

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1	3	0	0	0	0	Congratulations! Already in its present draft this chapter is mature, comprehensive and well elaborated; a great achievement which has been accomplished in a relatively short period of time. My comments below are generally minor. In addition to further shaping the text of this chapter, I think it will be important for the authors to review the other chapters and ensure that their assessment (as documented in Chapter 3) is consistently followed in the other chapters of the report! (Klein Tank, Albert, KNMI)	Thanks. Have provided Chapter 3 ES to other chapter CLAs to ensure consistency.
2	3	0	0	0	0	Some further structuring of the text is recommended, in order to avoid redundancy and in order to shorten the number of pages. I must admit that I had some difficulty reading until the very end. This is partly due to the fact that I am not an expert on areas such as waves, coastal impacts, glaciers, etc. Also, I suggest placing the details and examples further apart from the main messages and conclusions where possible. At present, a significant fraction of the literature is mentioned several times. This is due to the fact that each section is broken down into the three parts: observed changes, causes and projected changes. Most of the examples are from either Europe or Australia. Given the sparse information from other regions of the world (with the exception of North America), it may be difficult to avoid this regional bias. (Klein Tank, Albert, KNMI)	Noted. Have worked to remove duplication for SOD. Recruited CAs to improve geographical coverage.
3	3	0	0	0	0	NOTE: I am a contributor of Chapter 3, so I do not review this chapter (Cavazos, Tereza, CICESE)	Noted.
5	3	0	0	0	0	No comment (Jegillos, Sanny, UNDP)	Noted.
6	3	0	0	0	0	The wording has to be consistent within the whole manuscript: e.g., the expressions "return period" and "return times" are different expressions for the same issue (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Agreed. Have worked to ensure consistency in these terms. Have also noted in the SREX glossary that the term "waiting time" is equivalent to return period.
7	3	0	0	0	0	Deep-convection and associated extremes such as lightning, convective gusts, tornadoes, and hail causes high amount of damage. Besides, it is widely discussed whether these events have increased in past decades. Although this topic is of high relevance, it is only marginally discussed in SREX. Therefore, I suggest including a section, e.g. "3.4.5. Thunderstorm-related extremes" (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Noted. Have increased focus on these hazards. Hail was already considered in precipitation. In addition, small-scale wind events are newly considered in 3.3.3.
8	3	0	0	0	0	Emphasis is very much on large-scale and from an atmospheric perspective (climate, land-atmosphere) rather than a more balanced climate-hydrology section. (van Lanen, Henny A.J., Wageningen University)	Do not understand comment. There is already considerable focus on floods, droughts, etc.
9	3	0	0	0	0	The regions used for Africa (Figures 3.4 and 3.2) are appropriate and represent a good compromise between clarity and complexity (having too many regions). The definition captures the main areas of precipitation increase/decrease projected by GCMs, but with a little conflation of signals across West into Central Africa and East into the Horn, but I still think it's a reasonable breakdown to use (it's also consistent with earlier IPCC regions (Conway, Declan, University of East Anglia))	Noted.
10	3	0	0	0	0	I have only made suggestions for where additional examples for Africa might be sourced. I think overall the chapter is very good and provides a well balanced review and discussion of evidence. Given that there are so many uncertainties with projections and issues about attribution of trends/extreme events it will be important that conclusions of other chapters in the report are consistent with these messages. (Conway, Declan, University of East Anglia)	Thanks. The help with identifying African literature is appreciated.
11	3	0	0	0	0	I have reviewed sections 3.5.2 and 3.5.6, and I have no comments to these sections. (Rickenmann, Dieter, Swiss Federal Research Institute WSL)	Noted.
12	3	0	0	0	0	This chapter states some essential statements that could be mentioned earlier in the report. Important chapter with elements that should be evident in the introduction. Climate change does not necessarily mean more extreme weather events, which the other eight chapters make an impression of. (Asphjell, Torgrim, Climate and Pollution Agency (Norway))	Agreed. Much of the definitional discussions in Chapter 3 have been moved to Chapter 1, in a text section being drafted by Robert Muir-Wood.
13	3	0	0	0	0	A risk framework requires hazard quantification. This chapter will not meet the expectations of engineers and risk analysts as it does not provide estimates of sea-level changes at particular locations worldwide. Ideally, I would like to see a general sea-level prediction model (or several models) as functions of time with coefficients, and tabulated coefficients provided by cities worldwide. The table would also include standard deviations that account for both historical randomness and epistemic uncertainty for predictions in the future. Such information would help users to develop risk profiles and examine solutions. (Ayyub, Bilal, University of Maryland)	Rejected. Far too much detail for a report of this kind, and especially in a chapter limited to 75 pages (to cover all extremes and impacts).
14	3	0	0	0	0	Well written, clear and compact. No comments. (Bosello, Francesco, Fondazione Eni Enrico Mattei, Milan University \)	Noted.
15	3	0	0	0	0	Some Tables and Figures such as Table 3.1 and Fig. 1 - 4 should provide the main references. (Zhao, Zong-Ci, National Climate Center)	Noted. Where appropriate tables and figures now reference literature or sections in the chapter from which conclusions are drawn.
16	3	0	0	0	0	An overall comment on this chapter is that it would benefit from a more explicit bringing together of process understanding where that is possible. By partitioning each type of extreme into observations, causes and projections, there is both some repetition of information and some loss, in the latter case because there is not always clearly a sense of where the observational, attribution and projection studies are all consistent in terms of the mechanisms behind particular changes. Examples of where discussion of mechanisms would be helpful include cold air outbreaks, page 35 line 12-15; different changes in extreme wind speeds in tropic and extra tropics page 45 lines 19-22; weakening of tropical circulations, page 48 25-27. (Stott, Peter, Met Office)	Agreed. Subsections of 3.3-3.5 now include an introductory (short) paragraph discussing mechanisms and process understanding (where possible).

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17	3	0	0	0	0	It is recommended that for the better understanding of the text and reducing the vagues points ,more charts and figures be added. (Sehat kashani, Saviz, Atmospheric Sciences and Meteorological Research Center)	Now include more figures where appropriate, and replaced some from previous draft that were taken from AR4.
18	3	0	0	0	0	It is recommended that in the literature review except the studies done by the developing countries more references related to the studies done by other countries be added to it. (Sehat kashani, Saviz, Atmospheric Sciences and Meteorological Research Center)	Agreed. Authors have tried to broaden the geographical coverage.
19	3	0	0	0	0	All figures: are begging for zoom and recenter options (IPCC WGII TSU)	Noted. Figures have been improved.
20	3	0	0	0	0	Throughout: There needs to be a serious discussion about the uncertainty terms in this chapter and whether it will use the revised guidance. If it does, the sense one gets is that it should use likelihood in some places and confidence in many others. (IPCC WGII TSU)	Agreed. The chapter has been completely revised to match the new guidance on uncertainty This means that in some cases confidence is used, rather than likelihood. Overview is provided in new Section 3.1.5
21	3	0	0	0	0	Throughout: Almost none of the applications of calibrated uncertainty language in this chapter is transparent. For most, one gets a fuzzy feeling that the calibrated term is consistent with the sweep of the underlying material, but the stricy, quantitative, transparent basis of the assignment is almost never clear. (IPCC WGII TSU)	Noted. We have tried to indicate more clearly than in previous IPCC reports, exactly how we have reached our assessments. We have continued to work on this, although we additionally needed to reframe everything that had been done, wthin the new uncertainty guidance framework. A full overview is provided in new Section 3.1.5
22	3	0	0	0	0	The WGI Co-Chairs and TSU firstly want to congratulate the Chapter 3 authors on an excellent FOD and acknowledge the obvious hard work involved in restructuring this Chapter. This has been very beneficial, and has led to a much improved and now very clear structure. The overall responsiveness of the authors to the ZOD review comments is acknowledged and appreciated. Being a FOD, there is understandably room for improvement, and we offer guidance here with general comments, with many more detailed comments submitted in the formal review sheet. (Stocker, Thomas, IPCC WGI TSU)	Noted.
23	3	0	0	0	0	CHAPTER LENGTH: The length of the chapter must be reduced, in keeping with an overall need to reduce the report length. In particular, significant reductions can be made within sections 3.1 - 3.2 where there is a large amount of background 'text-book' style introductory material that is not crucial to SREX. Some specific suggestions of where text appears redundant or not crucial have been noted in the detailed comments, and more guidance with this will follow. In addition, reductions can be made by limiting the repetition of material already assessed in AR4. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Section 3.1. was very significantly shortened and Box 3.1 was removed and donated to chapter 1. Results in reduction of 30-40%. Section 3.2 also shortened.
24	3	0	0	0	0	FAQs and BOXES: Their placement should be reconsidered, because currently the reading of sections 3.1 and 3.2 becomes very disjointed. This is mostly because 4 Boxes/FAQs appear within the first 10 pages of the chapter. Please consider better positioning these throughout sections 3.1 and 3.2. Please also note: Many box titles are phrased as questions - this creates some confusion with FAQs and we hope all boxes can be re-titled to avoid this. Boxes should not be seen as "more technical version of an FAQ", but should provide important background details which would not fit well in the main text or even breaks the flow of the main text. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Several previous boxes have been reframed into subsections of Section 3.1 to improved the flow. Remaining Boxes and FAQs have been more appropriately positioned in the SOD. Also, titles of Boxes are not framed as questions anymore.
25	3	0	0	0	0	REGIONAL BALANCE: There are several regions which are not well covered within the assessment 3.3 - 3.5. In some instances, information is given in the tables or figures, but not in the text or vice-versa, or complete gaps exist. We suggest to explicitly mention whether this is because of a lack in available data/models, that makes the assessment difficult or even impossible, and if additional regional expertise are needed we encourage further use of Contributing Authors. This comment also applies to the summary tables where it needs to be explicitly mentioned what an empty cell means (no information, no change, no....). The most notable gaps in the text are (others may be noted in detailed comments): a) Temperature: Africa (obs and pro), Asia (pro), Australasia (obs) b) Tropical cyclones: Indian Ocean – Africa (obs and pro). c) Droughts: Asia (obs and pro), Australasia (obs). d) Wind: Central and south America (obs), Africa (obs). e) Extreme sea level: Projections only given for Europe and Australasia. Nothing for Africa or Asia. f) Waves: US and Europe focus. g) Coastal impacts: Small island state focus. (Stocker, Thomas, IPCC WGI TSU)	Noted. The authors worked (with additional CAs) to improve the geographical coverage. This was difficult in some cases, of course, because of scarcity of literature for some regions and because of the requirement to reduce the length of the chapter.
26	3	0	0	0	0	SECTION STRUCTURE: The setup of the individual sections throughout the chapter dealing with Climate Phenomena and/or Physical impacts (sections 3.3-3.5) should be homogenized; most of the sections follow the very good approach of having (1) status of assessment in AR4, (2) assessment of new science since AR4, (3) key conclusions and summary of how AR4 assessment needs to be revised (if at all). This setup makes a lot of sense and should be applied throughout the chapter (and actually in other chapters too, in particular Chapter 4). (Stocker, Thomas, IPCC WGI TSU)	Agreed. Have restructured each subsection somewhat, and ensured more consistency. Titles within each subsection (eg "projected changes") have been removed. Each subsection now consists of: introduction, mechanisms/processes, paleo work, observed changes, attribution, projected changes. each of the last three start with the AR4 position. A summary paragraph completes each subsection.

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27	3	0	0	0	0	CONSISTENT TREATMENT OF UNCERTAINTY: The most effective subsections within 3.3 - 3.5, contain concise summaries in which likelihood statements are clearly given in italics, or, it is commented that no assessment is possible. Such statements are missing for some extremes. In some cases, likelihood statements appear in Table 3.1, and executive summary, but not in the corresponding section within 3.3 - 3.5. The assessment of uncertainty to specific findings and the use of the IPCC uncertainty language needs to be consistent throughout the text; the result of such an uncertainty assessment should be highlighted by consistently putting the words "likely" etc. in italics. Only use these words in relation to the formal treatment of uncertainty. (Stocker, Thomas, IPCC WGI TSU)	Agreed.
28	3	0	0	0	0	UNCERTAINTY BOX: Uncertainty is a crucial theme for the entire SREX. A box is needed where the basis for the likelihood and uncertainty language used in SREX is provided, and we recommend this should appear in Chapter 1, eg, Box TS.1 from AR4. Current text relating to this (lines 28-54, p. 27, and lines 39-51, p. 30) should be moved to this box, unless it is Chapter 3 specific, and therefore needs to remain outside the general box and be introduced specifically in Chapter 3. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Discussions were conducted with Chapter 1 to move some chapter 3 text to chapter 1 (text sections revised by Robert Muir Wood). Some chapter 3 specific text was retained.
29	3	0	0	0	0	ANNUAL MEAN vs SEASONALITY: For some of the Phenomena/Physical Impacts the coverage of the seasonality should be strengthened. Many of the Extremes Events have a strong seasonal component and e.g., projections of the annual mean are not the most relevant information needed in risk assessments. The seasonal component of the assessed changes thus needs to be highlighted much more. To highlight this in the structure of the chapter, we suggest carefully arranging the paragraph structure within the various extreme sections to provide a clear separation between text assessing seasonal and annual changes in the various extremes. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Have focused more on seasonality, where possible (see new figures: 3.1, 3.4, 3.5, 3.7, 3.10).
30	3	0	0	0	0	PALEOINFORMATION: There is still a lot to be done regarding the inclusion of Paleoclimatic information about extremes in the parts dealing with observations and causes behind the changes. We suggest to add more emphasis on this currently underused source of information in the revisions. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Each subsection in 3.3-3.5 now includes wherever possible a short paragraph on relevant paleo information. This provides some information about natural variability to place recent changes within this context.
31	3	0	0	0	0	PROJECTIONS: When referring to results from climate models, it is important to specify which models and which scenarios the results are based on, as well as what year of the projection the results are from. This is particularly important, as for example, the uncertainties in scenarios should not be mixed with the uncertainties in scientific understanding. Currently this information is often missing in Chapter 3 and thus needs to be added. (Stocker, Thomas, IPCC WGI TSU)	Noted. Have improved this. In particular new figures 3.6 and 3.8 include scenario-specific information, and distinctions between scenarios are now mentioned in the ES. See also information in new Box 3.1. But it was challenging to provide much more information, given the need to reduce the length of the chapter, and to ensure it was readable by the target audience.
32	3	0	0	0	0	SREX OVERLAP: There is quite some overlap between Chapter 3 and 4 with regard to the assessment of Climate Phenomena/Physical Impacts. Most of the overlap, it seems to us, comes from the fact that Chapter 4 reassesses the Science assessed in Chapter 3 instead of referring to the assessment in Chapter 3. This should clearly be avoided and coordination between these two chapters should be strengthened. This coordination, at much smaller levels however, should also be done with all the other chapters to avoid reassessments of the Science assessed in Chapter 3. One effective mechanism to avoid some of the inaccurate, general, overarching statements appearing in many Chapters, would be for Chapter 3 to draft a series of specific key messages that other chapters could repeat in these instances. (Stocker, Thomas, IPCC WGI TSU)	Noted. We did this at the time of the ZOD, and early in the process of drafting the FOD. We have also again distributed our key messages (in the form of an early draft of the ES) to other chapters well before deadline for SOD.
33	3	0	0	0	0	SEA LEVEL/ WAVES/ IMPACTS: We would favour the merging of sections 3.5.3 and 3.5.4 into a single "Extreme Sea Levels and Waves" section. It is not clear why this current separation is necessary. Section 3.5.5 "Coastal Impacts" could be removed, with this material left for Chapter 4 to consider. Currently there is significant overlap with Chapter 4 regarding 'coastal impacts', and we feel Chapter 4 is the more appropriate chapter to provide this assessment. (Stocker, Thomas, IPCC WGI TSU)	Reject. Physical coastal impacts are clearly Chapter 3 material, not Chapter 4. We have worked with Chapter 4 to minimise duplication in this area. Literature and concepts and impacts are rather different for 3.5.3 and 3.5.4, so authors consider two subsections appropriate.
34	3	0	0	0	0	OPEN OCEAN: There is limited coverage of 'open ocean' in Chapter 3, although this is a topic/region that is explored in Chapter 4 particularly. Chapter 3 authors or CAs with relevant experience should be prepared to review/rewrite these sections appearing in other chapters. (Stocker, Thomas, IPCC WGI TSU)	Reject. Extremes in the open ocean are, by definition, not related to disasters - until they impact on a coast (and there they are covered in 3.5.3 and 3.5.4).
35	3	0	0	0	0	FIGURES: Figures can be significantly improved in many instances and detailed comments are given. As a general comment, it is questionable whether the precious SREX page space should be used to simply reproduce figures from AR4. It would be great if such figures (eg, figures 3.7 and 3.8) could be updated with new higher resolution model results. Relevant authors could be contacted and may be able to help update these figures, eg, Meehl, Arblaster, etc. We would also strongly encourage that a relevant and informative figure is added to the Tropical Cyclone section. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Have removed AR4 figures. More figures have been added.
36	3	0	0	0	0	PAST/PRESENT TENSE: Throughout the individual sections there seems to be a back and forth between past and present tense: suggest to decide on either/or and be consistent all along. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Have worked towards a consistent use of tense.
37	3	0	0	0	0	The main critical statement on chapter 3 is that it could be shortened in order to mainly pay respect to newest research results in higher detail and older research results in far less detail. The uncertainties highlighted in this chapter should be taken stronger into account in the previous and the following chapters. (Schmidt-Thome, Philipp, Geological Survey of Finland)	Agreed. Previous text was shortened and we increased the focus on new results. However, the audience this report is aimed at requires some background material.

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38	3	0	0	0	0	This Chapter is done properly for the selected way of presenting climate change extremes. The focus is on broad climate change. In my opinion considerably more useful will be to present (a) most significant processes related to particular disasters and then show how is climate change going to affect them. For example select flooding, identify key processes (rainfall, evaporation, infiltration, etc...) and elaborate how is climate change affecting these processes. I am missing a very important point in this Chapter that climate change is global but extreme events have local significance. How to relate these scales. (Simonovic, Slobodan, University of Western Ontario)	Noted. Many possible ways to arrange the material. Authors believe the current version does focus on specific extremes related to disasters. Proposed rearrangement not obviously any improvement for readers.
39	3	0	0	0	0	In generell links to other chapters should be made (Luterbacher, Juerg, Justus Liebig University)	Agreed. But other chapters should be linking to assessments in this chapter.
40	3	0	0	0	0	Replace "AR4 MME" with CMIP3 as the multimodel archive was an initiative of WCRP and CLIVAR, not IPCC (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Noted. Was changed.
41	3	0	0	0	0	RCM intercomparisons such as ENSEMBLES, NARCAAP and CORDEX are rarely mentioned by name which I found surprising as this can help with interpretation (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Noted. However, chapter authors were instructed to remove these names within the ZOD review.
42	3	0	0	0	0	The general structure in 3.3 -> 3.5 is good overall but the causes behind projected changes gets somewhat buried in the single heading of "Projected changes and uncertainties". Suggest splitting this section into "Projected changes" and "Uncertainties and processes behind projected changes" or something to that effect. (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Noted. We used this arrangement originally, but reviewers for the ZOD suggested this led to duplication and recommended treating these two aspects jointly.
43	3	0	0	0	0	It strongly needs to describe in the Asian/Pacific small-islands region where there are much more low-land areas. (NISHIMORI, Motoki, National Institute for Agri-Environmental Sciences)	Agreed. A new box now addresses small island issues.
44	3	0	0	0	0	The report is very descriptive. It is better to present more figures for introducing indices ,.. more tables . This chapter should make an excellent attempt to improve the quality of figures including their coloures, fonts, legend...and also the tables. The chapter should make and effort for considering a fair for presenting the information in the regions. It was not considered yet. The chapter can be made less word intensive avoiding duplications and less relevant details. (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	Agreed. More work has been undertaken to cover more regions. More information is now provided in the tables and maps.
45	3	0	0	0	0	It seems that the Gery literatures were not used. Using this literature should be improved in this report. (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	Noted. Authors are trying to avoid grey literature (ie literature that has not been peer-reviewed) as much as possible. But we have used peer-reviewed reports where necessary.
46	3	0	0	0	0	The full report should be survey to identify overlaps. (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	Agreed. Many overlaps have been removed for the SOD.
47	3	0	0	0	0	It seems that Grey literature were not used. (I will described it later in not using the grey literature for projection of extreme event in west Asia) and the other subjects. (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	Noted. See response to comment #45.
48	3	0	0	0	0	A general comment is the quite frequent repetition of the words "more likely than not" which in some cases devaluates the statistical significance of some statements (Zerefos, Christos, Academy of Athens)	Noted. This terminology is now used less frequently because the chapter has been revised using the new uncertainty guidance. As a general rule, we now replace such statements with "medium confidence" of a sign of change. See also new Section 3.1.5
49	3	0	0	0	0	no comment (Yasseen, Adel, Ain Shams University - Institute of Environmental Research and Studies)	Noted.
50	3	0	0	0	0	This chapter is very long and somewhat wordy. The two introductory sections (3.1 and 3.2) were particularly long and difficult to read. There was 30 pages of this dense/opaque material before any real results were presented. I suggest that these section be shortened by at least 20%, but preferably 40%. I have given one example of a heading that can be reduced by 50% by a simple change of words. The text is littered with these type of convoluted constructions. Sections 3.3 on were somewhat better (Church, John, CSIRO)	See answer to 23
51	3	0	0	0	0	Throughout the chapter, low and high latitude regions are described without any specific description of the latitude range. I suggest inserting specific latitudes through the chapter. (Church, John, CSIRO)	Note. Have included specific lats/longs where appropriate. But often this detailed information is not required.
52	3	0	0	0	0	In overall, each section of IPCC SREX Chapter 3 is documented well according to the current understanding of climate change since the AR4. However, regarding the uncertainty of the extreme weather and climate elements, it sounds rather vague through whole Chapter. Though, we acknowledge the difficulty to express this in definitely, but it is desired more to make clarify the uncertainty with quantitatively. Otherwise the Chapter is not effective for the overall risk assessment and management for the future changes including the direction and magnitude in extremes depends on the type of extreme. (TANAKA, Tadashi, University of Tsukuba)	Noted. Using the new uncertainty guidance led to more quantitative assessments in some cases. Also we now include some quantitative assessments on changes in return periods and return levels of extremes (with necessary uncertainty ranges) for changes in temperature and precipitation extremes.
53	3	0	0	0	0	Chapter is well written and generally explains the science clearly (I have examined the drought & floods sections in most detail) (Bell, Victoria, Centre for Ecology and Hydrology)	Noted.
54	3	0	0	0	0	Studying chapter 3 of the book entitled "Managing the Risks of Extreme Events and Disasters to Advance Climate Change Adaptation", I found it informative, technological and practical; however, I appreciate your consideration on the cases as follows: (Davtatab, Rahman, Ministry of Energy)	Noted.

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55	3	0	0	0	0	It is an interesting review article but isn't it supposed to be all-encompassing? 1. Where is information on precipitation types? This is a huge issue for mid-high latitude regions. Perhaps I missed it but I do not recall seeing the word SNOW, let alone FREEZING RAIN and WET SNOW. A closely related phenomena is 'rain on snow' events. 2. Where is information on cloudiness? Extended period of cloudiness can have major implications on people. Think of Vancouver; months of cloudiness affect people's well-being. (Stewart, Ronald, University of Manitoba)	Noted. Added some more on frozen precipitation where feasible and appropriate (although snow is not always an extreme; nor does it always lead to disaster). Note comment #58.
56	3	0	0	0	0	There is no need to reference any of these but some of my own papers that are somewhat relevant include the following. Many others have of course contributed to cold season issues. Gearheard, S., M. Pocerich and R.E. Stewart, J. Sanguya and H.P. Huntington, 2009: Linking Inuit knowledge and meteorological station observations to understand changing wind patterns at Clyde River, Nunavut. Climatic Change. DOI: 10.1007/s10584-009-9587-1. Note: This comments on, for example, some of the issues linked with observational deficiencies. In fact, the whole review has completely missed reference to 'traditional knowledge'. Nawri, N. and R.E. Stewart, 2008: Short-term temporal variability of atmospheric surface pressure and wind speed in the Canadian Arctic. Theor. Appl. Climat. DOI: 10.1007/s00704-008-0098-1. Note: Some implications for using surface pressure gradients and implications for surface winds. Roberts, E., N. Nawri and R.E. Stewart, 2008: On the storms passing over southern Baffin Island during autumn 20, Arctic, 61, 309-321. Note: Several instances of record-high temperatures were observed. Henson, W. and R.E. Stewart, 2007: Severity and return periods of icing events in the Montreal area. Atmos. Res., 84, 242-249. (Stewart, Ronald, University of Manitoba)	Noted. Thanks.
57	3	0	0	0	0	Chapter 3: The separate treatment of "wind" in Section 3.3.3, of "tropical cyclones" in Section 3.4.4, and of "extra-tropical cyclones" in Section 3.4.5 may be justified from a scientific point of view but increases the risk of presenting inconsistent messages about changes in extreme winds, including cyclones. Statements in the current draft Executive Summary suggests that this risk is real. If it is impossible to integrate these three sections, there is a strong need for ensuring complementarity as well as consistency. (Fuessel, Hans-Martin, European Environment Agency)	Noted. We have worked to ensure consistency between these sections.
58	3	0	0	0	0	The chapter bravely tries to cover too many things and this leads to it being a very long chapter that is unpleasant to read. 140 pages for a chapter is excessive and a more concise chapter would be more effective. For example, even the executive summary is 3 pages long which by any standards is too long for a summary. The authors should try to make the chapter more concise by a) better defining the scope of the chapter, and b) removing ALL unnecessary dead wood. (Stephenson, David, University of Exeter)	Noted. The chapter has been reduced in length. But we are expected to be comprehensive, and many other reviewers wanted even more events/hazards included. ES of 3 pages is typical for an IPCC chapter of this length.
59	3	0	0	0	0	A clear definition of what is meant here by an extreme event as opposed to a rare or severe event needs to be given. The definitions given in the summary and Section 3.1.1 "(lines 1-41) could benefit by referring to clearer expositions such as: Stephenson, D.B. (2008): Chapter 1: Definition, diagnosis, and origin of extreme weather and climate events, In Climate Extremes and Society, R. Murnane and H. Diaz (Eds), Cambridge University Press, pp 348 pp. It is not true that from a statistical perspective extreme events are equivalent to rare events (lines 1-3 of page 6), for example, observing a temperature of 3.1415926C is a rare event but hardly extreme! Another point the authors should note is that extreme events are not binary in nature – they form a continuum. Just because exceedances above thresholds are used to infer properties of the tail, it doesn't mean we should think of extremal properties of a process in a binary way." (Stephenson, David, University of Exeter)	Noted. Much of the definitional text has been moved to chapter 1.
60	3	0	0	0	0	The statistical methodology used in climate science to quantify extreme behaviour needs to be clearly explained either in this chapter or in a special annex to the book. Essentially three approaches are currently used in climate science: 1. So-called "extreme" indices – crude sample statistics often based on sample means above pre-defined thresholds, 2. Extreme value theory models fitted to either maxima or peaks over high thresholds, 3. Inference of extremal behaviour from likely changes in bulk properties of location and scale. The three approaches are not guaranteed to give similar conclusions so it should be made clear which approach is used when referring to changes in extremes. (Stephenson, David, University of Exeter)	Noted. Have tried to do this better, without increasing the length of the chapter (note same reviewer believed chapter was already too long - see #58). Some material is now covered in Glossary and Chapter 1.
61	3	0	0	0	0	Throughout the chapter, PDF is used to "Probability Distribution Function" where in statistics p.d.f. is used to mean the more precise "probability density function" (i.e. a smooth function that when integrated gives the probability distribution function). I would suggest that the authors replace all occurrences of PDF with the generic phrase "probability distribution" UNLESS they are specifically referring to the probability density function. For example, on line 42 of page 8 where a quantile should be defined in terms of the probability distribution not the density function. Histograms such as those presented using lines in Fig. 3.5 should be referred to as "histograms" since they are not smooth probability density functions. (Stephenson, David, University of Exeter)	Agreed.
62	3	0	0	0	0	The chapters seem to have different structures (for example, this one has an executive summary, others do not) (Trewin, Blair, Australian Bureau of Meteorology)	All chapters will have an ES.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
63	3	0	0	0	0	If new regional analysis at sub-continental scale is to be undertaken specifically for SREX, then consideration should be given to consistency with previous reports. WG II in the TAR agreed on 32 sub-continental regions that are similar to but not the same as the Giorgi/Franciso regions used frequently by WG I. WG II authors at the time of the TAR recognised that contiguous regions defined by G/F were not always regionally appropriate, either climatologically or in terms of potential impacts. Hence, the regions adopted in WG II for TAR and AR4 (see for example, Chapter 2, P. 150-151) might be considered for follow-up analysis in SREX rather than selecting yet another set of regions. (Carter, Timothy, Finnish Environment Institute (SEVEI))	Authors of Chapter 3 considered the AR4 regions were not always appropriate and thus revised the regions (thereby continuing the efforts at redesign started by WGII in the AR4).
64	3	0	0	0	0	In general, it is crucial to inform the readers about which problems, uncertainties etc. are in principle unknowable and which uncertainties might be reduced to what extent by further scientific improvements and research (see e.g. comment no. 2 above) (Neu, Urs, Swiss Academy of Sciences)	Noted. We have tried to do this (see revised section 3.2) - although very difficult within the space limitations of the chapter.
65	3	0	0	0	0	I'm not very happy in the widespread use of referencing chapters of IPCC AR4, particularly as this is not done in a consistent way. Sometimes original research that contributed to an AR4 conclusion is quoted explicitly and sometimes not. This not only makes for rather unexciting reading, it also will not help this document be a reverence for scientists in the field as they will have to go through another review document to get to the original. A further argument against quoting the AR4 is the current credibility issue with IPCC reports. I would have thought it would be far more authoritative to quote original research rather than IPCC processed views. There can then be no criticism that the truth has been lost in the writing process. I accept that there will be situations where an AR4 review conclusion needs to be quoted but the current document seem to use this far too frequently. (Brown, Simon, The Met Office Hadly Centre)	Rejected. We cannot repeat the assessment done for AR4. Where new research has indicated a change in the assessment we make this clear.
66	3	0	0	0	0	I am concerned of the general lack of representation of the scientific advances contained in the 2009 UK Climate Projections and where they are mentioned the impression given is wrong. In preface to this comment I notice the US Climate Change Science Programme is quoted extensively. To date the UKCP predictions are the most comprehensive assimilation of all the different known uncertainties pertaining to regional predictions and they do contain information on extremes in contrast to what is currently in the text. The extremes of particular note are hottest, coldest and wettest summers or winters day and hottest and coldest nights, which correspond to the 99th percentile, more extreme than many of the results quoted in the text. UKCP09 is the first attempt to provide information on probabilities of levels of change in extremes and needs to be included in a review of extremes such as this. In no way am I arguing that these projections are perfect, far from it but they do represent a significant development in the provision of information of future extremes. Finally do not make the mistake of dismissing these projections as being from a single model Perturbed physics ensembles and multi-model ensemble information are incorporated in the results. For more information see http://ukclimateprojections.defra.gov.uk/content/view/12/689/ and more specifically annex 2 of the science report http://ukclimateprojections.defra.gov.uk/content/view/2087/500/ (Brown, Simon, The Met Office Hadly Centre)	Noted. Not obvious that much space should be given to the results of a single small-country study. Section 3.2.3.3. mentions this study in the case of the use of a new methodology.
67	3	0	0	0	0	I like the new structure of the Chapter, and commend the authors on the substantial improvements that have been made since the Zeroth order draft. I think the chapter is in good shape for this stage of development in the assessment process. (Zwiers, Francis, Environment Canada)	Noted.
68	3	0	0	0	0	I feel that the chapter is both too long and too short. In my view, too much space is allocated to Sections 3.1 and 3.2, while there is insufficient coverage of some aspects of Sections 3.3-3.5. While not meaning to diminish the enormous effort that this draft represents, I nevertheless have the impression that its coverage frequently tends to be Eurocentric and, to a somewhat lesser extent, Australia centric. This maybe unavoidable if literature covering other regions is not available, but I suspect that this is not entirely the case. (Zwiers, Francis, Environment Canada)	Agreed. The restructure has meant that all introductory material had been collected into sections 3.1 and (especially) 3.2. This has meant that 3.2 in particular was very long and much material was duplicated. This was not a problem in the previous structure. We have shortened 3.1 very significantly (see also answer to #23) as well as 3.2. We have also worked with CAs to improve geographical coverage (within the confines of length limits on chapter)
69	3	0	0	0	0	Even if the coverage is not comprehensive, the chapter nevertheless does a great deal of descriptive reporting. It would be good if this could be made more concise in places, and if the authors did more assessment, indicating which aspects of the results that are described are more or less credible, and more or less consistent with other results. (Zwiers, Francis, Environment Canada)	Noted. Have worked on doing more assessing and less descriptive reporting.
70	3	0	0	0	0	A more specific comment is that the chapter needs to take a consistent view on what it means by external forcing. In the IPCC context, this means external to the climate system (as a whole, from sources such as increasing ghgs), as opposed to external to a region or system (e.g., the short term forcing associated with ENSO in a given phase or a blocking episode). (Zwiers, Francis, Environment Canada)	Agreed. Have worked to ensure consistency.
71	3	0	0	0	0	There has been no mention, anywhere in the chapter, of UKCP09 (Zwiers, Francis, Environment Canada)	See response to #66.
72	3	0	0	0	0	Is it always clear from the context whether the word "significant" means "statistically significant" or significant in some other sense? I ask the question because the word significant is often used in this chapter in contexts where it is implicitly understood to mean statistically significant. (Zwiers, Francis, Environment Canada)	Noted. Have tried to use "statistically significant" and terms such as "substantial" to avoid possible confusion.
73	3	0	0	0	0	The Chapter as a whole has a strong focus on definitions and methods (30 pages) and is repetitive at times (many overlaps). (Brönnimann, Stefan, University of Bern)	Agreed. Have reduced 3.1 and 3.2.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
74	3	0	0	0	0	Overall, Chapter 3 has done an outstanding job covering a diverse set of contentious topics in a balanced manner. I was especially impressed with the treatment of tropical cyclones and droughts, among other sections. Well done! The authors will probably be pressured to weaken or strengthen various statements, but in my view they've already got the required balance in nearly all cases - I point out some important exceptions to this below. I will also offer below several specific comments on particular sections where I found problems or lack of clarity. I also want to suggest that the authors go back over the whole chapter and seek to shorten/remove repetition. In some spots, there seemed to be a lot of repeating what had been said already; I indicate a few examples and urge the authors to search for more. (Solomon, Susan, NOAA)	Noted.
75	3	0	0	0	0	A major problem with chapter 3 are a series of statements stating that extreme impacts occur in association with threshold phenomena. That may be plausible but it is not sufficiently backed up anywhere in the chapter. Unless clear referencing can be provided for this remark (and please do so across a broad range of impacts, not just one or two, if you are going to be make such an important statement), they will need to be removed or greatly modified. I give examples below. (Solomon, Susan, NOAA)	Noted. Some of this was moved to Chapter 1. We also clarify better the role of thresholds vs linear changes in Sections 3.1.2 and 3.1.7
76	3	0	0	0	0	I had hoped to find more in this chapter on extreme seasons - in particular extreme hot summers. While daily extremes or heat waves are far more difficult to quantify, the likelihood of extremely hot summers increasing with climate change seems more straightforward since it would require a narrowing of the distribution to avoid an increasing frequency of warmer summers. See the NRC stabilization targets report (available at http://dels.nas.edu/Report/Climate-Stabilization-Targets-Emissions-Concentrations/12877); figures in the summary and main body of the chapter show estimates of changes in very hot summers. (Solomon, Susan, NOAA)	Agreed. Have included more on extreme seasons, where appropriate. However, not all extreme seasons are relevant to this report, especially extremely hot summers. As an example, in Melbourne Australia the summer of 09/10 was extremely warm with a record number of days over 20C. But there were few extremely hot days and so there was only limited risk of "disasters" through this summer. By contrast, the previous summer (08/09) was relatively cool but included two periods of record hot days that did lead to disasters (a heat wave of 3 days of extreme heat followed a week later by Black Saturday). So there is not a one-to-one correspondence between extremely hot summers and disasters (which is the focus of this report).
77	3	1	1	143	0	Chapter 3 and 4 are mixed up. While the former contributes from the climate-only potential effects, the fourth integrate climate and human-effects (global change). There is a large degree of repetition between the two chapters. At least chapter 3 should incorporate comments on the implications of human as synergic component of the effect of climate (SERGI, SABATFR University Girona)	Reject. Chapter 3 is forbidden from examining human impacts.
78	3	1	10	1	13	I would prefer El Nino-Southern Oscillation (abbreviated to ENSO after the first use) as this will also include extremes associated with La Nina. (Trewin, Blair, Australian Bureau of Meteorology)	Refers to page 2. Agreed.
79	3	1	23	1	23	Item 3.1 is not mentioned on this content page. It is mentioned on Chapter 3, page 5, line one s Weather and Climate Events Related to Disasters, but not here. (Wen, Jet-Chau, National Yunlin University of Science and Technology)	Formatting error. Will fix.
80	3	1	29	0	0	the sentence should be written as a title rather than in an indicative sentence as follow: "Categories of weather and climate events". And also this correction is needed for lines 33 and 36. (Davtala, Rahman, Ministry of Energy)	Reject. Proposed title too inclusive.
81	3	1	44	0	0	moisture can be added to the observed changes in climate (Incecik, Salahattin/Selahattin, Istanbul Technical University)	Moisture covered in 3.3.2, as precipitation extremes.
82	3	1	48	1	48	It's not only attribution - literature on the observed changes in strong winds themselves is very limited. (Trewin, Blair, Australian Bureau of Meteorology)	Agreed.
83	3	1	0	172	0	In previous chapters, a term "cost" usually refers to "opportunity cost", and a phrase "economic loss" are used to mean "damage cost", which is used in this chapter. Although "damage cost" is simply defined as the same as "economic loss" in page 70 line 50, this definition seems contradictory to the previous emphasis on the concept of "opportunity cost". Additionally, a phrase "cost of adaptation", which is compatible to opportunity cost, is often used in this chapter, so I would like to recommend avoiding the use of "damage cost" and using "economic loss". Or at least, more extensive and precise explanation of the concept of "damage cost", which is not compatible with opportunity cost, should be given. (Kondo, Masahide, University of Tsukuba)	Comment refers to a different chapter.
84	3	2	1	2	1	"important forcings" may be too strong - consider "important potential forcings"? (Trewin, Blair, Australian Bureau of Meteorology)	Comment refers to page 3; agreed. Add "potential".
85	3	2	1	4	40	It is noted that no linkage to subchapters have been included like in executive summaries of other chapters. It is suggested to include such linkages in the future. (Radunsky, Klaus, Umweltbundesamt GmbH)	Agreed.
86	3	2	1	4	40	ES: It will be important to check all of the statements in the ES against the revised uncertainty guidance (IPCC WGII TSU)	Agreed.
87	3	2	1	79	13	The key points should be indicated more clearly and the take home messages should be more short and precise (e.g. in bullet points) (Ammann, Walter J., Global Risk Forum GRF Davos)	Agreed, but probably not in bullet points.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
88	3	2	1	0	0	Executive Summary: the ES is far too long and needs to be cut down to only cover the key messages of the chapter; one suggestion would be to cut most of the repetition from AR4, which won't be lost but can still be found in the main chapter text. (Stocker, Thomas, IPCC WGI TSU)	ES very similar length to WGI AR4 chapters of similar length. AR4 conclusions were removed.
89	3	2	1	0	0	Executive summary: I suggest to harmonise the structure of the various paragraphs (e.g., p2 L25-37 begins with changes in the past, while p3,L41-21 begins with the causes behind the changes) (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Agreed. But ES structure has been substantially revised for SOD.
90	3	2	1	0	0	Executive summary: Research since AR4 mainly focuses on (i) high-resolution regional climate modelling for specific regions to better resolve relevant processes and (ii) considers larger ensembles of climate models to reduce and quantify uncertainty. These approaches should be mention in the summary. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Because of space issues, no references are provided specifically on AR4 in the present ES (see also #88)
91	3	2	1	0	0	Executive Summary: The Executive Summary requires substantial shortening and focusing. Most likelihood assessments appear rather conservative. A revision (i.e., strengthening) of some likelihood statements from the IPCC AR4 appears justified based on new results in the post-AR4 literature (e.g., p. 2, ll. 40-42). (Fuessel, Hans-Martin, European Environment Agency)	ES structure being substantially revised for SOD. Disagree about conservative nature of assessments.
92	3	2	1	0	0	Executive Summary: This section very clearly summarizes the state of the science regarding different types of extremes, and what types of events are covered in the chapter. Much of the presentation focuses on increasing support for conclusions from the AR4, often expressed in terms of likelihood statements. Authors should consider whether the new evidence available since the AR4 alters the likelihood given for key findings rather than simply increasing confidence in the same likelihood assignment made in AR4. For example, in the sentence "New studies since AR4 have substantially strengthened the AR4 assessment that it is more likely than not that anthropogenic influence has contributed to a global trend towards increases in the frequency of heavy precipitation events over the second half of the 20th century", does this new information allow the authors to change "more likely than not" to a stronger likelihood statement indicating a higher degree of certainty? Also, authors should consult the new uncertainty guidance that will be available by LAM3 and consider how this revised guidance may affect presentation of uncertainty in key findings of this chapter. Under the new guidance, likelihood statements should be employed where supported explicitly by probabilistic information, and otherwise, confidence statements should be employed to qualitatively describe the degree of certainty authors have in a given finding. Authors should consider whether this suggests modification of any of the current uncertainty statements. (IPCC WGII TSU)	Likelihood assessments were revised based on new guidance. Authors have considered whether post-AR4 evidence allows us to increase strength of assessments, or requires revisions of the assessments. ES structure has been substantially revised for SOD and due to space issue does not refer to AR4 material anymore (see #88).
93	3	2	2	2	13	It might be good to say something about the temporal scale in the Executive summary. Do extremes have to be disruptive or can they be continuous or recurrent? Is sea ice decline included? Is drying-out of lakes included? Is the ozone hole included? (Brönnimann, Stefan, University of Bern)	ES structure has been substantially revised for SOD. More detailed definition of extremes and associated issues is provided in Sections 3.1.1. and 3.1.2.
94	3	2	2	0	0	The report needs to define both the terms 'weather' and 'climate' as well as 'extremes'. A great deal of effort is going into explaining the difference between weather events and climate change, and this report is not sufficiently precise regarding these definition, but rather bungles these together. Likewise, it is important to set the right frame of mind on extremes: they are rare, often intense, and with an irregular recurrence. (Benestad, Rasmus, The Norwegian Meteorological Institute)	See response to comment #93.
95	3	2	3	2	13	What is the difference between a "weather event" and a "climate event"? The climate is the average weather. The only answer I can think of is that a climate event is an extreme weather event that is so extreme that it alters the climate. But often in this chapter the term seems to have no particular meaning. See also my comment on Ch 4 Page 1 Line 27. (Cogley, J. Graham, Trent University)	See response to comment #93.
96	3	2	3	2	13	It would be valuable to state in this paragraph that events in the tails of a present-day probability distribution may not be "extreme" once the climate has changed. That is, it is important to define extremes with respect to a stated base period. The text of the chapter should then be searched for use of base periods other than 1960-1990, which is the base period mentioned (twice) in the paragraph beginning at Page 1 Line25. (Cogley, J. Graham, Trent University)	See response to comment #93.
97	3	2	3	2	13	Exec summary paragraph 1: This will be in ch 1 and/or 2 (IPCC WGII TSU)	See response to comment #93. Definitions of extremes belong in chapter 3.
98	3	2	3	2	13	ES p1: The ES is so strongly tied to the AR4. Is there a way to present the conclusions with equal (or greater clarity) without making everything so referential to the AR4? (IPCC WGII TSU)	All references to AR4 have been removed.
99	3	2	3	2	23	Introductory material is not needed within an ES. As a more general comment, the ES should be restricted to the most important, robust and confident findings coming from the chapter. (Stocker, Thomas, IPCC WGI TSU)	ES has been significantly revised. Only kept essential information.
100	3	2	3	2	3	add in addition to severity and frequency "scale" (van Lanen, Henny A.J., Wageningen University)	See response to comment #93.
101	3	2	3	2	4	Intensity of extremes could also change themselves - new thresholds for 98 percentile, for example. (Bojariu, Roxana, National Meteorological Administration)	See response to comment #93.
102	3	2	3	2	4	This summary would be stronger if it could include a brief paragraph summarizing the major changes in understanding of extremes since the AR4. Page 19, lines 45 to 54 could be moved in here, and some material added, to make a short statement of perhaps 20 lines. (Solomon, Susan, NOAA)	All references to AR4 have been removed.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
103	3	2	3	79	13	I found this chapter one of the weakest of the document... I found on its reading diferent levels of incoherencies, contradictions and inconsistencies... I won't coment specific deficiencies because are to many... I strongly recomend a complete review of this part of the document... (Linayo, Alejandro, Research Center on Disaster Risk Reduction CIGIR)	Authors will review entire chapter. Unfortunate that reviewer did not provide even a single example - this would have at least allowed authors to attempt to respond to the problems apparently seen by the reviewer.
104	3	2	4	2	5	It is still arguable if an 'extreme' is the same as a 'rare' event. In accordance with the extreme theory, an extreme distribution [5] is obtained by taking maxima (or minimum) of a set of samples (e.g. maximum daily temperature or wind speed, yearly maximum daily rainfall etc.), which are not necessarily rare. However, the event might be considered as rare when it falls in the tail of an extreme distribution. [5] J. Beirlant, Y. Goegebeur, J. Segers, and J. Teugels (2004). Statistics of Extremes: Theory and Applications. John Wiley & Sons, Ltd, England. (Wang, Xiaoming, Commonwealth Scientific and Industrial Research Organisation (CSIRO))	See response to comment #93.
105	3	2	4	0	0	...frequency of occurrence of an extreme ...: an extreme is defined by its frequency, and so this frequency cannot change. It's better to say that the intensity of rare events changes (Van den Hurk, Bart, KNMI)	See response to comment #93.
106	3	2	4	0	0	The AR4 and post AR4 conclusions should be reported in a dedicated chapter to which the other chapters must refer to. Anyway not in the executive summary. (BOVO, STEFANO, ARPA Piemonte)	All references to AR4 have been removed.
107	3	2	5	2	5	To improve intelligibility, I suggest to add "at a specific site" after "extreme" (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	See response to comment #93.
108	3	2	7	2	7	replace "social" with "socio-economic" (van Lanen, Henny A.J., Wageningen University)	See response to comment #93.
109	3	2	10	2	10	add after "rain". "....rain, or heat waves associated with drought". (van Lanen, Henny A.J., Wageningen University)	See response to comment #93.
110	3	2	12	2	12	"likely" -- is this an assessed "likely", then italicize, otherwise replace by other word (Stocker, Thomas, IPCC WGI TSU)	See response to comment #93.
111	3	2	12	2	12	This then raises an important (and largely unresolved, to my knowledge) question - are ENSO impacts driven primarily by the SSTs themselves, or the changes in trans-Pacific SST gradient? (A uniform Pacific warming of, say, 1 C would certainly lead to an increased frequency of El Nino events under current definitions, but would it force atmospheric changes?) (Trewin, Blair, Australian Bureau of Meteorology)	Comment refers to page 3. Too detailed discussion to include in ES.
112	3	2	17	2	18	It might be more appropriate to list the examples such that their time scale is increasing, i.e., "(tropical and extratropical cyclones; monsoons; El Nino ...)" (Wernli, Heini, ETH Zürich)	Do not understand reason for changing the order. ES structure being substantially revised for SOD.
113	3	2	19	2	19	"Weather or climate realted" impacts on the natural.... (Brönnimann, Stefan, University of Bern)	Will consider rewording, but need to keep this brief.
114	3	2	19	2	20	As discussed in the main text, but not in the summary, changes in impacts on the natural physical environment are to some or large extent affected by non-climatic variables, thus isolating climatic changes is much more difficult for this category. It could be important to mention this here. (Mechler, Reinhard, INTERNATIONAL INSTITUTE FOR APPLIED SYSTEMS ANALYSIS)	Not appropriate for ES.
115	3	2	19	2	20	It is interesting that in Chapters 1, 3, and 9 the impacts of frosts (in agriculture and health, in particular) are not considered. Is there any particular reason? A recent example of northern hemisphere proportion were the severe frosts, snow spells, and floods that occurred this winter/spring 2009-2010, possibly associated to El Nino and NAO. I understand that there may not be published articles citing these events, yet, but my point is made for historical low temperatures that have been associated to disasters (Cavazos Tereza, CICESE)	Frosts were considered in ZOD but excluded on advice from those who live where frost is not uncommon or extreme. Temperature extremes section (3.3.1) mentions now explicitly that we do not address changes in frost (though a few mentions are included)
116	3	2	19	2	20	Surprised that wildfire is not included as a relevant "impact on the natural physical environment" (Staudt, Amanda, National Wildlife Federation)	Bushfires covered by Chapter 4, as part of the biological environment.
117	3	2	19	0	0	"Drought", "Flood" and ... are not environmental impacts. They are hydro-climatic phenomena. So this classification does not match with the previous two categories. (Davtalab, Rahman, Ministry of Energy)	Reject. Drought and flood are impacts, and qualitatively different to some other weather extremes such as hot days.
118	3	2	21	2	21	Wording: it is not relevant whether these appear to be changing, but whether we can establish with a prticular level of certainty whether they in fact are changing, or not. (Bouwer, Laurens, Institute for Environmental Studies)	Deleted in revised ES
119	3	2	22	2	22	"as well as projections of future changes" -- sentence seems to be missing a verb (Stocker, Thomas, IPCC WGI TSU)	Deleted in revised ES.
120	3	2	22	2	22	add in addition to severity and frequency "scale" (van Lanen, Henny A.J., Wageningen University)	Reject - unnecessarily complicated to be inserted here (although it is discussed in chapter)
121	3	2	25	2	25	What is meant by the term reinforced ? If this means added strength to (as in Collins dictionary), one could ask why the assessment hasn't increased from very likely to eg extremely likely. My sense from the evidence reviewed here is that there isn't sufficient additional evidence to justify an increase in the likelihood assessment - very likely is appropriate - but that the evidence since the ar4 supports this conclusion and our confidence in that conclusion remains high (see discussion on page 11). So why not simply state support and avoid introducing yet another term to the uncertainty language ? (Stott, Peter, Met Office)	Reference to AR4 and "reinforced" deleted.
122	3	2	25	2	37	A base period 1960-1990 is mentioned twice, but it should be 1961-1990 . Base period should be 30 year long. (Wibig, Joanna, University of Lodz)	Base period has been corrected.
123	3	2	25	2	37	Please present cold days and nights like hot days in page 9 line5 for more clarification. (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	Unclear what comment refers to.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
124	3	2	25	2	37	The various confidence determinations (likely, very likely, etc.) in this paragraph are confusing. Also, can't tell which of the confidence determinations are from AR4 and which are newly updated. (Staudt, Amanda, National Wildlife Federation)	Have removed reference to AR4 in ES. Confidence/likelihood statements are explained in Section 3.1.5.
125	3	2	27	2	28	The increase/decrease in the number of warm/cold days and nights since 1950 is a general statement, independent of a reference period. Therefore, I think including the 1960-1990 base period in this place could be misleading. (Gutiérrez, José Manuel, Consejo Superior de Investigaciones Científicas (CSIC))	Agreed. Reference to base period has been removed in this paragraph.
126	3	2	29	0	0	...warm spells have increased...: strange sentence. It is the number of warm spells that can increase. (Van den Hurk, Bart, KNMI)	Agreed. Insert "number of".
127	3	2	30	2	31	I find this statement on intense storms rather peculiar and hard to see why it is being proposed, ie a low confidence statement on there being a difference in response in different ocean basins rather than a statement about the overall increase. (Stott, Peter, Met Office)	Refers to page 3. Statement revised for clarity.
128	3	2	30	2	32	Christidis 2005 was quoted in AR4 so strictly speaking studies since AR4 have confirmed this result rather than newly suggesting a human influence. Christidis, N., P. A. Stott, S. Brown, G. C. Hegerl, and J. Caesar (2005), Detection of changes in temperature extremes during the second half of the 20th century, Geophys. Res. Lett., 32, L20716, doi:10.1029/2005GL023885. (Brown, Simon, The Met Office Hadly Centre)	Reference to AR4 deleted from ES.
130	3	2	36	2	37	The issue of extremes and critical health thresholds does not appear to me to be covered in this chapter, and hence doesn't belong in this summary. The summary needs to be tightly restricted to what is covered. (Solomon, Susan, NOAA)	Agreed. Delete sentence.
131	3	2	39	2	42	Your assessment that the "new studies since AR4 have 'substantially strengthened' the likelihood statement of 'more likely than not' that anthropogenic influence has contributed to a global trend towards increases in the frequency of heavy precipitation..." -- If the statement has been "substantially strengthened", can the associated likelihood be strengthened as well? Refer to comment #39. In 50-52. (Stocker, Thomas, IPCC WGI TSU)	Reference to AR4 deleted from ES
132	3	2	39	2	46	What about hail? (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Confidence is low regarding hail, because of limited studies, so not considered appropriate for ES (but discussed in body of Chapter)
133	3	2	39	2	46	This conclusion (particularly the sentence starting at line 40) does not appear consistent with the summary material in the chapter at page 39, lines 48-53). The latter is weaker, and, I think, more justified. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	ES was substantially revised for SOD. Ensured consistency with 3.2.
134	3	2	39	2	46	ES P4: landslides should be in ch 4 (IPCC WGII TSU)	Disagree. This is related to the natural physical environment, not to "human systems and ecosystems"
135	3	2	39	2	46	ES p4: Dust storms should be in ch 4 (IPCC WGII TSU)	See response to comment #134.
136	3	2	39	2	55	in the executive summary it is apparent that words related to severe storms such as hail, lightning and tornados are grouped together in "wind events" and "heavy precipitation events". A more direct statement on these is missing. As an example, The reference "Will moist convection be stronger in a warmer climate?" by Dal Genio et al 2007 - GRL - VOL. 34, L16703, doi:10.1029/2007GL030525 is missing. The problem with severe weather is that the damage is small scale. However the environment that produces severe storms is in the larger scale and thus evolving environmental conditions prone to severe weather could be investigated. Using as baseline for this report the AR4 is adequate up to a certain point when new approaches have to be adopted to detect extreme events. Even if the literature falls short of comprehensive studies on severe weather, this should be made clear in the report so that perhaps new research will develop. (Silva Dias, Maria Assuncao, University of Sao Paulo)	Too little material. Also see comment #132, and #7.
137	3	2	40	2	42	I'm not sure what it means to "substantially strengthen" an assessment that, in the AR4, was "more likely than not" and remains more likely than not in the SREX. I think this is trying to say that there is new evidence that points in the direction of a human influence on heavy precipitation, but that we still do not have adequate confidence to go beyond a better than even odds assessment. (Zwiers, Francis, Environment Canada)	Reference to AR4 removed from ES.
139	3	2	48	2	55	The assessment on changes in tropical winds, and how that might related to projected changes in the intensity of TCs, is not very clear. (Zwiers, Francis, Environment Canada)	Agreed, but this is not simple - overall wind speeds expected to weaken, but with possible increase in strongest TCs. Has been reworded.
140	3	2	48	2	55	What about tornadoes? (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	See response to comment #136.
141	3	2	48	2	55	The statements in II. 50-51 ("decreased frequency of the strongest wind events in the tropics") and in II. 54-55 ("a likely increase in tropical cyclone winds") appear contradictory. These statements, as well as the related text on p. 3, II. 23-41, need to specify as clearly as possible whether they refer to the same phenomenon or to different phenomena. (Fuessel, Hans-Martin, European Environment Agency)	See response to comment #139.
142	3	2	48	0	0	No additional literature on attribution of strong winds, but has the description of observed trends improved since the AR4? (Klein Tank, Albert, KNMI)	Some studies discussed in body of chapter, but few studies available. Sentence deleted from ES.
143	3	2	48	0	0	Should mention whether there are any studies of observed trends in extreme winds, and what those trends are. Seems odd to jump right to attribution without addressing the past trends. (Staudt, Amanda, National Wildlife Federation)	Agreed. See response to comment #142.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
144	3	2	50	2	0	Need to specify that 'decreased frequency of the strongest wind events in the tropics' concerns wind not associated with tropical cyclones. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Agreed. ES rewrite has removed this issue.
145	3	2	51	2	52	Regional increases of wind-storm risk in the Atlantic-European area have been specified using a range of methodologies, from a consideration of storm track activity (i.e. synoptic time scale variability of sea level pressure) and of extreme cyclones to extreme wind speeds and damages. The focal region of these studies is western Europe, as the projected increases in wind climate and damages are particularly relevant there. In other regions, decreases in storm activity dominate the projections. (Ulbrich, Uwe, Freie Universitaet Berlin)	Such studies are discussed in body of chapter.
146	3	2	51	2	55	Suggest some rewording is necessary - line 51 states a decrease in frequency of the strongest winds but 55 states an increase in tropical cyclone winds. These are not necessary in conflict but some clarification is required. (Church, John, CSIRO)	Seeresponse to comment #144
147	3	2	52	2	52	"...small number of studies of projected extreme winds" I cannot follow this assessment since there is, in fact, a large number of studies on extreme winds related to extra tropical cyclones (cf. section 3.4.5). (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Reject. Few studies relevant to projected changes in extra-tropical EXTREME winds.
148	3	2	53	2	53	"credibly" -- suggest to not use "credible" in connection to projections. Projections of climate are laying out possible future climates asking "what-if". I don't think this should be expressed using words like "credible" or similar. (Stocker, Thomas, IPCC WGI TSU)	"Credibly" is not used anymore.
149	3	2	53	0	0	Change "together with" to "and", and "means" to "mean". (Cogley, J. Graham, Trent University)	Agreed. Was revised.
150	3	2	54	2	54	sentence "Further complicating..." -- I don't understand the logic of this sentence. Why is the "projection of a likely increase in tropical cyclone winds" "further complicating the projection of changes in tropica wind extremes"? Please clarify. (Stocker, Thomas, IPCC WGI TSU)	ES structure was substantially revised for SOD, comment is now irrelevant.
151	3	2	57	3	21	This part of the ES needs some preamble on the relationship between extremes or extreme impacts on the one hand, and ENSO and the other modes on the other. (Zwiers, Francis, Environment Canada)	This preamble is provided in the first paragraph of the ES
152	3	2	57	3	21	Not sure if this should come in this section but due reference should be given to the recent results showing the potential significance of including the stratosphere in future predictions of the storm track. (H Huebener, U Cubasch, U Langematz, T Spangehl, F Niehörster, I Fast and M Kunze Ensemble climate simulations using a fully coupled ocean-troposphere-stratosphere general circulation model Phil. Trans. R. Soc. A 2007 365, 2089-2101 doi: 10.1098/rsta.2007.2078) (Brown, Simon, The Met Office Hadly Centre)	Too detailed for ES. Consider for body of Chapter (section 3.4.5).
153	3	2	59	2	60	I think it would be helpful if such an assessment were possible, to elucidate why there is a tendency to weaker monsoonal flows and to evaluate the confidence in that mechanism as deduced from literature using theory, observations and model experiments. (Stott, Peter, Met Office)	Too detailed for ES.
154	3	2	0	4	0	I think the executive summary could be sorted somewhat by removing mention of some of the more minor assessments, reducing the frequency with which calibrated language is used, and assuring the consistency between the various assessments that are made. (Zwiers, Francis, Environment Canada)	ES will be revised taking all of these issues into account.
155	3	2	0	4	0	In the Executive Summary it is mentioned the different levels of "Expert judgement of likelihood". I suggest to add a figure in this page to show the likelihood. For example from 0-50% likelihood (very unlikely, unlikely), from 50-100% likelihood (likely, very likely). (Cavazos, Tereza, CICESE)	This information is provided in Section 3.1.5
156	3	2	0	4	0	The Executive Summary of Chapter 3 would be strengthened by including a single bolded sentence in each paragraph summarizing the most important takeaway message. This approach is especially important for this chapter to help the reader sort out where our current understanding lies. The way it is currently written, the reader is forced to sort through what was said in the AR4, what is new, and make their own judgment. These bolded sentences should simply state the current assessment, for example: "Heavy precipitation events have likely increased in many parts of the world, and this trend is very likely to continue during the 21st century." (Staudt, Amanda, National Wildlife Federation)	This is a good idea, however it might lead to perhaps half of each paragraph being bolded, since several summary statements are included in each paragraph. ES structure being substantially revised for SOD, so comment may be irrelevant.
157	3	2	0	114	0	Overall I like the chapter, but like the other chapters examined, it is way too long. I like the tables and they can be used to essentially eliminate a lot of text in the chapter. There is too much space spent on stating what each paper cited was about without making concise assessment-based conclusions about the value of these individual studies and what they imply overall for the chapter. Much of this discussion of individual papers can be easily eliminated. Reduc echapter overall by roughly a factor of two. (Wuebbles, Donald, University of Illinois)	Such a reduction would leave authors vulnerable to being attacked for not assessing the literature comprehensively.
158	3	3	8	3	8	"temporal/seasonal" pleas explain (Brönnimann, Stefan, University of Bern)	This paragraph was shortened as part of the revisions. Expression was removed.
159	3	3	14	3	15	Define "mode" (as in "Southern Annular Mode") in a parenthesis following this sentence. "Mode" is a technical term that baffles ordinary readers. (Cogley, J. Graham, Trent University)	Agreed. No mention of "modes" in the ES anymore
160	3	3	14	3	18	Please mention here, that the winter NAO trend from the mid 1990s is reversed and is now close to 0 (Luterbacher, Juerg, Justus Liebig University)	Too detailed for ES.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
161	3	3	14	3	22	It is not mentioned that some studies have shown that there has been an eastward shift of the centers of the pattern of interannual NAO variability. See for example Jung T, Hilmer M, Ruprecht E, Kleppek S (2003) Characteristics of the recent eastward shift of interannual NAO variability. Journal of Climate, 16:3371-3382. (Pavan, Valentina, ARPA Emilia-Romagna)	Too detailed for ES.
162	3	3	18	3	19	Perhaps add another example: "to anomalously rapid warming in the region of the Antarctic Peninsula". (Cogley, J. Graham, Trent University)	Too detailed for ES.
163	3	3	18	3	21	To provide better balance, this summary statement, and the main body of the report, should recognize that ozone depletion is the dominant reason for the changes in the SAM during austral summer-- and that this is expected to reverse in the future. Changes in the SAM in other seasons are not nearly as statistically significant (see Fogt et al., J. Climate, 2009) and should not be the basis for a weaker statement since much of the seasonality is expected. (Solomon, Susan, NOAA)	Too detailed for ES. No mention of modes in ES anymore.
164	3	3	20	3	20	For greater clarity the following language is suggested: ..., although there is some concern that possible anthropogenic impacts on circulation changes are poorly characterized by trends in the annular modes. (Radunsky, Klaus, Umweltbundesamt GmbH)	Too detailed for ES. No mention of modes in ES anymore.
165	3	3	21	3	21	This sentence is not applicable to the SAM. Miller et al (2006) showed strong consistency amongst CMIP3 models for projections of SAM to the end of the 21stC. (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Too detailed for ES. No mention of modes in ES anymore.
166	3	3	23	3	32	Specific mention of category 4 & 5 cyclones should be made here - this will be of crucial interest to governments. (Stocker, Thomas, IPCC WGI TSU)	Too detailed for ES.
167	3	3	23	3	41	Similarly to comment 1, I suggest to move this paragraph up (after the wind paragraph, before the monsoon paragraph) (Wernli, Heini, ETH Zürich)	Agreed, suggested change was implemented
168	3	3	28	3	0	The statement 'It is likely ...frequency of tropical cyclones...decrease in future decades' is based on GCM projections, but at least one study based on empirical relationships suggest otherwise (e.g. Benestad 2009, 'On Tropical Cyclone Frequency and the Warm Pool Area' Nat. Hazards Earth Syst. Sci., 9, 635-645.) (Benestad, Rasmus, The Norwegian Meteorological Institute)	However, bulk of evidence suggests decrease.
169	3	3	28	3	29	Suggest replacing "will either decrease or remain essentially unchanged" with "not increase". (Zwiers, Francis, Environment Canada)	Agree. Was changed to "unlikely to increase"
170	3	3	30	0	31	Draft states "It is more likely than not that the increases in frequency of the most intense storms will vary substantially between ocean basins." This seems different from the conclusion of Knutson et al. (2010) and also different (stronger) than the main text of the SREX (p. 57, lines 40-42). For instance, Knutson et al. conclude: "The frequency of the most intense (rare/high impact) storms will more likely than not increase by a substantially larger percentage in some basins." (The point of comparison for the "larger percentage" is the 2 to 11 % increase in mean maximum wind speeds projected by the models.) The sentence in the SREX exec summary seems to me to imply that it is "likely" that the frequency of the most intense storms will increase (same likelihood level as for mean intensity from the previous sentence) but that it is more likely than not that this change will vary between basins. On the other hand, the main text of SREX (p. 57, lines 40-42) concludes: "It is more likely than not that the frequency of the most intense storms will increase by more than 11% in some ocean basins". That version seems consistent with Knutson et al. (2010). (Knutson, Thomas, GFDL/NOAA)	Agreed. Revised ES matches body text.
172	3	3	31	3	32	Change "tropical cyclone related rainfall rates", depending on what is actually meant, to "the rainfall rates of tropical cyclones" or "rainfall from tropical cyclones". (Cogley, J. Graham, Trent University)	Reject. Current wording is very clear.
173	3	3	32	3	32	"greenhouse warming" -- this term should be avoided, suggest to replace by something like "continued warming due to increases greenhouse gas concentrations" (Stocker, Thomas, IPCC WGI TSU)	Agreed.
175	3	3	35	0	0	It is not clear at this point how the intensity of a cyclone is defined (and therefore its intensification). In contrast to most other topics (e.g., wind speed, precipitation amount, temperature extremes, ENSO index) there are various options of how the intensity of a cyclone can be defined (e.g., minimum sea level pressure, maximum vorticity, maximum local wind speed, kinetic energy integrated over the entire system) and these measures do not necessarily agree. I suggest to clarify here and throughout the document which intensity measure the results are based on. (A similar issue occurs with droughts, for which the definition issue is highlighted, p. 3 line 50.) (Wernli, Heini, ETH Zürich)	Difficult to include in ES because of space limits. Issue is discussed in body of chapter. Observed changes in intensity no longer mentioned in ES.
176	3	3	38	3	39	This assessment does not seem very informative to me as written - should it be dropped? (Zwiers, Francis, Environment Canada)	ES structure being substantially revised for SOD, Assessment has been rewritten.
177	3	3	39	3	0	Meaning 'the number of'? Different ways of analysing storms can result in different answers, and it is not yet established which approach is more reliable and whether they provide a representative description of past or future trends. Therefore, an international initiative IMILAST (http://www.proclim.ch/imilast/index.html) has been set up. (Benestad, Rasmus, The Norwegian Meteorological Institute)	("Number of" has been added to the text. We recognise the ambiguity in terms like number of cyclones, number of features, feature density, track density etc. In the same way intensity measures also vary between different studies. We have noted this in the 'Extratropical cyclone' section.
178	3	3	39	0	0	Sentence: 'A reduction in mid-latitude storms averaged over each hemisphere is likely': Please specify if this relates to storm frequency, intensity or both. (Neu, Urs, Swiss Academy of Sciences)	See response to comment #177.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
179	3	3	40	3	41	The study of Ulbrich et al. 2008 (Ulbrich, U., J.G. Pinto, H. Kupfer, G.C. Leckebusch, T. Spangehl and M. Meyers, 2008: Changing Northern Hemisphere Storm Tracks in an Ensemble of IPCC Climate Change Simulations. J. Climate, 21, 1669–1679.), though addressing SLP storm tracks instead of cyclone tracks (not sufficient data available), clearly demonstrates a common signal of an ensemble of 16 different models in terms of signal pattern correlations. They also note that "All signals but one are positively correlated to the ensemble mean signal, but for some of the models the correlation is rather modest, so that the median value is only about 0.5." Thus, I would not speak of "little consistency". (Ulbrich, Uwe, Freie Universität Berlin)	Too detailed for ES.
180	3	3	47	3	47	add following sentence before "... Lack of soil moisture...". A recent pan-European study on changes in river flow in near-natural catchments shows a coherent picture of annual streamflow trends, with negative trends in southern and eastern regions, and generally positive trends elsewhere (especially in northern latitudes). (van Lanen, Henny A.J., Wageningen University)	Relationship of this suggested sentence to extremes is not clear.
181	3	3	48	3	49	"have projected that an increase ... is likely". (Cogley, J. Graham, Trent University)	Agreed. But comment is now irrelevant because of substantial revisions to ES
182	3	3	48	3	50	It is not clear if the authors are taking responsibility for this assessment, or if they are repeating an assessment made elsewhere. (Zwiers, Francis, Environment Canada)	Agreed. No more references to AR4 in the ES.
183	3	3	48	3	50	Should mention studies concluding that many regions of the world are likely shifting to more permanent arid conditions... Solomon et al (2009, PNAS 106, 1704), Seager et al (2007, Science 316, 1181) (Staudt, Amanda, National Wildlife Federation)	Statements in the revised paragraph already represent this. But there are large uncertainties regarding projected changes in droughts as noted in the ES and the main chapter text.
184	3	3	50	3	50	revise "...dependent on the definition of the drought index, ..." in "...dependent on the type and definition of the drought, ...". (van Lanen, Henny A.J., Wageningen University)	Disagree. Uncertainty applies to all different types of droughts; indices are yet another issue.
185	3	3	52	3	55	There appears to be a contradiction between line 52 which says there is little evidence of change and line 55 which states greenhouse gases have affected floods. Some rewording is required. (Church, John, CSIRO)	Attribution statement was removed from ES.
186	3	3	53	3	55	I think it would be justified to add that it is "extremely likely" that warming will lead to a shift from glacial to nival hydrological regimes in streams that are fed by glacial meltwater at present. That is, in most regional climates glacier-fed streams will shift from having peak discharge in summer to having peak discharge in spring. (Cogley, J. Graham, Trent University)	Statement deleted from SOD ES
187	3	3	54	0	0	This is the first instance that a long term perspective is introduced in the text. Is this information missing for the changes described elsewhere? In the remainder of the chapter the longer time scale is covered only on page 65 about flooding. (Klein Tank, Albert, KNMI)	Statement deleted from SOD ES
188	3	3	54	0	0	The differences between regional and hemispheric/global past trends, and the distinction between changes in surface temperature and precipitation/drought fields, underscore the limited utility in the use of terms such as the "Little Ice Age" and "Medieval Warm Period" for describing past climate epochs during the last millennium (Jones and Mann, 2004). Therefore, I suggest to delete the term, instead mention the period you are referring to. Jones, P.D., Mann, M.E., Climate Over Past Millennia, Reviews of Geophysics, 42, RG2002, doi: 10.1029/2003RG000143, 2004.) (Luterbacher, Juerg, Justus Liebig University)	Statement deleted from SOD ES
189	3	3	55	3	57	This is not a very informative assessment and rests on a justification which, while stated as fact (ghg's have affected the hydrological cycle), is uncertain. For example, the assessment of human influence on precipitation extremes remains (appropriately) "more likely than not". (Zwiers, Francis, Environment Canada)	Statement deleted from SOD ES
190	3	3	55	0	0	...have affected floods... This statement is not specific. Please indicate in terms of frequency, intensity, spatial extent, etc. (Luterbacher, Juerg, Justus Liebig University)	Statement deleted from SOD ES
191	3	3	57	3	58	This part is duplication of chapter 3, chapter 3, line 53 (Luterbacher, Juerg, Justus Liebig University)	Duplication is not considered issue (main issue is consistency of material). But statement was shortened anyway as part of revisions.
192	3	3	57	3	58	The sentence "it is likely that anthropogenic rivers" can be deleted (Zerefos, Christos, Academy of Athens)	Text revised.
193	3	3	62	4	11	This text needs to mention recent (i.e., post-AR4) studies using semi-empirical models that suggest sea level will rise faster than estimated in the AR4. The main results of these studies are summarized in: Stefan Rahmstorf. A new view on sea level rise. Nature Reports Climate Change (6 April 2010). doi:10.1038/climate.2010.29 (Fuessel, Hans-Martin, European Environment Agency)	Appropriate for AR5, not SREX.
194	3	4	1	4	11	I thought this was weak and could be strengthened. (Church, John, CSIRO)	Thanks, was revised. No reference to AR4 material anymore
195	3	4	4	4	4	"significant wave height in some parts of the globe": is it a meaningful part of the ocean (it could be statistically significant but very small). (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	Sentence makes clear that it is a substantial portion but far short of universal. More detail inappropriate in ES.
196	3	4	4	0	0	Define the technical term "significant wave height" in a parenthesis. (Cogley, J. Graham, Trent University)	Agreed (or use different term). ES structure being substantially revised for SOD, so comment may be irrelevant.
197	3	4	7	4	8	Isn't this "virtually certain"? If there are future changes in storminess and wind patterns, a response in significant wave height would be virtually assured, I would think. Nevertheless, I suggest dropping this one since there does not seem to be sufficient evidence to make a confident assessment of projections of future winds. (Zwiers, Francis, Environment Canada)	Point is that such changes may dominate, on regional scales, any global increase in sea level due to warming.
198	3	4	7	0	0	For the same reason a formal assessment of wind extremes could also be precluded. (Klein Tank, Albert, KNMI)	Agreed see lines 48-49, page 2. and statement of low confidence in revised ES.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
199	3	4	7	0	0	please indicate whether positive or negative ...significant wave height... (Luterbacher, Juerg, Justus Liebig University)	Might be positive or negative. Was clarified.
200	3	4	13	4	23	Much of 3.5.6 is focussed on permafrost melt in mountain regions and increased landsliding - something on this is needed here. (Stocker, Thomas, IPCC WGI TSU)	Statement on permafrost is now included.
201	3	4	13	4	23	These all seem to be logical statements, although in the case of permafrost, projections of future changes could perhaps be more certain than estimates of past changes. The statement that permafrost will continue to thaw is complex because it implicitly includes the statement that permafrost is thawing (which has been separately assessed). I am concerned that the basis for the assessments in this paragraph has not been well enough established in the chapter (see comments below). (Zwiers, Francis, Environment Canada)	This was revised to not imply an attribution statement or assessment of past changes.
202	3	4	13	4	23	Has recent research affected these conclusions? Again, hard to tell what is straight from the AR4 and where there have been advances in knowledge. (Staudt, Amanda, National Wildlife Federation)	No references to AR4 assessments in ES anymore.
204	3	4	18	4	23	Most of these likelihood statements are not replicated in section 3.5.7 (permafrost and high-latitude impacts). Only the projected increased coastal erosion statement comes directly from 3.5.7. Given the directly observed evidence of permafrost warming, might 'very likely' be considered on line 18 for permafrost thawing?. (Stocker, Thomas, IPCC WGI TSU)	Sentence was removed.
205	3	4	19	0	0	Define "thermokarst", or find a way not to use the term. (Cogley, J. Graham, Trent University)	Agreed. Not using the term in the ES anymore
206	3	4	21	0	0	reduced winter snow thickness? (Luterbacher, Juerg, Justus Liebig University)	Sentence was removed.
207	3	4	22	0	0	please mention a couple of physical impacts (Luterbacher, Juerg, Justus Liebig University)	This paragraph was revised, sentence was removed
208	3	4	25	4	28	Do the statements on dust justify space in the ES? (Zwiers, Francis, Environment Canada)	Revised ES reduces statements on dust to single sentence indicating low confidence in projections (at the end of drought paragraph).
209	3	4	25	4	28	The linkage with the pattern of wind changes in tropics and subtropics could be mentioned. (Bojariu, Roxana, National Meteorological Administration)	Assesment on dust reduced to one sentence (see #208)
210	3	4	25	4	28	This is a weak paragraph. Although on p. 11 it becomes clearer why the conclusions about dust events are so vague, this paragraph should at least define what is meant by a "dust event" (intense dust mobilization in the desert? Dust transport to populated areas?). Also it is not clear what is meant by "dust activity". (Wernli, Heini, ETH Zürich)	Assesment on dust reduced to one sentence (see #208)
211	3	4	25	4	28	Please mention on the increasing of dust storm in some part of middle east including Iran, Iraq and Turkey. The region have experienced dusty days during recent decades that is unexpeted. Some causes relates to climatic condition especially drought in the region. (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	Assesment on dust reduced to one sentence (see #208). Need also literature to provide such statements.
212	3	4	30	4	34	This paragraph should be deleted since it considers only one aspect of extremes, whereas other important topics (e.g., natural variability, limits in the reconstruction of extremes, etc.) are not mentioned. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Reject. Important to discuss whether trends in extremes simply follow trends in means. But sentence was moved to beginning of ES for better flow.
213	3	4	30	0	0	This sentence is rather unspecific, please formulate more clearly (Luterbacher, Juerg, Justus Liebig University)	See #212
214	3	4	34	0	0	In addition, extreme precipitation is projected to increase for some seasons where total precipitation is projected to decrease (e.g. summer in Europe). (Klein Tank, Albert, KNMI)	Too detailed for ES.
215	3	4	36	4	40	Do the authors check that the different levels of confidence will be taken into account in a consistent manner in the other chapters of this report? (Klein Tank, Albert, KNMI)	Statement included to ensure this takes place. Moved earlier in the ES
216	3	4	36	4	40	This should be taken into account to in the analysis of contemporary risks, and studies of changes in historic impacts and trends in number and severity of disasters for those specific hazards and locations. This should be added. (Bouwer, Laurens, Institute for Environmental Studies)	Sentence was removed.
217	3	4	36	4	40	The conclusions should, in my opinion, say something about the fact that it will be always a level of remaining uncertainties, beyond any improvement in observation system and modelling efforts. Decision makers have to live with this and act accordingly. They have also to cope with continuously up-dated knowledge, especially, for regional and local details. (Bojariu, Roxana, National Meteorological Administration)	This seems obvious. Inappropriate for ES.
218	3	4	36	4	40	Delete - no content in this paragraph. (Church, John, CSIRO)	Reject. The frequency with which statements such as "the climate is becoming more extreme" are made in the media and by decisionmakers indicates that this is a very important message. It needs to be constantly reiterated especially in a report of this sort.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
219	3	4	36	4	40	Presumably this bolded paragraph is intended to be the single most important message the reader gets from reading the summary. The way it reads right now is: "All our preceding statements about extreme weather are uncertain and highly context specific, so use them with caution when designing response strategies." The first sentence is convoluted, giving the impression that everything preceding is highly uncertain and complicated. I suggest rewriting the paragraph to highlight how the information and confidence levels about changing extremes can help inform better response strategies, rather than emphasizing that they limit our abilities. For example: "Understanding how climate change is affecting weather and climate extremes in specific regions and seasons can help design more effective strategies for reducing disaster risk. Such efforts will need to account for limitations in our scientific understanding and to be revised as further study makes more information available." (Staudt, Amanda, National Wildlife Federation)	Reject. The point of the paragraph is that the rest of the chapter shows how our confidence varies, between regions and types of extremes. The statement is included to ensure that potential users stop saying things like "The climate is becoming more extreme".
220	3	4	36	4	40	It is unclear what the authors mean by confidence in this paragraph--whether statistical confidence is meant, or confidence as defined in the uncertainty guidance. If statistical confidence is meant, this is captured by the likelihood statements made above, and "levels of confidence" should not be used to refer to those likelihood statements. We suggest rephrasing. (IPCC WGII TSU)	Here "confidence" assumes its usual meaning. Paragraph was moved further up in the ES.
223	3	5	3	5	3	Maybe best to use Disaster Risk Management as opposed to risk management --this could be financial, health, or whatever unless specified. (Lavell, Allan, Programme for the Social Study of Risk and Disaster (FLACSO))	Section 3.1.1. has been removed; DRM aspects are treated in chapter 1
224	3	5	3	9	7	What is the definition of extreme events for climate change? The paper does not have this definition. (Wen, Jet-Chau, National Yunlin University of Science and Technology)	We do not understand this comment. Extreme events are defined independently of climate change.
225	3	5	3	9	7	Will extreme events cause any disaster or not? (Wen, Jet-Chau, National Yunlin University of Science and Technology)	This issue is mostly addressed in Chapters 1 and 2, since this implies that vulnerability and exposure is considered, which is not addressed in our chapter. We provide nonetheless a more detailed discussion of the definition of threshold-based vs probability-based indices and their links to impacts in the revised section 3.1
226	3	5	3	12	34	3.1.1 This whole section should be covered in ch1 and/or 2 (IPCC WGII TSU)	Section 3.1.1. was removed and material was donated to chapter 1.
227	3	5	3	0	0	Section 3.1.1.: A definition / discussion of the term "extremes" should be included at the beginning of the section (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Section 3.1.1 has been removed. The definition of the term "extremes" is provided in the new section 3.1.2.
228	3	5	3	0	0	Section 3.1.1.: The various statements of this section should be proved by appropriate references (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Section 3.1.1. removed (see 226)
229	3	5	5	5	11	Again here we have an item that is particularly sensitive to severe storm frequency and intensity: the design values for engineering structures. It is not only high wind speeds associated to extratropical storms; the problem is tornadoes, downburst and microburst. If they are likely to increase in frequency and/or intensity, in a given region, building will suffer. (Silva Dias, Maria Assuncao, University of Sao Paulo)	Section 3.1.1. removed (see 226); new version does not address design values anymore
230	3	5	5	5	5	I think the statement that extremes are rare needs to be qualified - given a fixed location (your backyard), extremes presumably are rare, but extremes happen somewhere all the time. (Zwiers, Francis, Environment Canada)	Section 3.1.1. was removed. New definition is provided in 3.1.2. The term "rare" is not used anymore. New definition mentions that extremes are drawn from pdf, hence have certain probability of occurrence. But threshold-based indices are also mentioned: these may have a zero-chance of occurrence in some regions.
231	3	5	5	0	0	"aspects of the climate" should be replaced with "aspects of the climate change". (Incecik, Salahattin/Selahattin, Istanbul Technical University)	Section 3.1.1. removed
232	3	5	7	5	7	Design is mentioned here, but too little focus is given to design needs in Chapter 3. Marine safety is one of the main concerns of the shipping and offshore industry. Extreme values presented in Chapter 3 are not necessarily those an engineer would choose. (Bitner-Gregersen, Elzbieta Maria, Det Norske Veritas AS)	Section was removed/donated to chapter 1; design is addressed in other chapters
233	3	5	10	5	0	The question of stationarity in terms of the upper tail of a PDF was discussed in Benestad (2004), 'Record-values, non-stationarity tests and extreme value distributions', Global and Planetary Change vol 44, issue 1-4, p.11-26, and explored using a test to see whether the data is independent and identically distributed – henceforth referred to as an 'iid-test'. More work on this is published in Benestad, R.E. (2008) 'A Simple Test for Changes in Statistical Distributions', Eos, 89 (41), 7 October 2008, p. 389-390; Benestad, R.E (2006) 'Can we expect more extreme precipitation on the monthly time scale?' J.Clim Vol. 19, No. 4, pages 630-637; Benestad, R.E. (2003) 'How often can we expect a record-event?' Climate Research Vol 23, 3-13. The question of stationarity of the upper tail of the distributions is important when applying extreme value theory, or inferring return-values from GEV/GPD. Furthermore, an anomalously high recurrence of record-breaking values is an indication of increasing trends in the extremes, as has been found in temperatures around the globe. The same technique has been applied to monthly precipitation from the CMIP3 multi-model GCM ensemble to show that wet regions are projected to see more extreme monthly rainfall totals. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Section 3.1.1. removed (comments 23, 50, 68, 226). Benestad (2003, 2004 and 2006) are now referenced under Section 3.1.2. Benestad (2008) is not in a standard ISI publication.
234	3	5	10	5	10	I do not understand what "this" includes. It is not clear to me that engineers do not take non stationarity into account. (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	Section 3.1.1. removed (comments 23, 50, 68, 226)

