.4 but equally 8.3 should also inform 8.4 so I see value in collapsing at least 8.3, 8.4 and 8.5 together. As I have said before the chapter should be integrating observations, models, attribution, projections and theory. Pulling them apart is antithetical to the scoping given to the 5-9 chapter family. At the absolute minimum 8.5 needs to precede 8.4. [Peter Thorne, Ireland]	Rejected - The chapter structure has been agreed at LAM1 and has not been changed. The model fitness-for-purpose should be addressed in each chapter and is here assessed in Section 8.5.1 (in a process-oriented way) for the sake transparency and in agreement with many SOD review comments that suggested additional references to further support the key findings. A recent comparison between CMIP5 and CMIP6 projections (Lehner et al., 2020) has been quoted in the revised introduction and highlights that the contribution of model response uncertainty to the total spread in projections of annual precipitation has not been reduced from CMIP5 to CMIP6 (cf. Fig.8.23).
53427	86	19	86	21	also quote Fatichi et al. (2016) and Chegwidden et al. (2019)? [Hervé Douville, France]	Accepted - quoted in the revised introduction paragraph
129245	86	19	86	31	Need to emphasize inherent limits of water cycle predictability and stochastic processes in this section in conjunction with the discussion of unpredictable events. The point also needs to be made that it is critical for uncertainty to be recognized and embraced in a probabilistic framework within a risk management approach. [Trigg Talley, United States of America]	Taken account - Stochastic processes related to internal variability have been emphasized in Section 8.5.2.
17301	86	31	86	31	Amend sentence to read: '…assessment of the advantages and disadvantages of a more narrative, less probablistic approach'. [Joelle Joelle Gergis, Australia]	Not applicable - The sentence has been removed for the sake of brevity.
45265	86	34	93	17	Section 8.5.1 - Model uncertainties of relevance for the water cycle. While this is an important subsection, the text is too long and needs to be shortened. [Krishnan Raghavan, India]	Noted - The text was shortened substantially (30%) from the FOD to the SOD and many SOD review comments suggest in contrast to quote more studies that could further support the key findings. Given that model fitness-for-purpose is within the scope of our chapter and for the sake of comprehensiveness and transparency, we have been careful to address the multiple constructive review comments without further increasing the length, and in fact we have further reduced it by about 10%

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
20525	86	34	100	11	Certainly this very detailed overview is necessary, as it provides the way to refer to a large number of research articles addressing the water cycle in the climate. Undoubtedly is a difficult work. Incidentally, it is painful to read. Some information is missing because the observation system is not providing them and will never, in some cases, be able to provide them. On the other hand, some additional possibilities exist. One of them is physical simulation, which might be of help when addressing convection or cloud/aerosol interactions. Also, the problems encountered here are common to climate research and meteorology, in such a way that any progress in extending weather forecast performances (leaving aside those associated to initial conditions) give hope of transposition to climate. [philippe waldteufel, France]	Noted - Thanks for supporting the necessity of Section 8.5.1 and 8.5.3.2. The reviewer did not tell if the reading is painful because of the style or content. Both have been hopefully improved in the revised version.
29177	86	36	86	48	Mention the lack of global, robust, systematic production of key water variable observations to initialize / evaluate the models? For instance surface river runoff, ground water dynamicsc, water extent and dynmics. [Catherine Prigent, France]	Rejected - The focus here is on model response uncertainty rather than on model evaluation/initialization.
64341	86	36			I do not think "inferred" is the appropiate word here - please, rephrase [CRISTINA Prieto, Spain]	Accepted - "inferred from" has been replaced by "estimated as"
72295	86	36			I do not think "inferred" is the appropiate word here - please, rephrase. I also suggest to add more clarity and rigour to this entire paragraph [CRISTINA Prieto, Spain]	Accepted - "inferred from" has been replaced by "estimated as"
29111	86	40			"far from negligible" --> "significant" or "not negligible" or can this be quantified more [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - "far from negligible" has been replaced by "significant". The recent study by Lehner et al. (2020) based on both CMIP5and CMIP6 models has been quoted to further support this statement.
51463	86	43	86	43	GHMs are used here but differences in LSM and GHMs are described later on. It would be helpful to define both here. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - A GHM definition has been added in the IPCC Glossary
12999	86	43			GHMs is again defined on p90, p92 but should have been defined much earlier, on p38. The later definitions can be removed. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - A GHM definition has been added in the IPCC Glossary
13001	86	48			Change "cyle" to "cycle" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit completed
71475	86	51			The title of this section should be “Fitness...”, not “Fit”. You should also refer to the corresponding subsections in Chapter 1, and potentially also in Chapter 10 (with a regional focus though). [Douglas Maraun, Austria]	Accepted. The title of the section has been changed accordingly

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45267	86	51			Section 8.5.1.1 Fit-for-purpose and poorly constrained key processes. For a lay person this can also mean that if "key processes are poorly constrained" in state-of-the-art climate models, how can there be "high confidence" on projections of future water cycle changes. This sub-section may be suitably reworded. [Krishnan Raghavan, India]	Taken into account - Given that model fitness-for-purpose is within the scope of our chapter and for the sake of transparency, we have kept the whole subsection but the text (also in the introduction paragraphs) has been adjusted and reworded to avoid this misunderstanding
13003	87	6			Change "here illustrated" to "illustrated here" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The suggested change has been applied.
72061	87	12	87	49	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]	Rejected - This is rather a regional issue that could be discussed in Chapter 10.
95899	87	12	88	40	I am missing a discussion of the fact that convective parameterisations in current GCMs have generally no cloud microphysics, cannot respond to microphysical aerosol perturbations or show cloud microphysical feedbacks to global warming. Given that these effects are discussed in great length, it should be clear what can actually be represented in current GCMs. [Philip Philip Stier, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. A discussion on the limitations of convective parameterization and their implications has been added in the revised subsection.
71479	87	12			In this subsection you may refer to our Section 10.3.3.5, where we also discuss the representation of convection, but from a slightly different angle. One could consider merging some of the precipitation-related aspects into Chapter 10, in particular lines 23-41 and page 88, lines 6-15. They would nicely complement the material we have already. [Douglas Maraun, Austria]	Taken into account - No paragraph has been removed since we would like our chapter to be self sustained and the issue of parametrized convection is a global rather than a regional issue. Nonetheless, a link to Section 10.3.3.5 has been added at the end of the first subsection paragraph.
6733	87	16	87	16	Why is there a reference only to Arakawa and Schubert (1974)? Other papers have proposed convection schemes. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have reworded the text and we have removed the reference (for the sake of brevity it would not be possible to have a long list of references)
3721	87	17	87	18	I think you mean to cite Kendon 2019 which is study over Africa. You have cited a paper focused on th UK, which is not in the tropics. [Declan Finney, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - The reference has been corrected.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59083	87	18	87	21	Here references about the Double-ITCZ are missing. Please refer to Adam et al. 2017, 2018, “Regional and seasonal variations of the double-ITCZ bias in CMIP5 models”; “The relation of regional and seasonal variations of the double-ITCZ bias to the atmospheric energy budget in CMIP5 models”. We also performed double-ITCZ analysis in CMIP6 models. Please refer to the Fiedler et al., 2020 currently in review in Monthly Weather research. It should be published in next couple of weeks. Fiedler et al., 2020: Simulated tropical precipitation assessed across three major phases of the Coupled Model Intercomparison Project (CMIP). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - For the sake of brevity, only one of the suggested reference has been added in order to further make this point.
67565	87	18	87	21	Add the reference Tian and Dong (2020) 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]	Accepted. The suggested reference has been included
27261	87	22	87	23	Are the authors really talking about CMIP5 models? Or CMIP6 as the paper is submitted? [Eric Brun, France]	Not applicable - The sentence has been removed from the revised text (but the paper was based on CMIP5 models).
13005	87	22	87	23	Reference needs to be properly included in the sentence. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
43279	87	22		23	Read " investigated by Zilli and Carvalho (submitted)" rather than " investigated by (Zilli and Carvalho, 22 submitted)" [Cyriaque Rufin Nguimalet, Central African Republic]	Accepted
103729	87	23	87	24	This raises the question: if only 4 out of 17 have skill to represent observed precip number, what models should be included in the ensemble? I suggest to make a general statement about how this selection process is organized [Philippe Tulkens, Belgium]	Rejected - This is a general issue that is discussed in Box 4.1.
29113	87	24			"levels" --> "amounts" [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
13007	87	24			Change "water vapour levels" to "levels of water vapour" [would it be better to state levels of humidity?] [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted - in line with the previous comment, "levels" has been replaced by "amounts".
53429	87	27	87	29	You may add that the precipitable water simulated over the equatorial Indian Ocean, which is primarily controlled by the treatment of atmospheric convection, is a key metric for understanding model biases and differentiating model skill in simulating South Asian monsoon precipitation (Hagos et al., 2019). [Hervé Douville, France]	Accepted - Reference quoted in the revised paragraph.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18359	87	31	88	40	Please note that Chen et al. (2020) evaluate the CAPE and CIN in a global climate model using ERA-I and WRF 4km simulations and found that the NCAR CCSM4 realistically simulates the CAPE and CIN distributions. Chen and Dai (2019) found that the "drizzling bias" found in many climate models is partly related to their use of coarse resolution that not only directly leads to high frequency and low intensity but also too much convective precipitation and too little non-convective preciptiation, which has higher intensity than that of convective precipitation in the NCAR CESM1. These findings are highly relevant to discussions here. Papers cited: Chen, D. and A. Dai, 2019: Precipitation characteristics in the Community Atmosphere Model and their dependence on model physics and resolution. J. Adv. Model. Earth Syst., 11, 2352-2374. https://doi.org/10.1029/2018MS001536. Chen, J., A. Dai, Y. Zhang, and K. L. Rasmussen, 2020: Changes in the convective potential available energy and convective inhibition under global warming. J. Climate, 33, 2025–2050, https://doi.org/10.1175/JCLI-D-19-0461.1 [Aiguo Dai, United States of America]	Accepted - References quoted in the revised Section 8.5.1.
53431	87	35			"has been related to both horizontal resolution and convective schemes (Chen and Dai, 2018) and has led..."? [Hervé Douville, France]	Taken into account - Chen and Dai (2019 rather than 2018) quoted in the revised paragraph.
20527	87	51	87	51	This double title raises accordingly 2 difficulties. Why "Fit-for-purpose"? This term, explained elsewhere in the SOD, is understood as referring to numerical tools optimized for a specific end, at the expense of supplying poor performances in other domains of climate simulation. However, the processes contemplated here are hard to tackle in any kind of numerical simulation frame. Why "constrained"? Assuming physical processes are exactly known, they are represented by laws [philippe waldteufel, France]	Rejected - As introduced in Ch1, a key issue to be addressed in Ch8 as in the whole WG1 report is whether climate models are adequate or “fitted” for purposes of interest, "i.e. whether they can be used to successfully answer particular research questions, especially about the causes of recent climate change and the future evolution of climate". Moreover, Ch8 is supposed to be a process-oriented chapter so that a particular attention should be paid to "poorly constrained processes", either because they cannot be observed directly or should be parametrized in most global and regional climate models.
31493	87	51	87	52	Climate models evaluation is limited by observational uncertainties present in both in situ and satellite datasets (Collins et al., 2013a; Shige et al., 2017; Lin and Huybers, 2019; Singh et al., 2019a; Tapiador et al. 2019). Comment: Please, consider including a recent publication. It highlights the potential role of precipitation in the evaluation of climate models. Observational uncertainties are described in page 227. Reference:Tapiador, F.J., Roca, R., Dewitte, B., Petersen, W., Zhang, F., 2019. Is Precipitation a Good Metric for Model Performance? Bull. Amer. Meteor. Soc. 100, 223–233. https://doi.org/10.1175/BAMS-D-17-0218.1 [Andrés Navarro, Spain]	Rejected - This is a relevant general comment which does not fit well this paragraph which focuses on model deficiencies that can be diagnosed in spite of such observational uncertainties.
89119	88	6		7	Pendergrass (2020) reviews studies on the role of changing convective organization (including aggregation) on extreme precipitation change, and some studies discussed therein are potentially relevant here. Pendergrass, A. G. (2020). Changing Degree of Convective Organization as a Mechanism for Dynamic Changes in Extreme Precipitation. Current Climate Change Reports, 6(2), 47–54. https://doi.org/10.1007/s40641-020-00157-9 [Angeline Pendergrass, United States of America]	Accepted - Reference quoted in the revised paragraph.
59059	88	9	88	10	Please provide references [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - General statement that does not need specific references.
51467	88	11	88	12	Suggeted edit:"better understanding and parameterisation of mesoscale convective systems" [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59085	88	11	88	12	I think that a reference here is needed. Please refer to Global Cloud-Resolving Models by Masaki Satoh et al., 2019. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - More relevant to section 8.5.1.2.1 on high-resolution global climate models where the paper has been quoted.
13009	88	11			either change "for" to "to" or change the whole wording of "for better understand and parametrize" to "for better understanding and parametrization of" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
59061	88	12	88	15	Please improve these sentences. Machine learning algorithms have been significantly used in literature with CMIP products. Also, please mention the potential of deep learning in this regards, although there is limited literature in this case. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - The comment is not clear and the focus is here on atmospheric convection (not on deep learning).
13011	88	14			Insert "scheme" before "but", i.e., "with a ... parametrization ... scheme" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
67567	88	17	88	18	Based on the long-term annual mean tropical precipitation from two observations (GPCP and TRMM) and 75 CMIP3/5/6 models, Tian and Dong (2020) find that all three generations of CMIP models share similar systematic annual mean precipitation errors in the tropics. The double-ITCZ bias with a big inter-model spread evident in CMIP3 and CMIP5 models persists in CMIP6 models but it is slightly reduced in CMIP6 models in comparison to CMIP3 and CMIP5 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]	Accepted - The suggested reference has been quoted.
72059	88	17	88	19	This is an updated relevant reference here, but missing. Tian, B., & Dong, X. (2020). The Double ITCZ Bias in CMIP3, CMIP5, and CMIP6 Models Based on Annual Mean Precipitation. Geophysical Research Letters, 47(8), e2020GL087232. [Samanta Dhrubajyoti, Singapore]	Accepted - The suggested reference has been quoted.
59087	88	17	88	26	Also here, you would find useful the manuscript on precipitation bias across different generation of CMIPs by Fiedler et al., 2020: Simulated tropical precipitation assessed across three major phases of the Coupled Model Intercomparison Project (CMIP). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - The suggested reference has been quoted.
72277	88	17	88	40	A post-AR5 study on future summer East Asian monsoon (Ose et al., 2020, accepted) gives a good example indicating the importance of the simulation of the present-day Asia and Pacific precipitation, which is a key to determine the future summer East Asian monsoon wind and precipitation via the associated future weakened vertical monsoon circulations. This study is also an example that the clear difference in the simulated present-day precipitation among the models are not necessarily distinguished by a generally used numerical verification using the observed precpitation data. (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]	Rejected - This section focuses on atmospheric convection rather than on model precipitation biases.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27263	88	18	88	18	Is is not Figure 3.10 (3.9 shows vertical profiles of telmperature)? [Eric Brun, France]	Accepted - Thanks!
20529	88	18	88	20	It is indeed baffling that simulations hampered by such weaknesses are able however to produce so many impressive successes [philippe waldteufel, France]	Noted - Note that the fit-of-purpose of global climate models used in CMIP should not be just evaluated as their ability to capture the main features of the present-day water cycle, but mostly as their ability to converge on projected water cycle changes for a given emission scenario, which still remains a challenge for the current-generation CMIP6 models (cf. Figure 8.23).
89121	88	18		21	Two more recent students looking at the double ITCZ bias are Song, Fengfei, and Guang J. Zhang. "The Impacts of Horizontal Resolution on the Seasonally-Dependent Biases of the Northeastern Pacific ITCZ in Coupled Climate Models." Journal of Climate, November 7, 2019, JCLI-D-19-0399.1. https://doi.org/10.1175/JCLI-D-19-0399.1. Song, Xiaoliang, Guang J. Zhang, Xiaoliang Song, and Guang J. Zhang. "Culprit of the Eastern Pacific Double-ITCZ Bias in the NCAR CESM1.2." Journal of Climate 32, no. 19 (October 27, 2019): 6349–64. https://doi.org/10.1175/JCLI-D-18-0580.1. [Angeline Pendergrass, United States of America]	Rejected - For the sake of brevity, balance between different modelling centres and priority given to multi-model studies.
113451	88	21	88	21	Caribean' [Diego Miralles, Belgium]	Editorial - copyedit completed
59063	88	28	88	31	Please split the sentence, its too long to immediately pickup. Please also provide references. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - The sentence has been split but no reference has been added since this final paragraph aims at summarizing the previous assessment which is supported by references.
22467	88	28	88	40	While this is all undoubtedly true I also got a message from your text that CMIP6 models probably tend to be better than CMIP5 models. I think it would be important regardless to include an unambiguous statement about whether the balance of evidence suggests that CMIP6 is better / worse / about the same as CMIP5. This is important information for policymakers and should be included in a revised summary statement. [Peter Thorne, Ireland]	Rejected - Ch3 rather than Ch8 is the right place for such a general statement. The superiority of CMIP6 models (regarding the simulation present-day climate) is only partly supported by our assessment and may need to wait for more CMIP6 studies to be published. Moreover, "fitness for purpose" is not only about model performance in simulating present-day climate but also about how models represent relevant physical processes (cf. Section 1.5.4.8).
71481	88	43			There is minor overlap with Section 10.3.3.7, we might cross link the Chapters though. [Douglas Maraun, Austria]	Noted. The chapters have been cross-referenced where relevant
22469	88	44	88	46	This, as written utterly undermines all of the attribution statements you made in 8.3. This is why this section in my view desperately needs to be integrated with 8.3 and 8.4. At the moment vested interests could take your text in this section and very simply use it to discredit all that precedes it. The uncertainty assessment needs to be an integral part of the main body assessment and not an appendix to it. Readers will now be confused whether they should take any notice of your earlier assessment findings and rightly so. [Peter Thorne, Ireland]	Accepted. The first paragraph of 8.1.1.2 was deleted. It does not belong there anyway, because this subsection title is "Aerosol microphysical effects on clouds and precipitation".
85057	88	47	88	48	See Bollasina, M., Y. Ming, V Ramaswamy, M. D. Schwarzkopf, and V. Naik, 2014: Contribution of Local and Remote Anthropogenic Aerosols to the 20th century Weakening of the South Asian Monsoon. Geophysical Research Letters, 41(2), DOI:10.1002/2013GL058183 [venkatachalam ramaswamy, United States of America]	Taken into account. The first paragraph of 8.1.1.2 was deleted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7135	88	47	88	48	Some other studies may be of relevance here such as Guo et al. (2016) http://dx.doi.org/10.1175/JCLI-D-15-0728.1 Local and remote impacts of aerosol species on Indian summer monsoon rainfall in a GCM. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The first paragraph of 8.1.1.2 was deleted.
78731	88	52	88	52	Mentioning that the aerosol research itself is not yet settled might be valid, too. This sentence could be added at the end of the paragraph: "Research on reducing the uncertainty of aerosol effects on climate is ongoing (Reddington et al., 2017)." Reddington, C. L., K. S. Carslaw, P. Stier, N. Schutgens, H. Coe, D. Liu, J. D. Allan, J. Browse, K. J. Pringle, L. A. Lee, M. Yoshioka, J. S. Johnson, L. A. Regayre, D. V. Spracklen, G. W. Mann, A. Clarke, M. Hermann, S. Henning, H. Wex, T. B. Kristensen, R. Leaitch, U. Pöschl, D. Rose, J. Schmale, Y. Kondo, N. Oshima, J. P. Schwarz, A. Nenes, G. C. Roberts, J. R. Snider, C. Leck, and P. K. Quinn (2017), The Global Aerosol Synthesis And Science Project (GASSP): Measurements and modelling to reduce uncertainty, BAMS, 98(9), doi:10.1175/BAMS-D-15-00317.1. [Heike Wex, Germany]	Taken into account. The first paragraph of 8.1.1.2 was deleted.
13013	88	52			Accent has not been included correctly in "Boe" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The first paragraph of 8.1.1.2 was deleted.
22471	88	54	89	12	This was assessed in depth by chapter 7. This text simply invites a reader to play spot-the-difference and should be removed. The chapter charge does not include radiative effects. [Peter Thorne, Ireland]	Accepted. The text was shortened and framed in the context of aerosol effects on precipitation from shallow clouds.
116765	88		88		coordination with ch 6 and 7 is needed here (aerosol cloud precipitation effects) [Valerie Masson-Delmotte, France]	Noted - A coordination on aerosol cloud precipitation effects with ch 6 and 7 has been implemented, with consistency of the assessment and cross-referencing where needed.
116767	88		90		Building on SRCCL chapter 2 could be useful here too. What about a table to identify major sources of uncertainties, progress since AR5, and possibly the relative importance of these sources of uncertainty (for instance related to land feedbacks). [Valerie Masson-Delmotte, France]	Taken into account - A reference to SRCCL Ch2 has been added at the beginning of Section 8.5.1.1.3.
116769	88		90		Check carefully the use of the confidence language here. The section could be shortned and sharpened. Is there anything for which the deep uncertainty approach is relevant? [Valerie Masson-Delmotte, France]	Taken into account - The confidence language has been checked. The section has however not been further shortened given the multiple suggestions to include additional references that further support the key findings. Fitness-for-purpose is within the scope of Ch8 and should be therefore assessed in a comprehensive and transparent way (in line with the general IPCC assessment criterion). Deep uncertainty is a concept that has been questioned by several reviewers and that may be more relevant to Section 8.6.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
95895	89	5	89	12	This paragraph assumes that AOD is a good proxy for CCN, which is not always true, as nicely discussed in chapter 6 - harmonisation would help. [Philip Philip Stier, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The containing paragraph was deleted, and its content is now referred to {7.3.3.2}.
103731	89	6	89	7	don't understad "lack of observational constraints': [Philippe Tulkens, Belgium]	Taken into account. The containing paragraph was deleted, and its content is now referred to {7.3.3.2}.
64991	89	8	89	12	Wouldn't it be better to discuss the shortcomings in aerosol – precipitation effects and leave the forcing to Chapter 7? [Johannes Quaas, Germany]	Accepted. The containing paragraph was deleted, and its content is now referred to {7.3.3.2}.
95897	89	14	89	26	As mentioned before in more detail the discussion of aerosol on deep convection and convective invigoration as well as proposed satellite methodologies in this chapter is too selective and not a balanced representation of the current scientific consent. It does heavily focus on the work of a small group of authors and does not take into account the considerable scientific discussions and lack of consensus on this topic (see above for specific comments that equally apply here). [Philip Philip Stier, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The paragraph was edited for better clarity. As responded to the previous comments, the paper of Grabowski (2020) which disputes the aerosol convective invigoration, is now shown to be erroneous in many ways (Fan and Khain, 2020, submitted), and therefore should be ignored. The main focus here is in showing the lack of confidence in the model simulations on the aerosol effects in deep convective clouds. So there is no implication that one group of studies is preferred, as hinted in the comment. It is now emphasized that satellite limitations are important especially at aerosol concentrations that are below the optical detection limit which undermine the ability to quantify the potentially very large aerosol effects in very clean situation (Ma et al., 2018). This is where inferring CCN from satellite retrieved drop concentrations and updrafts become the only way known to us to overcome it.
9881	89	17	89	19	I edited the content here to improve the accuracy, i.e., "These aerosol effects on deep convective clouds are not accounted in global climate models because (1) the convective parameterizations generally do not consider aerosol effects, (2) cloud parameterizations are too crude to properly represent responses of cloud microphysical processes to aerosols (Fan et al., 2016). Specifically, the potential major role of ultrafine aerosol particles in enlarging deep convective clouds and increasing precipitation (Fan et al., 2018) is not accounted at all since climate models do not resolve the high supersaturation in deep convective clouds for activating those numerous small aerosol particles." Reference, Fan, J., Y. Wang, D. Rosenfeld, X. Liu (2016), Review of Aerosol-Cloud Interactions: Mechanisms, Significance and Challenges, J. Atmos. Sci., 73, 4221-4252. http://journals.ametsoc.org/doi/abs/10.1175/JAS-D-16-0037.1 [Jiwen Fan, United States of America]	Taken into account. A pertinent discussion was added.
59183	89	22	89	26	The retrieval method was implemented in Rosenfeld et al., (2019) to demonstrate the influence of CCN on shallow cloud cover and water path. It could be included as a reference. Rosenfeld, D., Zhu, Y., Wang, M., Zheng, Y., Goren, T., & Yu, S. (2019). Aerosol-driven droplet concentrations dominate coverage and water of oceanic low-level clouds. Science, 363(6427). doi:10.1126/science.aav0566 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. While we agree with the comment, it is relevant mostly to cloud radiative forcing, while here we focus on effects on changes in the hydrological cycle.
59065	89	24	88	26	Please provide references [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Two references were added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
85059	89	28	89	33	It might be useful to distinguish the absorbing aerosol (mainly BC) effects and its uncertainties from that arising due to sulfate and its uncertainties. The uncertainty with regards to sulfate ari is much less than that for BC. However, the aci effect which in several models is largely driven by sulfate-cloud interactions, is a more important uncertainty for this aerosol type. [venkatachalam ramaswamy, United States of America]	Taken into account. The relevant sentence was deleted, because this subsection deals with aerosol microphysical effects.
22473	89	28	89	33	Again, where is a statement that tells us whether CMIP6 shows any improvement or otherwise over CMIP5 which is what policymakers will want to know? [Peter Thorne, Ireland]	Rejected - Policymakers want to know more about future climate than about model performance at simulating present-day climate and it may be too early to make such a statement given the limited number of available detailed studies based on CMIP6 models and the increasing use of tuning methods that may improve the simulation of present-day climate without increasing the reliability of the projected climate response. The wider range of TCR and ECS simulated by CMIP6 models despite their slight improvement regarding the simulation of present-day temperature (compared to CMIP5) illustrates this problem.
71483	89	36			There is some overlap with Section 10.3.3.7. Moving parts and cross linking might be useful. [Douglas Maraun, Austria]	Taken into account - Section 10.3.3.7.2 focus on land management which is hardly discussed in Section 8.5.1.1.3. A link to Ch10 has been however introduced at the end of the first paragraph in this section.
113453	89	37	89	37	Also longwave emission and ground heat flux, and net carbon exchange. [Diego Miralles, Belgium]	Taken into account - The sentence has been completed.
113455	89	37	89	37	Please cite also: Berg, A., and Shefield, J. Evapotranspiration Partitioning in CMIP5 Models: Uncertainties and Future Projections, journals.ametsoc.org, doi:10.1175/JCLI-D-18-0583.s1 [Diego Miralles, Belgium]	Rejected - Not relevant in this general sentence.
81179	89	39	89	46	We also evaluated the permafrost (soil freezing and thawing) processes in a state-of-the-art land surface model (a component of global climate model, MIROC). We found that the detailed descrription of thermal properties in soils (i.e., changes in heat conductivities of frozen soils, porous organic layers in Tundra regions) improved the model performance of permafrost distribution (active layer depth). The paper is under review as follows. Yokohata T, Saito K, Takata K, Nitta T, Sato Y, Hajima T, Sueyoshi T, Iwahana G (2020) Model improvement and future projection of permafrost processes in a global climate model, Submitted to the same issue of Progress in Earth and Planetary Science [Tokuta Yokohata, Japan]	Rejected - For the sake of brevity, this reference has not been quoted and may be more suitable in Ch9 on cryospheric processes.
70355	89	45	89	46	This statement misses an important consequence of Lian et al.'s finding that CMIP5 models systematically underestimate the ratio of plant transpiration (T) to total evapotranspiration (ET). If modeled T/ET is too low, models are underestimating the impact of plant functioning on the hydrologic cycle, i.e. plants have an even greater impact on future hydrologic changes than models suggest. [Abigail Swann, United States of America]	Taken into account - The sentence has been completed. Note however that other misrepresented processes (such as plant mortality) may in contrast lead to an overestimation of plant functioning (including WUE) on the water cycle.
27265	90	2	90	3	That is correct but what about precipitation which is the focus of this chapter? [Eric Brun, France]	Rejected - The focus of this chapter is the whole water cycle, not just precipitation.
3679	90	3	90	37	please change "CO2" into "CO2". [Jiafu Mao, United States of America]	Editorial - copyedit completed
51469	90	6	90	19	Suggest including Prudhomme et al 2014 (https://doi.org/10.1073/pnas.1222473110) and Vetter et al (2017) to weigh up the relative contributions to uncertainty as well as the differences between different GHM and LSM modelling approaches. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - A reference to Prudhomme et al. (2014) has been added in the revised paragraph.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13015	90	6			This is again defined on p92 but should have been defined much earlier, on p38. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Definitions and acronyms have been adjusted
69527	90	17	90	18	This report is an evaluation of the literature; I think the claim that online land-surface models are required needs to evaluated, and the evidence for it presented. [Martin Singh, Australia]	Rejected - This sentence says "it was claimed" rather than "we claim" and does not use the IPCC confidence language so that a single reference is enough to support the statement for the sake of comprehensiveness and given the potential implications for impact studies assessed in WG2.
51959	90	20	90	20	Insert new paragraph here (or end of last paragraph on line 19, page 90): "Groundwater is main storage component of the freshwater hydrologic cycle but remains inadequately represented in LSMs, ESMs, and Global Hydrological Models. This omission constrains current understanding of the interactions between groundwater and the rest of the hydrologic cycle, as well as past, present and future climates. Groundwater systems can have far-reaching effects on climate affecting modulation of surface energy and water partitioning with a long-term memory (Maxwell et al., 2016; Meixner et al., 2018; Wang et al., 2018; Keune et al., 2018). Despite significant advances in understanding the role of lateral groundwater flow on evapotranspiration (Maxwell & Condon, 2016; Bresciani et al, 2016), the interactions among irrigation, groundwater, and climate (Condon and Maxwell, 2019) remain largely unresolved. " [Richard Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - Thanks for this relevant paragraph which could not be introduced entirely for the sake of brevity. One sentence has been added and a few additional references have been quoted in the revised Section 8.5.1.1.3.
90559	90	21	90	22	For the increasing recognition of the need to better understand the role of land atmosphere coupling in the observations, you may consider the paper by Catalano et al (2016). This observational analysis revealed a strong rainfall coupling with soil moisture (up to 40% locally and about 20% globally), which is modulated by vegetation and evapotranspiration response. The sign and intensity of the feedback varies between wet, transition and semi-arid climates. Citation: Catalano, F., Alessandri, A., De Felice, M., Zhu, Z., and Myneni, R. B.: Observationally based analysis of land–atmosphere coupling, Earth Syst. Dynam., 7, 251–266, https://doi.org/10.5194/esd-7-251-2016, 2016. [Andrea Alessandri, Italy]	Accepted - The suggested reference has been quoted.
43777	90	22	90	22	I suggest appending the following reference after Santanello et al., 2018: Navarro et al., 2019. The reference is: Navarro, A.; Moreno, R.; Jimenez-Alcazar, A.; Tapiador, F. J. 2019. Coupling population dynamics with earth system models: the POPEM model. ENVIRONMENTAL SCIENCE AND POLLUTION RESEARCH. 26,2019. ISSN 0944-1344. DOI: 10.1007/s11356-017-0127-7 [Francisco Tapiador, Spain]	Rejected - More relevant to Ch5
113457	90	25	90	25	There are many examples of studies presenting tailored diagnostics of land–atmoshpheric coupling (I have a couple), but Humphrey et al. (2017) is not really dedicated to this. [Diego Miralles, Belgium]	Taken into account - Humphrey et al. (2017) has been replaced by Miralles et al. (2016, 2019)
93655	90	27	90	27	Add reference: Peano et al., 2019. ref. Peano, D., Materia, S., Collalti, A., Alessandri, A., Anav, A., Bombelli, A., & Gualdi, S. (2019). Global variability of simulated and observed vegetation growing season. Journal of Geophysical Research: Biogeosciences, 124(11), 3569-3587. [Stefano Materia, Italy]	Rejected - For the sake of brevity and since this interesting study is however not crucial for our assessment.
59067	90	32	90	32	2 should be in subscripts in CO2. Here and elsewhere as mentioned. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit completed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
113459	90	33	90	33	Please cite: De Kauwe, M. G., Kala, J., Lin, Y. S., Pitman, A. J., Medlyn, B. E., Duursma, R. A., Abramowitz, G., Wang, Y. P. and Miralles, D. G.: A test of an optimal stomatal conductance scheme within the CABLE land surface model, Geosci. Model Dev., 8(2), 431–452, doi:10.5194/gmd-8-431-2015, 2015. [Diego Miralles, Belgium]	Rejected - For the sake of brevity and since this specific study is however not crucial for our assessment.
70357	90	34	90	37	We agree that there is a lack of structural diversity in how models represent stomatal conductance. This section should also acknowledge that the lack of structural diversity in models means that models may not fully account for uncertainty in future ET changes. [Abigail Swann, United States of America]	Accepted. The sentence has been reworded accordingly
14853	90	39	90	40	The connection between land-use-land-cover changes, cimate and hydrology over the last 6000 years helped to reduce the uncertainties [Marie-France Loutre, Switzerland]	Rejected - Such a statement would need to be supported by concrete examples.
27267	90	39	90	40	Even if we agree with this sentence, land surface processes are not a major source of uncertainty in all models, for all hydrological variables. This section needs to be more accurate and balanced. Many of the processes discussed above have been introduced only in global climate models, not in impact (hydrological) models. When they are included in GCMs there is a need to check whether they improve the hydrological cycle simulated in those models, and this is not discussed here. When they are implemented in e.g. hydrological models, then their usefulness is tested against other hydrological variables. This is not clearly discussed. [Eric Brun, France]	Rejected - We did not write "in all models and for all hydrological variables" (although we would be curious to know which terrestrial hydrological variables are not affected by such uncertainties and which model you would recommend to use). Moreover, ignoring poorly constrained processes does not guarantee better projections although we agree that introducing them prematurely should be avoided. Nonetheless, the statement has been tempered to suggest that it does not hold for all regions (e.g., desert areas may be less sensitive to land surface processes although land surface albedo and longwave emissivity may still contribute to uncertainties in such regions, e.g. Terray et al. 2018).
51471	90	41	90	42	"caution is needed in the direct use of climate model outputs for water applications" - suggest caution is needed in the direct use of climate model outputs for any real-world application. Also, the latter part of the sentence regarding emergent constraints does not appear consistent with Lehner et al (2019), which argues that if runoff sensitivities are constrained using observational data, biases in climate models can be improved. Please check the use of this reference here. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - This statement is highly policy-relevant since many hydrological impact studies still underestimate the climate model deficiencies and the inter-model spread in the needed inputs of their impact models. Please also note that our understanding is that Lehner et al. (2019) argue that "constraining CMIP5 model runoff sensitivities with observed estimates could reduce uncertainty in runoff projection over the western United States by up to 50%" and "urge caution in the direct use of climate model runoff for applications". The sentence has been however shortened in the revised section for the sake of brevity and clarity.
22475	90	46	90	46	Replace new-generation with CMIP6 for transparency here [Peter Thorne, Ireland]	Rejected - ESMs also represented a significant fraction of CMIP5 models which are still widely used in the AR6.
27269	90	47	90	48	This is a crucial point. There are very important conclusions in this last paragraph and they are all present in the executive summary while they should: 1) improving land surface processes is crucial in climate models, even if this does not improve significantly the hydrological cycle ..... 2) improving land surface processes is also important in impact models 3) Having a perfect impact model does not guarantee a reliable impact if the driving climate model is biased What could be interesting is to point to aeras where climate models do a reasonable job with respect to the hydrological cycle, and where they are far from being OK. [Eric Brun, France]	Noted - The summary statement has been revised as: "In summary, since AR5 substantial advances have been made in the representation of land surface processes in current-generation Earth System Models (ESMs). Off-line hydrological models allow the application of bias-adjusted atmospheric forcings, but there is low confidence of an improved response compared to coupled climate models, given their inherent limitations (Box 10.2). While improvements in the representation of complex land surface feedbacks relevant to the water cycle are needed, there is currently low confidence that they will substantially improve of water cycle projections."