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
235	3	5	13	5	13	"the probability density functions (PDFs) for climate variables are modified" -- this is rather technical and I wonder whether this requires some sort of additional information in the chapter text or a reference one of the figures in Box 3.1 (Stocker, Thomas. IPCC WGI TSU)	Section 3.1.1. removed (comments 23, 50, 68, 226)
236	3	5	13	5	13	Heer and throughout the chapter, it would be better to use the generic term "probability distribution" rather than the more technical term "probability distribution function". Also, it would be best to avoid the acronym "PDF", which ordinarily is used to indicate the "probability density function" (first derivative of the probability distribution function). (Zwiers, Francis, Environment Canada)	"Probability distribution function" has been replaced with "probability distribution" or "probability density function" throughout the chapter
237	3	5	13	5	13	Here PDF is denoted probability distribution function whereas on p. 8, 142 is is probability density function (Brönnimann, Stefan, University of Bern)	See answer to #236
238	3	5	13	5	15	This statement is actually based on the assessment that "climate" is a property of an Independent and Identically Distributed (IID) random variable. If a climate variable is to be modeled as an autoregressive process of order 1 (AR(1)), which it is NOT, then the properties of the AR(1) can be changed WITHOUT altering the PDF of the process. Given the weight that is given to this statement (one figure), I suggest that an evaluation from professional statisticians, which I am not, is made. Everything that is written here is only valid and relevant for IID processes. This caveat should be accounted for. (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	Section 3.1.1. removed (comments 23, 50, 68, 226); this statement has been removed.
239	3	5	13	5	29	The authors should refer to changes in the "location" and "scale" of the distribution when referring to these changes in mean and variability. They also appear to have forgotten here about changes in "shape". It would be worth citing these recent papers for a clearer description of how to attribute such changes: Ferro, C.A.T., Hannachi, A. and Stephenson, D.B. (2005): Simple non-parametric techniques for exploring changing probability distributions of weather, Journal of Climate, 18, pp 4344-4354 Beniston, M. and Stephenson, D.B. (2004): Extreme climatic events and their evolution under changing climatic conditions, Global and Planetary Change, 44, pp 1-9 (Stephenson, David, University of Exeter)	Section 3.1.1. removed (comments 23, 50, 68, 226); location, scale and shape are addressed in Box ...
240	3	5	14	5	15	What is meant with past climatology? Please specify (Luterbacher, Juerg, Justus Liebig University)	Section 3.1.1. removed (comments 23, 50, 68, 226); baseline is mentioned in new definition for extreme events (see new section 3.1.2. and SREX glossary)
241	3	5	15	0	0	please specify what you mean with 'mean climate' (Luterbacher, Juerg, Justus Liebig University)	Section 3.1.1. removed (comments 23, 50, 68, 226); this statement has been removed.
242	3	5	19	5	20	This first allusion to the fact society adapts to gradual changes in averages or norms of climate may be contrasted with the idea that derives from the subject and title of this report that society also adapts to extremes. Although this is not a topic for this chapter as such most certainly the notion of adaptation to gradual change is not the same as the idea of adaptation to sudden extremes and this difference certainly places the multiple uses of the idea of adaptation under great stress. Many of us dont really believe society adapts to extremes given their far between and exceptional nature. Rather we adapt to changes in averages and norms and through this and more permanent and continuous adjustment to, or dealing with lower magnitude and intensity events, learn to deal with, cope with, get by with or whatever, extremes. One way or another something should be said somewhere as to what is the difference between adapting to gradual change and "adapting" to extremes and the relations between succesful reduction or elimination of risk associated with such lower level events and dealing with extremes once they occur. (Lavell, Allan, Programme for the Social Study of Risk and Disaster (FLACSO))	Section 3.1.1. was removed and material was donated to chapter 1.
243	3	5	21	5	24	Are the exemples relevant?! (Bojariu, Roxana, National Meteorological Administration)	Section 3.1.1. removed (comments 23, 50, 68, 226)
244	3	5	26	5	26	(see comment 2: Do extremes have to be disruptive or can they be continuous or recurrent? Is sea ice decline included? Is drying-out of lakes included? Is the ozone hole included?) (Brönnimann, Stefan, University of Bern)	Section 3.1.1. removed (comments 23, 50, 68, 226); new definition (3.1.2) addresses both recurrent extremes and threshold-based extremes; tipping points are not addressed; sea ice, drying out of lakes and ozone hole are not included (out of scope)
245	3	5	31	5	38	"In general a extreme that occurs on a small time scale also tends to have a small space scale". True, but... when you have a tornado outbreak the scale of the damage is far beyond the scale of a single tornado. (Silva Dias, Maria Assuncao, University of Sao Paulo)	Section 3.1.1. removed; this sentence is not in the text anymore
247	3	5	33	5	34	"In general, .. small space scale" - this statement appears too broad and needs supporting references. (MUJUMDAR, PRADEEP, INDIAN INSTITUTE OF SCIENCE)	See 245
248	3	5	34	0	0	Time scales down to minutes are iportant, particularly for winds, waves and surges. (Church, John, CSIRO)	Time scales not mentioned anymore; not essential to material; see also 245
249	3	5	35	5	38	I could not understand this sentence: what is the meant by "changes in other factors"? How can the local topography change?? (Wernli, Heini, ETH Zürich)	"Changes" referred to "variations". But text removed.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
250	3	5	40	5	47	The identification of these information needs in section 3.1.1 may be OK in itself, but I fear a political decision maker or disaster risk manager would be a little perturbed by the lack of specification of the territorial scale and temporality for which such information needs to be projected and depicted. Clearly the most important thing for a national, regional, local level or zonal disaster risk manager, political decision maker or member of the public is to know what is going to happen in his particular area or policial jurisdiction as regards particular types and combinations of events. The absolute notion of an extreme event is not really valid as extreme to one zone is what is above the 90 percentil of that zone's experience and this may be only a level 3 hurricane, or a 10 year flood, whereas in another area it will be a 100 year level 5 hurricane and a 200 year flood. Without clarification of this point and also an analysis of what opportunities for having this type of scaled down information on hand, any discussion of information needs for DRM is somewhat in the air when it comes to decision making and disaster risk management for particular areas and jurisdictions. Beyond information on extremes and the types of information identified in this part of the chapter, a DRM manager also needs to know what is likely to happen with the smaller scale but still very potentially dangerous events they will have to deal with, mitigate, prevent or reduce the risk thereby associated with, how does their patern of incidence and recurrence vary, what is the likely statistical and distribution relationship between such events and extreme events--if the extreme for that zone is in the 90 pèrcentil and above one knows there will be many non extreme recurrent events occurring over the years before the extremes appear in the same zone. And, the manager will also need to know how these climate events relate to non climate hazard events in the same zone. Management of risk requires holistic information across a series of frontiers and themes that relates to the physical events as such and also to exposure and vulnerability. I fear that what is in these first sections as regards what information is required is rather an excellent and scientifically very sound discussion of what we know as to certain characteristics of the events and their incidence, intensity etc. along with a discussion of what methods are used to get to this information and their veracity or limitations. That really is not the same thing as what we need to know. There should be a contrast I think between what is a thorough breakdown of what is really needed as information in order to manage risk or promote proactive adaptation and this should be compared to what we know or are likely to know at the scales we need to know it. This is what a disaster risk manager or promoter of adaptation will be interested to receive and comprehend. Personally given that this section touches on the crux of the matter if we are interested in seeing how managing risk of extremes and disasters can contribute to adaptation and passing on information to DRM managers in order to achieve this, I believe the final content of this chapter must be achieved in close relationship to the needs that arise from a consideration of what management of the risks signifies in terms of information and analysis. Thus, from my perspective, I would here simply state comprehensively what does a manager need to know, what do we know we can give him, what do we not know with levels of certainty he can use. And, avoid all use of discussion on methods, inadequacies, restrictions etc leaving that for another time, or simply put it in an annex-this is excellent science but not of much interest to a political decision maker-if it is in an annex it can be consulted without distracting from the central needs of a manager of risk or public, private or civil society decision maker. (Lavell, Allan, Programme for the Social Study of Risk and Disaster (FLACSO))	Section 3.1.1. was removed and material was donated to chapter 1.
250.2	3	5	40	5	47		
251	3	5	42	5	47	In this list, the (non-linear) superposition of different extreme events (multihazards or cascade effects), which may significantly enhance the impacts, should be mentioned (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Compound events are addressed in new section 3.1.3
252	3	5	42	0	0	It might be useful to make clear that these events are not necessarily extreme events in the statistical sense (Wernli, Heini, ETH Zürich)	This point is made clear in new definition section and in the compound event section (3.1.2 and 3.1.3)
253	3	5	43	5	44	Add downscaling to the second bullet. (Zwiers, Francis, Environment Canada)	Section 3.1.1. removed (comments 23, 50, 68, 226); downscaling addressed under section 3.2
254	3	5	45	5	45	It is suggested to add a new bullet point; Identification of relevant uncertainties and, if possible, their quantification. (Bitner-Gregersen, Elzbieta Maria, Det Norske Veritas AS)	Section 3.1.1. removed (comments 23, 50, 68, 226); uncertainties are addressed in section 3.1.5 and 3.2
255	3	5	46	5	47	In my opinion, prediction tools for early warning are not specifically developed for adaptation measures; it is more a weather type prediction. The adaptation measures need more than an early warning system, they need climate prediction in the form of what Lorenz classified as resolving boundary condition problem. (Bojariu, Roxana, National Meteorological Administration)	Section removed. Early warning is now addressed in case study chapter
256	3	5	54	5	56	This current wording 'Finally,' does not put enough emphasis on the importance of Sections 3.3 - 3.5. The chapter overview should very clear indicate that sections 3.1 - 3.2 are intended to provide the necessary context and background for the crux of the report which is contained in sections 3.3 - 3.5. (Stocker, Thomas, IPCC WGI TSU)	New text (end of new 3.1.1.) makes it clear that main material is in sections 3.3. to 3.5
257	3	5	59	7	15	Box 3.1: Material covered in (or move to) ch 1 or 2 (IPCC WGII TSU)	Box 3.1. was donated to chapter 1.
258	3	5	61	0	0	Box 3.1 -- this is a very well written and very important Box for the entire report. We wonder whether this Box would be better placed in Chapter 1 (section 1.2) as part of the introduction into the issue of extreme events vs. extreme impacts etc. Please discuss possibility with Chapter 1 CLAs. (Stocker, Thomas, IPCC WGI TSU)	Box 3.1. was donated to chapter 1.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
259	3	5	0	6	0	The title of Box 3.1 refers to extreme impacts from non-extreme events. The most striking example - in my view - are ecological irreversibilities like species extinctions which may occur due to a slow and gradual change in temperature and precipitation patterns, rather unrelated to extreme events (a number of case studies is presented in Settele, J., Penev, L., Georgiev, T., Grabaum, R., Grobelnik, V., Hammen, V., Klotz, S., Kotarac, M. and Kühn, I.(Eds) (2010) Atlas of Biodiversity Risk. Pensoft Publ., Sofia/Moscow). Box 3.2, however, when referring to non-linear effects states that they may be related to physical and social factors - biological/ecological factors are omitted (which is rather typical for the whole draft). A reference is made to chapter 4 where links to ecosystems are supposedly discussed (which they are not in any detail). (Spangenberg, Joachim H., Sustainable Europe Research Institute SERI Germany)	Box 3.1. was donated to chapter 1.
260	3	5	0	31	0	Please don't use an example for clarification of subjects and use different examples, It means , you can use maximum Temp. for somewhere and minimum Temp for some of them,...for description some events like heatwaves and coldwaves , you have referred mostly to European heatwave, but after 2003, there are many examples that different areas have experienced heatwave and coldwave. one of the relates to coldwaves that affect a spread area of Asia in 2005. by this means you can attract the reader. for this purpose, you can use the serial article "state of climate in years between 2001 to 2009" that is published every year in BAMS(Bulletin of American society). In these articles are presented many extreme events in the regions during recent decades. please something about unexpected and rare event such as snow in Saudi Arabia in summer or snow in Italy in summer,...and their causes. (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	Box 3.1. was donated to chapter 1; larger range of examples has been aimed for in the chapter.
261	3	6	1	6	5	It is necessary to take into account that not only the unusual or extreme events, according to the statistical treatment of maximum yearly level are important. Values that certain thresholds exceed (partial duration), or they become more aleatory in terms of the practices of handling of a territory, in spite of being less notorious and more complex of analyzing, they can impress very important. Example the pluvial erosion, drought. For it previous, not only the changes of the values, but changes in the seasonal condition, the cycles, randomness, they can cause negative impacts. (Lamprea Quiroga, Pedro Simon, Ideam - Advisor (Colombian institute of hydrology , meteorology and environmental studies))	Box 3.1. was donated to chapter 1; these aspects are treated in chapters 1, 2 and 4.
262	3	6	4	6	25	Not sure this discussion should be here. In fact there are many insurance schemes that do pay out on the basis of a clear separation of what damage can be attributed to the physical event being outside of the range of normal extremeness or experience and what is due to the lack of attention of the insured to basic vulnerability and exposure reduction principles known to those in the area--see the Yapachuri scheme in highland Bolivia for example. Extreme climate can be measured independantly of extreme social impacts and can be measured I presume on time scales for which there is adequate physical information. In general I feel it would be best if in this chapter no attempt is made to get into social and economic arguments and discussions. Just keep to the physical parameters and details and talk of extremes in physical terms. Other chapters with social and economic specialists can deal with all these collateral discussions which do tend to be overly simplified and somewhat lacking in detail and specification as they are put here. (Lavell, Allan, Programme for the Social Study of Risk and Disaster (FLACSO))	Box 3.1. was donated to chapter 1
264	3	6	7	6	14	Some discussion of specific "off the scale" events, such as central Europe 2003 and Russia 2010, would be useful here. Also, some mention should be given to possible positive feedbacks from a combination of extremes (e.g. the role of drought and low soil moisture in reinforcing both these heatwaves) (Trewin, Blair, Australian Bureau of Meteorology)	Box 3.1. was donated to chapter 1
265	3	6	7	6	15	This paragraph does not fit well to the theme given by the title of Box 3.1 (Wernli, Heini, ETH Zürich)	Box 3.1. was donated to chapter 1
266	3	6	13	6	13	Extreme events do not imply a change of any kind. (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	Box 3.1. was donated to chapter 1
267	3	6	13	0	0	Indirectly, the authors do address the abruptness of the changes by considering particular time periods for presenting the trends. (Klein Tank, Albert, KNMI)	Box 3.1. was donated to chapter 1
268	3	6	14	6	14	A reference period 1960-1990 is mentioned , but it should be 1961-1990. Reference period should be 30 year long. (Wibig, Joanna, University of Lodz)	Box 3.1. was donated to chapter 1.
269	3	6	14	6	14	Is 1960-1990 correct? 1961-1990 is the more usual reference period. This recurs several more times in this chapter. (Trewin, Blair, Australian Bureau of Meteorology)	(Box 3.1. was donated to chapter 1.
270	3	6	14	0	0	should it be 1961 instead of 1960? (Luterbacher, Juerg, Justus Liebig University)	Box 3.1. was donated to chapter 1.
271	3	6	14	0	0	phrase between brackets (e.g. temperature extremes) does not relate to the "abrupt change" that is addressed in the sentence before. Odd link. (Van den Hurk, Bart, KNMI)	Box 3.1. was donated to chapter 1
272	3	6	14	0	0	Should this be 1961-1990 ? (Stott, Peter, Met Office)	Box 3.1. was donated to chapter 1.
273	3	6	17	6	20	What is the evidence that moderately extreme ENSO trigger threshold crossing and extreme impacts? What is the evidence that there are always thresholds for human health? These statements don't seem to be supported. Back up or delete. (Solomon, Susan, NOAA)	Box 3.1. was donated to chapter 1

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
274	3	6	17	6	20	I don't understand the idea of this sentence, which seems to contradict the first paragraph of section 3.1.1. (p5, lines 5-11). A rather frequent event may cause thresholds, but should not normally produce large effects if adaptation to local climate (either in terms of design values, or in terms of any inhabitants that could potentially be affected) there was effective! (Ulbrich, Uwe, Freie Universitaet Berlin)	Box 3.1. was donated to chapter 1
275	3	6	17	6	21	It should be made clear here that in discussing the contribution of non extreme events to extreme impacts you are only talking of the ways that the interrelationship of physical factors may lead to greater intensity and thus maybe to greater physical impact. The discussion is devoid of the notion that non extreme events normally have greater impacts because of exposure and vulnerability of social elements. Here we are not sure if the authors are talking of extreme impacts in terms of what they call physical impacts--floods, drought etc -or in terms of human impacts. I assume it is the latter sense given that in the next sentence talk is made of impacts related to the levels of adaptation achieved when faced with extreme events that dont impact greatly, but it would be important to qualify and clarify this. (Lavell, Allan, Programme for the Social Study of Risk and Disaster (FLACSO))	Box 3.1. was donated to chapter 1; we now specify that we do not consider vulnerability and exposure (see new section 3.1.2)
276	3	6	22	6	23	It would be good to add a documented example of such an extreme. (Zwiers, Francis, Environment Canada)	Box 3.1. was donated to chapter 1
277	3	6	22	6	23	An extremely intense/rare weather phenomenon does also not lead to major impacts if it occurs in an unpopulated area. For instance very intense tropical cyclones can have virtually "no impact" if their life-cycle occurs entirely over the ocean. This indicates that the TRACK of weather phenomena can play an even more crucial role than their intensity. (Wernli, Heini, ETH Zurich)	Box 3.1. was donated to chapter 1; exposure is addressed in sections 1, 2 and 4
278	3	6	22	6	23	Another reason for lack of impact may be lack of exposure (e.g. a cyclone hitting an unpopulated area) (Trewin, Blair, Australian Bureau of Meteorology)	See 277
279	3	6	23	6	28	please make the link to the chapter where changes in physical extremes are addressed (Luterbacher, Juerg, Justus Liebig University)	Box 3.1. was donated to chapter 1
282	3	6	30	7	9	I'm not convinced that the figures add much to the box - I think the points that are being made can probably be made more as succinctly, or more so, in words alone. Note that the chapter does not seem to explicitly come back to these notions, although the box does make mention of Figs 3.11 and 3.12. Succinctly, the notions are (1) the response of impacted systems is likely non-linear, and critical thresholds at which extreme impacts occur are likely system dependent; and (2) predicting the response in impacts to climate change is difficult because the distribution of weather and climate extremes will change in complex ways (means, variances and even the shape of the distribution may change) and also, impact response functions and associated critical thresholds may change due to climatic conditions or adaptation, or both. I don't think much more needs to be said. (Zwiers, Francis, Environment Canada)	Box 3.1. was donated to chapter 1; main message was retained succinctly in revised version (3.1.2 and other FAQs/boxes)
283	3	6	31	6	31	Box 3.1, Figure 1, for clarity of the figure, it is suggested to include description of the vertical axes. (Bitner-Gregersen, Elzbieta Maria, Det Norske Veritas AS)	Box 3.1. was donated to chapter 1
284	3	6	31	6	41	The assertion that non-linear effects are generally linked with discrete thresholds is not proven by the Corti et al. reference (which refers to a very limited issue), nor does it seem to be proven anywhere in the text. This and related statements, and the box, need to be dropped or greatly changed. This weakens greatly what is otherwise a very good chapter. (Solomon, Susan, NOAA)	Box 3.1. was donated to chapter 1; thresholds effects are addressed more succinctly; references are added
285	3	6	33	6	33	It is not clear to me how this reference illustrates the statement about thresholds. (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	Box 3.1. was donated to chapter 1; other references are used to illustrate threshold effects in the chapter
286	3	6	33	6	34	It would be good to have not just one example for nonlinearity above thresholds when speaking from something "common". The nonlinear relations discussed in terms of wind speed induced loss are summarized in Klawe, M. und U. Ulbrich, 2003: A model for the estimation of storm losses and the identification of severe winter storms in Germany. Natural Hazards and Earth System Sciences, 3, 725-732, e.g. in their section 4. (Ulbrich, Uwe, Freie Universitaet Berlin)	see 285 and 284
288	3	6	44	6	63	It is remarkable that this box simply asserts that impact functions will have thresholds -- indeed, here it seems to assert that they all do! There is insufficient discussion in the text to support this, so at present the box seems speculative and ought to be dropped. If it is kept, then it would be important to also include the fact that some impacts may very well be linear, give examples, and include this in the diagram. (Solomon, Susan, NOAA)	Box 3.1. was donated to chapter 1
289	3	6	50	6	50	Box 3.1, Figure 2, for clarity of the figure, it is suggested to include description of the vertical axes. (Bitner-Gregersen, Elzbieta Maria, Det Norske Veritas AS)	Box 3.1. was donated to chapter 1
290	3	6	0	0	0	Box 3.1, Figure 1 is not self explanatory - indeed it is difficult to follow even with the text. Maybe some labels would help. (Church, John, CSIRO)	Box 3.1. was donated to chapter 1
291	3	6	0	0	0	Box 3.1, Figure 2 is not self explanatory - indeed it is difficult to follow even with the text. Maybe some labels would help. (Church, John, CSIRO)	Box 3.1. was donated to chapter 1
292	3	7	12	0	0	It is suggested to move the drought before parentheses. (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	Box 3.1. was donated to chapter 1
293	3	7	18	8	27	FAQ 3.1: Very well thought-out and necessary FAQ, I have heard the sentence tha extremes are going to increase far too many times. (van Oldenborgh, Geert Jan, KNMI)	Thanks.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
294	3	7	18	8	27	The concept to address FAQ is very much appreciated. (Radunsky, Klaus, Umweltbundesamt GmbH)	Thanks
295	3	7	18	8	27	FAQ 3.1: This FAQ would be more useful if it were framed to ask if some kinds of extremes are becoming more common. With the current version, many words are invested in saying this isn't the right question (IPCC WGII TSU)	The specific question as to whether a specific type of extreme is becoming more common is addressed in 3.3-3.5. This FAQ is an attempt, yet again, to stop people saying that the climate is becoming more extreme (see comment #293).
296	3	7	20	8	27	Nice FAQ, but I'm surprised that it doesn't mention the Climate Extremes Index, originally introduced by Karl et al in 1996 and recently revised by Gleason et al 2008. It is an attempt to provide an integrated and meaningful measure of climate and weather extremes for the United States. It would be useful for IPCC to comment on the utility of this index (and any comparable ones if they exist for other parts of the world). More info available at: http://www.ncdc.noaa.gov/extremes/cei/ (Staudt, Amanda, National Wildlife Federation)	CEI is now addressed in FAQ
297	3	7	20	8	27	Too much emphasis is put here on using insurance payouts (as an example of integrated measures) as an indicator of climate change. This may give a misleading message. Especially since integrated indicators are always strongly related to exposure and vulnerability and the (known) difficulties with and debate on normalizing flood losses and filtering out the climate change signal (see discussion Chapter 4, Executive Summary, page 4, lines 31-43). Also, it is well known in peer reviewed literature that after normalization there usually remains no trend in the normalized insured losses in many jurisdictions (e.g., Crompton and McAneney, 2008). Instead, in view of the question addressed in FAQ 3.1 it makes sense to put somewhat more emphasis here on the need for long enough homogeneous time series for detection analysis, especially in the case of extremes. Reference: Crompton, R.P., and McAneney, K.J., 2008. Normalised Australian insured losses from meteorological hazards: 1967-2006, Environmental Science & Policy 11(5), 371-378. (Feyen, Luc, Joint Research Centre, European Commission)	Reject. Only a single paragraph is devoted to this important and widely-discussed subject, and that paragraph is used to make exactly the point raised by the reviewer.
298	3	7	20	0	0	FAQ 3.1 -- this is a very important FAQ. To be an effective FAQ, however, it needs to be much less technical and more targeted towards summarizing in simple words the core of the scientific assessment with a wide ranging audience in mind. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Have tried to simplify language.
299	3	7	20	0	0	I understand that "is the climate becoming more extreme" features in this report as a FAQ - however it is not a scientifically well-posed question. I suggest to explain in the text that it is required to translate the question into "is the climate associated with more extreme weather phenomena and/or societal impacts", and then to make clear that the two issues are independent. For instance, it might be that the number of intense hurricanes will increase in the future climate (more extreme phenomena) but that they will less frequently make landfall (less extreme impacts). (Wernli, Heini, ETH Zürich)	Reject. This FAQ is an attempt to stop people saying that the climate is becoming more extreme (see comment #293). Question raised by reviewer is discussed in Box 3.1 (which might be moved to Chapter 1).
300	3	7	24	0	0	how is 'past' defined? (Luterbacher, Juerg, Justus Liebig University)	Deleted "in the past"
302	3	7	26	7	0	A comprehensive metric is the number of record-breaking events, examined by a simple iid-test: Benestad, R.E. (2003) How often can we expect a record-event? Climate Research Vol 23, 3-13. It does not rely on the exact shape and range of the distribution. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Agree. Added brief discussion of this sort of metric.
303	3	7	27	7	28	Insurance payouts seems to be a very indirect and problematic metric, and should not be discussed here - likewise the text from lines 22 - 25 (page 8) should be removed. (Stocker, Thomas, IPCC WGI TSU)	Disagree. Attempts to use insurance payouts to determine whether extremes (or disasters) are changing, is a controversial topic we cannot avoid. All we are saying is exactly what the review comment also says - that this is problematic. But will delete here and leave discussion to page 8).
304	3	7	27	7	28	The example of insurance payout is confusing to some extent; it could contain some pure socially and economically driven factors such as trends in building on specific areas, increasing the exposure to climate impact, independently of the changing climate. (Boiaru, Roxana, National Meteorological Administration)	See response to #303.
305	3	7	27	7	28	This is very problematic due to changes in vulnerability (insurance density, assets, building materials), claim settlement and awareness. At least, these points should be mentioned here (cf. p7, L7-14). An appropriate reference may be: Barredo, J.I., 2010: No upward trend in normalised windstorm losses in Europe: 1970-2008. Natural Hazards and Earth System Sciences, 10, 97-104 (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	See response to comments 303
306	3	7	27	7	28	I strongly disagree that we can use insurance payouts as a measure of changes in climate extremes. The Cyclone that devastated Myanmar caused over a hundred thousand deaths but I bet the insurance payouts were minimal - was this a minor event? (Church, John, CSIRO)	See response to comments 303
307	3	7	27	7	28	Changes in integrative metrics such as insurance payments seem a stupid way of indicating whether climate is becoming more extreme for which looking at climate indicators (which could be integrative across climate indicators) are appropriate because not contaminated by confounding influences. But if looking at climate related risk I can see the purpose. (Stott, Peter, Met Office)	See response to comments 303
308	3	7	27	0	0	could' is too unspecific (Luterbacher, Juerg, Justus Liebig University)	See response to comments 303
309	3	7	27	0	0	After '... such as insurance payouts' add '(if accounted for changes in values at risk)' (Neu, Urs, Swiss Academy of Sciences)	Agreed. See response to comments 303.
310	3	7	30	7	0	References are really needed here! What are the sources for these statements? What does media coverage have to do with this? (Benestad, Rasmus, The Norwegian Meteorological Institute)	Relevance is explained in following sentence (starting "With improving...")

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
311	3	7	30	7	34	This sentence is suggestive of a link between costs and observed extremes -- very dangerous juxtaposition that is very likely to be misinterpreted. This topic is not the purview of this chapter and should be dropped. Chapter 4 covers this in detail (and reaches the conclusion that no link can yet be established). (Solomon, Susan, NOAA)	FAQ considerably revised
312	3	7	30	7	34	Suggest stick to the facts. (Church, John, CSIRO)	See response to comment # 311
313	3	7	36	7	0	References needed! These sections can probably also be made more concise and more specific. Too much hand-waving now. Statistics on record-breaking events and the iid-test can provide some documentation here. Last paragraph is not really to the point, and can be shortened. I suggest avoiding sentences with 'could' and 'would' (they are a bit too fuzzy). (Benestad, Rasmus, The Norwegian Meteorological Institute)	Disagree. Addresses important and controversial point. References cannot be included in FAQ; nor can the statistics requested by reviewer.
314	3	7	36	7	46	It would be useful to include here a discussion of the fact that more extremes will be recorded with longer records, but do not necessarily indicate that there has been a change in extremes; it may simply reflect the extension of the sampling. It would be useful to bring in the study of Meehl et al., GRL, 2009 in discussing this, which used both max and min temperatures to get a meaningful measure of the impact of long-term warming. (Solomon, Susan, NOAA)	Not added, because of space limits.
315	3	7	39	0	0	The definition of the extremes is still a bit problematic. For instance, high minimum temperatures at night can also be considered extremes (see the recent Moscow heat wave). A table explaining the categories of extremes dealt with in this report maybe useful. possibly in combination with Table 3.1? (Klein Tank, Albert, KNMI)	Don't understand comment, in context of this FAQ.
316	3	7	40	7	41	AR4 WGI concluded that the decline in DTR has ceased (P 251, Fig 3.2) (Stott, Peter, Met Office)	Yes, but this FAQ does not discuss whether or not this has ceased.
317	3	7	43	0	0	Greater increases in Tmin than Tmax could still lead to more extreme impacts if they are conditioned no Tmin, eg some heatwave health effects could be linked to Tmin not dropping below a particular threshold for several nights running. (Stott, Peter, Met Office)	Do not understand relevance of ths comment.
318	3	7	45	7	47	Over what period? (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	Substantial revision to text.
319	3	7	47	7	56	Box 3.1: Another approach would be to consider changes in joint distributions of sequential day temperatures in order to incorporate the impacts of such types of extreme events (MUJUMDAR, PRADEEP, INDIAN INSTITUTE OF SCIENCE)	Not sure this comment is correctly placed?
320	3	7	53	7	53	This also depends on social and architectural habits/standards. (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	text revised
321	3	7	53	7	56	Why are two negative consequences mentioned? In many regions, mortality from cold waves exceeds mortality from heat waves currently, so the importance of a decrease in cold extremes outweighs the importance of the increase in warm extremes. at least in the short run. (van Oldenborgh, Geert Jan, KNMI)	See response to comment #320
322	3	7	54	0	0	It is not only daytime maximum temperature that is related to mortality, but also the lack of nocturnal cooling (Fischer and Schar, Nature Geosci DOI: 10.1038/NGEO866) (Van den Hurk, Bart, KNMI)	See response to comment #320
323	3	7	55	7	55	Suggest referring to forest bark beetles (as a class of pests - see the box on this topic in CCSP 3.3) rather than pine bark beetles specifically. (Zwiers, Francis, Environment Canada)	See response to comment #320
324	3	7	55	7	56	For balance the impact of extreme low temperatures (and changes thereof) on human mortality should be mentioned. (Trewin, Blair, Australian Bureau of Meteorology)	See response to comment #320
325	3	7	55	0	0	I am not an expert on this, but should this read "increases" because less frequent low temperatures generally increase diseases (Klein Tank, Albert, KNMI)	See response to comment #320
326	3	7	60	7	60	Sterl et al (GRL, 2008, already cited elsewhere) found the same tendency for the variability and shape of the PDF to cause stronger increases of summer extremes in many other regions of tyhe world. (van Oldenborgh, Geert Jan, KNMI)	Not sure this comment is correctly placed?
327	3	7	60	7	60	it should be indicator instead of indictor (Wibig, Joanna, University of Lodz)	Agreed. Thanks. Was corrected.
328	3	7	0	0	0	FAQ 3.1 - this is long, dense and opaque - suggest shorten to less than 50% of current version. (Church, John, CSIRO)	Already quite short - about one page. But was further shortened, and simplified
329	3	8	4	8	14	This doesn't seem to belong in this chapter; it is covered in chapter 4. Delete. (Solomon, Susan, NOAA)	Disagree. This para addresses question of whether insurance payouts can be used to determine if physical extremes are changing. Important and controversial topic that needs to be included. But revised text to ensure this point is clear.
330	3	8	4	8	4	Many weather of climate extremes have no economic consequences or positive ones, eg a hurricane over an ocean outside shipping routes, or an extremely nice summer in Britain. Maybe focus on relevant weather or climate etrxemes (van Oldenborgh, Geert Jan, KNMI)	Bulk of chapter does this.
331	3	8	4	8	6	I strongly disagree that we can use insurance payouts as a measure of changes in climate extremes. The Cyclone that devastated Mynmar caused over a hundred thousand deaths but I bet the insurance payouts were minimal - was this a minor event? (Church, John, CSIRO)	Agree, and this is the point of this paragraph - that these cannot be used in this way (but others do try to use them).
333	3	8	6	9	6	Suggest replacing "payoff" with "payout". Depending upon the quarter, a "payoff" can have a rather negative connotation. (Zwiers, Francis, Environment Canada)	Agreed. Was corrected.
334	3	8	6	0	0	"quantity" -> "quantify" (Van den Hurk, Bart, KNMI)	Agreed. Thanks. Was corrected.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
335	3	8	16	8	0	I would cut this paragraph – what does it really say? And, besides, the recurrence of record-breaking events (test of iid) is a metric that is appropriate for interpretation that encompasses multiple aspects in 'extremeness', as it makes no assumption about the shape or form of PDF. Hence, the statements made in this paragraph as misleading. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Text revised.
336	3	8	16	8	25	The U.S. Climate Extremes Index (CEI) is a step in this direction and should be cited (Gleason, K.L., J.H. Lawrimore, D.H. Levinson, T.R. Karl, and D.J. Karoly 2008: A Revised U.S. Climate Extremes Index. J. Climate, 21, 2124-2137) (Trewin, Blair, Australian Bureau of Meteorology)	Agreed. Include discussion of this sort of index, but earlier in FAQ
337	3	8	22	8	25	This doesn't seem to belong in this chapter; it is covered in chapter 4. Delete. (Solomon, Susan, NOAA)	See response to comment #335.
338	3	8	22	8	25	This appears to be a very risky statement! Economic measures tend to be strongly biased by population density, property distribution, etc. and therefore these instruments might be unsuitable to detect changes in the natural system. (Wernli, Heini, ETH Zürich)	See response to comment #335.
340	3	8	24	8	25	How could be avoided the mixture of climatic and non-climatic impacts in economic indices such as insurance payout? Imho, some comments would be very useful in this respect. (Bojariu, Roxana, National Meteorological Administration)	See response to comment #335.
341	3	8	24	0	0	Unclear what goal this instrument would serve? (Klein Tank, Albert, KNMI)	See response to comment #335.
342	3	8	30	9	7	Isn't it better to present this section with a figure including a flowchart and a short description that user with look at figure can understand all the definitions about extreme events, extreme weather,...? (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	Section 3.1.1. removed (comments 23, 50, 68, 226)
343	3	8	32	8	0	Repeat? Cut? (Benestad, Rasmus, The Norwegian Meteorological Institute)	Section 3.1.1. has been removed; DRM aspects are treated in chapter 1
345	3	8	35	8	36	This issue is not well covered in Box 3.1, because of unproven assumptions about thresholds. Delete the statement here. (Solomon, Susan, NOAA)	Box 3.1. was donated to chapter 1; definition of threshold-based vs probability-based indices is now provided in section 3.1.2
346	3	8	38	9	0	This is a definition of extremes that should come in the beginning. But there is also some repetition Collect all the paragraphs discussing the same, and condense the text. Is the discussion about definition of "rarity" really interesting? The point about irregular recurrence and exact percentiles can be rephrased more concisely and in a more elegant manner: "Extreme events are rare and tend to recur at irregular intervals. The question of whether their frequency or magnitude change over time is difficult to establish, as the observational records do not have sufficiently number of measurements required to provide reliable statistics on extremes - precisely because such rare events only show up in the data only a few times." (Benestad, Rasmus, The Norwegian Meteorological Institute)	Section 3.1.1. removed (comments 23, 50, 68, 226); now all definition material in section 3.1.2.
347	3	8	38	0	0	If the definitions present here, it may help to user, of course in a summary . (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	revised definition of extreme event has been included in SREX glossary
348	3	8	41	8	42	Suggest to add: For engineering purposes rare events with annual probability of occurrence of O(10 ⁻²) is used. (Eide, Lars Ingolf, Det Norske Veritas)	reject. too detailed comment
349	3	8	42	8	42	(see comment 5 concerning pdf) (Brönnimann, Stefan, University of Bern)	See answer to 236
350	3	8	43	8	45	This sentence is somewhat misleading as the chance that a single extreme event is due anthropogenic climate change can indeed be quantified (Stott, Allen, Stone, 2004). In many cases, this chance may be quite large. (wehner, Michael, Lawrence Berkeley National Laboratory)	Section 3.1.1. removed; extreme event definition has been revised (see 3.1.2)
351	3	8	43	8	46	Is it really important to make the distinction between extreme weather and extreme climate events? Seems rather arbitrary to me. (Klein Tank, Albert, KNMI)	Section 3.1.1. removed; extreme event definition has been revised (see 3.1.2); distinction is kept but less prominent
352	3	8	45	8	47	I do not agree with such a statement (unless a definition of weather vs. climate is given). From a probabilistic standpoint (which is the point of view adopted by this report), weather is a trajectory of a random process, climate is a statistical property of the random process (see famous quote attributed to Edward Lorenz (1982): "Climate is what you expect, meteorology is what you get"). The notion of duration induces too much subjectivity to make that distinction. (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	Section 3.1.1. removed; extreme event definition has been revised (see 3.1.2); weather and climate are defined in the glossary
353	3	8	45	8	47	The definition presented here of extreme climate event is confusing, to say the least. The requirement that a pattern of extreme weather persist for a season, for example, is much more than is needed for that season to qualify as extreme. And besides what is said here stays in clear contradiction with what is said in page 14, line 10. It seems much better then to invert the logic of the definition, and say something like "Over a suitable period of time, such as a season, an extreme climate event is one that yields a total or average over that period that is itself extreme, especially if it contains spells of extreme weather". (López-Díaz, José Antonio, Agencia Estatal de Meteorología (Spain))	Section 3.1.1. removed; extreme event definition has been revised (see 3.1.2)

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
355	3	8	53	8	54	I very much support this statement. At this point of reading the document the challenge becomes most apparent of clearly and consequently distinguishing between extreme events (as identified by statistical means from long time-series of local basic meteorological parameters), physical and societal impacts, and the specific weather and climate phenomena that lead to the extreme local conditions. It might be useful to add a schematic figure where the weather and climate phenomena (e.g., cyclones, blocking anticyclones, monsoon, ENSO) appear as the "drivers of extremes", then there are the local extremes (in wind, precipitation, temperature) identified by objective statistical techniques, and finally vulnerability of natural and societal systems decides about the physical and societal impacts of the meteorological extreme event. Interestingly, a non-extreme cyclone (moderate intensity) can have a huge impact if it affects a densely populated and not-well protected area. In contrast, an extremely intense cyclone can have virtually no impact if it remains over sea. In this comment I used the term "event" for the local meteorological parameters, and suggest to use the term "weather and climate phenomena" for the driving systems (cyclones, ENSO, ...). From my point of view this terminology could clarify several passages of the report as currently the term "(extreme) event" is used for both local parameters and large-scale weather systems (which I found confusing in some places). (Wernli, Heini, ETH Zürich)	Section 3.1.1. removed (comments 23, 50, 68, 226). This statement has nonetheless been retained under the new section 3.1.1. An additional figure cannot be included due to space restrictions
356	3	8	54	8	54	Isn't drought an extreme event rather than an impact? (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	Reject. Extreme event refers to all considered climate events including drought. The categorization of drought under physical impacts is a matter of definition and this is terminology used in this chapter
357	3	8	55	8	55	What is an extreme combination? (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	Section 3.1.1. removed (comments 23, 50, 68, 226); "combination" is used in new section 3.1.3; we believe this term is clear to the reader
358	3	8	59	9	2	I think the distinction between extreme weather and climate events on the one hand, and extreme impacts on the other, needs to be made more clearly here. A 1-in-5 event is not extreme, so the text should not say that it qualifies as an extreme. Nevertheless, impacts may be extreme (presumably in systems that are not well adapted to current climate conditions ...). (Zwiers, Francis, Environment Canada)	Section 3.1.1. removed
359	3	8	59	9	7	The integrative approach of risk reduction and disaster management should be mentioned in this complex definition of the term "rare". (Ammann, Walter J., Global Risk Forum GRF Davos)	Section 3.1.1. removed; DRM aspects are treated in chapter 1
360	3	8	59	9	7	It should be noted here that there has been limited work on the most extreme extremes. (Trewin, Blair, Australian Bureau of Meteorology)	Section 3.1.1. removed
361	3	8	60	8	60	At this point, it should be emphasized and clarified that a statistical definition of "extreme events" (for example, used by hydrologists since the works of Gumbel) is to be opposed to an impact-based definition of extreme. (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	Section 3.1.1. removed; this point is clarified in the revised version (see 3.1.2)
362	3	8	62	0	0	In many cases more extreme quantiles are appropriate (e.g. once in 50 yr events) but since the statistics for these events are lacking, changes in less rare events are generally considered. An important issue here is whether the extremes scale. In other words: are the changes in the moderate extremes and the extremes in the far tails of the distribution of the same magnitude? (Klein, Tank, Albert, KNMI)	This is addressed in Box ...; Section 3.1.1. has been removed
363	3	8	63	9	1	I have doubts in the concept of "frequent extremes" brought across here (and previously, see my comment 20). There are phenomena that are related to the occurrence of extremes even when they are not extreme themselves, but there is just a probability for an extreme event under the non-extreme circumstances, which is not unusual in a sense). The other possibility is that a non-local critical threshold is crossed frequently. In this case, I would not expect any major impact because of adaptation (why should anything frequently affected by a major hazard persist?!). (Ulbrich, Uwe, Freie Universität Berlin)	Section 3.1.1. removed; the concept of "frequent extremes" has been removed. The issue is rather with "extremes" (e.g. 1 in 10) which do not have impact. This has been made clear in revised version; see 3.1.2
364	3	9	10	10	15	Box 3.2: Very good (IPCC WGII TSU)	Noted. Thanks.
365	3	9	12	9	12	The authors might want to think about when they use a question as a title. Currently the title of section 3.1.1 is cast as a question along with the titles of Boxes 3.2, 3.3 and 3.4, and FAQs 3.1 and 3.2. This also raises the question of whether there is a clear distinction between the roles of FAQs and Boxes in the Chapter? (Zwiers, Francis, Environment Canada)	Agreed. Recast title so it is not a question.
366	3	9	14	9	0	Need several references here! (extremes caused by changes in mean, most climate change research, estimated change in mean temperature and precipitation *has* been used to project changes in extreme 24-hr precip,...). The discussion is too hand-wavy. Besides, this paragraph is similar to others, and could be cut or made more concise (Benestad, Rasmus, The Norwegian Meteorological Institute)	Shorten (but see comment #364)
367	3	9	14	9	60	It is suggested to mention that although moderated changes of the mean value may be observed, the changes of extreme values maybe significant e.g. see the wave paper of Grabemann and Weisse (2008). (Bitner-Gregersen, Elzbieta Maria, Det Norske Veritas AS)	Noted.
368	3	9	25	9	25	BOX 3.2 "credible" -- replace with "appropriate" or similar? (Stocker, Thomas, IPCC WGI TSU)	Replace or reword.

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369	3	9	29	9	43	Temperature extremes may adequately scale with mean temperature even when the change in variability is 'significant'. The test is how large the impact is of variability change is relative to the effect of mean change, not whether the variability change is statistically significant per se. I am not unhappy with what the paragraph concludes, but it think text should acknowledge the point I make here. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Noted. Changed "significant" to "substantial"
370	3	9	29	9	43	Have any studies put forward causes for the apparent differences in scaling of extremes with mean temperature between urban and non-urban areas? These causes also need to be briefly discussed in the section. (MUJUMDAR, PRADEEP, INDIAN INSTITUTE OF SCIENCE)	Not aware of any such studies.
371	3	9	29	9	43	Clark 2006 show regions where mean changes are not a good indicator of changes in extremes, specifically their fig 3 and 5. It would be good to acknowledge that temperature changes across the distribution can be quite complex. (Brown, Simon, The Met Office Hadly Centre)	Noted.
372	3	9	29	9	43	Brown et al 2008 fig 4 shows where changes in the mean and extremes have not been the same in the observed daily temperature record. This would be useful to cite here. (Brown, Simon, The Met Office Hadly Centre)	Noted. Included reference.
374	3	9	38	9	39	"If there is evidence that temperature variations...had become significantly larger...it would be reasonable to conclude that temperature ... had become more extreme." I do not agree with this statement. Extremes may change differently than variability. (Brönnimann, Stefan, University of Bern)	Refers to page 7, not 9. Rewrite sentence to clarify.
375	3	9	41	9	41	There is no discussion of high-latitude locations in the preceeding text to support the assertion that changes in variance should also be considered at these locations. In addition to observational evidence, it might also be useful to briefly mention how temperature extremes scale in climate models (e.g., Hegerl et al., 2004 or Kharin et al., 2007). (Zwiers, Francis, Environment Canada)	This is a conclusion from the cited study - rewrite to clarify.
376	3	9	45	9	45	Replace "northern" with "North". (Zwiers, Francis, Environment Canada)	Text now under Section 3.1.6. This sentence is not there anymore
377	3	9	45	9	48	Also found for other regions of the world, including southern Europe (Klein Tank, Albert, KNMI)	Text now under Section 3.1.6. This sentence is not there anymore
378	3	9	47	9	48	There is evidence that hourly precipitation extremes scale even stronger than daily precipitation extremes (Lenderink, Nature Geoscience, 2008, doi:10.1038/ngeo262) (van Oldenborgh, Geert Jan, KNMI)	Noted. Thanks. Include citation.
379	3	9	50	9	0	Benestad, R.E.(2007) 'Novel Methods for Inferring Future Changes in Extreme Rainfall over Northern Europe Climate Research, CR34:195-210, doi: 10.3354/cr00693' examined the relationship between the shape and percentiles of 24-hr wet-day precipitation PDFs for different locations around Europe, and found a statistically significant relationship between the mean temperature and precipitation and the slope parameter of best-fit exponential distributions. This relationship was validated against independent locations as well as variations in time (Benestad, R.E. (2010) 'Downscaling Precipitation Extremes: Correction of Analog Models through PDF Predictions', Theor. & Appl. Clim, Volume 100, Issue 1, DOI: 10.1007/s00704-009-0158-1.(on-line version from 2009)). Furthermore, the relationship between the slope parameter of the exponential distribution was combined with similar analysis for wet-day frequency to make scenarios for future 95-percentiles for 24-hr precipitation. These results are derived from a large multi-model ensemble (CMIP3), rather than one or a few GCMs. Scenarios 2050 suggested 95-percentiles of up to 120-130% of present value for low-lying regions in southern Sweden, British isles, Denmark, northern Germany and the Netherlands. Themessl et al (2010) 'Empirical-statistical downscaling and error correction of daily precipitation from regional climate models' Int. J. Clim, DOI: 10.1002/joc.2168 writes a precautionary note: "State-of-the-art RCMs feature significant errors and are therefore often not directly applicable to climate change impact research.", being relevant for extremes. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Noted. Include citations to this work where appropriate.
380	3	9	50	9	60	In Japan, influence of global warming on precipitation including heavy precipitation during warm season was studied using RCMs nested in high resolution GCMs. The results show that rates of frequency of intense precipitation increase in the warm climate (Wakatsuki et al., 2008; Kitoh et al., 2009). Wakazuki,Y., M.Nakamura, S.Kanada, and C.Muroi, 2008: Climatological reproducibility evaluation and future climate projection of extreme precipitation events in the Baiu Season using a High-Resolution Non-Hydrostatic RCM in comparison with an AGCM. Journal of the Meteorological Society of Japan, Vol. 86 (2008), No. 6, 951-967. Kitoh, A., T. Ose, K. Kurihara, S. Kusunoki, M. Sugi and KAKUSHIN Team-3 Modeling Group, 2009: Projection of changes in future weather extremes using super-high-resolution global and regional atmospheric models in the KAKUSHIN Program: Results of preliminary experiments. Hydrological Research Letters, 3, 49-53. (Kurihara, Kazuo, Meteorological Reserach Institute)	Not relevant to this specific discussion.
381	3	9	55	9	55	I am not sure that this is straightforward. The C-C relation states something about the pressure of saturated vapour, i.e. the ratio of liquid vs. gaseous water. It is not clear that there is more water, just from that statement. (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	Noted. Perhaps delete last half of this setence?

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
382	3	10	1	10	0	The cited study analysed temperature and precipitation from GCMs – models with coarse resolution. To what extent – temporal and spatial scales – do these models reproduce real features? Often extremes are more local in nature or rely on blocking – whose recurrence is not well reproduced by models with coarse resolution – or soil moisture. Caveats such as these are discussed only loosely in lines 31-39, but my suggestion is to bring these paragraphs together. This also ties in with L25-44 on p. 11. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Reject. Too much detail for the point reached in this Box.
383	3	10	3	10	3	"Cold extremes warmed faster..." Please reword (Brönnimann, Stefan, University of Bern)	Wording seems correct.
384	3	10	3	0	0	Are cold and warm defined here by the 10th and 90th percentiles? (Klein Tank, Albert, KNMI)	Delete so this detail is no longer required here.
385	3	10	4	10	4	How is this percentage defined? (Trewin, Blair, Australian Bureau of Meteorology)	Delete.
386	3	10	9	10	13	In principle I agree with this statement; however, it is not consistent with the statement on page 5, L5-7 (The probability of an extreme event ... is closely related to the statistical properties of climate...). Furthermore, it should be clarified which weather systems / met. variables scale with the mean and which not and what's the physical explanation behind this. Besides, I miss a reference (or more) for this topic. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	This para summarises studies cited in above paras. Rest of comment requires more detail than is available or necessary to make the point of this paragraph.
388	3	10	11	10	11	BOX 3.2 "credible" -- delete (Stocker, Thomas, IPCC WGI TSU)	Delete and reword.
389	3	10	12	10	13	Can you elucidate why this is the case for short duration precip and temperatures at urban locations and in mid and high latitudes? (Stott, Peter, Met Office)	No - studies demonstrate that this is the case, but do not provide explanation.
390	3	10	13	10	13	It should not be assumed that the distributions of extremes in high latitude regions won't scale with changes in the mean. This statement should be modified. You may wish to consider/reference the paper by Zazulie et al., J. Clim., 2010, which explicitly shows that the extremes at one station in the Antarctic peninsula region have indeed scaled with the mean -- and have done so as that part of the world has undergone very large warming. (Solomon, Susan, NOAA)	Reject. Statement is general and text notes exceptions.
391	3	10	14	10	14	The box hasn't really discussed precipitation - and the zeroth order expectations for scaling in precipitation would be quite different than for temperature. The zeroth order expectation for temperature is indeed a shift of the entire distribution (this is in fact not really scaling - i.e., multiplication of all values by a constant factor - but rather just the addition of a constant value across the distribution). The zeroth order expectation for precipitation is scaling of short period (e.g., daily) amounts by a multiplicative factor, resulting in a change in the mean and also the variance, although not the shape of the distribution. (Zwiers, Francis, Environment Canada)	Reject. Too detailed for the point of this discussion. Moreover text has been significantly shortened following comments from reviewers (under new section 3.1.6)
392	3	10	18	0	0	Section 3.1.1.2.: This section is redundant because the issues are discussed extensively in section 3.2. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Section was removed because of overlap with 3.2
393	3	10	23	10	23	"(despite a number of issues with these data) -- suggest to delete this bracket and the comment; does not add anything important here (and thus is given brackets, I guess). (Stocker, Thomas, IPCC WGI TSU)	Section was removed because of overlap with 3.2.; the issues are treated in that section
394	3	10	31	10	0	The reason why the models disagree may be due to internal chaotic variability on decadal to inter-decadal scales. Each of these may be equally realistic, but even though one individual scenario is not reliable in terms of its trajectory, the model runs may still produce fairly reliable statistics for the variability based on the combined forced and internal component. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Section was removed because of overlap with 3.2
395	3	10	31	10	32	"credibly", "reliably" -- suggest to not use "credible", "reliable" etc. in connection to projections. Projections of climate are laying out possible future climates asking "what-if". I don't think this should be expressed using words like "credible" or similar. (Stocker, Thomas, IPCC WGI TSU)	Reject: credible, reliable are common terms in climate research community. Section was removed because of overlap with 3.2, but terms are used elsewhere in the chapter
396	3	10	31	0	0	I highly doubt that GCMs incorporate all the 'relevant processes' for many types of extremes. (Stewart, Ronald, University of Manitoba)	This is exactly what this section was about. Section nonetheless removed following restructuring (topic addressed under 3.2)
397	3	10	32	10	32	This sentence sets out a requirement for GCMs, but wouldn't one have the same requirement for RCMs, and an analogous requirement for statistical downscaling (i.e., that the technique has been exposed to sufficient observational data to learn, empirically, the full scope of the present and future relationship between the large scale predictors and small scale predictands)? (Zwiers, Francis, Environment Canada)	This is correct and is addressed under section 3.2 (previous section 3.1.1.2 removed)
398	3	10	39	0	0	please give some examples for ...other extremes... (Luterbacher, Juerg, Justus Liebig University)	Section was removed because of overlap with 3.2
399	3	10	41	10	41	Suggest a shorter title "Confidence in Estimates of Changes in Extremes" - there are numerous examples in the text where similar direct constructions would make the chapter easier to read. (Church, John, CSIRO)	Section was removed because of overlap with 3.2
400	3	10	41	10	42	Tropical cyclones are an interesting philosophical question here - are they an extreme in their own right or phenomena which force other extremes (e.g. strong winds, heavy rainfall, high sea levels)? (Trewin, Blair, Australian Bureau of Meteorology)	Seems to be referring to wrong part of the text; general answer: tropical cyclones are considered in chapter 3 as climate phenomena, hence second category
401	3	10	41	11	17	Assessment of Confidence -- should refer here to the revised IPCC Uncertainty Guidance Document. Note that this revised uncertainty guidance note of IPCC AR5 should become available soon (early November, in advance of the WGI LA Meeting 1). (Stocker, Thomas, IPCC WGI TSU)	This text is now in revised form under section 3.1.5; uncertainty guidance is referred to
402	3	10	44	10	44	"credibility of climate models" -- suggest to reformulate, e.g. "the ability of climate models to capture...". (Stocker, Thomas, IPCC WGI TSU)	see 395

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
403	3	10	48	10	0	References and documentation needed regarding model simulation of temperature and precipitation extremes! What kind of models and what kind of extremes? Where? (Benestad, Rasmus, The Norwegian Meteorological Institute)	Performance of model is addressed under 3.2
405	3	10	51	10	51	"...recent studies...": which ones? (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Hawkins and Sutton 2009 and 2010; this is addressed in section 3.2; this text is not anymore available in this form under section 3.1
406	3	10	51	0	0	Please cite some of the "recent studies". One potential reference from Belgium would be: "Baguis P., Roulin E., Willems P., Ntegeka V. (2010), 'Climate change scenarios for precipitation and potential evapotranspiration over central Belgium', Theoretical and Applied Climatology, 99(3-4), 273-286; doi 10.1007/s00704-009-0146-5" (Willems, Patrick, Katholieke Universiteit Leuven)	See 405; suggested study is too local, cannot be considered
407	3	10	52	10	52	This statement needs a reference. (Solomon, Susan, NOAA)	see points 405 and 406
408	3	10	55	10	55	"convective systems" should be added to the list in parentheses (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	This text is removed due to space limitations; but this issue is addressed under 3.2
409	3	10	55	0	0	Along the line of comment 11, I suggest to delete here "tropical and extratropical cyclones" - they are not per se "extremes" and belong to a different category than "wind extremes". (Wernli, Heini, ETH Zürich)	correct; but text was removed altogether due to space limitations (now addressed under 3.2)
410	3	10	57	10	0	Confidence also depends on the amount and quality of observational data. (Benestad, Rasmus, The Norwegian Meteorological Institute)	This is mentioned in new section 3.1.5
411	3	10	57	10	59	Should it be stated somewhere that confidence does not pertain to a rigorous probabilistic definition, and is based on the heuristic hypothesis that "truth" is bracketed by models? (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	don't understand this comment; when we state that we have low confidence in projections of a given extremes, this means that we do not even assess if the models may include the truth or not because uncertainty is too large; we only assume that models "bracket" the truth in the cases of high confidence; in case of medium confidence, we only assume that they can provide tendency of change but not necessarily quantitative information
412	3	10	57	10	59	The statement is clearer here than in the Executive Summary. Could be simplified even further. For example: "Our confidence in connecting trends in extremes to climate change depends on the type of extreme, the region, and season, as well as how well we understand and can simulate the underlying processes." (Staudt, Amanda, National Wildlife Federation)	Executive summary has been significantly revised
413	3	10	61	11	17	As noted in the general comments, this paragraph could be made more concise and included within a new box outlining the basis for likelihood and uncertainty language used in the SREX report as a whole. (Stocker, Thomas, IPCC WGI TSU)	Paragraph has been revised according to new uncertainty guidance
414	3	10	61	11	17	Presumably this chapter, and others, will be adjusted to use the revised guidance on uncertainty language, which is anticipated to be available in Nov, 2010. (Zwiers, Francis, Environment Canada)	See 413
415	3	10	61	11	17	This paragraph is very clear and in agreement with the recommendations of the IAC review of AR4 procedures. However, in practice there are no separate confidence statements either in the summeray, nor in Table 3.1. These should follow the guidelines of this paragraph. (van Oldenborgh, Geert Jan, KNMI)	see 413; this has been now applied consistently for all assessments (see sections 3.3. to 3.5)
416	3	10	61	11	17	See also the recent IAC recommendations for the treatment of uncertainty information (Klein Tank, Albert, KNMI)	See 413
417	3	10	61	12	59	Is Table 3.1 based on AR4? If YES, authors should add "over most land areas" at the temperature line. (Zhao, Zong-Ci, National Climate Center)	Table 3.1 reflects the IPCC SREX assessments not AR4; table now mentions that assessment is provided on the global scale
418	3	10	61	12	59	Table 3.1, at precipitation line and observed row, adding "over most areas" (Zhao, Zong-Ci, National Climate Center)	Revised this assessment substantially based on SREX material. Now states that increase is observed in more regions than those with decreases
419	3	10	61	12	59	Table 3.1, at wind line and projected row, authors should mention that annual wind or strong wind? Annual or seasonal (which seasons)? All or most or parts of regions of mid- to high- latitudes? (Zhao, Zong-Ci, National Climate Center)	Reject. Not enough information available (see also ES)
420	3	10	61	12	59	Table 3.1, at droughts line and observed row, adding "since 1970" (Zhao, Zong-Ci, National Climate Center)	Statement significantly revised. All provided assessments are for the period since 1950.
421	3	10	0	10	0	P 10, bold statement: depends on is the wrong verb. The meaning is closer to varies with (IPCC WGII TSU)	Agreed, sentence was changed.
422	3	11	8	0	0	add ', not homogenous' after quality (Luterbacher, Juerg, Justus Liebig University)	Was changed in new section 3.1.5
423	3	11	12	0	0	something is missing in this sentence ...change., however (Luterbacher, Juerg, Justus Liebig University)	Sentence was removed since this referred to our previous assessment approach
424	3	11	13	11	17	This confirms my statement above: all those adjectives are subjective and are NOT based on a probabilistic framework of climate variability. This is not a problem in itself, but should be acknowledged. (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	Yes, confidence levels are based on expert opinion and not on a probabilistic framework. This is now explicitly mentioned in the text (Section 3.1.5)
426	3	11	20	12	23	Box 3.3 Figures 1 and 2: Don't add much to the content of the box. A better figure would somehow show how confidence increases with the size of the spatial domain. Something with, for example, concentric rings of confidence could be terrific. (IPCC WGII TSU)	(new box 3.1) Figures were removed (but referred to in the text using AR4 reference). Do not think the suggested schematic would add anything to the text.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
427	3	11	22	11	22	Box 3.3. -- change title to (1) not be formulated as a question and (2) to not use "Credibility" (see several of the previous comment on this) (Stocker, Thomas, IPCC WGI TSU)	Agree. Changed to "Variations in confidence of projections of climate change"
428	3	11	22	11	23	I think the use of the term "credibility" is not appropriate in this context. I would suggest something like "the skill" of climate change projection. (Bojariu, Roxana, National Meteorological Administration)	See response to comment 427
429	3	11	22	12	23	This box seems repetitive with text elsewhere in the chapter and it is not clear to me that it adds much. Delete? (Solomon, Susan, NOAA)	Box was kept (now Box 3.1), but overlaps with text have been carefully checked and removed.
430	3	11	22	0	0	This Box is about extremes, but the text largely refers to projected changes in the mean. Suggest to change the order of the text. (Klein Tank, Albert, KNMI)	See suggested new title in response to comment 427.
431	3	11	22	0	0	I think that somewhere in this section should be mentioned that the agreement between observed and projected extremes under present climate conditions doesn't guarantee solely a high confidence for the future extreme projections. A more reliable approach would be to check the agreement of observed and simulated mechanisms involved in extreme generation. Here is the link with the phenomena like ENSO and NAO. (Bojariu, Roxana, National Meteorological Administration)	Although the reviewer is correct, this is not the point of this box, which is to indicate that our confidence in projections varies geographically and between variables.
432	3	11	22	0	0	Box 3.3 should be about extremes but most of the text is not about extremes! In the present form this box tries to hide the fact that uncertainties of climate change projections are large when it comes to extremes - by writing a lot about the more robust parameters like global mean temperature. I suggest to strongly shorten the text on averaged quantities and make clear that they are mentioned briefly to put the larger uncertainties associated with the extremes in context. Also, the final sentence of the box is rather weak. (Wernli, Heini, ETH Zürich)	Revised to ensure that the discussion of the means is to place the discussion on the extremes in context. Don't understand last comment, since the last sentence of the box says exactly what the reviewer is pointing out, namely that confidence in projections of extremes is lower than for means
433	3	11	22	0	0	Box 3.3: there is an inconsistency between the heading (how does the credibility ... of extremes differ geographically ...?) and the text: the first two paragraphs (p10, L25-44) discuss only means with respect to the location, while the last paragraph (p12, L7-17) discusses uncertainties of extremes, but without considering regional differences. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	See response to comment 427 and 432.
434	3	11	25	11	0	Good agreement for many variables... Which in particular? "An intercomparison of future climate changes between models shows a better agreement for changes in temperature than that for precipitation and sea level pressure, but some aspects of change in the latter two variables are also quite consistent between models." - also see Fig. 6 in the paper: Do the climate models really demonstrate a good agreement? New reference is needed for the statement about model failure without anthropogenic influences. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Cited reference is sufficient detailed to make this point.
435	3	11	25	11	26	This statement is too general and should be more specific (which time period, which parameters? means or extremes?) (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	The simple point being made here, and supported by the cited reference, does not need the extra detail suggested by the reviewer.
436	3	11	25	11	35	This para is misleading or potentially so. As the scale reduces the S/N will generally decrease but the confidence in the projection does not necessarily reduce. For example a perfect model would capture the future mean climate in a small region in the late 21st century and its variability perfectly, even if there is no initial condition predictability by then and the year to year variability is greater than the mean change. The reasons why we would have less confidence in projections at smaller scales relates to the models ability to simulate the relevant processes not to the effects of S/N per se. (Stott, Peter, Met Office)	Reword. The inability of models to simulate the processes that are relevant at smaller scales (and which are averaged out at larger scales) does mean that the S/N for smaller scales is noisier (and explains why we do have more accurate simulations at large scale than at small scale).
437	3	11	25	11	44	I think that somewhere in this section should be mentioned that the agreement between observed and projected extremes under present climate conditions doesn't guarantee solely a high confidence for the future extreme projections. A more reliable approach would be to check the agreement of observed and simulated mechanisms involved in extreme generation. Here is the link with the phenomena like ENSO and NAO. (Bojariu, Roxana, National Meteorological Administration)	See response to comment 431.
438	3	11	26	11	26	I would prefer keeping the global figures global, rather than splitting figures like Fig 9.12 from WG1 AR4 into two parts. I have a similar comment (below) about Figs 3.1-3.4. (Zwiers, Francis, Environment Canada)	See response to comment 426.
440	3	11	33	11	33	I disagree with the assessment that consistency is poorer on subcontinental scales. From a statistical perspective, the consistency between models and obs is equally as good in the subcontinental panels as it is in the global panels - observations do not depart from the uncertainty band derived from model simulations of the twentieth century any more frequently on the regional scales than on the global scale. The behaviour seen in the regional panels is not indicative of a model problem (which is what is implied with the word "poorer") but rather the reduced ability to filter out internal variability on regional scales. (Zwiers, Francis, Environment Canada)	This comment contradicts comment by Stott at 436. Will rewrite text to try to satisfy these contradictory comments. The figure clearly demonstrates wider inconsistency between model simulations at smaller scales.
441	3	11	34	11	35	This statement should be proven by a reference (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Box cites appropriate references already.
442	3	11	37	11	44	The increased uncertainty at smaller scales is also due to systematic problems in the climate models. Van Oldenborgh et al (Clim. Past, 2009, doi:10.5194/cp-5-1-2009) found that in Europe, observed local trends are outside the CMIP3 ensemble spread (conversely, the ensemble mean is also outside the observational uncertainty at >2 sigma over large areas of Europe). The same result holds for global maps (unpublished). These systematic differences include changes in circulation outside the natural variability, aerosol, cloud, soil moisture and snow treatment, missing fog parametrisation (for the last factor see Vautard et al, Nature Geoscience, 2009, doi:10.1038/NNGEO414). (van Oldenborgh, Geert Jan, KNMI)	Too much detail for point being made in this Box.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
443	3	11	37	11	44	I recommend to briefly discuss the difference between 1) regional "stochastic" noise (from chaotic weather and atmospheric circulation variability), which is unpredictable in the long range, and 2) regional variability which is due to known processes or phenomena (like monsoon, El Niño, orographic effects) and might possibly be simulated and projected in the future. (Neu, Urs, Swiss Academy of Sciences)	Too detailed for simple point being made here - that users of climate projections need to consider the variations in our confidence in their projections.
444	3	11	38	11	38	I'm not sure how you would judge that there is less consistency due to model uncertainty in the face of a much reduced signal-to-noise ratio due to a reduced ability to filter away the effects of internal variability. (Zwiers, Francis, Environment Canada)	It doesn't matter what causes this - the point is that there is a difference in confidence related to scale. That is the simple point being made here, for an audience that has to decide how to use the projections.
445	3	11	41	11	41	"In the tropics the signal expected" => "In the tropics the temperature signal expected" (for precipitation, this is not true) (van Oldenborgh, Geert Jan, KNMI)	Rewritten to clarify that this statement is about temperature.
446	3	11	52	11	52	In Figure 1 (Box 3.3), only 8 and not 22 sub-continental scale regions are shown (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Figure was deleted.
447	3	11	0	11	0	P11: confidence text needs to be harmonized with revised uncertainty guidance (IPCC WGII TSU)	Noted.
449	3	12	7	12	0	RE-phrase the sentence "There is more model uncertainty". Also, wind should be mentioned, and the fact that measurement of extreme wind and precipitation is difficult because they often involve small spatial scales – e.g. falling between rain gauges – or so intense that the instruments fail (e.g. blow away or get taken by the flood). Another issue is that temperature anomalies tend to involve larger spatial scales than precipitation and wind do. Without good measurement, it's hard to know how well the model do... (Benestad, Rasmus, The Norwegian Meteorological Institute)	Too detailed for simple point being made here.
450	3	12	7	12	17	This repeats things said in almost similar terms earlier in the section. (Lavell, Allan, Programme for the Social Study of Risk and Disaster (FLACSO))	Revise and shorten to avoid duplication.
451	3	12	9	12	10	"Thus climate models simulate changes in extreme temperatures quite well," Kharin et al (J.Clim, 2007, doi:10.1175/JCLI4066.) and Sterl et al (GRL, 2008, doi:10.1029/2008GL034071) find differences of 5 degrees in T20/T100 in arid regions (their Fig.4, Fig.2 respectively), is this "quite well"? (van Oldenborgh, Geert Jan, KNMI)	This statement is clearly within the context of comparing the performance of temperature simulations with those of other variables that are more poorly simulated. Replaced "quite" with "relatively" to stress this point.
452	3	12	9	12	9	Delete "Thus" ("Thus" implies that you are drawing a conclusion from previously presented evidence, while in this case the intent is to state a finding from Randall et al (2007)). (Zwiers, Francis, Environment Canada)	Replaced "Thus" with "For instance".
453	3	12	14	0	0	True not only for tropical cyclones, but also for extratropical storms. (Klein Tank, Albert, KNMI)	TCs are used here as an example, not meant to be comprehensive.
454	3	12	14	0	0	Not all tropical cyclones are extreme (the term tropical cyclone includes also relatively weak vortices that can not be classified as hurricanes/taifuns). As mentioned in comment 11, I suggest to clearly separate the weather phenomena like tropical cyclones from the local events that can be induced by the passage of these phenomena. (Wernli, Heini, ETH Zürich)	The term "tropical cyclone" requires exceedance of an intensity threshold. There is no better term than "extreme" for use here.
455	3	12	17	12	17	In a recent study, Kunz et al. (2010) investigated the ability of different RCMs to realistically simulate extreme wind speeds over Central Europe. They found that all RCMs tend to underestimate gust wind speed between 10 and 30%, whereas the spatial distribution is well reproduced. Reference: Kunz, M., S. Mohr, M. Rauthe, R. Lux, and Ch. Kottmeier, 1020: Assessment of extreme wind speeds from regional climate models - Part I: Estimation of return values and their evaluation. Natural Hazards and Earth System Sciences, 10, 907-922. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Too detailed for purpose of this box.
456	3	12	26	12	34	3.1.1.4 is the one part of 3.1.1 that makes sense in ch 3 (IPCC WGII TSU)	All of section 3.1.1. has been removed. 3.1.1.4 is now addressed in a case study under chapter 9
458	3	12	28	12	0	Is it established whether seasonal-to-interannual predictions can give useful information on extreme events? The paragraph must be more specific. E.g. one issue is ENSO and forecasting of such phenomena, but more reference ought to be given assessment of the seasonal forecasts of the tropical cyclones. Another issue is whether model initialisation – and potential shortcomings with data assimilation - that may affect such forecasts can tell us anything about the model response to changes in the boundary conditions (i.e. predictability of first kind versus second kind). (Benestad, Rasmus, The Norwegian Meteorological Institute)	All of section 3.1.1. has been removed. 3.1.1.4 is now addressed in a case study under chapter 9
459	3	12	28	12	30	The sentence is misleading. Climate models are not generally used for subseasonal to interannual predictions, while there is some overlap with models used for these purposes, but not in a global warming context. It is also not relevant for the report that first experiments on decadal prediction are on the way. If you meant that Climate models can be used to address the occurrence of extremes by covering all relevant time scales (and thus helping adaptation, see following sentence) I would agree. This should, however, not be mixed up with the term "forecast". (Ulbrich, Uwe, Freie Universitaet Berlin)	All of section 3.1.1. has been removed. 3.1.1.4 is now addressed in a case study under chapter 9
460	3	12	28	12	34	It would be useful to talk about forecasting and how forecasting advances adaptation, but it should be noted that this is still an area of research that is in an embryonic state, particularly with respect to extremes. I would think that the key short term benefit would be to aid in adaptation to present day climate variability rather than climate change (as stated on line 31). (Zwiers, Francis, Environment Canada)	All of section 3.1.1. has been removed. 3.1.1.4 is now addressed in a case study under chapter 9

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
461	3	12	28	12	34	I think this subsection is way too short. It should be expanded and documented with some examples, describing the products availability and reliability with some details for the single continents and regions. Furthermore if seasonal predictions could be used in early warning system of climate extremes, monthly probabilistic predictions (Vitart, F., 2004: Monthly forecasting at ECMWF. Mon. Wea. Rev., 132, 2761–2779, or Vitart, F., S. Woolnough, M.A. Balmaseda & A. Tompkins, 2006: Monthly forecast of the Madden-Julian Oscillation using a coupled GCM. Mon. Wea. Rev., 135, 2700–2715 or Ferranti, L., T. N. Palmer, F. Molteni, E. Klinker, 1990: Tropical-Extratropical Interaction Associated with the 30-60 Day Oscillation and Its Impact on Medium and Extended Range Prediction. Journal of the Atmospheric Sciences: Vol. 47, No. 18, pp. 2177-2199) or medium range probabilistic high resolution forecasts can be used to reduce impacts of floods and storms. (Montani A., Marsigli C., Nerozzi F., Paccagnella T., Tibaldi S., Buizza R., 2003. The Soverato flood in Southern Italy: performance of global and limited-area ensemble forecasts. Nonlin. Proc. Geophys., 10, 261-274, or Marsigli C., Boccanera F., Montani A., Paccagnella T., 2005. The COSMO-LEPS mesoscale ensemble system: validation of the methodology and verification. Nonlin. Proc. Geophys., 12, 527-536.) Furthermore the discussion should include a description of the current knowledge on sources of predictability, in particular ENSO, MJO, tropospheric-stratospheric interactions, sea-ice and snow cover, together with the relevance of land-surface atmosphere interaction which are already mentioned many times in this chapter.....Finally, I think that this section should include a brief description of good practices and references to few documents describing them in detail. (Pavan, Valentina, ARPA Emilia-Romagna)	All of section 3.1.1. has been removed. 3.1.1.4 is now addressed in a case study under chapter 9
462	3	12	29	12	29	"prediction" -- replace with "projections"; climate models can not be used for predictions, only for projections. They are not to be confused with models used in NWP. (Stocker, Thomas, IPCC WGI TSU)	Seasonal forecasting include predictions not projections; but this section has been removed
463	3	12	29	12	29	Please define the expression "short-term" in this context (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	All of section 3.1.1. has been removed. 3.1.1.4 is now addressed in a case study under chapter 9
464	3	12	30	12	31	In my opinion, warning systems are not relevant for adaptation which is dealing with a long term response to threats. They are relevant for the short term response to extremes. (Bojariu, Roxana, National Meteorological Administration)	All of section 3.1.1. has been removed. 3.1.1.4 is now addressed in a case study under chapter 9
465	3	12	32	0	0	needs citations. (Incecik, Salahattin/Selahattin, Istanbul Technical University)	All of section 3.1.1. has been removed. 3.1.1.4 is now addressed in a case study under chapter 9
466	3	12	33	12	33	reference to Chapter 9? Is there a Case Study dealing with advances in the predictability of extremes? (Stocker, Thomas, IPCC WGI TSU)	Section 3.1.1.4 was removed. No reference to case study as part of our chapter anymore.
468	3	12	36	12	46	I think the approach is very much like the one used for AR4. This report should go beyond. When you classify as "Weather and Climate elements (temperature, precipitation and winds" you are using a very classical approach. Severe weather clearly stands out as not being included. (Silva Dias, Maria Assuncao, University of Sao Paulo)	Reject. Not clear what the reviewer is asking in terms of expansion of categories
469	3	12	36	13	23	3.1.2 Handoff from ch 3 to 4 needs to be examined very carefully. Is there a strong motivation for including clear impacts (landslides, dust storms) in ch 3? Even with events with a stronger meteorological connection (floods and droughts), the handoff should be fine-tuned (IPCC WGI TSU)	Landslides, dust storms are physical impacts, hence ch3 material
470	3	12	36	0	0	This section about the categories covered in this chapter may be moved more upfront and accompanied by a table of the various types of extremes assessed. Is this grouping also followed in the other chapters of this report? (Klein Tank, Albert, KNMI)	The categories of extremes are now addressed at very beginning of chapter; it is not clear whether this grouping is followed by other chapter, but this is mostly an internal chapter structure
471	3	12	40	12	40	Is large-scale fog considered an extreme event? It can have large economic impacts. Qualitative projections for Europe are made in van Oldenborgh et al, Atm. Chem. Phys. 2010, doi:10.5194/acp-10-4597-2010. (van Oldenborgh, Geert Jan, KNMI)	Reject. Fog is not considered an extreme.
472	3	12	40	12	45	(see comment 6 on the temporal aspects) (Brönnimann, Stefan, University of Bern)	Not clear what this comment is referring to
473	3	12	41	12	41	The wording "Phenomena influencing the occurrence of weather and climate extremes" is confusing (e.g., cyclones are weather systems and not phenomena) and not consistent with that of the Executive Summary (p2, L17-18: Weather and climate phenomena) and the relevant Section 3.5 (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	accept, was changed to formulation of ES (see new 3.1.1)
474	3	12	44	12	44	Pedantic, but the 'cryosphere' includes permafrost. Better to just refer to cryosphere-related impacts. (Stocker, Thomas, IPCC WGI TSU)	accept, was changed (see new 3.1.1)
475	3	12	47	12	48	The possible relevance' - this doesn't seem like quite the right word because they are all clearly relevant. 'The possible importance' of these elements.... ?? (Stocker, Thomas, IPCC WGI TSU)	This paragraph was rephased (see end of new section 3.1.1). It was changed to an overview on the overall structure. "Relevance" is not mentioned anymore since all considered events are relevant
478	3	12	57	12	57	It would be useful if the table contained references to the chapter sections that support the various assessments that are listed. It would also be useful, somewhere early in the chapter, to define what is meant by a "trend". Most people will think of a linear trend, which precludes more complicated types of evolution over time. (Zwiers, Francis, Environment Canada)	Disagree. Readers of this report are unlikely to think that trends can only be linear. But will suggest to add "trend" to SREX glossary.
479	3	13	9	13	9	drought...may in many cases also be impacted by enhanced temperature. Why 'may'? Surely they will, the question is by how much. (Solomon, Susan, NOAA)	This sentence was removed to save space (issue mentioned elsewhere)

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
481	3	13	26	14	0	What is a 'climate event'? If 'climate' means 'expected weather' or 'typical weather pattern', then this phrase is not a very good one when speaking about extremes – unless one talks about ice ages. Note, weather normally refers to a particular state of the atmosphere, and does not necessarily exclude lasting conditions (e.g. blocking events). In communicating issues on climate change and extreme events, we are often faced with the confusion between 'weather' and 'climate' (e.g. Hurricane Katarina, the cold winter over Europe in 2009-2010), and the way the text is now, doesn't make it better. I'd use the phrase 'climate phenomena' instead, as this includes ENSO, monsoons, rainy seasons, etc. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Definitions are now under 3.1.2; this whole text was significantly revised
482	3	13	26	14	47	This section was written very excellent. But please add something about necessities to develop a number of indices for the other climatic parameters like wind speed, humidity, and the phenomena. (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	accept. Is now mentioned in the revised version of this paragraph (new section 3.1.2)
483	3	13	28	13	0	Isn't this repetition of previous text? The most appropriate place for this is in the beginning under a section 'Definition of extreme events' that collects all these general discussions. (Benestad, Rasmus, The Norwegian Meteorological Institute)	The text has been condensed and all the parts related to definitions are now under 3.1.2
484	3	13	37	13	41	I like this observation which calls for context dependent definitions (Klein Tank, Albert, KNMI)	Sentence was actually removed during the revision phase due to space limitation; but the revised text further highlights the need for context dependent definitions using the heatwave indices as example
485	3	13	48	13	48	Perhaps also cite Frich et al to indicate that the indices used by Alexander et al have a provenance from a longer term activity. (Zwiers, Francis, Environment Canada)	This section was significantly reduced. "Day-count indices" are not explicitly mentioned. Frich et al. 2002 is cited.
486	3	13	50	13	52	Instead of "may not" you should state that they "will not" reflect extremes in all locations. (Ulbrich, Uwe, Freie Universitaet Berlin)	The text has been significantly revised; corresponding text in revised version has been modified (see new section 3.1.2)
488	3	13	53	13	53	Replace "PDFs" with the more general term "distributions" throughout. (Zwiers, Francis, Environment Canada)	ok; see answer to comment 236
489	3	13	55	13	0	comment to "Nonetheless, the comparability... that the PDFs may look different in the tail...". The PDFs more likely look different in the tail, and one of the motivations for applying iid-tests to such data, is that it does not rely on the different series having the same PDF – see Benestad, R.E. (2003) 'How often can we expect a record-event?' Climate Research Vol 23, 3-13. L 58-62 should be re-phrased, because it is not clear what the message is. Alternatively, cut. (Benestad, Rasmus, The Norwegian Meteorological Institute)	This text has been cut due to space limitations
490	3	13	58	13	60	This statement is true only if the index is defined with respect to deseasonalised data - true of the standard ETCCDI indices but not some others. (Trewin, Blair, Australian Bureau of Meteorology)	This text has been cut due to space limitations
491	3	13	59	13	60	This is not correct, except if you write that you refer to the 10th percentile of the day of the year (which again would make the understanding of the meaning difficult). Else, in mid-latitudes an annual percentile of minimum temperature in absolute terms is not likely to be affected by a change of summer temperatures! This is even not the case if you consider fixed winter minimum temperatures and increasing summer minimum temperatures, as the percentiles for the former remain the same! (Ulbrich, Uwe, Freie Universitaet Berlin)	This is true for deseasonalized data (see 490). But this text has been cut due to space limitations
492	3	13	0	14	0	P 13-14 text on defining extremes should be in ch 1 or 2 (IPCC WGII TSU)	Reject. These are physical definitions. Chapters 1 and 2 do not deal with physical extremes, only with hazards
493	3	14	4	14	13	For some applications (e.g. urban drainage), indicators are needed at sub-daily time scales (e.g. 5 or 10 minutes). (Willems, Patrick, Katholieke Universiteit Leuven)	This text has been significantly reduced due to space limitations; time scale is not mentioned anymore
495	3	14	7	14	13	Important, but almost trivial remark. (Klein Tank, Albert, KNMI)	Remark was removed due to space limitations
496	3	14	10	0	0	The same argument holds for the spatial distribution of extremes. In an area of 100km ² a 1/100yr event of a 1 km ² rain storm occurs on average 1/100 yrs at any location, but every year somewhere in the larger domain. (Van den Hurk, Bart, KNMI)	Remark this comment was referring to was removed due to space limitations (see 495); hence spatial dimension was not added
497	3	14	13	0	0	While discussing that a "range of characteristic time scales" needs to be considered, I suggest that reference is made to IDF and QDF approaches (IDF: intensity/duration/frequency; QDF: flow/duration/frequency relationships, which are commonly used in water engineering, for instance on the basis of the construction of design events). One potential reference on IDF-relationships constructed for Belgium, which cover the range of time scales from 10 minutes to 15 days, is: "Willems, P. (2000). Compound intensity/duration/frequency-relationships of extreme precipitation for two seasons and two storm types, Journal of Hydrology, 233, 189-205" (Willems, Patrick, Katholieke Universiteit Leuven)	This was text was removed because not essential to chapter and due to space limitations; hence reference cannot be added
498	3	14	15	14	21	Note that the extremes in these analyses are not especially extreme (see my comment 4) (Trewin, Blair, Australian Bureau of Meteorology)	Accept. This is mentioned in new section 3.1.2 (mention that typical extreme indices are "moderate" and do not consider "extreme extremes")
500	3	14	17	14	0	Is the sentence "Moreover, in ..." really relevant here? (Benestad, Rasmus, The Norwegian Meteorological Institute)	Yes, relevant for several projects (e.g. ECA&D, CECILIA database); means a limitation for climate data analysis
501	3	14	18	14	18	"generally" is probably too strong as an increasing number of countries are making at least some data available. Suggest replacing with "often". (Trewin, Blair, Australian Bureau of Meteorology)	Reject. Lack of access to raw data is a major and general issue (despite a few exceptions).

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
502	3	14	23	14	23	EVT is not an alternative: it is a generalization. (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	Sentence was modified. However, EVT is not a generalization of extreme indices. Revised sentence states that EVT is "another (more general) approach"
503	3	14	23	14	42	Here the report becomes rather technical. It might be appropriate to include a box where the basics of EVT and GEV are explained and illustrated. (Wernli, Heini, ETH Zürich)	Reject. Box on EVT and GEV cannot be added due to space limitations. Text was simplified.
504	3	14	23	14	42	A significant advantage of EVT is that one is using and comparing distributions of the whole tail rather than evaluating the tail at a specific quantile and so greater statistical insight. (Brown, Simon, The Met Office Hadly Centre)	Agree, good point. Sentence was added on this (see section 3.1.2)
505	3	14	23	0	0	In my view, EVT is not really an alternative for studying the same feature. EVT is used to analyse the behaviour of extremes further in the tails of the distribution compared to the descriptive indices which refer to moderate extremes. (Klein Tank, Albert, KNMI)	This is correct, text was adapted (see also answers to 502 and 504)
506	3	14	30	14	30	Replace "very high threshold" with "high threshold". Typically, the threshold would be set below the typical annual maximum in order to permit the use of more data than is used in the block maximum approach. (Zwiers, Francis, Environment Canada)	Paragraph was significantly shortened, does not entail sentence anymore.
507	3	14	30	0	0	Next to the alternative consideration of "exceedances above a very high threshold" I suggest to complete this by "... or exceedances extracted from the time series using independence criteria". It also would be good to clarify that this method is called "POT" (peak-over-threshold) method or "PDS" (partial duration series) method. (Willems, Patrick, Katholieke Universiteit Leuven)	mention of POT was added
508	3	14	31	14	32	I disagree: there are a large number of papers which use the peak-over-threshold method in combination with the GPD. The major advantage is that this method increases the number of events considered in the analysis and, correspondingly, reduces statistical uncertainty (e.g., Brabson, B. B., and J. P. Palutikof, 2000: Tests of the Generalized Pareto Distribution for Predicting Extreme Wind Speeds. Journal of Applied Meteorology, 39, 1627–1640. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	agree, this sentence was cut; whole paragraph was very significantly shortened to save space
509	3	14	31	0	0	Do we have any indication to say that GPD is used less than GEV? I'm not so sure. Cut? (Benestad, Rasmus, The Norwegian Meteorological Institute)	agree, this was cut (see also 508)
510	3	14	31	0	0	Add the abbreviation for the Generalized Pareto Distribution: GPD (Willems, Patrick, Katholieke Universiteit Leuven)	Was not added because revised version of paragraph is much shortened and does not provide details on GPD anymore
511	3	14	31	0	0	I do not agree that the POT/PDS method is "used less frequently" in comparison with the method based on annual maxima. It clearly depends on the field of application. In (river and urban drainage related) flood frequency analysis, the POT/PDS method is becoming to be more often used. Many authors have shown that the POT/PDS method has clear advantages above the method of annual maxima, esp. when data sets are limited. One potential reference in this respect is: "Madsen, H., Rasmussen, P.F., Rosbjerg, D., 1997. Comparison of annual maximum series and partial duration series methods for modeling extreme hydrologic events. 1. At-site modeling. Water Resources Research 33 (4), 747-757" (Willems, Patrick, Katholieke Universiteit Leuven)	agree, this was cut (see also 508 and 509). Reference was not added, not critical for argumentation and relatively old.
512	3	14	32	14	32	See Nogaj et al. (2006), Yiou et al. (2008). Nogaj M, Yiou P, Parey S, Malek F, Naveau P (2006) Amplitude and frequency of temperature extremes over the North Atlantic region. Geophys. Res. Lett. 33:doi:10.1029/2005GL024251; Yiou P, Goubanova K, Li ZX, Nogaj M (2008) Weather regime dependence of extreme value statistics for summer temperature and precipitation. Nonlin. Proc. Geophys. 15:365–378 (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	We removed the long list of references in this paragraph to save space. Hence we did not add suggested references.
513	3	14	32	0	0	Accounting for non-stationarity is also straightforward when using the descriptive indices, and therefore not an immediate advantage of EVT. Moreover, including nonstationarity in EVT is not straightforward. (Klein Tank, Albert, KNMI)	Agree, sentence was removed
514	3	14	33	14	0	The reference is to a manuscript that is submitted and not guaranteed publication. The sentence 'An advantage of the GEV... account for non-stationarity... in a relatively straight forward manner' is a bit misleading. Non-stationarity has been accounted for too in GPD-type approach, and in a fairly 'straight forward manner' by allowing the exceedance level to vary. I'm not aware of much work where GPD has accounted for non-stationarity, and if it's fairly straight-f forward, I'd expect to see reference to several publications. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Agree, sentence was removed
515	3	14	33	14	33	also: Nogaj et al. (2006) (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	We removed the long list of reference to save space. Hence we did not add suggested reference.
516	3	14	40	14	42	There is a study by van den Brink and Konnen (doi:10.1029/2008GL035967) that describes a way of making estimates of very long return period (10**4 year) winds from reanalysis. The working assumption is that ERA 40 represents synoptic scale disturbances well, and that they are spatially homogeneous across a broad part of the North Atlantic, thereby increasing the sample available for analysis. It might be worth citing this paper as an example of a technique for deriving very long period return estimates (conditional on some assumption). (Zwiers, Francis, Environment Canada)	We removed the long list of reference to save space. Hence we did not add suggested reference.
517	3	14	40	0	0	In the hydrological literature, many more examples exist of using EVT for analysis of flooding, etc. (Klein Tank, Albert, KNMI)	We removed the long list of reference to save space. Hence we did not add references on EVT analyses for flooding.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
519	3	14	41	14	41	The following citations could be added: Michael Wehner, "Changes in daily precipitation and surface air temperature extremes in the IPCC AR4 models." US CLIVAR Variations, 3, (2005) pp 5-9. M.F. Wehner, Predicted 21st century changes in seasonal extreme precipitation events in the Parallel Climate Model, J. Climate 17 (2004) 4281-4290 (wehner, Michael, Lawrence Berkeley National Laboratory)	We removed the long list of reference to save space. Hence we did not add suggested references.
520	3	14	41	14	42	This list is only an arbitrary sample from the literature, for example many other studies already cited in this chapter use EVT (eg Sterl et al 2008, Sterl et al 2009, van den Brink et al (2005), Shongwe et al (2009). (van Oldenborgh, Geert Jan, KNMI)	We removed the long list of reference to save space. Hence we did not add suggested references.
521	3	14	42	14	42	Nogaj et al. (2006), Yiou et al. (2008) (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	We removed the long list of reference to save space. Hence we did not add suggested references.
522	3	14	42	14	42	A recent study using EVT for estimating changes in extreme wind speed is: Rauthe, M., M. Kunz, and Ch. Kottmeier, 2010: Changes in wind gust extremes over Central Europe derived from a small ensemble of high resolution regional climate models. Meteorologische Zeitschrift, 19(3),299-312 (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	We removed the long list of reference to save space. Hence we did not add suggested reference.
523	3	14	42	14	42	Hanel et al 2009 should be included in this list and the conclusions noted. Martin Hanel, T. Adri Buishand, and Christopher A. T. Ferro, A nonstationary index flood model for precipitation extremes in transient regional climate model simulations, JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 114, D15107, doi:10.1029/2009JD011712, 2009 (Brown, Simon, The Met Office Hadly Centre)	We removed the long list of reference to save space. Hence we did not add suggested reference.
524	3	14	42	0	0	The iid-test is designed to study changes in the intensity and frequency of extremes by examining the recurrence of record-breaking events. No reference is given to this approach, which should be relevant here (Benestad, 2003, 2004, 2006, and 2008). (Benestad, Rasmus, The Norwegian Meteorological Institute)	Agree. The iid test and Benestad (2003, 2004 and 2006) are now mentioned under Section 3.1.2. Benestad (2008) is not published in a standard ISI publication.
525	3	14	42	0	0	A useful study on the effect of outliers in GEV distributions is given by van den Brink, H.W. and G.P. Konnen, The statistical distribution of meteorological outliers; Geophys. Res. Lett., L2370, 2008, 35, 1-5, doi:10.1029/2008GL035967. (Van den Hurk, Bart, KNMI)	We removed the long list of reference to save space. Hence we did not add suggested references.
526	3	14	49	15	13	Compounding can be seen with weather events or hydrological events as shown in this section. But they can also be seen between types of event-geological, technological and climate or oceanographical. Maybe some mention of this is warranted as climate does not operate in a self contained vacuum but in real live, dynamic, multi hazard and multi vulnerability situations. So for example, if important changes in rainfall regimes are expected in earthquake prone areas with steep slopes occupied by many persons in rural or urban areas this is very important information for managers and population alike. Analysing parts of reality as independent units of information has its rationale but when it comes to managing risk only holistic, integral analyses will lead to adequate understanding of process and hopefully, intervention. This is why climate change specialists have to work together in multi interest teams if we are to hope to advance adaptation and development integrally. (Lavell, Allan, Programme for the Social Study of Risk and Disaster (FLACSO))	Reject. This is chapter 1 material and should be addressed there
527	3	14	49	15	13	3.1.4: Critical material for ch 1 or 2 (IPCC WGII TSU)	Reject. Compound events are key material for chapter 3, in particular because of their relevance for impacts to the physical environment, which are considered in this chapter. There is also an impact dimension, which should indeed be addressed in chapters 1 and 2.
528	3	14	49	15	13	3.1.4: It would be useful to include a figure (in another chapter) providing a conceptual map of types of factor interactions (IPCC WGII TSU)	Reject. Because of space limitation, such a figure cannot be included.
529	3	14	49	0	0	Is there a rigorous definition of 'compound events'? Regardless, such a definition wouldn't account for impacts from extreme events that are made worse (better) by conditions not directly linked with a previous extreme event. Is a major thunderstorm linked in part with wet ground a 'compound' event if the soil wetness was due to snow melt? (Stewart, Ronald, University of Manitoba)	There is no clear definition, but compound events are partly addressed in the literature using multivariate statistics (in particular "copulas"). This literature is now referenced in the new section 3.1.3.
530	3	14	49	0	0	Section 3.1.4 on Compound (Multiple) Events. Comment: It is suggested that some reference be made to the multivariate extreme value techniques, that offer some promise in this field (though little known to climatologists). The book "Statistics of Extremes", by J. Beirlant et al. (Wiley Series in Prob. and Stat., 2004) already contains several examples of application in the environmental sciences. (López-Díaz, José Antonio, Agencia Estatal de Meteorología (Spain))	Agree: General literature has been added. Reference to Beirlant et al. has been added
531	3	14	59	14	63	Add the following example: "spring warm precipitation causes some extreme floods in the mountainous basins having heavy snow pack. (Davtalyan, Rahman, Ministry of Energy)	We added a reference to Benestad and Haugen 2007, which addresses this point (see 532)
532	3	15	5	0	0	More references should be given. A combination of high spring-time temperature and heavy precipitation can lead to flooding (Benestad & Haugen, 2007, 'On Complex Extremes: Flood hazards and combined high spring-time precipitation and temperature in Norway', Climatic Change, vol. 85, DOI. 10.1007/s10584-007-9263-2, 381 – 406, but there may also be more references on multivariate extremes that deserve citation.. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Agree. Reference was added.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
533	3	15	11	15	12	As far as I understood at a recent workshop, the statistical sciences have recently developed ways to deal with compound events, these methods have not yet been used by the climate community though. (van Oldenborgh, Geert Jan, KNMI)	Agree. This section has been expanded (new 3.1.3) and literature has been added on this topic based on comments from other reviewers (e.g. 530, 532, 534). But literature is sparse in the climate community.
534	3	15	11	0	0	'neither climate science nor the statistical sciences...assessing whether their frequency and intensity is changing' is not well-phrased and possibly misleading: Complex extremes can be represented as multi-dimensional PDFs (as in Benestad & Haugen, 2009), which is a framework for examining frequency and intensities of combined variables (Benestad, Rasmus, The Norwegian Meteorological Institute)	Agree. Some literature is available on multivariate extreme analyses, and literature is now given on this topic. Benestad and Haugen (2009) was not found, probably refers to Benestad and Haugen (2007) already included in this section (532)
536	3	15	18	15	18	add after "soil moisture content". "soil moisture content, river flow,". (van Lanen, Henny A.J., Wageningen University)	Section (now 3.1.4) has been shortened and refocused only on feedbacks. Physical impacts are described earlier in 3.1 and in the respective sections in 3.5
537	3	15	19	15	19	Replace "after" (two instances) with "during and after". I would have thought that the lag between extreme weather and climate events and either flooding or wild fire is pretty much zero. (Zwiers, Francis, Environment Canada)	See 536; section has been shortened and refocused only on feedbacks
540	3	15	28	15	0	The example of positive feedback is not really very convincing: droughts are usually characterised by dry conditions, and evaporation is constrained to the availability of water. Also, such examples must be documented – it's not sufficient to refer to an imagined situation. I suggest cutting these lines. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Example has been expanded to better explain interactions. This is a well documented effect in the literature.
541	3	15	29	0	0	The implication seems to be that droughts are always directly lined with heat waves. This is not always correct. (Stewart, Ronald, University of Manitoba)	This is not necessarily the case, only applies to regions where soil moisture is seasonally dry (transitional climate regions between dry and wet climates). This is now specified in the text.
542	3	15	31	15	31	add between brackets "(e.g. Teuling et al., 2010, see also Box 4.4, ...". Teuling, A.J., Seneviratne, S.I., Stöckli, R., Reichstein, M., Moors, E., Ciais, P., Luyssaert, S., van den Hurk, B., Ammann, C., Bernhofer, C., Dellwik, E., Gianelle, D., Gielen, B., Grünwald, T., Klumpp, K., Montagnani, L., Moureaux, C., Sottocornola, M. and Wohlfahrt, G. (2010) Contrasting response of European forest and grassland energy exchange to heatwaves. Nature Geoscience, DOI: 10.1038/NGEO950 (van Lanen, Henny A.J., Wageningen University)	Reference has been added. Section has been significantly rewritten (now 3.1.4)
543	3	15	35	15	38	Not sure what is meant by "(more indirect)". Land-use change can have significant local impacts but are not significant globally. (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Sentence was removed, not directly relevant to chapter material
544	3	15	40	17	36	I think this rather long response should be shortened given that the conclusion is that the question is simply not easy to answer. Much of the response seems to rely upon the thesis that the correlation between component events of a compound event may change. This is a possibility, of course, but little is known about the correlations between different types of extreme events and a firmer foundation to work from might simply be that increases in the frequency of component events (whether correlated or not) would inevitably lead to an increase in the frequency of a given type of compound event unless, of course, the component events are negatively correlated. Note that an external forcing factor (such as ghg increases) may change the expected frequencies of two types of events (e.g., extreme warm temperature and extreme heavy precipitation), but that does not necessarily imply that the correlation between the occurrence of these two types of events has increased. (Zwiers, Francis, Environment Canada)	Box removed. Some text now included in main text.
545	3	15	40	17	36	Box 3.4 is rather long given the relatively low level of knowledge and weak conclusions about compound events. I have the impression that the issue of studying compound events is a bit overemphasized in the current version and I suggest to strongly shorten this box. Some formulations are particularly unclear (p. 16 line 18; p. 17 line 15) and the final example (p. 17 line 20ff; flooding due to bushfire-induced thunderstorms) appears very speculative and somehow exotic. The following is an interesting paper to discuss, dealing with the occurrence of clusters of extratropical cyclones and their impacts: Mailier PJ, Stephenson DB, Ferro CAT, et al., 2006. Serial clustering of extratropical cyclones. Mon. Wea. Rev., 134, 2224-2240. (Wernli, Heidi, ETH Zürich)	Box removed. Some text now included in main text.
546	3	15	40	17	36	Box 3.4: good section (IPCC WGII TSU)	Box removed. Some text now included in main text.
547	3	15	42	17	34	The frequency of compounded or contrasting extremes in the future is a very complex phenomena especially if they turn to humanitarian disaster. For risk reduction and disaster management concerning compounded extremes an integrative perspective involving all stakeholders and factors (e.g. socioeconomic factors) is important. Explicit references to such an integrative perspective are missing. (Ammann, Walter J., Global Risk Forum GRF Davos)	Box removed. Some text now included in main text.
548	3	15	45	15	63	Seems repetitive with material in the text; please edit so that this appears once. (Solomon, Susan, NOAA)	Box removed. Some text now included in main text.
549	3	15	45	0	0	One has to realize that bi-poles are common as well. A drought in one region may be occurring simultaneously with heavy precipitation in nearby regions due to coupled processes. (Stewart, Ronald, University of Manitoba)	Box removed. Some text now included in main text.
550	3	15	46	15	46	spelling: "Chapter" do not start with capital. (van Lanen, Henny A.J., Wageningen University)	Box removed. Some text now included in main text.
551	3	15	60	15	60	Replace "mutual correlation" with "correlation". (Zwiers, Francis, Environment Canada)	Box removed. Some text now included in main text.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
552	3	15	60	15	60	add after "...and floods), ". "...and floods, dry spells, drought, heat waves and wildfires)". (van Lanen, Henny A.J., Wageningen University)	Box removed. Some text now included in main text.
557	3	16	8	16	0	analysis of changes in monthly-mean temperatures across the globe has been carried out by Benestad, R.E. (2004) Record-values, non-stationarity tests and extreme value distributions Global and Planetary Change vol 44, issue 1-4, p.11-26: Taking the mean number of records from 17 climate stations spread around the globe, it is shown that by the end of the 20th century, it is higher than expected if the series had been stationary. The spatial auto-correlation is high for monthly mean anomalies, and hence a sub-sampling is needed to get series that are not correlated. Hence, even a fairly small sub-sample is expected to provide a representative picture of the actual situation. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Comment does not seem relevant to indicated text.
559	3	16	18	0	19	give definition of warm nights, dry days; related definitions don't appear until p31 (Stocker, Thomas, IPCC WGI TSU)	Box removed. Some text now included in main text.
560	3	16	21	16	27	A concern with this example, and many others like it, is what statisticians refer to as "selection bias" - we have noticed, anecdotally, that something is unusual, which then makes it difficult to come to an independent assessment of exactly how unusual. We somehow need to articulate objectively what constitutes a compound event (just as we know, irrespective of whether we have just experienced one, what is meant by a 100-year precipitation event). A further comment is that this paragraph (and perhaps this box - see also the next paragraph on ENSO impacts) uses the notion of external forcing in a loose way. A persistent circulation regime (e.g., a blocking situation) is external to something, but it is not external to the climate system. (Zwiers, Francis, Environment Canada)	Box removed. Some text now included in main text.
561	3	16	21	16	27	This para can be deleted. It is too specific (Luterbacher, Juerg, Justus Liebig University)	Box removed. Some text now included in main text.
562	3	16	29	16	0	Does this paragraph belong to a different chapter? There are similar paragraphs discussing risk/vulnerability on other pages too, and the report can be more concise and to-the-point by collecting these to one place, making reference to other relevant chapters, and removing repeating statements. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Box removed. Some text now included in main text.
563	3	16	32	16	32	Suggest "yields of certain crops" instead of "crop yield" (Trewin, Blair, Australian Bureau of Meteorology)	See response to comment 562.
565	3	16	40	16	48	Would be worth explicitly mentioning Russia 2010/central Europe 2003 in this section - there should be citeable references for the latter. (Trewin, Blair, Australian Bureau of Meteorology)	Box removed. Some text now included in main text.
566	3	16	44	16	44	add to the references: Teuling et al., 2010 (for full reference, see comment 8) (van Lanen, Henny A.J., Wageningen University)	Box removed. Some text now included in main text.
567	3	16	52	16	60	This para can be deleted. It is too specific (Luterbacher, Juerg, Justus Liebig University)	Box removed. Some text now included in main text.
569	3	17	1	17	3	I don't see the reinforcing aspect. (Zwiers, Francis, Environment Canada)	Box removed. Some text now included in main text.
570	3	17	1	17	3	This statement needs a reference (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Box removed. Some text now included in main text.
571	3	17	8	17	8	Are likelihood and risk synonymous? (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	Box removed. Some text now included in main text.
572	3	17	13	17	14	Proposed clarification: "at a rate proportional to up to two times the Clausius Clapeyron relationship for hourly extremes (Lenderink and van Meijgaard, 2008). (van Oldenborgh, Geert Jan, KNMI)	Box removed. Some text now included in main text.
573	3	17	16	17	16	Why is this a paradox? (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	Box removed. Some text now included in main text.
574	3	17	16	0	0	Which other national scenarios do capture this? I don't think there are that "many". (Klein Tank, Albert, KNMI)	Box removed. Some text now included in main text.
575	3	17	17	17	18	I don't think the case has been made that correlations have changed or will change. The observational study that you cite presumably makes the case that there is a correlation - that we presumably expect will also operate in warmer conditions. (Zwiers, Francis, Environment Canada)	Box removed. Some text now included in main text.
576	3	17	17	0	0	Another reference from Belgium would be: "Baguis P., Roulin E., Willems P., Ntegeka V. (2010), 'Climate change scenarios for precipitation and potential evapotranspiration over central Belgium', Theoretical and Applied Climatology, 99(3-4), 273-286; doi 10.1007/s00704-009-0146-5" (Willems, Patrick, Katholieke Universiteit Leuven)	Box removed. Some text now included in main text.
578	3	17	20	17	21	It is not clear what constitutes "points 2 and 3". "Thus" is used inappropriately (it implies a judgement is being made based on previously presented evidence, which doesn't seem to be the case here). (Zwiers, Francis, Environment Canada)	Box removed. Some text now included in main text.
580	3	17	21	0	0	I don't understand the use of the word "thus" (Van den Hurk, Bart, KNMI)	Noted. See response to comment 578.
581	3	17	23	17	25	This could be linked to the Korean example on page 12 of chapter 1. (Trewin, Blair, Australian Bureau of Meteorology)	Box removed. Some text now included in main text.
582	3	17	24	17	24	add after "... soil characteristics, thereby". "... soil characteristics (e.g. Stoof et al., 2010), thereby". Stoof, C.R., Wesseling, J.G. and Ritsema, C.R. (2010) Effects of fire and ash on soil water retention, Geoderma (2010), doi:10.1016/j.geoderma.2010.08.002 (van Lanen, Henny A.J., Wageningen University)	Box removed. Some text now included in main text.
584	3	17	29	17	30	What we require is some process understanding of why some combinations occur rather than "anecdotic" evidence (Stott, Peter, Met Office)	Box removed. Some text now included in main text.
585	3	17	30	0	0	What is a "suprising combination of events"? Not well predicted? Never occurred before? Not well understood theoretically? (Wernli, Heini, ETH Zürich)	Box removed. Some text now included in main text.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
586	3	17	31	17	31	The synergistic effects should replace the term "compound events". An example of a discussion about synergistic effects can be seen as attached 1 (Zerefos, Christos, Academy of Athens)	Box removed. Some text now included in main text.
587	3	17	31	17	34	The last sentence includes too many time events. This sentence should be more clearly formulated (Luterbacher, Juerg, Justus Liebig University)	Box removed. Some text now included in main text.
588	3	17	32	0	0	take two subsentences together: "...can also occur due to increased vulnerability or exposure..." (Van den Hurk, Bart, KNMI)	Box removed. Some text now included in main text.
589	3	17	39	31	4	3.2 Interesting and important but very different from the rest of the assessment. Including similar methodological sections for all of the disciplines involved in this report will make it very difficult to produce a compact assessment (IPCC WGII TSU)	Thank you for the comment.
590	3	17	39	31	4	3.2 This section is too detailed and methodological for the special report (IPCC WGII TSU)	We have worked to shorten this section by reducing repetition and removing more extraneous text.
591	3	17	41	17	41	It might be useful to include part of the title of Section 3.2 in the subsection titles of 3.2, so that it is clear that 3.2.1, for example, is about requirements and methods as opposed to the observed changes themselves. Overall this subsection seems a bit loose and unsatisfying. I think it provides an overly rosy picture of the state of the data; has all that much really changed since Trenberth et al (2007)? (Zwiers, Francis, Environment Canada)	section has been re-written.
592	3	17	41	0	0	Section 3.2: the section is extensively long and I suggest to considerably shorten it. I think by remembering the focus on extreme climate events it should be possible to cut down a lot of the more general climate/climate change related parts. For example, there is no need to go into all the details of problems in observing systems (section 3.2.1; like, e.g., undercatch of rain gauges, the introduction of the Stevenson Screen, etc...), detecting/attributing extremes (section 3.2.2; e.g., para on page 20, lines 42-47 about the need to use models to attribute changes; or all the details/definitions on attribution given in multiple paras on page 22), or projecting extremes (section 3.2.3, e.g., shortcomings in AOGCMS/RCMs, downscaling methods etc.). This very specific background information would be better dealt with in an additional, focused Summary Box, thereby only keeping the most relevant (for the assessment!) part of the background information currently provided in this section. Perhaps the observations/attribution/projection details could even be combined into one single box "From observations, to attribution and projection of Extreme Events (related to Climate Change)? Furthermore, there is no need to repeat a lot of what was done in AR4, but just the key conclusions from there with the proper references. (Stocker, Thomas, IPCC WGI TSU)	See comment 590.
593	3	17	41	0	0	Section 3.2.1.: It suggest including rough estimates of the minimum time periods necessary to determine sound trends which are separated from internal natural variability with respect to the different weather systems. Furthermore, the whole section (as well as the whole report) is devoted to linear trends. But what about abrupt changes of the climate systems, probably related to tipping points? (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Thank you for the comment. We do not examine abrupt changes in the context of extreme events due to the complete lack of studies. This is a relevant area, but very under-researched.
594	3	17	41	0	0	I am missing a section explaining the fact that the same effects in the past may have been caused by different mechanisms than active today, and so there may be little comparability. For example, river floods a few centuries ago were often associated with ice dams. Flooding first occurred both upstream of the dam, and downstream, when the dam broke. I do not have a reference deeply studying this issue at hand, but the role of ice dams for the past flood events was mentioned for river Rhine (Fink, A., Ulbrich, U. Engel, H., 1996: Aspects of the January 1995 flood in Germany. Weather, 51 (Feb. 1996), 34-39.) and Elbe (Ulbrich, U., T. Brücher, A. H. Fink, G. C. Leckebusch, A. Krüger, and J. G. Pinto, 2003: The Central European Floods in August 2002, Part I: Rainfall periods and flood development. Weather, 58, 371-376.). (Ulbrich, Uwe, Freie Universität Berlin)	A section like this would be better suited for Section 3.1.
596	3	17	45	17	45	I like the general idea of Figs 3.1 and 3.2, but I don't like the presentation very much. I find the little pictographs hard to read, and the figure hard to synthesize. Also, the notion of "medium" and "large" change seems imprecise and subjective. I'm wondering if the authors could try plotting individual global figures (I also don't like the split into two figures) for each type of extreme, with regions shaded according to direction and assessment, and where the colour indicates the direction of change (rather than using the same colour for both increases and decreases!). (Zwiers, Francis, Environment Canada)	We have modified the maps to include only change and little/no change. Also, the maps need to be split to be able to read the symbols.
597	3	17	46	17	46	I'm nervous about Table 3.2 ... (Zwiers, Francis, Environment Canada)	Thanks for the concern, we want to do it correctly.
598	3	17	50	17	0	Should also include new methods such as the iid-test (Benestad, R.E. (2008) 'A Simple Test for Changes in Statistical Distributions', Eos, 89 (41), 7 October 2008, p. 389-390): The iid-test tells you whether the upper tail in the PDF is changing over time. This test has been applied to observed temperature as well as precipitation. Another consideration is the number of parallel and independent measurements – many series means that changes in extremes potentially can be detected even if the series are not very long. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Thank you for the suggestion, but we are space limited and none of the analysis techniques are specifically discussed in the SOD. The references here are illustrative and not comprehensive.
599	3	17	50	17	50	Try to find another way to put this, avoiding the word "searching", which implicitly makes it sound like there has been a quest for information supporting a specific thesis. (Zwiers, Francis, Environment Canada)	changed in text.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
600	3	17	50	17	57	Data transformation is not the best way to deal with trends in extremes of non-normal data. Much better approaches exist such as the inclusion of trend covariates in the GPD and GEV parameters, for example, as was demonstrated in this article: Coelho, C.A.S., Ferro, C.A.T., Stephenson, D.B. and Steinskog, D.J. (2008): Methods for exploring spatial and temporal variability of extreme events in climate data, Journal of Climate, 21, pp 2072-2092 (Stephenson, David, University of Exeter)	These are simply illustrative examples that different statistical techniques are employed. This text was removed to save redundancy anyway.
601	3	17	50	17	57	This paragraph puts too much weight on the traditional method of looking for changes in extremes by looking at trends in a given percentile, which by necessity means looking at a not so extreme percentile. A much more powerful method which avoids some of the problems mentioned is to use EVT and to determine whether a stationary or non-stationary extreme distribution is the best description of the data. If it is non-stationary then there are trends in the extremes. Brown et al 2008 is such an example of this approach. (Brown, Simon, The Met Office Hadly Centre)	see comment 600
602	3	17	55	17	55	There is evidence that the best estimate of EVT parameters from serially correlated data is to use all the data and not to decluster. Confidence intervals still remain a problem however, (Lee Fawcett and David Walshaw, Improved estimation for temporally clustered extremes, Environmetrics 2007; 18: 173-188) (Brown, Simon, The Met Office Hadly Centre)	Thank you for the comment.
603	3	17	61	18	1	The text should acknowledge that even for long instrumental records there are limits on the duration of events that can be analysed. For example, there are only 10 decades in a century, so there are inherent challenges in analysing decadal variability. <u>let alone extremes. from a 100-year record. (Zwiers, Francis, Environment Canada)</u>	Included in text.
604	3	17	79	17	0	In a study based on the recurrence or record-breaking events, (Benestad, R.E (2006) 'Can we expect more extreme precipitation on the monthly time scale?' J.Clim Vol. 19, No. 4, pages 630-637) the iid-test was applied to results from 31 SRES A1b GCM simulations, and for different seasons. According to this study, the projected trend for some regions such as north-western Europe indicate more extreme monthly precipitation totals in the winter, and a shift in the upper tail of the distribution towards dryer conditions in the summer – hence contrasting extremes, depending on the season. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Thank you for the comment, but there is no line 79 on p. 17.
605	3	18	1	18	2	This sentence is superficial. It ignores a whole body of research based on historical climatology and climate reconstructions from historical evidence, which is actually one of the best sources of past extreme events, see e.g. Xoplaki et al. (2005) (Xoplaki, E., J.Luterbacher, H. Paeth, D. Dietrich, N. Steiner, M. Grosjean, and H. Wanner (2005), European spring and autumn temperature variability and change of extremes over the last half millennium, Geophys. Res. Lett., 32, L15713, doi:10.1029/2005GL023424.) or more recently Brazdil et al (2010) (European climate of the past 500 years: new challenges for historical climatology, Climatic Change (2010) 101:7–40, DOI 10.1007/s10584-009-9783-z) (von Storch, Hans, GKSS Research Center)	Sentence removed.
606	3	18	1	18	2	Need to distinguish between documentary records and physically-based paleoclimate indicators in this section. Also add 'and thus recorded in historical documents' after 'memories'. This paragraph leaves a gap between 1700 and the late 19th century; the gradual growth of observing networks during this period (especially in Europe) should be mentioned. (Trewin, Blair, Australian Bureau of Meteorology)	see comment 605
607	3	18	2	0	0	In addition, a large body of documentary evidence exists. (Klein Tank, Albert, KNMI)	see comment 605
608	3	18	8	18	11	Maybe in this context the authors might point to the WMO Res40Cg-XII WMO "As a fundamental principle of the World Meteorological Organization (WMO), and in consonance with the expanding requirements for its scientific and technical expertise, WMO commits itself to broadening and enhancing the free and unrestricted exchange of meteorological and related data and products". "Free and unrestricted" means non-discriminatory and without charge [Resolution 23 (EC-XLII)-Guidelines on international aspects of provision of basic and special meteorological services]. "Without charge", in the context of this resolution means at no more than the cost of reproduction and delivery without charge for the data and products themselves. (Luterbacher, Juerg, Justus Liebig University)	Thanks, but this would be lost on the audience of this report.
609	3	18	8	18	9	"the situation is changing" -- add reference to support this statement (Stocker, Thomas, IPCC WGI TSU)	No reference is available, but it is true.
611	3	18	10	18	11	The final sentence 'The last two items....' is not needed. Please delete. (Stocker, Thomas, IPCC WGI TSU)	Sentence removed.
612	3	18	13	18	15	You should check with experts on temperature data but my understanding is that airport data is often higher quality data, eg in the US often away from urban warming influences. The issue is that for long term records, it is the shifting of stations eg from city centres to airports, that is relevant to the longer term records or the systematic encroachment of urbanisation on a particular siting. This sentence is misleading and needs rewriting. (Stott, Peter, Met Office)	This paragraph has been removed to reduce length and redundancy with AR4.
613	3	18	13	18	27	The first several lines of this paragraph cast lots of doubt on instrumental data, including the data from the first line met stations that, despite problems, are overall very high quality. This clearly needs a more balanced and nuanced approach. The discussion of QC procedures also does not engender a lot of confidence. The paragraph seems to end up endorsing Durre et al (2008), but have they overcome the issue raised earlier in the paragraph that identifies QC a particular issue for extremes because standard approaches for flagging outliers could be suspected of erroneously flagging real extremes? (Zwiers, Francis, Environment Canada)	see comment 612

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
614	3	18	14	0	15	Not clear why obs for weather forecasts are of lower quality than for climate purposes? (Stocker, Thomas, IPCC WGI TSU)	see comment 612
615	3	18	16	18	0	Here, an earlier and more original reference to this problem is Hanssen-bauer, Inger, Eirik J. Førland, 1994: Homogenizing Long Norwegian Precipitation Series. J. Climate, 7, 1001-1013; Alternatively, this fits in on L34. Even though the reference list is long, scientific ethics, code of conduct, and norm plays a role for due credit to work and ideas, and this ought to be honoured in reports by the IPCC. (Benestad, Rasmus, The Norwegian Meteorological Institute)	see comment 612
616	3	18	16	18	16	Replace "in winter" with "for frozen precipitation". (Trewin, Blair, Australian Bureau of Meteorology)	see coment 612
617	3	18	19	18	19	After "incorrect values" I propose to add ", such as a misplaced decimal point (which is surprisingly common)." (van Oldenborgh, Geert Jan, KNMI)	see comment 612
618	3	18	25	18	27	This study "Wan, H., X. L. Wang, and V. R. Swail, 2007: A Quality Assurance System for Canadian Hourly Pressure Data. J. App. Meteor. Climatol., 46, 1804-1817. DOI: 10.1175/2007JAMC1484.1" is suitable to cite on line 27, page 18, before or after Durre et al. 2008. (Wang, Xiaolan, Environmen Canada)	see comment 612
619	3	18	29	18	41	It is better to refer to the effect of urbanization as a factor affecting data homogeneity. (Fujibe, Fumiaki, Meteorological Research Institute, JMA)	This sentence has been removed.
620	3	18	29	18	41	With respect to extreme winds, you should mention the efforts to estimate such events from MSLP data (which turns out to be more difficult than thought, as also the MSLP measurements are often not homogeneous. A recent analysis is published by Xiaolan L. Wang, Francis W. Zwiers, Val R. Swail, Yang Feng, 2009: Trends and variability of storminess in the Northeast Atlantic region, 1874–2007. Clim Dyn DOI 10.1007/s00382-008-0504-5 (Ulbrich, Uwe, Freie Universitaet Berlin)	This suggestion is more appropriate in the winds section, not here.
621	3	18	30	0	0	"Homogeneous" is a difficult word/concept. Scientists use statistical tests to identify inhomogeneities, but it is impossible to say that in the absence of such detected breaks a series is 100% homogeneous. (Klein Tank, Albert, KNMI)	Agree, thanks for comment.
622	3	18	31	18	34	Add "non-representative location of stations" as well. (Davtala, Rahman, Ministry of Energy)	These sentences have been removed to reduce "textbook" feeling and reduce length.
623	3	18	32	18	32	Trewin is far from being the first to identify station moves as a cause of inhomogeneity. This type of thing happens relatively often in the chapter, and I'm not sure what the appropriate solution should be. It is good to use recent examples from the literature - but there is also a need to appropriately acknowledge those who first introduced concepts, etc. (Zwiers, Francis, Environment Canada)	These sentences have been removed to reduce "textbook" feeling and reduce length.
624	3	18	34	18	36	This study "Wan, H., X. L. Wang, and V. R. Swail, 2010: Homogenization and Trend Analysis of Canadian Near-Surface Wind Speeds. J. Clim., 23, 1209-1225. DOI:10.1175/2009JCLI3200.1" is suitable to cite in line 36 on page 18, right after "bias in wind measurements". See also Comment No 4. (Wang, Xiaolan, Environmen Canada)	Agree, reference included.
625	3	18	34	0	0	Strange not to mention the urban heat island effect in this respect, in a report on extremes (Van den Hurk, Bart, KNMI)	The list given here is only several examples to illustrate the sort of problems. We do not think the list needs to be exhaustive
626	3	18	35	18	36	Depending upon the proximity, erecting a building is not a small change. (Zwiers, Francis, Environment Canada)	Acknowledged in text.
627	3	18	41	0	0	Please add the following publications of Kuglitsch et al. (2009) and Toreti et al. (2010a); Toreti, A., Kuglitsch, F.G., Xoplaki, E., Luterbacher, J., and Wanner, H., 2010a: Homogenization of daily temperature series: a new version of Higher Order Moments method (HOM), J. Climate, in press, DOI: 10.1175/2010JCLI3499.1 (Luterbacher, Juerg, Justus Liebig University)	Included Toreti reference.
628	3	18	43	0	0	For daily and sub-daily data (both temperature and precipitation), the issues of homogeneity have not been addressed yet. (Klein Tank, Albert, KNMI)	True, thank you for the comment.
629	3	18	44	18	0	References? (Benestad, Rasmus, The Norwegian Meteorological Institute)	Sentence refers to issues in previous paragraph with associated references. But this has been re-written to reduce redundancy.
631	3	18	46	18	46	In Chapter 1 it is claimed that small-scale events such as thunderstorms and tornadoes are not considered, but here they are. (van Oldenborgh, Geert Jan, KNMI)	This outlines why we cannot say much about changes in these storms.
633	3	18	50	18	53	I suggest changing the first sentence into "Thunderstorms and related extremes such as severe wind gusts, tornadoes, heavy rainfall or hail, ..." to be more general. L53: "A similar problem occurs with other thunderstorm-related extremes". (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Sentence modified.
634	3	18	50	18	55	Reviewer proposes to add: The use of non-meteorological data, such as damage data from insurance companies (M. Kunz, J. Sander, Ch. Kottmeier: Recent trends of thunderstorms and hailstorm frequency and their relation to atmospheric characteristics in southwest Germany, Int.J.Climat., 29, 2283-2297, 2009) have a certain potential to fill the gaps between meteorological observations. (Kottmeier, Christoph, Karlsruhe Institute of Technology)	Thank you, that may be true, but these data are not considered here and thus are not discussed to save space.
635	3	18	57	18	63	The abbreviation (ETCs) is not used elsewhere in the text, including the extra-tropical cyclone section. Please either adopt throughout or delete from this paragraph. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Abbreviation is not used anymore.
636	3	18	57	18	63	A recent review of papers on trends of cyclones is available: Ulbrich, U., G.C. Leckebusch, J. Pinto, 2009: Extra-tropical cyclones in the present and future climate: a review. Theo. Appl. Climatology, 96, 117-131. DOI 10.1007/s00704-008-0083-8. The studies cited there do not only give indications of trends, but also question the statements on data insecurity cited in the current text paragraph. (Ulbrich, Uwe, Freie Universitaet Berlin)	Thank you for the comment.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
637	3	18	58	18	58	Due reference to the ACRE project should be given which is trying to push back the start date of reanalyses to pre 1900. (http://www.met-acre.org/) (Brown, Simon, The Met Office Hadly Centre)	Thank you, but there is no peer-reviewed publication describing ACRE. We have included the Compo et al. 2010 reference.
638	3	18	63	0	0	Please add the following reference after Compo et al. 2006: Compo et al. 2009 : Compo, G.P., J.S. Whitaker, P.D. Sardeshmukh, N. Matsui, R.J. Allan, X. Yin, B.E. Gleason, R.S. Vose, G. Rutledge, P. Bessemoulin, S. Brönnimann, M. Brunet, R.I. Crouthamel, A.N. Grant, P.Y. Groisman, P.D. Jones, M.C. Kruk, A.C. Kruger, G.J. Marshall, M. Maugeri, H.Y. Mok, Ø. Nordli, T.F. Ross, R.M. Trigo, X.L. Wang, S.D. Woodruff, S.J. Worley, 2009: The Twentieth Century Reanalysis Project. Quart. J. Roy. Meteor. Soc., submitted. (Luterbacher, Juerg, Justus Liebig University)	added thank you.
639	3	18	63	0	0	Add: 'However, studies have shown that observed and projected trends might strongly depend on the reanalysis data set that is used and on the specific method that is applied to identify ETCs from meteorological fields (Raible et al. 2008, Ulbrich et al. 2008).' References: Raible C.C., P. M. Della-Marta, C. Schwierz, H. Wernli, R. Blender, 2008: Northern Hemisphere Extratropical Cyclones: A Comparison of Detection and Tracking Methods and Different Reanalyses. Mon Wea Rev 136: 880–897. - Ulbrich U., G.C. Leckebusch, J.G. Pinto, 2009: Extra-tropical cyclones in the present and future climate: a review. Theor Appl Clim 96: 117-131 (Neu, Urs, Swiss Academy of Sciences)	Added text to reflect this, and references.
640	3	19	2	19	15	There have been efforts to homogenize the historical tropical cyclone records, for instance by Vecchi and Knutson, J.Clim, 2008, doi:10.1175/2008JCLI2178.1 (van Oldenborgh, Geert Jan, KNMI)	Thank you, this is discussed in the Tropical Cyclone section (3.4.4.1).
641	3	19	10	19	10	Add "in some basins" after "reconnaissance" - there has never been systematic aircraft reconnaissance outside the Atlantic and NW Pacific. (Trewin, Blair, Australian Bureau of Meteorology)	added.
642	3	19	10	19	12	It should be stated explicitly that the effect of these changes is that the frequency of cyclones prior to the introduction of these systems is likely to have been under-reported. (Trewin, Blair, Australian Bureau of Meteorology)	This is discussed in section 3.4.4.1
643	3	19	12	19	12	Insert "North" ahead of "Atlantic"? (Zwiers, Francis, Environment Canada)	added.
644	3	19	15	19	15	A definition of the term "tropical cyclone activity" or at least a link to p55, L56-57 would be helpful (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	This is better done in an earlier section.
645	3	19	17	19	0	Does this paragraph belong to an impacts chapter? (Benestad, Rasmus, The Norwegian Meteorological Institute)	No, soil moisture ties directly back to drought and runoff.
646	3	19	17	19	28	New measurements of water availability using GRACE (eg. Chen et al 2009) could be mentioned here (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Not clear what that would add in the context of extremes.
647	3	19	17	0	0	In section 3.2, on page 19, L17, the Chapter is mentioning scarce the data set of soil moisture which is a key for assessing the impacts to the physical environment. Although, it is true the lack of the data set of soil moisture in the World wide region, more sophisticated researches shall be needed regarding this and refer to the recent research results since AR4 of the research groups concerning the land surface-atmosphere interaction and the boundary layer meteorology and hydrology. (TANAKA Tardashi, University of Tsukuba)	Thank you for the comment.
648	3	19	21	19	21	These are also produced in the US, eg Fan and van den Dool, JGR, 2004, doi:10.1029/2003JD004345 (van Oldenborgh, Geert Jan, KNMI)	Yes, thank you the number of references here is sufficient.
649	3	19	26	19	26	add before "Additionally..". Hannah et al. (2010) provide a comprehensive analysis of large-scale river flow archives, importance, current state and future needs. (van Lanen, Hennv A.J., Wageningen University)	Not clear what that would add in the context of extremes.
650	3	19	26	19	26	Hannah, D.M., Demuth, S., Van Lanen, H.A.J., Looser, U., Prudhomme, C., Rees, R., Stahl, K., Tallaksen, L.M. (2010) Large-scale river flow archives: importance, current status and future needs. Hydrological Processes. doi: 10.1002/hyp.7794. (van Lanen, Hennv A.J., Wageningen University)	Not clear what that would add in the context of extremes.
651	3	19	28	19	28	Snow is also important in the prediction of low temperature extremes, see eg Shongwe et al, Mon. Wea. Rev. 2007 doi:10.1175/2007MWR2094. (van Oldenborgh, Geert Jan, KNMI)	Thank you for the comment.
653	3	19	30	19	43	Due reference to Caesar et al 2006 should be given in this paragraph, data homogenization notwithstanding. (Brown, Simon, The Met Office Hadly Centre)	added
654	3	19	30	19	43	Due reference to Brown et al 2008 should be given in this paragraph as an example of a quasi global analysis of regional trend in observed daily temperature. (Brown, Simon, The Met Office Hadly Centre)	added.
655	3	19	32	0	0	Please delete the last part of sentence referring to the IPCC - "partly in response to previous IPCC assessments" (Stocker, Thomas, IPCC WGI TSU)	deleted.
658	3	19	45	19	54	It may be worth mentioning the potential use of pressure-based circulation indices as a proxy for extreme winds. A paper currently in review on this subject is L Alexander, XL Wang, H Wan, BC Trewin (2011?), Significant decline in storminess over south-east Australia since the late 19th century, Aust. Met. Oceanogr. J., submitted. (Trewin, Blair, Australian Bureau of Meteorology)	Thanks but don't have the paper and it is not yet published or accepted so we cannot use it.
659	3	19	45	19	55	The details given in this final paragraph are not needed. There is no need to provide a mini-summary of observed changes here. (Stocker, Thomas, IPCC WGI TSU)	Agree, need to reduce space anyway.
660	3	19	46	19	47	We're still lacking coverage in large parts of the world for even temperature and precipitation (especially daily, but also monthly means). And for some variables, including precipitation, we have virtually no instrumental data over the oceans, which greatly limits the confidence with which we can detect and attribute change, even on global scales. (Zwiers, Francis, Environment Canada)	Thanks for the comment.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
661	3	19	47	19	48	Give examples of improved datasets. (Zwiers, Francis, Environment Canada)	This paragraph has been removed to reduce length and redundancy with AR4.
662	3	19	49	19	54	Please provide references indicating that extremes related to extra tropical cyclones have increased in frequency and intensity (von Storch, Hans, GKSS Research Center)	Done later in section on extratropical cyclones.
663	3	19	51	19	52	See comment on trends of mid-latitude cyclones (p18,lines 57-63): review paper available (Ulbrich, Uwe, Freie Universitaet Berlin)	Done later in section on extratropical cyclones.
664	3	19	53	19	53	I think I would not characterize the result as "increased uncertainty" but would rather point out that the uncertainty estimates have been revised upwards because uncertainties are now better understood or quantified. Uncertainty has not suddenly increased, but rather, our overconfidence in the data has been decreased. (Zwiers, Francis, Environment Canada)	Paragraph removed, and this is discussed in section 3.4.4.1
665	3	19	58	19	59	The captions of figures 3.1 and 3.2 are not clear. Does a downward arrow in cold nights mean that cold nights have become colder, or that the number of cold nights has decreased? (van Oldenborgh, Geert Jan, KNMI)	Upward and downward arrows indicate an increase or decrease in incidence. This is indicated in the legend.
666	3	19	0	0	0	what about the "heat island" effect (given that many meteo stations are located in or close to the city center)? I think it is not mentioned in this section, while it could be an important factor as well. (Willems, Patrick, Katholieke Universiteit Leuven)	It is not included here but is mentioned in the AR4 as a possible factor.
667	3	20	1	20	1	Brown et al 2008, Fig 3 provides regional estimates of changes in both Tmax and Tmin for most of the regions in the table, but only seems to be used for central and south america. Burke et al 2006 fig 3 provides regional estimates of observed trends in PDSI for most regions in the table. (Brown, Simon, The Met Office Hadly Centre)	Thanks, where we have regional studies they are used instead of global studies.
668	3	20	2	0	0	Table 3.2: Countries in the South Pacific (other than Australia and New Zealand) do not appear to have been considered in this Table. Page 30 states this is due to spatial issues of the models and lack of literature, however, literature does exist for this region. E.g. Griffiths, G.M., Chambers, L.E., Haylock, M.R., Manton, M.J., Nicholls, N., et al. 2005. Change in mean temperature as a predictor of extreme temperature change in the Asia-Pacific region. Int.J.Climatol. 25: 1301-1330. Nicholls, N., Baek, H.-J., Gosai, A., Chambers, L.E., et al. 2005. The El Nino – Southern Oscillation and daily temperature extremes in east Asia and the west Pacific. Geophysical Research Letters 32: L16714. Manton, M.J., Della-Marta, P.M., Haylock, M.R., Chambers, L.E., et al. 2001. Trends in extreme daily rainfall and temperature in southeast Asia and the South Pacific: 1961-1998. Int. J. Climatology 21:269-284. (Chambers, Lynda, Australian Bureau of Meteorology)	We are putting these areas into a box instead of into the tables.
669	3	20	6	0	0	Section 3.2.2: Pleaes give a short introduction explaining the difference between detection and attribution. Up to 3.2.2.3 only detection is mentioned, not attribution. (van Oldenborgh, Geert Jan, KNMI)	The revised text gives brief introduction for section 3.2.2, details about detection and attribution are given in section 3.2.2.2.
670	3	20	6	0	0	Section 3.2.2 I could not find a clear definition of changes in extremes. If only the mean climate is changing without any further changes in the probability distribution, is this considered to be also a change in the frequency of extremes ? Are changing extremes a change in the number of events above a fixed threshold defined by a quantile of the observed climate or are changes in extremes a change in the tail of the probability distribution once an overall shift of the distribution has been accounted for ? this aspect remains unclear and pervades the interpretation of the whole section (von Storch, Hans, GKSS Research Center)	A definition of changes in extremes is now given in SECTION 3.1.1(?). The change in extremes is defined as changes to a fixed threshold, but not the tail distribution relative to the mean, as such fixed threshold is the most relevant to impacts.
671	3	20	10	20	10	This introductory sentence is a bit vague. Possible rewording - 'This section addresses the main requirements, methods, and considerations for the attribution of causes for observed or projected changes in extremes'. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Text modified
673	3	20	10	20	13	The introductory paragraph doesn't give a very clear view of what this subsection is about. Presumably the intent is not to identify causes, but rather to discuss the approaches used, and their requirements. (Zwiers, Francis, Environment Canada)	Agreed. Text modified
674	3	20	10	20	51	Much of this appears as very general 'text-book' material. Please focus on what is the most relevant key message for this report. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Text modified
675	3	20	14	20	0	Reference to a study by Lockwood et al (2009), 'Are cold winters in Europe associated with lowsolar activity?' Environ. Res. Lett. Should be included, where he argues that due to low solar activity, there may be severely cold winters over northern Europe, even in the future. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Solar irradiance change has been discussed already.
676	3	20	14	20	27	Here, and in other sections, I suggest to start with discussing what is known about extremes (here lines 35-40). In the present text, one immediately relates the findings on changes in the mean to the extremes mentioned in the title. In reality, they apply to changes in the mean only. (Klein Tank, Albert, KNMI)	Agreed. Much of discussion about mean has been removed to make this subsection more focused.
677	3	20	14	20	27	Mention that attribution has been made on the continental scale (Hegerl et al 2007) and define regional to be smaller than continental (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Deleted in revised text
678	3	20	14	20	33	Presenting the causes with a selected and appropriate figure is suggested. As I said before, the report is very descriptive and presenting tales, figures and numerical result may attract the readers. (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	Rejected. While it would be nice to do as suggested, the length of Chapter 3 is rather limited.
679	3	20	14	0	0	Is really the "caotic nature of the climate system" or the complex nature of the system ? (Suarez, Avelino, Institute of Ecology and Systematic, Cuban Environmental Agency)	Climate system is a caotic system. The term "caotic" seems to be too technical here and is removed.
680	3	20	25	0	0	CO2 has no half-life. Please reword. (Stocker, Thomas, IPCC WGI TSU)	Deleted in revised text

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
681	3	20	29	20	30	It should be also highlighted, that an observed trend can be just a consequence of stochastic variability, not driven by external forcing or mirroring really changed internal variability. (Kottmeier, Christoph, Karlsruhe Institute of Technology)	Rejected, internal variability means stochastic variability.
682	3	20	33	20	47	The need to discriminate between natural and antropogenic causes is not high when an increased vulnerabiity leads to an increased number of "climate disasters" also under natural conditions. (Van den Hurk, Bart, KNMI)	This is true in that particular context, but here we discuss physical problems.
683	3	20	33	20	47	This distinction between natural and antropogenic can often not be made, certainly in a complex system as the climate where many processes interact mutually and modes of variability are not simply additive. (Van den Hurk, Bart, KNMI)	Agreed, but this was in the original text.
684	3	20	35	20	0	The spatial distribution of the parameters describing the PDFs and corresponding distribution of mean values, however, can also provide some information regarding the association between climate means and extremes – this question does not only affect the temporal dimension. (Benestad, Rasmus, The Norwegian Meteorological Institute)	agreed, but there is not enough space to discuss at this level of details.
685	3	20	42	20	0	Does this paragraph belong to the discussion about GCMs earlier? (Benestad, Rasmus, The Norwegian Meteorological Institute)	Text has been modified to improve the flow.
686	3	20	42	20	42	Geoengineering may indeed experiment with the atmosphere. Suggest replacing this opening sentence with "With few possibilities of extending the observed records, our main source ..." (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Agreed, the text "Since ...", is now removed.
687	3	20	42	20	47	Mention possibility of extending palaeo records (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Our main interest here is in the observations.
689	3	20	51	21	0	Additional analysis -relevant for much of the discussion here - for Europe should be included: Benestad, R.E. (2010) 'Downscaling Precipitation Extremes: Correction of Analog Models through PDF Predictions', Theor. & Appl. Clim, Volume 100, Issue 1, DOI:10.1007/s00704-009-0158-1; Benestad, R.E.(2007) Novel Methods for Inferring Future Changes in Extreme Rainfall over Northern Europe Climate Research, CR34:195-210, doi: 10.3354/cr00693. These studies link the parameter describing the 24-hr precipitation PDF to the mean temperature and precipitation levels, in addition to other geographical conditions, looking at variations in both space and time. The former looks at the relationship between mean and extremes for locations scattered across the whole of Europe, whereas the latter focuses more on one location and in which the projected change in extremes was related more to a general warming rather than change in mean precipitation. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Consider including more references, but already >900 references in chapter.
690	3	20	56	20	56	I suggest changing the emphasis slightly by replacing "in particular" with "including". (Zwiers, Francis, Environment Canada)	Agreed, text modified as suggested
691	3	20	57	20	57	This sentence is not correct if changes in the mean climate are discounted in the definition of changes in t extremes. Please clarify the definition of changes in extremes. (von Storch, Hans, GKSS Research Center)	Agreed, there is now a clear definition of changes in extremes.
692	3	20	0	21	0	P 20-21 too much on attribution of changes in mean conditions (IPCC WGII TSU)	Agreed, text is shortened.
693	3	21	1	21	0	Reference should be given to 'solar cooling', but much of this section concerns AR4, and could perhaps be shortened. Not much new information here. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Agreed, text is shortened.
694	3	21	1	21	2	This statement has already been made earlier in this section (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	The earlier statement is removed.
695	3	21	1	21	3	It would be best to use the exact quote from the AR4 rather than attempting to paraphrase. (Zwiers, Francis, Environment Canada)	Agreed, the original AR4 text is now used.
696	3	21	1	21	3	Reword to be the same as the original. As it stands it refers to multiple increases which could include eg the 20 yr trend from 1950 etc which is not the sense implied, ie to present. (Stott, Peter, Met Office)	Agreed, the original AR4 statement is used in the revised text.
697	3	21	1	21	9	Need to make clear that these are all AR4 assessments. (Zwiers, Francis, Environment Canada)	Agreed, text modified as suggested.
698	3	21	8	21	8	The statement that 20thC solar variability would likely have produced cooling needs a reference (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Agreed, AR4 assessment has been shortened.
699	3	21	8	21	8	This assertion is contested, as there are two competing estimations of solar irradiance in the satellite era: PMOD (Fröhlich, 2006: "Frohlich, C., 2006. Solar irradiance variability since 1978: revision of the PMOD composite during solar cycle 21. Space Sci. Rev. 125, 53–65 10.1007/s11214-006-9046-5.)and ACRIM (Wilson and Mordvinov, 2003:Willson, R.C., Mordvinov, A.V., 2003. Secular total solar irradiance trend during solar cycles 21–23. Geophys. Res. Lett. 30, 1199–1202 10.1029/2002GL016038). (von Storch, Hans, GKSS Research Center)	Agreed, AR4 assessment has been shortened.
700	3	21	11	21	20	Replace "natural variability" with "internal variability" or "unforced variability" (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Agreed, text modified as suggested.
701	3	21	15	21	16	"Temperature changes associated with some modes of variability are poorly simulated by models in some regions and seasons." Temperature chngaes are not simulated well on the local scale also due to many other reasons, the sentence would be sstronger without the "associated with some modes". (van Oldenborgh, Geert Jan, KNMI)	Agreed, text modified as suggested
702	3	21	16	0	0	You could cite Dean and Stott (as well as lower down) because in Dean and Stott we provide a nice example of where models don't capture an important mode of variability. (Stott, Peter, Met Office)	Reference added