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29115	90	47			presumably the possibility of highly amplifying or non-linear responses cannot be excluded and the plausibility of these pathways are policy relevant [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted - This issue is rather the topic of Section 8.5.3.2
27271	90	51	90	51	Why is the added value of resolution only discussed for downscaling issues and not in the view of improving the hydrological cycle in global climate models? [Eric Brun, France]	Rejected - Section 8.5.1.2.1 is not about dynamical downscaling but about high-resolution global climate models.
71485	90	51			Here we have quite some overlap. We might discuss moving parts of 8.5.1.2.1 and 8.5.1.2.2 into 10.3.3 [Douglas Maraun, Austria]	Noted - Ch8 aims at being a stand-alone and self-sustained chapter and has already linked to Section 10.3.3 at the beginning of section 8.5.1.2 (cf. first paragraph).
59069	90	55	91	2	Please provide some references. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - The reader is here invited to read Section 10.3.3 for more details and this is just an introductory paragraph rather than the core of our assessment which is provided in the subsequent paragraphs.
112237	90				Consider adding socioeconomic changes that can lead to land-use change. Population increase, changing diets, bio-fuel etc. [Rutger Hofste, Netherlands]	Rejected - Rather the focus of WG3.
27273	91	11	91	11	This could be combined with the sub-section above [Eric Brun, France]	Rejected - This is the first part of subsection 8.5.1.2.
21083	91	12	91	12	"… number OF studies..." [Marcelo Barreiro, Uruguay]	Editorial - copyedit completed
59071	91	12	91	12	"Since the AR5, a growing number studies …" should be "Since the AR5, a growing number of studies …" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit completed
59073	91	12	91	18	Even though benefits of downscaling are mentioned, no methods are referred from recent literature. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - Rather the focus of Ch10.
72063	91	12	91	31	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]	Rejected - Rather the focus of Ch10.
29117	91	21			Improvements in the diurnal cycle of convection in higher resolution is actually the result of suppressing the convective parametrization rather than the resolution per se e.g. Pearson et al. (2014) QJRMS https://doi.org/10.1002/qj.2145 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - This sentence does not refer to the diurnal cycle but to daily precipitation intensities including extremes.

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31495	91	28	91	31	Variable resolution based on grid stretching may also represent a valuable alternative to a uniform increase in horizontal resolution for the simulation of regional phenomena like regional monsoons (Sabin et al, 2013; Krishnan et al., 2016) or tropical cyclones over specific ocean basins (Harris et al., 2016; Chauvin et al., 2017). This practice may avoid inconsistencies in the physics that are found in RCMs driven by GCMs (Tapiador et al., 2020). Comment: A potential benefit of variable resolution models is that the same physics is applied across all scales, making the modeling consistent. Reference: Tapiador, F.J., Navarro, A., Moreno, R., Sánchez, J.L., García-Ortega, E., 2020. Regional climate models: 30 years of dynamical downscaling. Atmospheric Research 235, 104785. https://doi.org/10.1016/j.atmosres.2019.104785 [Andrés Navarro, Spain]	Accepted - The additional sentence and reference to Tapiador et al. (2020) has been added in the revised subsection.
54363	91	34	91	50	Caldwell et al 2019 would be an appropriate reference here adding further support to increased precip [Luke Van Roekel, United States of America]	Accepted - The reference has been even quoted twice given the thorough and balanced assessment of the influence of horizontal resolution in the DOE model within HighResMIP.
103733	91	35	91	38	Be a bit more precise: these trends are also observed in the lower resolution modeling simulations, so what other trends can be derived from the high res simulations? [Philippe Tulkens, Belgium]	Taken into account - We are not here assessing climate change but just the influence of horizontal resolution on simulated present-day climate (and model biases) which has been clarified in the revised sentence.
13017	91	37	91	38	Is "in coupled mode" meant to say "in coupled models"? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Clarified in the revised sentence.
66927	91	41			At least for some regions, CMIP6 models do not show a clear improvement over CMIP5, and vary considerably in their ability to reproduce the observed regional circulations associated with extreme precipitaton events at daily timescales (Agel et al., in review, a, b). 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. [Mathew Barlow, United States of America]	Rejected - While we agree with this finding, our assessment has been already considered as too negative and too long by several reviewers so that we avoid to add new references that question the added values of CMIP6 versus CMIP5 models unless they provide key new findings
13019	91	42			Change "would be however necessary" to "however would be necessary" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit completed
80615	91	44	91	44	This should refer to Figure 3.10, not 3.9. [Malcolm J. Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Thanks!
129247	91	49			Na et al. (2020) should be cited here: Na, Y., Q. Fu, and C. Kodama, 2020: Precipitation Probability and Its Future Changes From a Global Cloud-Resolving Model and CMIP6 Simulations. J. Geophys. Res. Atmos., 125, 5, doi:10.1029/2019JD031926. [Trigg Talley, United States of America]	Accepted - This original and highly relevant study has been quoted and discussed in the revised subsection.

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13021	92	2	93	3	Why would the benefits be easier to detect offline? It is not clear why this is so. Are offline LSMs routinely run at higher resolution than when coupled to atmospheres? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - Because of the absence of internal variability since the atmospheric forcings are prescribed. This has been clarified.
108103	92	4	92	4	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]	Accepted
4993	92	7	92	7	typo: insert "of" after "lack" [Bart van den Hurk, Netherlands]	Editorial - copyedit completed
59075	92	7	92	7	"….and the lack coupling with…" should be "….and the lack of coupling with…" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit completed
113461	92	10	92	10	Remove 'potentially' [Diego Miralles, Belgium]	Accepted - although the substantial additional cost relative to the limited added value remains an open question.
20531	92	12	92	15	One needs to be careful when formulating firm conclusions. Do the authors of chapter 8 believe that we suffer limitations in the UNDERSTANDING of small scale processes? While they may certainly be right, section 8.5.1.1 does not present a single concrete example of a process imperfectly understood. [philippe waldteufel, France]	Taken into account - While Section 8.5.1.1 does suggest that our understanding of atmospheric convection and its coupling with atmospheric dynamics (e.g., no consensus on a general MJO theory) or of cloud-aerosol interactions is not complete, we recognize that this statement may be too strong. It was tempered as follows in the revised summary of the subsection: "Nonetheless, there is low confidence that higher resolution is currently sufficient to guarantee an improved simulation of large-scale and seasonal features of the water cycle, although a more realistic representation of topography and land cover can be helpful".
64343	92	15	92	16	model performance and selection of dominant mechanisms - this links to model realism [CRISTINA Prieto, Spain]	Noted
20155	92	19	92	25	Pity this figure was not available in SOD [philippe waldteufel, France]	Unfortunately this figure could not be included in the FGD.
113463	92	28	92	28	ModelS' [Diego Miralles, Belgium]	Editorial - copyedit completed
27275	92	28	92	28	Most Regional Climate Models poorly represent land surface processes while those are the models where LSP absolutely need to be good! Most RCMs do not include land use changes .... which we now know can lead to biases in regional trajectories of climate change. It is a pity not to discuss this in this section. [Eric Brun, France]	Taken into account - "potential" has just been added in the revised sentence to suggest the limits of current RCMs (since you did not suggest specific references).
17303	92	28	92	28	Are these CORDEX papers of relevance to the discussion here? These are new papers for Australia that might improve geographical biases towards Northern Hemisphere locations/studies: Di Virgilio G; Evans JP; Di Luca A; Grose MR; Round V; Thatcher M, 2020, 'Realised added value in dynamical downscaling of Australian climate change', Climate Dynamics, http://dx.doi.org/10.1007/s00382-020-05250-1 Di Virgilio G; Evans JP; Di Luca A; Olson R; Argüeso D; Kala J; Andrys J; Hoffmann P; Katzfey JJ; Rockel B, 2019, 'Evaluating reanalysis-driven CORDEX regional climate models over Australia: model performance and errors', Climate Dynamics, vol. 53, pp. 2985 - 3005, http://dx.doi.org/10.1007/s00382-019-04672-w [Joelle Joelle Gergis, Australia]	Taken into account - The reader is referred to Section 10.3.3.3.2 for a detailed assessment of the RCM performance at simulating present-day climate. Nonetheless, the first study has been quoted to suggest the added value of RCMs (compared to lower resolution GCMs) for simulating the precipitation response over regions with a complex orography.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1289	92	28	93	17	There is more to downscaling than regional climate models (RCMs). Empirical-statistical downscaling (ESD) makes use of information from observations and statistical theory as opposed to coded equations of physics, and because it is so computationally efficient, enables the downscaling of large multi-model ensembles. ESD and RCMs complement each other, and discussing just one is like only talking about one type of tool or instrument when there are several in the toolkit. There appears to be some mislpaced prejudice against ESD. I don't see how this paragraph adds any relevant information here since the use of a small number of RCM simulations doesn't give any robust answers due to pronounced internal variability on regional scales (Deser et al., 2012) and the "law of small numbers". I suggest leaving this discussion for chapter 10. [Rasmus Benestad, Norway]	Rejected - Ch8 is a process-oriented chapter so that the focus is on physically-based models rather than on downscaling techniques which are the focus of Ch10. The question addressed in Section 8.5.1.2 is clearly about the added value of increased model resolution, not about downscaling. Just like a few global CRMs cannot be used to draw robust conclusions but may be used to highlight consistencies/discrepancies with GCM and RCM hydrological responses, RCMs are also useful tools that need to be assessed with a water cycle perspective and belong to the multiple lines of evidence that can be used to build our assessment of water cycle changes.
23639	92	38	92	41	I recommend citing a recent study showing the important role of the spatial resolution on added value in precipitation over the 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
43779	92	43	92	43	I suggest appending the following sentence and reference: "A comparison of RCM precipitation estimates with high resolution GCMs and ESMs estimates can be found in Tapiador et al. 2019". The reference is: Tapiador, F.J., R Moreno, A Navarro, JL Sánchez, and E García-Ortega, 2019. Climate Classifications from Regional and Global Climate Models: Performances for Present Climate Estimates and Expected Changes in the Future at High Spatial Resolution. Atmospheric Research. Volume 228, 107-121, DOI: 10.1016/j.atmosres.2019.05.022 [Francisco Tapiador, Spain]	Accepted - This relevant reference has been quoted as follows at the end of the RCM paragraph: "Nonetheless, a systematic comparison between climate classifications derived from model outputs suggests that GCMs have bridged the gap that led to the emergence of RCMs thirty years ago, although the modelling of precipitation remains the “Achilles' heel” of both GCMs and RCMs and should be considered with extreme caution when informing regional climate policies (Tapiador et al., 2019)."
20533	92	45	92	46	Has there been attempts to use models with a variable grid size? Such a model (ARPEGE) is run by the French weather service for forecasting purposes; according to their Internet page, a "climate" version exists [philippe waldteufel, France]	Noted - Variable resolution global climate models are briefly discussed in subsection 8.5.1.2.1, including a French application for tropical Atlantic cyclones (Chauvin et al. 2017).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
88929	92	50	92	53	It would be good to have more balance in these reference lists with tropical applications. There are a number of new studies using a CP model over Africa for example (sorry, a bit parochial, I know, but I think this kind of regional balance is important): Finney, D.L., Marsham, J.H., Rowell, D.P., Kendon, E.J., Tucker, S.O., Stratton, R.A. and Jackson, L.S., 2020: Effects of explicit convection on future projections of mesoscale circulations, rainfall and rainfall extremes over Eastern Africa. J. Climate, 33, 2701-2718 Fitzpatrick, R.G.J., Parker, D.J., Marsham, J.H., Rowell, D.P., Guichard, F.M., Taylor, C.M., Cook, K.H., Vizy, E.K., Jackson, L.S., Finney, D., Crook, J., Stratton, R. and Tucker, S., 2020: What drives the intensification of mesoscale convective systems over the West African Sahel under Climate Change? J. Climate, 33, 3151-3172 Berthou, S., Kendon, E.J., Roberts, M., Rowell, D.P., Tucker, S. and Stratton, R., 2019: Larger future intensification of rainfall in West Africa in a convection-permitting model. Geophys. Res. Lett., 46, 13299-13307 Berthou, S., Rowell D. P., Kendon E.J., Roberts. M.J, Stratton R., Crook J. and Wilcox C., 2019: Improved climatological precipitation characteristics over West Africa at convection-permitting scales. Climate Dynamics, 53, 1991–2011 Finney, D.L., Marsham, J.H., Jackson, L.S., Kendon, E.J., Rowell, D.P., Boorman, P.M., Keane, R.J., Stratton, R.A. and Senior, C.A., 2019: Implications of improved representation of convection for the East Africa water budget using a convection-permitting model. Journal of Climate, 32, 2109-2129 [Dave Rowell, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - Two of the suggested recent references have been quoted for the sake of geographical balance.
4995	92	51	92	53	A reference to Atlas.5.6.3 might be considered (Evaluation of projections and models for Europe) [Bart van den Hurk, Netherlands]	Accepted
109809	92	52	92	52	could cite new paper by Chan et al. here - Europe wide convection permitting simulations added value - Chan, S.C., Kendon, E.J., Berthou, S., Fosser, G., Lewis, E., Fowler, H.J. Europe-wide climate change projections at convection-permitting scale with the Unified Model. Climate Dynamics, in press. You could also cite any previous refs by the same author back to 2013. [Hayley Fowler, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - For the sake of diversity, we cannot cite all studies from the same modelling group.
3719	92	54	92	54	Not in terms of percentage changes, and this is important when considering at percentage changes in rain rates. See figure 2 of Finney et al. (2020) 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)]	Taken into account - Your paper has been quoted in the next sentence which highlights the overall stronger response in CRMs.
53433	93	3			also quote Chen et al. (2020)? [Hervé Douville, France]	Accepted
4997	93	5	93	5	What is meant with "such a mechanism"? [Bart van den Hurk, Netherlands]	Rejected - The sentence clearly refers to the increase in both CAPE and CIN.
13023	93	11	93	17	Section 10.3.3.5.1 within Chapter 10 could be cross-referenced here. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13025	93	16			This would be better worded as "larger differences between wet and dry extremes". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
13027	93	20	95	22	The following references, taking into account the effects of internal/decadal variability on historical and future (near/mid-term) projections of the Asian monsoon, respectively, may be of relevance here: Huang et al (2020a) https://doi.org/10.1175/JCLI-D-19-0833.1 Huang et al (2020b) https://doi.org/10.1126/sciadv.aay6546 E.g. on page 94 somewhere in lines 28-49. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. One of the suggested references (related to the use of large ensembles) have been included in the revised text to strengthen the assessment.
89123	93	20	96	14	I think it should be mentioned somewhere in this subsection the general notion that we expect precipitation variability increase with warming over all timescales in most places merely because of the increase in moisture, mitigated or amplified to varying degrees depending on location and timescale by changes in circulation. I document this in Pendergrass et al (2017), but this is not the first nor the only study to show it. Pendergrass, A. G., Knutti, R., Lehner, F., Deser, C., & Sanderson, B. M. (2017). Precipitation variability increases in a warmer climate. Scientific Reports, 7(1), 17966. https://doi.org/10.1038/s41598-017-17966-y [Angeline Pendergrass, United States of America]	Noted. However the notion is already included in section 8.4.3.1 where changes in precipitation variability is assessed and there is no need to repeat here.
45269	93	20	98	2	Section 8.5.2 Uncertainties due to natural climate variabilty. This is a very good section [Krishnan Raghavan, India]	Noted. Thank you for the comment
66929	93	20			Perhaps I wasn't reading closely enough but it appears there is a lot of switching back and forth between "internal variability" and "natural variability in section 8.5.2 and the distinction does not always appear clear to me. [Mathew Barlow, United States of America]	Taken into account. The text has been revised and the use of "natural variability" and "internal variability" is now appropriate and in line with the whole report.
17305	93	24	93	24	Please add citation to Australia's CSIRO national climate change projections that show that natural variability masks the emergence of a climate change signal in rainfall the Australian region: CSIRO and Bureau of Meteorology (2015). Climate Change in Australia Information for Australia's Natural Resource Management Regions: Technical Report, CSIRO and Bureau of Meteorology, Australia. [Joelle Joelle Gergis, Australia]	Noted. Emergence is briefly addressed in 8.5.2.1 but without specific regional details, and cross-referenced to Ch 10 where it is assessed more comprehensively
5001	93	25	93	25	Here you could add the 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]	Noted - To avoid a too long list of references with specification of all possible regions, we have not included the suggested reference.
27277	93	25	93	25	Lehner et al. 2020 could also be cited, as this study shows that previous estimations may underestimate the uncertainties due to internal variability for methodological reasons. Lehner, F., Deser, C., Maher, N., Marotzke, J., Fischer, E., Brunner, L., Knutti, R., and Hawkins, E.: Partitioning climate projection uncertainty with multiple Large Ensembles and CMIP5/6, Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2019-93, in review, 2020. [Eric Brun, France]	Accepted. The suggested reference has been added to the list
13029	93	33	94	49	The arrangement of this section seems a little odd in that the modes in models are tested before describing what they might be in observations (PDV, AMV...) [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Ch 8 does not assess the modes of variability, for that ch 2 and ch3 are the references (now included in the text) where the modes are assessed in the observations first and then in the models. Ch8 deals with implications of the modes of variability on water cycle changes.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
20535	93	33	95	22	It is striking that this subsection, in spite of its title, does not include a single number. With the exception of figure 8.8 which deals with heavy precipitation, the reader of this report will know practically nothing about rain climatology. [philippe waldteufel, France]	Taken into account - We have included a new figure in section 8.5 (Fig.8.23 in the FGD) adapted from Lehner et al 2020 to see the quantification of the different sources of uncertainties in projections of precipitation, including the internal variability.
3199	93	33	95	22	What about projections of extreme flooding related to Coastal El Niño in the western South America (e.g. Jauregui and Takahashi 2017. 10.1007/s00382-017-3745-3)? [Jhan Carlo Espinoza, France]	Rejected. Comment and suggested reference not relevant for this section
71487	93	33			This section overlaps a lot with our 10.3.4. You might move parts, delete parts, and then refer to our Chapter. [Douglas Maraun, Austria]	Taken into account. The content of this section has been compared with section 10.3.4 and revised to avoid major overlaps, some repetitions remain but are reduced at the minimum. Anyway the focus of section 8.5.2 and 10.3.4 is different and the literature assessed in the two subsections is different.
88931	93	35	93	38	Perhaps helpful to add an introvuctory sentence comparing internal variability to modelling uncertainty, and how this comparison varies spatially? (Rowell 2012; see Ch.8 reference list) [Dave Rowell, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The text has been substantially revised and these aspects are considered but we have not included the suggested specific reference
45573	93	40	93	41	Global extended reanalyses like 20CR may be used as an entry point to asses the historical water cycle variability over the last 150 years, bridging the gap between recent instrumental data over the last decades and paloclimate archives. Hydrometeorological chains may be used based on these reanalyses to derive long-term variability in the regional water cycle components (Caillouet et al., 2017). Such historical hydrological reconstructions may be further refined through the merging with historical meteorological and hydrological observations (Bonnet et al., 2017 ; Devers et al., 2020). 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 Caillouet, L., Vidal, J.-P., Sauquet, E., Devers, A., Graff, B. (2017) Ensemble reconstruction of spatio-temporal extreme low-flow events in France since 1871. Hydrology and Earth System Sciences, 21, 2923–2951, https://doi.org/10.5194/hess-21-2923-2017 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]	Taken into account. The suggested references as well as the use of extended global reanalysis have been included in the assessment with a reminder to specific sections of ch 10 where these types of methodologies and related uncertainties are specifically assessed.
22477	93	40	93	41	This misses the point that the available record samples a period in which anthropogenic perturbation has been rapid and thus it is very difficult, if not impossible, to tell what in that record represents forced change and what constitutes natural variability. [Peter Thorne, Ireland]	Accepted. The words "and human-influenced" have been inserted in the text after "short".
59077	93	42	93	44	Please provide references [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. However the statement is quite general and it does not need specific references. The sentences that follow explain better the point with specific references