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
703	3	21	19	21	20	"Because of these, regional scale detection is still hard to achieve." Attribution is even harder, as observed trends often are outside the PDF of simulated trends on the local scale (Knutson J. Climate 2006 doi:10.1175/JCLI3709.1, van Oldenborgh et al Clim. Past 2009) (van Oldenborgh, Geert Jan, KNMI)	Deleted in revised text
704	3	21	22	21	0	Perhaps a reference to van der Oldenborg et al (2009) Climate of the Past, vol 5 (1), 1-12 for the regional/local attribution discussion? They argue that the observed temperature trend in western Europe over the last decades appears much stronger than simulated by state-of-the-art GCMs. (Benestad, Rasmus, The Norwegian Meteorological Institute)	reference added
706	3	21	23	0	0	Please add the following references: after Stott et al. 2004: Christidis et al. 2010 and Hegerl et al. 2010: Christidis, N., Stott, P.A., Jones, G.S., Shiogama, H., Nozawa, T., and Luterbacher, J., 2010: Human activity and warm seasons in Europe. Int. J. Climatol., in revised; Hegerl, G., Luterbacher, J., González-Rouco, F.J., Tett, S., Crowley, T., and Xoplaki, E., 2010: Influence of human and natural forcing on European seasonal temperatures. Nature Geoscience, in revision. (Luterbacher, Juerg, Justus Liebig University)	references added
707	3	21	24	21	27	To the best of my knowledge, there are only 5 populated continents; besides, I cannot find the cited statements in the paper of Min and Hense (2007). (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	the related text has been removed.
708	3	21	31	21	33	In this context, it should be explained how the two cited studies detect the anthropogenic signal in the trends. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	This is an assessment, details of nature have been removed as much as possible.
709	3	21	33	21	33	Reviewer proposes to add: The observed finding of weekly cycles of daily meteorological data in Germany (Bäumer, D., Vogel, B.: An unexpected pattern of distinct weekly periodicities in climatological variables in Germany Geophys. Res. Lett., 34, L03819, 1-4, 2007), that has been supported for other regions, though being under debate to be generally, also indicates an anthropogenic (emission) impact at smaller time-scales. (Kottmeier, Christoph, Karlsruhe Institute of Technology)	There is useful but not very relevant paper to the chapter.
711	3	21	37	21	56	Based on availability of information, briefly write about changes in dew point temperature. (Davtalah, Rahman, Ministry of Energy)	the specific humidity which has one-to-one relationship with dew point temperature is assessed.
712	3	21	43	0	0	The results of the following two recent papers by should also be discussed: Willett KM, Jones PD, Thorne PW, et al., 2010. A comparison of large scale changes in surface humidity over land in observations and CMIP3 general circulation models. Environm. Res. Lett., 5, article number: 025210; Simmons AJ, Willett KM, Jones PD, et al., 2010. Low-frequency variations in surface atmospheric humidity, temperature, and precipitation: Inferences from reanalyses and monthly gridded observational data sets. J. Geophys. Res., 115, article number: D01110. (Wernli, Heini, ETH Zürich)	new references assessed.
713	3	21	46	21	51	I think an assessment of the greater than 1 scaling factors in Zhang et al should include a reflection that a simple averaging of precip patterns from models may smear out and reduce the model signal (a paper by Knutti et al points this out). (Stott, Peter, Met Office)	good point, text modified to reflect this.
714	3	21	52	21	53	It should be commented that the conclusion of Lenderink and van Meijgaard (2008) is under debate: see Haerter and Berg, 2009 and Lenderink and van Meijgaard, 2009 (Haerter, J.O., Berg, P., 2009. Unexpected rise in extreme precipitation caused by a shift in rain type? Nature Geoscience, 2, 372-373; Lenderink, G., van Meijgaard, E., 2009. Reply to: Unexpected rise in extreme precipitation caused by a shift in rain type? Nature Geoscience, 2, 373) (Willems, Patrick, Katholieke Universiteit Leuven)	those are now assessed.
715	3	21	53	21	53	The C-C relation does not say anything about precipitation (but phase equilibrium). (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	it is clear now that increase in temperature has a potential to increase extreme precipitation because of more moisture available.
717	3	21	53	21	53	Reviewer proposes to add: Haerter and Berg (Haerter, J.O., Berg, P. Unexpected rise in extreme precipitation caused by a shift in rain type? Nature Geoscience, 2, 372-373, 2009) propose a conceptual framework that explains the steeper-than-Clausius-Clapeyron-increase of precipitation in global models by a shift in rain type to more convective rain in a warmer climate (Kottmeier, Christoph, Karlsruhe Institute of Technology)	this is now assessed.
718	3	21	58	23	3	This section should be improved. It is not specific enough about extremes and contains too many references to papers that do not deal with extremes in particular. It also contains too much about why attribution is difficult etc. such that in the end, the question posed in the title of this section remains rather open. (Wernli, Heini, ETH Zürich)	The section is now shortened and more focused to extremes.
719	3	21	58	0	0	Should this not be worded as "How to Attribute Change in Extremes to Causes' (Stocker, Thomas, IPCC WGI TSU)	modified as suggested.
720	3	21	58	0	0	Section 3.2.2.3. This content of this section is not really relevant for extremes. Most is devoted to explain the method for detection and attribution of climate change in general, without providing the specifics for extremes and without reviewing specific literature for attribution of changes in extremes. To my knowledge this literature is very limited and I wonder if this section really makes sense at all. (von Storch, Hans, GKSS Research Center)	it is more focused to extremes. But the general idea and the concepts used in detection and attribution for mean climate still apply though the details need to take the distributional property into consideration.
721	3	21	58	0	0	Christidis et al should be quoted in this section as the first study to detect a human influence on extreme daily temperature. Christidis, N., P. A. Stott, S. Brown, G. C. Hegerl, and J. Caesar (2005), Detection of changes in temperature extremes during the second half of the 20th century, Geophys. Res. Lett., 32, L20716, doi:10.1029/2005GL023885. (Brown, Simon, The Met Office Hadley Centre)	This paper is now cited.
722	3	21	61	21	0	More concise citation: Hegerl et al., 2007, 2010 (Benestad, Rasmus, The Norwegian Meteorological Institute)	agreed, text modified.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
723	3	21	62	22	1	This meeting will be a bit more distant from the present when the SREX report is released, so perhaps delete the second sentence, and start the third sentence with "The guidance paper on detection and attribution (Hegerl et al, 2010) from the joint WGI/WGII expert meeting on detection and attribution (give dates) provides the following definitions ..." (Zwiers, Francis, Environment Canada)	modified as suggested.
724	3	22	1	22	15	Please shorten with reference to the Hegerl et al. 2010 guidance paper. There is no need to repeat material here. (Stocker, Thomas, IPCC WGI TSU)	text has been shortened.
726	3	22	1	22	6	This discussion of the Detection and Attribution guidance note has missed out an important sentence, namely that "The process of attribution requires the detection of a change in the observed variable or closely associated variable". This is relevant to the discussion of tropical cyclone intensity later on. There may be evidence that supports the attribution of a particular effect, eg an increased probability of seasonal summer temperatures exceeding a very high threshold even when there is no detectable trend in the frequency of such a rare extreme. (Stott, Peter, Met Office)	text modified as suggested
729	3	22	8	22	0	Some words about the difficulties is perhaps in order, e.g. common trends, such as in solar output and GHGs over the 20th century, can hamper attribution (Benestad & Schmidt, 'Solar trends and global warming', JGR-atmospheres, 114, D14101, doi:10.1029/2008JD011639). (Benestad, Rasmus, The Norwegian Meteorological Institute)	Agree, but there is really no space for that.
730	3	22	8	22	14	The difference between Single-step and Multi-step approaches to attribution lead to fundamental differences in D/A between WG1 and WG2 in AR4. Now that these two communities work together in SREX, I hope that a more consistent approach is possible. From the remainder of this chapter it seems that the Single-step approach is favoured by the authors. Is this consistently followed throughout the other chapters of the report? (Klein Tank, Albert, KNMI)	it is consistent under the same guidance, though different approaches may still be used for different chapter because of nature of problem (such as daily availability etc.)
731	3	22	16	22	0	Parts of this section is repetition. The paragraph can be shortened, upon re-organising the contents regarding data quality, uncertainties, etc. L23: "operate on model grids much larger..." - refers to grid boxes and not grids. There is also the question of the models 'skillful scale'; A paper by Grotch & MacCracken (1991; J. Climate) has been re-interpreted by Zorita & von Storch (1997;) as: "at finer spatial resolutions, with scales of a few grid distances, climate models have much smaller skill", and by von Storch et al (1993; J. climate) as: "the minimum scale is defined as the distance between two neighbouring grid points, whereas the skillful scale is larger than N gridpoint distances. It is likely that $N \geq 8$ ". The issue is that discretisation of numerics, numerical noise, and parametrisation affect grid-box values. (Benestad, Rasmus, The Norwegian Meteorological Institute)	the section is modified to remove repetition.
732	3	22	19	22	19	I suggest replacing "reasonable" with "sufficient". (Zwiers, Francis, Environment Canada)	text modified as suggested
733	3	22	20	22	28	Recently, very high resolution models with grid spacings of several km were developed and showed ability to reproduce extremes such as daily maximum precipitation (Wakazuki et al., 2008; Sasaki et al., 2008; Kanada et al., 2008; Kanada et al., 2010). Sasaki, H., K. Kurihara, I. Takayabu and T. Uchiyama, 2008: Preliminary experiments of reproducing the present climate using the non-hydrostatic regional climate model, SOLA, Vol.4, 25-28. Kanada, S., M. Nakano, S. Hayashi, T. Kato, M. Nakamura, K. Kurihara and A. Kitoh, 2008: Reproducibility of Maximum Daily Precipitation Amount over Japan by a High-resolution Non-hydrostatic Model. SOLA, Vol. 4, 105-108. Kanada, S., M. Nakano and T. Kato, 2010: Climatological characteristics of daily precipitation over Japan in the Kakushin regional climate experiments using a non-hydrostatic 5-km-mesh model: Comparison with an outer global 20-km-mesh atmospheric climate model, SOLA, Vol. 6, 117-120 (Kurihara, Kazuo, Meteorological Research Institute)	agree, but these simulations are so limited that detection and attribution analysis are not yet possible to make use of them.
734	3	22	24	22	24	I suggest replacing "to produce point estimate of" with "to produce localized". (Zwiers, Francis, Environment Canada)	text modified as suggested
736	3	22	27	22	27	Both resolved motions and surface features (topography) are limitations, amongst many other factors. (Zwiers, Francis, Environment Canada)	text modified as suggested
737	3	22	32	0	0	The following result may reinforce the result of Wang et al. (2009c): "the future wave climate changes to lower mean and higher maximum wave heights in the middle latitudes, and higher mean and maximum wave heights in the high latitudes," (Nobuhito Mori, Tomohiro Yasuda, Hajime Mase, Tracey Tom and Yuichiro Oku: Projection of Extreme Wave Climate Change under Global Warming, Hydrological Research Letters, Vol. 4, pp.15-19, (2010)) (Nakaegawa, Toshiyuki, Meteorological Research Institute)	agree, but the assessment here is about observed past changes.
739	3	22	40	22	40	Replace "cause and effect" with "causes to effects". (Zwiers, Francis, Environment Canada)	text modified as suggested
740	3	22	40	22	54	Attribution of observable extreme events to climate change requires a reliable statistical framework for making inference about observed extremes from multi-model projections that contain biases. Such a framework has yet to be developed and so it is not clear to me how any definitive attribution statements can be made about observed extreme events. Recent attempts such as Stott et al. (2004) rely on least-squares regression which is inappropriate when dealing with the tail of the distribution. (Stephenson, David, University of Exeter)	this comment is now reflected in the text. Note that the existing d&a method may still be used if the analysis is done properly. For example, averaging extremes values over a large region would produce the mean of extremes that would follow a normal distribution and as such, the usual method for mean climate can be applied.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
741	3	22	40	22	54	Again, the changing mechanism leading to floods could be mentioned. River floods a few centuries ago were often associated with ice dams. Flooding first occurred both upstream of the dam, and downstream, when the dam broke. I do not have a reference deeply studying this issue at hand,, but it the role of ice dams for the past flood events was mentioned for river Rhine (Fink, A., Ulbrich, U. Engel, H., 1996: Aspects of the January 1995 flood in Germany. Weather, 51 (Feb. 1996), 34-39.) and Elbe (Ulbrich, U., T. Brücher, A. H. Fink, G. C. Leckebusch, A. Krüger, and J. G. Pinto, 2003: The Central European Floods in August 2002 ,Part I: Rainfall periods and flood development. Weather, 58, 371-376.). (Ulbrich, Uwe, Freie Universitaet Berlin)	this is a good example, but there is not enough space to give many examples.
743	3	22	53	22	53	add reference after ".....Haarsma et al., 2009)." - ".....Haarsma et al., 2009; van Lanen et al., 2004a; 2004b)." (van Lanen, Henny A.J., Wageningen University)	we have already several references here
744	3	22	53	22	53	Lanen, H.A.J. van, Fendeková, M., Kupczyk, E., Kasprzyk, A. & Pokojski, W. (2004a) Flow Generating Processes, Chapter 3. In: Tallaksen, L.M. & van Lanen, H.A.J. (Eds.) (2004) Hydrological Drought. Processes and Estimation Methods for Streamflow and Groundwater. Developments in Water Science, 48, Elsevier Science B.V., pg. 53-96. (van Lanen, Henny A.J., Wageningen University)	we have already several references here
745	3	22	53	22	53	Lanen, H.A.J. van, Kašpárek, L., Novický, O., Querner, E.P., Fendeková, M. & Kupczyk, E. (2004b) Human Influences, Chapter 9. In: Tallaksen, L.M. & van Lanen, H.A.J. (Eds.) (2004) Hydrological Drought. Processes and Estimation Methods for Streamflow and Groundwater. Developments in Water Science, 48, Elsevier Science B.V., pg. 347-410. (van Lanen, Henny A.J., Wageningen University)	we have already several references here
746	3	22	56	23	0	Some repetition here. The paragraph can be made more concise. Another point is that the recurrence of extremes are irregular – in addition to being rare – and that is a main reason why attribution is so difficult. (Benestad, Rasmus, The Norwegian Meteorological Institute)	repetition is reduced.
747	3	22	58	22	60	I think the authors have in mind anthropogenic causes, so they should write this explicitly. Otherwise one could always assign causes to an event extreme or not. (Bojariu, Roxana, National Meteorological Administration)	this is now more explicit.
748	3	22	62	22	63	I suggest including "from observations and an ensemble of GCM data" directly after "mean summer temperature". Besides, the investigation region extends from 30°N to 50°N and, thus, covers more than southern Europe. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	the detailed text is now removed.
749	3	22	0	0	0	Please add: and the exceptional warm winter 2006/2007 after 'in 2003' (Luterbacher, Juerg, Justus Liebig University)	Page/line not identified by reviewer
750	3	23	3	0	0	it can't harm to point out that this attribution is carried out by (and thus depends on the quality of) climate models, with their usual skill limitations. (Van den Hurk, Bart, KNMI)	Reject. Section is about using models for attribution. Do not need to repeat.
751	3	23	8	24	31	FAQ 3.2 addresses an important question that will be on the minds of many readers. I wonder if the question/response does a disservice by focusing on whether we can attribute an entire extreme event to climate change. Seems that a more appropriate question is: "Can we determine whether climate change has intensified/affected individual extreme events?" Framing the question this way allows for a more clear discussion of the fact that because human activities have modified the climate generally, it is reasonable to conclude that all/many extreme events reflect some influence of climate change, particularly those that are intensified in the ways expected as climate warms. As Kevin Trenberth put it in a recent interview: "there is a systematic influence on all of these weather events now-a-days because of the fact that there is this extra water vaport lurking around in the atmosphere." The same could be said about heat waves and the extra heat lurking in the atmosphere. This idea of a systematic influence does not come across in the current FAQ. Link to the Trenberth interview: http://climateprogress.org/2010/06/14/ncar-trenberth-global-warming-extreme-weather-rain-deluge/ (Staudt, Amanda, National Wildlife Federation)	Good point. Consider changing title of FAQ. Or expanding the discussions to include the point about intensification/affecting. See response to #753.
752	3	23	8	0	0	All the text refers to increasing extremes. Can any comments be made regarding decreases? (Stewart, Ronald, University of Manitoba)	Decrease in frosts already included.
753	3	23	10	18	18	It is possible to attribute the changed probability of an event to a particular cause. I would prefer the response to this question to be phrased more positively. Suggestion : "Changes in climate extremes are... fossil fuels. However, clearly, a wide range of extreme events can occur in an unchanging climate and extreme events are often caused by a combination of factors, most of which would not be directly related to changing atmospheric composition. Nevertheless, it is possible to make an attribution statement about a specific single weather event by attributing the changed probability of its occurrence to a particular cause. Analysis of the ... [etc to end of para]." (Stott, Peter, Met Office)	Good point. Adopt this text (replaces changes in response to #751).
754	3	23	10	23	18	This is a very important FAQ and therefore the heading of this section should be as clear as possible. However, instead of rather clearly answering the question with "no", the text (lines 15-18) addresses an other issue, the likelihood of more extreme events in a changing climate. However, this is not the same as attributing INDIVIDUAL events to climate change. (Wernli, Heini, ETH Zürich)	First half of paragraph clearly indicates "No" - remainder of paragraph and FAQ then proceeds to discuss the answer to the way the question should have been phrased. See responses to comments 751, 753.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
755	3	23	14	23	14	I'm stumbling on the statement most of the factors contributing to a given extreme event "would not be directly related to changing atmospheric composition". I'm not sure how readers will interpret this , or whether the nature of the connection with atmospheric composition (direct or indirect) is relevant. There is better wording in the response itself that could be used here (lines 42-43 - multiple factors implies that it is difficult to attribute to a single factor). (Zwiers, Francis, Environment Canada)	Good point. Revise text.
756	3	23	17	23	18	Perhaps also mention the best estimate of the change in likelihood of a hot summer like 2003. (Zwiers, Francis, Environment Canada)	Too detailed.
757	3	23	17	0	0	Insert "Using model simulations" before "it has been estimated" (Van den Hurk, Bart, KNMI)	Too detailed for summary (more detail is provided in paragraph at bottom of page 23).
758	3	23	18	0	0	Since which time period has this probability doubled? (Stocker, Thomas, IPCC WGI TSU)	Detail is later in Box - cannot include all details in a summary paragraph.
759	3	23	20	23	26	I think the prolonged and intense heat wave which took place in Russia in 2010 should be mentioned here. (Bojariu, Roxana, National Meteorological Administration)	Already enough exmaples.
760	3	23	20	23	26	You can include examples from the summer of 2010 here. (Stott, Peter, Met Office)	Already enough examples - trying to shorten.
761	3	23	21	23	21	An approach to communicating the attribution problem for an individual extreme event is presented by: Hall, J.W., Twyman, C. and Kay, A. Influence diagrams for representing uncertainty in climate-related propositions. Climatic Change, 69 (2005) 343-365. (Hall, Jim, Newcastle University)	Too detailed for purpose of this FAQ.
762	3	23	22	23	22	Include the dates for the Australian drought. (Zwiers, Francis, Environment Canada)	Delete example.
763	3	23	22	23	22	Add "southern" before "Australia". (Trewin, Blair, Australian Bureau of Meteorology)	Delete example.
764	3	23	22	23	23	The extreme flood in Pakistan in 2010 should be added as the most actual and most extreme example (Kottmeier, Christoph, Karlsruhe Institute of Technology)	Already enough examples. Trying to shorten.
765	3	23	25	23	25	I suggest an update with major catastrophic events in 2010: large-scale flood in Pakistan and forest/turf fires associated with the exceptional heat wave in Russia. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Already enough examples. Trying to shorten.
766	3	23	26	0	0	Suggest adding: 'ie, would they not have occurred if CO2 had remained at pre-industrial levels?' (Stocker, Thomas, IPCC WGI TSU)	Agreed.
767	3	23	28	23	0	Reference should be provided. If the 'simple statistical calculation' merely assumes that November 2009 mean temperature is 3.5 standard deviation above its mean – assuming a Gaussian distribution – then this is disappointing. The reason why GEV/GPD and other extreme modelling approaches are used is precisely because the upper tails cannot be taken as Gaussian (which go too quickly to zero). In fact, it is legitimate to ask how the standard deviation was computed – a calculation of the second moment or using the standard L-estimates (which assumed Gaussian data)? The same goes for the refrence to June 2003. The Schär et al (2004) paper in Nature also assumed the summer temperature followed a Gaussian distribution, and substantially over-estimated the return-interval. Their conclusion was probably not valid, and not in accord with proper statistics. Another issue is the uncertainties associated with return periods that far exceed the length of observations – error bars must be provided! The method given in footnote 1 is not entirely clear, but if it is as I understand, it is extremely prone to sampling uncertainties. Does it treat different seasons separately or does it aggregate different locaitons? 1/00107 gives about 934, but the period 1950-2008 is only 59 years. Anyhow, it is well known that the magnitude of temperature variability varies both with season and location, and such an approach would not be valid. Hence, it this is what has been done, then this footnote will destroy some of the authority of the entire report. (Benestad, Rasmus, The Norwegian Meteorological Institute)	None of this comment contradicts the point being made here - that a single value of a variable widely removed from the mean of that variable is not unexpected, given the number of different locations we have available across the world. Although we could have used more complex statistics as proposed by the reviewer, this would not have changed the point, and the more complex statistics would have confused readers, and distracted them from the simple point of the paragraph. Removed numerical calculations dependent on a specific distriubtion, to avoid distracting readers from main point - that even an extreme event can be observed in an unchanging climate.
768	3	23	28	23	40	The potential non-normality of temperature frequency distributions in some locations is relevant here - a global figure of one in 1000 is not necessarily relevant for comparison if the NSW November data show a non-normal distribution. (I haven't checked whether they do or not, but could do so). Also, a region the size of NSW may contain multiple gridpoints which could affect the results. (Trewin, Blair, Australian Bureau of Meteorology)	See response to comment 767. Reviewers are missing the simple point here, by focussing on the detail. Remove numerical calculations dependent on a specific distribution, to improve focus on main point.
769	3	23	31	23	32	Suggest replacing "...would be observed in the 1950-2008 climate..." with "...woud have been observed in the 1950-2008 climate assuming stationary conditions ..." (i.e., state the assumptions implicit in the calculation). (Zwiers, Francis, Environment Canada)	Agreed.
771	3	23	34	23	34	Suggest replacing "1000" with "900" (1/0.00107 = 934; rounding to the nearest 100 yields 900 rather than 1000). (Zwiers, Francis, Environment Canada)	Agreed.
772	3	23	38	23	38	Delete "much". It is still a challenge to quantify attributed temperature changes on regional scales (such as the NWS scale) to specific agents such as ghgs, even if detection is possible. (Zwiers, Francis, Environment Canada)	Reject. The reviewer has not read the text carefully - it does nto attempt in any way to attribute the warm anomaly - it says the opposite in fact.
773	3	23	39	23	39	Add "global" before "precedent" - it was definitely unprecedented in Switzerland. (Trewin, Blair, Australian Bureau of Meteorology)	Agreed.
774	3	23	50	24	0	References needed. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Cannot include references in a FAQ.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
775	3	23	50	24	5	FAQ 3.2: I am a bit worried about this paragraph and the conclusions drawn for the assessment; is it correct that the conclusion of "more than doubled likelihood of having a summer in Europe as hot as that of 2003" is based on model simulations from one single model ("a climate model was run")...this could hardly be sufficient for a comprehensive assessment... (Stocker, Thomas, IPCC WGI TSU)	Will revise the text to make it clearer.
776	3	23	50	0	0	There is no citation here; probably this may be a reference: Stott, P. A., Stone, D. A. & Allen, M. R. Nature 432, 610–614 (2004). (Nakaegawa, Toshiyuki, Meteorological Research Institute)	Cannot include references in a FAQ.
777	3	23	0	0	0	The grid resolution should be stated in footnote 1. (Trewin, Blair, Australian Bureau of Meteorology)	Reject. Too detailed for purpose of this FAQ.
778	3	24	8	24	31	FAC 3.2: It is important to make it clear that the difficulty of attributing single extremes to human factors does not mean that the events are not caused by human actions. There is an opportunity to add real value with clear language on this point (IPCC WGII TSU)	Needs clarification. See responses to above comments.
780	3	24	20	24	21	Regarding Pall's study, I suggest that you say the region studied and what the result was. (Zwiers, Francis, Environment Canada)	Comment does not appear to refer to indicated text.
781	3	24	23	24	29	There may be a better chance of attributing a large-scale event (e.g. a regional monthly mean) than a value at a specific location for signal-to-noise reasons. (Trewin, Blair, Australian Bureau of Meteorology)	Agreed, but reviewer misses the point of the FAQ.
782	3	24	24	0	0	For the reasons outlined earlier in the text "very difficult" should read "impossible" when an event might have occurred naturally. (Klein Tank, Albert, KNMI)	Reject. It is theoretically possible that, despite the problems outlined in the text, an extreme might be so extreme that it is impossible to accept that it is not due to a new external agent. So, cannot replace "very difficult" by "impossible" here.
783	3	24	34	25	60	Much if this material is too general and of a 'text-book' /tutorial style. These paragraphs should be shortened and condensed to provide only an assessment relevant for extremes. (Stocker, Thomas, IPCC WGI TSU)	Done. The text has been reduced considerably
784	3	24	34	0	0	I am missing references of extreme events in models, compared to extreme events in reality. The models are generally not able to reproduce locally measured extremes for several reasons, including scales. An approach used in several studies to overcome this problem is to consider the specific model specific percentiles as thresholds as indicators. An example referring to the intensity of cyclones and wind speeds is found in Leckebusch, G.C. and U. Ulbrich, 2004: On the relationship between cyclones and extreme windstorm events over Europe under climate change. Global and Planetary Change, 44, 181-193. (Ulbrich, Uwe, Freie Universitaet Berlin)	Validation is now discussed briefly in the new standard structure adopted for each specific extreme in Sections 3.3 to 3.5. This is now noted in the second paragraph of Section 3.2.3.1
785	3	24	38	24	38	Why Ch 11 rather than Ch 10 when I think most people would regard Ch 10 as providing the more definitive assessment of projections that were available for the AR4? (Zwiers, Francis, Environment Canada)	Agreed. The reference has been changed
786	3	24	41	24	41	Table 3.3 is too complicated to see it. Suggestion is to plot projected changes by several global maps as IPCC AR4 presented. (Zhao, Zong-Ci, National Climate Center)	Table 3.3 is retained but global maps from OS2010 are now also included.
789	3	24	50	24	0	Please specify information on how the model resolution - improved between AR3 and AR4 - lead to more credible simulations of processes important for regional change. In AR3, the GCMs had a resolution spanning from T21 – T47 in the atmosphere (Table 8.1), whereas in AR4 the resolution is mainly T42, but with some T63 and one T106, but. The question is whether this has resulted in an improvement of the regional climate description. There are two views within the community: keeping the model resolution low and focus on the large-scale phenomena while keeping weather noise down, or increasing the resolution to capture as much as possibly of the processes. Higher resolution may place higher demands on parametrisation schemes. There are also some indication that resolution substantially higher than T106 is required to provide a realistic description of the north-Atlantic blocking frequencies. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Disagree. This introductory paragraph does not seem the right place to specify about consequences on increasing model resolution. Nevertheless, a reference to related AR4 discussion has been added. Section 3.2.3.4 notes that increased resolution does not necessarily mean more reliable projections.
791	3	24	55	24	0	based on physical laws as well as observations (Paul N Edwards, 2010, A Vast Machine: Computer Models, Climate Data, and The Politics of Global Warming, MIT Press). This is also acknowledged in the paragraph below, but much of the signal from the climate change simulation are sensitive to the parametrisation schemes, e.g. radiation, land-surface, and cloud schemes, and perhaps not so much to the model dynamics. Perhaps re-phrase this paragraph? References are required for statements about AOGCM skill, and the text should be more precise about what climate features. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Agreed. The paragraph has been re-phrased and also shortened following the suggestion of another reviewer.
793	3	25	7	25	7	Note that "wind gusts" are not mentioned in Section 3.2.1. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Deleted.
794	3	25	7	25	7	Mentioning "convective events and wind gusts" is misleading, since these phenomena cannot be resolved by regionalizing global climate. The resolution of e.g. ENSEMBLES models was 20 to 50 km, and only few ensembles regionalizations down to 7 km resolution are available. e.g. Früh, B., Feldmann, H., Panitz, H.-J., Schädler, G., Jacob, D., Lorenz, P., Keuler, K.: Determination of Precipitation Return Values in Complex Terrain and Their Evaluation, J. of Climate, 23 (9), 2257-2274, 2010; (Kottmeier, Christoph, Karlsruhe Institute of Technology)	Agreed. These examples have been deleted.
795	3	25	10	25	13	Resolution of global models is increasing, but perhaps not as quickly as this would imply. (Zwiers, Francis, Environment Canada)	Agreed. The sentence has been re-phrased.
796	3	25	12	25	13	I suggest to delete the part of the sentence given in brackets: "it should not be assumed that greater resolution necessarily translates into greater credibility of projections" -- of course not....and there is again the formulation "credibility of projections". (Stocker, Thomas, IPCC WGI TSU)	Agreed. The whole paragraph has been revised accordingly. Though it is noted that this phrase was originally included in response to a ZOD comment.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
797	3	25	15	25	0	One concern with RCMs is that they may involve different parameterisation schemes to the driving models (GCM or coarser RCM), and do not allow coupling between ocean and atmosphere. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Agreed. The comment has been added to the text with Wang et al., 2004 as a supporting reference. The wording used reflects the fact that some coupled atmosphere-ocean RCMs are now available (e.g., for the Mediterranean).
798	3	25	15	25	15	RCMs with 4-5km resolutions, which were used to study climate change and climate projection over Japan, successfully conducted 5years for all seasons (Sasaki et al., 2008) and 10years for warm season (Kanada et al.,2008; Kitoh et al., 2009; Kanada et al.,2010). Sasaki, H., K. Kurihara, I. Takayabu and T. Uchiyama, 2008: Preliminary experiments of reproducing the present climate using the non-hydrostatic regional climate model, SOLA, Vol.4, 25-28. Kitoh, A., T. Ose, K. Kurihara, S. Kusunoki, M. Sugi and KAKUSHIN Team-3 Modeling Group, 2009: Projection of changes in future weather extremes using super-high-resolution global and regional atmospheric models in the KAKUSHIN Program: Results of preliminary experiments. Hydrological Research Letters, 3, 49-53. Kanada, S., M. Nakano, S. Hayashi, T. Kato, M. Nakamura, K. Kurihara and A. Kitoh, 2008:Reproducibility of Maximum Daily Precipitation Amount over Japan by a High-resolution Non-hydrostatic Model. SOLA, Vol. 4, 105-108. Kanada, S., M. Nakano and T. Kato, 2010: Climatological characteristics of daily precipitation over Japan in the Kakushin regional climate experiments using a non-hydrostatic 5-km-mesh model: Comparison with an outer global 20-km-mesh atmospheric climate model, SOLA, Vol. 6, 117-120. (Kurihara, Kazuo, Meteorological Reserach Institute)	Kanada et al., 2010 and Wakazuki et al 2008 are now cited as these references provide the best illustration of the improved performance of the non-hydrostatic model at 5 km resolution compared with the 20km hydrostatic model.
799	3	25	15	25	31	It should be noted that RCMs not only better reproduce orographically-induced meteorological phenomena (gusts, precipitation), but also are able to represent specific extremes due to the higher resolution of the relevant meteorological fields (e.g., higher pressure gradients). (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Agreed. The new version of this paragraph better describes this point.
800	3	25	15	25	31	A good reference on stretched grid modelling is Fox-Rabinovitz, M., J. Côté, B. Dugas, M. Déqué, J. L. McGregor, and A. Belochitski, 2008: Stretched-Grid Model Intercomparison Project: decadal regional climate simulations with enhanced variable and uniform-resolution GCMs. Meteor. Atmos. Phys., 100, 159-178. Also, the paragraph ends with a note on RCM disadvantages but only notes cost and resolution. There are more theoretical concerns regarding RCMs which could be mentioned. This may be a good reference: Wang, Y., L. R. Leung, J. L. McGregor, D.-K. Lee, W.-C. Wang, Y. Ding, and F. Kimura, 2004: Regional climate modeling: progress, challenges, and prospects. J. Meteor. Soc. Japan, 82, 1599-1628 (Whetton, Penny, CSIRO Marine and Atmospheric Research)	The stretched-grid reference has been added. Other disadvantages of RCMs are now mentioned and Wang et al cited along with Laprise et al 2008.
801	3	25	15	0	0	The MRI/JMA 20-km mesh AGCM experiment forced with prescribed SST is a kind of global scale downscaling: (Akio Kitoh, Tomoaki Ose, Kazuo Kurihara, Shoji Kusunoki, Masato Sugi and KAKUSHIN Team-3 Modeling Group: "Projection of changes in future weather extremes using super-high-resolution global and regional atmospheric models in the KAKUSHIN Program: Results of preliminary experiments", Hydrological Research Letters, Vol. 3, pp.49-53, (2009). (Nakaegawa, Toshiyuki, Meteorological Research Institute)	This reference is now cited along with Kamiguchi et al 2006 and Kim et al 2010
803	3	25	24	25	25	I think it is obligatory here to mention the Japanese time slice experiments. (Zwiers, Francis, Environment Canada)	Agreed, 3 references are now cited - see comment 801.
805	3	25	28	25	31	I think this understates uncertainties associated with RCMs, of which there are many. While confidence is increasing, the community is still coming to an understanding of the "value added" provided by RCMs, and questions remain about things like domain size (larger domains imply weaker constraints on interior points when spectral nudging or a similar driving technique is not used) and the extent to which biases, etc., are inherited from the driving model. (Zwiers, Francis, Environment Canada)	Agreed. The paragraph has been rewritten accordingly and two references added (Wang et al., 2004 and Laprise et al 2008).
806	3	25	29	0	0	Insert "when given realistic (observed) boundary conditions" after "around the world" (Van den Hurk, Bart, KNMI)	The new version of the text does not include that particular sentence.
807	3	25	33	25	33	explain "cross-spatial-scale relationship" (Brönnimann, Stefan, University of Bern)	Agreed. The text has been modified accordingly.
808	3	25	33	25	38	Since there has been a substantial effort to produce local scenario of both mean values and extremes over Europe and the Mediterranean region I think that it should be mentioned in this list. See for example the reference already included in this chapter Schmidli et al (2007), but also Tomozeiu R., Cacciamani C., Pavan V., Morgillo A., and Busiuc A. (2007) 'Climate change scenarios for surface temperature in Emilia-Romagna (Italy) obtained using statistical downscaling models.'Theoretical and Applied Climatology, 90, 25-47. (Pavan, Valentina, ARPA Emilia-Romagna)	Schmidli et al., 2007 is now added as a European example. Space precludes including more examples.
809	3	25	33	25	51	I suggest that a more systematic overview is given of the different types of statistical downscaling techniques: regression based, stochastic modelling based, weather typing or re-sampling based, ... (Willems, Patrick, Katholieke Universiteit Leuven)	Disagree. The description of different downscaling methods has been considerably reduced as suggested by other reviewers. Such a systematic overview/review would be beyond the scope of this chapter/report.
810	3	25	34	25	34	they "may" also include weather generators (Brönnimann, Stefan, University of Bern)	Agreed. Text modified.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
811	3	25	35	25	35	Consider also Maraun, D., F. Wetterhall, A.M. Ireson, R.E. Chandler, E.J. Kendon, M. Widmann, S. Brienen, H.W. Rust, T. Sauter, M. Themeßl, V.K.C. Venema, K.P. Chun, C.M. Goodess, R.G. Jones, C. Onof, M. Vrac & I. Thiele-Eich (2010): Precipitation downscaling under climate change. Recent developments to bridge the gap between dynamical models and the end user. - Reviews of Geophysics, accepted. (Brönnimann, Stefan, University of Bern)	Reference added.
812	3	25	35	25	35	The computational "lightness" is a marginal virtue of statistical downscaling. The main advantage is that it provides natural estimates of uncertainty. (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	Noted. The meaning of this comment is not very clear. The computational lightness implies that multiple simulations can be undertaken to explore uncertainty. Some, but not all, statistical downscaling methods include a stochastic component whereas others are deterministic.
813	3	25	36	25	38	What about Europe and North America? (Brönnimann, Stefan, University of Bern)	Schmidli et al., 2007 is now added as a European example. A 'new' North American category has been included citing Vrac et al., 2007, together with the existing Canadian example.
814	3	25	38	25	0	and Europe (e.g. Benestad, R.E.(2007) Novel Methods for Inferring Future Changes in Extreme Rainfall over Northern Europe Climate Research, CR34:195-210, doi: 10.3354/cr00693). Remove 'potentially' in potentially able to access finer spatial scales. Statistical downscaling is almost always made for point-measurements. (Benestad, Rasmus, The Norwegian Meteorological Institute)	This reference has been added as an earlier example of downscaling of the extremes distribution. Potentially has been removed (it was included to indicate that observed data are required).
815	3	25	38	25	38	Also add Europe in this enumeration (e.g., Enke et al., 2005): Enke, W., Th. Deuschländer, F. Schneider, and W. Küchler, 2005: Results of five regional climate studies applying a weather pattern based downscaling method to ECHAM4 climate simulations. Meteorologische Zeitschrift, 14, 247-257 (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	There is only space for one European example - the example chosen is from a major intercomparison project and is a multi-institutional paper which also compares statistical and dynamical downscaling.
816	3	25	47	25	48	Stationarity is a fundamental assumption, it cannot really be resolved by adequate training and validation. (Brönnimann, Stefan, University of Bern)	Agreed. The comment in brackets has been deleted. Though it is noted that this was originally included in response to a ZOD comment.
817	3	25	49	25	50	This can't be completely true for all downscaling techniques - weather generators, for example, must be able to simulate unobserved values, including at least some values beyond the range of the observations on which the weather generator was trained. (Zwiers, Francis, Environment Canada)	Agreed. Notice that the sentence originally said "some statistical methods" - this has been modified to "some analog statistical methods" to further emphasize that it does not apply to all.
818	3	25	50	25	0	But it is possible to use statistical downscaling to model the shape of the PDF, (Benestad, R.E., 2007, Novel Methods for Inferring Future Changes in Extreme Rainfall over Northern Europe Climate Research, CR34:195-210, doi: 10.3354/cr00693.) and then re-calibrate the data through a local quantile transform (Themessl et al (2010) 'Empirical-statistical downscaling and error correction of daily precipitation from regional climate models' Int. J. Clim, DOI: 10.1002/joc.2168; Benestad, R.E. 2010, 'Downscaling Precipitation Extremes: Correction of Analog Models through PDF Predictions', Theor. & Appl. Clim, Volume 100, Issue 1, DOI:10.1007/s00704-009-0158-1;). (Benestad, Rasmus, The Norwegian Meteorological Institute)	See comment 817. Space precludes going into further technical detail. Benestad 2007 is now cited earlier as an example of downscaling a distribution rather than time series. Themessl et al 2010 is now cited in section 3.2.3.4 in the discussion on bias correction.
819	3	25	50	25	51	Ideally should have a reference. Charles et al. (1999) Climate research 12, 1-14. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Reference added, along with Hewitson and Crane, 2006.
820	3	25	53	25	0	Should also refer to Themessl et al (2010) 'Empirical-statistical downscaling and error correction of daily precipitation from regional climate models' Int. J. Clim, DOI: 10.1002/joc.2168 for the Alpine region, arguing that empirical-statistical downscaling and error correction methods can improve RCM results, and that these two methods should be combined. (Benestad, Rasmus, The Norwegian Meteorological Institute)	This reference is now cited in section 3.2.3.4 in the discussion on bias correction.
821	3	25	58	25	58	add: "Prudhomme and Davies (2009) assessed uncertainty in modelling climate change impacts on river flow in the UK. They used three different GCMs, two different emission scenarios and two different downscaling techniques to investigate the main sources of uncertainty in climate change impact studies. They found that the largest uncertainty comes from the choice of GCM. Downscaling techniques and emission scenarios are a smaller source of uncertainty and are of similar magnitude." Prudhomme, C. & Davie, H. (2009) Assessing uncertainties in climate change impact analyses on the river flow regimes in the UK. Part 2: future climate. Climatic Change, 93, 197-222. (van Lanen, Henny A.J., Wageningen University)	The paragraph has been moved to section 3.2.3.3. But this reference has not been included as it is not relevant to the specific text and sufficient references are already provided on the partitioning of uncertainty.
822	3	25	0	25	0	You should mostly refer to a few references such as Christensen et al ,2007 that is ok. And it is very excellent But please think to the user and reader who wants the mathematical and statistical methods for doing a work. When the user refer to that section of IPCC,2007 , he should again to refer to the other references again. Therefore I suggest, in addition of these references, name the other articles. (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERCI))	Noted. It's not clear what the reviewer is asking for. The focus here is on post-AR4 material and there is not space to give full mathematical and statistical details in this chapter.
823	3	26	8	26	0	Should these paragraphs be at the beginning or towards the end? Or together with the earlier discussion about GCMs? At the moment, the report is a bit fragmented with these kinds of discussions. There is also some degree of repetition. The question is whether organising the material on model skill in one place or have that scattered around in the chapter. Also when stating 'cannot always be made with a high level of confidence' (L42-43) after a lengthy discussion about why it is very hard to say anything about changes in extremes or regional climate change, the report seems to be double communicating. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Agreed. 3.2.3.1 and 3.2.3.2 have been revised considerably in order to avoid repetitions and to organize better the discussion.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
825	3	26	26	26	26	Delete "in many". While there may be a few models that run at cloud resolving scales over small regions, the statement would nevertheless be correct for all RCMs as well as AOGCMs. (Zwiers, Francis, Environment Canada)	Done.
826	3	26	26	26	26	Another important shortcoming seems to be the absence of stratospheric processes in many CMIP4 models that influence (changes in) the large-scale circulation that effects extremes. (van Oldenborgh, Geert Jan, KNMI)	Agreed. It is now included in the paragraph
827	3	26	26	26	39	Mentioned blocking is not well simulated in models (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Agreed. It is now included in the paragraph
829	3	26	32	0	0	"are still not resolved sufficiently" -> "still don't resolve the atmospheric processes sufficiently" (Van den Hurk, Bart, KNMI)	Done
830	3	26	33	26	35	Note that even NWP models have difficulty with this. (Trewin, Blair, Australian Bureau of Meteorology)	Agreed. The comment has been included
831	3	26	35	26	37	Simulation with high resolution (4-5km) RCMs showed ability in reproducing climatic characteristics of extreme precipitation events (Wakazuki et al., 2008; Sasaki et al, 2008; Kanada et al., 2008; Kanada et al., 2010). Wakazuki, Y., M. Nakamura, S. Kanada, and C. Muroi, 2008: Climatological reproducibility evaluation and future climate projection of extreme precipitation events in the Baiu Season using a High-Resolution Non-Hydrostatic RCM in comparison with an AGCM. Journal of the Meteorological Society of Japan, Vol. 86 (2008), No. 6, 951-967. Sasaki, H., K. Kurihara, I. Takayabu and T. Uchiyama, 2008: Preliminary experiments of reproducing the present climate using the non-hydrostatic regional climate model, SOLA, Vol. 4, 25-28. Kanada, S., M. Nakano, S. Hayashi, T. Kato, M. Nakamura, K. Kurihara and A. Kitoh, 2008: Reproducibility of Maximum Daily Precipitation Amount over Japan by a High-resolution Non-hydrostatic Model. SOLA, Vol. 4, 105-108. Kanada, S., M. Nakano and T. Kato, 2010: Climatological characteristics of daily precipitation over Japan in the Kakushin regional climate experiments using a non-hydrostatic 5-km-mesh model: Comparison with an outer global 20-km-mesh atmospheric climate model, SOLA, Vol. 6, 117-120. (Kurihara, Kazuo, Meteorological Research Institute)	The two most informative references on the added value of the high-resolution non-hydrostatic models (Wakazuki et al 2008 and Kanada et al 2010) are now referred to in an earlier part of this section. They are not directly relevant to the specific issues being discussed in this paragraph.
832	3	26	45	26	51	This paragraph is very "theoretical". You could give one or two examples for such processes and relationships, also showing the limitations of this approach. For example relating extreme gust to extreme wind, to extreme pressure gradients and to extreme cyclones is a possible example. In the end, it is the physical, synoptic understanding of what a model does which is part of the basis for assessing the credibility of any signals. (Ulbrich, Uwe, Freie Universitaet Berlin)	An example from Kendon et al 2009 which is already referred to in this sentence has been added.
833	3	26	47	26	47	The reference to van Oldenborgh (2005), which treats ENSO, does not seem to be relevant here. (van Oldenborgh, Geert Jan, KNMI)	Deleted.
834	3	26	52	26	53	Perhaps cite the report from the IPCC Expert Meeting on Assessing and Combining Multimodel Climate Projections. See http://www.ipcc.ch/pdf/supporting-material/IPCC_EM_MME_GoodPracticeGuidancePaper.pdf . (Zwiers, Francis, Environment Canada)	The citation has been added.
835	3	26	52	0	0	If we don't know how to evaluate climate models, how can the text elsewhere state that climate model projections are to a certain extent reliable? (Klein Tank, Albert, KNMI)	Agreed. That sentence was confused. The whole paragraph has been revised following suggestions made by other reviewers.
836	3	26	53	26	53	A credible representation of trends up to now is one factor that increases credibility of future model trends. Temperature trends are by now strong enough that in many regions of the world they are larger than internal variability, making this additional validation possible for this variable. (van Oldenborgh, Geert Jan, KNMI)	Agreed. The comment has been added to the text
837	3	26	53	0	0	Reto Knutti has published a lot on this issue. Knutti (2010), Climatic Change DOI 10.1007/s10584-010-9800-2, would be a good reference. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Agreed. The reference has been included.
838	3	26	55	26	55	Insert "dynamical" ahead of downscaling. (Zwiers, Francis, Environment Canada)	Due to the suggestions of other reviewers the paragraph has changed considerably. That sentence no longer exists.
839	3	26	55	27	7	Again - this is not focussed on extremes and is too general. (Stocker, Thomas, IPCC WGI TSU)	Agreed. 3.2.3.1 and 3.2.3.2 have been revised considerably in order to avoid repetitions and to organize better the discussion.
840	3	26	55	27	7	This would be a good paragraph to discuss the approach of integrating all known sources of uncertainty to produce probabilistic predictions of future changes in extremes produced by UKCP for the UK. (see comment 2 for relevant references) (Brown, Simon, The Met Office Hadley Centre)	Due to the suggestions of other reviewers the paragraph has changed considerably and now primarily refers to the new IPCC guidance on assessing and combining multi-model climate projections. See also response to comment 864.
841	3	26	59	26	61	I think there is an inherent contradiction in this statement. If feedbacks from the high resolution region onto the larger scale were seen to be important, then we would know that the simulations of the large scales would be incorrect, since feedbacks from small scales in regions where resolution was not enhanced onto large scales would then obviously be missing or distorted. (Zwiers, Francis, Environment Canada)	Agreed. The sentence has been re-phrased.
842	3	26	60	0	61	This sentence seems not to be relevant to uncertainty sources. (Nakaegawa, Toshiyuki, Meteorological Research Institute)	Agreed. The sentence has been deleted.
843	3	27	3	0	0	Underused? If the quality of RCMs is shown to be limited? This is a qualitative statement, and should not be given here (Van den Hurk, Bart, KNMI)	Agreed. The sentence has been deleted.
844	3	27	7	27	7	Care should be taken that the downscaled results reflect the full range of AOGCM uncertainty (eg van den Hurk et al, Water Science and Technology, 2007, doi:10.2166/wst.2007.533). (van Oldenborgh, Geert Jan, KNMI)	Following the suggestions of other reviewers the paragraph has been deleted.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
845	3	27	9	0	0	Section 3.2.3.3 on Exploring and Quantifying Uncertainties: As noted in the general comments, this is key to more than just Chapter 3; I wonder whether it wouldn't be better to combine the information on how to address uncertainty within IPCC Reports in a Box which would be moved to Chapter 1. The Chapter 3 specific definitions would of course need to stay in this section, such as the "congruence/evidence" terminology introduced here (Stocker, Thomas, IPCC WGI TSU)	Uncertainty language is now addressed in Section 3.1.5 of our chapter.
846	3	27	14	27	17	To avoid the impression of arbitrariness in the design of ensembles, the kind of ensemble approach should be referred to the kind of uncertainty: (i) multi-model ensembles to account for aleatoric uncertainty due to climate noise and (ii) intra-model ensembles to account for epistemic uncertainty due to limited knowledge and reproduction in the model of the climate system. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Agreed. The paragraph has been rewritten accordingly
848	3	27	22	27	22	CMIP3: Suggest to mention here not just the CMIP3 effort, but to also refer to the ongoing CMIP5 effort. And please note that those efforts are organized and maintained by the research community, and thus completely outside of IPCC. This said, there is nothing like an AR4 MME...however, IPCC has been using the CMIP3 MME heavily in its AR4. Perhaps one could mention in this regard the IPCC Expert Meeting on Assessing and Combining Multi-Model Climate Projections and the related Meeting report and Good Practice Guidance Paper (available from http://www.ipcc-wg1.unibe.ch/publications/supportingmaterial/supportingmaterial.html)+G48 (Stocker, Thomas, IPCC WGI TSU)	Agreed. The text has been modified accordingly.
849	3	27	23	27	0	Isn't it better to use the common reference to the GCM ensemble reported in AR4: 'CMIP3'? Why introduce a new name? Besides, the CMIP3 multi-model ensemble is an initiative technically independent of IPCC/AR4; albeit that the IPCC draws heavily on its results. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Agreed. The text has been modified accordingly.
850	3	27	23	0	0	Mention PCMDI here (Stocker, Thomas, IPCC WGI TSU)	Done
851	3	27	23	0	0	Please provide link to the archive (Luterbacher, Juerg, Justus Liebig University)	Done
852	3	27	23	0	0	The central archive should be specified or identified (Zerefos, Christos, Academy of Athens)	Done
853	3	27	28	27	28	and uncertainty due to randomness itself, which is the canonical statistical definition. (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	Agreed. The text has been modified accordingly.
854	3	27	28	27	32	Add to this sentence (after '...provided in Tables 3.1-3.3'): '...and the uncertainty due to the lack of understanding of the related physical process.' Reason: The understanding of processes is an important part of the assessment of the confidence in results in the report. Thus corresponding uncertainties should be mentioned here, too. Maybe it should also be discussed that for certain phenomena, where the processes are not well understood (e.g. clouds), even if there are many models available, the range of model results might not be representative for the actual uncertainty range and thus underestimate uncertainties. (New, Urs, Swiss Academy of Sciences)	The lack of understanding is included within the uncertainty due to insufficient evidence, following the new IPCC Guidance Notes on the consistent treatment of uncertainties.
855	3	27	28	27	54	Nice discussion (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Noted. Thanks.
856	3	27	28	0	54	Uncertainty relating to emission scenarios should NOT be considered as an influence on model performance or congruence. The uncertainty relating to scenario selection must be treated separately from uncertainty relating to the model performance itself. Please clearly differentiate these different types of uncertainty, eg, see Figure 10.1 of AR4. (Stocker, Thomas, IPCC WGI TSU)	Agreed. The text has been modified accordingly.
857	3	27	37	27	38	Initial conditions may be important for decadal and shorter time scale forecasting, but there is virtually no discussion of short term climate predictability or prediction in this chapter, so I see no specific reason to raise this issue here. (Zwiers, Francis, Environment Canada)	Agreed. The paragraph has been reduced extensively and the discussion about the uncertainties in short-term climate projections has been omitted.
858	3	27	38	27	54	I think the term "uncertainty in the initialization of climate projections" produces a wrong impression and should be avoided. Slightly different initialisation leads to different climate realisations of the models, in particular in terms of long term climate variations. Such variations are not just existing in reality, but are also produced by models. These variations contribute to the fact that climate signals differ in an ensemble of model runs. The exact starting conditions (initialisation) are of less importance. This may change in the future when we can produce reliable decadal predictions, but not now. (Ulbrich, Uwe, Freie Universität Berlin)	Agreed. The paragraph has been reduced extensively and the discussion about the uncertainties in the initial conditions has been omitted.
859	3	27	43	27	43	It may be better to use a decadal mode here rather than ENSO, for example the Atlantic Multidecadal Oscillation which is skilfully hindcast by initialised models (Pohlmann et al, J.Clim., 2009, doi:10.1175/2009JCLI2535.1) (van Oldenborgh, Geert Jan, KNMI)	Due to the comments made by other reviewers, that particular discussion has been omitted.
860	3	27	47	27	0	This discussion concerns temperature – as mentioned below – and the text should be more specific about that. Part of the paragraph can be re-phrased and made more concise. And what about wind, and phenomena such as cyclones, ENSO, and natural modes? (Benestad, Rasmus, The Norwegian Meteorological Institute)	Agreed. The paragraph has been reduced considerably.
861	3	27	62	27	62	This statement, that there was no sampling protocol, begs the question of what sampling protocol you would impose? Despite some research on QUMP (notably by the Hadley Centre and collaborators), the community does not have the resources to build into CMIP a robust multi-model QUMP-like experiment. Moreover, even if such an experiment were to be conducted, we would still not be able to treat the available 20-30 global models as a random sample drawn from some hypothetical population under a known sampling procedure. So, how would one design an experiment that "spans the full possible range of uncertainty"? And if we were able to do that, would the result be useful? Consider UKCP09. (Zwiers, Francis, Environment Canada)	Following the suggestions of other reviewers, the discussion about projection uncertainties has been reduced considerably.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
862	3	27	62	27	63	This point needs a reference, especially since quite a bit has been written on it. Various recent Knutti papers are relevant for a start. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Agreed. A reference to Knutti et al (2010) has been added.
863	3	28	8	0	0	"had" -> "have" (Van den Hurk, Bart, KNMI)	Done
864	3	28	10	28	10	As per comment 2 this statement about UKCP is misleading. A full discussion of the UKCP aggregation of uncertainty, including parameter uncertainty, downscaling, time-scaling, structural and carbon uncertainty. See annex 2 of UK Climate Projections science report annex 2 (Brown, Simon, The Met Office Hadly Centre)	There is not space for a full discussion. But the papers listed in comment 922 on analysis of extremes in the Hadley Centre PPE are now included, together with a reference to the UKCP09 technical report.
865	3	28	11	28	0	Additional reference: Benestad, R.E., 2007, Novel Methods for Inferring Future Changes in Extreme Rainfall over Northern Europe Climate Research, CR34:195-210, doi: 10.3354/cr00693 used a set of weighted ensemble mean of CMIP3 simulations to derive PDFs for 24-hr precipitation for northern Europe. (Benestad, Rasmus, The Norwegian Meteorological Institute)	The reference has not been added here as it does not discuss the construction of probabilistic projections in the sense meant here.
866	3	28	13	0	0	Please add the reference Maraun et al. (2010) for a review after 'coverage' (Luterbacher, Juerg, Justus Liebig University)	Added
867	3	28	21	28	0	Reference is needed here on the sensitivity of results to the domain size. E.g. Benestad, R.E. (2001) A comparison between two empirical downscaling strategies, Int. J. Climatology, Vol 21, Issue 13, pp.1645-1668. DOI 10.1002/joc.703. Regarding testing statistical downscaling, this is usually done against independent data, and hence over-fit is usually not an issue. It is also possible to test these models to see if they reproduce the trends in the past or if – when calibrated entirely with GCM data, taking grid-point values as predictand – there are any information in the GCMs that suggest that the relationship between the large and small scales will change in the future (e.g. Benestad, R.E., E.J. Førland & I. Hanssen-Bauer: An evaluation of statistical models for downscaling precipitation and their ability to capture long-term trends Int. J. Clim, 27: 649-665, DOI: 10.1002/joc.1421). (Benestad, Rasmus, The Norwegian Meteorological Institute)	The specific sentence actually refers to RCM domain size, so two more appropriate references have been added. That domain size is also an issue for statistical downscaling is now stated, with Benestad 2001 as a supporting reference. The point about overfitting is noted - but it is not claimed here that all statistical models are overfitted. The Benestad et al 2007 reference has been added.
868	3	28	24	28	24	Replace "may not" with "fits well but will not". (Zwiers, Francis, Environment Canada)	Done
869	3	28	29	28	29	Insert "dynamical" ahead of "downscaling". (Zwiers, Francis, Environment Canada)	Done
872	3	28	34	0	0	Only some extremes can be assessed at daily time scales. (Stewart, Ronald, University of Manitoba)	Agreed. The sentence has been modified accordingly.
873	3	28	41	28	42	It is no more possible to avoid an ensemble of opportunity with RCMs than it is with AOGCMs, so I'm not sure how one would ensure "adequate sampling of RCMs". (Zwiers, Francis, Environment Canada)	Agreed. Noted.
875	3	28	43	0	0	"contribution on ... annual timescales": don't know what is implied here: extremes that have an annual time scale, or projection horizons multiple years ahead, or something else? (Van den Hurk, Bart, KNMI)	This refers to the 'spectrum of variability' - as now stated.
876	3	28	45	28	45	A recent study (Raje and Mujumdar, 2010) has used GCM simulation of natural variability to constrain uncertainty in downscaled streamflow index projection, in a monsoon regime in Orissa, India. (MUJUMDAR, PRADEEP, INDIAN INSTITUTE OF SCIENCE)	This reference is now cited in an earlier part of this section where it is more relevant.
877	3	28	47	28	0	This paragraph is a repetition of previous text. Cut. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Paragraph has been deleted.
878	3	28	47	28	51	The information in this short paragraph is repeated many times throughout the chapter, and in relation to specific extremes later in Sections 3.3 - 3.5. It does not need to be repeated here. In any case, this describes a source of uncertainty, rather than a method for exploring or quantifying uncertainty. (Stocker, Thomas, IPCC WGI TSU)	The paragraph has been deleted.
879	3	28	48	0	0	I found no explanation in which extreme weather/climate impacts glacier mass balance. Need references. (Fujita, Koji, Nagoya University)	Following the suggestion of other reviewers, the whole paragraph has been deleted.
880	3	28	51	0	0	Uniform accross the globe? (Klein Tank, Albert, KNMI)	Following the suggestion of other reviewers, the whole paragraph has been deleted.
881	3	28	53	28	62	This statement from Roe and Baker (2007) is being over-interpreted here in my view, who were mainly discussing the difficulties in constraining the upper limit on equilibrium climate sensitivity (there was a commentary by Myles Allen who argued it didn't matter - what is more relevant is the constraint on transient climate response). Knutti et al (J. climate, 21, 2651-2663, 2008) conclude that while projections for given scenarios have not changed much in recent years, recent progress has increased the confidence in uncertainty estimates and now allows a better separation of the uncertainties introduced by scenarios, physical feedbacks, carbon cycle and structural uncertainty. In my view this para should be reworded with more a careful attention to the literature, including the emerging literature, eg from Alex Hall which is showing potential to observationally constrain aspects of Arctic climate change, and such a rewrite would result in a more balanced and more hopeful assessment of the science. (Stott, Peter, Met Office)	Following other suggestions that this report is not the right place to assess climate sensitivity, the whole paragraph has been removed.
882	3	28	54	28	56	Presumably this result is due to the spread of emission scenarios? (Trewin, Blair, Australian Bureau of Meteorology)	Following other suggestions that this report is not the right place to assess climate sensitivity, the whole paragraph has been removed.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
883	3	28	54	28	62	This paragraph gives too much credence to one study on climate sensitivity; there have been a number of papers critiquing the Roe and Baker work. A number of more recent studies suggest that the high values of climate sensitivity are less well founded than was thought at the time of the AR4 (see e.g. Joshi et al., ACP, 2010). This report is not the place for a detailed re-evaluation of climate sensitivity but it does need to be balanced. See the NRC stabilization targets report (available at http://dels.nas.edu/Report/Climate-Stabilization-Targets-Emissions-Concentrations/12877) for a current review of this literature. Then include some of the papers and discussion referenced there and indicate that whether the 'long tail' suggested by Roe and Baker is plausible is still not clear and is not assessed here. (Solomon, Susan, NOAA)	Agreed. The whole paragraph has been removed.
884	3	28	58	28	0	Reference is required to back up the statement about multiple models and emission scenarios. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Following other suggestions that this report is not the right place to assess climate sensitivity, the whole paragraph has been removed.
885	3	29	1	24	25	I could not see the aim of this section. It reads a bit like a mixture of rather unrelated issues. It would be helpful for the reader if the text could be better streamlined and the paragraphs linked with each other. (Wernli, Heini, ETH Zürich)	This section has been considerably shortened including the removal of some technical details which tended to break up the flow. Note that other reviewers comment positively on the need for this section.
886	3	29	1	29	1	Should this subsection come sooner? Also, I think this subsection could profitably be reduced to half its length, or less. (Zwiers, Francis, Environment Canada)	The subsection has been considerably reduced in length but has not been moved.
887	3	29	1	30	25	This section along with section 3.1 are really the crux of what should guide content of this chapter on the physical aspects of extremes and associated things, as I see it, given the whole study is not on events as such but rather on management concerns when faced with them and disasters. As the study as a whole is not called to be a strictly scientific study of extremes, disasters, risk, management etc as, but rather one that tries to provide guidelines to how the managing of the risk associated with extremes and disasters in the past, up to now and in the future could guide future adaptation measures and planning, this topic and central concern should guide what is actually included in each chapter. Despite the very high level of scientific information and debate available in this chapter I really do think what is here in general will not appeal to decision makers and that they will not read it as it goes way over their heads and beyond their needs. What basically should be here from my perspective (and to repeat a former comment) is not a discussion of limitations to knowledge due to lack of as yet adequate or complete methods, or diverse, but not comprehensive regional and location specific information, or debates on problems of compound events, attribution, etc. Rather what a decision maker needs is a very clear statement on what he can know with a certain level of certainty, what he can't know, at what scales they can know this etc etc. That is to say, a good deal of the scientific info and debate in this chapter is, from the perspective of the objectives of the study, probably unnecessary and despite its excellent quality and thoroughness, will not be read by the target audience. Decision makers once they have a summary picture of what they can be certain about in general and what they can't, and as to what options for action are, will then search out the complementary information elsewhere as it pertains to their particular jurisdiction, but it is not necessary to have it in a chapter in a study which is centrally concerned with the problem of management of risk and the promotion of adaptation. A decision maker or disaster risk manager reading this study will want to know what scientists can say with certainty or determined levels of certainty, what hopes exist for improvement in the future, but not the whole story as to methods, their limitations, the data base problems etc etc. So personally I would cut this back to its basics and firstly lay out clearly what information users and decision makers need to manage risk and the scale at which it must exist; then simply lay out what of this is available or could be available with high, medium and low levels of certainty and what is simply not available and not likely to be in the near future etc. Once the risk manager has this general statement of well held truths globally, they can then search out the specifics in other studies. This problem of delimiting and understanding the motives and motivations of those we are writing for and what information is relevant to them, is not only a problem of this chapter (although due to the highly specialised nature of the discourse the dissonance between a reader of this study and the scientific discourse presented is greater than in more social chapters), but also of other chapters that present too much detail and not enough evidence and guiding principles and parameters for risk management and adaptation action as such. This is one of the reasons the study is at present 850 pages long whereas it should probably only be 350 at most if it is going to be read by those we want to read it. Unlike specialised studies and interests, someone interested in the theme of disaster and extreme event management need to read all the study in order to grasp the options and opportunities, given the integrated nature of the overall discourse and practice of risk management as such. This is different to being an expert in climate or hydrology and just reading chapter 3 and 4 for example. This is so much different as a study to the regular IPCC evaluations where one can concentrate on reading WG 1, 2 or 3 work on a specialised basis and get by with it. Here this is not likely to be the case and we have to guarantee consistency and harmony, continuity and consolidation of arguments that do allow decision makers and managers to move forward and take advantage of the knowledge we put forward in this study. At present we are far from that I fear, for a number of reasons. In sum I would search to reduce this chapter to not more than 35 focussed pages and put all methodological and detailed info in an annex or elsewhere. (Lavell, Allan, Programme for the Social Study of Risk and Disaster (FLACSO))	Noted. This chapter, along with others, has been written to be consistent with the structure, scope and outline agreed at an earlier IPCC scoping meeting before the author teams were appointed. Thus many decisions were outside the remit of the chapter authors. While the report is clearly important for decision makers and must be policy relevant, there is also a scientific audience. This subsection, and the whole chapter, have been considerably shortened and quite a lot of material is now summarised in a series of tables and maps (Tables 3.1 to 3.3 and accompanying maps). User needs are very diverse - depending on their problem, sector and geographical region etc etc, so within the space limits of this chapter it would not be possible to identify and answer all such specific needs. Many of these issues are, however, picked up in subsequent chapters including the case studies.
887.2	3	29	1	30	25		

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
888	3	29	1	30	62	Section 3.2.3.4. This was a good discussion, but it didn't really touch on risk. Users are often not just interested in the most likely future climate but may have a specific interest in the more extreme changes (to extremes) that some models may predict because of their interest in managing risk. The para starting line 55 on page 29 may be the place to bring this idea in. I think the Dessai ref in the previous para deals with this. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	The discussion on spatial scaling has now been deleted. Thus the para referred to is now part of the previous one citing Dessai et al 2009 and risk assessment is now explicitly mentioned in the context of that reference. Other chapters discuss risk in more detail.
889	3	29	1	0	0	This section appears to contain redundant information and it is not clear how much of this is specific to extremes, for example, lines 36 - 53. (Stocker, Thomas, IPCC WGI TSU)	The section has been considerably shortened with a closer focus on extremes. The text on scaling (lines 36-53) has been deleted and replaced by a single sentence in Section 3.2.3.
890	3	29	3	29	12	Please mention in this paragraph that raw GCM and RCM output in general needs a bias correction to bring the statistical properties of present-day simulations in line with observations. (van Oldenborgh, Geert Jan, KNMI)	Bias correction is now discussed, with two supporting references.
891	3	29	10	0	0	Lack of resolution is not the main concern in hydrological applications. Lack of quality in present day simulations are of greater concern. Significant biases need correction before climate model simulations can be used in a meaningful way in e.g. hydrological modelling applications. (Klein Tank, Albert, KNMI)	Bias correction is now discussed, with two supporting references.
892	3	29	18	29	23	Here specific results are reported which should better appear within section 3.3. (Stocker, Thomas, IPCC WGI TSU)	These have been deleted but are not included in Section 3.3 which now does not include regional details (this information is now in Table 3.3).
893	3	29	18	29	23	This bit about Beniston (2009) seems an odd digression - how does it relate to user needs? (Zwiers, Francis, Environment Canada)	This text has been deleted (see comment 892). It was included as a rare example of 'joint' projections.
894	3	29	21	29	23	What does the word "mode" mean in this context? (van Oldenborgh, Geert Jan, KNMI)	This text has been deleted (see comment 892).
895	3	29	22	29	23	Mentioning of the two locations, Lugano and Copenhagen, is too specific. As written in the reference (Beniston, 2009), the two locations are representatives of the Mediterranean and maritime Climate, respectively. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	This text has been deleted (see comment 892).
896	3	29	22	29	23	Were these the only two sites examined? If so, quote as examples. (Trewin, Blair, Australian Bureau of Meteorology)	This text has been deleted (see comment 892).
897	3	29	25	29	53	It was indicated that precipitation at short time step were well reproduced by RCMs and that high-resolution RCMs simulated improved precipitation comparing with that by GCMs (Sasaki et al., 2005; Wakazuki et al., 2008; Sasaki et al., 2008; Kanada et al., 2010). Sasaki, H., K. Kurihara, I. Takayabu, 2005: Comparison of climate reproducibility between a super-high resolution atmosphere general circulation model and a Meteorological Research Institute regional climate model. SOLA, 1, 81-84, doi:10.2151/SOLA2005-022. Wakazuki, Y., M. Nakamura, S. Kanada, and C. Muroi, 2008: Climatological reproducibility evaluation and future climate projection of extreme precipitation events in the Baiu Season using a High-Resolution Non-Hydrostatic RCM in comparison with an AGCM. Journal of the Meteorological Society of Japan, Vol. 86 (2008), No. 6, 951-967. Sasaki, H., K. Kurihara, I. Takayabu and T. Uchiyama, 2008: Preliminary experiments of reproducing the present climate using the non-hydrostatic regional climate model, SOLA, Vol.4, 25-28. Kanada, S., M. Nakano and T. Kato, 2010: Climatological characteristics of daily precipitation over Japan in the Kakushin regional climate experiments using a non-hydrostatic 5-km-mesh model: Comparison with an outer global 20-km-mesh atmospheric climate model, SOLA, Vol. 6, 117-120. (Kurihara, Kazuo, Meteorological Research Institute)	The sentence on cloud/convection resolving models has been deleted. Two of these references are now cited in section 3.2.3.1 (see comment 798)
898	3	29	25	0	0	"heat exhaustion" may be an example as well as urban drainage. (Nakaegawa, Toshiyuki, Meteorological Research Institute)	Noted. This can largely be captured by considering Tmax and Tmin. Urban drainage is a clearer example.
899	3	29	31	29	32	Here (and a few lines below) the paper by Hay et al. (2006) is referenced to state that higher model resolution does not necessarily improve prediction of precipitation. It should be made clearer whether this is a statement for modeling precipitation on long (climate) time scales. Currently, in the weather prediction community, there are several papers clearly highlighting the benefit of high-resolution models (in particular when going to the convection-resolving scale) for quantitative precipitation forecasts. The report should make clear that this aspect of numerical weather prediction is not discussed in this paragraph. Also, other papers on the (potential) benefit of high-resolution climate modeling (e.g., Hohenegger C, Brockhaus P, Schar C, 2008. Towards climate simulations at cloud-resolving scales. Meteorol. Z., 17, 383-394; Hohenegger C, Brockhaus P, Bretherton CS, et al., 2009. The Soil Moisture-Precipitation Feedback in Simulations with Explicit and Parameterized Convection. J. Climate, 22, 5003-5020) should be discussed in order to get a more balanced discussion of the resolution issue in climate modeling. (Wernli, Heini, ETH Zürich)	This text has been deleted in shortening the section. The two Hohenegger references are cited in section 3.2.3.1
900	3	29	34	0	0	As described earlier in the text, a large number of weather generators have been developed to overcome this lack of information. (Klein Tank, Albert, KNMI)	This is now said here, with Maraun et al 2010 as a supporting reference.
901	3	29	39	29	0	remove 'has the potential to' (Benestad, Rasmus, The Norwegian Meteorological Institute)	Deleted.
902	3	29	42	29	45	Models not only sample areal averages, they also simulate processes with areal averages in mind. Precipitation at a point for instance is formed when the local humidity gets supersaturated. In an area sub-saturation is already enough to form precipitation somewhere in the gridbox. (Van den Hurk, Bart, KNMI)	The text on scaling has been deleted and replaced by a single sentence in Section 3.2.3.
903	3	29	49	29	49	The meaning of the 0.66 value needs more explanation. (Trewin, Blair, Australian Bureau of Meteorology)	The text on scaling has been deleted and replaced by a single sentence in Section 3.2.3.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
904	3	29	49	0	0	"exceeding" -> "even less than" (Van den Hurk, Bart, KNMI)	The text on scaling has been deleted and replaced by a single sentence in Section 3.2.3.
905	3	29	56	29	56	"...should no be assumed..." I do not understand this context. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Text has been reworded to clarify the context.
906	3	29	59	29	59	I don't understand what it means for some aspect of a model to be insufficiently constrained by observational data. Naive readers will imagine that climate models must continuously ingest observations; others might understand that this as relating to the specification of parameters, land surface properties etc. Some clarification is required. (Zwiers, Francis, Environment Canada)	This sentence has been deleted.
907	3	30	1	30	1	The time horizon relevant for re-insurers is much shorter since the contracts with the direct insurers have durations of 1-3 years. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Noted. Text has been slightly modified. The point being made is that 20 years is long for them.
908	3	30	2	30	2	Would be better just to say "the 2011-2040 period" - the current wording looks contradictory. (Trewin, Blair, Australian Bureau of Meteorology)	Agreed. Text changed.
909	3	30	3	30	4	Sentence "The focus of this chapter is on what the IPCC defines as long-term projections out to the end of the century -- as distinct from near-term seasonal-to-decadal predictions" -- I don't think "IPCC defines" anywhere where exactly the (fuzzy) boundary between near-term vs long-term lies -- in fact, e.g., WGI avoided to do this so far throughout AR5. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Deleted 'what the IPCC defines as'.
910	3	30	4	30	4	On p12, L29, the wording is "seasonal-to-interannual predictions"; this should be consistent in the text. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Disagree. Seasonal-to-interannual and seasonal-to-decadal are two different things. This text has, however, been deleted in order to shorten and better focus this section.
911	3	30	9	30	9	The EU project ENSEMBLES already included a co-ordinated decadal hindcast experiment, the first results of which are now becoming available. (van Oldenborgh, Geert Jan, KNMI)	This text has been deleted in order to shorten and better focus this section.
912	3	30	11	30	11	Should this be "starting in 1850"? (Trewin, Blair, Australian Bureau of Meteorology)	This text has been deleted in order to shorten and better focus this section.
913	3	30	11	30	25	Some more detail on what RCM runs are available for regions other than Europe and NA would be useful. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	This text has been deleted in order to shorten and better focus this section.
914	3	30	11	30	25	Projections for near future (2026-2035) were conducted with AOGCMs and RCMs (Japanese domain) (Kitoh et al., 2009). Kitoh, A., T. Ose, K. Kurihara, S. Kusunoki, M. Sugi and KAKUSHIN Team-3 Modeling Group, 2009: Projection of changes in future weather extremes using super-high-resolution global and regional atmospheric models in the KAKUSHIN Program: Results of preliminary experiments. Hydrological Research Letters, 3, 49-53. (Kurihara, Kazuo, Meteorological Research Institute)	This text has been deleted in order to shorten and better focus this section.
915	3	30	13	30	13	Usually, the RCMs are less computationally expensive. (Bojariu, Roxana, National Meteorological Administration)	This text has been deleted in order to shorten and better focus this section.
917	3	30	37	0	0	Focusing on the direction of change rather than on the magnitude of change is a wise decision. This deserves more emphasis in the text. (Klein Tank, Albert, KNMI)	Noted. Table 3.3 does not now include any information about the magnitude of change.
918	3	30	39	0	0	See also the recent IAC recommendations for the treatment of uncertainty information. (Klein Tank, Albert, KNMI)	Noted. The new IPCC guidance notes on consistent treatment of uncertainties now used in the assessment are consistent with these recommendations.
920	3	30	54	30	60	Figure 3.3 and Figure 3.4 should provide time period (from ?? year to ? ?year) relative to ?? year to ?? year. Scenarios are SRES?? (Zhao, Zong-Ci, National Climate Center)	This is clarified in Section 3.2.3.5 text and in the Table foot note.
921	3	31	0	36	0	The following text and table from a paper in review may be useful to help with adding some examples from Africa to Sections 3.3.1 and 3.3.2 (NOTE: Please refer to supporting material for the text and table) (Conway, Declan, University of East Anglia)	Noted. Many thanks.
922	3	31	1	0	0	Clark 2006, Barnett 2006 and Clark et al 2010 all provide regional estimates of future changes in daily temperature extremes and both Clark papers have heatwave projections too so should be included in the table. Barnett 2006 provide regional estimates of future changes in daily precipitation extremes and should be included in the table, Burke and Brown 2008 provide regional estimates of future drought and should be included in the table. UKCP (see comment 2) provides probabilistic predictions for the UK and should be included in the Europe section. David N. Barnett & Simon J. Brown & James M. Murphy David M. H. Sexton & Mark J. Webb, Quantifying uncertainty in changes in extreme event frequency in response to doubled CO2 using a large ensemble of GCM simulations, Climate Dynamics (2006) 26: 489-501, DOI 10.1007/s00382-005-0097-1 Robin T. Clark, James M. Murphy, and Simon J. Brown, Do global warming targets limit heatwave risk? GEOPHYSICAL RESEARCH LETTERS, VOL. 37, L17703, doi:10.1029/2010GL043898, 2010 (Brown, Simon, The Met Office Hadley Centre)	These references have been added to Table 3.3.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
923	3	31	6	45	46	In Japan, we are on the stage of verifying the credibility of the dynamically downscaled data. For temperature extremes, we are on the way writing papers related with the increase of the heat wave around the mega cities in Japan, by using sophisticated urban canopy models with the horizontal resolution of 3 - 4 km. Unfortunately, we still have no paper already published in the international Journal. On the precipitation extremes, we have checked the representation of the heavy precipitation events by comparing the very high resolution regional climate model's simulation with the simulation by using medium resolution regional climate model, and found that very high resolution model up to 2km is useful to represent the heavy rain fall around Japan in the summer monsoon season (Kanada et al., 2008). For wind extremes, we have rresearch related with the typhoon hit Japan Islands (Maruyama et al., 2010). This paper compared the model produced wind data with the observation, in very high interval (every model timestep), and found that the model could hardly represent the gust of wind directly. This suggests that combining with the statistical downscaling method will produce a better projection results on gust-winds. Ref: (1) Knada, S., M. Nakano, S. Hayashi, T. Kato, M. Nakamura, K. Kurihara and A. Ktoh, 2008. Reproducibility of Maximum Daily Precipitation Amount over Japan by a High-resolution Non-hydrostatic Model. SOLA, Vol. 4, 105-108, doi:10.2151/sola.2008-027. (2) Maruyama, T., E. Tomokyo and J. Maeda, 2010. Simulation of Strong Wind Field by Non-hydrostatic Mesoscale Model and Its Applicability for Wind Hazard Assessment of Buildings and HOuses. Hydrological Research Letters, 4, 40-44, doi: 10.3178/HRL.4.40. (Takavabu, Izuru, Meteorological Research Institute)	Thank you but there is no peer reviewed paper available for examination yet and its not clear when it would be published.
925	3	31	16	31	16	A reference should be added e.g. Luterbacher, J., E. Xoplaki, C. Casty, H. Wanner, A. Pauling, M. Kuttel, T. Rutishauser, S. Bronnimann, E. Fischer, D. Fleitmann, F.J. Gonzalez-Rouco, R. Garcia-Herrera, M. Barriendos, F. Rodrigo, J.C. Gonzalez-Hidalgo, M.A. Saz, L. Gimeno, P. Ribera, M. Brunet, H. Paeth, N. Rambu, T. Felis, J. Jacobeit, A. Dunkeloh, E. Zorita, J. Guiot, M. Turkes, M.J. Alcoforado, R. Trigo, D. Wheeler, S. Tett, M.E. Mann, R. Touchan, D.T. Shindell, S. Silenzi, P. Montagna, D. Camuffo, A. Mariotti, T. Nanni, M. Brunetti, M. Maugeri, C. Zerefos, S. De Zolt, P. Lionello, "Mediterranean Climate Variability Over the Last Centuries: A Review", Chapter 1, in: The Mediterranean Climate: an overview of the main characteristics and issues, (Eds. P. Lionello, P. Malanotte-Rizzoli and R. Boscolo), Elsevier, 27-148, 2006. (Zerefos, Christos, Academy of Athens)	Thank you for the suggestion, but this is before the AR4 and its not clear it is peer-reviewed, plus it doesn't appear to add anything in context of extremes.
926	3	31	16	31	17	It is not clear whether the terms "warm" and "cold" refer to the absolute values (i.e., warm days in summer, cold days in winter) or to the relative values (i.e., warm days include relatively warm days in winter, cold days include relatively cold days in summer). Please clarify this in this paragraph. (van Oldenborgh, Geert Jan, KNMI)	This is a general discussion on issues such as data, so the definition is left general here, but defined later.
927	3	31	16	31	17	It is important as mentioned to distinguish between the daily, monthly or annual temperature data and of course to distinguish between mean, maximum or minimum data. It is also important to distinguish the type or method of the data collection for significant conclusions of time series. (Ammann, Walter J., Global Risk Forum GRF Davos)	Where possible this is done, but generally the reader would have to go back to the cited literature for more information on data observing methods. Too much detail for this assessment.
928	3	31	21	31	0	The issue of homogenisation has been discussed before, and this paragraph repeats earlier text. (Benestad, Rasmus, The Norwegian Meteorological Institute)	This paragraph is removed due to repetition with earlier section and AR4.
929	3	31	21	31	32	Briefly write about homogenizing the observed input data of the models. (Davtalab, Rahman, Ministry of Energy)	Thank you for the suggestion, but we decline as too much detail for this report. I think this kind of explanations are available in statistical methods books and are not necessary to insert in this report.
930	3	31	23	31	23	Suggest adding "substantial" or "widespread" before "attention" (the first paper in the field appeared in 1996) (Trewin, Blair, Australian Bureau of Meteorology)	This paragraph is removed due to repetition with earlier section and AR4.
931	3	31	24	0	0	Please add the following two references after 'promise': Kuglitsch et al. (2009) and Toreti et al. (2010a): (Luterbacher, Juerg, Justus Liebig University)	This paragraph is removed due to repetition with earlier section and AR4.
932	3	31	26	3	27	"there is not yet a global data set of adjusted daily temperature data as there is with monthly data" I am not sure a global dataset of homogenised monthly data even exists, at least I have been unable to locate one that collects homogenisation efforts of separate countries. (van Oldenborgh, Geert Jan, KNMI)	GHCN-Monthly and the HadCRU datasets both have homogenized data.
933	3	31	34	31	41	from personal observation in my desert house -out of Cairo- I found that the difference in temperature between day and night for this summer was around 4 degrees C., which is not the usual figure that was around 10-12 oC. So, we might consider the difference in temperature between day and night. (Yasseen, Adel, Ain Shams University - Institute of Environmental Research and Studies)	Thanks, but it is not clear what the comment is requesting.
934	3	31	36	33	39	Possibly relevant papers on recent temperature variability/extremes in Africa Aguilar E, Barry AA, Brunet M, Ekang L, Fernandes A, Massoukina M, Mbah J, Mhanda A, do Nascimento DJ, Peterson TC, Thamba Umba O, Tomou M, Zhang X. Changes in temperature and precipitation extremes in western central Africa, Guinea Conakry, and Zimbabwe, 1955–2006. Journal of Geophysical Research 2009, 114(D2). doi: 10.1029/2008JD011010 King'uyu, S. M., L. A. Ogallo, and E. K. Anyamba (2000) 'Recent Trends of Minimum and Maximum Surface Temperatures over Eastern Africa'. Journal of Climate 13, 2876–2886. New, M, Hewitson, B, Stephenson, DB, Tsigas, A, Kruger, A, Manhique, A, Gomez, B, Coelho, CAS, Masisi, DN, Kululanga, E, Mbambalala, E, Adesina, F, Saleh, H, Kanyanga, J, Adosi, J, Bulane, L, Fortunata, L, Mdoka, ML, Lajoie, R. (2006) Evidence of trends in daily climate extremes over southern and west Africa. Journal of Geophysical Research-Atmospheres 111, D14. (Conway, Declan, University of East Anglia)	Thank you for the comment, these have been added.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
935	3	31	36	33	40	Why the used references in this pages limit to a few area mostly Europe and north America. Of course you used some of them in tables, but you should mention here if you describe the other article . For example a article relaties to middles east by Xuebin Zhang, et al, 2006. (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	Thank you for the suggestion, but this was included in the AR4 and is referenced using Trenberth et al. 2007.
936	3	31	37	31	37	"The following paragraphs provide a summary of the main results of this assessment (AR4)" -- indeed there is a lot of repetition from AR4, but this should be substantially shortened and reduced to the key conclusions. (Stocker, Thomas, IPCC WGI TSU)	Agree, we have reduced dependence on the AR4 except more as a starting point.
937	3	31	40	31	41	I suggest removing the bits in parentheses. (Zwiers, Francis, Environment Canada)	Agreed.
938	3	31	43	31	45	The authors might have a view on whether the available global temperature record provides evidence of acceleration or whether the more appropriate intpretation is a relatively constant rate of change that is modulated by low frequency internal variability. (Zwiers, Francis, Environment Canada)	Thank you but this is beyond the scope of this report, esp. since the global temperature has leveled out over the past 5 or so years and would require much more text to discuss properly.
939	3	31	47	0	0	insert "boreal" before "winter" (Van den Hurk, Bart, KNMI)	Not necessary (we have mentioned Northern Hemisphere at the end)
940	3	31	51	31	51	Does this mean a 70-75% reduction or that information was available over 70-75% of global land regions? I suspect the latter. (Trewin, Blair, Australian Bureau of Meteorology)	Thank you, it is a reduction over 70-75% of the land area where data are available.
941	3	31	54	0	0	Please add a reference to Christidis N. " Detection of changes in temperature extremes during the second half of the 20th century", GRL, 32:20, 2005 (Zerefos, Christos, Academy of Athens)	Agreed.
942	3	31	62	32	2	I don't recall what data set was used, but coverage would not have included all global land areas. Formulations like "in most areas except Australia" implicitly invite readers to generalize to areas where there is no data (because the latter are not part of the area excluded by the statement). (Zwiers, Francis, Environment Canada)	Text modified to clarify this is area analysed, not all area.
943	3	31	62	32	2	This is imprecise and confusing language. The daily minimum temperatures have increased in most areas except western Australia etc. Do you mean all areas except ? (Stott, Peter, Met Office)	Text modified to clarify this is area analysed, not all area.
944	3	31	0	42	0	I have sent in separately a file with bits of text and a table from a paper in review which may be useful in adding some African examples in these sections. Filename 'IPCC SREX review notes-Chapter 3 Declan Conway' (Conway, Declan, University of East Anglia)	Thank you for the material.
946	3	32	8	32	16	Does this include assessments of published results from ETCCDI and APN workshops on extremes? (Zwiers, Francis, Environment Canada)	The APN results will be included in box on islands and are referenced by the Griffiths ref, results from ETCCDI are indices etc. that have been used throughout much of the literature.
947	3	32	17	32	32	This paragraph is confusing. Please clarify so that the non-expert reader will understand the relevance of DTR and 'dimming' to the points being made. (Solomon, Susan, NOAA)	This paragraph was removed (see comment 951).
948	3	32	18	32	22	How are cold and warm nights / cold and warm days defined? (Koppe, Christina, Deutscher Wetterdienst)	Defined in prior text.
949	3	32	24	32	0	A similar analysis analysis of record-events, but for monthly temperature anomalies from 17 stations scattered around the globe, indicated that he recurrence of record-high values were recurring at a greater rate than expected if the PDF were changing (Benestad, R.E. (2004) Record-values, non-stationarity tests and extreme value distributions Global and Planetary Change vol 44, issue 1-4, p.11-26). Reference to these studies is perhaps more appropriate on p. 33 L 8-22. The Meehl et al (2009a) study refers to ideas based on Vogel et al (2001; Water resource res.) and the iid-test proposed by Benestad (2003), but this is not explained here in the report. (Benestad, Rasmus, The Norwegian Meteorological Institute)	The Meehl study does not reference Vogel, and thus the reference does not add anything to the discuss. We need more justification for adding material.
950	3	32	25	32	25	There is a new paper here which is relevant - suggest adding after '1 to 1', 'while broadly similar results were obtained for Australia by Trewin and Vermont (2010)'. The reference is Trewin BC and Vermont H (2010), Changes in the frequency of record temperatures in Australia, 1957-2009, Aust. Met. Oceanogr. J., 60, 113-119. (Trewin, Blair, Australian Bureau of Meteorology)	agreed.
951	3	32	25	32	32	Is there a link between DTR and extremes? It's not immediately obvious to me that there should be one, so I suspect that this bit could be dropped. (Zwiers, Francis, Environment Canada)	The only relevant part of max/min/dtr discussion is consistency of changes in extremes with changes in max/min temps, so this is dropped.
953	3	32	30	0	0	Please add the reference Zerefos, C.S., K. Eleftheratos, C. Meleti, S. Kazadzis, A. Romanou, C. Ichoku, G. Tselioudis, A. Bais, "Solar dimming and brightening over Thessaloniki, Greece, and Beijing, China", Tellus B, DOI: 10.1111/j.1600-0889.2009.00425.x. 2009. (Zerefos, Christos, Academv of Athens)	This paragraph is removed (see comment 951).
954	3	32	34	32	44	Brown et al 2008 specifically looked at both the warm and cold tails of Tmax and Tmin for the Caesar 2006 dataset. The results of which should be referenced here. (Brown, Simon, The Met Office Hadly Centre)	added reference to Brown.
955	3	32	54	33	6	Somewhere here it could be noted that the development of indices which satisfactorily represent multi-day indices is a significant gap in the current literature. There is a conference paper on this (Trewin, B.C. 2009. A new index for monitoring changes in heatwaves and extended cold spells. 9th International Conference on Southern Hemisphere Meteorology and Oceanography, Melbourne, 9-13 February 2009) but this is unlikely to be submitted as a paper before the IPCC deadline. (Trewin, Blair, Australian Bureau of Meteorology)	Thank you for the information but unless the peer-reviewed vesion gets published in time we cannot use it.
956	3	32	56	0	0	Does the value of 0.4 refer to one standard deviation? (Klein Tank, Albert, KNMI)	No, actual temperature.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
957	3	32	58	32	61	It's not clear from the text how maximum heat wave length is defined - is this a moving window type of calculation over a multi-year window (eg., a decade)? Given that I am guessing that it is an extreme of annual extremes, I'm wondering about the robustness of the doubling estimate. I have the same kind of question about "maximum summer variability" - how is this defined, and is the statistic robust? (Zwiers, Francis, Environment Canada)	heat waves defined in glossary but here is number of consecutive days above the 95th percentile for each day, maximum summer variability references to the variance of daily maximum temperature.
958	3	32	60	32	0	Temperature trend is given in percentage – 6% - but this is inappropriate, as it assumes deg C (?) rather than absolute values (K). (Benestad, Rasmus, The Norwegian Meteorological Institute)	This is what was provided in the paper.
959	3	32	62	32	62	PDFs don't have variances, variables do. Also, it would be preferable to talk about probability distributions rather than PDFs. (Zwiers, Francis, Environment Canada)	modified to remove term "PDFs".
960	3	32	63	33	2	discussion of the effects of soil moisture should be moved to the causes section 3.3.1.2 (Brown, Simon, The Met Office Hadly Centre)	agree sentence removed).
961	3	33	8	33	21	Also mention recent events in Russia? Mention in the text that Luterbacher et al (2004) is based on instrumental data (I think ... I don't have access to the paper here as I write this) in order to distinguish their result from those from paleo reconstructions that are discussed in the next paragraph. (Zwiers, Francis, Environment Canada)	Luterbacher could not have been based solely on instrumental data since a truly useful and (more or less) accurate mercury thermometer was not invented until the early 1700s. This is based on multi-proxy reconstructions.
962	3	33	8	33	21	I am confused here. As far as I could understand, the previous paragraphs were all concerned with cold and warm days relative to the climatology of the season, whereas here without any warning the emphasis shifts to absolute high temperatures. These effects are not directly comparable. For instance, relative to the climatology the European autumn of 2006 was more extreme than the summer of 2003 (van Oldenborgh, Clim.Past, doi:10.5194/cp-3-659-2007), but in terms of impacts the summer was more extreme. Different mechanisms also play a role between warm winter days and warm summer days. This distinction should be made more clear. (van Oldenborgh, Geert Jan, KNMI)	The discussion shifts from cold and warm days/nights to heat waves, and clearly states that so additional explanation is unnecessary.
963	3	33	8	33	21	I think that two more events should be mentioned in this list. First the summer 2007 heat waves in South Eastern Europe, when there was also an anomalous number of fires in Greece (Tolika et al, 2007 already in the references of this chapter). This event is also mentioned later in the chapter. By now, I also think it should be mentioned the summer 2010 in Russia, with great impacts on local agriculture and population health. I think it should be noticed that after 2000 the frequency of heat waves with relevant impacts somewhere in continental Europe has been very high. (Pavan, Valentina, ARPA Emilia-Romagna)	The European heat wave discussion is provided for context since a number of papers have been written on it, other more recent heat waves are discussed later.
964	3	33	8	33	21	You can include the Russian July 2010 heatwave here and it would also be an opportunity to discuss that extremes are often related meteorologically as was the case in 2010 with the meteorological connections between the Russian heatwave and the Pakistan floods as well as the Chinese landslides. (Stott, Peter, Met Office)	Russia event has not been documented in peer reviewed papers yet like Pakistan flood.
965	3	33	8	33	28	Nothing is said about warm winters. The European 2006/2007 winter broke a warm record, with an amplitude comparable to the one of the summer 2003 heatwave. The causes seem to be related to favourable atmospheric circulation and warmer than usual sea-surface temperatures (Cattiaux et al., 2009, 2010). Cattiaux J, Vautard R, Yiou P (2009) Origins of the extremely warm European fall of 2006. Geophys. Res. Lett. 36:doi:10.1029/2009GL037339; Cattiaux J, Vautard R, Yiou P (2010) North-Atlantic SST amplified recent wintertime European land temperature extremes and trends Clim. Dyn.:DOI:10.1007/s00382-00010-00869-00380 (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	Seasonal "extremes" are not discussed. Some detail of seasonal extremes have been added.
966	3	33	12	33	12	Suggest, for the Australian event, citing Bureau of Meteorology (2009), The exceptional January-February 2009 heatwave in southern Australia, Special Climate Statement 17, available at http://www.bom.gov.au/climate/current/special-statements.shtml . (Trewin, Blair, Australian Bureau of Meteorology)	This publication is not peer-reviewed. But will consider if reference is needed.
967	3	33	18	33	28	The heat wave that struck Greece in 2007 should be mentioned in connection to the paper by Founda and Giannakopoulos (2009) cited in the bibliography. Also in lines 23-28 the reference to Luterbacher et al (see above page 31, line 16) could be added (Zerefos, Christos, Academy of Athens)	Agreed.
968	3	33	23	33	28	The use of paleoclimatic evidence is not very consistent in the chapter. This part could be extended, as extremes have always been a focus of paleoclimate research and are well covered by documentary data. Paleoclimate and proxies are mentioned here (but too briefly), even more briefly in the wind chapter (p. 43, 1 58 and following) and in the flood section (p. 65, 1.9 and following). This may be more of a general comment (Brönnimann, Stefan, University of Bern)	Added more paleo data through chapter, where feasible.
969	3	33	23	33	28	This para can be deleted. It is too specific and not of relevance in this context as it deals with mean summer reconstructions in central Europe rather than heat wave variability/heat wave reconstructions. (Luterbacher, Juerg, Justus Liebig University)	Agreed, paragraph removed and some information incorporated into previous paragraph.
971	3	33	25	33	27	It is not clear from the text what instrumental period is being compared with the reconstructed record. If I assume a 100-year instrumental record, then the expected number of exceedances above 2 standard deviations (assuming annual statistics) is about 2.5. The text mentions 2, which on the face of it, is not unexpected, so this begs the question of how to interpret those two events (Zwiers, Francis, Environment Canada)	see comment 969.
972	3	33	27	0	0	Not ERROR - should be std dev or uncertainty. (Stocker, Thomas, IPCC WGI TSU)	see comment 969.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
973	3	33	33	33	33	A recent study by Portmann et al. in PNAS (2009) sheds new light on the southeast US anomaly and merits a reference and a sentence or so of discussion here. In brief, that paper shows that the longitudinal changes in extreme temperature trends going from east to west across north america are strongly linked to precipitation. This should be mentioned. You may also want to mention that this in turn may be linked to aerosol forcing as discussed in Portmann et al. (Solomon, Susan, NOAA)	Added.
974	3	33	40	34	45	3.3.1.2 Good section (IPCC WGII TSU)	Thanks
975	3	33	42	33	52	Rowell DP, Jones RG, Causes and uncertainty of future summer drying over Europe, Climate Dynamics Volume 27, Numbers 2-3, 281-299, DOI: 10.1007/s00382-006-0125-9 should be referenced in this paragraph (Brown, Simon, The Met Office Hadly Centre)	reference added.
976	3	33	44	0	0	In the context of D/A one could replace "caused by" by "associated with" . (Klein Tank, Albert, KNMI)	Rejected. "Caused by" and "associated with" are very different in the context of D/A.
977	3	33	47	33	47	Replace "impacts on" with "reductions in" (presumably the impact is to reduce evapourative cooling). (Zwiers, Francis, Environment Canada)	Agreed. Text modified.
978	3	33	51	0	0	Please add the reference Ordóñez, C., N. Elguindi, O. Stein, V. Huijnen, J. Flemming, A. Inness, H. Flentje, E. Katragkou, P. Moinat, V-H. Peuch, A. Segers, V. Thouret, G. Athier, M. van Weele, C. S. Zerefos, J-P. Cammas, M. G. Schultz, "Global model simulations of air pollution during the 2003 European heat wave", Atmos. Chem. Phys., 10, 789–815, 2010. (Zerefos, Christos, Academy of Athens)	reference added.
979	3	33	54	33	59	Are these assessments based on modelling? formal D&A studies? If yes, please indicate. (Stocker, Thomas, IPCC WGI TSU)	These are AR4 assessment. It is now clear in the revised text.
980	3	33	54	33	59	The only specific detection of human influence on daily extreme temperatures in Hergle et al 2007 was Christidis 2005 so why not quote that reference? (Brown, Simon, The Met Office Hadly Centre)	Christidis 2005 is cited now.
981	3	33	59	33	59	Is Alexander et al (2006) the correct reference? From memory they only looked at observations (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	it was in a wrong place, it has been corrected now.
982	3	33	61	34	0	Does the optimal detection assume Gaussian distribution (i.e. based on regression) – if it does, then it is likely to result in a biased fit and bias in the significance estimation because the extremes are most probably not Gaussian. (Benestad, Rasmus, The Norwegian Meteorological Institute)	optimal detection does assume Gaussian distribution. The method has been applied to averages of extreme tempertaure and thus the assumption of Gaussian distribution is likely still valid.
983	3	33	62	33	63	This sentence is very confusing and should be reformulated (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	text modified.
984	3	33	62	0	0	Which method do you consider in the "optimal detection method"? If the author doesn't want to introduce the method, please eliminate it. (Davtalab, Rahman, Ministry of Energy)	"optimal detection method" has a specific meaning in which s/n ratio is optimized. We agree that there is no need to mention "optimal method" as such. the related words have been rmeoved.
985	3	33	62	0	0	Unknown what an "optimal detection method" is (Van den Hurk, Bart, KNMI)	deleted in the revised text.
986	3	34	1	34	2	Shortening the quotation changes the statement of the reference. Hence, include ", while human influence was not detected in the warmest day" after the part "...and night from 1950-1900". (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	text modified.
987	3	34	4	34	18	Should mention Stott et al (2004). (Zwiers, Francis, Environment Canada)	Stott et al. (2004) was cited earlier. Here we discuss post-AR4 studies.
988	3	34	4	34	18	Alexander & Arblaster (2009) found that trends in 'warm nights' over Australia could only be captured by a coupled model that included anthropogenic forcings (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	reference added.
989	3	34	4	34	4	Insert "and attribution" after "Detection". (Zwiers, Francis, Environment Canada)	text modified.
990	3	34	9	0	0	I don't understand why the word "However" is used. The LUC could also be anthropogenic. (Klein Tank, Albert, KNMI)	"However" removed
991	3	34	12	34	18	If human influence of growing season length is to be discussed then the following should be referenced Christidis, Nikolaos, Peter A. Stott, Simon Brown, David J. Karoly, John Caesar, 2007: Human Contribution to the Lengthening of the Growing Season during 1950–99. J. Climate, 20, 5441–5454. (Brown, Simon, The Met Office Hadly Centre)	reference added.
992	3	34	20	34	32	suggest to not use the abbreviations ANT, ALL, GEV etc. in the main text. They chould of course be used in the Figure/caption if needed. (Stocker, Thomas, IPCC WGI TSU)	abbreviations are removed.
993	3	34	20	34	32	This paragraph is based on submitted paper. It should be checked before publication of this report if in final version of the paper (if it will be published) there is no change regarding these statsments ! (Wibig, Joanna, University of Lodz)	the paper is published now.
994	3	34	25	34	25	Replace "all the extreme temperature" with "all four extreme temperature" (Zwiers, Francis, Environment Canada)	text modified.
995	3	34	29	34	32	I think you should caveat this since these estimated changes in waiting times remain quite uncertain. (Zwiers, Francis, Environment Canada)	additional words added reflecting uncertainty.
996	3	34	29	34	32	This language is difficult to understand. It is suggested to use a construct similar to that one on page 35, lines 23 to 27. This would also help the reader to compare results. (Radunsky, Klaus, Umweltbundesamt GmbH)	text modified.
997	3	34	30	0	0	"extreme" -> "extremely low" (Van den Hurk, Bart, KNMI)	rejected, "extreme" has a specific meaning here.
998	3	34	43	34	0	Paragraph can be dropped. Does it provide any new information? (Benestad, Rasmus, The Norwegian Meteorological Institute)	paragraph dropped.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
999	3	34	47	35	37	Coverage of regional changes as projected in Africa, Asia, South-America missing (Stocker, Thomas, IPCC WGI TSU)	These are included in table 3.3
1000	3	34	47	36	37	3.3.1.3 For most of the likelihood statements, it would be difficult to construct a traceable account of the statement's origin. In particular, it is often not clear whether the likelihood is a product of the IPCC assessment or taken from other papers. It is also not clear when likelihoods have been downgraded from the output of the formal analysis, based on qualitative considerations. (IPCC WGII TSU)	Not quite sure what is meant here. The first likelihood statements are straight from the AR4, others from assessments like CCSP 3.3. And the last statement about downgrading is confusing.
1001	3	34	47	36	37	3.3.1.3 SREX does not need to extensive repeat information that is in the AR5. It can efficiently cite and update. (IPCC WGII TSU)	Assume meant AR4, we are working to reduce redundancy with the AR4.
1003	3	34	49	0	58	Unnecessary repetition of reference to AR4 (Meehl et al.) at the beginning of the last two paragraphs; merge corresponding sentences. (Stocker, Thomas, IPCC WGI TSU)	Agree and we have merged the text.
1004	3	34	51	0	0	Note that in several regions (including Europe) the observations in recent decades show an increase in DTR. (Klein Tank, Albert, KNMI)	Thanks but this is the projections section.
1005	3	34	54	0	0	It would be helpful if the authors could also mention that frosts, though less frequent but later in the year might lead to significant damages to agriculture, phenology, etc (Luterbacher, Juerg, Justus Liebig University)	This is the domain of Chapter 4.
1006	3	34	57	34	57	According to the definition in Section 1.1.3.1., the term "risk" is determined by the convolution of hazard and vulnerability factors. To be consistent, "risk" should be replaced here, e.g. by "probability". (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Agree, replaced.
1007	3	34	57	0	0	Please replace word 'risk'. (Stocker, Thomas, IPCC WGI TSU)	Agree, replaced.
1008	3	34	58	0	0	Please add Beniston (2004) before 'Meehl and Tebaldi' (Luterbacher, Juerg, Justus Liebig University)	Added
1009	3	34	0	0	0	In 'Fig. 3.6', it is questionable to represent and refer the results from un-accepted article. (NISHIMORI, Motoki, National Institute for Agri-Environmental Sciences)	Understand, but the paper should be accepted by the due date.
1010	3	35	3	35	3	Clark 2006 and Clark et al 2010 should also be referenced (Brown, Simon, The Met Office Hadly Centre)	Thanks, but to add references we need the entire reference.
1011	3	35	6	35	6	Use the word likelihood rather than risk (which has a particular meaning in the context of this report). Also, discuss the paper that is the basis for this assessment (Stott et al, 2004) in 3.3.1.2, and cross-link to that discussion. (Zwiers, Francis, Environment Canada)	Replaced.
1012	3	35	6	35	6	same as above (term "risk") (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Replaced.
1013	3	35	7	35	7	Can the region discussed here be defined more precisely? 2003 was arguably more a central European event than a southern one. (Trewin, Blair, Australian Bureau of Meteorology)	July 2003 was warm all over Europe, not just central Europe.
1014	3	35	7	0	0	After "warm or warmer" please add "during" 50% (Zerefos, Christos, Academy of Athens)	Declined, that would change the meaning.
1015	3	35	9	35	0	After the discussion regarding the uncertainties of regional climate description in GCMs, and the necessity for downscaling, it may be an idea to shorten the discussion about projections relying directly on GCMs, and use this more as a background for what the downscaling indicates. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Unclear what the comment is requesting.
1016	3	35	9	35	10	Again, I'm not sure that DTR is relevant. (Zwiers, Francis, Environment Canada)	Agree, removed.
1017	3	35	9	35	15	Note that the definition of cold outbreaks in this paragraph will be affected by the frequency distribution of winter temperatures, which is strongly skewed in some areas (e.g. NE Europe). Also, is the 100% figure real? (Trewin, Blair, Australian Bureau of Meteorology)	This is implied in the text.
1018	3	35	9	35	15	This is one of a number of examples where the chapter would benefit from a better elucidation of the mechanism and an assessment of its robustness. Why and how does the circulation change with increasing greenhouse gases and why does this affect cold air outbreaks in the regions specified rather than other regions? (Stott, Peter, Met Office)	This is beyond the scope of this chapter, need to reduce text.
1019	3	35	11	35	15	I would have thought that a cold air outbreak would have a synoptic definition (related to air mass properties and circulation) rather than a statistical definition. (Zwiers, Francis, Environment Canada)	yes, but the statistical definition is necessary to analyze long-term changes.
1020	3	35	14	35	15	Worth also stating where the largest reductions will be. (Trewin, Blair, Australian Bureau of Meteorology)	Thank you.
1021	3	35	17	35	27	check the usage of "likely" here; does it refer to a proper assessment of the uncertainty, then clarify this by italics, (Stocker, Thomas, IPCC WGI TSU)	These are assessments made by the authors of CCSP 3.3 using the same evaluation criteria as IPCC. This is now indicated in the text.
1022	3	35	17	35	27	A good place to present the findings of UKCP (See comment 2) (Brown, Simon, The Met Office Hadly Centre)	UKCP is not peer-reviewed, decline.
1024	3	35	19	35	20	Does this assessment agree with CCSP 3.3? (Zwiers, Francis, Environment Canada)	Yes, this is directly from CCSP 3.3
1025	3	35	19	0	0	Please provide for which period (Luterbacher, Juerg, Justus Liebig University)	added.
1026	3	35	19	0	0	The period of study should be stressed (Zerefos, Christos, Academy of Athens)	added.
1027	3	35	22	35	22	What's the definition of "much"? (Trewin, Blair, Australian Bureau of Meteorology)	much is defined as "to a great degree or extent".
1029	3	35	23	35	27	It should be "Climate models indicate that some of currently rare extreme event..." Some because there are extreme events that become less frequent (cold extremes for instance), references are necessary for an example mentioned. (Wibig, Joanna, University of Lodz)	These all come from CCSP 3.3 and are indicated as such.
1030	3	35	27	0	0	I think a little discussion on Europe should be added here, maybe also for Asia (Zerefos, Christos, Academy of Athens)	Discussed in next paragraphs.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1031	3	35	29	35	34	Add Watterson et al. 2008, Changes in extreme temperatures of Australasian summer simulated by CCAM under global warming, and the roles of winds and land-sea contrasts, Aust. Met. Mag. 57, 195-212 (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Added.
1032	3	35	29	35	34	Another relevant study is Perkins et al (2009) which also showed some benefit in model filtering. Perkins, S.E,1 A. J. Pitman,1 and S. A. Sisson (2009) Smaller projected increases in 20-year temperature returns over Australia in skill-selected climate models GEOPHYSICAL RESEARCH LETTERS, VOL. 36, L06710 (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Added
1033	3	35	29	35	48	Is there any literature from other parts of the world - eg, from Asia based on Japanese time slice runs, or North America based on NARCCAP or other simulations? (Zwiers, Francis, Environment Canada)	Have attempted to provide as much geographical coverage as possible (also see coverage in tables).
1034	3	35	39	35	39	Check the Greek record - seems too low to me. (Trewin, Blair, Australian Bureau of Meteorology)	Were checked."last January was the second warmest since 1936 while the top temperature this summer – 44.8 Celsius (112.6 Fahrenheit) – is the highest ever recorded in Greece."
1038	3	35	53	0	0	should it be 1961 instead of 1960? (Luterbacher, Juerg, Justus Liebig University)	yes, corrected.
1039	3	35	53	0	0	reference period should be probably 1961-1990. (Wibig, Joanna, University of Lodz)	yes, corrected.
1040	3	35	55	35	39	Clark 2006 and Clark et al 2010 show that the incorrect simulation of the control climate could lead to either an over estimate or an underestimate of the correct change signal. If the present day climate is simulated as dry when it should be wet and would become dry then the drying out amplification of the warming will not be present in the climate simulations as the region is already (incorrectly) dry and thus the change is underestimated and vs. a vs. if the control is wet when it should be dry (Brown, Simon, The Met Office Hadly Centre)	Thanks, but to add references we need the entire reference.
1041	3	35	56	0	0	Consider replacing "now established" by "further evidence suggests". (Klein Tank, Albert, KNMI)	modified, thanks.
1042	3	35	59	35	59	is pertinent here re modeling parameters which drive uncertainty in changes in extreme temperature (Brown, Simon, The Met Office Hadly Centre)	Not clear what comment is suggesting, appears to be incomplete.
1043	3	35	61	35	61	Please add Shongwe et al (Mon.Wea.Rev. 2007, doi:10.1175/2007MWR2094.1) who show the effect of snow cover on cold extremes in spring in Eastern Europe. (van Oldenborgh, Geert Jan, KNMI)	Thanks but seasons are not covered in the SREX. Will consider adding seasonal extremes.
1044	3	35	61	35	61	Rowell 2006 should be cited on the driving mechanisms for summer drying over Europe, Rowell DP, Jones RG,Causes and uncertainty of future summer drying over Europe, Climate Dynamics Volume 27, Numbers 2-3, 281-299, DOI: 10.1007/s00382-006-0125-9 (Brown, Simon, The Met Office Hadly Centre)	Already sufficient references.
1046	3	36	6	36	16	Alexander & Arblaster (2009) note that the heat wave index shown in this figure is 'statistically volatile'. Could a different index for extreme temperatures be shown here rather than propogate one that the observational community has deemed unfit? (Arblaster, Julie, NCAR: Australian Bureau of Meteorology)	Revised text on heat waves, to stress volatility of indices.
1047	3	36	6	0	0	Note the patter of heatwave change is different in Clarke 2006 Fig 8, a result of doubling CO2 in a perturbed physics ensemble. (Brown, Simon, The Met Office Hadly Centre)	Thanks
1048	3	36	8	36	14	Nevertheless, with supported methodologies in the regional analysis of frequencies they can go aboard such challenges, such as proposes it Hosking and Wallis (1997. Regional frequency analysis. An aproach based on L-Moments. New York: Cambridge University Press. 224 p.) (Lamprea Quiroga, Pedro Simon, Ideam - Advisor (Colombian institute of hydrology , meteorology and environmental studies))	Not clear what comment is suggesting, appears to be incomplete.
1049	3	36	19	36	28	Can something be said about the sign of these influences and impacts - positive or negative? (Stocker, Thomas, IPCC WGI TSU)	Comment is not clear.
1050	3	36	19	36	28	Please also mention the local effects of aerosol concenbrations, which lower day-time temperatures by scattering sunlight. The effects of aerosols are not well represented in the AR4 models (eg Ruckstuhl and Norris, GRL, 2009, doi:10.1029/2008JD011066 for Europe, Dwyer, Norris, Ruckstuhl, JGR, 2009, doi:1029/2009JD012945 for Japoan and China). (van Oldenborgh, Geert Jan, KNMI)	Added
1051	3	36	22	36	23	Again, it is not quite how observations would constrain models. (Zwiers, Francis, Environment Canada)	Comment is not clear.
1053	3	36	30	36	0	If ensemble simulations ar said to be 'validated', and the observed heat wave intensities are larger than worse-case projections, then the models are not validated, but shown to be deficient. This is also discussed in van der Oldenborgh et al, 2009. Climate of the Past. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Paragraph deleted
1054	3	36	30	36	33	Worst case projections for the current decade? This is an example where it would be useful for the authors to assess the result. I would be surprised if model projections were sensitive to intial conditions, for example. It is also not clear from the text what is uncertain, and how the uncertainty bounds were obtained. (Zwiers, Francis, Environment Canada)	Paragraph deleted
1055	3	36	31	36	31	Projections from when, for when? (Trewin, Blair, Australian Bureau of Meteorology)	Paragraph deleted
1056	3	36	32	36	32	It is not clear to me whether "initial conditions" means sampling internal unforced natural variability or whether it mean the state of the control climate. If it is the latter then Clark 2006 and Clark et al 2010 would stroyly disagree with Ganguly as the control simulation of soil moisture is crucial. (Brown, Simon, The Met Office Hadly Centre)	Paragraph deleted

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1057	3	36	35	36	37	Please add a statement that the magnitude of the trend is highly uncertain, both because of the spread in the multi-model ensembles and because observed trends in heat waves are larger than the modelled one in many areas. (van Oldenborgh, Geert Jan, KNMI)	Disagree. Spread is not so large to require this caveat.
1058	3	36	35	0	0	Please use the word "possible" instead of "virtual certain". Also between lines 40 and 50 I suggest a repetition of my comment for page 33 lines 23-28 with the addition of the reference by Luterbacher et al. This will give the flavour of the interannual variability of extreme cases in precipitation as well. (Zerefos, Christos, Academy of Athens)	Decline, virtually certain is an assessment using the likelihood lexicon
1059	3	36	39	0	0	The 'precipitation section' reads more like a literature review than an assessment. Some attention is needed to mould this section into a more effective assessment, which might also allow a reduction in the length of this section. (Stocker, Thomas, IPCC WGI TSU)	The section has been restructured significantly.
1060	3	36	39	0	0	As noted in the general comments, the paragraph structure here should give a clear separation between seasonal and annual changes in precipitation. For many readers, seasonal changes will be as important, or more important than annual changes. Currently, sentences relating to seasonal observations and projections are mixed in with others, and a clearer separation would be useful. (Stocker, Thomas, IPCC WGI TSU)	Text restructured to reflect this. (Note that seasonal and annual changes have NOT been separated.)
1061	3	36	39	0	0	Section 3.3.2 on "precipitation": No information from paleo records is used here to support the assessment (Stocker, Thomas, IPCC WGI TSU)	There is few paleo records related to the extreme precipitation except for Europe (Pauling and Paeth 2007), which is added to SOD.
1062	3	36	39	0	0	Section 3.3.2.: The term "precipitation" includes several species such as rain (stratiform, convective), hail, snow, or sleet. Hence, a clear definition about what is considered here is necessary. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Note that we do not distinguish between rain and snowfall (both considered as contributors to overall extreme precipitation events), but do distinguish changes in hail from other precipitation types. This has been made clear in SOD
1063	3	36	39	0	0	Section 3.3.2 This section appears unstructured. It mostly reads as a re-collection of a list of studies for different regions of the world, without a clear storyline. The contradicting results from different studies are not discussed, which often makes the text inconsistent. Problems of data homogeneity and better observing systems are not mentioned. Most of the included citations indicate changes in precipitation extremes in the last decades. Studies that indicate no changes in precipitation or hydrological extremes are not included. One example is the well know, large scale, study by Andreadis and Lettenmaier, which report no changes in hydrological extremes in the 20th century across the contiguous US (Andreadis, K. M., and D. P. Lettenmaier (2006), Trends in 20th century drought over the continental United States, Geophys. Res. Lett., 33, L10403, doi:10.1029/2006GL025711.). (von Storch, Hans, GKSS Research Center)	Restructured as suggested. Note that these are now addressed in various parts of Ch3 (e.g. data issues in section 3.2, and flood and drought in other sections).
1064	3	36	39	0	0	Section 3.3.2 Comment: In relation to the increased frequency and / or intensity of extreme events, besides the difficulty of numerical models of climate prediction to reproduce such events, highlighted in this report, mainly because of their low spatial resolution, we highlight the difficulty in measuring such events in a spatially homogeneous way. In our company in Brazil (the National Electric System Operator - ONS) we receive daily information about rainfall accumulated in 24 hours over 200 rain gauge stations, which are analyzed weekly for its consistency. Occasionally, some precipitation observed values could be considered as being associated with extreme precipitation events, and which, actually, are measurement errors, typically caused by acquisition of data by telemetry. In order to achieve this conclusion, several tests are usually required of other meteorological informations, such as satellite imagery and radar, which can only be performed by an experienced team of meteorologists. I think that both in Brazil and elsewhere in the world this type of analysis is often not possible. Thus, it is necessary to consider what extreme events or intense precipitation really mean, and what are measurement errors. It is notable that these measurement errors became more frequent after the installation of telemetry stations, which occurred in Brazil, mainly from the 80s. (Rocha, Vinicius, Operador Nacional do Sistema Elétrico)	Data issue discussed in section 3.2
1065	3	36	41	36	0	It is difficult to provide a single definition of "extreme temperature" and "extreme wind" too. (Benestad, Rasmus, The Norwegian Meteorological Institute)	rejected.
1066	3	36	41	36	44	It would be better to say that there are two ways to define extreme precipitation, one based on relative thresholds and the other on absolute thresholds. The first one can be based either on percentiles or on return values, both of them being essentially the same since a return period is another form of expressing an exceedance probability. (López-Díaz, José Antonio, Agencia Estatal de Meteorología (Spain))	agreed, text modified.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1067	3	36	41	38	52	Possibly relevant papers on recent precipitation variability/extremes in Africa (New et al. is probably the most detailed and spatially extensive study) Aguilar E, Barry AA, Brunet M, Ekan L, Fernandes A, Massoukina M, Mbah J, Mhanda A, do Nascimento DJ, Peterson TC, Thamba Umba O, Tomou M, Zhang X. (2009) Changes in temperature and precipitation extremes in western central Africa, Guinea Conakry, and Zimbabwe, 1955–2006. Journal of Geophysical Research 114(D2). doi: 10.1029/2008JD011010 Camberlin, P, Moron, V, Okoola, R, Philippon, N, Gitau, W (2009) Components of rainy seasons' variability in Equatorial East Africa: onset, cessation, rainfall frequency and intensity. Theoretical and Applied Climatology 98, 237-249. Kniveton, DR, Layberry, R, Williams, CJR, Peck, M (2009) Trends in the start of the wet season over Africa. International Journal of Climatology 29, 1216-1225. Kruger, AC (2006) Observed trends in daily precipitation indices in South Africa: 1910-2004. International Journal of Climatology 26, 2275-2285. Nel W (2009) Rainfall trends in the KwaZulu-Natal Drakensberg region of South Africa during the twentieth century. International Journal of Climatology 29, 1634-1641. Nel, W (2008) Observations on daily rainfall events in the KwaZulu-Natal Drakensberg. Water South Africa 34, 271-274. New, M, Hewitson, B, Stephenson, DB, Tsiga, A, Kruger, A, Manhique, A, Gomez, B, Coelho, CAS, Masisi, DN, Kululanga, E, Mbambalala, E, Adesina, F, Saleh, H, Kanyanga, J, Adosi, J, Bulane, L, Fortunata, L, Mdoka, ML, Lajoie, R. (2006) Evidence of trends in daily climate extremes over southern and west Africa. Journal of Geophysical Research-Atmospheres 111, D14. Seleshi, Y, Camberlin, P (2006) Recent changes in dry spell and extreme rainfall events in Ethiopia. Theoretical and Applied Climatology 83, 181-191. (Conway, Declan, University of East Anglia)	relevant additional references added
1068	3	36	41	0	53	what about changes in snowfall? it is not clear where snowfall is considered within this assessment. (Stocker, Thomas, IPCC WGI TSU)	We do not distinguish between snow and liquid precipitation here, which is made clear now in SOD.
1069	3	36	42	0	38	same as temperature for precipitation, Why the used references in this pages limit to a few area mostly Europe and north America. Of course you used some of them in tables, but you should mention here if you describe the other article . For example a article relates to middle east by Xuebin Zhang, et al, 2006. (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	The suggested reference has been added.
1070	3	36	43	0	0	The definitions reflect the rarity of the event and the type of impact one is interested in. (Klein Tank, Albert, KNMI)	Thanks
1071	3	36	47	36	0	Different units – on purpose? (Benestad, Rasmus, The Norwegian Meteorological Institute)	No, modified
1072	3	36	47	36	47	Inches/day - mm/day Please stick to one unit, probably mm/day. (Stocker, Thomas, IPCC WGI TSU)	Modified as suggested
1073	3	36	47	0	0	2 "inches"/day of rain in the US' - not appropriate in the international report. (NISHIMORI, Motoki, National Institute for Agricultural Environmental Sciences)	Modified as suggested
1074	3	36	47	0	0	I guess this reference to inches is unavoidable, but it is unfortunate. A conversion mm perhaps should be given (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Modified as suggested
1075	3	36	51	36	51	Often engineers are also interested in extreme accumulations over periods shorter than one day, such as hourly. (Zwiers, Francis, Environment Canada)	Modified
1076	3	36	51	36	52	As already written in Section 3.1.3. (p 14), return values can be estimated either from annual extremes or from peaks over a defined threshold. Therefore, I suggest changing the sentence into "...estimated from annual or absolute (peaks over threshold) maximum one day..." (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Modified as suggested
1077	3	36	53	0	0	Return values may change over time indeed. The point here is to assess if they do. (Klein Tank, Albert, KNMI)	yes.
1078	3	36	55	36	60	Indirect approach such as in Dal Genio et al 2007 - GRL - VOL. 34, L16703, doi:10.1029/2007GL030525 have to be used to detect changes in the environment that produces severe weather. (Silva Dias, Maria Assuncao, University of Sao Paulo)	agreed, but not covered.
1079	3	36	58	36	58	There must be data for other parts of the world as well. (Zwiers, Francis, Environment Canada)	agreed, text modified.
1080	3	36	58	36	58	Some specific cost examples could be given here (e.g. for the Sydney 1999 or Melbourne or Perth 2010 events) (Trewin, Blair, Australian Bureau of Meteorology)	Not used due to space limitation.
1081	3	36	0	0	0	Section 3.3.2 Precipitation - should explicitly and separately address the question of changes in winter precipitation extremes, especially heavy snowfall, sleet, and ice storms. These can create major disruptions, at least in the US, and there is much general confusion about how climate change could lead to more heavy snowfall in the short term. Three relevant references for the US include: (1) Changnon, S.A., 2007. Catastrophic winter storms: An escalating problem. Climatic Change 84(2): 131-139. (2) Changnon, S.A., D. Changnon, and T.R. Karl, 2006. Temporal and Spatial Characteristics of Snowstorms in the Contiguous United States. Journal of Applied Meteorology and Climatology 45: 1,141-1,155. (3) Kunkel, K.E., et al., 2009. Trends in Twentieth-Century U.S. Extreme Snowfall Seasons. Journal of Climate 22: 6,204-6,216. (Staudt, Amanda, National Wildlife Federation)	Agreed, relevant literatures added when available.
1082	3	37	1	37	14	Considering hail: it should be mentioned (i) that hail is not a diagnostic variable in most of the numerical models and (ii) that hail is not captured accurately by current observation systems (conventional gauges and/or radar). (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Agreed, text modified.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1083	3	37	1	0	0	I suggest to insert "(e.g., Kim et al., 2010)" before the first sentence is closed. The English reference to be added is "Kim, S., E. Nakakita, Y. Tachikawa and K. Takara, 2010: Precipitation changes in Japan under the A1B climate change scenario, Annual Journal of Hydraulic Engineering, JSCE, vol. 54, pp, 127-132." (Nakakita, Eiichi, Kyoto University)	rejected, there is no need for a reference here.
1084	3	37	6	37	7	It should be noted that in the Mediterranean and Central Europe, observed summer daily mean temperature trends have risen roughly twice as fast as the modelled ones over the last 60 years (van Oldenborgh et al, 2009, doi:10.5194/cp-5-1-2009), a discrepancy of three standard deviations in some areas. As long as the cause of this discrepancy is not determined it is difficult to assess the probability of heat waves in the future. (van Oldenborgh, Geert Jan, KNMI)	Rejected, not relevant
1085	3	37	16	38	52	3.3.2.1 Summary paragraph is dense and confusing (IPCC WGII TSU)	agreed, text refined
1086	3	37	16	0	0	Section 3.3.2.1.: In the whole section, declarations of (i) temporal basis of precipitation amounts (hourly, daily, multi-daily), (ii) statistical significance and (iii) definitions of the term "extreme precipitation" is missing. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	noted, and addressed where possible
1088	3	37	42	37	63	Extreme precipitation changes were also analysed for Poland (Łupikasza E., 2010, Spatial and temporal variability of extreme precipitation in Poland in the period 1951-2006, International Journal of Climatology, 30:991-1007). the highest 5-day precipitation total, precipitation total from events ≥90th and 95th percentiles as well as number of days with precipitation ≥90th and 95th percentiles of daily precipitation amount. Trends in extreme precipitation indices were analysed over semi-annual periods as well as over the standard climatological seasons. Trends were calculated for each of the 30-year moving periods within 1951-2006 using a simple linear regression method. Their significance was tested with the Mann-Kendall method. Decreasing trends dominate, but since 1970s the increasing tendency has appeared, but still there is a lot of stations with decreasing trends. (Wibig, Joanna, University of Lodz)	Reference added.
1089	3	37	42	37	63	Results that can be added for Belgium are: "For Belgium, 10-minutes extreme precipitation quantiles of the past 15 years are for the winter period (DJF: December-January-February) 25% higher in comparison with the same quantiles based on the full-period of 108 years observations since 1898 at Uccle, Brussels (Ntegeka and Willems, 2008)" (Ntegeka, V., Willems, P. (2008). Trends and multidecadal oscillations in rainfall extremes, based on a more than 100 years time series of 10 minutes rainfall intensities at Uccle, Belgium, Water Resources Research, 44, W07402, doi:10.1029/2007WR006471) (Willems, Patrick, Katholieke Universiteit Leuven)	Reference added.
1090	3	37	44	37	44	About precipitation trends in the Alps, and their interpretation I recommend to add the reference to the paper by 1) Auer, I., Böhm, R., Jurkovic, A., Lipa, W., Orlik, A., Potzmann, R., Schoener, W., Ungersboeck, M., Matulla, C., Briffa, K., Jones, P., Efthymiadis, D., Brunetti, M., Nanni, T., Maugeri, M., Mercalli, L., Mestre, O., Moisselin, J.-M., Begert, M., Mueller-Westermeier, G., Kveton, V., Bochnicek, O., Stastny, P., Lapin, M., Szalai, S., Szentimrey, T., Cegnar, T., Dolinar, M., Gajic-Capka, M., Zaninovic, K., Majstorovic, Z., and Nieplova, E. (2007) HISTALP – historical instrumental climatological surface time series of the Greater Alpine Region, International Journal of Climatology, 27, 17–46. and by 2) Brunetti, M., Lentini, G., Maugeri, M., Nanni, T., Auer, I., Böhm, R. and Schöner, W. (2009) Climate variability and change in the Greater Alpine Region over the last two centuries based on multi-variable analysis, International Journal of Climatology, doi:10.1002/joc.1857 (Ranzi, Roberto, University of Brescia)	Rejected, as these two references are irrelevant to changes in extreme precipitation.
1091	3	37	44	0	0	Please add the following reference after Rodda et al. 2009: Toreti et al. 2010b: Toreti, A., Kuglitsch, F.G., Xoplaki, E., Maraun, D., Wanner, H., and Luterbacher, J., 2010b: Characterisation of extreme winter precipitation in Mediterranean coastal sites and associated anomalous atmospheric circulation patterns. Nat. Hazards Earth Syst. Sci., 10, 1037-1050 (Luterbacher, Juerg, Justus Liebig University)	Reference added.
1092	3	37	45	37	46	This sentence says that "trends ... have increased significantly". Does that mean that there has been acceleration? (Zwiers, Francis, Environment Canada)	No; text modified.
1093	3	37	46	37	47	precipitation totals' - what about extremes? (Trewin, Blair, Australian Bureau of Meteorology)	The sentence is deleted.
1095	3	37	49	37	52	Changes in extreme rainfalls concern frequency, intensity or both? (Wibig, Joanna, University of Lodz)	original text has been modified, comment not relevant anymore.
1096	3	37	57	37	59	Please rephrase the sentence in the following way: 'In contrast, in Emilia-Romagna, a region of Northern Italy, the frequency of intense to extreme events decreases during winter over the central mountains, but increases during summer over the plain of the Po River basin, while the number of rainy days decreases in summer during 1951–2004 (Pavan et al., 2008).'	original text has been modified, comment not relevant anymore.
1097	3	37	62	0	0	What is the concept of "wetness indices"? Please introduce the indices. (Davlatab, Rahman, Ministry of Energy)	original text has been modified, comment not relevant anymore.
1098	3	37	63	0	0	Please add the following text: Toreti et al. (2010b) applied a declustered Peak Over Threshold model to a set of 20 series of daily precipitation (1950-2006) recorded at coastal stations in the Mediterranean basin. They identified a significant negative tendency in the occurrence of extreme events (i.e. daily events above a station dependent threshold identified with an objective procedure) during the extended winter season (October-March) in the series of Amiantos (Cyprus), Marseille (France), Rhodes and Argostoli (Greece), Genoa and Teramo (Italy). Please add those information also in Table 3.2 (Luterbacher, Juerg, Justus Liebig University)	reference added.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1099	3	38	1	0	0	Nothing is said there on the Mediterranean droughts of the past and particularly the most recent 1989-90 water shortage and this year crisis that stroke eastern Mediterranean. A more specific discussion on the Sahelian long lasting drought should also be added. (Zerefos, Christos, Academy of Athens)	rejected. Out of the scope of this section.
1101	3	38	10	38	29	Increase of heavy precipitation has also been found for Japan, as reported in the following paper: Fujibe, F., N. Yamazaki and K. Kobayashi, 2006: Long-term changes of heavy precipitation and dry weather in Japan (1901-2004). J. Meteorol. Soc. Jpn., 84, 1033-1046. (Fujibe, Fumiaki, Meteorological Research Institute, JMA)	reference added.
1102	3	38	31	38	45	Another reference regarding past trends of hailstorms is: Piani, F., A. Crisci, G. DeChiara, G. Maracchi, and F. Meneguzzo, 2005: Recent trends and climatic perspectives of hailstorms frequency and intensity in Tuscany and Central Italy. Natural Hazard and Earth System Sciences, 5, 217–224. Trends in environmental conditions: DeRubertis, D., 2005: Recent trends in four common stability indices derived from U.S. radiosonde observations. Journal of Climate, 19, 309-323. Hail-day occurrences are in a 100-yr period are examined by Changnon and Changnon (2000): Changnon, S. A., and D. Changnon, 2000: Long-Term Fluctuations in Hail Incidences in the United States. Journal of Climate, 13, 658–664 (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	references added.
1103	3	38	33	38	33	Change into "The environmental conditions are typically taken from reanalysis or from radiosonde data." (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Text modified as suggested
1104	3	38	37	38	37	Change into "...that both hail damage days and convective instability, for example the convective available potential energy (CAPE), increased during...". According to the study of Kunz (2007) for a test region in Germany, several convective parameters are directly related to hailstorm occurrence in a long-term perspective. Reference: Kunz, M., 2007: The skill of convective parameters and indices to predict isolated and severe thunderstorms. Natural Hazards and Earth System Sciences, 7, 327-342. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Text modified as suggested
1105	3	38	43	38	45	Cao (2008) was also mentioned at lines 37 and 38. (Zwiers, Francis, Environment Canada)	Text refined
1106	3	38	44	38	44	Delete the first "...changes in..." ("...in association with atmospheric changes in convective...") (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Text modified as suggested
1107	3	38	47	38	52	Should a likelihood statement be added to this summary? It should match the information given in Table 3.1. (Stocker, Thomas, IPCC WGI TSU)	Agreed; text modified as suggested.
1108	3	38	47	38	52	The fact that the tables have no entries for many regions should be noted here in the text. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Noted, and addressed where possible
1109	3	38	47	38	52	I think a fair summation of these results is that more locations/studies show an increase in extreme precipitation than a decrease, but there are wide regional and seasonal variations, and many results are not statistically significant. (Trewin, Blair, Australian Bureau of Meteorology)	Agreed, text modified.
1110	3	38	48	38	53	I am not certain how the AR4 conclusion has been further supported. Results still seem very patchy regionally (as evident in the table). I think the problem is the qualification 'in many regions'. This is vague. Is the support from new studies added more regions, or just further supported known regions? (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Yes, but this what was in the AR4 assessment
1112	3	38	56	39	53	Section 3.3.2.2. I think the issue of whether there is an apparent relationship between observed mean rainfall changes and observed extreme rainfall changes should be considered in this discussion if possible. Increases in rainfall where drying has occurred is especially interesting (or indeed the reverse). (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Agreed, addressed where possible
1113	3	38	61	38	61	Please mention a few other variables that can easily cause changes in extreme precipitatin besides global warming: local land use changes (including urbanisation), aerosol concentrations. (van Oldenborgh, Geert Jan, KNMI)	Not relevant
1114	3	38	0	39	0	You might want to add reference here to recent papers on rainfall in the Sahel and areas of recovery; L'Hôte Y, Mahe G, Some B, Triboulet JP. Analysis of a Sahelian annual rainfall index updated from 1896 to 2000; the drought still goes on. Hydrological Sciences Journal 2002, 47(4):563-572. Dai AD, Lamb PJ, Trenberth KE, Hulme M, Jones PD, Xie P. The recent Sahel drought is real. International Journal of Climatology 2004, 24(11):1323-1331. Nicholson SE. On the question of the "recovery" of the rains in the West African Sahel. Journal of Arid Environments 2005, 63:615–641. Ali A, Lebel T. The Sahelian standardized rainfall index revisited. International Journal of Climatology 2009, 29:1705-1714. (Conway, Declan, University of East Anglia)	These should be addressed in drought section, not relevant here.
1115	3	39	4	39	0	In this paragraph, it would also be natural to mention that the shape of the wet-day PDF for 24-hr precipitation in Europe exhibits a systematic relationship with mean temperature and precipitation levels, and a projected increase in the extreme values is due to a general warming (Benestad, R.E.(2007) 'Novel Methods for Inferring Future Changes in Extreme Rainfall over Northern Europe Climate Research, CR34:195-210, doi: 10.3354/cr00693; Benestad, R.E. (2010) 'Downscaling Precipitation Extremes: Correction of Analog Models through PDF Predictions', Theor. & Appl. Clim, Volume 100, Issue 1, DOI: 10.1007/s00704-009-0158-1.) These projections were based on weighted ensemble mean of 23 temperature scenarios and 21 precipitation scenarios from CMIP3 GCMs. Alternatively, this would also fit in the paragraph L18-27 on p. 41. (Benestad, Rasmus, The Norwegian Meteorological Institute)	These are now referenced in the subsection of projected change.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1116	3	39	7	39	8	Suggest deleting the sentence that begins with "Additionally, one observational study ...". (Zwiers, Francis, Environment Canada)	Deleted as suggested.
1117	3	39	11	39	11	Only valid for daily extreme precipitation, hourly values scale much stronger than Clausius-Clapeyron (Lenderink & van Meijgaard 2008, since then verified for other European stations (Lenderink, Atm. Env. Lett., 2010) and a subtropical station (Lenderink et al. in preparation). (van Oldenborgh, Geert Jan, KNMI)	Added
1118	3	39	11	39	13	The reference by Pali et al 2007 seems to indicated that moisture content is associated to extreme precipitation except where circulation changes are seen. It seems to me that this is a quite irrelevant statement since what really matters for extreme precipitation is circulation. As an average, for the globe, yes, more moisture content is associated with a warmer climate. Other than that, regional extremes of precipitation are related to circulation anomalies, persistence of large scale patterns and special locations where topography and winds are particularly associated. Horizontal moisture advection, and an unstabilizing mechanism, matters more that vertically integrated moisture content. (Silva Dias, Maria Assuncao, University of Sao Paulo)	Disagree. In regions where circulation changes, circulation plays a major role in extreme precipitation, but this does not mean moisture change is not important anymore
1119	3	39	11	39	13	This holds true only if evaporation enhances in the same way. Is this the case also over the continents, where the moisture supplement on the surface is limited? (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	This might be the case in inland far away from oceans.
1120	3	39	14	39	19	Here and elsewhere, try to avoid long run-on sentences. (Zwiers, Francis, Environment Canada)	modified.
1121	3	39	17	39	17	This dependence of mean and extreme precipitation on changes in circulation were also studied for Europe in van Ulden and van Oldenborgh (Atm. Chem. Phys., 2005, doi:1680-7324/acp/2006-6-863). (van Oldenborgh, Geert Jan, KNMI)	Rejected, not relevant
1123	3	39	31	39	32	Same comment as for page 21: It should be commented that the conclusion of Lenderink and van Meijgaard (2008) is under debate: see Haerter and Berg, 2009 and Lenderink and van Meijgaard, 2009 (Haerter, J.O., Berg, P., 2009. Unexpected rise in extreme precipitation caused by a shift in rain type? Nature Geoscience, 2, 372-373; Lenderink, G., van Meijgaard, E., 2009. Reply to: Unexpected rise in extreme precipitation caused by a shift in rain type? Nature Geoscience, 2, 373) (Willems, Patrick, Katholieke Universiteit Leuven)	Noted, text modified
1124	3	39	32	39	32	Replace "assumed" with "expected". (Zwiers, Francis, Environment Canada)	Done.
1125	3	39	34	39	34	It is suggested to delete the qualifier "perfect" because it is unclear what the criteria have been used for such qualification. (Radunsky, Klaus, Umweltbundesamt GmbH)	perfect' deleted.
1126	3	39	34	39	46	This discussion completely neglects to mention that climate models so far have been unable to quantify the trend in mean precipitation. For instance, the observed increase in winter precipitation at high northern latitudes is about two times faster in the observations than in the models (Zhang et al, Nature, 2007, doi:10.1038/nature06025, Behnd and von Storch, Clim. Dyn, 2008, 10.1007/s00382-007-0335-9). An attribution can only be succesful if the model simulates the change well. This seems to be the main reason attribution is difficult, not he poor signal/noise ratio or the other factors mentioned. (van Oldenborgh, Geert Jan, KNMI)	noted, but the text already contains such information
1127	3	39	35	39	37	Limited spatial coverage of station data, over land only and with large voids, is also an important limiting factor. (Zwiers, Francis, Environment Canada)	noted, but this is part of uncertainty in the observation
1128	3	39	39	0	0	The following result shows a consistent result even on a monthly time-scale: T. Nakaegawa 2010: Characteristics of the Largest Recorded Annual Maximum Monthly recipitation in an Atmospheric Global Climate Model Experiment. J. Japan Soc. Hydrol. And Water Resour. 23(5), 312-322. (Nakaegawa, Toshiyuki, Meteorological Research Institute)	Noted. But space is limited
1129	3	39	42	39	42	include "and orographic precipitation" after "convective storms" (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Done.
1130	3	39	42	39	43	Sentence "It may be still a decade away" -- this reads like a purely speculative statement. Suggest to make it more concrete or to delete it. (Stocker, Thomas, IPCC WGI TSU)	Done.
1131	3	39	42	39	43	Indicate that this is a result from a single study by starting the sentence with "One study has suggested that ... (Zwiers, Francis, Environment Canada)	Done.
1132	3	39	50	39	50	I'm not sure this conclusion can be justified globally; it would, in my view, be a better reflection of the science is the words "especially in middle and higher latitudes of the Northern Hemisphere" were added after "regions" (Trewin, Blair, Australian Bureau of Meteorology)	Text modified as suggested
1134	3	39	50	39	52	Your assessment that "...there is still not enough evidence to make a more confident assessment..." is inconsistent with the Executive Summary statement that "new studies since AR4 have 'substantially strengthened' the likelihood...". If the strength is substantial, then consider if it is appropriate to strengthen the likelihood statment to "likely"? See comment for p2, ln39-42. (Stocker, Thomas, IPCC WGI TSU)	ES modified, text here is also modified.
1135	3	39	52	0	0	A warmer environment should lead to more melting of hail aloft, limiting the amount reaching the surface. This may well be countered by more moisture in the air. The point is that there can be counter-processes at play. (Stewart, Ronald, University of Manitoba)	not relevant.
1136	3	39	55	39	55	There is a lot of information in this subsection, but its organization is not particularly apparent to this tired reader. (Zwiers, Francis, Environment Canada)	The subsection is restructured.
1137	3	39	55	0	0	Section 3.3.2.3.: In the whole section, I miss declarations of (i) levels of significance of the change signals, and (ii) time basis of precipitation extremes. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Noted, but the assessment cannot provide such details.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1138	3	39	55	0	0	section 3.3.2.2 A section devoted to compare simulations and observations, and thus validate models, would be quite helpful here. This aspect is not considered at all (von Storch, Hans, GKSS Research Center)	Agreed, but no relevant literature to assess.
1139	3	39	58	0	0	"heavy fall" should be "heavy rainfall." In addition, both precipitation and rainfall are used in a sentence. Either of them should be used. (Nakaegawa, Toshiyuki, Meteorological Research Institute)	Done.
1140	3	39	0	39	0	P 39: will readers understand "Perfect model studies" (IPCC WGII TSU)	perfect' deleted.
1141	3	40	2	0	0	I suggest to insert "(e.g., Kim et al., 2010)" before the last sentence is closed. The English reference to be added is "Kim, S., E. Nakakita, Y. Tachikawa and K. Takara, 2010: Precipitation changes in Japan under the A1B climate change scenario, Annual Journal of Hydraulic Engineering, JSCE, vol. 54, pp, 127-132." (Nakakita, Eiichi, Kyoto University)	Reference added.
1142	3	40	4	40	0	How many independent GCMs were included in this analysis? It would be natural also to discuss the analysis of record-breaking monthly precipitation totals projected by e.g. 31 GCM runs following SRES A1b meission scenario (Benestad, R.E (2006) 'Can we expect more extreme precipitation on the monthly time scale?' J.Clim Vol. 19, No. 4, pages 630-637). (Benestad, Rasmus, The Norwegian Meteorological Institute)	Reference added and text modified.
1143	3	40	4	40	14	It appears from Figure 3.9 that waiting times over land only regions are projected to decrease more than for global regions. Could a discussion of possible causes be added? (MUJUMDAR, PRADEEP, INDIAN INSTITUTE OF SCIENCE)	the difference is so small and is not significant and does not warrant an explanation.
1144	3	40	4	40	14	Note of the results of Barnett et al 2006 should be made in this paragraph. (Brown, Simon, The Met Office Hadly Centre)	Barnett et al. (2006) was assessed in AR4.
1145	3	40	10	40	10	delete "period" to be read as "20-year return values" (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Done.
1147	3	40	17	40	28	Figure 3.8: Do we really want to be reproducing figures from the AR4? Doesn't the work on the SREX provide access to new dimensions of the assessment? (IPCC WGII TSU)	figure 3.8 changed.
1149	3	40	45	0	0	Acronim STR should be added in Southern subtropic description. (fig. 3.9) (Wibig, Joanna, University of Lodz)	Added.
1150	3	40	52	31	9	The results obtained from the MME approach and those of a single model or two approach are equivalently presented, although the likelihood or consistency are different between the approaches. (Nakaegawa, Toshiyuki, Meteorological Research Institute)	Agree
1151	3	40	57	40	57	delete "change", only the mean is compared with observations by Shongwe et al. (van Oldenborgh, Geert Jan, KNMI)	change' deleted.
1153	3	41	11	41	11	Please explain "extreme precipitation indices" (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Text modified and the word 'indices' removed.
1154	3	41	12	41	12	Explain that the model was run in time slice mode. (Zwiers, Francis, Environment Canada)	Text modified as suggested.
1155	3	41	14	41	14	Change to "southern Australia" to avoid confusion with the state of South Australia. (Trewin, Blair, Australian Bureau of Meteorology)	Done.
1156	3	41	18	41	0	High resolution is also important for capturing important cyclone and blocking characteristics. Part of this paragraph is repetition. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Text refined.
1157	3	41	18	41	18	Not just in areas of complex topography - presumably other places as well due to, for example, limitations of convective schemes. (Zwiers, Francis, Environment Canada)	Noted and text modified.
1158	3	41	18	0	0	Even over flat terrain, there can be gradients due to, for example, farming practices that may influence some extremes. (Stewart, Ronald, University of Manitoba)	Noted and text modified.
1159	3	41	18	0	0	I suggest to insert "(e.g., Kim et al., 2010)" before the first sentence is closed. The English reference is to be added is "Kim, S., E. Nakakita, Y. Tachikawa and K. Takara, 2010: Precipitation changes in Japan under the A1B climate change scenario, Annual Journal of Hydraulic Engineering, JSCE, vol. 54, pp, 127-132." (Nakakita, Eiichi, Kyoto University)	Reference added.
1160	3	41	29	30	40	Ishihara(2010) showed increase of extreme precipitation in the future over Kagoshima-prefecture of Japan with ststistical method using results from a 20km-mesh RCM. Ishihara, K., 2010: Assessment for Probable Precipitation Change due to Global Warming using Regional Frequency Analysis, Hydrological Research Letters, 4, 30-34. (Kurihara, Kazuo, Meteorological Research Institute)	Noted; but MRI-RCM20 results have already been assessed.
1161	3	41	29	41	40	The paragraph states in the middle that precipitation extremes will tend to increase in Northern Europe and decrease in Southern Europe. By the end of the paragraph, a 'consistent pattern of change to(intense precipitation events) is robust across all European regions'. This paragraph is inconsistent. (von Storch, Hans, GKSS Research Center)	Text modified to avoid inconsistency.
1162	3	41	33	41	40	Results for the UK from UKCP (See comment 2) should be quoted here (Brown, Simon, The Met Office Hadly Centre)	Rejected. No single-country assessments considered except for US.
1164	3	41	42	41	42	Tomassini and Jacon find positive trends over Germany: of which variable? (von Storch, Hans, GKSS Research Center)	Text modified to clarify this.
1165	3	41	42	41	43	"...find positive trends over Germany": for which variable and for which time periods? (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Text modified to clarify this.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1166	3	41	42	41	53	Simulations with high resolution RCMs projected that frequency of extreme precipitation increases in the warm climate for warm or rainy season in Japan.(Nakamura et al., 2008, Wakazuki et al., 2008, Kitoh et al., 2009) Nakamura, M., S. Kanada, Y. Wakazuki, C. Muroi, A. Hashimoto, T. Kato, A. Noda, M. Yoshizaki and K. Yasunaga, 2008: Effects of Global Warming on Heavy Rainfall during the Baiu Season Projected by a Cloud-system-resolving Model. Journal of Disaster Research, vol.3, No.1, 15-24. Wakazuki,Y., M.Nakamura, S.Kanada, and C.Muroi, 2008: Climatological reproducibility evaluation and future climate projection of extreme precipitation events in the Baiu Season using a High-Resolution Non-Hydrostatic RCM in comparison with an AGCM. Journal of the Meteorological Society of Japan, Vol. 86 (2008), No. 6, 951-967. Kitoh, A., T. Ose, K. Kurihara, S. Kusunoki, M. Sugi and KAKUSHIN Team-3 Modeling Group, 2009: Projection of changes in future weather extremes using super-high-resolution global and regional atmospheric models in the KAKUSHIN Program: Results of preliminary experiments. Hydrological Research Letters, 3, 49-53. (Kurihara, Kazuo, Meteorological Reserach Institute)	References added.
1167	3	41	42	0	53	The following result may serve as reference: "an increase in 90th-percentile values of daily precipitation on the Pacific side of the Japanese Islands during July in the future climate with a 5-km mesh cloud-system resolving non-hydrostatic RCM." (Sachie Kanada, Masuo Nakano and Teruyuki Kato: "Changes in mean atmospheric structures around Japan during July due to global warming in regional climate experiments using a cloud-system resolving model", Hydrological Research Letters, Vol. 4, pp.11-14, (2010)) (Nakaegawa, Toshiyuki, Meteorological Research Institute)	Reference added.
1168	3	41	49	41	49	Correct wording is "Westernport" (Trewin, Blair, Australian Bureau of Meteorology)	Word corrected.
1169	3	41	50	41	53	There must be something wrong here - the text seems to suggest that forcings are only specified in the autumn season. Also, avoid formulations such as "suffer less severe" that imply that extreme precipitation is always a bad thing. Neutral language such as "experience less extreme" would be better. (Zwiers, Francis, Environment Canada)	Text corrected.
1170	3	41	55	41	0	The interesting question is how many different GCM simulations are providing input to these RCMs. If they all are using the same GCM, then they are not independent, and any difference is due to different biases in the different RCM. That only addresses a minor part of the uncertainties associated with the projection of extremes. (Benestad, Rasmus, The Norwegian Meteorological Institute)	2 GCMs, which is clarified in the text.
1171	3	41	59	41	59	Another appropriate study in this context is: Feldmann, H., B. Früh, G. Schädlér, H.-J. Panitz,K. Keuler, D. Jacob, and P. Lorenz, 2008: Evaluation of the Precipitation for South-western Germany from High Resolution Simulations with Regional Climate Models. Meteorologische Zeitschrift, 17, 455-465 (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	This is noted. But space is limited and there is already detailed discussion of European RCM results (see comment 1173).
1172	3	42	1	42	14	UKCP provides a formal breakdown of the various contributions of uncertainty to changes in withertime rainfall. Whilst the figure on page 152 of the report is for seasonal mean rainfall the principle for daily ppt would be similar. (Brown, Simon, The Met Office Hadlv Centre)	OK.
1173	3	42	1	42	53	There is more detailed discussion here of European RCM results than is warranted for one region. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Text refined to reduce the length here.
1174	3	42	5	42	5	which a very small step' going from 100 km resolution to 50 Km resolution. I guess step is not the right word here. But more importantly, if 50 km is considered a fine resolution, the scale mismatch between model results and point observations is not really considered here, and it seems to be very important. How good is the reliability of models to simulate extremes with such a coarse resolution ? (von Storch, Hans, GKSS Research Center)	Wehner et al. (2010) is removed from the text, so that the comment is not relevant now.
1175	3	42	5	0	0	"a fairly small step" is not clear language (Whetton, Penny, CSIRO Marine and Atmospheric Research)	The word 'step' is changed to 'change'.
1176	3	42	9	0	0	"pattern" -> "patterns" (Van den Hurk, Bart, KNMI)	Corrected. Thanks.
1177	3	42	12	42	14	It's not clear what is being reported here - what does "other" refer to in "other large scale changes"? (Zwiers, Francis, Environment Canada)	The sentence is removed (as only one model was used in Kendon et al. 2009; see also comment 1178). Therefore the comment is not relevant now.
1178	3	42	14	0	0	Confident statements on the basis of one model only? (Klein Tank, Albert, KNMI)	The sentence is removed so that the comment is not relevant now.
1179	3	42	16	42	0	Here, the study by Themessl et al (2010) 'Empirical-statistical downscaling and error correction of daily precipitation from regional climate models' Int. J. Clim, DOI: 10.1002/joc.2168 is also relevant. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Reference added.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1180	3	42	31	42	33	Isn't this at least partially suspect? Shouldn't I expect a model, even at 60km, to simulate long return period extremes that are smaller than observed at stations? The only logical interpretation for precipitation in such models is as a water flux (downwards) that is distributed equally across the entire surface of the grid box. It is hard to imagine that this would be comparable, even in 24 hour accumulations, to the quantity that is measured at a rain gauge. Does Mike give a justification for why we should begin to have confidence in the magnitude of simulated precipitation extremes at the 60km scale? You could imagine an argument that has to do with the rate at which convecting features are advected past a rain gauge over a 24 hour period - in effect resulting in a kind of spatial average for some kind of representative area (a swath across space). On the other hand, weather is advected into a 60km box in the model throughout a 24 hour period as well, implying that the width of the swath in time that is represented by a grid box is nevertheless much larger than that of a station. (Zwiers, Francis, Environment Canada)	Accessment of Wehner et al. (2010) is removed from the text, so that the comment is not relevant now.
1181	3	42	38	0	0	It could be helpful to indicate whether this model underestimation is related to the coarse spatial resolution of the models used (Van den Hurk, Bart, KNMI)	Text modified as suggested.
1182	3	42	55	45	46	wind, as an important factor in shaping deserts, our desert in Egypt and other deserts in Asia and Australia will suffer more from extreme wind speeds. Sand dunes will attack and overcome more of roads and desert settlements. Sandy winds will attack more of the western banks of the Nile, causing more desertification. Nothing of the text in this part show the importance of this part of the world. (Yasseen, Adel, Ain Shams University - Institute of Environmental Research and Studies)	the importance of winds in shaping deserts is now mentioned in the introductory paragraph
1183	3	42	55	45	46	Wind included both averaged and extreme wind. In this Report, it should focused on extreme wind. But now, most observed studies investigated the averaged wind changes. It should be mentioned at the beginning of 3.3.3. (Zhao, Zong-Ci, National Climate Center)	this point has been clarified
1184	3	42	55	0	0	Section 3.3.3 on "wind": No information from paleo records is used here to support the assessment (Stocker, Thomas, IPCC WGI TSU)	This has been addressed
1185	3	42	55	0	0	Section 3.3.3 on "wind": Coverage of regional changes as projected in Africa, Asia, South-America is weak or missing (Stocker, Thomas, IPCC WGI TSU)	A new study on trends in China has been added but I was not able to find studies for Africa or South America
1186	3	42	55	0	0	Section 3.3.3.: The measurement/averaging intervals should be specified in the whole section (gusts, 1-min mean, 10-min mean, daily mean, daily maximum...). (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	reference to the measurement/averaging intervals has been given where specified in the cited study
1187	3	42	55	0	0	Section 3.3.3.: I miss a discussion about trends in the frequency / intensity of downbursts and tornadoes. At least a statement is expected that both the current observation networks as well as the models are unable to reliably reproduce convective gusts and, thus, any analysis is subject to high uncertainty. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	a statement to this effect has been added
1188	3	42	57	43	4	It should be mentioned that extreme wind due to winter storms is the cause of more than 50% of total loss from natural hazards in central Europe. A suitable source are publications from Munich Re. I just have two german language one at hand: Munich Re (1999) Naturkatastrophen in Deutschland: Schadenerfahrungen und Schadenpotentiale. Munich Re. www.munichre.com/publications/302-01037_de.pdf, Munich Re (2007) Zwischen Hoch und Tief – Wetterrisiken in Mitteleuropa. Edition Wissen, Munich Re, www.munichre.com/publications/302-05481_de.pdf (Ulbrich, Uwe, Freie Universität Berlin)	The impact of extreme wind on insurance losses is relevant for chapter 4.
1189	3	42	57	45	0	Why not look at analysis of geostrophic wind, gradient wind, and cyclone statistics? Large-scale extreme wind events – which have strongest impact – are often associated with deep low pressure systems. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Some studies that utilise these approaches have been cited where appropriate (e.g. Wang et al 2009)
1190	3	42	57	0	0	wind energy potential can be added give citation Prior et al.,xx. (Incecik, Salahattin/Selahattin, Istanbul Technical University)	Don't understand comment.
1191	3	42	59	42	0	Winds and forest fires – are not the winds affected by the fires themselves, and hence subjected to different conditions than for winds not associated with forest fires? (Benestad, Rasmus, The Norwegian Meteorological Institute)	the connection between forest fires and winds is now mentioned in the introduction
1192	3	42	0	0	0	There is nothing in this section about snow - if there are no relevant studies the report should say so. (Trewin, Blair, Australian Bureau of Meteorology)	Wrong location of comment. Consider including extreme snow seasons/events if literature exists.
1193	3	43	3	43	4	citation of AR4 with almost the same words, though not really understandable for general reader (von Storch, Hans, GKSS Research Center)	These sentences have been removed
1194	3	43	6	43	12	Yes, tropical and extratropical cyclones are phenomena that will produce extremes but they are not the only ones. The Mesoscale Convective Complexes, for example, in the subtropical regions of the several continents are producers of severe weather. Perhaps the literature is lacking a more focused approach to climate change in the MCC case. Here again this report should point that out in the hope that reearchers will look into this matter from the climate change perspective. (Silva Dias, Maria Assuncao, University of Sao Paulo)	MCCs are mentioned but the literature is not sufficient to assess wind changes associated with these phenomena
1195	3	43	14	44	5	Some studies concentrated on the averaged wind, some on the extreme wind. Authors should mention each study carefully. Please do not mention some results by only using WIND. (Zhao, Zong-Ci, National Climate Center)	Studies dealing with mean and extreme winds are now described separately
1196	3	43	16	43	56	This section has many repetitions (Brönnimann, Stefan, University of Bern)	The section has been shortened and repetition eliminated
1197	3	43	20	43	56	paragraphs should be merged, repeated use of words, even sentences (von Storch, Hans, GKSS Research Center)	these paragraphs have now been merged