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22479	93	44	93	47	This statement could be lifted and used to discredit huge swathes of the WG1 report as written. It thus needs substantial modification to make clear that it pertains to aspects of the hydrological cycle or to be deleted. [Peter Thorne, Ireland]	Rejected. This sentence is balanced by the sentence just following with more "positive" evidences. Sentence should not be extrapolated as self-sustained when embedded in wider arguments.
103735	93	55	94	1	Not clear: "low confidence' that climate models underestimate internal variability" You mean low confidence in estimating internal variability? The wording may suggest that there would be high confidence that they do not underestimate this variability. Is that supported? [Philippe Tulkens, Belgium]	Taken into account. The statement has been changed as "In summary, there is medium confidence that climate models reproduce the general magnitude and character of internal variability that influences water cycle variables. There is high confidence that internal variability will continue to be a major source of uncertainty, at least for near-term water cycle projections at the regional scale."
98093	93	55	94	4	Could be mentioned that an impact of underestimating internal variability of rainfall in models is that it may make it too easy to detect anthropogenic climate changes because the noise in the signal to noise ratio is underestimated. As one approach to dealing with this, climate change detection analysis can use standardized variables (e.g., the Standardized Precipitation Index) from both models and observations so that the variance is equivalent between model and observational series. An example of this approach was used by Knutson and Zeng (2018) as a sensitivity test (supplemental material). Knutson and Zeng also presented maps comparing observed (estimated) and simulated low frequency internal variability of precipitation (their Fig. 1), suggesting that the CMIP5 models they examined were, if anything, overestimating internal low-frequency variability of precipitation on average, compared to observed estimates. Ref: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637 https://doi.org/10.1175/JCLI-D-17-0672.1. [Thomas Knutson, United States of America]	Taken into account. This aspect has been considered and included at the beginning of section 8.5.2.1 and the suggested reference has been included
29119	93	55			It may be more useful to say something like "high confidence that climate models do not underestimate internal climate variability" since low confidence is to be avoided based on the uncertainty language webinar. On the other hand, there is some evidence that climate models could underestimate multi-decadal variability.(e.g. unprecedented trade wind increase during early 2000s though it can't be ruled out this is a forced response) e.g England et al. (2014) Nature Climate http://www.nature.com/nclimate/journal/v4/n3/full/nclimate2106.html [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text has been changed as ".. there is medium confidence that climate models reproduce the general magnitude and character of internal variability that influences water cycle variables."
3197	94	3	94	3	Please, this is "Southern Amazon basin" instead "the Amazon basin" [Jhan Carlo Espinoza, France]	Accepted. The term has been updated in the revised text
22481	94	6	94	7	It doesn't follow that this will be the case in all models yet this is the implication. Also, all models are imperfect approximations to a complex reality so all models are in some limit wrong from an epistemiological viewpoint. There is little policy value in the point at least as presently stated. [Peter Thorne, Ireland]	Accepted. Specification has been removed, as it is not fully needed to understand the text that follows.
72065	94	6	94	24	One relevant reference could of study on westward extended SST variability in the tropical Pacific in many climate models and its potential impact on ENSO water cycle. -- 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]	Rejected. The suggested references is not specifically relevant for the assessment here as specifically focused on model biases.
13031	94	9	94	12	This seems vague and a reader may get confused with ENSO. Are you (or the study in question) talking about the IPO/PDV or something else? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Actually the study is about the relationship between ENSO and Australian rainfall. The words "In the western Pacific" at the beginning of the sentence were there by mistake and were confusing. They have been removed in the revised text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13033	94	12			Pertaining to the discussion of the ENSO teleconnection, it seems illogical to be discussing internal variability in teleconnections prior to discussing long-term variations in regional climates themselves. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. However internal variability in teleconnections for water cycle implications is part of the scope of this chapter and it has not been our choice to "come before regional chapters".
21085	94	17	94	17	To complement the reference on the South Atlantic Convergence Zone I would add. “Moreover, Talento and Barreiro (2012) have shown that the detection of the anthropogenic forcing on the South Atlantic Convergence Zone is strongly dependent on the characterization of the model internal variability.” The reference is: Estimation of Natural Variability and Detection of Anthropogenic Signal in Summertime Precipitation over South America, Advances in Meteorology https://doi.org/10.1155/2012/725343. [Marcelo Barreiro, Uruguay]	Accepted. The text and related reference has been included in the revised text.
4999	94	20	94	20	What is Section AV I.8? [Bart van den Hurk, Netherlands]	Accepted. It refers to the Technical Annex IV on the Modes of Variability. It is cited following the rules given for the whole report.
22483	94	22	94	26	This is a very esoteric statement pertaining to one country in western Europe. Is such specificity really warranted? [Peter Thorne, Ireland]	Rejected. References to specific processes related to water cycle changes even if occurring also in specific places is part of the assessment and scope of ch 8. Anyway the sentence has been shortened
13037	94	28	94	49	Does much of this not belong in the following "implications for near-term..." section? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. One of the primary use of SMILEs is related to the quantification of the internal climate variability, so it is likely in the reasonable place of the assessment.
27279	94	29	94	29	Single-Model Initial-Condition Large Ensembles (SMILEs) seems to be more commonly used. Also, the different chapter of the report should be consistent on the terminology. [Eric Brun, France]	Accepted. The term has been updated in the revised text
13035	94	29			It may be useful to cross reference to many discussions on large/grand ensembles in Chapter 10.3.4. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Section 10.3.4 is cited in the revised text for further assessments specifically related to regional aspects, and overlaps reduced at the minimum for the self-consistency of the assessment.
71477	94	37			You could cite Maraun, Env. Res. Lett., 2013, doi:10.1088/1748-9326/8/1/014004, here. There I have applied the ToE to changes in mean and heavy precipitation over Europe. I think I also was one of the first to attach uncertainties to ToE estimates based on a multi-model ensemble, and to identify the ToE as a fundamental property of the climate system (with Hawkins and Sutton, and different to Giorgi et al). [Douglas Maraun, Austria]	Not applicable. The sentence has been removed in FGD
20157	94	52	95	6	It is suggested that this figure 8.25 will be easier to read and more convincing if the bottom diagrams are changed to Internal-Forced on the left (change title and plot) and Internal+Forced on the right (change only the title) [philippe waldteufel, France]	Noted with thanks. This figure is adapted as it is from Deser et al. 2017.

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6735	94	54	94	54	Delete "future" here: the period (2016-2045) is specified just afterwards, so the reader can see it is a trend computed over a period that is largely, though not completely, in the future. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Thank you. This correction is incorporated in the FGD.
13039	95	12			This needs a little more detail in order to suggest why this method may lead to erroneous attribution. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Details for the reason of possible bad attribution have been included in the revised text
74171	95	13	95	15	There are also a number of studies which chracterize weather-scale "noise" as opposed to climate-variability-scale "signal" using daily-scale stochastic modeling. Maybe it could be written as, "Other methods to measure the portion of precipitation variability linked with internal dynamics include the partitioning into dynamical versus thermodynamical components (Fereday et al., 2018), the analysis of variance (Dong et al., 2018b), and direct characterization of stochastic "weather-noise" (Short Gianotti 2014 https://doi.org/10.1175/JCLI-D-13-00695.1)." [Daniel J. Short Gianotti, United States of America]	Accepted. The suggested change and related reference have been inserted in the revised text
22485	95	19	95	22	Again, a policymaker would expect a clear statement as to whether CMIP6 does better or not than CMIP5 in these aspects [Peter Thorne, Ireland]	Taken into account. New Fig. 8.23 emphasize the uncertainties related to internal climate variability. As from the recent published work from Lehner et al 2020, there is not striking difference between CMIP5 and CMIP6.
29121	95	19		20	Not clear: should it be "internal water cycle variability" and does this statement say that models mostly capture this but it is only medium confience because it is based on the limited set of models? Can the second sentence be simplified/be made more policy relevant? Is the extensive body required for the short bottom line summary? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The summary statement has been revised as "In summary, there is medium confidence that climate models reproduce the general magnitude and character of internal variability that influences water cycle variables. There is high confidence that internal variability will continue to be a major source of uncertainty, at least for near-term water cycle projections at the regional scale. There is low confidence in the region-dependent time of emergence of water cycle changes (see also Section 10.4.3), but there is medium confidence that changes in wet extreme events will emerge earlier than changes in average conditions."
113465	95	20	95	20	internal water cycle' add'variability' [Diego Miralles, Belgium]	Accepted. The change has been included in the revised text
13041	95	27	44		Is near-term being used in the correct context, i.e. 2020-2040? The discussion in this paragraph is focused on 3-7 years. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Usage of near term in the text has been corrected. When used it strictly refers to the definition as agreed in WG1.
100801	95	27	91	27	Davini et al. 2017 showed an improved of blocking frequency with increasing model resolution (four different resolutions were analysed). (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.) [Corti Susanna, Italy]	Accepted. The suggested reference has been included in the FGD

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6737	95	29	95	30	There are slow components of the atmosphere that also should be initialised. Volcanic aerosol and ozone depleting substances, for example. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The specification has been included in the revised text.
29123	95	37			Are confidence statement warrented here? What is the supporting evidence (agreement/evidence)? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Confidence statements are not needed here, as it is not part of the assessment done here. Text has been revised accordingly
53441	95	39	95	44	Both AMC and IPO also provide hope to more reliable projections of near-term South Asian monsoon (Sandeep et al., 2020; Huang et al., 2020). [Hervé Douville, France]	Accepted. The suggested references have been added in the revised text
13043	95	46	95	55	Is this paragraph on additional benefits of land surface or cryosphere initialisation in the context of seasonal prediction? The purpose of this paragraph needs to be more firmly stated. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The paragraph is clearly associated to decadal prediction issues
29125	95	50			also Kumar et al. (2019) J. Clim https://doi.org/10.1175/jcli-d-18-0540.1 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The suggested reference does not add new information to the assessment and so it is not included.
5003	95	53	95	53	word missing after "additional" [Bart van den Hurk, Netherlands]	Noted. Sentence has been re-drafted
13045	95	53			What does "as an additional generates" mean? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. A specification was missing after "additional" and the sentence has been redrafted
64345	96	3			define "perfect model approach" [CRISTINA Prieto, Spain]	Taken into account. The sentence has been reworded and the term "perfect model approach" removed
13047	96	8	96	14	The wording used here suggests this section has been about decadal prediction over the next decade, specifically, rather than of the near-term (to 2040) in general. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The summary has been rewritten. The distinction between "near-term" (used with the AR6 WG1 meaning) and "decadal" is clear in the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29127	96	8			Suggest simplification e.g. "unforced fluctuations in the climate system limits the skill of regional decadal predictions of near-term water cycle change." [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The sentence has been removed in FGD
18361	96	17	96	55	Please note that Hua et al. (2019) found that changes in volcanic aerosols have contributed to a seesaw rainfall pattern between the Sahel and Southeast Amazon since 1950 mainly through aerosols' impact on tropical Atlantic SST and ITCZ. Hua, W., A. Dai, L. Zhou, M. Qin, and H. Chen, 2019: An externally-forced decadal rainfall seesaw pattern over the Sahel and southeast Amazon. Geophys. Res. Lett., 46, 923-932. https://doi.org/10.1029/2018GL081406. [Aiguo Dai, United States of America]	Taken into account. The suggested reference has been included in the assessment of this subsection.
14857	96	17			what do we know about the past-present response of ENSO to volcanis forcing? [Marie-France Loutre, Switzerland]	Noted. This aspect is assessed in other chapters and it is not part of the assessment for ch8
45271	96	17			Section 8.5.2.3 One of our papers on Fingerprinting of volcanic forcing on the ENSO and Indian Monsoon Coupling is under advanced stage of review. If this paper is accepted for publication, it would be relevant to this section. This paper was submitted for publication before the 31 Dec 2019 cutoff date. [Krishnan Raghavan, India]	Accepted. We have included the suggested reference in the revised text
22489	96	17			The section on volcanic forcing could make use of the Bethke et al. ensemble run with NorESM in CMIP 5 configuration using plausible volcanic futures as used in chapter 4. Why is this not even alluded to in this section? [Peter Thorne, Ireland]	Noted. Actually the reference is in the SOD text, and in the summary there is also a reference to section 4.4.4 (that is the part of ch 4 dedicated to plausible volcanic future). However, the subsection has been revised and now a specific paragraph is dedicated to the topic with proper citations.
129249	96	17			[PROGRESS] The section on the influence of volcanic eruptions on the water cycle is far too simplistic and significant insight has been gained since AR5. Research has shown that there are many contingencies that determine what the water cycle response to an eruption will be, how well such eruptions are sampled by the observational and proxy record, and how well they can be simulated by models. The latter topic then needs to be broken down by irreducible uncertainties in forcings and their consequences on ability to simulate past eruptions (see, for example, Stevenson et al. 2016 and Fasullo et al. 2019). The influence of initial states at the time of the eruption is also consequential for understanding of past eruptions and prediction of events as they unfold. Citations: Pausata, Francesco SR, et al. ""ENSO response to high-latitude volcanic eruptions in the Northern Hemisphere: the role of the initial conditions."" Geophysical Research Letters 43.16 (2016): 8694-8702. Stevenson, S., B. Otto-Bliesner, J.T. Fasullo, 2017: Considering Eruption Season to Reconcile Model and Proxy Responses to Tropical Volcanism, Proc. Nat. Acad. Sci., 114, 1822-1826, doi: 10.1073/pnas.1612505114. Fasullo, J.T., B. Otto-Bliesner, S. Stevenson, The Influence of Volcanic Aerosol Meridional Structure on Monsoon Responses over the Last Millennium, Geo. Res. Lett., doi:10.1029/2019GL084377. [Trigg Talley, United States of America]	Take into account. Some of the suggested references specifically relevant for water cycle changes have been included in the revised assessment. Other more specifically related to ENSO are already included in the assessment in ch 4.
1913	96	23	96	24	I have no idea what this means: "Global precipitation changes have a sensitivity to temperature changes due to volcanic forcing doubled than to GHG forcing (Iles et al., 2013)." [Alan Robock, United States of America]	Not applicable. The sentence has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22487	96	23	96	24	This sentence makes no sense as written. It feels like some context that is required is missing. [Peter Thorne, Ireland]	Not applicable. The sentence has been removed.
1915	96	24	96	26	I also don't understand, "This stronger apparent hydrological sensitivity (medium confidence) arises due to differing magnitudes of the fast response to GHG and sulfate aerosol forcing, despite consistent slow responses to these forcings" [Alan Robock, United States of America]	Not applicable. The sentence has been removed.
29129	96	25			could link back to Section 8.2.1. It would be useful to state upfront that variation in the frequency of climate-relevant volcanic eruptions is not predictable but varies on decadal time-scales and therefore presents an uncertainty on near-term climate projections. Also, much of the body text seems to discuss past hydrological responses to volcanic forcing which belongs in Section 8.3. This section can then simply state that the response is substantial (quantify?) and and pointing out where the response is uncertain across models or volcanic magnitude/forcing response uncertainty (e.g. Schmidt et al. 2019 JGR https://doi.org/10.1029/2018JD028776) and the fact that future eruptions are a known unknown. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - We agreed within the chapter to keep the assessment of unpredictable volcanic forcing in section 5 because in a sense it represents another source of uncertainties in water cycle projections. Also dividing this small part into section 3 (for the past changes) and section 4 (for projections with new approaches as also described in ch 4) would have largely fragmented the assessment.
33209	96	28	96	36	Zuo et al. (2019) revealed the reduced global monsoon precpitation following tropical volcanic eruptions. They further compare the different impact of Northern, tropical and Southern volcanic eruptions on monsoon precipitaiton, and found that monsoon precipitation in one hemisphere can be enhanced by the remote volcanic forcing occurring in the othern hemisphere. 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]	Accepted. The suggested reference has been added to the revised assessment.
1917	96	29	96	31	Alsom what does this mean? "specifically in monsoon-fed regions like South Asia and the tropical African rainbelt for the weakening and equatorward displacement of the Hadley cell (Dogar, 2018)." [Alan Robock, United States of America]	Accepted. The sentence has been rephrased as "specifically over South Asia …."
1919	96	31			"the effects on the hydrology may be opposite" is not correct. It needs to be explained. What effects? [Alan Robock, United States of America]	Accepted. The sentence has been rephrased as "Over the Sahel, the sign of the change in the hydrology depends …"
29131	96	31			"may be" --> "can be" or "are" [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The sentence has been removed.
1921	96	34	96	35	"equatorial eruptions have weaker effects in weakening off equatorial monsoon circulation than subtropical or extra-tropical volcanoes do" I don't understand what this means. What is "off equatorial monsoon circulation?" [Alan Robock, United States of America]	Not applicable. The sentence has been removed.

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1923	96	44			"but probably unable to mitigate the effect of increased" is unclear. This has nothing to do with mitigation (in the IPCC definition), there is no subject, and "probably" is an unclear assessment term. It should be rewritten as "If volcanism of the next century follows the recent observed statistical distribution, the radiative forcing from episodic eruptions will only temporarily cool Earth, and will not reverse the impacts of increased anthropogenic GHGs." [Alan Robock, United States of America]	Accepted. The sentence has been rephrased as suggested.
1925	96	45	96	46	I have no idea what this means: "From a risk assessment perspective, it is a reasonable assumption as long as volcanic aerosols compete with anthropogenic GHG forcings." What is "it?" [Alan Robock, United States of America]	Not applicable. The sentence has been removed.
5005	96	45	96	46	"From a risk perspective, this is a reasonable assumption": it's quite unclear what the assumption is that you refer to and what the relevant for risk assessment is [Bart van den Hurk, Netherlands]	Not applicable. The sentence has been removed.
1927	96	46			Again, what does "A possible exception" mean. An exception to what? [Alan Robock, United States of America]	Taken into account. The sentence has been rephrased as "However, the occurrence … by co-occurrence of volcanic …."
22491	96	52	96	55	But the Bethke et al analysis of plausible volcanic futures looks at the past 2.5k yr or volcanism and may say something very different in this regard. Given that chapter 4 included it it feels odd for chapter 8 to ignore it. [Peter Thorne, Ireland]	Accepted. Sentence has been removed. The citation is used in the assessment and this sentence was highly confusing.
29133	96	52			I think a mean volcanic forcing is specified so it is only the variation in volcanic forcing that is ignored (but we need to check mean volcanic forcing applies across models since if no forcing is specified this will constitute a spurious positive forcing) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The sentence has been removed.
14855	96	54	96	55	could you please include what we could learn from the past? Is the response to volcanic forcing the same during warm and cold global climate? [Marie-France Loutre, Switzerland]	Noted. However this is not part of the assessment of ch8, in ch 4 and in Cross-Chapter box 4.1 there is more specific assessment of this aspect
1929	96	55			What is "last centuries?" This is very imprecise. Do you mean of the last 1000 years? Certainly much larger eruptions can occur, and the sequence of eruptions at the end of the 13th Century CE produced the Little Ice Age, with profound impacts on the hydrological cycle. [Alan Robock, United States of America]	Taken into account. It has been corrected as it was a typo for "century"
116771	96		96		Link to chapter 4 which explores sensitivity studies accounting for possible future eruptions. This section could provide more substance in the summary statement (also building on insights from historical data and paleoclimate infomatiion). [Valerie Masson-Delmotte, France]	Taken into account. The subsection has been revised. Specific reference to Ch4 has been included. More substance in the assessment and summary statement has been included.
71017	97	7	97	18	There is a potential confusion about nonlinearity which needs to be clarified up front. In a mathematical sense, nonlinearity refers to the dependence on the amplitude of climate change. But in a transient scenario, there is aliasing between the amplitude and the time lag from the forcing, such that a dependence on warming level can arise from purely linear processes (see e.g. Zappa et al. 2020 doi: 10.1073/pnas.1911015117). Pattern scaling will fail in the latter case as well. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - This has been clarified in the revised paragraph and a reference to Zappa et al. (2020) has been added..
27281	97	13	97	13	Given what has been discussed above, that at low warming levels changes in hydrological cycle may very well be insignificant, this sentence leads to some inconsistencies in the text. [Eric Brun, France]	Rejected - The focus is here on the forced climate response so that there is no contradiction with the fact that such a response can be small compared to the natural climate variability.
17307	97	21	98	43	This paper is relevant to the discussion in this section, espcially regional precipitation extremes: Bao, J., Sherwood, S. C., Alexander, L. V. and Evans, J. P. (2017). Future increases in extreme precipitation exceed observed scaling rates. Nature Climate Change 7: 128–132. [Joelle Joelle Gergis, Australia]	Rejected - This paper is mostly about the observed scaling of precipitation extremes with local daily temperatures over Australia. Moreover the assessment of the scaling of projected precipitation extremes with global warming levels is rather the topic of Ch11 (Section 11.4.5).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71019	97	37	97	41	Indeed, there are different timescales in the SST response which lead to quite different patterns in the water cycle changes, quite apart from the rapid adjustment (Zappa et al. 2020 doi: 10.1073/pnas.1911015117). [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - A reference to this study has been added in the next paragraph as well as in the introductive paragraph of Section 8.5.3
29135	97	39			a link to 8.2.2.2 could be more appropriate rather than the section in internal variabilty uncertainty [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Thanks for noting the mistake.
71021	97	50	97	54	This is an important point to make, but it's very confusing to make it in this paragraph, which starts off with a statement about linear processes, given that this particular point is definitely nonlinear (in the mathematical sense). It belongs instead on p.98, lines 29-36, where, indeed, the paper is anyway referred to. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - The sentence has been shifted to the more suitable paragraph.
116773	97		97		Please check the consistency of section 8.5.3.1 with chapter 4 (section 4.2.4, pattern scaling). The outcomes of this section on the importance of the state and rate of change for effects on the water cycle are important and should be reflected in the ES and in the decription of levels of warming in the TS. Could this be further illustrated maybe in a table looking at specific aspects for different levels of warming, or different scenarios for comparable levels of warming (esp SSP 7 that have different SLCF aspects)? [Valerie Masson-Delmotte, France]	Noted - A link to Section 4.2.4 on pattern scaling (generally less accurate for mean precipitation than for mean temperature) has been added in Section 8.5.3.1, as well as a link to Section 11.4.5 (to suggest that the scaling is generally better of extreme precipitation than for mean precipitation). The ES has been also edited to better reflect these key findings.
103737	97		97		This section should certainly discuss atmospheric internal variability as a source of non-linearity, eg wave-resonances (Petoukhov et al, PNAS 2013, Coumou et al, PNAS 2014) l 38-42: The 'high confidence' here is a bit confusing. The sections above state many (but not all-inclusive) possible non-linearities in the climate system, and we know that the models are not very good in capturing such non-linearities. So how come we have 'high confidence' in that large-scale atmospheric circulation will respond in a linear way to GHG forcing? We do not have high confidence in how large-scale atmospheric circulation responds to GHG forcing in the first place, let alone if the response is linear or not. [Philippe Tulkens, Belgium]	Rejected - Many studies (especially for detection-attribution) assume the additivity of the forced climate response and of internal climate variability so that the non-linearity of the forced response is not thought to be primarily driven by internal variability. While the suggested papers are interesting studies, they focus on recent observed extreme events rather than on water cycle projections and could be more suitable in Ch11 about changes in extremes and/or in Cross-Chapter Box 10.1 on the influence of Arctic on mid-latitude climate.
13049	98	3			Missing space in "Box10.1" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit completed
7137	98	13	98	16	The issue of the monsoon as a tipping point seems to be dealt with fairly in this section without given undue regard to single-theoretical model studies. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Thanks

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29137	98	13			"simple model" may be better than "theoretical" since "theory" has speculative connotations for some policy makers [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Edited accordingly.
89153	98	21		43	The response precipitation to increasing warming levels should cross reference and be consistent with the analogous discussion for extreme precipitation in Chapter 11, section 11.4.5. [Angeline Pendergrass, United States of America]	Noted - The key findings of Section 11.4.5 have been first summarized at the beginning of the paragraph and are then only tempered by quoting additional studies suggesting some non-linearities in the response of extreme precipitation.
89125	98	27			The GCM Pendergrass et al (2019) shows this for is CESM1, which is a CMIP5 (not CMIP6) generation model. [Angeline Pendergrass, United States of America]	Accepted - Edited accordingly.
17309	98	29	98	29	A few more sentence on CMIP6 results are needed here. At least summarise what is shown in Figure 8.26. [Joelle Joelle Gergis, Australia]	Taken into account. CMIP6 related references and outputs of Fig 8.25 (8.26 in the SOD) have been included in the FGD
71023	98	30	98	33	This point is also shown for the CMIP5 models in Fig 1 of Zappa et al. (2020 doi: 10.1073/pnas.1911015117). [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - A reference to Zappa et al. (2020) has been added in the revised paragraph.
86429	98	38	98	40	Consistency in the assessment of patterns of projected precipitaion change in needed between Chapter-4 and Chapter-8. Chapter-4 assess that pattern of projected precipitation changes will exhibit substantial regional and seasonal contrast in response to global warming (high confidence). [Swapna Panickal, India]	Rejected - Here the assessment is not about the geographical pattern of projected precipitation changes but rather about the validity of the traditional pattern scaling approach, and our final conclusion is consistent with Section 4.2.4 which states that "pattern scaling is not expected to work well for climate variables that have a large fast-adjustment component". A link to this section has been added in this paragraph.
17311	98	38	98	43	This summary statement is important but needs clarification/simplification. Rephrase to say that there are non-linear responses that may impact projected warer cycle changes on a regional scale. [Joelle Joelle Gergis, Australia]	Accepted - Edited accordingly.
129251	98	38	98	43	Perhaps this summary statement can be reworded to make it sound less self-contradictory. [Trigg Talley, United States of America]	Accepted - Edited accordingly.
113467	99	12	99	12	Where is interception (~15-20% of ET) or sublimation here. https://www.essoar.org/doi/pdf/10.1002/essoar.10503229.1 [Diego Miralles, Belgium]	Taken into account - The observed breakdown of terrestrial evaporation has been clarified and quantified based on Wei et al. (2017).
29143	99	14			Rapid reduction in aerosol pollution in the mitigating scenarios is uncertain but is found to accelerate precipitation increases, for example in the south Asian monsoon, based on modelling e.g. Wilcox et al. (2020) ACPD https://doi.org/10.5194/acp-2019-1188 (under review) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - Not relevant to Section 8.5.3.2
5007	99	16	99	17	I would argue that evaporation is generally both water and energy limited, but one of these can become dominant for seasons and regions [Bart van den Hurk, Netherlands]	Taken into account - rephrased.