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1198	3	43	24	43	26	This study "Wan, H., X. L. Wang, and V. R. Swail, 2010: Homogenization and Trend Analysis of Canadian Near-Surface Wind Speeds. J. Clim., 23, 1209-1225. DOI:10.1175/2009JCLI3200.1" is suitable to cite in line 26 on page 43, right after "increasing trends in high latitudes". (Wang, Xiaolan, Environmen Canada)	This study is now cited
1199	3	43	26	43	28	The sentence beginning 'Several studies....' should be deleted. These studies are described in more detail shortly afterwards 'lines 49-56'. (Stocker, Thomas, IPCC WGI TSU)	this section has been restructured and shortened
1200	3	43	29	43	31	Are there references to support this assessment? (Zwiers, Francis, Environment Canada)	this section has been restructured and shortened and the mentioned lines have been deleted
1201	3	43	29	43	31	Please add references to the statements (Luterbacher, Juerg, Justus Liebig University)	this section has been restructured and shortened and the mentioned lines have been deleted
1202	3	43	29	43	36	A recent study based on ERA40 data quantifies storm events over the North Atlantic and Europe through an index, finding an positive trends for the severity of storms during the historic ERA40 period (1960–2000) in this area. Leckebusch, G.C., D. Renggli, U. Ulbrich, 2008: Development and Application of an Objective Storm Severity Measure for the Northeast Atlantic Region. Meteorol. Z. 17, 575-587. (Ulbrich, Uwe, Freie Universitaet Berlin)	this has been cited
1203	3	43	29	0	31	give references (Stocker, Thomas, IPCC WGI TSU)	this paragraph has now been rewritten
1204	3	43	33	43	33	Surface wind might decline, but not wind above the boundary layer. (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	Recent studies that find opposing trends between surface winds and higher winds have now been cited
1205	3	43	33	43	34	It might be better to start this paragraph by talking about a large NH region first (e.g., the Pryor et al study) and then move onto smaller regions such as the central Mediterrean region. Starting the paragraph with "In the Northern Hemisphere" and then immediately talking about the Mediterranean study confused me a bit. (Zwiers, Francis, Environment Canada)	This paragraph has now been rewritten
1206	3	43	33	43	41	I would like to inform you, that my article about wind variabiltiy over Iran during 1960-2005 and it's impact on wind power...has been accepted for publishing in Meteorological Application (2010). It is on the final stage. And all procedure for publishing it has finished. I extremely suggest you , Please use it for covering the information about some part of asia (Iran).I know that there are a lack for the articles about the wind variability If you don't access to it via online yet, I am able to send you the final version and also the form of it's acceptance. It has been used in special report on renewable energy...that in under providing by IPCC. (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (AS MERC))	I have e-mailed you for a copy of this manuscript and am awaiting a response
1207	3	43	33	43	41	Please add a new result by Jiang et al., 2010, Changes in wind speed over China during 1956-2004, Theor Appl Climatol, 99:421-430, DOI 10.1007/s00704-009-0152-7 (I have sent the digital paper to TSU).They found that days of strong wind and maximum wind all show declining trends over broad areas of China. (Zhao, Zong-Ci, National Climate Center)	This study is now cited
1208	3	43	38	43	38	Suggest replace "In North America, Pryor..." with "In North America, Wan et al. (2010) homogenized Canadian long-term (1953-2006) near-surface wind speed data series and reported significant decreases throughout western Canada and most parts of southern Canada (except the Maritimes) in all seasons, with significant increases in central Canadian Arctic in all seasons and in the Maritimes in spring and autumn. Pryor..." (Wang, Xiaolan, Environmen Canada)	This reference has now been added
1209	3	43	49	0	0	The authors use wrong terminology. What does it means "anemometer trends" ? (Incecik, Salahattin/Selahattin, Istanbul Technical University)	In shortening this section, the paragraph referred to has now been deleted
1210	3	43	49	0	0	This seems to repeat a point made at line 28 (Whetton, Penny, CSIRO Marine and Atmospheric Research)	In shortening this section, the paragraph referred to has now been deleted
1211	3	43	52	43	52	This should read "Smits et al., (2005) compared in-situ trends with both NCEP and ECMWF reanalyses" (van Oldenborgh, Geert Jan, KNMI)	This has been corrected
1212	3	43	52	0	0	Smits et al. compared ERA40 and pressure based indices too. (Klein Tank, Albert, KNMI)	This has been corrected
1213	3	43	55	43	56	This statement might be misleading, I think that the problem here is that over time the source of observational data used in data assimilation is inhomogeneous (i.e., availability of satellite data) and this inhomogeneity can introduce spurious trends in the reanalysis fields. (Wernli, Heini, ETH Zürich)	In shortening this section, the paragraph referred to has now been deleted
1214	3	43	58	43	60	This is also repetition (Whetton, Penny, CSIRO Marine and Atmospheric Research)	In shortening this section, the paragraph referred to has now been deleted
1215	3	43	58	44	5	(see comment 16) (Brönnimann, Stefan, University of Bern)	Linkages between process understanding and causes for trends has been strengthened through a general restructuring of the sections in this chapter
1216	3	43	60	43	62	This sentence is more or less a repetition of L29-31 and should be deleted (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	These sentences have been removed
1217	3	43	60	0	62	give references (Stocker, Thomas, IPCC WGI TSU)	These sentences have been removed

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1218	3	44	9	44	12	A study exploring the association of extreme wind events over Europe and weather types (Donat et al., 2009) demonstrated the importance of westerly circulation types for windstorm in this area. "80% of the storm days in Central Europe are connected with westerly flow and that Central European storm events primarily occur during a moderately positive NAO phase, while strongly positive NAO phases (6.4% of all days) account for more than 20% of the storms. A storm occurs over Central Europe during about 10% of the days with a strong positive NAO index." This study also considered large scale conditions associated with the storms, and typical tracks. It was found that storms over central Europe are indeed associated with particularly intense cyclones. Donat, M.G., G.C. Leckebusch, J.G. Pinto, U. Ulbrich, 2009: Examination of Wind Storms over Central Europe with respect to Circulation Weather Types and NAO phases. International Journal of Climatology, DOI: 10.1002/joc.1982. ; With respect to the cited study of Pirazolli and Tomasin (2003): Nissen et al. (2010) also computed trends of winter storms in the Mediterranean region over the ERA40 period, identifying a negative trend in the western part and small positive trends in its central and eastern parts .(their fig. 12b). These trends are largely explained by changes of the NAO (their fig. 10a). (Ulbrich, Uwe, Freie Universitaet Berlin)	This study is now cited
1219	3	44	10	44	10	See Vautard et al. (2010), accepted in Nature Geoscience. R. Vautard, J. Cattiaux, P. Yiou, P. Ciais, JN Thépaut, Northern Hemisphere atmospheric stalling partly attributed to increased surface roughness, Nature Geoscience, accepted (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	This work is now referenced
1220	3	44	21	0	0	I don't think "SAM" has been defined. Could also cross reference the relevant section later on. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	SAM has been spelt out
1221	3	44	21	0	0	spell out "SAM" (Van den Hurk, Bart, KNMI)	SAM has been spelt out
1222	3	44	23	44	27	"..., a weakened summer monsoon circulation, were attributed to a cooling in central China associated with increased air pollution", it is an argue question. Please delete the last half sentence. Reference: Jiang et al., 2010, Changes in wind speed over China during 1956-2004, Theor Appl Climatol, 99:421-430, doi 10.1007/s00704-009-0152-7 (Zhao, Zong-Ci, National Climate Center)	This section has been revised
1223	3	44	24	44	24	Cause-and-effect is unclear here - should it be "result in" or "arise from"? (Trewin, Blair, Australian Bureau of Meteorology)	This has been revised
1224	3	44	25	44	27	How formal was the attribution here? If no formal study then a different word would be better. (Trewin, Blair, Australian Bureau of Meteorology)	agreed, associated with is better
1225	3	44	31	44	36	This statment is incomplete and thus misleading. (Church, John, CSIRO)	It is not clear that this statement is incomplete. It is a summary of the AR4 SPM on wind change
1226	3	44	38	44	43	Many mid-latitude winter storms are indeed caught at the resolution of the GCMs already. They are, for example, largely included in the reanalysis, as demonstrated by the excellent agreement of damage numbers from insurance companies in Germany and loss estimates from surface winds (Pinto et al., 2007). Pinto, J.G., L. Fröhlich, G.C. Leckebusch, U. Ulbrich, 2007: Changing European Storm loss potentials under modified climate condition according to ensemble simulations of the ECHAM5/MPI-OM1 GCM. Natural Hazards and Earth System Sciences, 7, 165–175. It is still an open question, however, how large the improvement of storm estimations is by including local effects and small scale features like gusts (parameterized in both GCMs and RCMs). In an integral view considering wind-storm produced damage in Germany, the available gust parameterisations in 5 different RCMs seem not to lead to a clear improvement of storm representation by the models (Donat et al., 2010). Donat MG, Leckebusch GC, Wild S, Ulbrich U. (2010a) Benefits and limitations of regional multi-model ensembles for storm loss estimations. Climate Research . DOI: 10.3354/cr00891 (accepted, in press) (Ulbrich, Uwe, Freie Universitaet Berlin)	This sectin has been rearranged and the assessment has been revised
1227	3	44	39	44	40	This low confidence needs a reference (AR4?) and/or more detail. I would have thought that the SAM change in models, for example, would be considered fairly robust. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	this section has been rearranged and this statement removed
1228	3	44	45	44	60	I think some caveats on model simulation of extreme winds should be included. (Zwiers, Francis, Environment Canada)	a caveat has been added
1229	3	44	45	44	60	Please add references to the statements (Luterbacher, Juerg, Justus Liebig University)	references have been added
1230	3	44	45	44	60	please add a recent result on wind projection by the regional climate models over China. Jiang et al., 2010, Projections of wind changes for 21st century in China by three regional climate models, Chin.Geogra.Sci., 20(3): 226-235, doi:10.1007/s11769-010-0226-6 (I have sent the digital version paper to TSU). They found that annual and winter mean wind speed for 2081-2100 will be lower than those of 1971-1990 in the whole China. (Zhao, Zong-Ci, National Climate Center)	this has been cited
1231	3	44	47	44	48	Delete - (defined as the). An explanation of percentiles is not needed at this point. (Stocker, Thomas, IPCC WGI TSU)	This has been deleted
1232	3	44	48	0	0	"MMD" -> "MME" (Van den Hurk, Bart, KNMI)	This has been corrected

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1233	3	45	8	0	17	It is just a comment; The capability of a 1-km mesh non-hydro-static model in reproducing a typhoon-induced wind has been examined in Maruyama et al. (2010): Takashi Maruyama, Eriko Tomokiyo and Junji Maeda: "Simulation of Strong Wind Field by Non-hydrostatic Mesoscale Model and Its Applicability for Wind Hazard Assessment of Buildings and Houses", Hydrological Research Letters, Vol. 4, pp.40-44, (2010) (Nakaegawa, Toshiyuki, Meteorological Research Institute)	due to the space constraints of this assessment, this paper was not cited because it does not deal with observed trends or future projections in wind
1234	3	45	8	0	17	I suggest to insert new sentences "The capability of a 1-km mesh non-hydro-static model in reproducing a typhoon-induced wind has been examined in Maruyama et al. (2010) showing that the temporal variation of the calculated wind speeds with the 1 km horizontal mesh grids is considered to correspond to the observed wind speeds averaged over the time period of approximately ten to fifteen minutes. Oku et al. (2010) made comparison of typhoon-induced extreme winds and precipitations computed by two different 1-km mesh non-hydro-static models with similar physics parameterizations, showing that reproduction of steep terrain in models plays a significant role in representing extreme value in addition to other conceivable factors such as numerical and physical schemes." The English references to be added are "Maruyama, T., E. Tomokiyo and J. Maeda, 2010: Simulation of strong wind field by non-hydrostatic mesoscale model and its applicability for wind hazard assessment of buildings and houses, Hydrological Research Letters, Vol. 4, pp.40-44." and "Oku, Y., T. Takemi, H. Ishikawa, S. Kanada and M. Nakano, 2010, Representation of extreme weather during typhoon landfall in regional meteorological simulations: A model intercomparison study for Typhoon Songda (2004), Hydrological Research Letters, Vol. 4, pp.1-5." (Nakakita, Eiichi, Kyoto University)	due to the space constraints of this assessment, this paper was not cited because it does not deal with either observed trends or future projections in wind
1235	3	45	19	45	22	This paragraph beginning 'Since the AR4.....' needs to come earlier in the text, before the Gastineau and Soden citation first appears. Suggest incorporating these sentences within the earlier paragraph beginning line 45, page 44. (Stocker, Thomas, IPCC WGI TSU)	The section has been substantially rearranged and this point has been addressed
1236	3	45	19	45	22	It might be useful to indicate whether we have greater confidence in model simulated winds at the 850 hPa level than at 10m. (Zwiers, Francis, Environment Canada)	we expect consistency between changes at 850 and the surface (which is now stated) and this is indicated by the results of Gastenau and Soden and the figure 3.10
1237	3	45	24	45	30	Unclear to which time horizon the results refer, in particular 5% change by when? (Klein Tank, Albert, KNMI)	Time horizons have been added and the text revised
1238	3	45	30	0	0	give references (Stocker, Thomas, IPCC WGI TSU)	the text has been revised and references added
1239	3	45	35	45	0	Reference should be Haugen and Iversen? (Benestad, Rasmus, The Norwegian Meteorological Institute)	spelling mistake in 2nd author name has been corrected in text and refs
1240	3	45	37	45	37	By applying extreme value statistics to gust wind speeds from an ensemble of RCMs, Rauthe et al. (2010) found a small, but significant increase in gust wind speed over north Germany (0.7-2.8% for a 10-year return period). Over the other parts of Germany, the results are not uniform. Reference: Rauthe, M., M. Kunz, and Ch. Kottmeier, 2010: Changes in wind gust extremes over Central Europe derived from a small ensemble of high resolution regional climate models. Meteorologische Zeitschrift, 19, 299-312. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	This study is now cited
1241	3	45	38	0	0	This predominance of NW extreme winds is not confirmed over the North Sea, as shown by Sterl, A., H. van den Brink, H. de Vries, R. Haarsma en E. van Meijgaard, An ensemble study of extreme North Sea storm surges in a changing climate; Ocean Science. 2009. 5. 369-378. (Van den Hurk. Bart. KNMI)	This study is now cited
1242	3	45	39	45	39	I am not sure if this is correct. In fact, an increase of storm risk may be associated with a change of the pressure pattern. Donat et al. (2010) found the projected increase of storm risk over Europe associated with an increasing number of westerly flow situations, explaining part of the signal in an ensemble of simulations. Donat MG, Leckebusch GC, Pinto JG, Ulbrich U. (2010c) European storminess and associated circulation weather types: future changes deduced from a multi-model ensemble of GCM simulations. Climate Research 42:27-43. DOI:10.3354/cr00853 (Ulbrich, Uwe, Freie Universitaet Berlin)	the final paragraph has been redrafted to better articulate the reason for the assessment
1243	3	45	40	45	42	This sentence needs rewriting for better clarity. Presumably '_low_level of agreement' is intended. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	this sentence has been reworded
1244	3	45	40	45	42	This conclusion is not logical after citing a broad agreement of results for central and northern Europe with respect to wind-storms under climate change. While in general an agreement of model simulations is not a firm proof, I see no contradicting argument for a statement on the agreement of models with respect to Europe in particular. (Ulbrich, Uwe, Freie Universitaet Berlin)	the final paragraph has been redrafted to better articulate the reason for the assessment
1245	3	45	44	45	46	This final short paragraph appears very odd, and there is no reason why the Sailor et al reference needs its own separate paragraph. It should be incorporated into one of the two paragraphs coming beforehand. (Stocker, Thomas, IPCC WGI TSU)	this section has been rewritten
1246	3	45	44	45	46	There must be other statistical downscaling studies.... (Zwiers, Francis, Environment Canada)	I was unable to find additional recent studies employing statistical downscaling
1247	3	45	44	45	46	These sentences are lost here. Please put them in context, moving the statement to the section describing other results. (Ulbrich, Uwe, Freie Universitaet Berlin)	this section has been rewritten

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1248	3	45	49	45	49	Overall, I think the intro and observed subsection could be improved. It would be useful if the introduction could include a bit of discussion on the approach that is taken. Monsoons could be treated as a phenomenon or it could simply be used as a way to delineate regions. I think the former would be preferable, although clearly there is literature of both types. Also, I think it would be useful to briefly define things like monsoon rainfall, etc. In the observed section, I think it would be good to keep to things that pertain directly to the monsoon, rather than to discuss changes in the monsoon region at all times of the year. (Zwiers, Francis, Environment Canada)	We will work in an improvement of the sub section following the suggestions of the reviewer. The focus of this section 3.4.1 has been on rainfall, because there have been more studies using this variable as indicator on monsoons. In fact we consider monsoon as a phenomenon and not as a region, but we do the analysis at regional scales.
1249	3	45	49	50	9	Section 3.4.1 on "Monsoon": No information from paleo records is used here to support the assessment (Stocker, Thomas, IPCC WGI TSU)	Some brief discussion of paleo studies now included.
1250	3	45	49	0	0	Section 3.4: Parts of the assessment covered in Section 3.4 are sometimes based on single or very few studies. Suggest to be careful here and make sure that no key conclusions are drawn from those parts of the assessment. Also, given the overall extensive length of the chapter, the individual section in 3.4 and 3.5 need all be shortened. (Stocker, Thomas, IPCC WGI TSU)	Section has been shortened. No key conclusions reached in this section now.
1251	3	45	53	45	62	Briefly mention the precipitation characteristics used in studying the monsoon phenomena (e.g. intensity, duration, threshold value or etc.) (Davtalab, Rahman, Ministry of Energy)	it has been based mostly in intensity, either on monthly totals and also on extremes or intense rainfall events, as defined by IPCC. With the data available and the definition of extremes indices we do not consider much the duration
1252	3	46	1	0	0	Section 3.4.1.1.: The language of this section is substandard. Define the term "heavy precipitation" (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	The definition of heavy precipitation is based on the IPCC definition, that was based on Frich et al (2002) and that has been used extensively in the Chapters 3 and 10 of the IPCCAR4 WG1, and by Alexander et al (2006)
1253	3	46	14	46	28	I suggest not starting with the American monsoon as it is less important than the ones that follow. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Agree. Subsection moved.
1254	3	46	14	0	0	Please replace word 'vulnerable' - sensitive perhaps? (Stocker, Thomas, IPCC WGI TSU)	We have replaced vulnerable by sensitive
1255	3	46	14	0	0	American monsoon ?? (Incecik, Salahattin/Selahattin, Istanbul Technical University)	Yes, there are many studies and papers published since the late 1990's in the North American and south American monsoon, and CLIVAR has a panel on American monsoons
1256	3	46	20	46	23	What is the link between the North American monsoon and tropical cyclones (e.g., are there variations in land-falling TCs that are linked to variations in the strength of the North American monsoon)? If there is none, then perhaps the impact of TC related precipitation is not relevant to this section? (Zwiers, Francis, Environment Canada)	The paper by Cavazos et al (2008) finds increases in precipitation intensity in the mountain sites of NW Mexico during 1961-98, possibly related to TCs. Most monsoon areas have links with TCs.
1257	3	46	27	46	27	The wording of this is a bit strange as the winter season is not normally considered a monsoon period in this region. (Trewin, Blair, Australian Bureau of Meteorology)	Yes, winter is not the peak of the monsoon season, but it looks like winter rainfall is now playing a bigger role in total rainfall. It is possible that the winter rainfall may play a role in annual rainfall and not on the monsoon rainfall. In fact, the paper by Arriga-Ramirez and Cavazos shows that an increase in winter rainfall may delay the onset of the North American monsoon
1258	3	46	27	46	28	In the last sentence: occurrence of drought is mentioned, but there is no description about relation of this occurrence to monsoon. (Davtalab, Rahman, Ministry of Energy)	Correction has been made
1259	3	46	30	46	44	The part of this paragraph that seems to pertain directly to monsoon is pretty limited (i.e., summer winds in east and southeast China). (Zwiers, Francis, Environment Canada)	It is limited, yes, but we believe it is relevant to the context of the discussion, and we did not want to extend more on this.
1260	3	46	41	46	41	Since - at least on the large scale - the wind field is always in balance with the other forces, the statement about "the weakening in the lower-tropospheric pressure-gradient force" is trivial and should be deleted. In fact, the question is: what are possible explanations for the weakening of the pressure gradient? (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Sentence deleted.
1261	3	46	43	46	43	What is the urban effect referred to here? (Trewin, Blair, Australian Bureau of Meteorology)	Deleted.
1262	3	46	43	0	0	Unclear why the decrease in light precipitation is associated with the UHI effect in this example. (Klein Tank, Albert, KNMI)	Deleted reference to UHI effect.
1263	3	46	50	46	51	What is meant by "extreme heavy precipitation events"? (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	The correct term is extreme hourly precipitation events as defined by Sem Roy (2009) for India
1264	3	46	53	46	53	"explainne" is the wrong term here (Brönnimann, Stefan, University of Bern)	Correction has been made
1265	3	46	53	46	54	This sentence is unclear and has to be reformulated (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Sentence deleted.
1266	3	46	57	46	61	On line 60 I assume that the intent is to say 1.5 degrees of latitude, not Celsius. It might be simpler to say 150 km. Are these results considered to be credible given the mix of data that are used? Does the shift in deep convection correspond to that in precipitation? (Zwiers, Francis, Environment Canada)	Fixed.
1267	3	46	57	46	61	Is there only one relevant reference for African monsoon? (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Yes, that was one the few ones we found. The other were not relevant to extremes and monsoons.
1268	3	46	60	46	60	Delete the "C" so it reads "+1.5° for the 400 mm July..." (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Fixed

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1269	3	46	60	0	0	the northward migration suggest that it should be degrees in latitude not Celsius (+1.5 degree for the 400 mm July to September isohyets) (Wibig, Joanna, University of Lodz)	Fixed.
1270	3	46	62	46	62	Is there any kind of consistency across monsoon systems that can be synthesized? (Zwiers, Francis, Environment Canada)	The last paragraph of Section 3.4.1.1 may serve as synthesis. However, it is hard to say on changes of extremes that show consistency among monsoon regions. Perhaps a common pattern is the increase in extreme precipitation in all regions, but the different data sets used on this as well as the different period used may complicate an intercomparison analyses.
1271	3	47	1	47	3	This text belongs below the heading on line 5 (Zwiers, Francis, Environment Canada)	Restructuring of section removed this issue.
1272	3	47	1	0	0	This statement from Hegerl et al. (2007) is too general - What time frame is being referred to here in relation to 'climate change'? (Stocker, Thomas, IPCC WGI TSU)	Its is more like an introduction statement now. The time frame used in the Chapter 9 of the IPCC AR4 WG1 is 20th century
1273	3	47	5	0	0	Section 3.4.1.2: add discussion about the impact of aerosols (black carbon and sulfate) on monsoons. Eg. Meehl et al. 2008, Effects of Black Carbon Aerosols on the Indian Monsoon, J Climate, 21, 2869-2882; Lau, K. M., M. K. Kim, and K. M. Kim, 2006: Asian summer monsoon anomalies induced by aerosol direct forcing: The role of the Tibetan Plateau. Climate Dyn., 26, 855-864 (Arblaster, Julie, NCAR: Australian Bureau of Meteorology)	Some discussion included.
1274	3	47	7	47	11	"The observed decrease in monsoon rainfall": this is in contrast with the section above pointing at an increased monsoon rainfall in Africa. (Van den Hurk, Bart, KNMI)	The observed rainfall increase in N Africa (Fontaine et al 2010) is detected since the middle 1990. Zhou et al (2008) and Wang and Ding (2006) report that the combination of monsoon area and rainfall intensity change has led to an overall weakening trend of global land monsoon rainfall accumulation during the last 54 years. This decreasing tendency is mainly caused by the North African monsoon and South Asian monsoon, due to the significant decreasing tendencies of both rainfall intensity and monsoon coverage. Therefore the tendencies reported by Zhou et al and Fontaine et al reflect the interdecadal variability of rainfall in the N. African monsoon, and do not reflect any contradiction.
1275	3	47	7	47	7	Define monsoon rainfall. (Zwiers, Francis, Environment Canada)	In the papers mentioned in comment 1274, monsoon rainfall is defined based on monthly rainfall data (CRU, other sources) during the April to October peak season.
1276	3	47	7	47	7	This is the first mention of such an observed negative trend - if it is going to be discussed here then it should be described in an earlier section. (Trewin, Blair, Australian Bureau of Meteorology)	The observed trend detected by Fontaine et al (positive) using the CRU data is detected only since the middle 1990s, less than 20 years, while other studies (see comment 1274) show negative trends.
1277	3	47	7	0	0	The observed negative trend in global land monsoonal rainfall'. This statement had a confusing effect. The previous section, if anything, created the impression of increased monsoonal rainfall. There is a need for greater clarity in this monsoon discussion (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Increased monsoonal rainfall in North Africa is detected only since middle 1990's, while on the longer term the tendency is for rainfall reduction. Decadal rainfall variability plays a major role here.
1278	3	47	10	47	17	Both L10 and L16 describe decreasing trends in north African monsoon rainfall; on p46, L58-59, it is written that the results of Fontaine et al. (2010), which is the only reference, show a rainfall increase in north Africa since the mid-90th. This seemingly contradiction has to be clarified (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Please refer to comments 1274, 1277 and 1278.
1279	3	47	20	0	0	La nina like SST pattern" The text at this point implies that there is a trend of this nature. I didn't get that impression from section 3.4.2 (Whetton, Penny, CSIRO Marine and Atmospheric Research)	We believe that these are different issues. Section 3.4.2. discusses El Nino or La Nina and trends on those conditions, and what is on page 47 reflects some associations between La Nina like conditions and rainfall in N Australia, as explained by correlation analysis. We do not say anything about trends on this association
1280	3	47	21	47	0	What were the conclusion of this study? (Benestad, Rasmus, The Norwegian Meteorological Institute)	The main conclusion is on the increased in rainfall in NW Mexico associated with TCs. (see comments 1256 and 1257)
1281	3	47	26	47	27	"The main cause..."; this repetition from p6, L41 should be deleted, see comment above (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Deleted
1282	3	47	31	47	34	This presumably requires a reference. (Zwiers, Francis, Environment Canada)	More discussion of this point and references added.
1283	3	47	31	47	48	It would be helpful to elucidate why summer monsoon rainfall weakens in the Northern hemisphere. If it were possible a clearer marshalling of the arguments of what is happening, why (in terms of eg SST variability) and what the models project and why, including the interplay of ghg and aerosol forcings would be really helpful for the reader to understand the overall picture. Splitting the sections up between observations, causes and projection is a bit of a barrier to this overall understanding. Maybe it would be possible to have a one para summary at the start or end of the section, eg summarising the evidence on monsoons as it stands, crucially including our current understanding of mechanisms of changes and their uncertainties. (Stett, Peter, Met Office)	Discussion revised.

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1284	3	47	33	0	0	I think a reference is needed to confirm the better monsoon precipitation skill of higher resolution models (Van den Hurk, Bart, KNMI)	Discussion revised.
1285	3	47	50	0	0	From the previous text I learn that the models do not do a good job on precipitation characteristics associated with the monsoons. (Klein Tank, Albert, KNMI)	Yes, that is correct and is now explained in the text
1286	3	47	51	47	52	This statement indicating that models do not simulate observed monsoon circulation changes (presumably because coupled models do not simulate the right pattern of SST change) is in contrast to material in the previous paragraph indicating some agreement in precipitation trends. Would a comment on the implications be in order? (Zwiers, Francis, Environment Canada)	The discussion in here is more related to the issue of space resolution, and the text has been rewritten including this
1287	3	47	58	48	12	I am missing a discussion on possible aerosol effects on the monsoons. (Brönnimann, Stefan, University of Bern)	We have added a discussio on this at the last paragraph in sections 3.4.1.2.
1288	3	47	61	47	0	Monsoons are not mentioned in sections 3.4.2 or 3.4.4. For some of these natural modes, the link to the monsoon is not clear. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Do not understand point being made by reviewer.
1289	3	47	0	0	0	In general section 3.4.1.2, whilst nominally devoted to causes, is mixed in with results of observed changes which should be in earlier sections. (Trewin, Blair, Australian Bureau of Meteorology)	Section has been restructured.
1290	3	48	23	48	24	Does the winter weakness still hold in the tropics? (Trewin, Blair, Australian Bureau of Meteorology)	Reference deleted.
1291	3	48	24	48	0	Which models? All CMIP3 runs? (Benestad, Rasmus, The Norwegian Meteorological Institute)	The paper by Tanaka et al (2005) analyses 15 CMIP 3 global models
1292	3	48	39	48	39	There is little consensus in the CMIP3 with respect to changes in the Australian summer monsoon eg. Moise A. and R. Colman, 2010: Tropical Australia and the Australian Monsoon: General assessment and projected changes, Managing Climate Change: Papers from the Greenhouse 2009 conference, CSIRO publishing, 278 pp (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	We have added some text based on the Moise and Colman 2010 reference
1293	3	48	39	0	0	Implies that there is consistency on simulated rainfall increase in the Australian monsoon. This is not true. Disagreement is strong in the CMIP3 models - see that AR4. Also see p49, line 39 in draft. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	yes, the reviewer is right, we have changed the text (as also suggested in comment 1292, adding a new reference by Moise and Colman 2010
1294	3	48	47	48	0	Which simple physical arguments? (Benestad, Rasmus, The Norwegian Meteorological Institute)	It refers to the associations between water vapor and warming, with the starting point as the Clausius–Clapeyron equation for the saturation vapor pressure:
1295	3	48	54	48	60	Is one of these studies more credible? (Zwiers, Francis, Environment Canada)	We stated the fact that the Ashfaq paper used um global model and 1 regional model (downscaling from 1 global model), and the Kripalani paper uses 19 CMIP global models
1296	3	48	54	48	60	Two mentioned paper present different projections of south Asian summer monsoon. The cause of such difference should be discussed if the final conclusion is that the monsoon could be weakened and its onset delayed. (Wibig, Joanna, University of Lodz)	We suggesst (comments 1295) that the fact that one paper analyses 19 CMIP3 models and the other uses the doenwcalaing of 1 CMIP3 model (using the RecGM3 regional model) may be the cause of that difference
1297	3	48	55	0	0	It is not clear if Asfaq (2009) also serves as the reference for the previous sentence (Stocker, Thomas, IPCC WGI TSU)	We have rearranged the text, the statements from the different papers haven orgnaized in scuh way that the reader can see the differences among the studies.
1298	3	48	55	0	0	Is this a one model or a multi-model study? (Whetton, Penny, CSIRO Marine and Atmospheric Research)	The ashfaq paper is one model only (one global and one regional)
1299	3	48	62	49	13	Kurihara et al.(2005) indicated for the first time that precipitation in summer increases over the western part of Japan due to intensified water vapor flux along the rim of the subtropical high pressure intensified by the El Nino-like high SST over the eastern equatorial Pacific. Kurihara K., K. Ishihara, H. Sasaki, Y. Fukuyama, H. Saitou, I. Takayabu, K. Murazaki, S. Yukimoto and A. Noda, 2005: Projection of climatic change over Japan due to global warming by high-resolution regional climate model in MRI. SOLA, Vol.1, 97-100.. (Kurihara, Kazuo, Meteorological Reserach Institute)	We have included this reference on the text
1300	3	48	63	0	0	What is the Baiu-Changma-Meiyu band? (Klein Tank, Albert, KNMI)	It is a rain band, a convergence zone that in the Asian monsoon that shows in satellite images as na organized rain band
1301	3	49	6	0	0	I think that "(Kanada et al., 2009)" should be replaced by "(Kanada et al., 2010)". (Nakakita, Eiichi, Kyoto University)	We have added the updated reference
1302	3	49	9	49	0	Do we have sufficient confidence in the hourly precipitation output from GCMs to say anything about trends? These are likely to represent small spatial scales, whereas GCMs have grid-boxes of the order 200kmx200km. See following paragraph... (Benestad, Rasmus, The Norwegian Meteorological Institute)	Yes, in fact the projections (or even simulations) or hourly precipitation still have uncertainties.
1303	3	49	22	49	22	Consider Shukla, J. (2007) Monsoon mysteries. Science 318, 204-205. (Brönnimann, Stefan, University of Bern)	This reference and comments have been added
1304	3	49	24	49	34	Does this bit, assessing models, fit better in 3.4.1.2? (Zwiers, Francis, Environment Canada)	Yes, the reviewer is right, we have moved this text to Section 3.4.1.2 with some modification
1305	3	49	42	49	0	The paragraph repeats earlier discussion regarding monsoons. (Benestad, Rasmus, The Norwegian Meteorological Institute)	unnecessary text has been deleted
1306	3	49	42	49	60	Does this further bit, also assessing models, fit better in 3.4.1.2? (Zwiers, Francis, Environment Canada)	Yes, the reviewer is right, we have moved this text to Section 3.4.1.2 with some modification

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1307	3	49	45	49	47	This is probably a naive question, but what is the relevance to monsoon simulation and change? (Zwiers, Francis, Environment Canada)	We want to state that if the propagation of some waves is not well represented in model run on simulation mode, projections of these patterns in future climate are still uncertain
1308	3	49	49	49	0	The terms 'AOGCM', GCM, and 'CGCM' are all used, probably referring to the same thing: global climate model (involving coupling of atmosphere-ocean) (Benestad, Rasmus, The Norwegian Meteorological Institute)	Corrections have been made, to make the text more homogeneous.
1309	3	50	2	50	9	This seems generic and could be said for projections of all phenomena considered in this chapter. (Zwiers, Francis, Environment Canada)	Yes, but as this stage we would like to keep this text, so at least it would show as relevant to monsoons
1310	3	50	6	50	9	This dependence of model results on the time slice analysed deserves more emphasis when judged critically important. (Klein Tank, Albert, KNMI)	This type of explanation is already shown on previous subsections of this Section on monsoons
1311	3	50	11	52	37	3.4.2 ENSO: This is a long section relative to the main conclusion that there is no clear evidence about past or projected trends related to anthropogenic forcing. (IPCC WGII TSU)	Agreed. Shortened somewhat.
1312	3	50	11	0	0	Section 3.4.2 Comment: To effectively understand how possible changes in rainfall patterns may influence the potential for hydroelectric generation in a given region, for example, it is important to understand how the low frequency oscillations of the air will behave. Among them, the teleconnection caused by the ENSO events must have an outstanding importance, since the canonical patterns of influence of such phenomena in the atmosphere are already studied for several decades, as presented in this IPCC report. Therefore, understanding possible changes in frequency and intensity of this type of event should be a priority in studies of this panel, because this type of variability is better represented by numerical models of climate prediction than the occasional occurrence of extreme events, eg. Importantly, between the years 2000 and 2010 there were four episodes of El Niño and three episodes of La Niña, with the last episode of La Niña setting in mid-2010. This frequency of occurrence of ENSO is one of the highest, if not the highest, of the historic of occurrence of this type of event. This fact has brought in recent years, at least to Brazil, a larger variance of precipitation in relation to its historical average, which led to extra difficulties in the energetic operation planning, which is based on stochastic models and does not contain periods in its historic with such a large variance. Furthermore, it requires investment in basic research to investigate other regions of the lithosphere that can also exert significant changes in atmospheric general circulation, so that they can also be considered in the analysis process and results of numerical predictions of the IPCC models, such as those occurring in the South Atlantic region of confluence of currents from Brazil and the Malvinas / Falkland (Cataldi and Torres Junior, 2006). CATALDI, M. ; TORRES JUNIOR, A. R. . South Atlantic SST anomalies impacts on the south Brazilian region climate through a numeric experiment with the model CAM 2.02 and data mining techniques. In: 8th International Conference on Southern Hemisphere Meteorology and Oceanography, 2006, Foz do Iguacu. Anais da 8th International Conference on Southern Hemisphere Meteorology and Oceanography, 2006. v. 1. p. 521-231. (Rocha, Vinicius, Operador Nacional do Sistema Elétrico)	Noted. No obvious changes in response to comment.
1313	3	50	13	50	27	This introductory bit should also mention the Southern Oscillation part of the phenomenon. (Zwiers, Francis, Environment Canada)	Agreed.
1314	3	50	13	50	27	Is it worth mentioning La Nina impacts more specifically? (Trewin, Blair, Australian Bureau of Meteorology)	Agreed.
1315	3	50	16	0	0	Please add 'abnormally' before 'cool' (Luterbacher, Juerg, Justus Liebig University)	Agreed.
1316	3	50	17	50	21	This only discusses continental rainfall anomalies. Some of the most marked ENSO rainfall changes affect the various Pacific island nations and these should be mentioned. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Agreed.
1317	3	50	32	50	32	Suggest replacing "A climate shift" with "An apparent climate shift". The term climate shift seems to have a life of its own (Zwiers, Francis, Environment Canada)	Agreed.
1318	3	50	34	50	35	Please provide references regarding this 'paleoclimatic evidence'. (Stocker, Thomas, IPCC WGI TSU)	Space limitations - see comment #1311.
1319	3	50	34	50	35	Delete this last sentence? I'm not sure that this is useful in the context of this report. (Zwiers, Francis, Environment Canada)	Agreed.
1320	3	50	39	50	45	Also cite Jansen et al, 2007 (Ch 6, WG1 AR3). (Zwiers, Francis, Environment Canada)	Should we be adding even more cites to AR4? There are requests that we reduce our citations of AR4.
1321	3	50	50	0	0	"with perhaps a trend..." is a rather weak statement. One could test the trend significance. (Klein Tank, Albert, KNMI)	Simplified statement.
1322	3	50	52	50	54	What would drive a change in the SOI? (Zwiers, Francis, Environment Canada)	Authors cited do not specify this.
1323	3	51	9	51	44	More details on models are needed - what models are assessed and how do they perform? (Stocker, Thomas, IPCC WGI TSU)	Will consider, but note comment 1311.
1324	3	51	11	51	0	One relevant reference is DiNezio et al. (2010; EOS, vol 91, number 16), discussing whether the ocean or atmospheric are more important for the trend. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Disagree with relevance of reference
1325	3	51	13	51	25	The section on orbital forcings of ENSO is irrelevant to the discussion how ENSO might respond to anthropogenic forcing and could be deleted. Please keep the sentence on Witenber (2009). (van Oldenborgh, Geert Jan, KNMI)	Reject. Need to consider other possible forcings that might change behaviour of ENSO. Otherwise we will be attacked for "ignoring" natural climate change and variability.
1326	3	51	17	51	26	consider Meehl, G.A. et al. (2009) Amplifying the Pacific Climate System Response to a Small 11-Year Solar Cycle Forcing. Science 325,1114-1118. (Brönnimann, Stefan, University of Bern)	Agreed.

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1327	3	51	31	51	31	Suggest deleting "still" (its inclusion suggests the anticipation of a trend). (Zwiers, Francis, Environment Canada)	Agreed.
1328	3	51	37	51	44	I think it would also be useful to refer to Ch8, WG1 AR4 (section 8.4.7 and Figure 8.13). (Zwiers, Francis, Environment Canada)	Agreed.
1329	3	51	46	0	0	Surprised not to find Guilyardi et al. 2009 (BAMS vol 90) included in here, which would help improve the coverage of the most recent 09/10 studies. (Stocker, Thomas, IPCC WGI TSU)	Agreed.
1330	3	51	54	51	55	"This change may be described as an "El Niño – like" average change because during an El Niño episode these winds generally weaken" should read "This change should not be described as "El Niño-like, even though during an El Niño episode these winds are generally also weaker" The inaccurate meme "ENSO will change to an El Niño-like state" is so entrenched that it should not be encouraged, even if the next sentence shows the difference." (van Oldenborgh, Geert Jan, KNMI)	Agreed. Good point.
1331	3	51	56	51	57	"climate models project that the Indonesian region would become wetter" I can find no evidence of this in the CMIP3 data, precipitation in most of Indonesia is projected to stay the pretty much the same. (van Oldenborgh, Geert Jan, KNMI)	Deleted sentence.
1332	3	51	61	51	63	Is it useful or credible to discuss a 16xCO2 simulation? (Zwiers, Francis, Environment Canada)	The point is that there is not a simple relationship between El Nino and CO2. This is important. To omit it would leave us open to the charge that we are pretending it is simple.
1333	3	51	61	51	63	If this result is going to be mentioned it should be stated that a 16-fold increase is not a realistic 21st century scenario (Trewin, Blair, Australian Bureau of Meteorology)	Text revised to clarify that the point is not that this is a projection, but that there is not a simple relationship between CO2 and ENSO.
1334	3	51	63	51	63	It was shown that although the background state in the ENSO region shows large changes, most of these changes cancel in their effects on the ENSO cycle (Philip & van Oldenborgh, GRL, 2006, doi:10.1029/2006GL026196). For instance, the impact of shallower thermocline is countered by larger atmospheric damping, and an increase of heat released in a warmer background state by a more stable atmosphere. The balance of these terms is very model-dependent as the sum is much smaller than the terms themselves. (van Oldenborgh, Geert Jan, KNMI)	Should have been covered in AR4. Space limits here preclude including this discussion.
1335	3	52	2	52	3	Sterl et al (Clim.Dyn, 2007, doi:10.1007/s00382-007-0251-z) showed that no change in teleconnections can be detected in the next century due to the poor signal/noise ratio (a change cannot be distinguished from random weather variability). Van Oldenborgh & Burgers (GRL, 2005, doi:10.1029/2005GL023110) showed that no change in ENSO teleconnection can be detected in the observations up to 2004. So the changes in teleconnections are not very important relative to all other factors that change. (van Oldenborgh, Geert Jan, KNMI)	Text revised.
1336	3	52	9	52	14	Worth adding something here (and citations) about the differential impacts of central (Modoki) and eastern Pacific events. (Trewin, Blair, Australian Bureau of Meteorology)	This is done earlier in section.
1337	3	52	17	52	26	More insights in the lack of representation of non-linear interactions in GCMs was provided by Philip, S.Y. en G.J. van Oldenborgh, Significant atmospheric nonlinearities in the ENSO cycle; J. Climate, 2009, 22, 14, 4014-4028 (Van den Hurk, Bart, KNMI)	Precluded by requirement to shorten section - see comment 1311.
1338	3	52	18	52	18	Add Collins et al (2010), The impact of global warming on the tropical Pacific Ocean and El Niño, Nature Geoscience 3, 391 - 397 (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Agreed.
1339	3	52	41	52	42	On what ground is a variability mode such as IOD chosen here and others are not mentioned (Eastern Atlantic pattern for instance)? In my opinion, IOD is still not clear defined as an independent variability mode. (Bojariu, Roxana, National Meteorological Administration)	The IOD is cited much more frequently in the scientific literature and in public documents and discussions, than is the Eastern Atlantic pattern.
1340	3	52	42	52	47	It could be mentioned that a negative NAO is associated with a higher frequency for winter storms to move over Central Europe (which are the main source of damage due to natural hazards). (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Sufficient detail on impacts already included.
1341	3	52	43	52	43	Replace "Atlantic" with "North Atlantic". (Zwiers, Francis, Environment Canada)	Agreed.
1342	3	52	44	0	46	this results not only in a shift of winter storms but in stronger westerlies in general; give reference (Stocker, Thomas, IPCC WGI TSU)	Text revised.
1343	3	52	46	0	0	Please add 'parts of' before 'Europe' (Luterbacher, Juerg, Justus Liebig University)	See response to comment 1344.
1344	3	52	46	0	0	Insert "North-Western" before "Europe" (Van den Hurk, Bart, KNMI)	Agreed.
1345	3	52	52	0	0	Is there a reference for this Australian result? I find it surprising. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	References shifted to match statement.
1346	3	52	57	0	0	reference for fore result is Cai et al (2009) GEOPHYSICAL RESEARCH LETTERS, VOL. 36, L19710, 6 PP., 2009 doi:10.1029/2009GL039902 (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Include reference.
1347	3	52	58	52	58	Add after "Indonesian region", "as well as below-normal rainfall in parts of Australia". Could also mention the correlation between positive IOD and El Nino. (Trewin, Blair, Australian Bureau of Meteorology)	These sentences have been simplified and shortened.
1348	3	52	60	52	0	What weather extremes? (Benestad, Rasmus, The Norwegian Meteorological Institute)	Provide examples.
1349	3	52	60	52	60	Insert "over North America" after "weather extremes". (Zwiers, Francis, Environment Canada)	Agreed.
1350	3	52	60	0	0	Please specify what kind of extremes (Luterbacher, Juerg, Justus Liebig University)	Agreed.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1351	3	52	0	0	0	Section 3.4.3: This section mixes impacts of the NAO, SAM etc with actual changes in these modes. Suggest removing all mention of the impacts as these have been discussed elsewhere. The section is also quite SH heavy. Add references on limitations of SAM record (Jones and Fogt) and also discussion of different projections to 2050 from chemistry climate models and CMIP3 models (Son et al 2010, Science) (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Impacts are needed to explain focus on these modes, but section has been streamlined to avoid mixing impacts with changes in modes themselves. SH bias reflects recent work.
1352	3	53	1	53	48	Recent and future behaviour of IOD and East African rainfall papers; Spencer H, Sutton RT, Slingo JM, Roberts M, Black E. Indian Ocean climate and dipole variability in Hadley Centre coupled GCMs. Journal of Climate 2005, 18:2286-2307. Conway D, Hanson CE, Doherty R, Persechino A. GCM simulations of the Indian Ocean dipole influence on East African rainfall: Present and future. Geophysical Research Letter 2007a, 34:L03705. doi: 10.1029/2006GL027597 Kay G, Washington R. Future southern African summer rainfall variability related to a southwest Indian Ocean dipole in HadCM3. Geophysical Research Letters 2008, 35, L12701. (Conway, Declan, University of East Anglia)	Thanks. Added Conway et al and Kay and Washington references.
1353	3	53	3	53	5	It would be worth pointing out that this recent reversion to mean levels is consistent with the NAO containing stochastic trends as discussed in Stephenson, D.B., Pavan, V. and Bojariu, R. (2000): Is the North Atlantic Oscillation a random walk?, International Journal of Climatology, 20, pp 1-18. (Stephenson, David, University of Exeter)	Reference published too early for inclusion here. Should have been in AR4 or TAR.
1354	3	53	15	53	0	More concise phrasing: Ummerhofer et al., 2008, 2009a, 2009b (Benestad, Rasmus, The Norwegian Meteorological Institute)	Agreed. Change.
1355	3	53	16	53	17	This statement about time scales of multi-decadal modes which are often longer than instrumental data series is important for understanding changes in extremes. It deserves more upfront attention. (Klein Tank, Albert, KNMI)	The point is clear, although concise.
1356	3	53	21	53	27	This part could be a bit more detailed (Luterbacher, Juerg, Justus Liebig University)	Insufficient literature and space to expand this subsection.
1357	3	53	21	53	27	The attribution of complex phenomena like NAO, SAM etc to human activities is very doubtful. It can only be based on models that generally have great difficulties in representing these complex phenomena adequately. In addition, the response of the phenomena to a temperature increase needs a solid, undisputable link between this temperature increase and human activity. IPCC never makes a statement firmer than "very likely". The attribution of this kind of phenomena is therefore purely academic and based on imperfect statistical relationships and physical understanding. This should be reflected somewhere in this chapter. (Van den Hurk, Bart, KNMI)	Uncertainty language now used throughout chapter, where appropriate.
1358	3	53	21	53	43	Please edit this section on the SAM for better balance and accuracy, and provide a fuller set of references. This section doesn't really reflect the current state of understanding. Stratospheric ozone is expected to recover over the course of the 21st century (not 'stabilize or recover'). Please acknowledge that numerous studies have shown that the largest SAM trends occur in Dec/Jan/Feb, and are mainly due to ozone changes, not GHG. The robustness and seasonality of the tropospheric circulation response to Antarctic ozone depletion has been established in numerous climate simulations (e.g., Arblaster and Meehl, J. Clim. 2006; Cai and Cowan, J. Clim., 2007; Karpechko et al., GRL, 2008; Perlwitz et al. GRL 2008, Fogt et al., J. Clim. 2009; Son et al. GRL, 2009, 2010; SPARC CCMVal, Chapter10). Table 3 of Fogt et al. 2009 provides an updated view of observed and simulated tropospheric circulation trends expressed in terms of the Southern Annular Mode. The positive trends in the Southern Annular Mode are most robust in observations and models during the austral summer and fall seasons (labelled DJF and MAM, respectively). During the austral summer season, a statistically significant positive Southern Annular Mode response is simulated only in those integrations that include the observed Antarctic ozone depletion. During the austral fall season, the simulated trends are half the observed value and occur whether or not ozone forcing is present. In the austral winter (labelled JJA), the observed and simulated trends are not significant. In the austral spring (labelled SON), there is a poorly understood discrepancy between the models and observations: the models with ozone loss produce a positive Southern Annular Mode trend when none is observed in that season (Fogt et al. 2009). (Solomon, Susan, NOAA)	Agreed. Text revised.
1359	3	53	26	53	26	Osborn (Clim.Dyn, 2004, doi:10.1007/s00382-004-0405-1) and van Oldenborgh et al (Clim.Past, 2009, doi:10.5194/cp-5-1-2009) also show that the NAO does not describe the observed trend very well. This is a point well worth emphasising, as there still is a tendency to believe that climate change will project stringly on these modes. (van Oldenborgh, Geert Jan, KNMI)	Relationship of these papers to point being made in text is obscure.
1360	3	53	31	53	32	According to Stephenson et al., 2006 (Stephenson DB, Pavan V, Collins M, Junge MM, Quadrelli R, participating CMIP2 modelling groups (2006) North Atlantic Oscillation response to transient greenhouse gas forcing and the impact on European winter climate: a CMIP2 multi-model assessment. Clim Dyn 27:401-420) almost all models producing a NAO are simulating a shift towards more positive index values. This should be mentioned in addition to the spread of signals. (Ulbrich, Uwe, Freie Universitaet Berlin)	Text revised.
1361	3	53	43	53	48	Woolings et al. (2010) was not the first paper to comment on a weak positive trend in NAO due to the greenhouse gas trend. A similar conclusion was noted earlier by Stephenson, D.B., Pavan, V., Collins, M., Junge, M.M., Quadrelli, R. and participating CMIP2 modelling groups (2006): North Atlantic Oscillation Response to Transient Greenhouse Gas Forcing and the Impact on European Winter Climate: A CMIP2 multi-model assessment, Climate Dynamics, 27, pp 401-420. (Stephenson, David, University of Exeter)	Yes, but the focus here should be on more recent work, not a comprehensive review of all work.

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1362	3	53	47	53	47	I think this should read "offset local warming" (van Oldenborgh, Geert Jan, KNMI)	Check.
1363	3	53	50	0	0	As a general comment, it is not always clear which region/ocean basins are being referred to in this section. Please clarify where necessary and highlight regional differences between ocean basins to the extent possible. So far the assessment appears mostly to be covering the Atlantic Ocean, and much less Pacific and/or Indian Oceans. This might simply reflect the available literature, but this needs to be indicated. As a specific example, many other SREX chapters refer to tropical cyclones impacting Africa, so the appropriate scientific assessment needs to appear here to support or correct their statements. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Text has been expanded / clarified wherever possible.
1364	3	53	50	0	0	It would be good if specific mention of category 4 & 5 cyclones could be made in this section - this will be of crucial interest to governments. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Text has been added.
1365	3	53	50	0	0	We would strongly encourage that a figure is included within the Tropical Cyclone section. This is an important and highly topical section of Chapter 3, and a relevant and informative figure will be well received. Perhaps something from Knutson et al 2010? (Stocker, Thomas, IPCC WGI TSU)	This suggestion was taken under serious consideration and discussed among the lead authors, but it's not clear how to do this, and there's a concern that creating a figure may emphasize one aspect over another. We chose to not attempt to add a figure.
1366	3	53	50	0	0	Section 3.4.4 on "Tropical Cyclones": suggest to delete the rather long citations of the AR4 on pages 55 and 56 (Stocker, Thomas, IPCC WGI TSU)	This is giving a somewhat mixed message. We have been following the procedure of stating previous assessment likelihood statements, and then stating the new ones. We feel that this is a very effective way to allow the reader to make accurate comparisons.
1367	3	53	60	53	61	A citation is needed to support this 'great majority of damage and loss of life' statement. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Citations added.
1368	3	54	15	54	15	Records in the Australian region extend back to 1906, but are very incomplete for the first half of the 20th century. (Trewin, Blair, Australian Bureau of Meteorology)	Right. And the newer ibtracs data makes our original statement somewhat obsolete as well. The text has been reworded accordingly.
1369	3	54	35	54	0	By studying the annual cycle rather than trend over time, however, it may be possible to bypass some of the problems concerning inhomogeneities, and an analysis of the seasonal relationship between the area of sea surface temperature higher than 26.5C – the threshold temperature above which tropical cyclones form - and the number of cyclones suggests a highly statistically significant link (Benestad, 2009, 'On Tropical Cyclone Frequency and the Warm Pool Area' Nat. Hazards Earth Syst. Sci., 9, 635-645). One caveat using the annual cycle is that one common driver – e.g. the annual variation in solar declination – may be responsible for both, however, using the relationship established for the annual cycle on year to year variations gave significant correlation scores for the Atlantic (the basin with the best quality observations). The relationship between the number of tropical cyclones and warm sea surface area moreover had similar character for the north-west Pacific and the north Atlantic, both indicating a non-linear relationship between the two. For this reason, a linear trend in the warm sea surface area is not expected to produce a linear trend in the number of cyclones, and the response in the frequency will only be substantial beyond a critical size. This relationship may therefore explain why no trend in the number of tropical cyclones yet has been detected. For the Indian Ocean, the relationship between the number of cyclones and warm sea area was different to the other two basins, probably due to the ocean basin being bounded in the north and the area restricted in size. These results are directly relevant for bullet point 1 on p. 55. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Thanks for providing these insights. It's true that the observed variability supports these statements, but the problem comes later when the SST changes due to greenhouse forcing are deconvolved from the natural variability. There is a large and growing body of literature that shows that the SST threshold is not stationary. Substantial text has been added to better address this issue.
1370	3	54	61	54	0	Did Lau et al 2008a find significant changes in cyclone-related rainfall, and there are few other studies? Perhaps consider rephrasing this paragraph. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Thanks, we've rephrased the sentence.
1371	3	55	26	55	26	Suggest replacing "likely confidence" with "likelihood or confidence" since likelihood and confidence are separate scales and concepts. (Zwiers, Francis, Environment Canada)	Thanks, the text has been modified.
1372	3	55	30	55	31	The sentence does not match with the text literature. It is better to eliminate the word "our" as well. (Davtalab, Rahman, Ministry of Energy)	Thanks, the text has been modified
1373	3	55	32	55	40	Some of these conclusions from the observations stay into detection and attribution which should be left until the next section. (Stocker, Thomas, IPCC WGI TSU)	Agreed. The bullets have now been separated into the appropriate sections.
1374	3	55	41	56	44	3.4.4.2. Is the difference between Ch3 and Hegerl in the AR4 based on the extra information available now, or is it based on the conclusion that the earlier paper overstated confidence? (IPCC WGII TSU)	It is the former. We have been careful to use language such as "Based on subsequent research (i.e., subsequent to AR4) that further elucidated the scope of uncertainties in the historical tropical cyclone data,..."
1375	3	55	41	0	0	Section 3.4.4 on "Tropical Cyclones": what about wind-shear? Should probably be discussed too, in the "Causes behind the Changes" section 3.4.4.2 (Stocker, Thomas, IPCC WGI TSU)	Text has been added. Also in section 3.4.4.3
1376	3	55	42	55	54	The importance of not just SST but also wind shear should be brought in here, and it should be noted that while there is confidence in warming of SSTs, there is little confidence in the understanding of wind shear changes in the upper troposphere in the tropics, both for the past and future. (Solomon, Susan, NOAA)	Agreed. Clarifying text has been added to this section and the section on projections (see also the response to Michael Wehner's comment 1411 below).
1377	3	55	56	55	0	The depth of the thermocline is also important, as mixing and upwelling caused by strong winds can cool the surface and switch off the tropical cyclones. Furthermore, the vertical wind shear and organised disturbances – such as Easterly African Waves – are often needed for cyclone genesis. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Agreed, but this may be entering into too fine a detail for an assessment report.

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1378	3	56	2	56	4	How strong is this relationship? Some individual cyclones can intensify at a much stronger rate than this. (Trewin, Blair, Australian Bureau of Meteorology)	Agreed, but this is the climatological mean (as stated in Emanuel 2000 and not too dissimilar to Dvorak's climatology in terms of T-numbers per day). The point is now moot though as the section has been removed.
1379	3	56	4	56	11	Is this paragraph on the relationship between intensity and duration of tropical cyclones crucial for the assessment? Could it be deleted? (Stocker, Thomas, IPCC WGI TSU)	It's agreed that this paragraph is less relevant than the others and has been removed to allow space for other more relevant additions.
1380	3	56	5	0	0	Please specify what a 'favourable environment' is (Luterbacher, Juerg, Justus Liebig University)	The paragraph has been removed.
1381	3	56	6	56	6	Which aspect of tropical cyclone wind speeds is it that intensifies at this rate? (Zwiers, Francis, Environment Canada)	This referred to maximum sustained surface wind speed. But the section has been removed and the point is now moot.
1382	3	56	6	0	0	Are the units ok? (Luterbacher, Juerg, Justus Liebig University)	Yes, the units were correct. But the section has now been removed.
1383	3	56	13	56	0	GCMs have been used to infer trends in tropical cyclones, but the confidence in their results is weak. The GCMs first need to demonstrate that they are able to reproduce the relationship between SST and intensity, warm sea surface area and frequency (e.g. the seasonal cycle), and between ENSO and frequency, before we can have confidence in them projecting changes due to changes in the boundary conditions. One problem is the coarse resolution in the GCMs and the insufficient/missing small -scale details, such as cloud micro-physics, air-sea interaction, and small-scale dynamics. The tropical cyclones may also effect their ambient climate – e.g. in terms of energy and mass flow. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Agreed.
1384	3	56	32	39	0	The chapter seems to suggest to revisit the assessment of attribution of change in tropical cyclone intensity, countering with the fact that the WMO report does not draw a conclusion with regard to detection of changes in TCs. However, the detection of change in a variable of interest is not necessarily a prerequisite of attributing changes - note the good practice guidance paper on attribution does no longer require detection in the same variable. Based on the GPGP, it would be fine to detect and attribute a change in SST, and then, if the authors are convinced about a strong enough physical link for 'more likely than not' attribution of increases in frequency to that change, draw the attribution conclusion. This is basically what we did in AR4 - say we would be willing to bet larger than even odds that the increase in SSTs has contributed to changes in various intensity indices. I would recommend against changing the AR4 conclusion unless the authors have strong literature supporting a downgrade of the assessment, eg findings that lead to questions about an assessment even as weak as 'more likely than not', or drastic drops in the observed timeseries. I apologize that this is rushed, the deadline came in the middle of another string of deadlines for me. (Hegerl, Gabriele, Univ of Edinburgh)	This is an important point being raised, and substantial text has been added to address this. There is a strong body of post-AR4 literature that has emphasized the uncertainties in the data and has introduced substantial uncertainty into our understanding of the physical mechanisms at work. Because of these uncertainties related to the observed SST/TC relationships, a multi-step approach cannot be applied confidently. Using the new AR5 guidance notes, it is now stated the "there is low confidence that any reported long-term increases in tropical cyclone activity are robust, after accounting for past changes in observing capabilities".
1385	3	56	32	56	44	I don't agree with the argument as it is presented here. As we elucidated in the detection and attribution guidance note by Hegerl et al, detection is not a pre-requisite for attribution. For extreme events you would not necessarily expect to detect a significant change in the occurrence of the extreme events themselves since they are rare events but you could see a very significant signal in underlying changes (eg mean regional temperatures or regional SSTs) which are strongly linked to the occurrence of the extreme event, the probability of which could therefore be attributed to anthropogenic influence. If Knutson et al (2010, WMO report) are arguing that the lack of a detection of tropical cyclone metrics precludes per se the attribution of the most intense tropical cyclones, then this is not supported by the IPCC guidance note on detection and attribution. If this assessment is backing off the assessment of AR4 re tropical cyclones then it needs to do so on the basis of a weakening of the evidence presented there for human influence, not on the basis of a lack of detectability. (Stott, Peter, Met Office)	Thanks for these insights. Please see response #1384 above.
1386	3	56	32	0	39	One note of clarification here. I don't believe that the AR4 ever claimed that there was a detectable (i.e., significant compared to natural variability levels) change in TC activity. Rather, they had a very weak (more likely than not) expert judgement statement that anthropogenic forcing had contributed to observed changes in TCs. Thus, the difference between AR4 and Knutson et al. (2010) is really quite small. Neither report claims a detectable change in TC activity, and one report (AR4) makes an expert judgement weak attribution statement about a human contribution, while the Knutson et al. report makes no expert judgement statement along those lines one way or the other. (Knutson, Thomas, GFDL/NOAA)	Thanks, this point is appreciated.
1387	3	56	41	56	44	I guess that this conclusion is nevertheless not inconsistent with model-simulated changes to TC behavior. I think it might be good to note this, lest this weakening of the AR4 conclusion does not get over-interpreted. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Thanks, agreed. This was the motivation for including a full paragraph discussing the projections of Bender et al 2010 and Knutson and Tuleya (2004) in this section. It's felt that additional text regarding projections might skew the balance between this section and the following section on projections.