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22493	99	20	99	31	Much of this was already discussed in section 8.4. This further argues for reconciliation of sections to avoid such redundancies. [Peter Thorne, Ireland]	Rejected - The overall chapter structure has been agreed at LAM1 and will not be modified until the FGD submission. The focus is here on non-linear snow and ice processes which have not been assessed in details in Section 8.4.
2075	99	28	99	28	The reference and the corresponding statements in Chapter 9 have not been found [Matthias Huss, Switzerland]	Taken into account - The right reference is Section 9.5.1.3 and a link to this Section has been made in the revised paragraph.
59079	99	29	99	29	"... glaciers which potential non-linear..." should be "... glaciers with potential non-linear..." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This part of the sentence has been removed.
6739	99	33	99	33	Subsurface drainage can replenish groundwater reservoirs, so is not necessarily part of the runoff. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part of the sentence has been removed.
51473	99	33	99	40	Figure 8.27 shows Amazon, Yangtse and Lena. Not Amu-Darya. However, the original paper includes it (Zhang et al., 2018; Figure 8.27). Please alter the text to include Lena or revise figure. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The purpose here is not to reproduce the original figure with CMIP6 models but to illustrate the more or less linear runoff response at the basin scale.
51475	99	33	99	40	Reference should be Zhang et al., 2018c. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Thanks! (Fixed when merging the different sections)
51961	99	34	99	34	Insert new sentence here: "A non-linear relationship between rainfall and recharge is observed in the tropics where intense seasonal rainfalls associated with large-scale controls of climate variability (e.g. El Niño Southern Oscillation, Indian Ocean Dipole) contribute disproportionately to recharge (Taylor et al., 2013c; Cuthbert et al., 2019)." [Richard Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The suggested sentence has been added
29145	99	35			Can this be simplified e.g. the smaller forced signal relative to natural climate variability in the high mitigation scenarios can reduce the apparent linearity of the climate response [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The sentence has been removed.
11147	99	37	99	37	Zhang et al., 2018 refers to which one? There are three of them, Zhang et al., 2018a, b, c and d. Probably 2018c. [Wen Wang, China]	Taken into account - The right reference is Zhang et al. (2018c) (Fixed when merging the different sections)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51477	99	38	99	40	This sentence is somewhat unclear, does this mean that global mean change in continental runoff? Or global mean runoff? Please clarify [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - The sentence is clearly about global mean runoff anomalies.
53435	99	47	99	48	Hydrological model simulations driven by individual and combined forcing not only show that decreased precipitation will cause larger deficits in soil moisture, stream flow and water table depth over the US High Plains than increased temperature or disturbed land cover, but also that these factors are not linearly additive when applied in combination (Hein et al., 2019). Both satellite observations and numerical model simulations suggest a scale-sensitive and therefore non-linear response of precipitation to deforestation in Amazonia (Khanna et al., 2018). [Hervé Douville, France]	Taken into account - The first sentence has been added to the revised section.
43773	99	48	99	48	I suggest appending the following sentence to the paragraph: "Localized CO2 emissions have also been found to exert an effect on global precipitation patterns (Navarro et al., 20018)". The reference is: Navarro, A., Moreno, R., Tapiador, F. J. 2018. Improving the representation of anthropogenic CO2 emissions in climate models: impact of a new parameterization for the Community Earth System Model (CESM). EARTH SYSTEM DYNAMICS. 9, 2018. ISSN 2190-4979. DOI: 10.5194/esd-9-1045-2018 [Francisco Tapiador, Spain]	Rejected - This paper does not provide strong evidence of significant effects of localized CO2 emissions versus global mean prescribed CO2 concentrations on the water cycle.
103739	99	50	99	51	Why is there low confidence that there are 'non linearities'in the water cycle, while this section list the importance of non-linearities? [Philippe Tulkens, Belgium]	Taken into account - This sentence has been rephrased as follows: "In summary, there is both numerical and process-based evidence that terrestrial water cycle changes can be nonlinear at the regional scale (high confidence)", to highlight the potential nonlinearities at the regional scale rather than the fairly linear global mean response.
29139	99	50			It may be more useful for policy makers to turn this around to "there is high confidence of an approximately linear global-scale response of the terrestrial water cycle to levels of warming with only limited evidence of non-linearities" [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - This sentence has been rephrased as follows: "In summary, there is both numerical and process-based evidence that terrestrial water cycle changes can be nonlinear at the regional scale (high confidence)", to highlight the potential nonlinearities at the regional scale rather than the fairly linear global mean response. Note that policymakers are generally more interested in regional than global mean hydrological changes given the spatial heterogeneity of such changes.
51479	99	55	99	55	"feeded" should be "fed" [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit completed
29141	99	55			"feeded" --> "fed". Is the "medium confidence" statement required? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit completed
53437	100	14			Could be moved into Section 8.4? [Hervé Douville, France]	Accepted - Subsection 8.5.3.3 has been shifted to Section 8.4.3

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5009	100	23	100	23	insert "decline" after "surface temperature", to indicate the direction of temperature (and CO2) change [Bart van den Hurk, Netherlands]	Accepted - Edited accordingly.
129253	100	31	100	37	[GAPS] The summary for the 8.5 section on the limits for projecting water cycle changes needs work. The summary says temperature changes are better predicted as a primary response to emissions whereas precipitation prediction is not well constrained as well as there is a challenge in terms of the signal-to-noise ratio in projected changes in precipitation due to strong natural variability. The paragraphs lacks guidance on how precipitation projections can and should be used given these limitations. Furthermore, one could infer -- based on the 14 pages documenting the precipitation prediction challenges in generating projections of changes in the global and subcomponents of the water cycle -- that there is no information of value for decisionmaking. That is not the intended message coming from this section on limitations, and there is a need for a discussion of the appropriateness, in terms of how and why, projections of the global and subcomponents of the water cycle can be informative and support decisionmaking. [Trigg Talley, United States of America]	Taken into account - There is here a misunderstanding. Lines 31-37 in page 100 are not aimed at summarizing the whole section 8.5 but just the final subsection, as for former subsections and sections. Regarding the whole Section 8.5, we feel that a comprehensive and transparent assessment should evaluate both accumulated knowledge and its limits. Accumulated knowledge about projected water cycle changes is mostly assessed in Section 8.4, while the limits to the assessment are the focus of Section 8.5.
22495	100	42	100	43	I don't think this is exactly how these do define this. It would be advisable to use the exact same formulation. [Peter Thorne, Ireland]	Noted. The text has been modified to precisely reflect the definition in the Glossary.
96817	100	42	100	54	While we agree with the definition of abrupt changes as a severe shift in the global or regional climate, we do not understand why they are generally referred to as "low-likelihood, high impact scenarios. This is misleading and frankly speaking not correct to be used in a general manner. The likelihood depends on the conditions such as e.g. the global temperature or the point in time. In a world with a global warming of 5 °C in 2300 (which we are at least aiming at our current emission levels), most of the abrupt changes discussed in this subsection do feature at least likelihoods of 50%. Actually most of them will happen, if emissions are not drastically cut or in the case of the Amazon or other tropical forest, deforestation is not stopped. It is only the questions when they will happen. We therefore request the authors to use another term to refer to abrupt changes. [Nicole Wilke, Germany]	Accepted. We have removed the low-likelihood/high impact statement.
73903	100	45	100	45	A tipping point is not a threshold that separates stable climate states. An unstable climate state is the threshold that separates stable climate states. The tipping point is the threshold at which a small perturbation would cause, an abrupt transition to an alternative steady state as referred to in the following sentence. [Paul Ritchie, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have modified the definition of tipping point accordingly, which is also now consistent with the definition in the Glossary.
103741	100	51	100	51	Why only Section 8.6 focuses on the year 2300? [Philippe Tulkens, Belgium]	Noted. We were requested to assess the likelihood of abrupt events beyond 2100, hence there is discussion here of likelihoods near 2300.
59041	101	5	101	41	I am not sure if you have noticed the work from Mann et al, "Absence of internal multidecadal and interdecadal oscillations in climate model simulations". Their work is contrary to what you want to show here. I would suggest that you at least include a discussion on this. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This section discusses abrupt (forced) AMOC change, not multidecadal variability.
132081	101	8	101	10	Demonstration of a correlation between heat and salt fluxes [Mourad Amara, Algeria]	Noted. A reference to Buckley and Marshall 2016 has been added here.
87365	101	10	101	11	Temperature can also lead the AMOC to change, this is not only a question of freshwater budget. See e..g Gregory et al. (GRL, 2005 or Swingedouw et al. 2007). Lenton is quite an old reference for this. Jackson et al. (2015, clim dyn) is more up to date (for complete references see IPCC SROCC chapter 6.7). [Didier Swingedouw, France]	Noted. The previous clause specifies that both heat and salt are important for AMOC. We have updated the reference here to Drijfhout 2015 and specifically reference SROCC 6.7
14803	101	10	101	11	due to melting ice sheets' -> 'due to freshwater fluxes from the Greenland Ice Sheet'. Also, update AMOC tipping point from Lenton et al. 2008 reference - there is a large body of literature that studies AMOC collapse, since 2008 (and since 2013, AR5). [Jeremy Fyke, Canada]	Accepted. Reference has been updated.

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83533	101	11	101	11	In Chapter 2 and Annex II the term "last deglacial transition" is used; so to be conform within AR6 WG I it might be good to adjust the wording here. [Antje H. L. Voelker, Portugal]	Accepted. Change has been made.
5011	101	11	101	11	A more recent publication is Drijfhout et al (2015) https://doi.org/10.1073/pnas.1511451112 [Bart van den Hurk, Netherlands]	Accepted. Change has been made.
105621	101	12	101	12	Usually, the Younger Dryas ends in 11,700 (which is also the start of the Holocene) [Frederik Schenk, Sweden]	Accepted. Change has been made.
83535	101	21	101	21	For European dryness you could cite in addition: Naughton, F., Costas, S., Gomes, S.D., Desprat, S., Rodrigues, T., Sanchez Goñi, M.F., Renssen, H., Trigo, R., Bronk-Ramsey, C., Oliveira, D., Salgueiro, E., Voelker, A.H.L., Abrantes, F., 2019. Coupled ocean and atmospheric changes during Greenland stadial 1 in southwestern Europe. Quaternary Science Reviews 212, 108-120, doi: https://doi.org/10.1016/j.quascirev.2019.03.033. [Antje H. L. Voelker, Portugal]	Accepted. Reference has been added.
22497	101	26	101	31	These aspects were assessed by chapters 3 and 4. Why are you repeating the assessment they undertook and inviting readers to play spot the difference rather than characterising their findings on the matter and cross-referencing? [Peter Thorne, Ireland]	Noted. We have deleted the text from this section that is assessed elsewhere, mainly in Chapter 9, and focus here on the response of the water cycle
87367	101	32	101	32	Smeed et al. (2018) do not show anything on multi-decadal variability, since their data covers less than two decades. On this topic you can cite Swingedouw et al. (2015) or Jackson et al. (2016). (for complete references see IPCC SROCC chapter 6.7). [Didier Swingedouw, France]	Noted. This sentence has been deleted since this assessment takes place in Ch. 9 (9.2.3.1)
13051	101	32			Sentence should start with capital. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This sentence has been deleted.
116775	101		101		Link to SROCC and chapter 9 for AMOC should be the starting point, rather than an assessment of AMOC here (there is duplication, as chapter 9 also discusses past AMOC changes). [Valerie Masson-Delmotte, France]	Accepted. We have added pointers to SROCC and Ch. 9 wherever possible and also deleted any text that was a duplication of what it is Chapter 9 already.
83537	102	5	102	6	Additional references -with a data perspective- showing impact of AMOC collapse/slowdown on northern Amazonia are 1) Stríkis, N.M., Cruz, F.W., Barreto, E.A.S., Naughton, F., Vuille, M., Cheng, H., Voelker, A.H.L., Zhang, H., Karmann, I., Edwards, R.L., Auler, A.S., Santos, R.V., Sales, H.R., 2018. South American monsoon response to iceberg discharge in the North Atlantic. Proceedings of the National Academy of Sciences, 115 (15) 3788-3793, doi 10.1073/pnas.1717784115. 2) Campos, M.C., Chiessi, C.M., Prange, M., Mulitza, S., Kuhnert, H., Paul, A., Venancio, I.M., Albuquerque, A.L.S., Cruz, F.W., Bahr, A., 2019. A new mechanism for millennial scale positive precipitation anomalies over tropical South America. Quaternary Science Reviews 225, 105990, doi: https://doi.org/10.1016/j.quascirev.2019.105990. [Antje H. L. Voelker, Portugal]	Noted. Since these references discuss paleoclimate, they have been added to the preceding section describing the paleo evidence for hydroclimatic change.
22499	102	10	102	22	This is not about the hydrological cycle but about ocean circulation so belongs in chapter 9 and not in chapter 8. It should be removed from here. [Peter Thorne, Ireland]	Accepted. This paragraph has been deleted from here.
87369	102	21	102	21	Heat transport is a negative feedback for the AMOC. See e.g. Swingedouw et al. (2007) among many others. (for complete references see IPCC SROCC chapter 6.7). [Didier Swingedouw, France]	Noted. This paragraph has been deleted per another review comment.
27283	102	29	102	29	It does not seem consistent with Figure 8.28, which doesn't show an increase in precipitation in Northern Europe [Eric Brun, France]	Noted. This sentence has been removed because it duplicates points already made.
132083	102	39	102	42	l'année 2050 est cité une deuxième fois, pour dire qu'il y aura absence totale de glace dans l'arctique, sachant que le scénario indique une période de 100ans lorsqu'il y a réchaffement de 1,5°C alors qu'il est de 10ans pour un réchauffement de 2°C (confiance [Mourad Amara, Algeria]	Noted. This sea ice section has been removed because it overlapped too much with assessments elsewhere in the report
132085	102	46	102	48	Indicate how much the decrease in warming should allow for rapid reversibility of glaciation [Mourad Amara, Algeria]	Noted. This sea ice section has been removed because it overlapped too much with assessments elsewhere in the report

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
103743	102	48	102	49	This is new: the reviewer is of the opinion that the decrease in sea ice lowers the albedo, and hence amplifies global warming, thus making it less plausible the sea ice returns. But it is assumed that with this statement, large scale emission reduction measures would offset this effect? [Philippe Tulkens, Belgium]	Noted. This sea ice section has been removed because it overlapped too much with assessments elsewhere in the report
132087	102	52	102	53	Nowhere does this reference mention the relationship between storm track and moisture flux, but rather between "evidence has been reported between strorm track and polar jet stream index reflecting the variability is North atlantic oscillation [Mourad Amara, Algeria]	Noted. This sea ice section has been removed because it overlapped too much with assessments elsewhere in the report
11635	103	4	103	5	The described evidence in the paleoclimate record for rapid climate changes in response to Arctic sea ice loss is also demonstrated in several other paleoclimate studies in both Greenland (Thomas et al., 2016, doi:10.1002/2016GL068513; Malmierca-Vallet et al., 2018, doi:10.1016/j.quascirev.2018.07.027) and Arctic Alaska (Gaglioti et al., 2017, doi:10.1016/j.quascirev.2017.05.012), which should be cited to strengthen this statement. The agreement between at least four studies showing this relation might warrant a confidence statement (I suggest medium confidence). [Ellie Broadman, United States of America]	Noted. This sea ice section has been removed because it overlapped too much with assessments elsewhere in the report
132089	103	4	103	5	Reference cite influence of Abrupt reductions in sea ice on both the moisture source and the regional temperature increase and this is attributable to Dansgaard–Oeschger events [Mourad Amara, Algeria]	Noted. This is why this reference is here (to link sea ice loss during D/O events and the hydrological cycle).
131579	103	10	103	10	what do "wave amplitude" and "weaker background flow" mean in this context? [Hans Poertner and WGII TSU, Germany]	Noted. This sentence has been deleted in the interest of brevity.
83377	103	14	103	17	In this sea ice summary, please add an equivalent statement relating to Antarctic sea ice. [Robert Massom, Australia]	Noted. This subsection was shortened.
59081	103	29	103	32	Please provide references for these statements [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. These sentences are an overview and are just introducing the assessment that follows below.
7395	103	30	103	30	It is important to note here or in a similar location that groundwater reacts slowly to changes in recharge. Equilibration to changed recharge can take decades to millenia (Cuthbert et al., 2019). This renders changes in groundwater availability "delayed abrupt" changes. Reference: Cuthbert, M.O., Gleeson, T., Moosdorf, N., Befus, K.M., Schneider, A., Hartmann, J., Lehner, B., 2019. Global patterns and dynamics of climate-groundwater interactions. Nature Climate Change, 9(2): 137. [Nils Moosdorf, Germany]	Noted. We have removed groundwater from this statement given that its response can be slow.
3203	103	38	104	36	A larger view of impacts of Amazon deforestation on the regional-to-global water cycle shuld be reported here. For instance regarding impacts on the Amazon-Andes connectivity (Espinoza et al 2020. doi: 10.3389/feart.2020.00064.) and over the hydrological cycle in South America (e.g. Martinez and Dominguez 2014. Journal of Climate 27(17):6737–6753. [Jhan Carlo Espinoza, France]	Rejected. This section specifically assesses abrupt change and impacts on the water cycle, which is not covered in the papers suggested.
53439	103	38			May be also quote Gomes et al. (2018) showing that climate and deforestation combined could cause a decline of up to 58% in Amazon tree species richness, whilst deforestation alone may cause 19–36% and climate change 31–37% by 2050. As well as Sullivan et al. (2020) suggesting that the effect of temperature is worse above 32°C and the tropical forest carbon stocks are thus likely to remain high under moderate climate change if they are protected from direct impacts such as clearance, logging, or fires. (https://science.sciencemag.org/content/368/6493/869). [Hervé Douville, France]	Accepted. This reference has been added to this discussion.
67615	103	43	103	44	Observational analyses indicate an increased dry season length over Amazon. "Intensification of dry season" is ambiguous. [Yang Tian, United States of America]	Noted. This sentence has been removed in the interest of brevity.
3201	103	44	103	44	Please, include here updated papers such as the review from Marengo and Espinoza (2016. doi:10.1002/joc.4420.) and Espinoza et al (2019. doi: 10.1007/s00382-018-4462-2) [Jhan Carlo Espinoza, France]	Rejected. References to observational changes in extremes in the Amazon are best assessed in Chapter 11.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13053	103	47			Change "is pronanly to continue" to "will probably continue" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This sentence has been revised
116777	103		104		There is duplication in parts of the assessment here and earlier sections. This section could be shortend and use a table for a synthesis of possible storylines of abrupt changes in the water cycle. [Valerie Masson-Delmotte, France]	Noted. This sea ice section has been removed because it overlapped too much with assessments elsewhere in the report
69529	104	6	104	7	How does this tree cover modelling relate to the climate simulations described further down? Are these numbers very certain? [Martin Singh, Australia]	Noted. This sentence has been removed in the interest of brevity.
67617	104	11	104	12	The effect of Amazon forest can also potentially induce teleconnection through wave propagations or inteference with large-scale circulation. [Yang Tian, United States of America]	Noted, however without some provided references we cannot assess this here.
7331	104	11	104	12	Tropical rainforests known as 'Earth's lung' growing in Southeast Asia (from Myanmar to the Philippines, Malaysia, Indonesia, Papua New Guinea and Sri Lanka; also in Sub-Saharan Africa from the Cameroon to the Congo (Congo Rainforest), South America (e.g. the Amazon rainforest), Central America (e.g. Bosawás, the southern Yucatán Peninsula-El Peten-Belize-Calakmul), Australia, and on Pacific Islands (such as Hawai'i) are playing key role in atmosphere quality by contribute net oxygen through photosynthesis and water cycle by serving as moisture maintaining and transportation, and water sources conservation. [SAN WIN, Myanmar]	Noted. The text here is assessing the possibility of abrupt change in the water cycle in the Amazon basin.
67621	104	11	104	14	The reference list "(Makarieva et al., 2013; Drumond et al., 2014; Yin et al., 2014a; Wright et al., 2017; Agudelo et al., 2018; Molina et al., 2019)" for the statement "The Amazon forest plays an active role in driving atmospheric moisture transport and the generation of precipitation in the South American region" should include the reference to Poveda, G., Jaramillo, L., and Vallejo, L. F. (2014), Seasonal precipitation patterns along pathways of South American low-level jets and aerial rivers, Water Resources Research, 50, 98– 118, doi:10.1002/2013WR014087 Generally, the complete lack in Chapter 8 of references to the work of Professor G. Poveda's group https://scholar.google.ru/citations?user=JhpJdJYAAAAJ , a leading hydrology group in South America, appears unjustified. [Antonio Nobre, Brazil]	Accepted, reference added.
67623	104	11	104	14	The reference list "(Makarieva et al., 2013; Drumond et al., 2014; Yin et al., 2014a; Wright et al., 2017; Agudelo et al., 2018; Molina et al., 2019)" for the statement "The Amazon forest plays an active role in driving atmospheric moisture transport and the generation of precipitation in the South American region" should include the reference to Makarieva, A.M., V.G. Gorshkov, D. Sheil, A.D. Nobre, P. Bunyard, and B. Li, 2014: Why Does Air Passage over Forest Yield More Rain? Examining the Coupling between Rainfall, Pressure, and Atmospheric Moisture Content. Journal of Hydrometeorology, 15, 411–426, https://doi.org/10.1175/JHM-D-12-0190.1 In that paper, first, a correction to the study of Spracklen et al. (2012) (quoted on p. 103 of Chapter 8) regarding moisture transport is provided. Second, it is shown that the theoretical predictions of how the dynamic effects of condensation should influence the moisture transport are in agreement with observations in the Amazon basin. [Antonio Nobre, Brazil]	Accepted, reference added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67629	104	11	104	18	In the view of the above comments, I have the following suggestion for this paragraph: "The Amazon forest plays an active role in driving atmospheric moisture transport and the generation of precipitation in the South American region (Makarieva et al., 2013; 2014; Poveda et al. 2014; Drumond et al., 2014; Yin et al., 2014a; Wright et al., 2017; Agudelo et al., 2018; Molina et al., 2019). Specifically, Wright et al. (2017) established using experimental isotopic data that transpiration from the Amazon rainforest in late dry season triggers the onset of the wet season significantly earlier than expected from the ITCZ seasonal movements. Boers et al. (2017) proposed that this aspect of forest-water cycle interaction may also lead to a tipping point in response to deforestation. They find that once Amazon deforestation is extensive enough to reduce transpiration, and thus atmospheric moisture, beyond the point where there is not enough energy released during condensation to maintain water vapor transport from the Atlantic Ocean, a rapid shift to dry climate state occurs. There is on-going research and discussions revealing the possible physical nature of deforestation-induced tipping points (Boos and Storelvmo 2016 https://doi.org/10.1073/pnas.1603626113 ; Levermann et al. 2016 https://doi.org/10.1073/pnas.1603130113 Jaramillo et al. 2019 J. Atmos.Sci.,75, 3305–3312, https://doi.org/10.1175/JAS-D-17-0293.1 ; Makarieva et al. 2019 J. Atmos.Sci.,76, 2181–2185, https://doi.org/10.1175/JAS-D-18-0358.1)." [Antonio Nobre, Brazil]	Noted. We have made modifications to this paragraph to both shorten it and emphasize the takeaway point, which is that the Amazon forest has the capacity for causing abrupt changes in the water cycle. While the theoretical papers mentioned (Boos, Jaramillo, Makarieva) are interesting they mainly concern general monsoon circulation, and not the Amazon basin. Thus they are not mentioned here.
67631	104	11	104	18	In the view of a number of key knowledge gaps that are listed without references, I believe that it is necessary not to omit in the main text the on-going discussions in the peer-reviewed literature as outlined above. As mentioned in Chapter 7, page 103 in the discussion of climate sensitivity "Looking back, the resulting debates have led to a deeper understanding, strengthened the consensus, and have been scientifically valuable." We should not be afraid of exposing that debates are ongoing. Further evidence that differences in ocean versus land condensation rates determines ocean-to-land moisture transport was provided in a recent study by Scoccimarro et al. 2020 "The typhoon-induced drying of Maritime continent" PNAS https://doi.org/10.1073/pnas.1915364117 When much moisture undergoes condensation in typhoons over the ocean, the atmospheric flow is modified and little reaches land. [Antonio Nobre, Brazil]	Rejected. We are not assessing general theories of monsoon transitions or condensation here. This section focused on land surface feedbacks leading to abrupt changes in the Amazon basin.
69531	104	11	104	18	This paragraph seems to say basically the same thing as the previous one? [Martin Singh, Australia]	Accepted. We have combined the two paragraphs and shortened the text accordingly.
67625	104	13	104	14	The work of Wright et al. (2017) is a significant milestone in the research of Professor Fu et al.'s group aimed at elucidating the cause-effect relationships in the seasonal ocean-to-land moisture transport in South America. As such, in my view, this study deserves more than a brief mention. Wright et al. (2017) found, using experimental isotope data, that atmospheric moistening by enhanced forest transpiration in late dry season triggers the regional wet season to begin two months earlier than the arrival of the ITCZ. Since enhanced transpiration is related to (genetically encoded) leaf phenology, as research in progress of Professor Scott Saleska's group indicates, regional deforestation and possible replacement of undisturbed forests by agricultural plants will likely have a major impact on the regional water cycle. These studies have a direct relevance to the land use related key knowledge gap described on p. 108 of Chapter 8. [Antonio Nobre, Brazil]	Rejected. While this paper contains important findings, we do not have the space to present all of those here. Rather, this section assesses the probability of abrupt change in the Amazon hydrological cycle.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67627	104	14	104	18	Boers et al. (2017) interpreted their deforestation-induced tipping point as based on the same mechanism as was earlier proposed (also by researchers from Potsdam Institute for Climate Impact Research) by Levermann et al. (2009) https://www.pnas.org/content/106/49/20572 and Zickfeld et al. (2005), the latter study quoted on p. 98 of Chapter 8. This assumed mechanism, captured by Eq. (6) of Boers et al. (2017), presumes proportionality between moisture transport by wind and the difference in latent heating rates between the ocean and land. Levermann et al. (2009, their Eq. 3) also associated different rates of latent heating with a temperature difference between ocean and land. This latent heating based mechanism was criticized by Boos and Storelvmo (2016) as incorrest as it ignores the adiabatic cooling of the rising air. This critique is referred to on p. 98 of Chapter 8 as “the limitation” of the theory of Zickfeld et al. and Levermann et al. It therefore appears inconsistent that one and the same approach is criticized in one place (p. 98) but is discussed uncritically in another place. At the same time, the main dynamic Eq. (6) of Boers et al. (2017) formally is not dependent on latent heating but simply relates wind speed to the difference in precipitation (condensation) rates between the land and the ocean. This is at least partially consistent with the biotic pump concept whereby the moisture transport depends on the differences in evaporation rates between the donor (ocean) and acceptor (land) regions. As discussed by Makarieva et al. (2014) Journal of Hydrometeorology, these theoretical predictions are broadly consistent with observations in the Amazon. [Antonio Nobre, Brazil]	Noted. This section is focused on assessing land surface feedbacks leading to abrupt changes in the Amazon basin. We do not have space to critically assess all of the theoretical papers here.
43281	104	14			Read " Boers et al. (2017) proposed that this aspect of forest-water" rather than " (Boers et al., 2017) proposed that this aspect of forest-water" [Cyriaque Rufin Nguimalet, Central African Republic]	Noted. This sentence has been modified.
5013	104	26	104	26	"tendancy" -> "tendency" [Bart van den Hurk, Netherlands]	Accepted, fixed.
34923	104	39	104	55	The SOD notes the greening of the Sahara and Sahel regions some 11k-5k years ago, a phenomenon which is now happening again due to increased global greening due to somewhat-elevated global CO2 levels. See general comment #16. [Jim O'Brien, Ireland]	Noted. No text changes are suggested, thus none are made.
13055	104	41	104	42	In addition to a source of precipitation change, greening of the Sahara/Sahel is also a consequence thereof, surely? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, sentence has been modified accordingly.
93641	105	2	105	2	Add: In a recent review, Pausata et al. (2020) highlight the key role that the Sahara-Sahel region plays in the global climate system and how changes in the intensity of the WAfriM can cause a domino effect perturbing both regional and remote climate. They conclude that the Earth-system-model community should conduct a concerted effort to address challenges in the understanding of the Sahara-Sahel future climate, to reduce uncertainties in the impact of global warming on the WAfriM. ref. Pausata, F. S., Gaetani, M., Messori, G., Berg, A., de Souza, D. M., Sage, R. F., & deMenocal, P. B. (2020). The Greening of the Sahara: Past Changes and Future Implications. One Earth, 2(3), 235-250. [Stefano Materia, Italy]	Accepted, we have added this reference.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28249	105	14	105	18	The cited references currrently do not really match with the statements. Bathiany 13 and Claussen 13 have to be swapped, then it makes sense: "These observations are consistent with theoretical studies suggesting that spatial heterogeneity and diversity in ecosystems can mitigate the probability of catastrophic change (Van Nes and Scheffer, 2005; Bathiany et al., 2013). Conversely, low ecosystem diversity can produce local or regional “hot spots” of abrupt change such as those seen in some paleoclimate records Claussen et al., 2013)." [Sebastian Bathiany, Germany]	Accepted, change made.
105769	105	23	105	23	This sentence about midHolocene monsoon should refer to Fig. 3.11 for additional cross-chapter support. [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, added a reference to this figure.
81699	105	30	105	32	From my understanding, this sentence mentions a precipitation effect on vegetation change in Sahara and Sahel in models (because CO2 fertilization effect is mentioned in line 35). However, “CO2 forcing causes rapid greening” and “albeit in response to longwave rather shortwave forcing” seem strange. The former may be misunderstood as fertilization effect. The latter sound to as if vegetation response to longwave or shortwave directly. I understand there are limitation of number of words and pages, but please consider to rephrase them to more specific, for example, “CO2 radiative forcing causes rapid greening via precipitation change” and “albeit in response to CO2 radiative effect rather insolation change due to different orbit. [Ryouta O'ishi, Japan]	Accepted. We have modified this sentence to make this point clear.
132073	105	30	105	37	scenario too optimistic what are the indicators for a sudden change specifically between 2050 and 2060 and speak of a coverage that will go from 50% to 75%, then already it is not 50%. [Mourad Amara, Algeria]	Noted. These are the results for the specified model simulation. No changes mad.
5677	105	32	105	34	Please check: given the soils and the vegetation in the Sahel, such an increase in tree cover in this comparatively short time is ecologically implausible and very unrealistic. Please either substantiate ths statement with additional sources or delete it. [Joachim Rock, Germany]	Noted. These are the results for the specified model simulation as reported in Drijfhout et al 2015.
42787	105	48	105	51	In West Africa, possible abrupt changes or tippings are not limited to a green sahara state. A recent preliminary investigation by Wendling et al. (2019) shows that part of the Sahelian hydrology may have already tipped from a healthy vegetation/low runoff equilibrium state to a degraded vegetation/hig runoff equilibrium state, in relation with both the effets of the big 1970-2000 drought and Land use changes. Ref: Wendling, V. et al., 2019. Drought induced regime shift and resilience of a Sahelian ecohydrosystem. Environ. Res. Lett., 14 105005, https://doi.org/10.1088/1748-9326/ab3dde [Thierry Lebel, France]	Noted. This paper supports the general idea that the green Sahara/desert Sahara states can switch rapidly, but since it is specific to the 1970-2000 drought we do not cite it here in our discussion of future projections.
132071	105	48	105	51	Too many inconsistencies in this sentence [Mourad Amara, Algeria]	Noted, the assessment has been truncated to low confidence.
103745	105	50	105	51	An abrupt change into a Greener Sahara? Please specify [Philippe Tulkens, Belgium]	Accepted, change made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116779	105		105		Please sharpen the assessment of paleoclimate evidence here (reconstructions and confidence, simulations), building on earlier assessments (AR4 and AR5 had paleoclimate chapters to build on and revisit their key findings on these aspects from novel knowledge). Insights from past warm periods in response to high CO2 levels (Pliocene) for greening in Africa may also be combined with insights from recent orbitally forced changes. [Valerie Masson-Delmotte, France]	Noted. We have clarified what is new since AR5, and noted that the inability of models to capture the Green Sahara is unchanged since AR4. However neither AR4 nor AR5 contains confidence statements concerning the Green Sahara so a direct comparison to our assessment here cannot be made. The Green Sahara existed back to the Miocene, as noted. However there is not enough detailed paleoclimate evidence to do a data-model assessment for the mid-Pliocene Sahara
43283	106	9			Read " analysis of CMIP5 projections by Drijfhout et al. (2015)" rather than " analysis of CMIP5 projections by (Drijfhout et al., 2015)" [Cyriaque Rufin Nguimalet, Central African Republic]	Noted, this was fixed in the FGD draft.
7441	106	16	106	16	« (e.g., (Ramanathan and Carmichael, 2008). » Remove one left bracket [Geremy PANTHOU, France]	Noted, this reference was removed in the FGD.
13057	106	16	106	17	Reference not properly incorporated in brackets [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted, this was fixed in the FGD
43285	106	16		17	Read "(Ramanathan and Carmichael, 2008)." rather than "(e.g., (Ramanathan 16 and Carmichael, 2008)." [Cyriaque Rufin Nguimalet, Central African Republic]	Noted, this was fixed in the FGD draft.
68213	106	19	106	20	Projected mass loss will lead to peak flow and then reduction due to glacier mass loss, but not tipping points [Guðfinna Aðalgeirsdóttir, Iceland]	Noted, we have removed this section of snowpack due to the lack of literature to put together a comprehensive assessment
20537	106	25	107	10	This issue should be of particular interest, as the Dust Bowl in the US in the thirties is still a living memory. The reference by Rosenfeld et al (2002) dealing with microphysical effects seems serious work, strengthened by observational evidence. They write in their conclusion: "Further research should be undertaken to develop a more complete understanding of these interactions". However, this recommendation does not seem to have been followed! [philippe waldteufel, France]	Noted. No text changes are suggested, thus none are made.
35895	106	32	106	34	The "however" in the last sentence of this paragraph suggests that is in conflict with the 10-60% quoted in the previous sentence. But a doubling of dust since pre-industrial would imply that 50% of current dust is anthropogenic, so these two statements are consistent. [Jasper Kok, United States of America]	Accepted. "however" has been removed for clarity
35897	106	38	106	40	I do not think that the statement here that "broadly speaking, dust aerosols suppress precipitation" is supported by current literature. There absolutely are mechanism by which dust suppress precip, as noted in this paragraph, but dust also enhances precip by providing a large fraction of ice nucleation particles and by increasing droplet size through its role as GCCN. It's not clear what the balance of all these different effects are for global precipitation, so this statement should be made more balanced and should reflect the mechanism through which dust increases precip as well. [Jasper Kok, United States of America]	Taken into account. We have modified this sentence to reference Box 8.1, which covers this issue.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13059	106	55	107	10	Somewhere in this paragraph may wish to point out that part of the difficulty may arise from poor/untested land surface models. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Agreed, however we could not locate a reference in the literature to support this statement.
116781	106		106		Having an overall picture of how dust is addressed in the whole AR6 WGI report, building also on SRCCL, could be helpful to focus the assessment here; it may be relevant to consider different timescales. [Valerie Masson-Delmotte, France]	Taken into account. We have added links to Box 8.1 which outlines the effect of aerosols and Chapter 6, which discusses the dust climate feedback, as well as added a reference to the SRCCL.
35899	107	5	107	5	Although there is indeed some evidence that dust will increase in the future, there's also evidence that it will decrease, for instance due to shrinking of deserts from CO2 fertilization. CMIP5 models are split on this question, with the median dust response being close to zero. See Allen et al. (Nature Climate Change, 2016) and Kok et al. (Nature Communications, 2018) [Jasper Kok, United States of America]	Accepted; sentence has been modified with the suggested references and the added point that it is unclear what the radiative impacts of increased dust would be.
68437	107	13	107	13	Recommend changing solar radiation modification to solar radiation management to keep terms consistent across chapters. [Durwood Zaelke, United States of America]	Rejected. The term in Chapter 4 and the Glossary is Solar Radiation Modification, consistent with what we have used here.
51481	107	17	107	18	definition of SRM says SRM refers to changes in long and shortwave radiation budget, however, this is not consistent with SR1.5 glossary. See Ch4 comments, SRM is used in ch4 to mean a modification of earth's shortwave radiation budget only. Please modify to use a consistent definition of SRM throughout the report. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Quoting Chapter 4, "as in SR1.5, the terminology of SRM is used in this report to refer to all direct interventions on the planetary radiation budget, including both shortwave and longwave modification."
41989	107	25			Chapter 8 page 107 line 25 says that marine cloud brightening would reduce global mean precipitation citing Bala et al 2008. Decision makers will read this as a bad feature. However Bala 2010 in Climate dynamics doi:10.1007/s00382-010-0868-1 showed that the very small reduction in precipitation that was more than offset by reducing land evaporation leading to greater river runoff. This, and the reduction of heat stress from cooler winds is what matters for food production. [Stephen Salter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This statement is applied to all shortwave techniques and is supported by multiple references.
17313	107	33	107	35	For regional balance, is it possible to include specific statements on Southern Hemisphere impacts reported by Crook et al (2015)? [Joelle Joelle Gergis, Australia]	Noted, however the focus here is on the large-scale changes in tropical circulation because that is the most robust response observed in the cited literature.
38493	107	46	107	48	Should be 'sudden termination of SRM' [LONG CAO, China]	Rejected. Unsure of which text the reviewer wants to change. The paragraph in question concerns sudden SRM termination.
130541	107	46	107	48	"sudden SRM terminatation"? Please seen 4.6.3.3. [Panmao Zhai, China]	Rejected. Unsure of which text the reviewer wants to change. The paragraph in question concerns sudden SRM termination.
129255	107	49	107	51	There is a reference to impacts of SRM termination being "method-dependent" in addition to "model-dependent". What is the difference between these (i.e., methods and models)? [Trigg Talley, United States of America]	Accepted, we have modified the sentence to clarify that it depends on the type of SRM (i.e. the technique)
113471	108	5	108	5	I would add to this least vegetation dynamics and their impact, specially via transpiration. [Diego Miralles, Belgium]	Taken into account - Combined in a general statement about land surface processes.
34925	108	5	109	22	The SOD appropriately admits to knowledge gaps in the water cycle due to short timescales of global observations and associated data gaps. See general comment #15 above. [Jim O'Brien, Ireland]	Noted - Consistent with our second statement about the need for longer observed timeseries.
114867	108	5	109	22	A comprehensive list of knowledge gaps. [Roxana Bojariu, Romania]	Noted - Thanks.