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1388	3	56	41	0	0	Noteworthy is that the review paper in WIREs by Stott et al. (2010) does not come to the same conclusion. (Klein Tank, Albert, KNMI)	Thanks. The conclusions of this paper are not incongruent with the statements made here. Stott et al state: "Attributing causes to changes in the frequency and intensity of hurricanes has remained very controversial. Two studies,106,107 have shown that human-caused changes in greenhouse gases are the main driver of the observed 20th-century increases in sea surface temperatures in the main hurricane formation regions of the Atlantic and the Pacific. However, the importance of the anthropogenic increase in sea surface temperature in the cyclogenesis region for past and future changes in hurricane activity is still poorly understood.108 The limitations of the observed database and of current climate models in resolving processes relevant for hurricanes make progress in this field difficult at present."
1389	3	56	43	0	0	I would say "...do not presently allow for the attribution of any *detectable* observed changes in tropical cyclone activity..." One could in principle still make an expert judgement attribution statement of some observed change that is below the detection threshold, as done in AR4. This seems to me a matter of choice of the authors--whether they want to include any attribution statements based on expert judgement in the absence of a detectable change. I don't think there is anything inherently wrong in doing this per se. It's just a choice for the authors to make. (Knutson, Thomas, GFDL/NOAA)	Agreed, and the change has been made. Regarding introducing an expert judgement in the absence of a detectable change, the choice here was to largely essentially the WMO expert team report choice.
1390	3	56	46	0	0	Kim et al 2010 suggest predictions of tropical cyclones should not be made with an uncoupled model in the Pacific. Not sure where this should go. (Joo-Hong Kim, Simon J. Brown, and Ruth E. McDonald, Future changes in tropical cyclone genesis in fully dynamic ocean- and mixed layer ocean-coupled climate models: a low-resolution model study, Climate Dynamics, accepted) (Brown, Simon, The Met Office Hadley Centre)	Thanks for pointing us to this. It's not clear how to apply this as part of the assessment (it would fit more naturally into a review rather than an assessment), but the citation will be very useful for the AR5's Ch.14 dealing with regional variability.
1391	3	56	48	57	58	Section 3.4.4.3. It would be good to read somewhere here about what might be causing the projected decrease to TC numbers (Whetton, Pennv. CSIRO Marine and Atmospheric Research)	Agreed. Text has been added.
1392	3	56	56	56	56	Replace "CMIP" with "CMIP3". (Zwiers, Francis, Environment Canada)	Done.
1393	3	56	57	56	57	Randall et al., 2007 ----> Randall et al., 2007; Walsh et al., 2010 Ref: Walsh, K., S. Lavender, H. Murakami, E. Scoccimarro, L.-P. Caron, and M. Ghantous, 2010: The tropical cyclone climate model intercomparison project, Hurricanes an climate change, J.B. Elsner and T.H. Jagger Eds., Springer, In press. (Kusunoki, Shoji, Meteorological Research Institute (MRI))	Thanks, the citation has been added.
1394	3	56	59	56	56	The following citation could be added to the list Michael F. Wehner, G. Bala, Phillip Duffy, Arthur A. Mirin, and Raquel Romano, "Towards Direct Simulation of Future Tropical Cyclone Statistics in a High-Resolution Global Atmospheric Model," Advances in Meteorology, vol. 2010, Article ID 915303, 13 pages, 2010. doi:10.1155/2010/915303 This study does differ in its conclusions than the others cited in that the total number of tropical cyclones increased and is somewhat at odds with the discussion beginning on line 51, page 57. (wehner, Michael, Lawrence Berkeley National Laboratory)	Thanks. The citation has been added here. Also, it has been added later in this section along with some related text about causes behind the frequency changes.
1395	3	56	59	56	59	Zhao et al., 2009 ----> Zhao et al., 2009; Walsh et al., 2010 Ref: Walsh, K., S. Lavender, H. Murakami, E. Scoccimarro, L.-P. Caron, and M. Ghantous, 2010: The tropical cyclone climate model intercomparison project, Hurricanes an climate change, J.B. Elsner and T.H. Jagger Eds., Springer, In press. (Kusunoki, Shoji, Meteorological Research Institute (MRI))	Thanks, the citation has been added.
1396	3	57	1	57	1	A recent reference in this context is: Done, J.M., G.J. Holland, and P.J. Webster, 2010: The role of wave energy accumulation in tropical cyclone genesis over the tropical North Atlantic. Climate Dynamics, DOI 10.1007/s00382-010-0880-5 (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Thanks. This is a theoretical paper using idealized model simulations to explore the dynamics of wave accumulation and TC genesis, and doesn't explore climate change connections in any way. So it is perhaps too specific and off-topic to fit well in this assessment report.
1397	3	57	7	57	0	But the model projections contrast with analysis based on empirical data, suggesting that the number of tropical cyclones is very sensitive to the area of warm sea surface once the size of this area grows large (Benestad, 2009, 'On Tropical Cyclone Frequency and the Warm Pool Area' Nat. Hazards Earth Syst. Sci., 9, 635-645). (Benestad, Rasmus, The Norwegian Meteorological Institute)	Please see response #1369 above.
1398	3	57	7	0	0	This sentence needs a more clear focus and formulation (Luterbacher, Juerg, Justus Liebig University)	Your point is well taken, and text has been added to improve the focus of the statement.
1399	3	57	11	57	11	Bender et al., 2010 ----> Bender et al., 2010; Murakami and Sugi, 2010 Ref: Murakami, H., and M. Sugi, 2010: Effect of model resolution on tropical cyclone climate projections. SOLA, 6, 73-76. http://www.jstage.jst.go.jp/article/sola/6/0/73/_pdf/-char/ja/ (Kusunoki, Shoji, Meteorological Research Institute (MRI))	The citation has been added.
1400	3	57	15	0	0	"...increased precipitation rates *in the models for which these metrics were examined*." (Not all of the studies in the list above the sentence examined TC precipitation or numbers of intense TCs.) (Knutson, Thomas, GFDL/NOAA)	Agreed, the change has been made.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1401	3	57	16	0	0	Knutson et al. report intensity changes between 2 and 11%, not 3 and 11%. (Knutson, Thomas, GFDL/NOAA)	Thanks, the change has been made.
1402	3	57	20	0	0	Bender et al. project an 80% increase in the frequency of Saffir-Simpson category 4 and 5 hurricanes over the next 80 yr (A1B scenario). One should include the time horizon that goes with the change; the 75% number in Bender et al. refers to a smaller sample of years that was included in the table 1 as an extra column to have a point of comparison with some individual model runs that also used a smaller sample of years. The main result (80%) is based on twice as many years and should be more robust (in the context of the Bender et al model). (Knutson, Thomas, GFDL/NOAA)	Agreed, thanks. The change has been made.
1403	3	57	24	57	29	Could have a cross link here to a part of the chapter that describes the basic assumptions in statistical downscaling. (Zwiers, Francis, Environment Canada)	Good idea. A link has been added.
1404	3	57	31	0	0	"about 300% in the next century." You could list Knutson et al. (2010) as the reference for this. I think it needs some reference. Alternatively, you could cite Vecchi et al.; they didn't present the changes in percentage terms in their figure, but one can back out the 300% number from the information they provide. (Knutson, Thomas, GFDL/NOAA)	Agreed. The citations have been added.
1405	3	57	32	0	0	It should be "tropical-averaged SST" and not "globally-averaged SST" (Knutson, Thomas, GFDL/NOAA)	Thanks.
1406	3	57	35	57	36	Is this the authors' assessment or is there debate in the literature (in which case a reference should be given). (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Agreed. A reference has been added.
1407	3	57	37	0	0	I suggest to add a new sentences "Despite neglecting detail dynamics of tropical cyclone, a stochastic typhoon model is useful to check the sensitivity of specific cyclone characteristics such as cyclogenesis latitude change. Assuming future cyclogenesis in the North West Pacific Ocean shift by 2.4 degree to the north and 4.7 degree to the east and number of typhoon decrease, Yasuda et al. (2010) obtained that the number of typhoon decreases around Japan by the stochastic typhoon model. The detail information of tropical cyclone projection such as intensity, cyclogenesis location and etc are required to understand the future tropical cyclone change at middle latitude, quantitatively." The English reference to be added is "Yasuda, T. and H. Mase and N. Mori (2010) Projection of future typhoons landing on Japan based on a stochastic typhoon model utilizing AGCM projections, Hydrological Research Letters, Vol.4, pp.65-69. (doi:10.3178/HRL.4.65)." (Nakakita, Eiichi, Kyoto University)	Thanks. This information is important, but is perhaps too specific in its nature for the purpose of an assessment (it would fit better in a review paper).
1408	3	57	40	0	0	"+3" should be "+2" (Knutson, Thomas, GFDL/NOAA)	Thanks, the change has been made.
1409	3	57	46	0	0	"Models *in which tropical cyclone precipitation rates have been examined* are highly consistent..." (This caveat is needed because not many of the existing studies have looked at TC precipitation in a changing climate. (Knutson, Thomas, GFDL/NOAA)	Agreed. The change has been made.
1410	3	57	49	0	0	likely' statement in italic (Stocker, Thomas, IPCC WGI TSU)	It's unclear why this and not others. Are italics to be used for our likelihood statements as well as those of previous assessment reports?
1411	3	57	51	57	58	Confidence in future projections in particular basins is undermined by the inability of global models to reproduce details at scales relevant to tropical cyclone genesis and evolution. Of particular concern in the North Atlantic is wind shear. If the number of future Atlantic hurricanes is modulated by changes in wind shear, confidence in the statement that there is no change in frequency is weakened as GCMs are not known to accurately simulate wind patterns and profiles. (Wehner, Michael, Lawrence Berkeley National Laboratory)	These are good points (and they address a similar theme to the comment (#1376) of Susan Solomon above). Text has been added to better convey this to the reader.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1412	3	58	1	58	10	This paragraph is rather short and incomplete. Extratropical cyclones include mid-latitude cyclones (where baroclinic instability is the major energy source, as written in the text) but also so-called polar lows for whose energetics surface fluxes are crucially important (see e.g., Zahn M, von Storch H, 2008. A long-term climatology of North Atlantic polar lows. Geophys. Res. Lett., 35, article number: L22702; Bracegirdle TJ, Gray SL, 2008. An objective climatology of the dynamical forcing of polar lows in the Nordic seas. Int. J. Climatol., 28, 1903-1919). The final sentence of this paragraph is highly important, also in the context of climate warming under the assumption of on average constant relative humidity conditions: latent heating has indeed been identified as a key component for the intensification of extratropical cyclones. Given its importance, further references and a slightly more detailed discussion might be appropriate. Relevant studies include, e.g., Davis, C. A., Stoelinga, M. T. and Kuo, Y.-H. 1993 The integrated effect of condensation in numerical simulations of extratropical cyclogenesis. Mon. Weather Rev., 121, 2309–2330; Mallet, I., Cammas, J.-P., Mascart, P. and Bechtold, P. 1999 Effects of cloud diabatic heating on the early development of the FASTEX IOP17 cyclone. Q. J. R. Meteorol. Soc., 125, 3439–3467; Rossa, A. M., Wernli, H. and Davies, H. C. 2000 Growth and decay of an extra-tropical cyclone's PV-tower. Meteorol. Atmos. Phys., 73, 139–156. In addition to intensifying baroclinic-instability driven mid-latitude cyclones, diabatic processes are also key for a special category of extratropical cyclones, so-called diabatic Rossby waves, that can undergo explosive intensification as documented for instance for the strongly damaging Central European storm "Lothar" in 1999 (Wernli, H., S. Dirren, M. A. Liniger, and M. Zillig, 2002. Dynamical aspects of the lifecycle of the winter storm "Lothar" (24-26 December 1999). Quart. J. Roy. Meteor. Soc., 128, 405-429) and for an US east coast cyclone event (Moore RW, Montgomery MT, Davies HC, 2008. The integral role of a diabatic Rossby vortex in a heavy snowfall event. Mon. Wea. Rev., 136, 1878-1897). (Wernli, Heini, ETH Zürich)	Agree with the reviewer that this is short and incomplete, we have however added a paragraph on the relation to the large scale flow, but the chapter is already 20% above the pagelimit set by IPCC, so we have decided not to expand on the theoretical details of cyclone formation and intensification. We have also added some text on mesoscale cyclone formation and development in the text.
1413	3	58	3	3	5	The main role of extratropical cyclones in the climate system is the poleward transport of heat in the mid-latitudes, or, more precisely, the reduction of large-scale horizontal temperature gradients. This is a different context than the local effects, and should not be seen as "additional". (Ulbrich, Uwe, Freie Universitaet Berlin)	Wording is changed to reflect this.
1414	3	58	8	58	8	"..., which is a reservoir of available potential energy (APE)" (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Noted and incorporated in the text
1415	3	58	12	59	36	Section 3.4.5.1 on "Observed Changes in Extratropical Cyclones": the section reads more like a review than an assessment. All the material seems there, but it now needs to be synthesized and assessed. (Stocker, Thomas, IPCC WGI TSU)	Text is substantially shortened and synthesized
1416	3	58	19	58	24	The uncertainties in reanalysis for such an application are very large. It would be better to say this in one sentence and avoid the list of studies that have been done despite this severe limitation. Until the data are improved, the studies trying to use it really don't mean much and that should be the message. (Solomon, Susan, NOAA)	Text to reflect this is added: "Even though different reanalyses correspond well in the Northern Hemisphere lower troposphere (Hodges et al., 2003; Hanson et al., 2004) changes in the observing system giving artificial trends in integrated water vapor and kinetic energy (Bentsson et al., 2004) may have influenced trends in both the number and intensity of cyclones" and it is noted in the conclusions: "Several studies using reanalysis indicate an intensification of high latitude cyclones, but there is still insufficient knowledge of how changes in the observational systems are influencing the reanalysis "
1417	3	58	19	58	24	A summary of recent results on observed cyclone trends in the Northern Hemisphere (and on the Southern hemisphere in section 3.3.2) is found in section 3.3.1 of the review published by Ulbrich et al., 2009 (Ulbrich, U., G.C. Leckebusch, J. Pinto, 2009: Extra-tropical cyclones in the present and future climate: a review. Theo. Appl. Climatology, 96, 117-131. DOI 10.1007/s00704-008-0083-8). (Ulbrich, Uwe, Freie Universitaet Berlin)	Noted.
1418	3	58	26	58	31	This paragraph is more about winds than cyclones. Although it can be assumed that most of these wind signals are linked to the passage of cyclones, I would prefer shifting these references to the wind section (3.3.3). (Wernli, Heini, ETH Zürich)	Paragraph is moved to wind section
1419	3	58	28	58	28	Suggest add "Wang et al. 2009b" right after "Wang et al. 2008" here. (Wang, Xiaolan, Environmen Canada)	This is added and paragraph is moved to wind section
1420	3	58	28	58	40	Wind is addressed in section 3.3.3. and should be moved. You may want to add Leckebusch et al., 2008 (Leckebusch, G.C., D. Renggli, U. Ulbrich, 2008: Development and Application of an Objective Storm Severity Measure for the Northeast Atlantic Region. Meteorol. Z., 17, 575-587.) as they find an increasing trend in North Atlantic Storm events. (Ulbrich, Uwe, Freie Universitaet Berlin)	Paragraph is moved to wind section
1421	3	58	30	58	31	This is repetition (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Paragraph removed (moved to wind section)
1422	3	58	30	0	31	all over the mid-latitudes? Cite reference to clarify this statement (Stocker, Thomas, IPCC WGI TSU)	Paragraph is moved to wind section and statement clarified to be valid for north Atlantic and coastal Europe
1423	3	58	36	58	38	The linkage made between this increasing trend and the one in the previous sentence seems inappropriate given the vastly different time periods (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Paragraph has changed and it is now noted that both papers have trends over the last few decades
1424	3	58	44	58	46	Note that Raible et al. (2008) state that cyclone numbers from different schemes they used give opposing trends for the Pacific, and cyclone intensity could not be compared between different schemes. Some more results are summarised in Ulbrich et al., 2009 (Ulbrich, Uwe, Freie Universitaet Berlin)	This has now been added to the text: "...", but signs of some of the trends disagreed when different tracking algorithms or reanalysis products are used (Raible et al., 2008)."

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1425	3	58	45	58	45	Insert ", which uses reanalyses,"; otherwise the note on Chang (2007) seems a bit stranded. (Zwiers, Francis, Environment Canada)	noted
1426	3	58	0	0	0	An interesting (novel) topic to add would be the question of how well climate models capture the structure of cyclones. The following paper addresses this issue: Catto JL, Shaffrey LC, Hodges KI, 2010. Can Climate Models Capture the Structure of Extratropical Cyclones? J. Climate. 23. 1621-1635. (Wernli, Heini, ETH Zürich)	A paragraph on models ability to capture the structure of cyclones and geographical location of the the storm track is added
1427	3	59	5	59	0	It is true that different re-analysis give different results, but this may not be the whole story; different methods for identifying cyclones and tracking these may give different results. Often different studies use slightly different approaches for analysing cyclones, and it is therefore not clear how much of the differences are due to different re-analyses and how much is due to different method. (Benestad, Rasmus, The Norwegian Meteorological Institute)	This is now incorporated in the text: "..., but signs of some of the trends disagreed when different tracking algorithms or reanalysis products are used (Raible et al., 2008)."
1428	3	59	6	0	0	systems -> low pressure systems (Stocker, Thomas, IPCC WGI TSU)	Changed
1429	3	59	8	59	8	Define cyclone density. (Zwiers, Francis, Environment Canada)	Density now changed to number of cyclones
1430	3	59	17	59	36	This summary is much too long and repetitive. A more concise summary, including likelihood statements as reported in Table 3.1 should be given. Not clear in Line 17 whether a 'poleward' shift related to both northern and southern hemispheres? (Stocker, Thomas, IPCC WGI TSU)	Summary is shortened and poleward shift statement is changes to make it clear that it relates to both northern and southern hemisphere stormtracks
1431	3	59	19	0	0	"a" -> "an" (Van den Hurk, Bart, KNMI)	noted
1432	3	59	23	0	0	"points" -> "point" (Van den Hurk, Bart, KNMI)	noted
1433	3	59	27	0	0	"a" -> "an" (Van den Hurk, Bart, KNMI)	Noted
1434	3	59	33	59	33	Suggest add "Want et al. 2009b" right after "Matulla et al. 2008" here. (Wang, Xiaolan, Environmen Canada)	This part of text is moved to storminess chapter
1435	3	59	33	0	0	Delete the references since it is a summary (Luterbacher, Juerg, Justus Liebig University)	Deleted (but references are in general not prohibited in summary)
1436	3	59	34	0	0	After '...and how the influence of reanalysis inhomogeneities', insert: 'or the choice of the analysis technique'. Reason: see comment no. 3 and refs therein above (Neu, Urs, Swiss Academy of Sciences)	Noted
1437	3	59	38	60	33	3.4.5.2. All of the sections labeled "Causes behind the changes" are interesting, but they sometimes wander into a mode that feels more like a textbook than an IPCC assessment. For the cases where the causes are not well known and/or there are no clear trends, the discussion of causes gets to be pretty academic. (IPCC WGII TSU)	This section has been shortened.
1438	3	59	57	59	62	I think I would take studies identifying intermittent relationships with a grain of salt - one suspects that they are often diagnosed as much for statistical reasons (low power tests due to the limited sample sizes needed to assess intermittent relationships) as they are for physical reasons. I'm not sure that intermittency would always be a symptom of non-stationarity (Zwiers, Francis, Environment Canada)	Noted, we mention intermittency, but with no mentioning of possible non-stationarity. A few papers looking into the physical movement of the center of actions are given (Vicente-Serrano and Lopez-Moreno, 2008; Zhang et al. 2008h)
1439	3	59	59	59	59	Suggest add "Want et al. 2009b" right after "Pinto et al. 2009" here. (Wang, Xiaolan, Environmen Canada)	Noted
1440	3	60	7	60	8	I don't understand this implication. In fact, wind storm is also related to the mean pressure gradient (e.g., Fink et al., 2009). Fink AH, Brucher T, Ermert V, Kruger, A., Pinto, J.G., 2009: The European storm Kyrill in January 2007: synoptic evolution, meteorological impacts and some considerations with respect to climate change. NATURAL HAZARDS AND EARTH SYSTEM SCIENCES, 9, 405-423, and this can imply a NAO anomaly related to storminess. This does not detract from or even exclude a NAO effect on cyclones. In fact, Pinto et al. (1999) give ample evidence of such influences. Pinto, J.G., S. Zacharias, A.H. Fink, G.C. Leckebusch, und U. Ulbrich, 2009: Factors contributing to the development of extreme North Atlantic cyclones and their relationship with the NAO. Climate Dynamics, 32, 711-737 doi: 10.1007/s00382-008-0396-4. (Ulbrich, Uwe, Freie Universitaet Berlin)	The text is no longer in the document
1441	3	60	8	60	9	There is also an influence of the PNA on the Atlantic storm track, which over the area of maximum intensity is even larger than the NAO influence (Pinto et al., 2010). Pinto, J.G., M. Reyers, und U. Ulbrich, 2010: The variable link between PNA and NAO in observations and in multi-century CGCM simulations. Climate Dynamics, DOI 10.1007/s00382-010-0770-x (Ulbrich, Uwe, Freie Universitaet Berlin)	This has been added, but if I have understood the paper correctly this was not significant in the reanalysis?
1442	3	60	14	60	23	I think it would also be important to put the assessment of the last 50-years into the context of longer surface pressure records and derived geostrophic winds from pressure triangles that, regionally in the eastern North Atlantic and western Europe, seem to suggest that low frequency variability is the dominant feature of change. (Zwiers, Francis, Environment Canada)	Text is added to the summary to reflect this: "... the poleward shift should be seen in light of new studies with longer time spans indicating that last 50 years coincide with relatively low cyclonic activity in northern coastal Europe in the beginning of the period and reaches a maximum in the 1990s"
1443	3	60	15	60	16	Unclear what the phrase "are inconsistent with natural variability" implies, or what the inconsistency is (Van den Hurk, Bart, KNMI)	Text is removed
1444	3	60	32	60	33	Should the discrepancy between modelled and observed changes (mentioned in the previous paragraph) not also be mentioned as a factor precluding a quantitative detection a(nd attribution)? (van Oldenborgh, Geert Jan, KNMI)	This is now taken into account and the likelihood statement is reduced to as likely as not.
1445	3	60	35	61	51	3.4.5.3 Here and in many other places, it is not clear that the assessment of likelihood is based on a comprehensive statistical analysis. If it is not, authors should consider switching to the confidence scale. (IPCC WGII TSU)	The new IPCC uncertainty language has been adopted and likelihood/confidence statemts are changed based on that.
1446	3	60	35	61	51	3.5.2.3. Here too, it seems like the summary conclusions should perhaps use the confidence scale, unless they are backed up by a formal uncertainty analysis. (IPCC WGII TSU)	See comment above

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1447	3	60	35	0	0	Section 3.4.5.3.: All references should include the time periods to which the results refer (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	This is difficult to do in a short assesment. When statements are made based on several results this is often generalized to last 50 years, end of century etc.)
1448	3	60	35	0	0	A summary of recent papers is found in Ulbrich et al., 2009. (Ulbrich, Uwe, Freie Universitaet Berlin)	Noted
1449	3	60	35	0	0	I found this section difficult to follow (Brown, Simon, The Met Office Hadly Centre)	It has been rewritten ans shortened
1450	3	60	40	60	45	Run on sentence. (Zwiers, Francis, Environment Canada)	Noted and changed
1451	3	60	47	60	47	This shuld be quoted here. U. Ulbrich, G. C. Leckebusch and J. G. Pinto, Extra-tropical cyclones in the present and future climate: a review, <i>Theoretical and Applied Climatology</i> (Brown, Simon, The Met Office Hadly Centre)	The reference is added.
1452	3	60	50	60	0	If there only is the paper by Lorenz and DeWeaver 2007, then it is wrong to talk about studies in plural. Either give reference to more papers in the paragraph, or skip the citation. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Two more references added: O'Gorman, 2010; Wu et al., 2010
1453	3	60	53	60	53	Ulbrich et al. (2008) use a storm track based on MSLP data. They do not find a poleward shift, but prominent regional increases of the storm track activity over the Eastern North Atlantic/Western European area and over the Aleutian islands. (Ulbrich, Uwe, Freie Universitaet Berlin)	Text has been changed to take this into account: "Using bandpassed mean sea level pressures from 16 AR4 coupled GCMs, Ulbrich et al., (2008) show a regional increases of the storm track activity over the Eastern North Atlantic/Western European area and over the Aleutian islands and a decrease in the southern part of the north Pacific. "
1454	3	60	57	60	57	This should be quoted here. Ruth E. McDonald Understanding the impact of climate change on northern hemisphere extra-tropical cyclones. <i>Clim. Dyn.</i> 2010 DOI: 10.1007/s00382-010-0916-x (Brown, Simon, The Met Office Hadly Centre)	Reference added
1455	3	61	4	61	15	Leckerbusch, Renggli and Ulbrich (2008) <i>Meteorologische Zeitschrift</i> , Vol. 17, 575-587 These authors indicate that over the North-east Altantic and Western Europe it is possible to identify statistically significant positive trends of their index of severity of storms both in ERA40 data and in model data under SRES A1B and A2 scenario conditions. They also notice that there is a 10% increase in spatial extent if storms between present and scenario model simulations. Pinto, Neuhaus, Leckerbusch, Reyes and Kerschgens (2010) <i>Tellus</i> , 62A, 188-201 use instead a statistical-dynamical downscaling approach to estimate changes in wind storm impacts over Western Germany under scenario conditions using ECHAM5 CGCM integrations. (Bayon, Valentina, ABPA Emilia Romagna)	The winds storm indicators are moved to storminess section to avoid overlapp.
1456	3	61	8	61	8	McDonald 2010 also report a southward shift in the eastern end of the N atlantic storm track that was related to a change in the baroclinicity due to a local minimum in the SST warming in the central Atlantic. (Brown, Simon, The Met Office Hadly Centre)	This is now added to the text: "Over the North Atlantic stormtrack many models show an eastward or southeastward extension of the stormtrack (Ulbrich et al., 2008; Laine et al., 2009; Pinto et al., 2007; McDonald, 2010) "
1457	3	61	17	61	23	Since this paragraph discusses the mechanisms behind the changes, it should be moved to the previous section (3.4.5.2.) (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	It is moved.
1458	3	61	17	61	23	An in-depth analysis of the mechanisms was also performed by Pinto et al., (2009): Pinto, J.G., S. Zacharias, A.H. Fink, G.C. Leckebusch, und U. Ulbrich, 2009: Factors contributing to the development of extreme North Atlantic cyclones and their relationship with the NAO. <i>Climate Dynamics</i> , 32, 711-737 doi: 10.1007/s00382-008-0396-4. They find an increase in track density and intensity of extreme cyclones over the North Atlantic close to the British isles in an ensemble of A1B scenario simulations with a particular model, and conclude that this change is associated with an eastward shift of the jet stream into Europe, in addition with more frequent extreme values of baroclinicity, upper level divergence and lower tropospheric equivalent potential temperature. An increased number of explosive developments close to Europe is found under increased greenhouse gas forcing, in agreement with the enhanced number of windstorms affecting Europe found in other studies. (Ulbrich, Uwe, Freie Universitaet Berlin)	This has now been added.
1459	3	61	25	61	30	I don't think this paragraph belongs in a section on projection - it is more about attribution. (Trewin, Blair, Australian Bureau of Meteorology)	Paragrap is moved
1460	3	61	32	61	33	This summary, and the likely assessment, seem somewhat at odds with the statment two paragraphs earlier that there are few indepth analyses of mechanisms. (Zwiers, Francis, Environment Canada)	Based on the new guidelines for IPCC assesment the assesment is changed to: as likely as not and it is explicitly stated: " the confidence is weakened by few in depth analysis of changes in physical mechanisms related to cyclone changes in coupled climate models and the fact that inter-model differences are not well understood. "
1461	3	61	32	61	36	In the preceeding paragraphs it has been emphasised that different models show different trends here and confidence in these projections is low. The risk-terminology section says that for low-confidence results, no qunatitative estimates (likely, etc) should be given. (van Oldenborgh, Geert Jan, KNMI)	Se comment above
1462	3	61	35	0	0	What time horizon is being considered here? (Stocker, Thomas, IPCC WGI TSU)	... at the end of the century ... is added to the text

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1463	3	61	37	61	41	This statment is in apparent contradiction to the multi-model results on storm track changes by Ulbrich et al. (2008) (Ulbrich, U., J.G. Pinto, H. Kupfer, G.C. Leckebusch, T. Spangehl and M. Reyers, 2008: Changing Northern Hemisphere Storm Tracks in an Ensemble of IPCC Climate Change Simulations. J. Climate, 21, 1669–1679.). Please have a look at the respective section of the review by Ulbrich et al. (2009).Ulbrich, U., G.C. Leckebusch, J. Pinto, 2009: Extra-tropical cyclones in the present and future climate: a review. Theo. Appl. Climatology, 96, 117-131. DOI 10.1007/s00704-008-0083-8 (Ulbrich, Uwe, Freie Universitaet Berlin)	While the Ulbrich paper show consstency among IPCC models this is not enough to raise the confidence to medium or high which is needed to make a likelihood statement. Based on the new guidelines for IPCC assesment we haw written "Regional changes may be substantial. While IPCC AR4 models show some regions with medium degree of agreement, studies using different analysis techniques, different physical quantities, different thresholds and different atmospheric vertical levels to represent cyclone activity and storm tracks are reaching different conclusions. This leads to low confidence in region-specific projections."
1464	3	61	44	61	44	You may want to add Pinto, J.G., S. Zacharias, A.H. Fink, G.C. Leckebusch, und U. Ulbrich, 2009: Factors contributing to the development of extreme North Atlantic cyclones and their relationship with the NAO. Climate Dynamics, 32, 711-737 doi: 10.1007/s00382-008-0396-4. (Ulbrich, Uwe, Freie Universitaet Berlin)	This paragrap is now removed
1465	3	61	44	61	44	and other models having a southward shift. McDonald 2010 (Brown, Simon, The Met Office Hadly Centre)	This has been added to the text.
1466	3	61	47	61	51	Please remove this direct repetition appearing in lines 45 - 48 p. 60 (Stocker, Thomas, IPCC WGI TSU)	This has been removed
1467	3	61	48	61	48	cite Ulbreich 2009 (Brown, Simon, The Met Office Hadly Centre)	This paragraph is removed, but Ulbrich paper is now cited in the text
1468	3	61	54	0	0	Section 3.5.1 on "Droughts": this is generally a very good and nicely written assessment! (Stocker, Thomas, IPCC WGI TSU)	thanks
1469	3	61	56	62	17	Here, at first, the drought in this report or chapter is to be defined much more clearly. In the draft, currently, the causes of it are mentioned and the perspectives for the definition are introduces. The definition itself is not clear. Drought can be defined with the condition of shortage of water. The "shortage" is to be defined by not only natural conditions but also social conditions including water demand and vulnerability of the society. This might affect the context of the section later. (Watanabe, Tsugihiko, Research Institute for Humanity and Nature (RIHN))	Drought definitions and implications are now treated in more detail in a Box. Water "shortage" caused by human overuse is out of scope out the chapter, but should be treated in the subsequent ones
1470	3	61	56	0	0	An assessment of drought is missing for Asia, in terms of both observations and projections. There are also no observations assessed from Australasia - these need to be provided as a context for wild-fire and other impacts discussed extensively in Chapter 4. (Stocker, Thomas, IPCC WGI TSU)	Is added according to available literature
1471	3	61	56	0	0	Available paleo-literature should be included to the extent possible. (Stocker, Thomas, IPCC WGI TSU)	yes, according to specific Luterbacher and van Lanen comments
1472	3	61	58	61	60	There is a substantial difference between drought and water scarcity based on the first being a temporary (and often normally associated to climatic patterns) decrease in water resources, and the second occurring when water demand exceeds the water resources exploitable under sustainable conditions (COM2007-414 final). This difference should be highlighted in this introductory part of the chapter. The problem is related to water scarcity- it applies to human needs. (SERGI SARATER University Girona)	Water scarcity is out of the scope of the chapter - will make this explicit in the Text and Box defining drought for the chapter
1473	3	61	59	61	59	add to the references: "... (Heim Jr,2002; IPCC , 2007a, glossary) ...". "... (Wilhite, 2000; Heim Jr,2002; Tallaksen and van Lanen, 2004; IPCC , 2007a, glossary) ...". Tallaksen, L.M. & van Lanen, H.A.J. (Eds.) (2004) Hydrological Drought. Processes and Estimation Methods for Streamflow and Groundwater. Developments in Water Science, 48, Elsevier Science B.V., 579 pg. Wilhite, D. A. (Ed.) (2000) DROUGHT A Global Assesment, Vol I &II, Routledge Hazards and Disasters Series, Routledge, London. (van Lanen, Henny A.J., Wageningen University)	We add only the earliest one (space limitations)
1474	3	61	60	61	60	add: ".....deficit of precipitation, agricultural drought related to root zone soil water balance, or hydrological". ".....deficit of precipitation, agricultural drought related to root zone soil water balance (often called soil moisture drought), or hvdrological". (van Lanen, Henny A.J., Wageningen University)	ok, changed
1475	3	62	1	62	1	add "....moisture and/or groundwater storage) are critical...". "...moisture, groundwater, lake and/or snow storage , e.g. van Lanen et al., 2004) are critical ..." (for reference, see comment 16). (van Lanen, Henny A.J., Wageningen University)	added, add
1476	3	62	1	62	17	Some comment about different timescales of "drought" (from weeks to a decade or more) may be worthwhile in the introduction. (Trewin, Blair, Australian Bureau of Meteorology)	Too detailed for Text
1477	3	62	2	62	2	Figure 3.11 is very much focused on the atmspere (meteo drought). In this impact section, I suggest to draw more attention to the soil moisture and hydro droughts. You might consider to include Fig. 1.2 from Tallaksen and van Lanen (2004). (for rreference, see comment 20) (van Lanen, Henny A.J., Wageningen University)	will be replaced by box
1478	3	62	4	62	4	add ".... These proxies include indices such as". ".... These proxies for global studies include indices such as". (van Lanen, Henny A.J., Wageningen University)	These proxies are also used for regional studies, so this specification is not correct
1479	3	62	5	63	5	Much of the material that follows is based on analyses of the PDSI, so perhaps it would be useful to have a small box explaining how it is derived, and contrasting it with other drought indices. (Zwiers, Francis, Environment Canada)	PDSI is now included in new box

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1480	3	62	7	62	7	add "..... proxies such as tree rings.". ".... proxies such as tree rings, shells (e.g. Versteegh et al., 2010) or grapes harvest days (e.g. Garnier et al., 2010). Versteegh, E.A.A, Vonhof, H.B., Troelstra, S.R. and Kroon, D. (2010) Can shells of freshwater mussels (Unionidae) be used to estimate low summer discharge of rivers and associated droughts? Int J Earth Sci (Geol Rundsch) DOI 10.1007/s00531-010-0551-0. Garnier, E., Daux, V., Yiou, P. and García de Cortázar-Atauri, I. (2010) Grapevine harvest dates in Besançon (France) between 1525 and 1847: Social outcomes or climatic evidence? Climatic Change DOI 10.1007/s10584-010-9810-0 (van Lanen, Henny A.J., Wageningen University)	changed according to comment 1482
1481	3	62	7	62	8	revise: Hence, soil moisture or hydrological drought indices often integrate temperature, precipitation and other variables (e.g. recharge), but may be problematic when not integrating all necessary information (Nicholls and Alexander, 2007). (van Lanen, Henny A.J., Wageningen University)	changed
1482	3	62	7	0	0	Please add 'speleothems and historical evidence' after tree rings (Luterbacher, Juerg, Justus Liebig University)	ok
1483	3	62	8	0	0	Please specify what you mean with 'all necessary information' (Luterbacher, Juerg, Justus Liebig University)	changed
1484	3	62	9	62	9	limited and biased list, add: ".....the impact of droughts (e.g., on crop yields, general ecosystem functioning, etc.), ...". ".....the impact of droughts (e.g., on crop yields, general ecosystem functioning, water resources, electricity production etc.), ...". (van Lanen, Henny A.J., Wageningen University)	changed
1485	3	62	9	62	9	add to timing, duration and frequency: "scale". Drought usually covers large areas and that makes it different from many other natural hazards. (van Lanen, Henny A.J., Wageningen University)	"spatial extent" added
1486	3	62	9	0	0	"I recommend to use the terms "frequency and magnitude of extreme events" throughout the entire chapter instead of "frequency and intensity". This is, because intensity is often related to the impact or severity of the event, while magnitude simply describes the event itself." (Jentsch, Anke, University of Koblenz-Landau)	"intensity" is a defined term (Sheffield and Wood 2008), while magnitude could also mean spatial extent or duration
1487	3	62	10	62	11	revise: "The maximum number of consecutive dry days is often used as an overall drought index for (semi-)arid regions where river flow is exceptional". (van Lanen, Henny A.J., Wageningen University)	removed
1488	3	62	10	62	11	I do not understand that "the maximum number of consecutive dry days is often used for the whole year, while other indices such as the PDSI characterize specific situations within a year.". I know many indices that characterize multi-year characteristics (e.g. streamflow deficit volumes). (van Lanen, Henny A.J., Wageningen University)	removed, possibly discuss in box
1489	3	62	10	62	11	This is true, but CDD is a weak index, especially in climates with strong seasonal variation in rainfall where the exact length of dry-season dry spells is largely irrelevant to water availability. (Trewin, Blair, Australian Bureau of Meteorology)	removed, possibly discuss in box
1490	3	62	10	62	38	Overall in the section. Climate change may provide most reasons for drought, but not to forget that the increasing pressure on the water resources by human uses may exert a positive feed-back on drought. The case of Mediterranean Spain may be taken as a paradigm. Drought episodes have been rising during the 20th Century, both as a result of climate change but also as a result of an intensive use of superficial and groundwater for agriculture. Decreasing groundwater levels reinforce lower runoffs and enforce the probability for permanent drought/scarcity in drought-sensitive areas (SERGI, SABATER, University Girona)	The feedback is now mentioned in the text, and may be assessed in more detail for the TOD with chapter 4.
1491	3	62	10	0	0	Drought is a sustained lack of precipitation, not necessarily an absence of precipitation. For example, precipitation can occur on the same number of days but, if the amount is less each time, this may well lead to drought. (Stewart, Ronald, University of Manitoba)	do not find this; we write "lack" at the beginning
1492	3	62	17	62	17	I suggest to add a few lines that include more land surface hydrology. "Drought are also complex because the various types of drought (meteorological, soil water, hydrological drought) are different in area covered, duration and severity because of the propagation through the land part of the hydrological cycle (e.g. Peters et al., 2003). The area covered is primarily climate driven, whereas variability within the affected area is influenced by land properties (e.g. Peters et al., 2005; Tallaksen et al., 2009). Thus, it is important to account for the combined effect of climatic input and catchment response when analysing hydrological drought development.". Peters, E., Torfs, P.J.J.F., Lanen, H.A.J. van & Bier, G. (2003) Propagation of drought through groundwater – a new approach using linear reservoir theory. Hydrological Processes 17(15) 3023-3040. Peters, E., Bier, G., van Lanen, H.A.J. & Torfs, P.J.J.F. (2006) Propagation and spatial distribution of drought in a groundwater catchment. J. of Hydrol. 321: 257-275. Tallaksen, L.M., Hisdal, H. & van Lanen, H.A.J. (2009) Space-time modeling of catchment scale drought characteristics. J. of Hydrol. 375:363-372 (doi:10.1016/j.jhydrol.2009.06.032). (van Lanen, Henny A.J., Wageningen University)	this goes too far given the space constraints

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1493	3	62	20	63	8	The discussion on observed droughts provides a somewhat biased picture. It is not correct to mention only studies that report changes (more specifically increases) in drought conditions (Section 3.5.2.1 on floods provides a better discussion on observed changes). For example, the works of Lloyd-Hughes and Saunders (2002) and van der Schrier et al. (2006) conclude that, based on the analysis of SPI and PDSI for the 20th century, no statistically significant changes can be observed in extreme and moderate drought conditions. Such works should also be included and discussed. References: Lloyd-Hughes, B., and M.A. Saunders, 2002. A drought climatology for Europe. International Journal Climatology 22, 1571-1592. van der Schrier, G., K.R. Briffa, P.D. Jones, and T.J. Osborn, 2006. Summer Moisture Variability across Europe. Journal of Climate, 19, 2818-2834. (Feyen, Luc, Joint Research Centre, European Commission)	this literature is included now, and the point made indeed that there is uncertainty
1494	3	62	28	0	0	Please add the following references after Kallis 2008: Esper et al. (2007) and Buentgen et al. (2010): Buntgen, U, Trouet, V., Frank, D., Leuschner, H.H., Friedrichs, D., Luterbacher, J., and Esper, J., 2010: A tree ring-based reconstruction of summer drought for Central Germany and the last millennium. Quat. Sci. Rev. 29, 1005-1016. Esper, J., Frank, D.C., Buntgen, U., Verstege, A., Luterbacher, J., and Xoplaki, E., 2007: Long-term drought severity variations in Morocco. Geophys. Res. Lett., 34, 117702 (Luterbacher, Juerg, Justus Liebig University)	Buntgen (2010) added
1495	3	62	28	0	0	Please add the following: Pfister et al. (2006) studied the hydrological winter droughts over the last 450 years in the Upper Rhine basin and found, that severe winter droughts were relatively rare in the 20th century compared to the former period. Pfister, C., Weingartner, R., and Luterbacher, J., 2006: Reconstruction of extreme hydrological winter droughts over the last 450 years in the Upper Rhine basin. A methodological approach, Hydrolog. Sci. J., 51, 966-985. (Luterbacher, Juerg, Justus Liebig University)	regionally too specific
1496	3	62	39	62	2	This is a very densely packed piece of text; I suspect that it will be a challenge for many readers. (Zwiers, Francis, Environment Canada)	which piece is meant (line is reverse)
1497	3	62	41	62	42	Need to explain what the "warm-dry" mode is. On the other hand, I'm not sure that it is necessary to say that a "quartile analysis" was used unless that somehow would increase or decrease confidence in the result. Not many readers (this one included) would know precisely what a quartile analysis is in any case, so we would have to either guess or look up the paper. (Zwiers, Francis, Environment Canada)	is better explained now
1498	3	62	49	62	51	Observations for the US. "Andreadis et al. (2005) and Andreadis and Lettenmaier (2006) used the VIC model to simulate soil moisture and runoff over the continental United States. Drought characteristics were calculated from the time series of simulated gridded data from this model. The most severe events in the US in the period 1920-2003 occurred in the 1930s and 1950s, where the 1930s Dust Bowl was most intense and the 1950s drought most persistent (Andreadis et al., 2005). Another severe drought started in the early 2000s, which was according to Andreadis et al. (2005) still developing in 2003. Sheffield et al. (2004), Sheffield and Wood (2008) and Sheffield et al. (2009) also used the VIC model to simulate soil moisture data globally. Sheffield et al. (2009) found severe droughts in the US in the 1950s and in 1999-2000 as well. Because they analysed a shorter period (1950-2000) the 1930s were not included. Despite differences in approach, such as a different spatial and temporal scale the results for the US were quite similar; the most severe droughts were identified in both studies (Sheffield et al., 2009). These identified drought events corresponded to already cited extreme droughts (Andreadis et al., 2005). In the period 1925-2003, there has been a wetting trend in many parts of the US, causing soil moisture and runoff droughts for the most part to become shorter, less severe, less frequent and less spatially extensive (Andreadis & Lettenmaier, 2006). Exceptions are the Southwest and parts of the interior of the West where an opposite trend in droughts was found (Andreadis & Lettenmaier, 2006). These results correspond well with the results of Dai et al. (2004) who found a wetting trend in the PDSI over large parts of the US". Andreadis, K. M. & Lettemaier, D. P. (2006) Trends in 20th century drought over the continental United States. Geophysical Research Letters, 33. Andreadis, K. M., Clark, E. A., Wood, A. W., Hamlet, A. F. & Lettemaier, D. P. (2005) Twentieth-century drought in the conterminous United States. Journal of Hydrometeorology, 6, 985-1001. Sheffield, J., Goteti, G., Wen, F. H. & Wood, E. F. (2004) A simulated soil moisture based drought analysis for the United States. Journal of Geophysical Research-Atmospheres, 109. Sheffield, J., Andreadis, K. M., Wood, E. F. & Lettemaier, D. P. (2009) Global and Continental Drought in the Second Half of the Twentieth Century: Severity-Area-Duration Analysis and Temporal Variability of Large-Scale Events. Journal of Climate, 22, 1962-1981 (van Lanen, Henny A.J., Wageningen University)	Andreadis and Lettenmaier (2006) and Andreadis et al. 2005 are included, and intense early droughts discussed. The studies by Sheffield are mentioned where no regional studies exist
1499	3	62	50	62	50	The citation to "Wang et al. 2009b" here does not seem fit, because this study is about NE Atlantic storminess as inferred from geo-winds, has nothing to do with US droughts. Maybe a typo? (Wang, Xiaolan, Environment Canada)	removed
1500	3	62	51	62	51	Most severe drought during what period? (Zwiers, Francis, Environment Canada)	clarified, 20th century
1501	3	62	0	63	0	It would be helpful to include references to observed changes in Australian drought to help contextualise the discussion in the following section on the causes behind Australian drought. The CSIRO/BoM Drought report http://www.daff.gov.au/_data/assets/pdf_file/0007/721285/csiro-bom-report-future-droughts.pdf may be useful. (Dunhrell, Amv, Australian Government Department of Climate Change and Energy Efficiency)	We refer now to Sheffield & Wood as peer reviewed reference, which is largely consistent with the report
1502	3	62	0	0	0	Section 3.5.1.1 - I was surprised there was little comment on Australia (Church, John, CSIRO)	see comment 1501

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1503	3	62	0	0	0	Figure 3.11. This figure is confusing. It is left to the reader to work out whether the "positive" and "negative" impacts refer to droughts as a whole or just the variable in the blue/yellow box (the latter). Confusingly, the variables themselves are either positive or negative (eg temperature increase but precipitation deficit). Suggest changing the label to "(red: positive feedbacks; blue: negative feedbacks)" and consider whether the diagram needs to be this complex to convey the idea that precipitation, evaporation and ground preconditions (soil moisture and groundwater storage) are critical factors leading to drought, but that other variables play their part. (Bell, Victoria, Centre for Ecology and Hydrology)	This figure was revised and is now found in the new box on drought
1504	3	63	1	63	4	Some discussion of southern Australia (and perhaps the Mediterranean) would be useful in this section. (Trewin, Blair, Australian Bureau of Meteorology)	added
1505	3	63	3	63	3	Please insert 'African' before 'monsoon regions' to avoid generalisation. (Stocker, Thomas, IPCC WGI TSU)	added
1506	3	63	5	63	5	please add. "Stahl et al. (2010) investigated streamflow trends in a newly-assembled, consolidated dataset of near-natural observed streamflow records from 441 small catchments in 15 countries across Europe. Trends were calculated by using time series from 1962-2004 and for longer time periods (with fewer stations) starting in 1932. A regionally coherent picture of annual streamflow trends emerged, with negative trends (lower streamflow) in southern and eastern regions, and generally positive trends (higher streamflow) elsewhere (especially in northern latitudes). Trends in monthly streamflow were more remarkably. These show positive trends in the winter months in most catchments. However, a marked shift towards negative trends was observed in April, gradually spreading across Europe to reach a maximum extent in August. Low flows have decreased in most regions where the lowest mean monthly flow occurs in summer, but vary for catchments which have flow minima in winter and secondary low flows in summer. The study largely confirms findings from national and regional scale trend analyses, but clearly adds to these by confirming that these tendencies are part of coherent patterns of change, which cover a much larger region. Hannaford et al. (2010) used a slightly-extended version of the earlier mentioned river flow dataset. Analysis of the drought indicators has shown 'drought-rich' and 'drought-poor' periods that occur contemporaneously across European regions. Some droughts are spatially coherent over a large area. These periods broadly agree with major European droughts identified by previous studies (e.g. Sheffield et al., 2009)". Stahl, K., Hisdal, H., Hannaford, J., Tallaksen, L.M., Lanen, H.A.J., Sauquet, E., Demuth, S., Fendeková, M. & Jódar, J. (2010) Streamflow trends in Europe: evidence from a dataset of near-natural catchments. Hydrol. Earth Syst. Sci. Discuss. (7): 5769–5804, doi:10.5194/hessd-7-5769-2010. Hannaford, J., Lloyd-Hughes, B., Keef, C., Parry, S., and Prudhomme, C. (2010)Examining the large-scale spatial coherence of European drought using regional indicators of rainfall and streamflow deficit, Hydrol. Processes, doi:10.1002/hyp.7725. (van Lanen, Henny A.J., Wageningen University)	essense is included: Stahl et al. (2010) investigated streamflow data across Europe and found negative trends (lower streamflow) in southern and eastern regions, and generally positive trends (higher streamflow) elsewhere (especially in northern latitudes). Low flows have decreased in most regions where the lowest mean monthly flow occurs in summer, but vary for catchments which have flow minima in winter and secondary low flows in summer.
1507	3	63	6	63	8	The short conclusion seems fine, but the text does not leave me with a sense of whether the literature has been covered comprehensively. (Zwiers, Francis, Environment Canada)	revised into a more cautious statement
1508	3	63	6	63	8	I fail to see how sentences like "there are no clear trends for North America as a whole", "For the Amazon, ... no particular trend has been reported", "For other parts of South America ... indicate that the probability of drought has increased during the late 19th and 20th centuries", a wetter eastern Sahel and no information about Australia or Asia can be summarised by this conclusion. (van Oldenborgh, Geert Jan, KNMI)	revised into a more cautious statement
1509	3	63	6	63	8	The logic of this concluding sentence is hard to follow - it would be helpful to restructure it. (Bell, Victoria, Centre for Ecology and Hydrology)	revised into a more cautious statement
1510	3	63	12	63	38	These paragraphs are not clear to me. Even when clear SST dependencies of droughts have been established (eg AMO and PDO teleconnections to Central US droughts), this does not mean that the trend to higher SSTs has the same teleconnections. Usually, teleconnections are driven by SST differences rather than absolute temperatures, so global warming will not affect the precipitation in the same way as decadal variability. This should be stated more clearly. (van Oldenborgh, Geert Jan, KNMI)	Last part was removed now - not needed
1511	3	63	17	63	17	South-west Australia has seen an increase in precipitation with the exception of a small region around Perth. (van Oldenborgh, Geert Jan, KNMI)	Last part was removed now - not needed
1512	3	63	19	63	38	This is another densely packed paragraph that seems to have a couple of different intents. I think it would help the reader if this could be separated into two smaller paragraphs - one on further research on the role of SSTs, and another on land-atmosphere feedbacks. (Zwiers, Francis, Environment Canada)	The paragraph has been rearranged
1513	3	63	19	63	38	I think in the drought problem we could also face locally with non climate influence derived from the land use management due to human socio-economic activities and this should be mentioned, too. (Bojariu, Roxana, National Meteorological Administration)	This general feedback/interaction is now mentioned (see comment 1490)

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1514	3	63	21	63	21	revise/add: "...as well as of potential impacts of land use changes (Deo et al., 2009), but large uncertainties remain. Fleig et al. (2010) studied links between severe hydrological droughts and weather types (WTs) in north-western Europe. The hydrological response time (i.e. the time over which WTs influence drought development) was found to vary markedly (45–210 days) between regions according to river basin storage properties. The study suggests that hydrological droughts are caused by a complex set of hydroclimatological processes, i.e. the dominant drought-yielding WTs changed between regions and between events within each region. There are also uncertainties in the field of land surface modeling". Fleig, A. K., Tallaksen, L. M., Hisdal, H., and Hannah, D. M (2010): Regional hydrological drought in north-western Europe: linking a new Regional Drought Area Index with weather types, Hydrol. Processes, 24, doi:10.1002/hyp.7644 (van Lanen, Henny A.J., Wageningen University)	we don't see how this add to the uncertainty
1515	3	63	33	63	33	Is this drought still continuing? If not, give an enddate. Also, it's not obvious from the evidence presented whether this single event is evidence of a trend. (Trewin, Blair, Australian Bureau of Meteorology)	the key is the period 2005/2006; we rephrase
1516	3	63	40	0	0	Reference from Belgium (impacts of 24 AR4 RCM runs on daily low flow extremes of 67 catchments of the Scheldt basin studied; the daily low flow extremes change from -20% to -70%, depending on the RCM run considered) is: Boukhris et al., 2008 (Boukhris O., Willems P., Vanneville W. (2008), 'The impact of climate change on the hydrology in highly urbanized Belgian areas', In: "Water and urban development paradigms: Towards an integration of engineering, design and management approaches" (Eds. J.Feyen, K.Shannon, M.Neville), CRC Press, Taylor & Francis Group, 271-276) (Willems, Patrick, Katholieke Universiteit Leuven)	Too specific for this chapter
1517	3	63	42	63	42	Please include as introduction the observation that AR4 models and theory project an increase of the globally averaged hydrological cycle of 1-3%, but in spite of this increase there are areas in which droughts are projected to increase. (van Oldenborgh, Geert Jan, KNMI)	The globally averaged hydrological cycle (which includes the Ocean), does per se not link to drought; hence there is no contradiction which would need to be reconciled
1518	3	63	42	64	42	Section 3.5.1.3. Presumably there is a correlation between increased drought and decreased mean rainfall. This should be referred to, and anomalies from this pattern discussed. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	much of the drought signal is related to increasing temperatures
1519	3	63	42	64	42	Recent paper on projected drier conditions in parts of Africa; Funk C, Dettinger MD, Michaelsen JC, Verdin JP, Brown M, Barlow M, Hoell A. Warming of the Indian Ocean threatens eastern and southern African food security but could be mitigated by agricultural development. Proceedings of the National Academy of Sciences of the United States of America 2008. 105:11081-11086. (Conway Declan, University of East Anglia)	included
1520	3	63	42	64	42	Why are hydrological droughts not mentioned here? Recently, some large scale assessments of changes in hydrological droughts (globally and for Europe) have been performed, which are relevant for the discussion on drought extremes in view of climate change. These include the works of Lehner et al. (2006), Hirabayashi et al. (2008), and Feyen and Dankers (2009). In the section on floods, the first two studies are mentioned (lines 18-19 on page 67, Section 3.5.2.3) in relation with droughts. This information should be in the discussion on droughts, rather than in the discussion on floods. References: Feyen, L., and R. Dankers, 2009. Impact of global warming on streamflow drought in Europe. Journal of Geophysical Research, 114, D17116, doi:10.1029/2008JD011438. Hirabayashi, Y., S. Kanae, S. Emori, T. Oki, and M. Kimoto, 2008. Global projections of changing risks of floods and droughts in a changing climate. Hydrological Sciences Journal, 53(4), 754-772. Lehner, B., P. Döll, J. Alcamo, T. Henrichs, and F. Kaspar, 2006. Estimating the impact of global change on flood and drought risks in Europe: A continental integrated analysis. Clim. Change, 75, 273-299, doi:10.1007/s10584-006-6338-4. (Feyen, Luc, Joint Research Centre, European Commission)	paragraph included
1521	3	63	47	63	47	revise/add "...More recent global and regional climate simulations, and hydrological models driven by high-resolution regional climate simulations support the". (van Lanen, Henny A.J., Wageningen University)	revised
1522	3	63	58	63	58	I think it is important to note that the changes in Burke and Brown are consistent across MME and PPE ensembles (Brown, Simon, The Met Office Hadly Centre)	added
1523	3	63	59	63	59	Changes must also be model dependent. Did Burke and Brown assess the sensitivity of their results to the choice of model? (Zwiers, Francis, Environment Canada)	yes, see comment 1522
1524	3	63	60	63	63	"southern Africa" overgeneralises: there is no evidence for more drought in south-eastern Africa (especially Lesotho), only in south-western Africa (Shongwe et al J.Clim 2009 doi:10.1175/2009JCLI2317.1) (van Oldenborgh, Geert Jan, KNMI)	we are referring here to Burke & Brown 2008 who not only look at precipitation, but now we make the point that there are regional differences
1525	3	64	0	67	0	Human effects on floods should be divided in two categories as follow: a) Development of cities and changes in the land use and then the extreme changes in runoff coefficient b) Other cases that lead to CO2 concentration and global warming Since the first one is not detectable by a wide range of Models. (Davtalah, Rahman, Ministry of Energy)	misplaced
1526	3	64	4	0	13	The following result may serve as reference: "by the end of this century, the Fertile Crescent of Middle East will lose its current shape and may disappear altogether."(Akio Kitoh, Akiyo Yatagai and Pinhas Alpert: First super-high-resolution model projection that the ancient "Fertile Crescent" will disappear in this century, Hydrological Research Letters, Vol. 2, pp.1-4, (2008) (Nakaegawa, Toshiyuki, Meteorological Research Institute)	this is very specific and the robustness of one single new model is not clear enough
1527	3	64	5	64	6	A brief reference to the normal seasonal cycle of Mediterranean rainfall would be useful here. (Trewin, Blair, Australian Bureau of Meteorology)	for clarity we include "summer" droughts

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1528	3	64	13	0	0	add "of the continent" at the end of sentence (Van den Hurk, Bart, KNMI)	added
1529	3	64	14	64	14	add: "Feyen and Dankers (2009) examined the impact of global warming on streamflow drought in Europe by comparing low-flow predictions of a hydrological model driven by high-resolution regional climate simulations for the end of the previous century and for the end of this century. In order not to mix drought events with different physical causes the analysis was performed separately for the frost and nonfrost season. Results show that in the frost-free season streamflow droughts will become more severe and persistent in most parts of Europe by the end of this century, except in the most northern and northeastern regions. In the frost season, streamflow drought conditions will be of less importance under future climate conditions. Lehner et al. (2006) present possible impacts of global change on future drought frequencies for Europe using the global hydrological model WaterGAP. 100-year droughts show strong increases for large areas of southern and southeastern Europe (Portugal, all Mediterranean countries, Hungary, Bulgaria, Romania, Moldova, Ukraine, southern Russia). In the long-term projection for the 2070s, the current 100-year events are calculated to occur every 40 years or even more often in these areas - in the most extreme cases reaching return periods of 10 years and below. Northern Europe shows a reduction in 100-year droughts. In addition to the pan-European studies, the impact of drought for large river basin have been investigated. Hurkmans et al. (2010) explored if the hydrologic behavior of the Rhine basin will shift from a combined snowmelt- and rainfall-driven regime to a more rainfall-dominated regime because of global warming. The Variable Infiltration Capacity model (VIC) was driven by high-resolution (0.088o) regional climate scenarios. The first half 21st C is dominated by increased precipitation, causing increased streamflow throughout the year. However, during the second half of the century, magnitudes of streamflow droughts increase.. Feyen, L. and Dankers, R. (2009) Impact of global warming on streamflow drought in Europe, J. Geophys. Res., 114, D17116, doi:10.1029/2008JD011438. Hurkmans, R.T.W.L., Terink, W., Uijlenhoet, R., Torfs, P.J.J.F., Jacob, D. and Troch, P.A. (2010) Changes in streamflow dynamics in the Rhine basin under three high-resolution climate scenarios, J. Climate, 23, 679-699, doi:10.1175/2009JCLI3066.1. (van Lanen, Henny A.J., Wageningen University)	a more consise paragraph was added, mentioning this and the Hirabashi study
1530	3	64	15	64	25	Could mention Cai et al (2006) who link part of the rainfall decline in southwest Western Australia to changes in the Southern Annular Mode (Cai et al. 2006, SAM and regional rainfall in IPCC AR4 models: Can anthropogenic forcing account for southwest Western Australian winter rainfall reduction?, Geophys. Res. Lett., 33, L24708, doi:10.1029/2006GL028037.) (Arhlaster, Julie, NCAR, Australian Bureau of Meteorology)	attribution is not the focus of the paragraph
1531	3	64	32	0	0	You can add "Van den Hurk, B and E van Meijgaard (2010): Diagnosing land-atmosphere interaction from a regional climate model simulation over West Africa; J. Hydrometeor., 2010, 11, 467-481" to the list of studies addressing land-atm interaction. (Van den Hurk, Bart, KNMI)	added, good for regional balance
1532	3	64	35	64	35	I suggest to use the term "soil moisture drought" instead of "agricultural drought. (van Lanen, Henny A.J., Wageningen University)	ok, used throughout
1533	3	64	35	0	0	The drought 'projection' section is lacking concluding statements, even though a likelihood statement is given in the executive summary and Table 3.1. Please note, the statement given in Table 3.1 and the ES only refers to 'area affected by drought'. What about dry spell length and frequency?? (Stocker, Thomas, IPCC WGI TSU)	This is added now, and made consistent with ES
1534	3	64	42	64	42	please add after "...Teuling et al., 2009)." "The space-time development of hydrological drought as a response to a meteorological drought and the associated soil moisture drought (drought propagation, e.g. Peters et al., 2003) also needs more attention. There are some experiences at the catchment scale (e.g. Peters et al., 2006; Tallaksen et al., 2009), but these need to be extended to the regional and continental scales. This would lead to better understanding of the spotted maps of hydrological droughts, which would contribute to a better identification and attribution of droughts and help to improve global hydrological models and land surface models.". (van Lanen, Henny A.J., Wageningen University)	Added
1535	3	64	44	0	0	Section 3.5.2 on "Floods": the section is quite a bit focused on the European situation. Need to expand to other regions as well (Stocker, Thomas, IPCC WGI TSU)	The author followed the comment. However, there is limitation to expand to regions with few information. Please understand that literature for Asia, Africa and South America is limited. The description on Europe is reduced to achieve a balance between regions.
1536	3	64	46	64	47	This sentence is too restrictive - for example pluvial flooding can occur anywhere, not just along river channels/lakes/coast. Suggest you change to " Floods are natural physical impacts produced by a transient high water level most commonly along a river channel....." (Bell, Victoria, Centre for Ecology and Hydrology)	Agree. Firstly, the definition of flood has been changed to the one from the American Meteorological Society. This description is in accordance with the definition in Chapter 9. In addition, pluvial flood is added in this paragraph.
1537	3	64	46	64	47	the definition in the first sentence might be too narrow, it "excludes" e.g. pluvial flooding (resulting from heavy precipitation and mentioned in the 3rd sentence as urban floods) (Wehrli, Andre, European Environment Agency)	Response is the same as above.
1538	3	64	46	64	57	It would be useful to include a definition of flood given that the use of the term by hydrologists may be quite different from that used in the public. Neither the AR4 WG1 or WG2 glossaries give a definition of "flood". A second comment is that infrequently used acronyms, such as GLOFs, should be avoided. This acronym appears only a few times, so not very much space is conserved through its use. (Zwiers, Francis, Environment Canada)	Agree. The same as above. In addition, GLOF is avoided.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1539	3	64	46	64	57	Labat, D., Godderis, Y., Probst, J., and Guyot, J. (2004) Evidence for global runoff increase related to climate warming, <i>Advances in Water Resources</i> , 27, pp. 631–642 (Ranzi, Roberto, University of Brescia)	This paper did not deal with floods, extremes and/or peaks.
1540	3	64	46	66	2	There are very few published studies on recent and future behaviour of floods in Africa, apart from studies of single events which generally do not look at long term context. The following have some discussion / analysis of floods but only specific events; Jury M. (In review) Meteorological scenario of Ethiopian floods in 2006 – 07. <i>Theoretical and Applied Climatology</i> . Tarhule A. Damaging Rainfall and Flooding: The Other Sahel Hazards. <i>Climate Change</i> 2005, 72(3):355-377. (Conway, Declan, University of East Anglia)	These two papers were read. However, the first one did not deal with the changes of floods, river discharge. The second one is relevant for Chapter 4 which should deal with changing social impact of floods.
1541	3	64	46	0	0	floods definition given here needs more scientifically and needs citation (Incecik, Salahattin/Selahattin, Istanbul Technical University)	Please see the response 1536. The main definition is taken from a WMO definition. Citations are shown in the text.
1542	3	64	47	64	47	When humans are impacted, floods can become "natural disasters" - Please delete this unnecessary sentence. This fact applies to ALL extremes assessed in Chapter 3, and is acknowledged numerous times elsewhere in the chapter/report. (Stocker, Thomas, IPCC WGI TSU)	Agree. Deleted.
1543	3	64	47	64	47	please do not use the term natural disasters (since there is agreement that disasters are not natural); rather use disastrous events or something like that (Wehrli, Andre, European Environment Agency)	Deleted.
1544	3	64	54	64	56	this sentence states that the chapter will focus on fluvial floods, still there are aspects on pluvial floods in there (e.g. p66, 40-42 the reference to the UK flooding). I would rather try to separate these two types of floods if possible. (Wehrli, Andre, European Environment Agency)	Firstly, literature and evidence are rare for pluvial floods. Secondly, it can be implied by the changes in heavy precipitation. Therefore, while this section mainly focuses on fluvial floods, the authors added a sentence at the end of this paragraph.
1545	3	64	59	66	2	Another, recent paper on this topic, for the US. Is Villarini, G., Serinaldi, F., Smith, J.A., and Krajewski, W.F., 2010, On the stationarity of annual flood peaks in the United States during the 20th century, <i>WATER RESOURCES RESEARCH</i> , VOL. 45, W08417, 17 PP., 2009 doi:10.1029/2008WR007645 This paper is very clear in its conclusion that there is no compelling evidence for changes in flooding: "it is easier to proclaim the demise of stationarity of flood peaks than to prove it through analyses of annual flood peak data." Overall, I would say this section of the draft seems to present a bias towards suggesting that there is a trend towards increased flooding, even though many studies have failed to find that. (Hirsch, Robert, United States Geological Survey)	The paper is cited, and description is added at the 4th paragraph of 3.5.2.1. By the way, there was no intention of the authors to bias toward the increase in floods. Some modification in the main text is made to avoid such misunderstanding.
1546	3	64	61	65	7	The comments about earlier occurrence of spring peak river flows don't make sense here. The assertion is that these peaks come earlier, and there is good evidence that this is the case in many areas, does not directly relate to the question that this paragraph poses: are floods getting larger or smaller over time. The fact that the peak runoff comes earlier says nothing about the magnitude of that runoff. A number of studies underway (but not yet out in the peer reviewed literature) suggest that this may be resulting in smaller floods rather than larger ones. The phrase "on the other hand" suggests that the observation about earlier peaks somehow implies larger peaks. There is no good reason to believe that is true. Earlier snowmelt MAY result in less flood risk because the large accumulations of snow are diminished because of warming, and in some cases it is the size of this accumulated snow, melting in a warm storm in the Spring, that is the cause of large floods. I would contend that the subject of earlier snowmelt does not belong here, or if it is mentioned it should be made clear that it may lead to smaller floods or larger floods. (Hirsch, Robert, United States Geological Survey)	We mostly agree with this comment, but it should be noted that whether spring flood peaks occur earlier or not is an important issue for some sectors of the society (e.g., agriculture). Therefore, the assessments regarding shifts in snowmelt and possible shifts in flow peak are kept. However, to avoid confusion or misunderstanding, corresponding modifications were made to this paragraph. We also highlight now there and elsewhere in this section that a shift of the flow peak does not necessarily (nor exclude) changes in flood occurrence.
1547	3	64	62	64	63	Flood damages are indeed increasing, but the underlying factors are related to increased exposure and vulnerability. This topic is addressed in Chapter 4. Chapter 3 should be consistent with Chapter 4 and this statement should be modified (including the reasons for the increase) or omitted. Otherwise the statement is misleading. (Feyen, Luc, Joint Research Centre European Commission)	Agree. The sentence is deleted.
1548	3	65	6	65	7	Consistent with my previous comment I would suggest a wording change here: "widespread evidence of observed changes in the magnitude of floods at the global level based on instrumental records. There is evidence for changes in the timing of floods in snow-dominated regions." (Hirsch, Robert, United States Geological Survey)	Following the suggestion, modification is made.
1549	3	65	9	65	23	The longer time scales covered in this section are missing for many of the previous sections. (Klein Tank, Albert, KNMI)	This chapter tries to incorporate longer-time scale issues in all sections as well as in this section.
1550	3	65	12	0	0	Please add the following reference after Brazdil et al. 2005: Pfister et al. (2006) (Luterbacher, Juerg, Justus Liebig University)	Pfister et al. (2006) is a paper on drought rather than flood. In addition, this assessment would not be modified by the inclusion of this paper.
1551	3	65	21	65	23	Note that European floods of past centuries were often associated with ice dams, which do not play a role in current or future climates. (Ulbrich, Uwe, Freie Universitaet Berlin)	The current description does not exclude the possibility of ice-dam-related floods. However, although the possibility of dam-break-related floods is mentioned at the first paragraph of 3.5.2, it is too detailed to specifically refer to ice-dam-related floods around here as an assessment.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1552	3	65	25	65	31	The comparison between Canadian and Russian data seems to be a bit of the apples and oranges type. Does "snowmelt spring flood" refer to what the public would understand as a flood (i.e., a river overflowing its banks) or to the annual maximum flow? (Zwiers, Francis, Environment Canada)	Firstly, some modification is made to this paragraph. Secondary, please see the first paragraph of 3.5.2. This section generally focuses on river discharge peak. The definition of floods must be somewhat different from the perception of the public. Therefore, the first paragraph of 3.5.2 describes the target of this section. "Floods" as recognized by the public are discussed mainly in Chapter 4. Also, the first paragraph of 3.5.2 was modified partially because of this comment. (NEED TO DISCUSS)
1553	3	65	25	65	50	The draft ignores an important work that could go in the paragraph on northern regions or the one on Europe. It is Lindström, Göran and Bergström, Sten(2004) 'Runoff trends in Sweden 1807-2002 / Tendances de l'écoulement en Suède entre 1807 et 2002', Hydrological Sciences Journal, 49: 1, 1 — 83, DOI: 10.1623/hysj.49.1.69.54000 I quote from the last sentence of this excellent study of 61 long discharge records from Sweden: "It is therefore difficult to conclude that flood levels are actually increasing." (Hirsch, Robert, United States Geological Survey)	The paper is cited as suggested. This information is very helpful.
1554	3	65	33	65	50	To what extent is regional land use change and change in control structures or their operating protocols an issue? (Zwiers, Francis, Environment Canada)	The authors of those trend-analysis studies seemed to choose river basins which were not much affected. On the other hand, because many rivers have been affected by land use change and control structures, research on river discharge changes, flood changes in particular, is not easy for many places.
1555	3	65	41	65	41	Yiou et al. (2006). Yiou P, Ribereau P, Naveau P, Nogaj M, Brazdil R (2006) Statistical analysis of floods in Bohemia (Czech Republic) since 1825. Hydrological Sciences Journal-Journal des Sciences Hydrologiques 51:930-945 (Yiou, Pascal, Laboratoire des Sciences du Climat et de l'Environnement)	Modified.
1556	3	65	48	65	50	Actually, a continent wide assessment on flood discharges is available for Europe; see for instance this paper that show coherent peak discharge responses in different European regions, that do not show trends but links to different indices of atmospheric forcing: Bouwer, L.M., Vermaat, J.E. & Aerts, J.C.J.H. (2008). Regional sensitivities of mean and peak river discharge to climate variability in Europe. Journal of Geophysical Research, 113, D19103. http://dx.doi.org/10.1029/2008JD010301 (Bouwer, Laurens, Institute for Environmental Studies)	The target of this paper is river discharge variations in relation to interannual climate variability indices like NAO, AO. Changes and trends relevant for climate change are not much analyzed in this paper. Thus, this paper is not cited. In addition, the text of this paragraph is modified.
1557	3	65	61	65	62	Once again, the discussion of earlier occurrence of spring flood peaks is introduced as if it were really relevant to the topic of increased flood magnitudes. It appears to this reviewer to be an attempt to make the point that floods must be increasing. I think that this is misleading. I have no problem with discussion of the phenomena, but it needs to be clear that there is no evidence that it leads to larger floods, and that one can easily argue that it might create smaller floods rather than larger ones. (Hirsch, Robert, United States Geological Survey)	Firstly, earlier snowmelt flood peak is an important issue as well as the magnitude/frequency changes of floods. Secondly, following the suggestion, the description is modified to avoid the confusion and misunderstanding. Summary of each subsection is also re-written.
1558	3	65	0	67	0	This entire section of flood changes seems to be grasping at straws. The issue of snowmelt timing is raised over and over and yet it is not clear how it relates to the topic at hand (changes in flood frequency and/or magnitude). It is clear that GCMs don't provide a useful basis for projections. It seems that the conclusion is pretty simple. We believe that flood magnitudes and frequencies may change, but, as yet, we have neither the empirical basis nor a sound theoretical basis for believing that increases is any more likely than decreases. (Hirsch, Robert, United States Geological Survey)	The same reply as 1557.
1559	3	66	6	66	58	Conway et al. (2009) concluded for sub-Saharan Africa that robust identification and attribution of hydrological change in nine major international river basins was severely limited by data limitations, conflicting behaviour across basins/regions, low signal-to-noise ratios, sometimes weak rainfall-runoff relationships and limited quantification of the magnitude and potential effects of land use change and other anthropogenic influences. Conway et al. (2009) assert that anecdotally there appears to be greater frequency and spatial extent of damaging floods in parts of Africa and smaller floods may be somewhat overlooked but locally significant, for example in Nigeria (Tarhule, 2005). Source Conway D, Persechino A, Ardoin-Bardin S, Hamandawana H, Dieulin C, Mahe G. Rainfall and water resources variability in sub-Saharan Africa during the 20th century. Journal of Hydrometeorology 2009, 10:41-59. doi:10.1175/2008JHM1004.1 (Conway, Declan, University of East Anglia)	Reflected in 3.5.2.1.
1560	3	66	6	66	58	The study of Ward et al (Ward, P.J., Renssen, H., Aerts, J.C.J.H., Van Balen, R.T. & Vandenberghe, J. (2008). Strong increases in flood frequency and discharge of the River Meuse over the late Holocene: impacts of long-term anthropogenic land use change and climate variability. Hydrology and Earth System Sciences, 12, 159-175) addresses trade-offs between land use and climate variability in the explanation of changes in flood frequencies of the Meuse river, and may be cited here (Van den Hurk, Bart, KNMI)	The suggestion is taken.
1561	3	66	14	66	20	Please note that despite dam construction, some studies show the persistent impact of climate forcing on flood frequency: Florsheim, J.L., M.D. Dettinger (2007). Climate and floods still govern California levee breaks, Geophys. Res. Lett., 34, L22403. (Bouwer, Laurens, Institute for Environmental Studies)	The suggestion is taken.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1562	3	66	17	66	19	The Pinter et.al. Paper is not on the reference list. The discussion here confuses some important concepts. The levees and flood walls have certainly influenced a change in the stage of recent floods (water level) but it is much less clear that they have changed the discharge. The paper needs to make this distinction clear. (Hirsch, Robert, United States Geological Survey)	Reference list is modified. Here, it is true that not only the water level but also flood river discharge has been increased due to river engineering in some or many places of the world. Because such information is rare in scientific journal papers, an appropriate additional journal paper is newly cited here. Pinter's paper also intends to mention the change in discharge as well as water level.
1563	3	66	22	0	0	It should written "drought effected areas" instead of "dry areas" (Davtalab, Rahman, Ministry of Energy)	Around page 66 line 22, "drought" and "dry area" cannot be seen.
1564	3	66	24	66	25	Move the bit in parentheses to after "including precipitation", and include a reference concerning "atmospheric moisture". Also, add "content" after "moisture". (Zwiers, Francis, Environment Canada)	Modified. "Atmospheric water content" is deleted, because this is not a place where "atmospheric water content" is necessary.
1565	3	66	25	66	26	I am fairly certain that the Barnett et.al. 2008 paper is silent on the subject of floods. It is about water supply and mean annual runoff. It has no place in this document on floods. Also, the Hidalgo et.al., 2009 paper deals with runoff timing and I do not believe it does any analysis of flood magnitude or flood risk. It has no place in this document on floods. (Hirsch, Robert, United States Geological Survey)	Again, this section deals with the change in the timing of the discharge peak as well as magnitude and frequency of flood. Based on this principle, citation and description are carefully checked again.
1566	3	66	25	0	0	Is stream regulation also included in the term "anthropogenic"? (Van den Hurk, Bart, KNMI)	No. Thus, the text is modified.
1567	3	66	41	66	41	Is this a case study of a specific event or a more general result? If it's the latter, rewording is required. (Trewin, Blair, Australian Bureau of Meteorology)	Modified.
1568	3	66	46	0	0	The following results may serve as a good reference: Kenji Kamiguchi, Akio Kitoh, Takao Uchiyama, Ryo Mizuta and Akira Noda; "Changes in Precipitation-based Extremes Indices Due to Global Warming Projected by a Global 20-km-mesh Atmospheric Model", SOLA, Vol.2, pp.64-67 (2006) (Nakaegawa, Toshiyuki, Meteorological Research Institute)	Here, papers both describing precipitation and floods in warm humid regions are taken. This paper will be cited in another section because this paper is a paper on precipitation change.
1569	3	66	49	66	51	It think it needs to be made clear here, rather than at the end of the paragraph, that the basis for this assessment is expert judgement given that the evidence remains indirect (except for Barnett et al - who deal with a small region) and that the global scale detection result that is available itself would perhaps only support "more likely than not" for precipitation change. The wording (anthropogenic forcing leading to enhanced ghg's) seems a bit odd since it is that enhanced concentrations that do the forcing. (Zwiers, Francis, Environment Canada)	Modified.
1570	3	66	49	66	51	I am not sure if it is really "more likely than not" (which by the way I think sounds horrible) that floods have been affected. I would rather state that there is evidence of alterations in the hydrologic cycle which, together with other changes (most of them mentioned in the first two sections of 3.5.2.2) , might have had an effect on the frequency or intensity of floods (Wehrli, Andre, European Environment Agency)	Modified based on this suggestion and other suggestions.
1571	3	66	56	66	58	Is this disclaimer regarding the use of expert judgement needed? Surely all such likelihood statements contain an element of expert judgment, and no disclaimer is needed. If a disclaimer is required here, then it may also be required for some other extremes. (Stocker, Thomas, IPCC WGI TSU)	Modified according to the suggestion.
1572	3	66	57	66	58	The Pall paper does provide a bit of support, but as with the Barnett paper, it deals with a small region, and thus doesn't provide much of a basis for generalization. (Zwiers, Francis, Environment Canada)	Deleted.
1573	3	66	63	67	2	The bias of the authors is clear. The Milly (2002) paper is mentioned even though it was published before AR4, and yet the vast amount of work published in the late 1990's and early parts of this decade that comes to different conclusions was not mentioned. In particular various US papers by Douglas, Lins, Wolock, McCabe and others as well as the Swedish work I mentioned. The Milly paper is a fine paper, but so are several other ones that come to quite different conclusions. The selectivity of citations is very troublesome and I hope that it will be resolved. I'd be happy to provide details on those references if the chapter authors are interested. As I said, the presentation comes across to me as highly biased, trying to make a point about increases in flooding even though there is very little empirical or model-driven basis for such an argument. (Hirsch, Robert, United States Geological Survey)	Many comments by Dr. Hirsch are very helpful. However, unfortunately to say, this is a wrong comment. Nevertheless, the text is slightly modified to avoid misunderstanding and to make the text clear. In this sense, this comment is very appreciated. The direct response to this comment is as follows. This paragraph is a subsection for future projection of floods, not for observed trends. Milly (2002) was cited as an explanation to the status when AR4 was published; this paragraph is effectively a kind of summary of AR4 on flood "projection" (not observed trends).
1574	3	67	1	70	30	The title of the Chapter is " Climate Change events and Impacts on Natural ". This is a document by a team consisting of WG I and WG II. Moreover, the title of the chapter as well as the title of the section clearly mentions as "Impacts of extreme sea level". The write-up in this section falls in line with the objectives of WG I. Describing the changes in Extreme Sea Level and discussing various projections etc... The section on Impacts 3.5.5 gives only a list of various studies. Its link with the previous sections 3.5.3 and 3.5.4 is not clear. The purpose of the Special Report will be served if the Impacts section is strengtened by linking to the observations and projections (Alakkat, Unnikrishnan, National Institute of Oceanography)	Sea Level rise
1575	3	67	1	70	30	Extreme Table is a repetition of what is given in AR4, (as far as sea level is concerned) (Alakkat, Unnikrishnan, National Institute of Oceanography)	Sea Level rise
1576	3	67	1	70	30	A recent book Chapter on Extreme Sea level and Waves has been published by Church et al. The Understanding the Sea Level Rise. This chapter is relevant to the present chapter. (Alakkat, Unnikrishnan, National Institute of Oceanography)	Four chapters from this book are now cited in section 3.5.3
1577	3	67	1	70	30	Impacts section discusses mainly on physical impacts, monetary losses etc. There is no mention of loss of human lives, as happened in the case of cyclones and storm surges in the Bay of Bengal. No mention of how early warning systems have decreased the loss of human lives. (Alakkat, Unnikrishnan, National Institute of Oceanography)	impacts on human and natural systems are dealt with in chapter 4.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1578	3	67	19	67	20	Sentence beginning 'From this it is possible' is not needed. Please delete. (Stocker, Thomas, IPCC WGI TSU)	The entire paragraph is deleted in order to focus on floods.
1579	3	67	21	67	23	Sentence "...but this result does not have high reliability because only one model was used " -- I wonder whether then this result should not be mentioned in the assessment at all? (Stocker, Thomas, IPCC WGI TSU)	Modified.
1580	3	67	22	67	22	Should this read "increases in the frequency of hydrological droughts"? (Trewin, Blair, Australian Bureau of Meteorology)	Modified.
1581	3	67	28	67	29	The following paper showed the change of projected river discharge at a typical Japanese basin using a distributed hydrologic model with the input of projected hourly precipitation by the MRI-AM20km (20km spatial resolution GCM). The paper also reveals a future change of water level of a dam reservoir, which indicates the increase of draught risk. KIM, S., Y. TACHIKAWA, E. NAKAKITA and K. TAKARA, 2009: Reconsideration of Reservoir Operations under Climate Change: Case Study with Yagisawa Dam, Japan, Annual Journal of Hydraulic Engineering, JSCE, Vol. 53, pp. 597-611. The paper is one of the examples of catchment-scale river discharge projection using the latest high resolution GCM output, which shows the change of the future river regimes. Adding the paper to the reference as below would be helpful to readers: "However, projections for catchments in other regions like Asia (Asokan and Dutta, 29 2008; Dairaku et al., 2008; Kim et al., 2009), the Middle East (Fujihara et al., 2008), Africa and South America are very rare." (Tachikawa, Yasuto, Kyoto University)	Considering recent situations of IPCC reports, an increase in the citation of grey literature should be avoided except that the paper is really necessary. Here, without this grey literature, the assessment does not change much. Therefore, this suggestion is not taken.
1582	3	67	29	0	0	The following result may be an example projection in South America: Toshiyuki Nakaegawa and Walter Vergara: First Projection of Climatological Mean River Discharges in the Magdalena River Basin, Colombia, in a Changing Climate during the 21st Century", Hydrological Research Letters, Vol. 4, pp.50-54, (2010) . (Nakaegawa, Toshiyuki, Meteorological Research Institute)	The comment is taken.
1583	3	67	36	67	36	Presumably the choice of emissions scenario matters too? (Trewin, Blair, Australian Bureau of Meteorology)	The text is modified based on this suggestion.
1584	3	67	36	67	38	Reference from Belgium (impacts of 24 AR4 RCM runs on hourly peak flow extremes of 67 catchments of the Scheldt basin studied; the hourly peak flow extremes increase up to +35%) is: Boukhris et al., 2008 (Boukhris O., Willems P., Vanneville W. (2008), 'The impact of climate change on the hydrology in highly urbanized Belgian areas', In: "Water and urban development paradigms: Towards an integration of engineering, design and management approaches" (Eds. J.Feyen, K.Shannon, M.Neville), CRC Press, Taylor & Francis Group, 271-276) (Willems, Patrick, Katholieke Universiteit Leuven)	The comment is appreciated. However, in general, grey literature should be avoided. In addition, the assessment of this section will not change if this reference is added. Therefore, it is decided not to add this book chapter. There are already much European literature cited in this section.
1585	3	67	36	67	39	The work of Dankers and Feyen (2009) should also be cited here, as this work clearly showed that in the assessment of changes in flood hazard in Europe for an ensemble of climate change projections (originating from different GCM and RCM combinations, including different initializations of the RCMs) the driving GCM is the major source of uncertainty. (Feyen, Luc, Joint Research Centre, European Commission)	The comment is taken.
1586	3	67	36	67	47	This is a very good discussion of a very important point (Whetton, Penny, CSIRO Marine and Atmospheric Research)	Thanks.
1587	3	67	38	67	0	It is stated here that hydrological model projections require downscaling and bias-correction of climate model outputs, with which I fully agree. I am somewhat surprised to see that the issue of bias-correction is not more emphasized in this report, and put in a more general framework. Earlier in the report it is acknowledged that output of climate models (especially precipitation, and the derived extremes) can show strong bias when compared with observational datasets. However, the document lacks a discussion on the need to correct climate output for this bias. In my opinion, bias-correction should not be limited to hydrological impact assessments, as suggested here. Rather, it should be applied to all climate model output, given that there is sufficient (both in temporal and spatial resolution) observational data for the particular variable, prior to using the climate output for assessing future changes, either using the variables directly (e.g., assessing changes in extreme precipitation using directly precipitation fields), or indirectly (e.g., assessing the changes of floods by forcing a hydrological model with climate model output). This report is an opportunity to call for the need for an increased effort on bias-correction for any assessment of changes in climate or climate-dependent variables. (Feyen, Luc, Joint Research Centre, European Commission)	The comment is very much appreciated. The comment on bias correction is more relevant for WG2 than WG1. Therefore, the comment is more suitable for Chapter 4 of SREX that deals with so-called Impact Assessment. The essence of this comment will be transferred to Chapter 4.
1588	3	67	41	67	42	Sentence "uncertainty analysis in the hydrological projections is still in its infancy" -- unclear... please clarify (Stocker, Thomas, IPCC WGI TSU)	Modified.
1589	3	67	42	67	42	Additional evidence of the sources and implications of uncertainty in impacts studies is provided by: Manning, L.J., Hall, J.W., Fowler, H.J., Kilsby, C.G. and Tebaldi, C. Using probabilistic climate change information from a multi-model ensemble for water resources assessment, Water Resources Research, 45 (2009): W11411. (Hall, Jim, Newcastle University)	Cited.
1590	3	67	49	67	50	Sentence "and those projections often show some degree of uncertainty" -- this needs to be rephrased...projections are always uncertain, that's inherent. (Stocker, Thomas, IPCC WGI TSU)	Modified.
1591	3	67	50	67	57	These projections do not consider the effect on the habitat by man-made structures and water use. It is possible that alteration of river structure, deforestation, forest fires, etc, may alter also (positive feed-back) the potential changes of climate (SERGI, SABATER, University Girona)	The comment is out of scope of this paragraph and this section.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1592	3	67	56	0	0	"The paragraph would profit from some sentences on the crucial role of seasonality and timing of extreme vents." (Jentsch, Anke, University of Koblenz-Landau)	"Crucial role" should be crucial role in human society or in ecosystem, which should be described in Chapter 4. Based on several suggestions and comments to this Chapter and this section, in order to focus only on physical change, we even deleted a description related to the impact of flood magnitude on society, from the first version of the manuscript.
1593	3	67	58	0	58	In any case, for some type of natural events, it is also possible to make a prediction without direct or complete understanding of the nature of the phenomenon, as in case of floods. They present, e.g. in the Mediterranean meteorological framework, common characters, occurring cyclically with a certain regularity, with greater or lesser intensity and with similar mechanism, determining ground effects that can usually be compared. In function of the recognition of analogous precursors, it's possible to estimate the effects in terms of entity and localization, according to a mechanism that crosses natural indicators from the precipitation measurements with the anthropic factors weighting on the territory (Campus et al., 2007). (BOVO, STEFANO, ARPA Piemonte)	It is difficult to get the point of this comment. However, if the comment indicates that the changes in precipitation can imply the changes in floods without direct evidence of flood changes, such an aspect is newly added.
1594	3	67	59	0	0	In section 3.5.3, some comment of consistency of changes in sea level extremes and winds is desirable. (Church, John, CSIRO)	a comment has been added
1595	3	67	60	0	0	We would favour the merging of sections 3.5.3 and 3.5.4 into a single "Extreme Sea Levels and Waves" section, and hope that the material in this section can be made more concise. It is not clear why this current separation is necessary. Section 3.5.5 "Coastal Impacts" could be removed, with this material left for Chapter 4 to consider. Currently there is overlap with chapter 4 regarding 'coastal impacts', and we feel Chapter 4 is the more appropriate chapter to be providing this assessment. (Stocker, Thomas, IPCC WGI TSU)	We prefer to retain the current structure and in discussions with chapter 4 have agreed to take some of their coastal impact material that deals with the physical environment. We have attempted to make the existing sections briefer
1596	3	67	61	68	2	In this introductory section, I think it would be useful to mention that mean sea level rise does not necessarily occur in a spatially uniform manner all over the globe. There has been an overall increase globally, but there have also been reductions in sea level in some places. I would think that one would also want to mention isostatic adjustment and other factors that affect land height (eg, ground water pumping) and the effects of coastal engineering (e.g., dredging, levees, etc) in the introduction. (Zwiers, Francis, Environment Canada)	this has been done
1597	3	67	0	74	0	I thought this was an excellent set of chapters including Chapter 3. However, I thought that the review of changes in extreme sea levels and waves could have been more complete. The most recent extensive review of this topic has been chapter 11 of the Church et al. book: Lowe JA, Woodworth PL, Knutson T, McDonald RE, McInnes K, Woth K, Von Storch H, Wolf J, Swail V, Bernier N, Gulev S, Horsburgh K, Unnikrishnan AS, Hunter J, Weisse R (2010) Past and future changes in extreme sea levels and waves. Chapter 11 in, Understanding sea-level rise and variability (eds. J.A. Church, P.L. Woodworth, T. Aarup and W.S. Wilson). Wiley-Blackwell (London). which in fact has authors in common with this SREX report. However, the time taken to produce that book meant that there were some later relevant references which I have since included in a mini-review in: Woodworth, P.L., Menendez, M. and Gehrels, W.R. 2010. Evidence for century-timescale acceleration in mean sea levels and for recent changes in extreme sea levels. Surveys in Geophysics (submitted). which followed from the ISSI University of Bern 2010 meeting on the Cryosphere and Sea Level Changes. The SREX Chapter 3 authors are welcome to contact me if they have difficulty in obtaining these publications. (woodworth, philip, national oceanography centre)	The additional references mentioned have been cited
1598	3	67	0	0	0	Section 3.5.3, The description on storm surges is insufficient while that on mean sea level rise seems sufficient. Is there any future prediction or sensitivity analysis on the change in extreme storm surges especially on any Asian country coast? (Kawai, Hirovasu, Port and Airport Research Institute)	there is a lack of material dealing with storm surge change on Asian coastlines
1599	3	68	6	68	12	These numbers should be updated to 2009 if possible. (Trewin, Blair, Australian Bureau of Meteorology)	the intention is to provide a summary statement of AR4 findings
1600	3	68	19	68	19	Does "mean sea level" refer to the local mean, or the global mean, in this context? (Zwiers, Francis, Environment Canada)	In the restructuring and shortening of this section, this sentence was removed
1601	3	68	23	68	30	Suggest to explicitly mention what it means that "extremes in sea level are broadly consistent with changes in mean sea level". Explain why is this important. (Stocker, Thomas, IPCC WGI TSU)	The importance of this finding is now explained in the final paragraph
1602	3	68	32	68	33	Some rewording is necessary as the current wording incorrectly implies that prior to 1970 ENSO did not affect sea level extremes. (Church, John, CSIRO)	this has now been reworded
1603	3	68	40	68	42	How does mean sea level rise contribute? (Zwiers, Francis, Environment Canada)	Do not understand comment. Mean sea level rise is not mentioned here. A change in mean sea level pressure is mentioned, but only as a cause of the change in winds.
1604	3	68	44	68	49	The influence of the ENSO signal on observed changes in sea level during the satellite period (and the west-east Pacific contrast) could be mentioned here. (Trewin, Blair, Australian Bureau of Meteorology)	this section has been shortened and in the interests of brevity this has not been added
1605	3	68	0	72	0	The section 3.5.5 is written as a literature review. The impacts section needs to be strengthened by linking with the observations and projections sections 3.5.3., 3.5.4 (Alakkat, Unnikrishnan, National Institute of Oceanography)	section 3.5.5 has now been substantially revised
1606	3	69	5	0	0	Do these modes of variability include ENSO, SAM, etc (i.e the ones discussed earlier in the chapter)? (Whetton, Penny, CSIRO Marine and Atmospheric Research)	these are now mentioned