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129257	108	5	109	22	[GAPS] Section 8.7 appears as a laundry list without context or even references. How does it relate to earlier conclusions of the chapter? Recommend to at least add cross-references to sections where these gaps have presumably been considered to some degree. [Trigg Talley, United States of America]	Rejected - For the sake of brevity, no reference can be quoted in the final section. As suggested by the introductive sentence, these avenues of research are however strongly connected to the previous sections of the chapter.
22501	108	7	108	18	These two bullets are saying fundamentally very similar things just from different angles. I would suggest they be merged. [Peter Thorne, Ireland]	Rejected - The first bullet is explicitly on pre-instrumental data and paleo studies whereas the second bullet is about the D&A of observed changes since the mid-19th century.
113469	108	8	108	8	also ET [Diego Miralles, Belgium]	Not applicable - The sentence has been removed for the sake of brevity.
13061	108	9			The discussion here could also cross-reference the data rescue discussion in Chapter 10.2.4. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - We agree that some of the proposed research avenues are shared by other chapters, but we here avoid links to other chapters for the sake of brevity.
13063	108	10	108	12	This could be stated with caveats of course, since there are no perfect analogues. (E.g. see caution in the cited D'Agostino et al., 2019 study.) [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - The lack of perfect analogue of anthropogenic climate change has been recognized in the revised sentence.
17315	108	14	108	18	This could be interpreted by some people as 'we don't know anything'. Consider rephrasing so it's clear there is still a lot we understand from theory, observations and models. [Joelle Joelle Gergis, Australia]	Not applicable - The first sentence has been removed for the sake of brevity.
27285	108	20	108	20	The role of aerosols (primary and secondary from VOCs) in cloud condensation nuclei, in precipitation remain also, largely unknown [Eric Brun, France]	Rejected - Too specific given the one-page limit for these final remarks.
51963	108	34	108	34	Insert new sentence after the period on this line: "Further, the partitioning of received precipitation at the land surface and the hydrological processes that govern responses to climate change remain inadequately observed, especially in drylands. This gap in understanding is particularly acute in quantifying the impact of the increased frequency and intensity of heavy precipitation on the replenishment of groundwater via focused or diffuse recharge." [Richard Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - Thanks for the suggestion but this paragraph should be shortened (rather than expanded) for the sake of brevity.
87081	108	34	108	37	This gap point is similar to point 1. [Tarul Sharma, Netherlands]	Accepted - This sentence has been removed for the sake of brevity.
87083	108	39	108	40	This gap point is similar to point 3. [Tarul Sharma, Netherlands]	Agreed - Aerosol microphysical processes have been merged in a single item.
87085	108	46	108	48	This gap point is similar to earlier points; hence, It can be merged. [Tarul Sharma, Netherlands]	Agreed - Aerosol microphysical processes have been merged in a single item.
22503	108	46	108	48	Didn't you have a bullet about understanding aerosol effects above which this should be merged with? At the very least they should be consecutive. [Peter Thorne, Ireland]	Agreed - Aerosol microphysical processes have been merged in a single item.
22505	108	49	108	52	This is a more general issue than hydrological cycle and so arguably instead should be in chapter 3 or chapter 4 than here? [Peter Thorne, Ireland]	Taken into account. Yes, the topic is general, but the consequences in terms of water cycle changes are important, hence need to be emphasized in this chapter. The text has been slightly revised to evidence this.

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13065	108	51			Is this discussed elsewhere in the report (e.g. Chapter 4) and thus can it be cross-referenced? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - For the sake of brevity, no link is provided here but a link to Box4.1 on model weighting is made at the beginning of Section 8.5.
22507	108	53	109	4	While I agree in principle with this the shortcoming is not framed as physical science basis. If retained it should be rescoped so it obviously pertains to WG1 and the chapter charge. [Peter Thorne, Ireland]	Agreed -This item has been shortened and reframed regarding the use of improved ESMs to assess the water limitations to specific mitigation strategies or the hydrological side-effects of SRM.
88993	108	53		54	The global water cycle is certainly in balance with the energy cycles, but "constrain" may actually be too strong. The role of latent heating in driving atmospheric circulation and salinity driving ocean circulation mean that the water cycle is not passive. If the goal of this bullet is place the water cycle on the spectrum between mitigation and adaptation, that should be brought out earlier in the sentence. [Angeline Pendergrass, United States of America]	Not applicable - The first sentence has been removed for the sake of brevity.
58999	108		108		Several knowledge gaps are not listed here such as the feedback between thawing permafrost (itself poorly understood) and water cycle changes (particularly groundwater recharge and discharge, see e.g., Walvoord and Kurylyk, 2016, doi:10.2136/vzj2016.01.0010) and water quality impact of thawing permafrost on groundwater quality (Beniston et al., 2018, doi: 10.5194/tc-12-759-2018; Cochand et al., 2020, doi: 10.1007/s10040-020-02109-x, Cochand et al., 2019, doi: 10.1002/ ppp.1998); impacts of water cycle changes on water quality or the the role of alpine groundwater to sustain streamflow in downstream regions (Hayashi, 2019, doi: 10.1111/gwat.12965) ; rock glaciers (Jones et al., 2019, doi: 10.1016/j.earscirev.2019.04.001); relationship between permafrost thaw and lake dynamics (Rey et al., 2019, doi: 10.1088/1748-9326/aaf06f). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted, however given space considerations we restrict this section to the topics for which there is enough literature to conduct an assessment.
67633	109	1	109	3	“Yet, water availability may also exert a strong control on terrestrial carbon and energy fluxes, alter the land sea warming ratio, and represent a limitation to some mitigation strategies such as afforestation and BECCS.” I strongly support highlighting the importance of the biospheric aspect of the water cycle in the climate change context (see Sheil et al. 2019 Science https://doi.org/10.1126/science.aay7309). The link between afforestation and the water cycle should be probably discussed in more detail in the main text of the Chapter. [Antonio Nobre, Brazil]	Noted - The point is at least made in our final section.
129259	109	1	109	4	[GAPS] It seems odd that CDR options such as BECCS and afforestation are only mentioned at the very end as key knowledge gaps. First, BECCS as an acronym should be introduced in this chapter. Second, if there is a knowledge gap, then state the literature indicating this gap (i.e., most literature on SRM, for example). It is unclear the connection of BECCS and afforestration in these knowledge gaps given the evidence presented in the preceding 108 pages of Chapter 8. [Trigg Talley, United States of America]	Not applicable - The whole paragraph has been shortened (and BECCS has been replaced by bioenergy crops), reframed (on using improved ESMs) and merged with another item on the hydrological side-effects of SRM.
129261	109	7			AMV and PDV are not defined in this chapter. [Trigg Talley, United States of America]	Not applicable - The first sentences of this paragraph have been removed for the sake of brevity.
65813	109	9	109	18	Suggest clarification on the concept of heat fluxes in relation to the energy balance so as to make clear the relationships between surface albedo, and rate of energy exchange. [Kushla Munro, Australia]	Not applicable - This comment probably applies to another part of the SOD.

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65815	109	12	109	13	Suggest clarification. Suggest changing the sentence to: "A reduction in the capacity of a surface to reflect sunlight means that there is an increase in the amount of energy absorbed by that surface. This increase in absorbed energy acts to warm the surface." An example such as melting of ice could also be included to illustrate this concept. [Kushla Munro, Australia]	Not applicable - This comment probably applies to another part of the SOD.
88995	109	16			The interplay between climate change and internal variability is not just possible, it is known to exist. One example is the increase in precipitation variability with warming (Pendergrass et al., 2017). The word "possible" should be removed. Pendergrass, A. G., Knutti, R., Lehner, F., Deser, C., & Sanderson, B. M. (2017). Precipitation variability increases in a warmer climate. Scientific Reports, 7(1), 17966. https://doi.org/10.1038/s41598-017-17966-y [Angeline Pendergrass, United States of America]	Not applicable - We agree with this comment but the whole paragraph has been shortened for the sake of brevity.
129263	109	19			CDR is not defined in this chapter. [Trigg Talley, United States of America]	Not applicable - CDR has been removed from the sentence.
65819	109	20	109	28	Suggest clarification to highlight that changes in wind regime (large scale circulation changes) also have a role to play in relation to surface evaporation, not just warming. [Kushla Munro, Australia]	Not applicable - This comment probably applies to another part of the SOD.
81161	109	21	109	22	Suggest adding that additional research is needed as is institutional support mechanisms identified in the event this does happen on a large scale. [Mary Matthews, Azerbaijan]	Not applicable - This item has been removed for the sake of brevity and given the lack of specific suggestions to make progress in this field.
22509	109	21	109	22	There is no actionable information in this as written. It should provide some idea as to how this could be addressed were it to be retained? [Peter Thorne, Ireland]	Agreed - This item has been removed.
65817	109	35	109	35	Suggest clarification about the way aerosols are related to the surface energy budgets. Suggest that by being more explicit, it will help the reader to follow the logic, i.e.: clouds comprised of more numerous smaller droplets reflect more of the incoming solar radiation, which acts to reduce the amount of solar radiation reaching the surface. [Kushla Munro, Australia]	Noted. This is addressed at Box 8.1.
41079	110	0			things sometimes sound a bit jargony/technical (e.g. condensation nuclei, boundary layer, ...) and I think the language could be simplified to be more accessible to a lay audience [TSU WGI, France]	Accepted - The text was revised and written in a simpler language
41145	110	0			very nice summary but it doesn't really reflect the rest of the content. [TSU WGI, France]	Noted - The FAQ was re-written reflecting the main message conveyed by this summary
40379	110	0			I think it would be clearer to have a short introduction on what land use is in the introduction (not just in the summary) then start listing the effects. [TSU WGI, France]	Accepted - Land use definition was included.
40155	110	0			FAQ8.1 Very interesting FAQ but the logical flow/structure of the text is a little hard to grasp: is it an exhaustive list of how the water cycle is affected by land use change? If not it should be clarified. [TSU WGI, France]	Taken into account - The text has been revised to improve the structure
39655	110	0			I think the use of energy and water balance is problematic in here. First of all, you seem to use interchangeably balance and budget, which can cause confusion. Second, you don't really explain the implications of a modification of the water balance or the energy balance. What does is concretely means? It is not self-speaking for a lay audience. Would you consider even removing those terms? Third in the title you mention the effect on the water cycle but then talk about the surface energy budget. Is the latter linked to the water cycle? If so how, can you explain it more clearly in the text? [TSU WGI, France]	Accepted - We deleted any reference to the surface energy balance and focused more on the impacts on water cycle.
113473	110	3	110	3	Please mention in this section changes in aerodynamic conductance and roughness. Crucial for evaporation. [Diego Miralles, Belgium]	Accepted
68835	110	3	110	54	In land use, how significant is the impact of urbanization on the water cycle, particularly the heat island effect, and how does this affect the global climate? [Kai Ma, United States of America]	Taken into account- The effects of urbanisation on runoff and the heat island effect are now mentioned and the text is improved though effects tend to be local rather than global

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27287	110	3			This is a very important FAQ but it needs complete re-writting as 1) it does not clearly describe the various processes, 2) it does not rank the processes according to the known importance, 3) it does not point to the remaining unknowns, 4) it does not list the various land uses that have been studied. A sketch diagram showing the various changes in characteristics and processes could be useful. [Eric Brun, France]	Taken into account- The text has been revised to improve the structure
27289	110	9	110	9	Direct and indirect changes are not clearly identified in the following text [Eric Brun, France]	Not Applicable - This text was modified
27291	110	10	110	11	there is a need to homogeneize the definition of surface albedo with that in previous IPCC reports, the SRCCL for example [Eric Brun, France]	Accepted - This definition was replaced by the definition given by the AR6 WGI Glossary
27293	110	11	110	11	In the FAQ this statement should also include how this change in energy balance affects the water cycle! Otherwise it is useless here [Eric Brun, France]	Accepted - We deleted any reference to the surface energy balance and focused more on the impacts on water cycle.
129265	110	22			The impact of soil moisture on the rapid land temperature changes isn't so much the heat capacity issue, as implied here, but the potential for more evaporative cooling when the soil is wet. [Trigg Talley, United States of America]	Noted - The reviewer is partially right, as both processes are important, the cooling for evaporation and the changes in the soil heat capacity, as stated in the text.
27295	110	24	110	24	This is not correct. Sensible heat flux is a convective flux, and not conduction! [Eric Brun, France]	Rejected - Sensible heat is also associated with conduction
27297	110	25	110	25	This is not correct. There is no direct link between soil temperature and evapotranspiration [Eric Brun, France]	Rejected - The evaporation of water from the soil requires a large amount of energy. Soil water uses the energy from solar radiation to evaporate thereby rendering it unavailable for heating up of the soil. Thus the greater the rate of evaporation, the more a soil is cooled and its temperature decreases. This is know as evaporative cooling.
78733	110	32	110	32	Exchange "stick and grow" with "condense and they can grow". [Heike Wex, Germany]	Accepted
13067	110	32	110	35	Much of this sentence has poor readability and should be revised. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - The text was revised and rewritten to improve readability
59043	110	37	110	40	Does the extraction of water belong to land use change? However, the intensive agricultural planning due to growing population should be included as a type of land use change especially after industry revolution. In addition, I would also expect to see some discussions on desertization and deforestation, which are very important land use change due to human activities. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account- Deforestation is now mentioned
40999	110	42		47	The process linked to the vegetation is not that clear to me. Could you clarify things? [TSU WGI, France]	Accepted - This linked was clarified
13069	110	42			"Last[ly] but not least" sounds rather informal but perhaps that is ok in an FAQ section. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - We changed this to a less informal expression
27299	110	43	110	43	soil water is probably more adequate than 'groundwater' that is often understood as deep water [Eric Brun, France]	Accepted

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83715	110	50	110	54	To be balanced, this section should also note the situations where forestry improves the water cycle i.e. where farmland is converted to forestry it could reduce runoff into waterways, improving water quality (and therefore could also be a benefit for water security). Afforestation can also regulate water flow, preventing flooding etc. [Dan Zwartz, New Zealand]	Accepted
40981	112	0			The logic of the structure is not entire clear to me though, and I would suggest to try to modify it to keep 1 theme per paragraph and follow a bit more the figure. a way around that could be to restructure as follows: 0) summary (= current §0) 1) definition of flooding (1st part of §1) 2) link between heavy rainfalls and flooding (§2 and 4?) 3) atmosphere (§3) 4) soil (part of §5) 5) cryosphere (part of §5) 6) land use (§6) 7) coastal flooding (part of §1) 8) conclusion (second part of §6) What do you think of that? [TSU WGI, France]	Noted: the FAQ8.2 has been updated although we mainly retain the logical structure
40157	112	0			FAQ8.2: very nice summary and interesting FAQ [TSU WGI, France]	Noted: thank you!
129267	112	1	112	55	The relationship between rainfall and flooding is far better described as a risk because the outcome depends hugely on what resilience has been built in, what drainage and storage there is, and many other factors. So with heavy rain there is an increased risk of flood but no guarantee. [Trigg Talley, United States of America]	Noted: we agree that flooding is better stated as a risk and this is dealt with in WGII so is beyond the remit of WGI which deals with the hazard aspects of flooding here
112241	112	1			Would make a clearer distinction between pluvial, riverine and coastal storm surge flooding [Rutger Hofste, Netherlands]	Taken into account: a link to the glossary, which contains a definition of "flood", is now included and pluvial and fluvial flooding are explicitly defined now
40975	112	23		27	The link between wind/storms and heavy rainfalls/flood could be more explicit [TSU WGI, France]	Noted: the effects of heavy and prolonged rainfall in determining pluvial flooding are now stated and the influence of storms on flooding are covered in a general sense which we consider appropriate for this FAQ
65821	112	37	112	39	Suggest clarification of the type of flood being referred to here. [Kushla Munro, Australia]	Accepted: it is now clarified that this is one in a hundred year river flooding
87087	112	37	112	39	This sentence is getting confused at the end. First it describes globe, with exception in Asia. However, it is confusing to understand the projected decrease in flood frequency in many regions, as described in the last part of the sentence. These regions are a part of Asia or globe? [Tarul Sharma, Netherlands]	Accepted: it is now clarified which regions river flooding may decline
17317	112	41	112	41	Please specify which regions may experience drying. [Joelle Joelle Gergis, Australia]	Accepted: it is now specified that regions include the Mediterranean where rainfall decreases and parts of central Europe and north eastern North America where snowmelt flooding declines.
81163	113	1	113	2	If you end on this negating note, you may miss the opportunity for action to be taken. I suggest switching this to second to last sentence with sentence above [Mary Matthews, Azerbaijan]	Accepted: the final paragraph has been modified to end with a more compelling statement
17319	113	1	113	2	Please specify which regions may experience less frequent flooding. [Joelle Joelle Gergis, Australia]	Accepted: these regions are now stated
40159	114	0			FAQ8.3: very nice summary and nice FAQ with a logical flow. [TSU WGI, France]	Thank you

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132207	114	1	115	3	FAQ 8.3: As mentioned in my comment to the whole chapter, this FAQ does not seem to be in the right chapter, since drought is addressed mostly in chapter 11. In addition, the figure is on water cycle changes (changes in runoff and soil moisture), i.e. on changes in climatologies, not in changes in droughts. A more suitable title would be "What are changes in land water availabiltiy and why?" or "Will climate change lead to less water being available on land and why?". The angle of water availabiltiy is interesting enough for the readers without going specifically into drought which requires more background and would need to be coordinated with chapter 11. [Sonia Seneviratne, Switzerland]	Noted. The cross-chapter team on drought agreed that this FAQ should remain Ch. 8.
27301	114	6	114	6	There are many more places in the world than arid subtropics that have experienced drought and will continue to experience drought. So even in a FAQ we would suggest you either list all of those that have medium to high confidence level, or none [Eric Brun, France]	Accepted. Reference to subtropics on this line has been removed.
84033	114	10	114	11	Brazil has different biomes. The Amazon is indeed in a humid tropical region. However, Brazil has a very large semi arid region. The Caatinga, a semiarid hinterland of northeastern Brazil, covers 912,529 km2, and has aroun 700 mm of rain per year; while the Cerrado, a tropical savanna, also semi arid region , covers 2,036,440 km2, with an average of 1200 mm of rain per year. Saying that Brazil is wet, ignores the very large and prone to drought areas. [Marco Tulio Cabral, Brazil]	Noted. We have removed references to any specific countries in this sentence.
132091	114	10	114	11	Best example for drier region should be sahara desert which is in the opposite side from equator line regarding to Brazil [Mourad Amara, Algeria]	Noted. We have removed references to any specific countries in this sentence.
27303	114	13	114	13	Soil mosture is concerned only during the crop growing season, otherwise it does not matter for agriculture [Eric Brun, France]	Rejected. Soil moisture is important for the growing season and thus agriculture. Sentence is unchanged.
17409	114	14	114	14	It might be useful to introduce the concept of socio-economic droughts referring to conditions whereby the available water supply cannot satisfy the human and environmental water needs. [Sabine Egerer, Germany]	Noted. Human management is mentioned in the text below. However socioeconomic aspects are better discussed in WGII
112239	114	20			And navigation [Rutger Hofste, Netherlands]	Accepted, added navigation
27305	114	29	114	29	There is here again a mention to the direct link between soil temperature and larger evapotranspiration rates which is not at all direct and shall be described properly in this chapter [Eric Brun, France]	Rejected. Higher temperatures lead to increased evaporation, this is assessed as high confidence in Chapter 8.
59045	114	32	114	34	It happens that when droughts happen, groundwater are often extracted as the sources for drinking and agriculture. So what would be the combined effects it has on drought? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted, we already specify that this action would make a drought worse.
132093	114	33	114	34	conversely should be replaced by "in the same time" especially if we consider croplands using groundwater in irrigation during drought period. [Mourad Amara, Algeria]	Accepted. Sentence has been changed accordingly.
27307	114	41	114	42	Evapotarnspiration is not driven by changes in land surface temperature. This sentence is therefore not correct [Eric Brun, France]	Rejected. Higher temperatures lead to increased evaporation, this is assessed as high confidence in Chapter 8.
17411	114	44	114	44	In northern/central Europe, it is expected that agricultural droughts become more severe due a shift of precipitation from summer to winter months in combination with higher evapotranspiration rates. [Sabine Egerer, Germany]	Noted. Details of regional projections are addressed in Chapters 11 and 12.
10171	114	44	114	45	I would not include this statement if it is based on a drought index like PDSI or SPEI, for the reasons given elsewhere in my comments on this chapter. To be consistent with FAQ 8.3 Fig 1, you could instead replace with the percent of land area expected to have a significant soil moisture decline, significant runoff decline, or decline in some other real physical quantity. [Jacob Scheff, United States of America]	Noted. The statements in this FAQ are not based on PDSI, but instead are based on multiple metrics of drought including soil moisture and runoff projections.
129269	114	45			"… suffer from at least moderate drought by 2100… " How often? At least once, or commonly? Or is the implication that the drought will be continuous or non-ending? A little confusing as written. [Trigg Talley, United States of America]	Noted. The exact frequency of moderate droughts is difficult to project, hence the assessment simply relates the percentage of land area that might experience moderate drought.

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52129	122	1	122	4	Delete erroneous and redundant reference: "Boukari, M., Kotchoni, D. O. V., Adjomayi, P., Taylor, R. G., Lawson, F. M. A., and Vouillamoz, J.-M. (2018).Relationships between rainfall and groundwater recharge in seasonally humid Benin: a comparative analysis of long-term hydrographs in sedimentary and crystalline aquifers Les relations entre les précipitations et la recharge des eaux souterraines dans le Hydrogeol. J., 27, 447–457. doi:10.1007/s10040-018-1806-2." [Richard Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Accept. This reference is changed to right one : "Kotchoni, D.O.V., Vouillamoz, J., Lawson, F.M.A. et al. (2019). Relationships between rainfall and groundwater recharge in seasonally humid Benin: a comparative analysis of long-term hydrographs in sedimentary and crystalline aquifers. Hydrogeol J 27, 447–457. https://doi.org/10.1007/s10040-018-1806-2"
30697	122	7	122	9	Update please: Bracegirdle, T. J., C. R. Holmes, J. S. Hosking, G. J. Marshall, M. Osman, M. Patterson and T. Rackow, 2020: Improvements in circumpolar Southern Hemisphere extratropical atmospheric circulation in CMIP6 compared to CMIP5. Earth and Space Science, doi: 10.1029/2019EA001065. [Ian Simmonds, Australia]	Accepted. Reference update
30699	122	10	122	11	Final details of this paper are: Bracegirdle, T. J., G. Krinner, M. Tonelli, F. A. Haumann, K. A. Naughten, T. Rackow, L. A. Roach and I. Wainer, 2020: Twenty first century changes in Antarctic and Southern Ocean surface climate in CMIP6. Atmospheric Science Letters, 21, e984, doi: 10.1002/asl.984. [Ian Simmonds, Australia]	Accepted. Reference update
52131	127	11	127	11	Change year on reference to: "(2019b)" [Richard Taylor, United Kingdom (of Great Britain and Northern Ireland)]	This paper was accepted the 14th APRIL 2020. Year on reference is update to : "(2020)"
3645	139	33	139	35	Correct reference: ul Hasson, S. Pascale S. Lucarini V., Böhner J., Seasonal cycle of precipitation over major river basins in South and Southeast Asia: A review of the CMIP5 climate models data for present climate and future climate projections, Atmos. Res. 180, 42-63 (2016), https://doi.org/10.1016/j.atmosres.2016.05.008 [Valerio Lucarini, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The reference is changed to : " Hasson, S. ul, Pascale, S., Lucarini, V., and Böhner, J. (2016). Seasonal cycle of precipitation over major river basins in South and Southeast Asia: A review of the CMIP5 climate models data for present climate and future climate projections, Atmos. Res. 180, 42-63, https://doi.org/10.1016/j.atmosres.2016.05.008 "
3647	139	36	139	37	Correct reference: Hasson, S., Lucarini, V., Pascale, S., and Böhner, J.: Seasonality of the hydrological cycle in major South and Southeast Asian river basins as simulated by PCMDI/CMIP3 experiments, Earth Syst. Dynam., 5, 67–87, https://doi.org/10.5194/esd-5-67-2014, 2014 [Valerio Lucarini, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference changed to : "Hasson, S. ul, Lucarini, V., Pascale, S., and Böhner, J.: Seasonality of the hydrological cycle in major South and Southeast Asian river basins as simulated by PCMDI/CMIP3 experiments, Earth Syst. Dynam., 5, 67–87 (2014), https://doi.org/10.5194/esd-5-67-2014
109409	140	21	140	22	Is it Held and Soden cited twice? [Roberta D'Agostino, Germany]	Accepted. Yes, this reference is cited twice and in the text it appears as Held and Soden (2006), Held and Soden (2006a) and Held and Soden (2006b). Reference and the text are corrected
79875	143	61	143	61	"Kanemaru, K., Kubota, T., Iguchi, T., Takayabu, Y. N., Oki, R., Kanemaru, K., et al." should be "Kanemaru, K., Kubota, T., Iguchi, T., Takayabu, Y. N., and Oki, R." [Shoichi Shige, Japan]	Accepted. Reference is update to : " Kanemaru, K., T. Kubota, T. Iguchi, Y. N. Takayabu, and R. Oki (2017). Development of a Precipitation Climate Record from Spaceborne Precipitation Radar Data. Part I: Mitigation of the Effects of Switching to Redundancy Electronics in the TRMM Precipitation Radar. J. Atmos. Oceanic Technol., 34, 2043–2057, https://doi.org/10.1175/JTECH-D-17-0026.1.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
5487	143	61	143	61	Kamae, Y., Mei, W., and Xie, S. (2017a). Climatological relationship between warm season atmospheric rivers and heavy rainfall over East Asia. J. Meteorol. Soc. Japan, v. 95, 411-431. Kamae, Y., Mei, W., Xie, S., Naoi, M., and Ueda, H. (2017b) Atmospheric rivers over the northwestern Pacific: Climatology and Interannual variability. J. Clim., v. 30, 5605-5619. [Jinwon Kim, United States of America]	Rejected. It is not clear if this is a suggestion to add/replace reference in the text.
5489	144	60	144	60	Kim, J., Moon, H., Guan, B., Waliser, D., Choi, J., Gu,T., and Byun, Y. (2020) Precipitation characteristics related to atmospheric rivers in East Asia. Int. J. Climatol., submitted. [Jinwon Kim, United States of America]	Rejected. It is not clear if this is a suggestion to add/replace reference in the text.
42789	147	35	147	37	This reference seems quite odd; doi:10.1175/1520- 37 0442(2002)015<0187 does not exist. Should rather be: Le Barbé, L., T. Lebel, and D. Tapsoba, 2002. Rainfall variability in West Africa during the years 1950-1990. J. Climate, 15(2), 187-202. doi:10.1175/1520-0442(2002)015<0187:RVIWAD>2.0.CO [Thierry Lebel, France]	Accepted.
59011	152	25	152	27	Marty 2017a and 2917b are the same references [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Yes, this reference is cited twice and in the text it appears as Marty et al. (2017a) and Marty et al. (2017b)
64347	164	10			i do not find the reference to the Sampson et al.'s work in the main text [CRISTINA Prieto, Spain]	Accepted. Sampson et al. (2015) is not quoted in the text
33045	164	28	164	28	The term "Arabian Gulf" must change to "Persian Gulf", as IPCC is a UN related organization should use official name recognized by UN(United Nations, 2006) and Document AD/311/1/GEN:dated 5 march 1991, Document ST/CS/SER.A/29/ADD.1: dated 24january 1992,Resolution UNLA45/ 8/2/ dated 10 august 1984,Resolution UNAD311/Qen dated 5 March 1971,Document CAB/1/87/63 dated 16 february 1987 UNESCO as "Persian Gulf". Even the reference used in the text mentioned this fact as footnote.IPCC report is not a journal or unofficial report, and has to follow UN official recognized name which off course is not offending anybody. [Sahar Tajbakhsh Mosalman, Iran]	Rejected. These instances where the Persian Gulf is mistakenly referred to differently are instances where the title of the paper is being listed in the References list of the WGI Chapter. The WGI has no ability to modify the titles of published works that are not IPCC products, as such the WGI report cannot change the text for these cases. It is recommended to contact the publication journal of the cited literature for further discussion on these published articles.
19521	164	28	164	28	as IPCC is a UN related organization should use official name recognized by UN(United Nations, 2006) and Document AD/311/1/GEN:dated 5 march 1991, Document ST/CS/SER.A/29/ADD.1: dated 24january 1992,Resolution UNLA45/ 8/2/ dated 10 august 1984,Resolution UNAD311/Qen dated 5 March 1971,Document CAB/1/87/63 dated 16 february 1987 UNESCO as "Persian Gulf". Even the reference used in the text mentioned this fact as footnote.IPCC report is not a journal or unofficial report, and has to follow UN official recognized name which off course is not offending anybody. [Hamideh Dalaei, Iran]	Rejected. These instances where the Persian Gulf is mistakenly referred to differently are instances where the title of the paper is being listed in the References list of the WGI Chapter. The WGI has no ability to modify the titles of published works that are not IPCC products, as such the WGI report cannot change the text for these cases. It is recommended to contact the publication journal of the cited literature for further discussion on these published articles.
32715	164	28	164	28	The term "Arabian Gulf" must change to "Persian Gulf", as IPCC is a UN related organization should use official name recognized by UN(United Nations, 2006) and Document AD/311/1/GEN:dated 5 march 1991, Document ST/CS/SER.A/29/ADD.1: dated 24january 1992,Resolution UNLA45/ 8/2/ dated 10 august 1984,Resolution UNAD311/Qen dated 5 March 1971,Document CAB/1/87/63 dated 16 february 1987 UNESCO as "Persian Gulf". Even the reference used in the text mentioned this fact as footnote.IPCC report is not a journal or unofficial report, and has to follow UN official recognized name which off course is not offending anybody. [sadegh zeyaeyan, Iran]	Rejected. These instances where the Persian Gulf is mistakenly referred to differently are instances where the title of the paper is being listed in the References list of the WGI Chapter. The WGI has no ability to modify the titles of published works that are not IPCC products, as such the WGI report cannot change the text for these cases. It is recommended to contact the publication journal of the cited literature for further discussion on these published articles.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18625	164	28	164	29	The term "Arabian Gulf" in the mentioned literature does not exist. If it is to mean the gulf south of Iran and north of the Arabian Peninsula, the correct term for this is the "Persian Gulf." This is according to many approved documents of the United Nations. Also, as this is well considered in the whole manuscripts of this and other chapters, the use of the Persian Gulf's incorrect term causes discrepancies with the rest of the report. I suggest mentioning this in parentheses after the title of the article or right after the wrong term. This comment does not mean that there may be any scientific inconsistency in the cited piece of literature. [Hossein Khajehpour, Iran]	Rejected. These instances where the Persian Gulf is mistakenly referred to differently are instances where the title of the paper is being listed in the References list of the WGI Chapter. The WGI has no ability to modify the titles of published works that are not IPCC products, as such the WGI report cannot change the text for these cases. It is recommended to contact the publication journal of the cited literature for further discussion on these published articles.
79877	167	1	167	1	"Shige, S., Nakano, Y., Yamamoto, M. K., Shige, S., Nakano, Y., and Yamamoto, M. K." should be "Shige, S., Nakano, Y., and Yamamoto, M. K." [Shoichi Shige, Japan]	Accepted. Reference is changed to : " Shige, S., Nakano, Y., and Yamamoto, M. K. (2017). Role of orography, diurnal cycle, and intra-seasonal oscillation in summer monsoon rainfall over Western Ghats and Myanmar coast. J. Clim. 30, 9365–9381. doi:10.1175/JCLI-D-16-0858.1. "
3711	174	26	174	26	Two authors are missing from the Wainwright et al. (2019) reference, and the authors listed are in the wrong order. https://www.nature.com/articles/s41612-019-0091-7 [Declan Finney, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference is update to : "Wainwright, C.M., Marsham, J.H., Keane, R.J., Rowell, D.P., Finney, D.L., Black, E. and Allan, R.P. (2019) 'Eastern African Paradox' rainfall decline due to shorter not less intense Long Rains. Clim. Atmos. Sci. 2, 1–9. doi:10.1038/s41612-019-28 0091-7.
58985	176	50	176	53	Wills et al. (2016a) and Wills et al. (2016b) refer to the same paper. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Yes, this reference is cited twice and in the text it appears as Wills et al. (2016a) and Wills et al. (2016b)
23315	179	48	179	48	Missing a "A" in the title. [Zhenzhong Zeng, China]	Accepted. The reference is changed to : " Zeng, Z., Wang, T., Zhou, F., Ciais, P., Mao, J., Shi, X., et al. (2014). A worldwide analysis of spatiotemporal changes in water balance based evapotranspiration from 1982 to 2009. J Geophys Res D Atmos 119, 1186–1202. "
59035	182	1	182	2	With respect to Figure a, the message of water storage in the different land based compartments is lost. The authors included the ocean on both sides of the land mass to depict the immense storage it provides; however, this is a given and does not need to be depicted as such. Instead, I suggest that the land mass be shifted to the right of the frame, and enlarged. Similar to Figure b. This will help provide clarity, and make the land based storage numbers more readable. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted – this is a design feature to highlight the size of ocean storage, but we appreciate people have different preferences for figures. No design changes made
59037	182	1	182	2	With respect to the legend on the right side of Figure a: the top box is difficult to read due to the color scheme. I would suggest picking colors that contrast a bit more, or using gradient fills to differentie between oceans, saline groundwater, freshwater, etc. This is also true for the depiction of ususable/unusuable freshwater. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account – colour scheme amended for improved clarity
22511	182	1	182	3	Figure has a lot of text that is too small to be readily readable. In both panels having the numbers +/-% takes a lot of working out that the number is an absolute but the range is a % of that number. Also, some of the +/-% make little sense. E.g. the range on permafrost implies zero water storage in permafrost is possible which it clearly isn't and the uncertainty must be asymmetric? There is no %age on fossil groundwater (also fossil not fossile). An overall title figure would help. [Peter Thorne, Ireland]	Taken into account – font size increased and overall title of 'The global water cycle' inserted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96819	182	2	182	2	Figure Water stores upper right panel: Blue text on blue background hard to read. [Nicole Wilke, Germany]	Taken into account – font size increased
7401	182	2	182	2	Fig 8.1. says "fossile/saline groundwater" - it should say "fossil/saline groundwater" [Nils Moosdorf, Germany]	Taken into account – rephrased to read saline/fossil groundwater
7403	182	2	182	2	The numbers for young and fossil groundwater reservoir size are outdated. They could be replaced by values provided by Gleeson et al., 2016. Reference: Gleeson, T., Befus, K.M., Jasechko, S., Luijendijk, E., Cardenas, M.B., 2016. The global volume and distribution of modern groundwater. Nature Geosci, 9(2): 161-167. [Nils Moosdorf, Germany]	Rejected - the review of Abbott et al. (2019) Nature Geosciences includes assessment of the Gleeson et al. 2016 estimates and so we retain the broader range in the later, more comprehensive assessment
74277	182	2	182	3	Figure 8,1 panel a. Some suggestions to change naming in figure: Reservoir to Reservoirs and Saline lake to Saline lakes (as Fresh lakes, wetlands and rivers are also plural), and on the right instead of 'Surface fresh water consists in', 'Surface fresh water consists of'. Also the 'M' in Soil Moisture is capitalised on the right list, but on the figure and in the other terms. [Inne Vanderkelen, Belgium]	Taken into account – plurals added as suggested, formatting issues corrected
82309	182	2	182	3	I propose to slightly update Fig. 1a by removing parts of the lower part of the figure where no information is included. The figure would benefit from larger font size of text and numbers embedded in the figure. [Schröder Marc, Germany]	Noted – this is a design feature to encompass information on the right hand side, but we appreciate people have different preferences for figures. No design changes made
13071	182	2			Much or the wording used in this figure is not very visible or out of focus. Fonts are tiny. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account – font increased
13073	182	2			Under "surface fresh water" should say "consists of" not "consists in". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account – edited to read 'consists of'
13075	182	2			Can't make out any of this part of the scale in panel (a). Are the colours supposed to be changing? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account – colour scheme amended to improve interpretation
59007	182	6	182	6	Fig.8.1. The literature does not contain the reference for Zhou et al., 2019b, please correct/add reference. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - Zhou et al (2019) added to the reference list: Zhou, Y.Q., A.H. Sawyer, C.H. David, and J.S. Famiglietti, 2019: Fresh submarine groundwater discharge to the near global coast. Geophysical Research Letters, 2019GL082749, doi:10.1029/2019gl082749.
7391	182	6	182	6	The fresh groundwater discharge volume mentioned in the figure (0.5 +- 70%) should be updated to the much more sophisticated estimate by Luijendik et al. 2020, which amounts to 0.2+- 120%. The reference is: Luijendijk, E., Gleeson, T., Moosdorf, N., 2020. Fresh groundwater discharge insignificant for the world's oceans but important for coastal ecosystems. Nature Communications, 11(1): 1260. [Nils Moosdorf, Germany]	Taken into account – Groundwater discharge updated to 0.25 +/-90%. Luijendijk et al 2020 also included in the reference list

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7393	182	6	182	6	The saline groundwater volume mentioned in the figure is wrong and its reference not fitting. The value should be 120 +-25%, and the reference should be Kwon et al., 2014. The reference used here for saline groundwater discharge also referencing to the Kwon et al. paper for this number. Zhou et al 2019 dfocues fresh groundwater discharge. Full reference: Kwon, E.Y., Kim, G., Primeau, F., Moore, W.S., Cho, H.-M., DeVries, T., Sarmiento, J.L., Charette, M.A., Cho, Y.-K., 2014. Global estimate of submarine groundwater discharge based on an observationally constrained radium isotope model. Geophysical Research Letters, 41(23): 2014GL061574. [Nils Moosdorf, Germany]	Taken into account: the groundwater discharge value has been updated to 0.25+-90% based on the assessment of Kwon et al. 2014 and Luijendijk et al. 2019 Nature Comms.
59171	182		182		Figure 8.1 (a): the font size is too small, particularly for the numbers, and there is too much wasted space in the bottom third of the figure. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account – font size increased
59173	182		182		Figure 8.1 (a): the "%" symbol is missing for the uncertainty value of Saline/fossile groundwater [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account – % inserted
59175	182		182		Figure 8.1 (b): the "%" symbol is missing for the uncertainty value of groundwater recharge [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted – Missing % symbol is added
59177	182		182		Figure 8.1 (b): the "plus/minus" symbol is incorrectly formatted for Land ice discharge [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account – formatting issue resolved
27309	182		182		The total water on Earth is 1'380'000'000 km3 [Eric Brun, France]	Taken into account – correct value is 1 380 thousand km3, Figure 8.1 has been corrected
22513	183	1	183	1	Font size is perhaps too small. Figure seems to go to greater section level detail than similar figures in other chapters. [Peter Thorne, Ireland]	The font size in Fig.8.2 has been increased and section level details are reduced in the FGD. With the support of TSU, the visual roadmap of Fig.8.2 has been improved in the FGD and also ensuring common structure across chapters.
13077	183	1			Fonts are small [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	The font size in Fig.8.2 has been increased and section level details are reduced in the FGD. With the support of TSU, the visual roadmap of Fig.8.2 has been improved in the FGD and also ensuring common structure across chapters. Done.
116783	183		183		please consider representing visually the links with other chapters too. [Valerie Masson-Delmotte, France]	Thank you. With the support of TSU, the visual roadmap of Fig.8.2 has been improved in the FGD and also ensuring common structure across chapters.
7429	184	0	184	0	Bottom left schema : apparent hydrologic sensitivity refer both to black and grey curves. Please correct the legend. [Geremy PANTHOU, France]	Accepted: typo in legend has been corrected

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3707	184	1	184	1	What does it mean when there is are up and down arrows next to H? Can this be explained in the caption please. [Declan Finney, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: it is now explained in the caption that small up/down arrows denote increase or decrease in the variable
59039	184	1	184	2	In the far left box, with respect to the equation, It would be useful to embolden the respective letters in the figure, i.e. Precipitation (P), Absorbed Sunlight (S), etc. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted: we considered this but don't think it improves the figure which is updated
22515	184	1	184	2	To stand alone the figure needs an overall title. Figure panels are very small and much of the text is too small to be easily read. Several terms are not defined either in a key or the figure caption. [Peter Thorne, Ireland]	Noted: the title is the first line of the caption
38065	184	1	184	4	η should be labelled as hydrological sensitvity, not apparent hydrological sensitivity. [Junhee Lee, Republic of Korea]	Accepted: typo in legend has been corrected
24319	184	1	184	4	η should be labelled as hydrological sensitvity, not apparent hydrological sensitivity. [Jonghun Kam, Republic of Korea]	Accepted: typo in legend has been corrected
89051	184	1		4	The "rapid" and "slow" resposnes are shorthand for the responses to idealized abrupt forcing responses which become convoluated when forcing changes continuously over time. This distinction should be explained somewhere and repeated here. Also, most of the chapter seems to use "fast" more than "rapid," and it would be better to be consistent. [Angeline Pendergrass, United States of America]	Taken into account: it is made clear in the caption that these different timescales combine during transient climate change
13079	184	1			Clarity of text needs improving for FGD. Font sizes too small in many instances in this figure. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: this diagram has been redrafted to improve the clarity
13081	184	1			Not clear what the horizontal arrow is referring to: reduced precipitation globally to increased global precipitation. Do the two intermediate panels (rapid adjustments and semi-rapid) feature reduced or net increased global precip? It is not obvious how to interpret this. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted: this just refers to progression in time. It is not possible to determine increased or decreased precipitation for each panel since it depends on forcing
13083	184	1			What of the "altered precipitation patterns" arrow? Are they more altered by the slow feedbacks than by the rapid adjustments (the natural implication of the arrowhead)? Perhaps better to group the columns together by braces if necessary. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted: the arrow denotes progressively increased changes in precipitation patterns when combining fast and slow responses
13085	184	1			Note that under panel (a), eta and eta_a have been given the same definition! Only eta_a is the apparent hydrological definition. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: typo in legend has been corrected

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129271	184	1			In the legend, presumably eta refers to actual hydrological sensitivity rather than the same quantity as eta_a. After reading the explanation for this figure repeatedly, it is still difficult to follow. [Trigg Talley, United States of America]	Accepted: typo in legend has been corrected
64903	184	2	184	4	Fig. 8.3: \eta and \eta_a both have the same explanation, which seems incorrect [Johannes Quaas, Germany]	Accepted: typo in legend has been corrected
64905	184	2	184	4	The introduction of the term “semi-rapid adjustments” requires coordination with Chapter 7 [Johannes Quaas, Germany]	Taken into account: coordination was undertaken and a version of this figure was adapted for the Technical Summary that focuses on effective radiative forcing
7583	184	3	184	3	Remove in the figure the word "apparent" for the first n: "apparent hydrological sensitivity" [Celine Bonfils, United States of America]	Accepted
13087	184	3	184	4	This caption needs to be much more informative. Is the reader to regard the left panel as the total response, somehow the sum of all the other panels? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: caption has been completed
59179	184		184		Figure 8.3: eta and eta(a) have both been assigned as the "apparent hydrological sensitivity" in the bottom left [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: typo in legend has been corrected
59181	184		184		Figure 8.3: It is not easy to understand what the subfigures at the bottom are demonstrating. There is an arrow from left to right indicating a change from "Reduced precipitation globally" to "Increased global precipitation" but there is no (clear) indication what drives this. Below this, there is an arrow leading to the right from "Altered precipitation patterns" but it is not clear what this arrow is leading to. A more concise figure showing how the global and land precipitation will change instantaneously, daily/monthy and on yearly/decadel time periods would be easier to understand. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account: this diagram has been redrafted to improve the clarity
45717	184		184		Fig. 8.3: It is almost impossible to read the contents of this figure without using a magnifier. [Sabine Wurzler, Germany]	Taken into account: this diagram has been redrafted to improve the clarity
27311	184		184		There are twice the same definition but different naming of the variable "apparent hydro. sensitivity" in the left bottom panel [Eric Brun, France]	Accepted: typo in legend has been corrected
27313	184		184		The figure is hard to read (small text) and difficult to understand. Important information are missing in the legend. More generally, the figure should be described in details, step by step, in the text or in the legend. [Eric Brun, France]	Taken into account: the caption has been completed and the figure is now linked to in the text
27315	184		184		We understand the rationale for this figure and we think it is useful. But just looking at the figure does not help understand, and the supporting text is not very clear either. [Eric Brun, France]	Taken into account: the caption has been completed and the figure is now linked to in the text
112425	185	1	185	12	Fig 8.4 scales - it would be useful to compare these quantities on y-axis scales that can be directly compared, with a ~-0.2 to 0.2 and b ~-0.3 to 0.4 [Paul Durack, United States of America]	Rejected: this is a reasonable suggestion but we think the figure work best as it is
22517	185	2	185	3	For figure to be useable standalone an overall title is required and the key should be bought below with the two scaling terms spelt out in full rather than being semi-acronyms [Peter Thorne, Ireland]	Taken into account: the scaling curves are now spelled out and defined in the caption and an overall title not though necessary

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13089	185	6	185	12	annual mean needs adding to the caption (presumably) [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
4841	185	8	185	10	"dashed" and "dotted" in the text seems contradictory to legend in the figure [Bart van den Hurk, Netherlands]	Taken into account: the figure has been modified and caption updated
4839	185	11	185	11	"three point" doesn't say at which distance these points are. Degrees? 5 degrees? [Bart van den Hurk, Netherlands]	Taken into account: it is now stated that this is 3 grid point average in the latitude direction
19205	185		185		The author should define the red solid lines, red dashed lines, and red dotted lines for the Figure 8.4 (a) like Figure 8.4 (b) [Mohamed Deyab, Egypt]	Taken into account: the figure has been modified and caption updated
116785	185		185		are megadroughts relevant? [Valerie Masson-Delmotte, France]	Rejected: no
112213	185				Rotate chart so north-south aligns with vertical axis. [Rutger Hofste, Netherlands]	Rejected: this is a reasonable suggestion but we think the figure work best as it is
28609	186	2	186	2	It would be interesting to add two boxes linking the agricultural drought to green water depletion and the hydrological drought to blue water. [Moctar Dembélé, Switzerland]	Noted. This figure has been completely redesigned for the final draft.
22519	186	2	186	3	Figure would benefit from a title such as 'Drought definitions' so that it can be used standalone [Peter Thorne, Ireland]	Noted. This figure has been completely redesigned for the final draft.
13093	186	2			Use of snow versus rain in the box implies that the two arrows emanating from the box are one associated with snow and one associated with rain - which I don't think is the intention. Might be better to state "snow/rain" or "snow and rain" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This figure has been completely redesigned for the final draft.
13095	186	3			I don't think the heading "socioeconomic impacts" is correct as used here. It implies an end result (e.g. on crop yields, the economy) which is clear in the downward arrows feeding the box (for agriculture, people, infrastructure...). Yet the socioeconomic impacts box is also feeding agricultural drought and hydrological drought. Clearly, long-term policies of land use and reservoir/irrigation practices can have feedbacks on drought, but these don't spring to mind under the definition of impacts. Perhaps, "socioeconomic impacts and feedbacks" or similar. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This figure has been completely redesigned for the final draft.
27317	186	6	186	7	Except for "snow vs rain", it is not temperature that matters but potential evapotranspiration. While some potential evapotranspiration formulas are based only on temperature, many studies show that it is not satisfactory. [Eric Brun, France]	Noted. This figure has been completely redesigned for the final draft.
45723	186		186		I am not sure what I am supposed to learn from this figure. In addition the anthropogenic influences part is completey missing. [Sabine Wurzler, Germany]	Noted. This figure has been completely redesigned for the final draft. This figure is designed to show the physical drivers of drought. Anthropogenic influences are discussed in WGII
51483	186				Figure 8.5. This is a somewhat confusing diagram to use as an explanation of drought. Given that the precipitation and evapotranspiration has been used as a way of explaining the water balance, it would be very helpful if evaporation would be included here. It is not clear why temperature is pointing downwards through soil moisture here. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This figure has been completely redesigned for the final draft.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28603	187	1	187	1	if "WaterGAP (GPCC precip)" is part of the legend, please do not mix it with the figure title "Land Surface Model P-ET" [Moctar Dembélé, Switzerland]	This figure is dropped in the FGD.
28601	187	1	187	5	In the title it is mentionned "P-ET", while it is mentionned "P-E" in the caption. Please check and be consistent. [Moctar Dembélé, Switzerland]	This figure is dropped in the FGD.
22521	187	1	187	7	Between the figure and caption there is insufficient information to glean what e.g. GLEAM is. None of the products used are referenced. Units of temperature are K and not K-1. The figure title is not self-describing. [Peter Thorne, Ireland]	This figure is dropped in the FGD.
28605	187	2	187	2	Please add labels to the Y-axes [Moctar Dembélé, Switzerland]	This figure is dropped in the FGD.
13097	187	2			Font within figure legend is too small. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	This figure is dropped in the FGD.
19215	187	3	187	3	The authors should define the X-and Y- legends in Figure 8-6 [Mohamed Deyab, Egypt]	This figure is dropped in the FGD.
28607	187	5	187	5	What are the land surface models? [Moctar Dembélé, Switzerland]	This figure is dropped in the FGD.
64947	187	6	187	6	should the unit not be a flux, i.e. a rate / per time unit? [Johannes Quaas, Germany]	This figure is dropped in the FGD.
45721	187		187		Missing axis labeling, missing legend [Sabine Wurzler, Germany]	This figure is dropped in the FGD.
7443	188	0	188	0	Figure 8.7 : please provide the period on which the trend have been estimated. [Geremy PANTHOU, France]	Taken into account in the FGD (Figure 8.7). Trends for two periods (1901-1984) and (1985-2014) are presented.
96821	188	1	188	1	Figure "Trend in precipitation": Please check if map a) and b) have been reversed. Usually such maps show summer (JJA) drying and winter (DJF) wettening. [Nicole Wilke, Germany]	Thank you. Taken into account. In the FGD, we have shown in Fig.8.7 the trend in annual mean precipitation. Furthermore, we have also verified the precipitation trend patterns for summer and winter that were presented in the SOD.
3715	188	1	188	1	Why not show the the other two seasons? These are referred to in the text, for example, East Africa's long and short rains are described. These seasons are important for tropical regions. [Declan Finney, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Future projections are shown for all the seasons. For the historical period, changes are shown for annual mean for two time-periods, along with CMIP6 DAMIP simulations
22523	188	1	188	2	The CRU dataset use is erroneous. The CRU product is not globally complete. Values are infilled with climatology and flagged as such. Use of the flags results in a much more incomplete map as given in the equivalent figure in chapter 2 where only data constrained grids have been used. The CRU panels need to be replotted using only gridboxes with an observational constraint as trends elsewhere will not be realistic. [Peter Thorne, Ireland]	We have replaced the CRU product by GPCC and GPCP products in the FGD (Figure 8.7).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98095	188	1	188	9	Fig. 8.7. has shortcomings which limit its usefulness for the chapter/report. An alternative or additional set of figures is proposed that addresses some of these limitations. Fig. 8.7 appears to use a relatively short time period (the year range is unfortunately not given), but this makes it harder to detect anthropogenic influence compared to using longer records (over land regions); the figure doesn't indicate where observed trends are detectable (i.e., have emerged from natural variability background according to the models), which is an important issue for this report. Shorter-term periods like 1981-2010 typically have few regions with detectable trends in annual or seasonal precipitation compared to longer periods like 1901-2010 (Knutson and Zeng 2018). While Fig. 8.7 shows areas with agreement between data sets or between models, it doesn't show where observations and models are consistent with (or alternatively significantly different from) each other. This is important for assessing confidence in projections with models. The figure doesn't identify regions with suggested anthropogenic influence, which would also be important and topical for this report. (Such regions are ones where the trends are significantly different from those produced in natural forcing runs (including internal variability effect) yet are consistent with All-Forcing runs that include anthropogenic forcing. Sometimes anthropogenic influence is also inferred if the observed change is both detectable and significantly larger than in the All-Forcing runs, if at least the All-Forcing runs have the trend going in the right direction. Examples of this type of detection/consistency/attribution analysis are shown for land regions for three time periods: (1901-2010; 1951-2010; and 1981-2010) in Knutson and Zeng (2018, Figs. 3-5). They also have sensitivity results for individual seasons, results for an alternative dataset (CRU data), individual model results along with CMIP5 ensemble results, and results for normalized (SPI) data from both modeled and observed precipitation time series (see their supplemental material). They also present an analysis comparing modeled and observed internal variability estimates (Fig. 1) to help gauge the models' reliability for such a trend assessment. Ref: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637 https://doi.org/10.1175/JCLI-D-17-0672.1. [Thomas Knutson, United States of America]	Noted with thanks. These points are taken into account in the FGD (Figure 8.7). Trends for two periods (1901-1984) and (1985-2014) are presented. Two observational datasets (GPCC and GPCP) and CMIP6 DAMIP are presented.
24331	188	4	188	4	What about the uncertainties in the CRU preciptiation data? The previous AR reports menitoned the uncertainties of station sampling error but this AR6 report doesn't mention the uncertainties in the CRU data. What is the period for trend calculation? It is not decsribed in either main text or the figure caption. To compare the CRU preciptation data, the oceans might better be masked out in (c)-(h). [Jonghun Kam, Republic of Korea]	Thank you. We have replaced the CRU product by GPCC and GPCP products in the FGD (Figure 8.7).
27319	188	4	188	4	The period is missing. It is very important to give the period. [Eric Brun, France]	Taken into account in the FGD (Figure 8.7). Trends for two periods (1901-1984) and (1985-2014) are presented.
38077	188	4	188	4	What about the uncertainties in the CRU preciptiation data? The previous AR reports menitoned the uncertainties of station sampling error but this AR6 report doesn't mention the uncertainties in the CRU data. What is the period for trend calculation? It is not decsribed in either main text or the figure caption. To compare the CRU preciptation data, the oceans might better be masked out in (c)-(h). [Junhee Lee, Republic of Korea]	Thank you. We have replaced the CRU product by GPCC and GPCP products in the FGD (Figure 8.7).
4903	188	4	188	7	The time range over which these trends are calculated is not indicated in the figure caption [Bart van den Hurk, Netherlands]	Taken into account in the FGD (Figure 8.7). Trends for two periods (1901-1984) and (1985-2014) are presented and indicated in the figure caption.
13099	188	4	188	7	The concepts of agreement are not explained within the caption. To what degree must the models agree with GPCP to be stippled? Is it the sign or something else? And in the model-along panels, 90% agreement on sign? (So 6.3 out of 7 models?) [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account in the FGD (Figure 8.7).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28611	188	7	188	7	Are the names of those models provided somewhere in the manuscript? Same at Page 190 [Moctar Dembélé, Switzerland]	Taken into account in the FGD. The model list is available in chapter data table (Table 8.SM.1).
116787	188		188		Please report the period on which calculations are performed. [Valerie Masson-Delmotte, France]	Taken into account in the FGD (Figure 8.7). Trends for two periods (1901-1984) and (1985-2014) are presented.
14985	188				There is no information of the period of time used for the calculation of the trends in Figure 8.7. I assume is 1979-201? because GPCP is involved in the calculation. Moreover, the names of the observational datasets should be included in the figure caption. [Juan Rivera, Argentina]	Taken into account in the FGD (Figure 8.7). Trends for two periods (1901-1984) and (1985-2014) are presented. The observational datasets are indicated in the figure caption.
22525	189	1	189	2	The figure is not self-describing at present. Needs a self-describing overall title. Font is too small on map panels. The [days] colour bar label is not intuitive. The figure caption is grossly incomplete and provides no reference to the contributing data products used. [Peter Thorne, Ireland]	This figure is dropped in the FGD.
82313	189	1	189	3	Please explain in the caption the red and blue shading as well as the dashed lines. [Schröder Marc, Germany]	This figure is dropped in the FGD.
4905	189	2	189	2	The acronyms used in the lower panel are not explained, don't know which reanalyses products are plotted [Bart van den Hurk, Netherlands]	This figure is dropped in the FGD.
96823	189	2	189	2	Explain hatching in figure caption please. [Nicole Wilke, Germany]	This figure is dropped in the FGD.
27321	189	2	189	2	The legend could be more complete. It could be said that the figure deals with observations (or reanalysis). The significance could be discussed (it that the black crosses?) etc. [Eric Brun, France]	This figure is dropped in the FGD.
13101	189	2			The caption is not detailed enough. What is the graph showing and how is this calculated globally? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	This figure is dropped in the FGD.
7445	190	0	190	0	Figure 8.9 : please provide the period on which the trend have been estimated. [Geremy PANTHOU, France]	Taken into account in the FGD (Figure 8.8). Trends for two periods (1901-1984) and (1985-2014) are presented.
22527	190	1	190	6	Between the caption, the figure and the text I can't work out what GLDAS is. It must be referenced. Does it have a long name that may be more intuitive? I doubt that the true observations are global so it must use assimilation or interpolation. Like CRUTS does it revert to climatology? If so only the data constrained field portions should be retained. Caption lacks critical details around data sources, models used etc. etc. [Peter Thorne, Ireland]	Taken into account. Information about the GLDAS product is provided in the Chapter 8 data table (Table 8.SM.1).
13103	190	2			See earlier comment regarding agreement. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account in the FGD (Figure 8.8).
27323	190	3	190	3	The period is missing [Eric Brun, France]	Taken into account in the FGD (Figure 8.8). Trends for two periods (1901-1984) and (1985-2014) are presented.
4909	190	3	190	4	What period is shown here? [Bart van den Hurk, Netherlands]	Taken into account in the FGD (Figure 8.8). Trends for two periods (1901-1984) and (1985-2014) are presented.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22529	191	1	191	2	Figure requires a self descriptive title to be added so it can be used as a standalone item in outreach / eductaion. Font size on labels is marginal. [Peter Thorne, Ireland]	In the FGD, the acronym TWS (Terrestrial Water Storage) has been expanded in the caption. The title is self descriptive.
112427	191	1	191	9	Expansion of "TWS" in the caption would be useful.This is a very interesting figure, nice work! [Paul Durack, United States of America]	Thank you. Incorporated in the FGD.
96825	191	5	191	5	Explain abbreviation TWS please. [Nicole Wilke, Germany]	Thank you. Incorporated in the FGD.
27325	191	5	191	5	Are these trends significant? How "Probable climate change impact" is defined on such a short period? [Eric Brun, France]	Take into account. This is assessed in the FGD.
88987	191	5		5	The acronym TWS should be written out. [Angeline Pendergrass, United States of America]	Thank you. Incorporated in the FGD.
116789	191		191		This is for a relatively short period, could it be possible to combine this with longer term records at different places, and also consider confidence related to the attribution? [Valerie Masson-Delmotte, France]	Accepted. Yes, the relatively short period of GRACE data is a constraint for attributing the ground water changes. Quantification of long-term ground water changes using different datasets is assessed in the FGD.
28613	192	1	192	1	What are the titles for (a) and (b) [Moctar Dembélé, Switzerland]	Taken into account in the FGD (Figure 8.9). Each panel of the figure has a clear title.
22531	192	1	192	2	Font in lower panels is not legible. The figure needs an overarching title so it can be used standalone. Each column should have a title added e.g. EOF1 and EOF2 rather than / as well as a / b. In the top row the colour bar should be identical and the two panels replotted to permit direct comparability. Given then identical colour bars each row each colour bar could be shown once which would allow then the maps to be made commensurately larger than they presently are. [Peter Thorne, Ireland]	Font size is increased in the FGD (Figure 8.9). Each panel of the figure has a clear title.
28617	192	1	192	15	For consistenecy with other maps in the manuscript, it will be better to plot these maps from 180°W to 180°E rather than 0-360° [Moctar Dembélé, Switzerland]	Thank you. This is incorporated in the FGD (Figure 8.9)
13105	192	1			The wording is illegible, especialy on the bottom panels. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	The font size is increased and wording is legible in the FGD (Figure 8.9)
28615	192	3	192	4	How was the normalization done such there are negatibe values in the maps? I would expect the values to range between 0 and 1 (in case of a min-max normalization). Are you doing a standardization (values minus the mean devided by the standard deviation). Please clarify. [Moctar Dembélé, Switzerland]	This figure is based on Bonfils et al. 2020 and mentioned in the FGD (Figure 8.9)
4889	192	3	192	13	No explanation is given for right column of panels (b), and vertical axis of lowest row of panels is quite unclear. A reference to this figure is made to explain attribution of aerosol concentration on precipitation trends in N America and Europe (Box 8.1) but I cannot follow the rationale of this reference from this figure [Bart van den Hurk, Netherlands]	Taken into account in the FGD (Figure 8.9). The reference to this figure (Bonfils et al. 2020) is included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13107	192	3	192	13	The hatching is not explained by the caption. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	This problem is resolved in the FGD.
113157	192		192		The caption and figure labeling needs to be moe comprehensive to make it self-standing. Hard to grasp now. [Diego Miralles, Belgium]	Accepted. This is incorporated in the FGD (Figure 8.9).
13109	193	1			Could the two pink blocks used on the x-axis of panel (b) be stippled as in the map? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This figure has been modified in the FGD (Figure 8.11).
27327	193	15	193	15	The period is missing [Eric Brun, France]	Taken into account in the FGD. The period is mentioned in the caption of Figure 8.11.
22533	194	1	194	2	Figure requires a self describing title. The lower colour bar should presumably be number of reanalyses in agreement as to sign of change or similar. [Peter Thorne, Ireland]	Done. Title was fully changed.
24333	194	1	194	6	Labels of sub-figures should be (a) through (d), instead of two (a)s and two (b)s. It might better show grid maps, instead of contour maps, in (a) and (b) of the Lower panel. (b) in the lower panel looks very spotty. [Jonghun Kam, Republic of Korea]	Done. Labels were changed to a-f. Maps in lines are better reflected the trends and now on panel “f” only significant at 90% level trends are shown what reduces significantly the spotty.
38079	194	1	194	6	Labels of sub-figures should be (a) through (d), instead of two (a)s and two (b)s. It might better show grid maps, instead of contour maps, in (a) and (b) of the Lower panel. (b) in the lower panel looks very spotty. [Junhee Lee, Republic of Korea]	Done. Labels were changed to a-f. Maps in lines are better reflected the trends and now on panel “f” only significant at 90% level trends are shown what reduces significantly the spotty.
13111	194	1			Given the top panels show NH and SH it is not clear why SH is neglected in bottom panels. In addition, summer and winter in the caption need to make reference to boreal seasons. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Done. SH was added in bottom panels and "boreal winter" and "austral summer" were added in the caption
13113	194	6			Change "pressure in the center" to "cental pressure" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. “Pressure in the centre” was removed from the caption.
45731	194		194		The quoted publication Tilinina et al is only submitted and apparently still in review. There might still be major changes.... [Sabine Wurzler, Germany]	Taken into account. The reference to Tilinina et al 2021 was removed and this figure was updated from Tilinina et al 2013.
4947	195	1	195	1	Would be good to add zero-line in plots [Bart van den Hurk, Netherlands]	Accepted – done

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
59115	195	1			Figure 8.1.4: I think it would be more worth to additionally show the changes in E besides P and P-E as it is very hard to distinguish differences between subplots a and b, c and d and e and f, respectively. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted – done
13115	195	2	195	8	List number of models used in each case. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted – done
27329	195	5	195	6	It is not clear how the confidence interval is estimated [Eric Brun, France]	Accepted – legend has been changed
27331	195	6	195	7	How is the confidence interval estimated precisely? Based on the standard deviation of 20-year means? Or of differences of 20-year means? [Eric Brun, France]	Accepted – legend has been changed
113159	195		195		Including also E would make this much clearer. So I would suggest adding a middle column with E profiles. [Diego Miralles, Belgium]	Accepted – done
69507	196	1	196	1	This may be relevant to other chapters also: I think the phrase "non significant" is potentially misleading in describing multi-model mean changes being smaller than 1 stdev of variability. In some regions (e.g., the Australian monsoon in CMIP5) small multi-model mean changes mask relatively large and opposing changes across models. [Martin Singh, Australia]	Noted - The stippling/hatching strategy used in Ch8 is the default AR5 method and distinguishes significance (vs a median estimate of internal variability) and consistency (across multiple models).for the sake of simplicity an
28619	196	1	196	1	It might be good to show the inter-model variability (i.e. maps of CV of models used in figure). Idem for Figure 8.17, 8.18 and 8.19. [Moctar Dembélé, Switzerland]	Thanks for the suggestion, but the format for these figures has been standardised across the chapters of the Report, so we cannot add maps of CV.
22535	196	1	196	2	Chapter 4 has a very similar figure. Is the inclusion of two grossly overlapping figures wise? If this is persisted with then it would be key to ensure that the same models and approaches are used otherwise the report will be inviting the reader to play spot the difference. [Peter Thorne, Ireland]	Taken into account – This figure has been changed to show the four seasons change for a medium scenario (SSP2-4.5).
24335	196	1	196	10	The grids where the annual precipitation is below 0.5 mm/day might better be maskouted out since these regions are deserts. Instead of a constant interval in the color scale, log or log10 scales might show better spatial patterns of preipitation changes. [Jonghun Kam, Republic of Korea]	Taken into account –for dealing with low precipitation areas, a cross-chapter decision was reached for showing the relative changes of multi-model means, which allows to get a consistent signal ; for scale type, there was varied feelings about linear vs log scale, and we kept a linear one
38081	196	1	196	10	The grids where the annual precipitation is below 0.5 mm/day might better be maskouted out since these regions are deserts. Instead of a constant interval in the color scale, log or log10 scales might show better spatial patterns of preipitation changes. [Junhee Lee, Republic of Korea]	Taken into account –for dealing with low precipitation areas, a cross-chapter decision was reached for showing the relative changes of multi-model means, which allows to get a consistent signal ; for scale type, there was varied feelings about linear vs log scale, and we kept a linear one
88989	196	1		1	I think MAM and SON seasons should be included. Alternatively, they could be included in the interactive atlas, and then information on how to find them could be included in the caption. [Angeline Pendergrass, United States of America]	Thank you for the suggestion, only JJA and DJF were included for space reasons. The other seasons are viewable in the interactive atlas. Unfortunately a link to the atlas was not included in the figure caption.
13117	196	4	196	10	It is not clear from the caption, and not obvious, why single-level precipitation data would be missing from models, and why this differs between seasons. There should be no divide by zero problem here. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected : a divide by very low value problem does exist, which leads to a single model relative change dominating the mean. This problem was alleviated by choosing, as for other chapters, to show relative change of multi-model means

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
113161	196		196		Having seasonal trend maps is great, but make sure the whole-year trends are also illustrated somewhere in the report… Also the maps of E trends and P-E trends. This may require an in-between figure from 8.14 to 8.15. [Diego Miralles, Belgium]	In the FGD, trends in the annual mean precipitation and evapotranspiration are shown in Figures 8.7 and 8.8 respectively.
22537	197	1	197	1	As some authors will be aware I spent a lot of time trying to understand this figure in TS drafting and failed to. The whole figure speaks in codes that are not easy to decipher. The key should be bought out below and the terms spelt out in full. The x-axis is implied to be some temporal aspect but then given in K. I am not sure it is intuitively obvious why the three variables should all be plotted on the same graphs. I would urge consideration of redrafting this figure so it could be used in a first year undergraduate class without recourse to the text or caption. This to my mind is not the case presently. [Peter Thorne, Ireland]	Taken into account : albeit the objective of « using the figure without recourse to the text or caption » seems utopic, a series of improvement have been brought for the key and the caption text ; regarding the « temporal aspect of x-axis », we rephrased the figure title (and caption first words) as « dependency of rate of change on global warming » with a sub-title which explains that the dependency parameter is the rank of a 20 years time slice along 21st century
96827	197	1	197	1	Looks like a draft. Replace abbreviations in legend, etc….. please. [Nicole Wilke, Germany]	Taken into account. Legend is now fully explicit, except for two abbreviations explained in caption
4949	197	1	197	1	Is it correct that the first and second time slice have a 10 yr overlap (as indicated in the x-axis label)? [Bart van den Hurk, Netherlands]	Accepted : there is a 10 year overlap for all time slices ; axis title has been fixed
59119	197	1	197	9	Figure8.16 and ist caption: In sub plots a) and b) JJA and DJF, respectively, and in subplots c) and d) summer and winter, respectively, is written. However, in the caption it is the other way round. Moreoever, is there a difference between JJA and summer or DJF and winter, respectively. f yes, please speify. Additionally, the legend text shown in sub plot b) should be less cryptic. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted : caption has been changed; winter here means a combination of DJF for Northern hemisphere and JJA for southern hemisphere ; this has been clarified in the caption and in the legend
28621	197	1	197	10	Why are the changes evaluated over periods of 20 years instead of 30 years as usually seen in climate change studies? It would be good to mention the reason somewhere in the manuscript in not yet the case. [Moctar Dembélé, Switzerland]	Noted - All reference periods for assessing near-term, mid-term and long-term climate changes are 20-yr periods in the AR6 and the reference period for the present-day climate (1995-2014) is also a 20-yr period, as agreed with all chapters and given the fast rate of the on-going global warming (typically 0.2°C per decade).
88991	197	2		9	This figure is updated and adapted from Pendergrass et al., (2017); perhaps it would be appropriate to provide a citation in the caption. Pendergrass, A. G., Knutti, R., Lehner, F., Deser, C., & Sanderson, B. M. (2017). Precipitation variability increases in a warmer climate. Scientific Reports, 7(1), 17966. https://doi.org/10.1038/s41598-017-17966-y [Angeline Pendergrass, United States of America]	Accepted
4951	197	4	197	4	Is the standard deviation taken over models or years? [Bart van den Hurk, Netherlands]	Accepted – « inter-annual » was added in front of « standard deviation »
113163	197		197		Add legend [Diego Miralles, Belgium]	Taken into account. Legend is now fully explicit, except for two abbreviations explained in caption
24337	198	1	198	10	The grids where the annual precipitation is below 0.5 mm/day might better be maskouted out since these regions are often classified in to desert. [Jonghun Kam, Republic of Korea]	Taken into account – This problem was alleviated by choosing, as for other chapters, to show relative change of multi-model means
38083	198	1	198	10	The grids where the annual precipitation is below 0.5 mm/day might better be maskouted out since these regions are often classified in to desert. [Junhee Lee, Republic of Korea]	Taken into account – This problem was alleviated by choosing, as for other chapters, to show relative change of multi-model means
4963	198	6	198	7	order of scenarios in panels is reverse to the caption order [Bart van den Hurk, Netherlands]	Accepted – fixed
112235	198	6			Maps and description SSP scenarios do not match [Rutger Hofste, Netherlands]	Accepted – fixed
4967	199	1	199	1	typo in title of figure ("evopotranspiration") [Bart van den Hurk, Netherlands]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112429	199	1	199	10	"evopotranspiration" typo should be fixed [Paul Durack, United States of America]	Accepted
27333	199	4	199	4	Changes over ocean are not discussed (the figure is included in a chapter about land evapotranspiration). Maybe the changes over sea should be masked? They are distracting, and in apparent contradiction with some affirmations in the text (i.e. water at surface implies an increase in evapotranspiration) [Eric Brun, France]	Taken into account - are briefly discussed in the revised Subsection
113165	199		199		Correct the 'o' at 'evopotranspiration'. Also, evapotranspiration is not used for oceans. [Diego Miralles, Belgium]	Editorial - Corrected
112431	200	1	200	9	Is there valid data over Antarctica? If not, suggest using a missing value rather than maxing out the colour scale [Paul Durack, United States of America]	Accepted – Antarctica has been masked out
59139	201	1	201	1	Figure 8.20: I guess this figure is still under construction as two subplots are missing... [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	This is fixed in the FGD (Figure 8.19).
82317	201	1	201	2	The top panels seem to be missing. Please insert. [Schröder Marc, Germany]	Taken into account in the FGD (Figure 8.19).
96829	201	1	201	5	Why CMIP6 SSP3-7.0? What does a > 60% increase of runoff in the Sahara imply? [Nicole Wilke, Germany]	Taken into account. In the FGD, we have only considered CMIP6 SSP1-2.6, SSP2-4.5 and SSP5-8.5 (Figure 8.19).
10173	201				On Fig. 8.20, are the VPD & WUE panels blank because the data is still being processed? Or are they blank because of computer error? Needs to be clarified if the former, and fixed if the latter. [Jacob Scheff, United States of America]	This is fixed in the FGD (Figure 8.19).
70359	201				Figure 8.20 - Surface soil moisture is not the most relevant variable for drought and may differ from deeper soil moisture (Berg et al. 2017). Would be more appropriate to use a depth range that contains the rooting zone, could be either total column or integrated to 3m. [Abigail Swann, United States of America]	Accepted. Surface soil moisture (top 10 cm) and soil moisture (total column) are shown in the FGD (Figure 8.19)
5591	201				For the figure 8.20, it lacks 2 figures : vapor pression and water use… [Benoit Laignel, France]	Taken into account in the FGD (Figure 8.19).
22539	202	1	202	2	An overall figure title would help. Can you not long hand PDSI up the left hand side rather than repeating the acronym four times for accessability. For comparability the left hand series should use the same y-axis range(-4 to 3). Why is the third timeseries distinct and why does it have no corresponding right hand panel? Chapter 11 deprecated use of PDSI. Consultation is required. [Peter Thorne, Ireland]	Noted. An overall figure title has been added. Not sure what is meant by the third time series. PDSI is used here because this is the quantity used in the paleoclimate community. Projected changes are qualitatively similar those indicated by other metrics of drought and aridity

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24329	202	1	202	14	Are PDSI for the Meditgerranean and western North American regions sefl-calibrated? Self-calibrated PDSI is more spatially comparable than the PDSI, and reports extreme wet and dry conditions with frequencies that would be expected for rare conditions. What is the meaning of a monotonic downward trend with a non-linear (e.g., exponential) rate over the next 60 years. If either precipitation are certainly decreased or evapotranspirtaion are certianly increased, then the environment should be changed and need to update the emprical constants in PDSI. That's why scPDSI proposed to overcome this limitation in previous studies (Wells et al. 2004). Referece: Wells, N., Goddard, S., & Hayes, M. J. (2004). A self-calibrating Palmer drought severity index. Journal of Climate, 17(12), 2335-2351. Physically, how the continued downward trend in PDSI could happen without no rain (which is I guess impossible to be seen in reality)? Could the auhors consider to show the relative chage of soil moisture compared to the long-term climatology such as Figure 8. 15 or 19. Also, it seems that time series of the PSDI vlaues seems to running averages, but there is no specific the moving average window size (10 years or 20 years). [Jonghun Kam, Republic of Korea]	Noted. PDSI is used here because this is the quantity used by the paleoclimate community. The projections here are qualitatively similar to the future projections using metrics like soil moisture, streamflow, and runoff.
38075	202	1	202	14	Are PDSI for the Meditgerranean and western North American regions sefl-calibrated? Self-calibrated PDSI is more spatially comparable than the PDSI, and reports extreme wet and dry conditions with frequencies that would be expected for rare conditions. What is the meaning of a monotonic downward trend with a non-linear (e.g., exponential) rate over the next 60 years. If either precipitation are certainly decreased or evapotranspirtaion are certianly increased, then the environment should be changed and need to update the emprical constants in PDSI. That's why scPDSI proposed to overcome this limitation in previous studies (Wells et al. 2004). Referece: Wells, N., Goddard, S., & Hayes, M. J. (2004). A self-calibrating Palmer drought severity index. Journal of Climate, 17(12), 2335-2351. Physically, how the continued downward trend in PDSI could happen without no rain (which is I guess impossible to be seen in reality)? Could the auhors consider to show the relative chage of soil moisture compared to the long-term climatology such as Figure 8. 15 or 19. Also, it seems that time series of the PSDI vlaues seems to running averages, but there is no specific the moving average window size (10 years or 20 years). [Junhee Lee, Republic of Korea]	Noted. PDSI is used here because this is the quantity used by the paleoclimate community. The projections here are qualitatively similar to the future projections using metrics like soil moisture, streamflow, and runoff.
13119	202	1			This is a nice and visually appealing figure. Why is there more than one line for each colour in the PDFs? The caption implies a single ensemble. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The caption now notes that the blue lines in the pdf panels are the four CESM LME ensemble members.
11109	202	8	202	9	Figure 8.21 shows the significant drop of PDSI in some regions after 2000, which is dramatic and doubtful. It is shown that the result about the past-to-future fully-forced simulations from the NCAR CESM Last Millennium (red line) comes from Otto-Bliesner et al. (2016), but I did not find such a curve of PDSI in the paper. [Wen Wang, China]	Noted. In fact the future projections are qualitatively similar to those shown in Section 8.4 for soil moisture, for example. The CESM Last Millennium results were re-calculated from the published dataset.
10175	202				For Fig 8.21, the caption should make clear that the major projected PDSI decreases in these regions are not always consistent with the same models' projections of actual drought impacts like runoff production (Swann et al. 2016 already cited; Scheff 2017 already cited; Scheff 2018 Curr. Clim. Change Reports https://doi.org/10.1007/s40641-018-0094-1) or vegetation coverage (Mankin et al. 2019 already cited; Mankin et al. 2018 already cited.) It should also make clear that the PDSI decreases may be strong overestimates because plant physiological effects are not accounted for in the PET computation (Yang et al 2018d already cited; Yang et al 2020 HESS https://doi.org/10.5194/hess-2019-701.) [Jacob Scheff, United States of America]	Noted. PDSI is used here because this is the quantity used by the paleoclimate community. The projections here are qualitatively similar to the future projections using metrics like soil moisture, streamflow, and runoff.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
69519	203	1	203	1	Fig. 8.22: I would like to see CMIP5 distributions of these projections here too. 7 GCMs is not enough to characterise the range of projections across the suite of climate models developed across the globe (which itself is not a representation of all the uncertainty in future rainfall change). [Martin Singh, Australia]	Taken into account in the FGD. In the FGD, 24 CMIP6 models are considered (Figure 8.22)
27337	203	3	203	3	How "seasons" are defined should be explained in the legend [Eric Brun, France]	This is taken into account in the FGD. Monsoon seasons for the different regions are defined in Annex V.
7139	203	3			The North American SSP1-2.6 result is very strange. I guess things will improve when there are more models available. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This is presumably related to the strong internal variability under SSP1-2.6. In the FGD, we have monsoon precipitation anomalies for different regions (Figure 8.22).
7141	203	3			It is interesting to see that the EqSAmer region follows largely the same pattern of signals as in the NAmerM, suggesting it is subject to the same forcings and drivers, and part of the same overall system. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Thank you. Yes, it appears the precipitation signals in NAmerM and EqSAmer are subject to the same forcings and drivers.
22541	203	4	203	4	Which seven? These should be spelt out in full. [Peter Thorne, Ireland]	Taken into account in the FGD.
28623	203	4	203	4	Please make sure the models are listed somewhere in the manuscript. [Moctar Dembélé, Switzerland]	Taken into account in the FGD. The model list is available in chapter data table (Table 8.SM.1).
27335	203		203		The bar plots do not need to be blue [Eric Brun, France]	This is taken into account in the FGD.
22543	204	1	204	2	Figure needs a self-describing title. Left hand axis label could be spelt out in full. [Peter Thorne, Ireland]	This figure is dropped in the FGD.
28625	204	3	204	8	Which RCPs are used for the projection period? Please provide the list of the 16 GCMs used in this figure somewhere in the manuscript. [Moctar Dembélé, Switzerland]	This figure is dropped in the FGD.
4991	205	4	205	19	Really hope to see this figure in future versions of this chapter [Bart van den Hurk, Netherlands]	Unfortunately this figure could not be included in the FGD.
22545	206	1	206	1	Figure needs a self describing title. Colour bar needs units label added (mm/day/30yr). But equally per decade would be a more intuitive time divisor than 30 years. [Peter Thorne, Ireland]	Taken into account in the FGD (Figure 8.25). Title, Units label for colour bar are included.
22547	207	1	207	2	In the lower two panels can't you use second - first rather than c-a and d-b? It would be more intuitive and reader friendly. Colour bar and in particular its label fonts could be larger [Peter Thorne, Ireland]	Accepted
13121	207	4	207	11	See earlier comment regarding meaning of missing. Hatching also not mentioned in caption. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	1st part : Rejected : a divide by very low value problem does exist, which leads to a single model relative change dominating the mean. This problem was alleviated by choosing, as for other chapters, to show relative change of multi-model means 2nd part : Accepted : hatching is now mentioned in the caption
116791	207		207		Could it make sense to also explore contrasted SSP7 scenarios with different SLCF mitigation [Valerie Masson-Delmotte, France]	Rejected - It could make sense but the number of available models is reduced and there is no space for such an additional figure in the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11149	208	3	208	9	In Figure 8.27, it is not quite reasonable to mix simulations in 200 year from 1900-2100 to calculate the standard deviation, because climate forcing in the 20th century is known whereas climate forcing in 21st century is quite uncertain . They are not the same thing. In addition, I cannot find the results in the refered paper by Decharme et al. (2019). [Wen Wang, China]	Taken into account. The figure now shows only the mean values, which alleviates the misunderstanding occuring in this comment. Regarding the reference to Decharme et al. (2019), which stands for the river network used in computing basin-averaged, we used that network by personal communication.
13123	209	4	209	8	Is this a single model, and if so what? What data or experiment name is it? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. There are two experiments. On the left, it is the TraCE experiment, on the right, a modern collapse scenario. Both experiments were conducted with the CCSM3 model. The relevant studies are cited in the caption.
116793	209		209		Could it be possible to have a multi model approach here (Liu et al 2009 was already used in the AR5 paleoclimate chapter), and show confidence in understanding of change (as done in the SROCC figure on AMOc change, 6.10)?I could be relevant to illustrate the mechanisms (interhemispheric change in temperature, heat transport, ITCZ shift)? [Valerie Masson-Delmotte, France]	Noted. It is not possible to have a multi-model assessment because TrACE is the only time-transient experiment available.
22549	211	1	211	1	Have the CRU and GPCC products been masked to exclude those gridboxes that are a climatology based estimate rather than observationally constrained? [Peter Thorne, Ireland]	Noted. The GRU and GPCC are presented in the figure, which is completely changed
82319	211	1	211	13	In case GPCC and CRU related results are kept please insert significance of observed trends and add "Becker et al., 2013)" after "Climatology Centre". Ref.: Becker A, Finger P, Meyer-Christoffer A, Rudolf B, Schamm K, Schneider U, Ziese M (2013) A description of the global land-surface precipitation data products of the Global Precipitation Climatology Centre with sample applications including centennial (trend) analysis from 1901–present. Earth Syst Sci Data Discuss 5:921–998. doi:10.5194/essd-5-71-2013. [Schröder Marc, Germany]	Noted. The figure was completely changed.
13125	211	1			As a continuous rather than discrete colour scale, it conveys little information. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The figure was completely changed.
22551	212	1	212	7	This aspect has been assessed in depth in chapters 6 and 7. It is unclear why this belongs in chapter 8. Discussion is required across chapters 6-7-8 around this figure in my view. [Peter Thorne, Ireland]	Noted. After cross chapter consultation the figure stays here.
27339	212	2	212	2	The legend is incomplete. What are the red lines, the plus signs etc.? [Eric Brun, France]	Accepted. The legend was expanded.
113167	212		212		Not clear what exactly this figure s conveying. Please mention what the red line is or add a legend. [Diego Miralles, Belgium]	Accepted. The legend was expanded.
45725	212		212		The red lines should be either described or omitted. [Sabine Wurzler, Germany]	Accepted. The legend was expanded.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22553	213	1	213	9	Between the figure and the caption it would be good to define what the measure of seasonality is. Is it e.g. wettest season precipitation/driest season precipitation? If a short definition of what the metric is can be added to the figure then the figure would become considerably more accessible to the reader. [Peter Thorne, Ireland]	Noted, thanks. We have expanded the caption to convey more clearly what is shown: Global maps of projected changes in 3 precipitation seasonality (simply defined as the sum of the absolute deviations of mean monthly 4 rainfalls from the overall monthly mean, divided by the mean annual rainfall as in Walsh and 5 Lawler, 1981) averaged across 31 to 33 CMIP6 models in the SSP1-2.6 (b), SSP2-4.5 (c) and SSP5-6 8.5 (d) scenario respectively. The simulated 1995-2014 climatology is shown in panel (a). All 7 changes are estimated in 2081-2100 relative to 1995-2014. Uncertainty is represented using the 8 simple approach: No overlay indicates regions with high model agreement, where ≥80% of models 9 agree on sign of change; diagonal lines indicate regions with low model agreement, where <80% of 10 models agree on sign of change. For more information on the simple approach, please refer to the 11 Cross-Chapter Box Atlas.1. Further details on data sources and processing are available in the 12 chapter data table (Table 8.SM.1).
59117	213	1	213	9	Figure Box 8.2, Figure 1 and its caption: It is not easy to understand what the two colour bars and their values represent regarding the subplots a) as well as b), c), and d). This should be described exactly. Moreoever, in the caption the sub-plot numbering is not correct as SSP1-1.6 should be (b) and not (a), SSP2-4.5 should be (c) not (b) and SSP5-8.6 should be (d) and not (c) regarding the sub-plot nummeration. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Thanks. The caption has been reworked and lengthened to better expain the quantities displayed. Error in the descriptuion/numeration of the sub-plots fixed in FGD.
13129	213	1			It is not clear what is meant by change in seasonality. What is the absolute seasonality shown in panel (a)? What are the units of change in panels (b-d), is it fractions of the annual cycle, so 1 = a 12 month change? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Thanks for the comment. We have expanded the caption to convey more clearly what is shown: Global maps of projected changes in 3 precipitation seasonality (simply defined as the sum of the absolute deviations of mean monthly 4 rainfalls from the overall monthly mean, divided by the mean annual rainfall as in Walsh and 5 Lawler, 1981) averaged across 31 to 33 CMIP6 models in the SSP1-2.6 (b), SSP2-4.5 (c) and SSP5-6 8.5 (d) scenario respectively. The simulated 1995-2014 climatology is shown in panel (a). All 7 changes are estimated in 2081-2100 relative to 1995-2014. Uncertainty is represented using the 8 simple approach: No overlay indicates regions with high model agreement, where ≥80% of models 9 agree on sign of change; diagonal lines indicate regions with low model agreement, where <80% of 10 models agree on sign of change. For more information on the simple approach, please refer to the 11 Cross-Chapter Box Atlas.1. Further details on data sources and processing are available in the 12 chapter data table (Table 8.SM.1).
111517	213	3	213	9	Describe briefly in the caption what the seasonality parameter is. [James Renwick, New Zealand]	Thanks, the figure caption has been extended to better explain what it is that is plotted.
24339	216	1	216	6	The grids where the annual precipitation is below 0.5 mm/day might better be maskouted out since these regions are often classified in to desert. [Jonghun Kam, Republic of Korea]	Not applicable - the figure has been completely revised and the issue no longer arises.
38085	216	1	216	6	The grids where the annual precipitation is below 0.5 mm/day might better be maskouted out since these regions are often classified in to desert. [Junhee Lee, Republic of Korea]	Not applicabl. The Figure has been completely revised and the problem no longer arises.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33049	216	1	216	7	Discuss the uncertainty of these forecasts statistically [Sahar Tajbakhsh Mosalman, Iran]	Rejected. This is an FAQ and so we do not have space to discuss this here. This is assessed in the main text of Chapter 8.
32719	216	1	216	7	Discuss the uncertainty of these forecasts statistically [sadegh zeyaeyan, Iran]	Rejected. This is an FAQ and so we do not have space to discuss this here. This is assessed in the main text of Chapter 8.
27341	216		216		The legend should mention what do the hatched regions correspond to. [Eric Brun, France]	Not applicable. The final figure has been completely revised and the hatch regions no longer feature there!
27343	216		216		This is figure is not consistent for runoff with Figure TS.20 for some regions (e.g. south of Spain, Morocco, North of Italia, American Southwest). [Eric Brun, France]	Not applicable - the figure has been completely revised and the issue no longer arises.
27345	216		216		We understand the usefulness of avoiding choosing a particular scenario but still information regarding how this map has been constructed would be useful. Is this a realistic schematic whatever the level of warming? If this is true then have we already started to see changes in those directions? [Eric Brun, France]	Noted. The intention here is provide a qualitative map of regions expected to experience further drying. Assessments of whether we have already seen observed changes may be found in the Chapter 8 main text.
40201	216				Fig FAQ8.3, although I like the idea of having a map as a figure, I have feeling that neither runoff or soil moisture fully capture the region that will likely get drier, as in the text there are various reasons (less precipitation, less snow, more evaporation) [TSU WGI, France]	Noted. However due to space restrictions we can only provide one map in the figure. The figure is now qualitative so that it can more generally highlight areas that will be affected.
10177	216				FAQ 8.3 Fig 1 and/or its caption should indicate that these are projected changes in soil moisture and surface runoff. Words like "changes", "differences", "projections" are entirely absent from the current caption. [Jacob Scheff, United States of America]	Noted. As this is a FAQ, the caption was edited by the TSU for the FGD and shortened. the phrasing "expected" was deeemed most accessible to a general audience.
70361	216				FAQ 8.3, Figure 1 - The depth range of soil moisture is not stated. Surface soil moisture is not the most relevant variable for drought and may differ from deeper soil moisture (Berg et al. 2017). Would be more appropriate to use a depth range that contains the rooting zone, could be either total column or integrated to 3m. [Abigail Swann, United States of America]	Noted. The final figure is qualitative but we are using total column soil moisture as a guide.
7051		15		15	Replace "it" on line 15 with "the SAM" for clarity. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The specific subsection for this comment has been identified and the suggested change applied
129281					Distinguish "precipitation" into the "precipitation amount" or else also account for the intensity, frequency, and duration (intermittency etc). [Trigg Talley, United States of America]	Done. “Precipitation” was changed where possible to “precipitation amount” and it was specified if we considered intensity, frequency or duration.
129283					[PRECISION] Authors need to more clearly summarize understanding of past changes and whether or not there is already evidence of an influence from anthropogenic forcing. A confusing issue here is the use of the word "trend". In reading Section 8.3, one is confronted with a somewhat bewildering set of statistics about various observed changes in different regions of the globe over different time periods -- often referred to as trends (or recent trends). In some cases, the time periods are identified, but not always. Figures 8.7 and 8.9 are examples where observed trends are displayed without information about the time period considered. At a minimum, having some agreement on terminology would be helpful here. The key point here is that decadal variability is almost certainly playing a role in many of these "recent trend" results. So, given the considerable decadal (and interannual for that matter) variability that exists especially at regional scales, a summary of what is understand about "time of emergence" and why certain regions already appear to be seeing the impacts of anthropogenic forcing rise above the noise of natural variability would be very helpful. Some mention of the special challenges imposed by the quality of the available observations of the various components of the water cycle would be helpful. [Trigg Talley, United States of America]	Thank you. In the FGD, the time period of the observed trends is mentioned in Figures 8.7 and 8.8. We also realize that decadal and interannual variability can contribute to observed trends. This aspect is discussed in the FGD [e.g., 8.3.1.3 (page 35, 37), 8.3.1.6 (page 41), 8.3.2.4 (pages 50-56)...]. Time of emergence in precipitation changes over Sahel is assessed in 8.3.2.4.3 (page 53). It is also assessed in 8.5.2.1 (page 106) that "There is low confidence in the region-dependent time of emergence of water cycle changes (see also Section 10.4.3), but there is medium confidence that changes in wet extreme events will emerge earlier than changes in average conditions."

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129285					This chapter should be checked for grammatical issues all the way through. The information reviewed was very strong and referenced appropriately. It's just the presentation that needs improvement. [Trigg Talley, United States of America]	Noted - The FGD has been hopefully improved in this respect.
113415					Adding a map of ET trends (not per-season) seems needed. Same as for P. [Diego Miralles, Belgium]	Thank you. We have included map of ET trends in the FGD (Figure 8.8). The map of precipitation trends is shown in Figure 8.7.
116745					Emergence and time of emergence are not strongly used in ch 8. [Valerie Masson-Delmotte, France]	Noted. Emergence and time of emergence are taken into account in ch 08 but with strong link and cross-reference to CH01 (where they are defined) and mostly to CH 10 where they are fully assessed
113169					Revise carefully the figure captions in this chapter. Thre are plenty of typing errors. [Diego Miralles, Belgium]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
21015					The chapter team has prepared a well structured and complete manuscript that includes a thorough explanation of the processes involved in changes in the water cycle as well as past and projected changes. My main concern is that the chapter keeps having substantial supperpositions with chapter 2,3, and 4, as I pointed out in the FOD revision. This resulted in a chapter well over 80 pages and should be shortened. A possibility to do so is to leave the discussion of large scale changes in variables considered in other chapters out (e.g. chapters 2,3,and 4). I believe the authors have done something along these lines in this version compared to the previous one, but additional cuts seem necessary. [Marcelo Barreiro, Uruguay]	Rejected - The chapter SOD IPCC length was exactly 80 pages. Nevertheless, a general effort has been made in the FGD to reduce overlaps when not needed and to build our assessment not only on the AR5 and SR but also on AR6 global chapters (Ch2/3/4).
113179					I aplaud the idea to include a water cycle chapter. Great initiative! [Diego Miralles, Belgium]	Noted - To be honest, we also fully support the idea but did not decide the AR6 WG1 outline.
13091					Need to reference labels of B&O'G and H&S as used in figure, within the caption. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
6697					There are references to "future projections" in quite a few places in this chapter. Most can probably be changed simply to "projections". See comment 9 on the entire report. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Thanks
13127					What is the red line? These are not labelled. Presumably red is the environmental temperature and blue the dewpoint? If so, why is the surface temperature warmed in the cloud-free but polluted environment? Wouldn't the reduced solar lead to cooling? What are the green plus/minus symbols indicating? CCL is also not defined. Caption needs a lot of work. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The comment fails to point to the specific place that it addresses.
64335					I miss mention to the catchments that are now ungauged. And i also miss mention to the transporse space by time paradigm [CRISTINA Prieto, Spain]	Taken into account. We do not fully understand this comment but space-time paradigm is discussed in updated version of section 8.3.1.5

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32861					Build some clarity on any possiblility of factors that trigger sea level rise contributing to water level rise in some fresh water resources such as Lake Victoria (One of the World's largest fresh water lakes). It expanded in width by about 15KM in some areas in the recent past submerging some islands while flooding neigboring communities across the three countries (Uganda, Kenya and Tanzania) sharing the basin. Islands on such large water bodies are also at a peril of submerging due to water level rise triggered by prolonged intense precipitation while neighboring communities suffer from floods. [Aaron Werikhe, Uganda]	Accepted, in terms of lake levels. Changing lake levels are discussed in Sections 8.2.3.1 and 8.4.1.7.3. Sea level rise is assessed in Chapter 9 and considered locally in Chapter 12.
130405					It would be very helpful to summarize for the non-specialist why addressing water cycle changes is more challenging than temperature changes, especially at regional scales (both fundamentally and as a result of model deficiencies); and how theoretical understanding of various physical constraints guides understanding of the uncertainties in the model results as a function of spatial scales (e.g., global, zonal means, regional), emphasizing the challenge of making regional projections. This is particularly needed for various projections where the uncertainty spans zero (e.g., Executive Summary, page 8-6, lines 26-33). Perhaps a few words on what those (+/-) ranges refer to would be helpful. There is also a need to more succinctly summarize what has changed regarding conclusions since AR5, both regarding past changes and the projections. [Trigg Talley, United States of America]	Taken into account - discussion throughout the chapter and in particular in Section 2 outlines the complexities and challenges in predicting how the water cycle responds regionally to a warming climate
64359					I think it is a wonderful chapter [CRISTINA Prieto, Spain]	Noted - Thank a lot!
72297					I think it is a wonderful chapter chapter but still some adustments should be done [CRISTINA Prieto, Spain]	Noted - Thanks
29323					very good work. [Zangari del Balzo Gianluigi, Italy]	Noted - Thanks
79505					The more detail discussion may be needed about big Dam construction projects effect on water cycle, in middle east and Africa (for example in turkey and its effect on surrounding countries like Iraq and Iran) (comment by: dr.entezam@gmail.com) [Hanieh Zargarlellahi, Iran]	Rejected - Rather the task of regional chapters or WG2
79507					A discussion about advance in projects like clouds trapping or cloud seeding may be needed , That How progress in these techniques may have effects on the water cycle in arid areas during coming years . (comment by: dr.entezam@gmail.com) [Hanieh Zargarlellahi, Iran]	Rejected - The main assessment of SRM is in Ch4 and cloud seeding seems to be here related to weather modification rather than SRM (Solar Radiation Modification) influence on regional climate.
83887					The chapter is generally well written and a welcome improvement since AR5 [Ulf Molau, Sweden]	Noted - Thanks
83889					The chapter is overloaded with graphs and maps (all high quality). Authors are urged to ones of primay importance (or perhaps combine some), leaving others to Supplementary materials. [Ulf Molau, Sweden]	Rejected - Compared to other chapters the number of figures is fairly limited.
53171					The first paragraph could be sufficient to introduce this section, but could finish as follows: "(…) global energy constraints (Section 8.2.1), thermodynamics and large-scale atmospheric circulation (Section 8.2.2), as well as smaller-scale physical porcesses (Section 8.3.3)." [Hervé Douville, France]	Rejected: the callouts to subsections is already provided in the introductory paragraph of Section 8.2.1
83909					It is really not very understable why floods are included among climate events. Floods results from climate events (and involves sevaral other non-climate factors). It would be more appropriate to say for example "floods linked events/extremes". Flood assessment is for WGII and not WGI. [Fatima Driouech, Morocco]	Rejected. While floods are not a focus area for the chapter and will be covered in much more detail in WGII, a brief physical-science based discussion is included here. This is because climate does play a primary role in many floods and because readers will be expecting some mention of floods in a chapter focusing on water.
115909					FAQ8.1 can this FAQ also link water cycle to greening / browning trends (vice versa)? (x Ch2, ch 5). Please check the use of the subscript for CO2. [Valerie Masson-Delmotte, France]	Subscript checked, regarding the greening/browning trends, the role of vegetation is considered but in more general way, as this FAQ has to consider many processes and mechanisms that modify the WC and Land use

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115911					FAQ8.2 The FAQ is focused on floods driven by heavy rainfall (could it be explicit in the title)? It seems that coastal flooding (also linked to sea level rise + extreme sea level change) could be integrated here (with ch 9) (it is shown on the figure but not discussed in the text). What about water and land management which can influence flood risks too (with WGII?). [Valerie Masson-Delmotte, France]	Accepted. Coastal flooding and water and land management are now discussed in the FAQ.
115913					FAQ8.3 could the FAQ also say something about drought metrics and the relative influence of precipitation deficit and temperature (in relationship with the choice of figures with soil moisture, and runoff here). What about a pattern (per °C of warming) rather than selecting a particular SSP? [Valerie Masson-Delmotte, France]	Noted. The text in the FAQ has been edited by the TSU for clarity for a general audience. The figure accompanying the FAQ shows a pattern of change.
116685					Congratulations for the maturation of the draft chapter. I encourage the chapter to strengthen links with chapter 3 on attribution (especially related to aerosol forcing), chapter 5 (coupling between energy, water and carbon fluxes, and feedbacks), chapter 6 (on SLCF) and chapter 7 (on feedbacks). Moreover, there is a need to integrate better the analyses of regional trends across chapters (including from chapter 8, with the regional chapters) (what is detected in observations; attribution to driver; trends of drivers especially when aerosols have a regional effect; projected trends depending on scenario and forcing). In terms of scenarios, and given the potential importance of SLCF, contrasting the two SSP7 scenarios (as done in ch 6) could be interesting. [Valerie Masson-Delmotte, France]	Noted - Thanks
116687					Congratulations for the maturation of the draft chapter. I encourage the chapter to strengthen links with chapter 3 on attribution (especially related to aerosol forcing), chapter 5 (coupling between energy, water and carbon fluxes, and feedbacks), chapter 6 (on SLCF) and chapter 7 (on feedbacks). Moreover, there is a need to integrate better the analyses of regional trends across chapters (including from chapter 8, with the regional chapters) (what is detected in observations; attribution to driver; trends of drivers especially when aerosols have a regional effect; projected trends depending on scenario and forcing) (maybe as a common Annex which each chapter could cite to avoid descriptions and duplications) (in the style of TS.10 table). In terms of scenarios, and given the potential importance of SLCF, contrasting the two SSP7 scenarios (as done in ch 6) could be interesting. [Valerie Masson-Delmotte, France]	Noted - Thanks
116693					Congratulations for the development of visuals which capture the chapter assessment. Please consider including the level of scientific understanding when possible. [Valerie Masson-Delmotte, France]	Noted - Thanks
112087					Some of the figures included in the chapter (e.g. Fig 8.15 and 8.22) can be also reproduced with the Atlas (some using the same monsoon regions). Some coordination for consistency check would be needed (taking also into account also technical aspects suchas as uncertainty communication, which is now done differently). [jose manuel gutierrez, Spain]	The pace of figure development did not allow for the requested level of coordination among chapters. Further, because figure developers used various data platforms, they may have used distinct sets of ensemble members. for similar figures, because they did not have the same data available ; this may result in slightly different figures. Hopefully would an AR7 be developed in a unique, comprehensive, technical framework, and with a more sensible pace than the one experienced during the pandemic.
64729					Land use change and water cycle feedbacks at regional level need to be addressed, they are of particular importance is centrin regions such the Mediterrnean Basin, Amazon Basin, Cango Basin etc. [Sanz Sanchez Maria Jose, Spain]	Taken in account, however given the fact that the FAQ is general review and there is a constrain in the number of words, there was not reference to specific regions on the final version
113141					This honest statement 'Model deficiencies and unresolved small-scale processes still preclude a strong model consensus about future water cycle changes whatever the scenario, time horizon or global warming level is' comes after having asigned 'high confidence to a number of water cycle changes in previous paragraphs. Are those reconcilable? What went wrong here? Perhaps this statement needs to mention 'regional'. [Diego Miralles, Belgium]	Noted. We have revised the ES statements and have couched uncertainties in terms of natural climate variability as well as limitations in modelled processes.

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129273					[ENSEMBLES] Because the new state-of-the science CMIP6 results were only made available late in the WGI AR6 process, there is an apparent need for authors to revisit how new findings and understanding are integrated into this chapter. [Trigg Talley, United States of America]	Noted - CMIP6 models will be used much more extensively in the next draft, including in figures, and the new findings will be further highlighted.
129275					[CONFIDENCE] It is critical to effectively communicate uncertainty of climate projections to policymakers, especially with the high signal-to-noise in the prediction and projection of changes in the water cycle in the near term of next few decades when natural variability can and likely will overwhelm expectations for long-term trends at regional spatial scales. [Trigg Talley, United States of America]	Noted - This has been emphasized in Section 8.5.2
129277					The Executive Summary generally follows the overall structure of this chapter, providing the physical basis for water cycle changes and discussing the observed changes, projected changes, and abrupt changes. This summary should include a small section on what limits ability to project future changes and the key knowledge gaps. It is useful to mention how confidence is defined even though other chapters may have already discussed this. For future projections, is confidence based on the robustness of the future projections across models and/or our understanding of the mechanisms of the changes? This chapter is mainly about water cycle changes associated with warming/cooling related to GHGs, aerosols, and LULCC. It also touches on anthropogenic influence related to irrigation, water management, etc, but at the outset, it should be clear about the scope of the chapter. At times, in the introduction, there are statements that lead the readers to think that the chapter covers both the direct and indirect anthropogenic impacts on the water cycle comprehensively. [Trigg Talley, United States of America]	Noted. We have endeavoured to do much of this, but a discussion of knowledge gaps is not included. Such material is contained in the "Final Remarks" section.
129279					Many captions do not provide enough information about the figures. While some figure captions are too short and do not provide enough information (e.g., Figure 8.3), other figure captions provide very detailed information about each panel, but they do not summarize the key messages. Figure captions are important as standing alone, they should provide sufficient information of what are shown as well as the key messages. Throughout the report, it would be beneficial to expand the use of CMIP6 model results if possible. Many more models now have results available on ESGF to be included in the analysis. [Trigg Talley, United States of America]	* a common guideline for figure and captions for this report is "captions should not be used to provide scientific interpretations – that should be done in the main text" [see §1.7.4 of AR6_WGI_StyleGuide_SOD_version] * more models will be included in chapter 8 final figures, according to AR6 panel guidelines and timeline; the analysis will be revised accordingly