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1607	3	69	9	69	11	"...to be 18-59 cm with a 90% confidence range ACROSS ALL SCENARIOS" (Stocker, Thomas, IPCC WGI TSU)	this has been added
1608	3	69	9	69	15	Should there at least be some acknowledgement of the much higher potential upper limits of projected SLR estimated from semi-empirical methods?, ie, Rahmstorf 2007 (science paper) (Stocker, Thomas, IPCC WGI TSU)	there is now a paragraph dealing with the semi-empirical models
1609	3	69	9	69	15	The following should be discussed here: Studies since the AR4 using statistical relationships between observed temperature changes and sea level rise (Rahmstorf 2007, Grinstedt et al. 2010) and analysis assessing physical constraints concerning the melt of ice shields (Pfeffer et al. 2008) combined with the lack of knowledge about how fast ice shield losses might procede especially in the West Antarctic (e.g. Katz and Worster 2010, Barnes and Hillebrand 2010, Jenkins et al. 2010) suggest, that sea level rise during the 21st century might be substantially higher than assessed in AR4 (a range of roughly 50-150cm sea level rise in the 21st century is proposed). Refs: Rahmstorf S., 2007: A Semi-Empirical Aproach to Projecting Future Sea-Level Rise, Science 315: 368-370 - Pfeffer W.T., J. T. Harper, S. O'Neel, 2008: Kinematic Constraints on Glacier Contributions to 21st-Century Sea-Level Rise, Science 321: 1340-1343 - Grinstedt A., J. C. Moore and S. Jevrejeva, 2010: Reconstructing sea level from paleo and projected temperatures 200 to 2100. Clim Dyn 34: 461- 472. - Katz R.F and Worster M.G., 2010: Stability of ice-sheet grounding lines. Proc. Roy. Soc. A: online 13 Jan 2010, doi: 10.1098/rspa.2009.0434) - Barnes D.K.A. and C.D. Hillenbrand, 2010: Faunal evidence for a late quaternary trans-Antarctic seaway. Glob. Change Biol.: online 5 Feb 2010, doi: 10.1111/j.1365-2486.2010.02198.x - Jenkins, A., Dutrieux, P., Jacobs, S.S., McPhail, S.D., Perrett, J.R., Webb, A.T. & White, D., 2010: Observations beneath Pine Island Glacier in West Antarctica and implications for its retreat, Nature Geoscience 3: 468 - 472) (Neu, Urs, Swiss Academy of Sciences)	Some of these papers are now discussed (the statistical-based projections and Pfeffer et al is cited but a more detailed treatment of ice sheet stability was deemed beyond the scope of this chapter
1610	3	69	17	69	20	Suggest some comments on the regional distribution of sea-level rise need to be added. (Church, John, CSIRO)	Regional sea level rise is now discussed in opening paragraph
1611	3	69	26	0	0	No need to change the AR4 assessment since the projected changes of the detailed studies are still consistent with earlier results reported in AR4. (Klein Tank, Albert, KNMI)	The consistency of the new european studies with the AR4 assessment is mentioned in the final paragraph
1612	3	69	33	69	51	It is not clear if the results from these studies include the effect of higher mean sea level. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	these studies focus on the storminess changes and not sea level rise. This has been clarified
1613	3	69	53	69	60	I'm wondering if there are studies for other regions. In searching, I found a review by Keith Thompson and colleagues that may be relevant (I'm speculating because I wasn't able to download the paper). See Thompson et al, 2009, Natural Hazards, 51, 139-150. (Zwiers, Francis, Environment Canada)	this paper presents mainly a review of the Bernier et al papers which have been cited
1614	3	70	9	0	0	Consistency between changes in waves and wind is missing in the present structure in which each variable is discussed in a separate section. Already on page 13 the remark is made that many extremes are linked. These links could be articulated somewhat more. (Klein Tank, Albert, KNMI)	the consistency of wind and wave changes is mentioned in the waves section
1615	3	70	11	70	11	Coastal safety is mentioned. It is suggested to mention also safety of marine structures. Waves and wind represent the most important input for specification of design and operational criteria for ships and offshore structures. The observed and projected climate changes have brought an important question: must rule and standards for marine structures be updated? (Bitner-Gregersen, Elzbieta Maria, Det Norske Veritas AS)	Have mentioned safety of marine structures in introduction to waves. The point about rules and standards is an issue for Chapter 4
1616	3	70	11	70	16	Suggest to add: Waves are also a very important factor for marine safety (Eide, Lars Ingolf, Det Norske Veritas)	This has been done
1617	3	70	14	70	15	"Direction" is mentioned. It is suggested to replace it by "directional wave energy spreading" or "the wave frequency-directional spectrum". Not only the main wave direction is important but also the wave energy spreading over wave frequencies. Directional wave properties are well described by the wave frequency-directional spectrum. Further, water velocity is a relevant parameter for safety assessment of marine structures. (Bitner-Gregersen, Elzbieta Maria, Det Norske Veritas AS)	direction has been replaced by the the wave energy directional spectrum
1618	3	70	20	70	23	Generally for trends reference periods should be provided. Studies focusing on longer periods suggest that short-term trends may be part of variability on longer time scales. (von Storch, Hans, GKSS Research Center)	Have gone through and added reference periods to all cited trends
1619	3	70	20	70	23	Gulev and Grigorieva (2004) reported positive trend in the NE Atlantic for the last half of the 20th century but negative trend first half. This should be reflected in this paragraph (Eide, Lars Ingolf, Det Norske Veritas)	the time interval relevant to this statement (1950-2002) has been inserted
1620	3	70	25	70	28	Reference periods for trends should be given. (von Storch, Hans, GKSS Research Center)	reference periods have been provided
1621	3	70	26	70	26	The study of Weisse and Günther (2007) provides analyses for the North Sea only. No statements are made for the North Atlantic and the North Pacific. (von Storch, Hans, GKSS Research Center)	restructuring of this section makes this clearer
1622	3	70	27	70	27	The "Wang et al. 2009b" here should be "Wang et al. 2009c". (Wang, Xiaolan, Environmen Canada)	it is the trends paper not the D&A paper I am citing here which is 2009b
1623	3	70	36	70	36	The "Wang et al. 2009b" here should be "Wang et al. 2009c". (Wang, Xiaolan, Environmen Canada)	it is the trends paper not the D&A paper I am citing here which is 2009b
1624	3	70	36	70	36	Wang et al (2009b) found that the storminess around 1880 was of the same magnitude as in the early 1990'ies, albeit at different season. This is consistent with Gulev and Griogorieva (2004) and should be reflected somewhere in chapter 3.5.4.1 (Eide, Lars Ingolf, Det Norske Veritas)	this has been noted
1625	3	70	44	70	0	Please specify the region considered in the study. (von Storch, Hans, GKSS Research Center)	Regions considered in the study have been mentioned
1626	3	70	45	70	45	Include a few words indicating the definition of wave height event intensity. (Zwiers, Francis, Environment Canada)	wave height event intensity has been defined
1627	3	70	50	70	50	Is the trend og 0.059m/yr for mean SWH? (Eide, Lars Ingolf, Det Norske Veritas)	trend is for waves > 3m and this is now stated

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1628	3	70	50	70	50	It should be stated what trend is referred here; the annual mean or the annual extreme? (Bitner-Gregersen, Elzbieta Maria, Det Norske Veritas AS)	this has been clarified (see above point)
1629	3	70	57	70	57	Do "wave heights" refer to SWHs? (Bitner-Gregersen, Elzbieta Maria, Det Norske Veritas AS)	the wave height metric has been inserted
1630	3	70	70	70	70	The word storm tide is used. In many places, storm surges are used. It may better to use storm surges (Alakkat, Unnikrishnan, National Institute of Oceanography)	In Australia, it is common place to use the term storm tide to represent the combined sea level from the tide and the surge, and the study cited refers to storm tide in its title, however I have clarified its meaning in the text
1631	3	70	0	0	0	In section 3.5.4, some comment of consistency of changes in waves and winds is desirable. (Church, John, CSIRO)	this has been addressed in the context of the projected changes by Mori et al
1632	3	71	7	71	11	Does the wind speed affect on the wave? If yes, please explain about the changes of the waves based on changes of the wind speed. (Davtalab, Rahman, Ministry of Energy)	this has been addressed in relation to the projections by Mori
1633	3	71	10	71	10	Does wave height refer to SWH? (Bitner-Gregersen, Elzbieta Maria, Det Norske Veritas AS)	yes, this has been amended
1634	3	71	15	71	16	Please cite the relevant Chapter (Chapter 3 of WGI) for the reference on Waves (Alakkat, Unnikrishnan, National Institute of Oceanography)	the relevant chapters have been cited by their lead authors
1635	3	71	15	71	0	AR4 more emphasizes a poleward shift of the mid-latitude storm track emerging as a more consistent results among different studies. Statements provided here reflect more the statements made in AR3. (von Storch, Hans, GKSS Research Center)	This sentence has been modified
1636	3	71	17	71	0	Please specify "north seas" (Northern Seas or North Sea); Pleae specify the mid-latitude regions considered. (von Storch, Hans, GKSS Research Center)	these regions have been specified
1637	3	71	26	71	26	Uncertainty arises not just from the small number of climate models, but also from their ability to simulated surface winds, historical change in surface pressure (modelled changes are weaker than observed), and consistency or lack there of in projections. (Zwiers, Francis, Environment Canada)	these points have now been reflected in the summary paragraph
1638	3	71	31	71	31	I wonder if "poorer" is the right word to use here, or whether there should be some explanation of the finding. One would expect statistical downscaling to perform better since it is more closely constrained by observations. On the other hand, one might still have greater trust in projections from RCMs given that they contain a (presumably) much more complete representation of the processes that will be responsible for future change. (Zwiers, Francis, Environment Canada)	reference to this study has been dropped due to space constraints and the fact that all post-Ar4 studies surveyed involved dynamical downscaling
1639	3	71	36	3	0	Is the study of Mori et al. (2009) indeed a global one? (von Storch, Hans, GKSS Research Center)	Mori et al 2010 paper is now cited and is a global study
1640	3	71	36	0	0	The following article seems more appropriate for citation, since it is peer-reviewed: Nobuhito Mori, Tomohiro Yasuda, Hajime Mase, Tracey Tom and Yuichiro Oku: Projection of Extreme Wave Climate Change under Global Warming, Hydrological Research Letters, Vol. 4, pp.15-19, (2010) . (Nakaegawa, Toshiyuki, Meteorological Research Institute)	Mori et al 2010 paper is now cited
1641	3	71	39	71	39	The "Wang and Swail 2006b" should be "Wang and Swail 2006". (Wang, Xiaolan, Environmen Canada)	In the restructuring and shortening of this section, I no longer cite this paper, focussing more on the post-ar4 papers
1642	3	71	41	0	0	After changing line, I suggest to add new sentences "Mori et al., (2010) also analyzed the same wave projection data set based on A1B scenario for extreme waves. The extreme wave heights due to tropical cyclones will increase significantly rather than averaged wave heights in the North-West Pacific and Southern Indian Ocean. The influence of global scale pressure change and tropical cyclone change have quite different impacts on wave climate and should be considered separately for extreme waves carefully." The English reference to be added is "Mori, N., T. Yasuda, H. Mase, T. Tom and Y. Oku, 2010: Projection of extreme wave climate change under the global warming, Hydrological Research Letters, Vol.4, pp.15-19. (doi:10.3178/hrl.4.15)" (Nakakita, Eiichi, Kyoto University)	Mori et al 2010 paper is now cited along with relevant extreme wave projections
1643	3	71	42	71	46	Debernard and Røed (2008) relate large uncertainty to the estimates presented. This should be reflected in this paragraph. (Bitner-Gregersen, Elzbieta Maria, Det Norske Veritas AS)	The general issue of uncertainty has been addressed in a summary paragraph. In the interests of brevity I have not cited all the quantitative uncertainty estimates
1644	3	71	48	71	50	Grabemann and Weisse (2008) assign an uncertainty to the mean value of 0.6 – 0.7m. This should be reflected in this paragraph. (Bitner-Gregersen, Elzbieta Maria, Det Norske Veritas AS)	see response above
1645	3	71	49	71	49	Grabmann and Weisse (2008) do not state: "increase of up to 18% from an ensemble mean". This number refer to one specific member of the ensemble only. (von Storch, Hans, GKSS Research Center)	This has been corrected
1646	3	71	54	0	0	In line with many earlier sections, it would be useful to end with a summary statement. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	the section has been restructured

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1647	3	71	56	72	4	Like Islands as in Japan, the local climate is strongly affected by coastal SST. Iizuka (2010) adopt a very high resolution SST instead of the SST of reanalysis data to nesting their regional climate model into AO-GCM's results, and found out the importance of SST distribution on the local climate of Japan. He found that around 200km off-shore SST is crucial for representing the local climate around the coastal climate. About the same thing is also reported around the coast of San-Francisco in CCArD10 project. Ref: (1) Iizuka, S., 2010. Simulations of wintertime precipitation in the vicinity of Japan: Sensitivity to fine-scale distributions of sea surface temperature. J. Geophys. Res. 115, D10107, doi.1029/2009JD012576. (Takayabu, Izuru, Meteorological Research Institute)	Due to space constraints, SST changes cannot be dealt with in a comprehensive way
1648	3	71	56	0	0	As noted in the general comments, this section (and figure 3.12) could be removed, with 'Coastal Impacts' left for chapter 4 to assess. (Stocker, Thomas, IPCC WGI TSU)	Wave height event intensity has been defined
1649	3	71	58	71	59	I think coastal upwelling should be also taken into account in the discussion, somewhere in the section. (Bojariu, Roxana, National Meteorological Administration)	The outcome of discussions with Chapter 4 was that we would take their coastal impacts material where it involves impacts on the physical environment
1650	3	71	0	0	0	Section 3.5.4, A brief description on increase in damage probability and magnitude on coastal defense facility such as seawall and tide gates due to extreme events under the climate change is necessary. (Kawai, Hiroyasu, Port and Airport Research Institute)	This is comment is relevant to chapter 4
1651	3	72	1	72	1	Suggest replacing "melting" with "reduction". Also, insert "mid and" before "high latitude". Reduction in the amount of seasonal sea ice for example, in the Gulf of St. Lawrence, is perceived as an important contributor to coastal erosion. This might also be an issue on the shores of the Great Lakes. (Zwiers, Francis, Environment Canada)	this change has been made
1652	3	72	7	72	8	I'm not sure that this kind of spaghetti diagram, linking virtually everything to everything, is all that helpful. (Zwiers, Francis, Environment Canada)	it has been deleted
1653	3	72	11	72	20	Wind driven coastal inundation (termed seiche) can also be an issue on the Great Lakes. I wasn't able to find literature, but perhaps someone at NOAA's GLERL can point to some literature. (Zwiers, Francis, Environment Canada)	The list here is only to provide a few examples. Does not need to be exhaustive and cover all regions of the world.
1654	3	72	14	72	15	Add "during a La Nina event" after "to the south" - also correct spelling is "Cape York". (Trewin, Blair, Australian Bureau of Meteorology)	this section has been shortened and the relevant text removed in the interests of brevity
1655	3	72	16	72	16	These frontal systems would normally be associated with an extratropical cyclone to the south. (Trewin, Blair, Australian Bureau of Meteorology)	True, but it is the winds in the front at the coast that cause the surge. Inverse barometer effect in most cases has only a small contribution to sea level heights because the storm system is so remote from the coast.
1656	3	72	22	72	32	Should this list also include isostatic rebound, land subsidence from activities such as ground water pumping, effects of coastal engineering, etc? (Zwiers, Francis, Environment Canada)	good point. It has been added to the list of causes for change in shoreline position as #2
1657	3	72	31	72	31	Is 'buffering' the right word here? Is it not the sea ice that was providing the buffering effect? (Stocker, Thomas, IPCC WGI TSU)	the word buffering has been removed
1658	3	72	41	72	47	Should this list include permafrost, seasonal coastal sea ice, etc.? (Zwiers, Francis, Environment Canada)	this has been added
1659	3	72	56	74	28	3.5.5. This material may be more appropriate for chapter 4. (IPCC WGII TSU)	it was decided to retain this section in chapter 3 and to transfer relevant text from chapter 4 to chapter 3
1660	3	72	56	0	0	Suggest in Section 3.5.5.1 you may wish to refer to Syvitski, J. P. M., A. J. Kettner, et al. (2009). "Sinking deltas due to human activities." Nature Geosci 2(10): 681-686 and related studies. (Church, John, CSIRO)	This reference has now been added
1661	3	72	58	72	58	Impacts' would be a better term to use here than 'hazards'. (Stocker, Thomas, IPCC WGI TSU)	this change has been made
1662	3	73	14	73	14	Replace "islands" with "island states" - each country has multiple islands. (Trewin, Blair, Australian Bureau of Meteorology)	this change has been made
1663	3	73	14	73	19	This increase in island area is an interesting result and may draw greater scrutiny as a result. For this reason the difference between 'net' and 'gross' changes needs to be better explained. (Whetton, Penny, CSIRO Marine and Atmospheric Research)	this has been rephrased
1664	3	73	25	73	25	There must be an enormous amount of regional literature, so I'm wondering what the strategy was for selecting papers for discussion. It might be useful to include a sentence or two at the beginning on the approach that was used for making the selection. (Zwiers, Francis, Environment Canada)	Very difficult to produce a simple explanation - many factors were considered in selecting regional literature. Not sure why reviewer thinks this is important.
1665	3	73	28	73	34	This section needs expansion, and more discussion of the Webb and Kench results. (Trewin, Blair, Australian Bureau of Meteorology)	the details of this study have been expanded slightly although space constraints limit the discussion
1666	3	73	53	0	0	No need for project advertisement such as SURVAS here. In the remainder of the text I find very few references to project acronyms which is appreciated. (Klein Tank, Albert, KNMI)	reference to SURVAS has been deleted
1667	3	74	30	74	30	I would prefer a heading like mass movements rather than mountain impacts (Wehrli, Andre, European Environment Agency)	the title has been changed to "Glacier, geomorphological and geological impacts"
1668	3	74	30	77	21	3.5.6. This material may be more appropriate for chapter 4. (IPCC WGII TSU)	It was discussed between Ch3 and Ch4 and decided to have the material reside with Ch3.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1669	3	74	30	0	0	"Glacial floods and landslides" would be a more appropriate, and more useful title focussing on the main processes assessed in this section. The section is dominated by permafrost related impacts with much less attention given to glacier retreat. This can probably be justified, as the report is about 'extreme events and impacts' and glacial retreat in most instances may not be considered 'extreme'. This focus can be made clearer if the new proposed title is used. (Stocker, Thomas, IPCC WGI TSU)	the title has been changed to "Glacier, geomorphological and geological impacts" to reflect that floods, landslides, and other geological impacts being covered in the section.
1670	3	74	30	0	0	The assessment is currently heavily focussed on research from the European Alps. Please improve the regional coverage and associated coverage of literature to the extent possible, and please indicate which regions are lacking from the assessment due to insufficient literature. The 'Projected changes' section for example, is almost entirely focussed on the European Alps. (Stocker, Thomas, IPCC WGI TSU)	This was in part unavoidable because of coverage of literatures. However, a CA from North America has added more literatures for other regions.
1671	3	74	30	0	0	The subsections of 3.5.6 (glaciers and mountain impacts) need to be restructured via some cut and pasting. Much of the material reported under 3.5.6.2 refers to observations, and should be put under 3.5.6.1. (Stocker, Thomas, IPCC WGI TSU)	the section 3.5.6.1 and 3.5.6.2 have been restructured.
1672	3	74	30	0	0	As with other sections of this chapter, this section should begin first with a clear statement referring to what was known/concluded in the AR4 which is then followed by the current assessment of the latest literature. If the AR4 had nothing to say on this subject, then this is important, and should also be clearly stated. (Stocker, Thomas, IPCC WGI TSU)	the section now follows a new structure as used in other sections that is also similar to what you suggested here.
1673	3	74	30	0	0	Section 3.5.6.: My review is focussed on this section. In general, the section is well written and clear. However, I have three substantive comments, that are outlined below: (Huss, Matthias, University of Fribourg)	thanks.
1674	3	74	30	0	0	Section 3.5.6.: I have noted a strong bias of the cited literature towards one scientific group (Huggel, Haeberli etc.) based at the same university: in the first subsection, the relation of references from this group to scientists based at other universities is 12:13, in the second subsection 18:41, and in the third subsection even 17:9. In total, almost 50% of all references in section 3.5.6. (Glaciers and Mountain Impacts) thus goes to the same group. Whereas I am aware of the activity of the mentioned group in this field of research - and I appreciate it - I strongly suggest providing a more balanced reference list. This is in my opinion particularly important in this kind of report that should give a broad overview on research activities performed worldwide. (Huss, Matthias, University of Fribourg)	This was in part unavoidable because of coverage of literatures. However, a CA from North America has added more literatures for other regions.
1675	3	74	30	0	0	Section 3.5.6.: The title of the section is "Glaciers and Mountain Impacts". Although the focus of the report is strongly on extreme events and disasters, I was a bit surprised to find that of the 19 paragraphs in section 3.5.6. only one (short) paragraph is on glaciers. Compared to the title this seems to be disproportionate. There are many other mountain impacts related to glacier retreat that are not at all discussed here: What about sea level rise, impacts on the water supply security, hydropower production. All these factors have a strong relation to glacier changes, and moreover, allow predictions with a significantly lower uncertainty than landslide or rockfall activity that are mainly addressed in this section. In my view, this section also focuses too strongly on permafrost changes (relative to the paragraphs for glacier retreat), as permafrost is a main focus of the section 3.5.7. My suggestion is to extend and complete the discussion of the direct impacts of glacier change. More details could be provided. Additionally, inserting the Himalaya case study (see section 4.3.4.5.) would probably make sense. (Huss, Matthias, University of Fribourg)	the section has been restructured with addition of geological impacts. Note that why what supply and hydro-power etc. are very important, the focus of SREX is extreme events and disasters. As a result, impacts on water supply (even though that may be more important) is not covered in this report. Sea level rise is covered in a different section.
1676	3	74	30	0	0	Section 3.5.6.: This comment is related to the one above: According to the introductory sentence, this section mainly discusses "gravity-driven mass movements". Are mass movements really the most important "Mountain impact" of climate change. According to the text, the direct relation of the number of mass movements and climate change is not clear beyond any doubt. Process understanding and modelling indicates that climate change is likely to have an impact on "gravity-driven mass movements", but this is not always proved by observational evidence. It might be misleading to highlight mass movements as the major impact of climate change in high-mountain areas. Mass movements are certainly very important, but other impacts are much more direct indicators of climate change. I do not want to urge the authors to completely change the focus of their section, but I suggest to reconsider the attention paid to other mountain impacts of climate change, such as glaciers (that prominently appear in the section-title, but obtain less than 5% of lines in the section). (Huss, Matthias, University of Fribourg)	see response above. Note that glacier retreat itself is not the focus unless it is related to extreme and disaster events.
1677	3	74	30	0	0	3.5.6 Comment: A large quantity of snow melting water sometimes triggers deep-seated landslides in heavy snow covered region (Matsuura et al., 2008). Occurrence frequency of deep-seated landslides triggered by snow melting water may change, when the heavy snowing regions change by the climate change. (Matsuura, S., S. Asano and T. Okamoto, 2008: Relationship between rain and/or meltwater, pore-water pressure and displacement of a reactivated landslide, Engineering Geology, 101, 49-59) (Asano, Shihou, Forestry and Forest Products Research Institute)	Was not included because of space limitations. Section 3.5.6 has been extensively revised and notes the possibility that changes in precipitation and melting can trigger landslides

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1678	3	74	32	74	32	I think a bit more introduction is required here, such as some mention of the climate factors that can affect the likelihood of gravity driven mass movements. Also, a brief description of the scope of this section should be included. Gravity flows are an obvious cause of extreme impacts within Glacier and mountain regions, but should there also be consideration of things that happen in these regions that might have disastrous impacts elsewhere? For example, I'm wondering about changes in snow and ice storage in mountain areas that affect downstream water supplies. (Zwiers, Francis, Environment Canada)	A brief introduction on the scope of this section is added.
1679	3	74	32	74	33	Rather rephrase to: The topography of mountains is prone to gravity-driven mass movements such as landslides, avalanches, debris flows and flash floods that can lead to disasters. Reasons: steep is relatively vague and you do not necessarily need a very steep slope to have a gravity-driven process; the processes are not restricted to high-mountains. (Wehrli, Andre, European Environment Agency)	Agreed. Text modified
1680	3	74	32	0	0	Do not hyphenate "high mountains". (Cogley, J. Graham, Trent University)	text modified.
1681	3	74	35	75	6	3.5.6.1. Are the numbers from Zemp for decreases in glacier mass global averages? (IPCC WGII TSU)	This is for European Alps. This is now noted in the text.
1682	3	74	37	74	37	"unprecedented": What does that mean? Unprecedented in historic times? Since the beginning of the measurements? Since the Ice Age? Systematic measurements are only performed since a few decades. The word "unprecedented" thus seems to be too bold. (Huss, Matthias, University of Fribourg)	"unprecedented" is removed.
1683	3	74	37	74	37	The available data and the respective literature do not allow to quantify the present glacier retreat is 'unprecedented'. This statement must be changed. (Kaser, Georg, University of Innsbruck)	"unprecedented" is removed.
1684	3	74	37	74	37	whole chapter 3.5.6: omit high- before mountains. (Wehrli, Andre, European Environment Agency)	text modified.
1685	3	74	37	0	0	A word 'unprecedented' is too strong to address the recent glacier retreat. (Fujita, Koji, Nagoya University)	"unprecedented" is removed.
1686	3	74	38	74	38	All over the world' would be better worded as 'in most regions of the world'. This would thereby silence sceptics who might point towards Norway and the west coast of New Zealand as exceptions. (Stocker, Thomas, IPCC WGI TSU)	text modified.
1687	3	74	38	74	38	The most recent paper that gives numbers from a global perspective is: Cogley, J.G. (2009): Geodetic and direct mass-balance measurements: comparison and joint analysis. Annals of Glaciology, 96 - 100. In order to save space I suggest to remove the papers that only address regional findings. (Kaser, Georg, University of Innsbruck)	text modified.
1688	3	74	39	74	40	A better reference than Haeblerli and Hohmann 2008 might be Reichert, B.K., L. Bengtsson and J. Oerlemans, 2002, Recent glacier retreat exceeds internal variability, Journal of Climate, 15(21), 3069-3081. (Cogley, J. Graham, Trent University)	new reference added.
1689	3	74	42	0	0	Change "are caused by" to "have occurred in parallel with". (Cogley, J. Graham, Trent University)	text modified.
1690	3	74	47	74	47	The Little Ice Age reached its maximum in about 1850 in Europe, but not everywhere in the world. (Huss, Matthias, University of Fribourg)	text deleted.
1691	3	74	47	0	0	"glaciers have been predominantly retreating". (Cogley, J. Graham, Trent University)	text deleted.
1692	3	74	48	74	50	Delete "been". Change "towards the" to "during". Insert "annual" before "thickness losses". (Cogley, J. Graham, Trent University)	text deleted.
1693	3	74	49	74	49	"with ANNUAL ice thickness losses" (Huss, Matthias, University of Fribourg)	text deleted.
1694	3	74	49	74	50	Are the figures quoted global averages? (Trewin, Blair, Australian Bureau of Meteorology)	text deleted.
1695	3	74	50	74	50	It might be more appropriate to cite Kaser et al. (2006) here. This was the basis of the IPCC report 2007, and includes all glaciers worldwide (Huss, Matthias, University of Fribourg)	Text substantially revised
1696	3	74	51	74	53	Delete this sentence. The numbers are typical and do not give any idea about secular increase or about extremes. (Cogley, J. Graham, Trent University)	text deleted.
1697	3	74	51	74	53	What is the definition of "downwasting"? The numbers provided here are determined almost exclusively by glacier size. They do not allow any climatic interpretation except that the sign of glacier change is negative. They are thus difficult to interpret, and not very informative in this context. (Huss, Matthias, University of Fribourg)	text deleted.
1698	3	74	51	74	53	Retreat rates of glacier terminus from specific regions are meaningless because it depends on glacier size and magnitude of climate change. (Fujita, Koji, Nagoya University)	text deleted.
1699	3	74	55	77	21	A general comment about section 3.5.6 (Glaciers and Mountain Impacts) is that a great deal of the material ought to be in section 3.5.7 (Permafrost and High-latitude Impacts). The partition into "glaciers and mountain" versus "permafrost and high-latitude" has not been useful to my mind, and reorganization is needed. (Cogley, J. Graham, Trent University)	the text has been reorganized.
1700	3	74	56	0	0	Change "including" to "ranging over". (Cogley, J. Graham, Trent University)	text deleted.
1701	3	74	58	74	63	I have the impression that, for example, this paragraph is too detailed compared to other topics. If half a dozen examples of slope failures are discussed, also large ice and snow avalanches would need to be mentioned. (Huss, Matthias, University of Fribourg)	Text revised
1702	3	75	2	75	3	Should highlight the use of satellite remote sensing as the primary reason for this increased level of documentation over recent years. (Stocker, Thomas, IPCC WGI TSU)	text modified.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1703	3	75	9	75	9	Again, I think it would be useful to have a sentence outlining the strategy used to select papers for discussion from the literature. (Zwiers, Francis, Environment Canada)	Reject. We have tried to provide a balanced overview of the literature.
1704	3	75	10	0	0	"glaciers", not "glacier". And surely "or" should be "and"? (Cogley, J. Graham, Trent University)	text deleted.
1705	3	75	13	0	0	Change "and/or" to the less confusing "or". (Cogley, J. Graham, Trent University)	text deleted.
1706	3	75	14	75	22	These sentences on GLOFS should be moved up to the end of section 3.5.6.1, as they report observations. Lines 10 - 14 should stay however, as these describe the possible causes. (Stocker, Thomas, IPCC WGI TSU)	text has been moved up
1707	3	75	16	0	0	Narama et al. (2006) and Aizen et al. (2007) do not deal GLOF issue while Narama et al. (2010, NHESS) do. Careful citation is required. (Fujita, Koji, Nagoya University)	Reference changed
1708	3	75	16	0	0	Narama, C., Duishonakunov, M., Kääh, A., Daiyrov, M., and Abdрахmatov, K.: The 24 July 2008 outburst flood at the western Zyndan glacier lake and recent regional changes in glacier lakes of the Teskey Ala-Too range, Tien Shan, Kyrgyzstan, Nat. Hazards Earth Syst. Sci., 10, 647-659, doi:10.5194/nhess-10-647-2010, 2010. (Fujita, Koji, Nagoya University)	Reference added
1709	3	75	17	0	0	Cite also Dwivedi et al. (2000) for the Himalayan GLOF. See Chapter 4, page 98, line 48-49. (Fujita, Koji, Nagoya University)	rejected. There are already many references
1710	3	75	20	75	22	After the 1998 GLOF of Tam Pokhari (Dwivedi et al., 2000; Osti and Egashira, 2009), no GLOF has occurred in the Himalayas. This should be noted. (Fujita, Koji, Nagoya University)	text modified.
1711	3	75	29	0	0	Change "small-scale rock fall events" to "small rock falls". (Cogley, J. Graham, Trent University)	text deleted.
1712	3	75	32	0	0	What does "follow a stochastic pattern" mean in this context? Perhaps "occur irregularly"? Not "occur unpredictably", because the text makes it clear that precipitation is likely to be a predictor. (Cogley, J. Graham, Trent University)	text deleted.
1713	3	75	37	75	38	A reference is needed for thei statement (Kaser, Georg, University of Innsbruck)	text modified.
1714	3	75	42	75	46	The sentence beginning 'Research has so far.....' should be moved up into the observation section (3.5.6.1). (Stocker, Thomas, IPCC WGI TSU)	text modified.
1715	3	75	45	0	0	Change "could be" to "has been", and delete "high-mountain". (Cogley, J. Graham, Trent University)	text modified.
1716	3	75	47	75	49	Despite the statistics not being clear, are there some citations to help support this sentence describing possible increasing debris flow activity? (Stocker, Thomas, IPCC WGI TSU)	text modified.
1717	3	75	47	75	49	Change "could be" to "might have been". What does "a snow fall line located at high elevation" mean? "a rise of the snowline"? (Cogley, J. Graham, Trent University)	text deleted.
1718	3	75	47	75	49	Please add a reference for this statement, otherwise it sounds like a speculation: "Statistics are not completely clear but there could be an increase of debris flow activity in alpine regions..." (Wehrli, Andre, European Environment Agency)	text deleted.
1719	3	75	51	0	0	Delete "post Ice Age and". Perhaps replace it with "late Wisconsinan and"? (Cogley, J. Graham, Trent University)	text deleted.
1720	3	75	53	75	59	These sentences on cascading/interacting processes should also be moved up into the observation section (3.5.6.1). (Stocker, Thomas, IPCC WGI TSU)	text deleted.
1721	3	75	57	75	59	as for the large events triggered by volcanic eruptions, I suppose that you do not expect them to become more frequent due to climate change (reads a bit like that now...) (Wehrli, Andre, European Environment Agency)	text deleted.
1722	3	75	63	0	0	Change "related depth increase" to "consequent thickening". More importantly, define "active layer". There seems to be a good case for a diagram here, to illustrate the technical terms used to describe the morphology of frozen ground. The term "frost table", or even "end-of-summer frost table", could make some of the discussion more accessible to general readers. (Cogley, J. Graham, Trent University)	text deleted.
1723	3	76	1	0	0	What does "thaw consolidation after melt" mean? (Cogley, J. Graham, Trent University)	text deleted.
1724	3	76	2	0	0	Change "permafrost" to "the frost table". (Cogley, J. Graham, Trent University)	text modified.
1725	3	76	7	0	0	Change "snow fall limit" to "snowline", or explain it if I have misunderstood it. (Cogley, J. Graham, Trent University)	text modified.
1726	3	76	8	76	13	These sentences on rock glaciers should also be moved up into the observation section (3.5.6.1) (Stocker, Thomas, IPCC WGI TSU)	text has been reorganized
1727	3	76	10	0	0	"glaciers surface flow speed of up to ..." is ambiguous. Say "glaciers, with surface flow speeds increasing by up to ...". If "by up" is not what is meant, omit it. (Cogley, J. Graham, Trent University)	text deleted.
1728	3	76	16	76	18	I do not understand what point this sentence is trying to make. That response can be rapid? Long delayed? Say "... confirmed that slopes may respond to ... or less". (Cogley, J. Graham, Trent University)	text modified
1729	3	76	16	0	0	Change "due to" to "following". (Cogley, J. Graham, Trent University)	text modified
1730	3	76	18	0	0	Change "reached" to "penetrated to". (Cogley, J. Graham, Trent University)	text modified
1731	3	76	20	0	0	Delete "duration". (Cogley, J. Graham, Trent University)	text deleted.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1732	3	76	23	76	30	An assessment is made here that there is a likely human influence on landslides, avalanches and GLOFS. Note this is a multi-step type of attribution and as such needs to be calibrated with other attribution assessments such as that of intense tropical cyclones at page 56. Top of page 65 it states "Quantification of trends in occurrence of such events is difficult.." which is precisely the sort of reservation expressed on tropical cyclones. Great care needs to be taken to ensure calibration of attribution assessment across the report and in particular the lack of detectability should not be applied inconsistently. (Stott, Peter, Met Office)	There is now a clear description on how the multi-step assessment is made.
1733	3	76	24	0	0	Hyphenate "high-mountain". (Cogley, J. Graham, Trent University)	text deleted.
1734	3	76	27	0	0	"influence of" (not "from"). (Cogley, J. Graham, Trent University)	text deleted.
1735	3	76	28	0	0	End sentence at "driven", then begin a new sentence with "It is difficult ...". (Cogley, J. Graham, Trent University)	text modified.
1736	3	76	32	77	21	3.5.6.3. Consider using the confidence scale for this kind of change. (IPCC WGII TSU)	there is mixed likelihood/confidence scale in the assessment, following the guidance note
1737	3	76	32	77	21	3.5.6.3. Is an increase in the number of glacier lakes a major conclusion requiring an uncertainty term? (IPCC WGII TSU)	text modified
1738	3	76	32	0	0	This section on projections must also assess/consider associated uncertainties. (Stocker, Thomas, IPCC WGI TSU)	uncertainties are now considered.
1739	3	76	35	76	37	"Glaciers in the European Alps are projected to shrink by 20% to more than 50% (of the glacier area in 2000; Zemp ...) for a 2-3 ...". Do not include both the date 2050 and the assumed temperature increase. (Cogley, J. Graham, Trent University)	text modified.
1740	3	76	35	0	0	Change "further reduce" to "shrink further". (Cogley, J. Graham, Trent University)	text deleted.
1741	3	76	37	0	0	"glacier decay" is not a technical term, and it may be ambiguous. Be more precise, e.g. "glacier mass loss". (Cogley, J. Graham, Trent University)	text modified
1742	3	76	38	76	38	Why are glaciers are in imbalance? This should be stated here more directly (Huss, Matthias, University of Fribourg)	text deleted.
1743	3	76	38	76	38	"..., and, as a result glaciers are increasingly in an imbalance". If this is meant as a general statement, it is not correct. Glaciers always tend to reach balance and, under changing climate conditions, they change their extent for this. I suggest to remove this part of the sentence. (Kaser, Georg, University of Innsbruck)	text deleted.
1744	3	76	38	0	0	Delete ", and as a result glaciers are increasingly in an imbalance", which is not logical and is also repetitive. (Cogley, J. Graham, Trent University)	text deleted.
1745	3	76	41	0	0	Change "result in impact waves" to "lead to damaging surge waves". (Cogley, J. Graham, Trent University)	text modified
1746	3	76	43	76	49	These sentences are quite confused, and fail to acknowledge results in heat conduction that have been understood for 200 years. I suggest instead "A change of temperature at the surface will propagate into the subsurface by thermal conduction, but will take longer and longer to reach progressively greater depths. Decades to centuries are required for the change to reach depths of a few tens of metres. Ice may have greater thermal conductivity than soil and rock, and may respond more rapidly to changes in near-surface air temperature." (Reinsert references as appropriate, and check the correctness of the second of these sentences - its counterpart in the existing text seems doubtful to me.) (Cogley, J. Graham, Trent University)	text modified
1747	3	76	50	0	0	Change "melting water" to "meltwater", and start a new sentence after "lower". (Cogley, J. Graham, Trent University)	text deleted.
1748	3	76	54	0	0	Explain what an "advection corridor" is. Change "have a triggering effect for" to "trigger". (Cogley, J. Graham, Trent University)	text deleted.
1749	3	76	56	0	0	Bring the parenthesis forward, so as to read "Warm extremes (lasting 5, 10 and 30 days) with a potential ... several RCMs are projected to increase in frequency, relative to a 1951-2000 reference period, by 1.5 ...". (Cogley, J. Graham, Trent University)	text deleted.
1750	3	76	62	76	62	Perhaps it would be better to say 'particularly local geological conditions...' as this probably remains the most important factor. (Stocker, Thomas, IPCC WGI TSU)	text modified
1751	3	76	62	0	0	Comma after "geological conditions". (Cogley, J. Graham, Trent University)	text modified
1752	3	77	4	77	21	Many readers will not grasp that a switch is being made here from avalanches and rock falls to "shallow landslides". Given that they are all projected to increase, it may be possible to write more economically by treating them all together. Alternatively, emphasize somehow at Line 4 that the subject is changing. (Cogley, J. Graham, Trent University)	text modified
1753	3	77	8	0	0	Change "have earlier onset" to "begin earlier in the year". (Cogley, J. Graham, Trent University)	text modified
1754	3	77	10	0	0	Why *lower* mountain ranges? (Cogley, J. Graham, Trent University)	text deleted.
1755	3	77	13	77	14	End the sentence with "long-lasting rainfalls (days to weeks) and short, intense rainfalls." (Cogley, J. Graham, Trent University)	text deleted.
1756	3	77	15	77	15	Avoid the use of the word 'likely' here unless it is intended to be a formal likelihood statement. (Stocker, Thomas, IPCC WGI TSU)	text deleted.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1757	3	77	17	77	18	Lemke et al. (2007) do not make such a statement. (Kaser, Georg, University of Innsbruck)	text deleted.
1758	3	77	19	0	0	"... uncertainty in the location and timing ...". (Cogley, J. Graham, Trent University)	text deleted.
1759	3	77	21	77	21	The Sidle and Ochiai citation might be better placed at the end of line 15, where the effect of land-use practices is first discussed. (Stocker, Thomas, IPCC WGI TSU)	text modified
1760	3	77	23	0	0	The title would be better as "High latitude changes in permafrost" - the previous section also includes permafrost, so this distinction here to a high latitude focus is essential. (Stocker, Thomas, IPCC WGI TSU)	title modified
1761	3	77	23	0	0	As with other sections of this chapter, this section should begin first with a clear statement referring to what was known/concluded in the AR4 which is then followed by the current assessment of the latest literature (Stocker, Thomas, IPCC WGI TSU)	the section has been restructured.
1762	3	77	23	0	0	It was surprising to find no assessment in this section regarding the relationship between thawing permafrost and C4 release, related implications for accelerated warming and feedbacks. This is a very important high latitude impact that should be covered here. (Stocker, Thomas, IPCC WGI TSU)	Disagree. While C4 release is perhaps one of the most important impacts of thawing permafrost, there is no apparent link between C4 release and extremes/disaster events that are the focus of SREX.
1763	3	77	25	0	0	"high-mountain". (Cogley, J. Graham, Trent University)	text modified.
1764	3	77	27	0	0	"Permafrost responds sensitively to climatic variability ...". But is this cliché true, and if true is it necessary to use it? On P76, lags of decades for temperature changes to reach depths of metres were described, so "sensitively" seems a bit of a stretch. (Cogley, J. Graham, Trent University)	text deleted.
1765	3	77	31	0	0	Change "over the permafrost" to "near the surface". A definition sketch for the morphology of frozen ground would be helpful. (Cogley, J. Graham, Trent University)	text modified.
1766	3	77	33	0	0	Delete "as a whole". (Cogley, J. Graham, Trent University)	text deleted.
1767	3	77	39	77	44	Observations must be very sparse, so I'm wondering if caveats are required. (Zwiers, Francis, Environment Canada)	"Limited" is added.
1768	3	77	39	77	45	"at the top of the permafrost" must be replaced with something more careful. By definition, the temperature is at the freezing point at the frost table, which attains its maximum annual depth some days to months (depending on how deep the thawing penetrates) after the maximum temperature is reached at the surface. So I cannot guess what is meant by the temperature at the "top of the permafrost" increasing by up to 3deg C. The notion of "permafrost temperature" in the following sentences may also confuse readers; it can only be the ground temperature - which happens to be below freezing. At Line 44, the first "permafrost" can simply be deleted. (Cogley, J. Graham, Trent University)	text modified
1769	3	77	39	0	0	"Observations show that ...". (Cogley, J. Graham, Trent University)	text modified
1770	3	77	42	77	42	It is not clear if this 1.0 degree increase is still referring to the period since the early 1980s? (Stocker, Thomas, IPCC WGI TSU)	text modified
1771	3	77	45	0	0	Explain the "snow insulation effect" briefly. I assume that it arises because the snow (having *low* thermal conductivity) reduces the penetration of the winter cold wave and thus enhances the efficiency of penetration of the summer warm wave. (Cogley, J. Graham, Trent University)	the text has been changed to "increase in snow depth" to make it clear.
1772	3	77	48	0	0	"Observations show that ...". (Or just begin "Active layer ..."). (Cogley, J. Graham, Trent University)	text modified
1773	3	77	50	0	0	", but there has been no significant trend in the North ...", and move the North American clause to the end of the sentence (after the Tibetan change). Delete "from". (Cogley, J. Graham, Trent University)	text modified
1774	3	77	52	0	0	Delete "the" before "central Yakutia". (Cogley, J. Graham, Trent University)	text deleted.
1775	3	77	58	77	62	Is permafrost melting the only factor here? I would have thought the longer open-water season (and hence more exposure to wave action) would also be relevant. (Trewin, Blair, Australian Bureau of Meteorology)	text modified "long sea ice free season" to "longer sea ice free season".
1776	3	77	58	0	0	Explain the sense in which coasts are the *most* sensitive regions. (Cogley, J. Graham, Trent University)	text restructured.
1777	3	77	59	0	0	"longer sea-ice-free seasons ... some Arctic coastlines are retreating at 2 to 3 ...". (Cogley, J. Graham, Trent University)	text modified
1778	3	77	61	77	62	Rather than citing figures from Karl et al (2009), which is a synthesis of assessment reports, it would be better to go to the primary literature. (Zwiers, Francis, Environment Canada)	agreed.original references cited
1779	3	77	61	0	0	Also Line 62, and Page 78 Line 21: it is probably not a good idea for this assessment to cite a different assessment (Karl et al.). Try to find the original reference. (Cogley, J. Graham, Trent University)	agreed. Original references cited.
1780	3	78	2	78	21	These two subsections are repetitive and weak. They should be improved as part of the reorganization suggested in my comment at Page 74 Line 55. (Cogley, J. Graham, Trent University)	the section has been combined, repetition is removed.
1781	3	78	2	78	3	Earlier/thicker compared with what? (Trewin, Blair, Australian Bureau of Meteorology)	this is the trend, text modified.
1782	3	78	5	78	7	This sentence needs to follow the sentence at Page 77 Lines 45-47. Say "much greater", not "much higher". (Cogley, J. Graham, Trent University)	text modified
1783	3	78	8	78	10	Except for the citation, this sentence repeats material in the last paragraph of Page 77. (Cogley, J. Graham, Trent University)	text in earlier paragraph has been removed.
1784	3	78	10	0	0	"Expansion of lakes in ...". (Cogley, J. Graham, Trent University)	text modified

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1785	3	78	15	78	21	This section on projections must also assess/consider associated uncertainties. (Stocker, Thomas, IPCC WGI TSU)	new uncertainty language used.
1786	3	78	17	78	21	It is not clear whether the assessment that is given comes from the AR4, or whether it is new. Irrespective, it should be supported by new evidence if available. I don't think citing Karl et al (2009), which is a synthesis of assessment reports produced in the same time frame as the AR4, is sufficient. (Zwiers, Francis, Environment Canada)	the assessment is based on what was in AR4 and new evidence
1787	3	78	23	78	23	please use the grey literature and mention on the severe dust storm and developing and increasing dust phenomena in some part of middle east including (Iraq, Iran and Turkey). Of course we (Iran and Pakistan) experience dust storm for 90 -120 days in Sistan and Balochistan (including southeast of Iran, west of Pakistan) that are expected and are stable too. (not significant trend) . (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	We are trying to avoid using grey literature, unless absolutely necessary.
1788	3	78	23	79	13	In this part we could- I think- mention what would specifically affect ways of life of dwellers of five oases in western sahara of Egypt - the vast sahara of north Africa and its people. The white desert and its rocky sculptures in western desert of Egypt tells the story of sand storms of that part. Whales valley - as a historic part in the world treasures- in Fayum, Egypt, could be damaged from that kind of storms. (Yasseen, Adel, Ain Shams University - Institute of Environmental Research and Studies)	Good idea but we'd need peer-reviewed literature to reference.
1789	3	78	23	79	13	3.5.8. This material may be more appropriate in chapter 4. (IPCC WGII TSU)	it was decided this being covered in Ch3
1790	3	78	25	79	13	This section is extremely short. I am not an expert on this topic, but there is most certainly much more literature on the frequency of dust storms and their impact on climate, weather systems, and human population. I suggest to either eliminate this section or to expand it and discuss the topic as thoroughly as the previous ones. (Wernli, Heini, ETH Zürich)	the literature is indeed quite limited.
1791	3	78	32	78	42	3.5.8.1. Last sentence has no citation. (IPCC WGII TSU)	text revised.
1792	3	78	41	78	42	How are dust events defined, and do all authors define them in the same way? (Zwiers, Francis, Environment Canada)	Many dust definitions - but do not believe such detail is useful. Would need to vastly expand the chapter if we were to include such detailed explanations, because we would need detailed discussions/explanations of many other features - if we really needed to define "dust event".
1793	3	78	49	78	49	Add "in source regions" after "surface conditions". (Trewin, Blair, Australian Bureau of Meteorology)	text modified.
1794	3	78	57	78	57	The "Wang et al. 2006c" here should be "Wang et al. 2006b" as a result of the change suggested in Comment No 14. (Wang, Xiaolan, Environment Canada)	reference modified.
1795	3	78	0	0	0	Section 3.5.7.3: Add Lawrence, D.M. and A.G. Slater, 2005, A projection of severe near-surface permafrost degradation during the 21st century, Geophys. Res. Letts., 32, L24401, doi:10.1029/2005GL025080 (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Reference added
1796	3	79	4	79	13	3.5.8.3. This section does not cite a single paper on the topic of dust storms. Can we really justify a section of an IPCC report on a topic with no literature that addresses it directly? (IPCC WGII TSU)	This does not provide projections to dust storm but some important contributors to dust storm. It is now combined with another section.
1797	3	79	4	0	0	This section on projections must also assess/consider associated uncertainties. (Stocker, Thomas, IPCC WGI TSU)	it has been assessed that way now.
1798	3	80	1	113	0	The use of references so far in the draft is quite unbalanced, with missing references in parts and abundance in others. The list of references is also somewhat excessive, stretching over 33 pages, but not all references are equally relevant. I find that direct relevant publications are missing, which weakens the authority of the report. Furthermore, due recognition of work and ideas is both the norm and ethics of the scientific community - hence I was puzzled to see so few papers by D.B. Stephenson and R. Huth, who have published a number of papers on extremes/downscaling, and by not seeing any reference to any of the ~20 papers on analysis of extreme climate events or downscaling which I have published in the peer review literature. In fact, parts make statements directly connected to some of my work, without any references. And if one carries out a Google Scholar Search with key word "record-event", none of the 3 first hits relevant to climate are mentioned in the report. This is a concern because it begs the more important question: How many other relevant studies has this report missed? If this report is meant to provide an assessment of the present state of knowledge, and several central issues and studies are omitted, the report is bound to be torn apart by critics. Furthermore, it is important that the inclusion and exclusion of references are not politically motivated nor affected by the choice of authors and reviewers. There are already many references made to submitted papers not yet published - doesn't the IPCC have any guideline about this? The reference list must reflect the body of knowledge. Therefore, I advise the authors to review the literature once more - e.g. using Google Scholar or ISI web of science. I have provided some suggestion regarding some publications of which I'm aware about. If the reference list already is too long, then many of the references are older and covered in AR4 - should this report repeat those findings and is it necessary to include a large number of old papers published by some of the authors? Some publications must be more relevant than others, and efforts need to be devoted to finding a good balance, and yet live up to scientific norms and ethics. (Benestad, Rasmus, The Norwegian Meteorological Institute)	Checked the 3 "first hits" - all were published prior to AR4 and all have been cited very few times. We have included a very large number of references. We have now included some papers by reviewer, and more from Stephenson (where appropriate and necessary). But this is an assessment, not an annotated bibliography.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1799	3	80	1	113	6	Please include references which were shown in the upper comments if necessary. (Kurihara, Kazuo, Meteorological Reserach Institute)	Don't understand comment.
1800	3	81	18	0	18	Suggestion to add: Barbero, S., Bovo, S., Campus, S., Forlati, F., Limitation and constraint of forecasting, Evaluation and prevention of natural risks, Taylor and Francis Group NL/Balkema, 394-398, 2007 (BOVO, STEFANO, ARPA Piemonte)	Not clear why this should be added.
1801	3	81	55	81	56	I think it is unfair to use "et al." to refer to additional authors in references especially if some of them contributed extensively to the publication. Why not write out all author names. (Stephenson, David, University of Exeter)	Will use standard IPCC citation details.
1802	3	87	1	113	6	The papers with status submitted should be carefully checked before the publication of this report. Specially if conclusions in papers are still in agreement with those in the report: there are Emanuel, K (p.87, l.40-41), Fischer et al. (2 papers (88/28 and 31)), Jakob D (93/54), Kanada et al (94/24), Pall et al, (102/7), Zwiers et al (113/5). (Wibig, Joanna, University of Lodz)	Agreed.
1803	3	87	3	87	8	Two papers of Donat et al. have status submitted, the first one was actually published but in Climate Research not in International Journal of Climatology. It is necessary to check before the publishing of the report if in final versions of these papers the cited statements still exist in unchanged form. (Wibig, Joanna, University of Lodz)	Agreed - all references have been re-checked before final draft.
1804	3	94	24	0	0	I think that "Kanada, S., M. Nakano, and T. Kato, 2009: Mean atmospheric structures around Japan during July in global warming experiments using a semi-cloud resolving model. Hydrological Research Letters, submitted" should be replaced by "Kanada, S., M. Nakano, and T. Kato, 2010: Changes in mean atmospheric structures around Japan during July due to global warming in regional climate experiments using a cloud-system resolving model. Hydrological Research Letters, 4, 11-14." (Nakakita, Eiichi, Kyoto University)	Agreed - updated reference.
1805	3	106	30	0	0	"Part I" -> "Part II" (Van den Hurk, Bart, KNMI)	Agreed. Change.
1806	3	110	17	110	18	If Comment No 1 is taken, add the following between lines 17 and 18 on page 110: "Wan, H., X. L. Wang, and V. R. Swail, 2007: A Quality Assurance System for Canadian Hourly Pressure Data. J. App. Meteor. Climatol., 46, 1804-1817. DOI: 10.1175/2007JAMC1484.1". DOI:10.1175/2009JCLI3200.1". (Wang, Xiaolan, Environmen Canada)	Don't understand comment.
1807	3	110	17	110	18	If Comments No 3-5 are taken, add the following between lines 17 and 18 on page 110: "Wan, H., X. L. Wang, and V. R. Swail, 2010: Homogenization and Trend Analysis of Canadian Near-Surface Wind Speeds. J. Clim., 23, 1209-1225. DOI:10.1175/2009JCLI3200.1". (Wang, Xiaolan, Environmen Canada)	Don't understand comment.
1808	3	110	32	110	34	The "Wang, X.L., V.R. Swail and W. Perrie (2006b)" should be replaced by "Wang, X. L. and V.R. Swail (2006)" on line 32; and "(edited by W. Perrie)" should be added right after "Atmosphere-Ocean Interactions" on line 33. (Wang, Xiaolan, Environmen Canada)	Agreed. Change.
1809	3	110	41	110	41	The "2006c" here should be "2006b" as a result of the change suggested in Comment No 14. (Wang, Xiaolan, Environmen Canada)	Don't know what comment was "No 14".
1810	3	114	0	0	0	Chapter header including authors is given a second time (Rock, Joachim, Johann Heinrich von Thuenen-Institute)	Because figs and tables in separate file at this stage.
1811	3	115	1	116	2	Reference period should be 1961-1990 instead of 1960-1990. (Wibig, Joanna, University of Lodz)	Agreed.
1812	3	115	1	116	2	Table 3.1: The scale (likely, more likely, very likely, etc.) should be clearly defined and described. (Ammann, Walter J., Global Risk Forum GRF Davos)	These terms are clearly defined in IPCC guidance on uncertainty.
1813	3	115	1	0	0	Please note that many of the likelihood statements given here do not appear in the corresponding sections of 3.3 - 3.5, or the executive summary. For example, observed precip, monsoon, and El Nino statements. (Stocker, Thomas, IPCC WGI TSU)	We have checked for consistency between Tables, ES, sections 3.3-3.5.
1814	3	115	4	0	0	How can projections of stronger winds at mid- and high latitudes be likely, if observed trends lack basic evidence? This is not very credible. (Van den Hurk, Bart, KNMI)	Projections are for end of 21st century - by this time the signal might be much larger than at the end of the 20th century. So the confidence in projected trends does not depend on the confidence in observed trends up to the present date.
1815	3	115	0	116	0	Table 3.1: Tropical cyclones. Does it make sense to have a "Likely decrease or no change" for projections? (IPCC WGII TSU)	Yes. Reflects current state of science.
1816	3	115	0	116	0	Table 3.1: Structure good (IPCC WGII TSU)	Thanks.
1817	3	115	0	116	0	Table 3.1: Should landslides and lake outbursts be in ch 3 or ch 4? Ch4 seems more appropriate to me. (IPCC WGII TSU)	Clearly should be in Chapter 3 - physical environment.
1818	3	115	0	0	0	Table 3.1: Can subcategories of of precipitation such as snow storms and hail be included? These can cause large damage but will have different characteristics from the general precipitation events. They are discussed in detail in the text further on. why are they not mentioned here? (van Oldenborgh, Geert Jan, KNMI)	Hail is included in text but not in table because little can be said about them. Need to consider extreme snow falls - but again little can be said about these, so cannot put into table.
1819	3	115	0	0	0	Table 3.1: "More likely than not increase in high-latitude cyclone number and intensity." Matthias Zahn, Hans von Storch, Nature 467, 309-312, claim that polar lows at least will decrease. (van Oldenborgh, Geert Jan, KNMI)	Chapter does not address polar lows much - main focus is on mid-latitude lows because more important for disasters.
1820	3	115	0	0	0	Table 3.1: "Likely reduction of mid-latitude storms." How does this compare with the likely increase in extreme wind? (van Oldenborgh, Geert Jan, KNMI)	Have now worked to reconcile winds and storms sections.
1821	3	115	0	0	0	Table 3.1: The level of detail is very uneven in this table, from very general precipitation statements to unstable glacier lakes. (van Oldenborgh, Geert Jan, KNMI)	Agreed. But not sure of relevance. Information about various extremes is not even.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1822	3	116	0	116	0	The table entry on floods is seriously misleading. The earlier spring snowmelt is not relevant unless it can be tied to increased flood magnitudes and the authors have not done that. It simply has no place on this table. The observation about earlier snowmelt is true, but it has no demonstrated connection to increased flood magnitude or frequency. (Hirsch, Robert, United States Geological Survey)	Have considered reviewer's comments in floods section and will ensure table reflects position in literature.
1823	3	117	1	117	3	It is not clear whether the "overall increase in unusually warm days, decrease in unusually cold days" refers to the absolute values (i.e., warm days in summer, cold days in winter) or to the relative values (i.e., warm days include relatively warm days in winter, cold days include relatively cold days in summer). Please clarify this in the caption or in the table. (van Oldenborgh, Geert Jan. KNMI)	Generally refers to days above the 90th percentile or below the 10th percentile based on the Frich indices.
1824	3	117	1	117	3	HPC and HWD are not defined, the definition of EFI is not exact and only given at the bottom of the table. Please give above the table exact definitions of all quantities used. If these coincide with the definitions of Tables 3.1 and 3.2 please give them above these, if not please emphasize the differences. (van Oldenborgh, Geert Jan. KNMI)	Adding definitions of acronyms used in tables.
1825	3	117	1	0	0	Some suggested minor edits to tables 3.2 and 3.3: 'Sub-region' title is not needed, 'observations' and 'projections' titles are not needed as it is clear for the table captions what each table is providing. Suggest using alternating shades of grey for different regions to clearly separate the regions. Caption should say what the empty cells indicate - insufficient literature? . Multi-page tables should also repeat the header on each page. (Stocker, Thomas, IPCC WGI TSU)	Suggestion taken and changes made.
1826	3	117	1	0	0	I like the attempt to provide guidance on the regional results and place them into context. But some questions remain: How is the likelihood statement derived for particular subregions? On the basis of what evidence has the decision been made to leave likelihood statements out? Why are some references to the original papers, whereas others are to review papers and/or assessment reports? (Klein Tank, Albert. KNMI)	Likelihood statements are made based on guidance from IPCC TSU etc. This is amount of literature, quality of literature and expert judgement. All peer-reviewed literature is used, including original papers, reviews and assessments.
1827	3	117	0	121	0	Table 3.2: "Warm spells" is not as good a heading as "Heat waves" (IPCC WGII TSU)	Not all warm spells, as defined here, are heat waves. HWs are generally in summer, warm spells can occur any time.
1828	3	117	0	121	0	Table 3.2: Are the boundaries of the regional domains sufficiently clear? (IPCC WGII TSU)	Yes, we think so.
1829	3	117	0	121	0	Table 3.2: Emphasis on what is known from where is very useful (IPCC WGII TSU)	Thanks for the comment.
1830	3	117	0	0	0	Tables 3.2 & 3.3: It needs to be clarified in the table caption whether or not the cited references are simply illustrative examples, or whether these represent the full literature in each case. (Stocker, Thomas, IPCC WGI TSU)	We have clarified that in most cases this represents all known literature.
1831	3	118	0	119	0	Table 3.2 on regional observed changes. Additional information for Africa can be found in the references listed below by hazard type (Conway, Declan, University of East Anglia)	Thanks, and thanks for your input to the revision.
1832	3	120	0	121	0	Please refer to excellent work of Xhubin Zhang, et al, 2006 that relates to middle east (a spread area in Asia) . I know that Xhubin Zang is one of lead author, and I wonder not to refer to his work. (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	This reference is contained in the AR4 (Trenberth et al.) that represents our starting point. The Trenberth reference has been included.
1833	3	121	1	0	0	Australia/New Zealand - Salinger and Griffiths 2001 paper should be used here (Trends in New Zealand daily temperature and rainfall extremes, Int Journ of Clim, 21). (Stocker, Thomas, IPCC WGI TSU)	Article not included. This was never referenced in TAR or AR4
1834	3	121	0	0	0	Can the Australia/NZ section be broadened to the SW Pacific, taking in the APN results? (Trewin, Blair, Australian Bureau of Meteorology)	We have included island nations in a box instead.
1835	3	122	1	124	2	All Europe in column warm spells lack of factor of increase; in column dry spell unclear note "21" (Wibig, Joanna, University of Lodz)	Comment not clear, not sure what "21" references. Address after finalisation of Table 3.3.
1836	3	122	0	126	0	Table 3.3: How can there be statements with several references (especially Asia) and no likelihood statements? (IPCC WGII TSU)	Address after finalisation of Table 3.3
1837	3	124	0	124	0	Table 3.3 - I am not aware of many papers on this issue that deal in detail with extremes (most only consider changes in seasonal/annual). I think the Shongwe et al papers already cited would be most relevant for this table, and also Figure 3.4 (Conway, Declan, University of East Anglia)	Address after finalisation of Table 3.3
1838	3	125	0	125	0	As I described before, you don't use the grey literature. I know, that projection of some indices has been done in Iran. This work has done by cooperation of Atmospheric Science and Meteorological research center (ASMERC, Tehran -Iran) and center of climatology in Mashhad-Iran. I know, because of I was one of colleagues in this project. This report should present a national report by focal points of UNFCCC in Iran. If you would like to add this information, I am able to connect you with the manager of this project. (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	Address after finalisation of Table 3.3

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1839	3	125	0	0	0	Table 3.3 'East Asia' 'Heavy precipitation' HP frequency (1-72hourly) increases in rainy season over Japan in the warm climate.(Yoshizaki et al., 2005; Nakamura et al., 2008; Wakazuki et al., 2008) Yoshizaki,M., C. Muroi, S. Kanada, Y. Wakazuki, K. Yasunaga, A. Hashimoto, T. Kato, K. Kurihara, A. Noda and S. Kusunoki, 2005: Changes of Baiu (Mei-yu) frontal activity in the global warming climate simulated by a Non-hydrostatic Regional Model. SOLA, Vol. 1, .25-28. Nakamura, M., S. Kanada, Y. Wakazuki, C. Muroi, A. Hashimoto, T. Kato, A. Noda, M. Yoshizaki and K. Yasunaga, 2008: Effects of Global Warming on Heavy Rainfall during the Baiu Season Projected by a Cloud-system-resolving Model. Journal of Disaster Research, vol.3, No.1, 15-24. Wakazuki,Y., M.Nakamura, S.Kanada, and C.Muroi, 2008: Climatological reproducibility evaluation and future climate projection of extreme precipitation events in the Baiu Season using a High-Resolution Non-Hydrostatic RCM in comparison with an AGCM. Journal of the Meteorological Society of Japan, Vol. 86 (2008), No. 6, 951-967. (Kurihara, Kazuo, Meteorological Reserach Institute)	Address after finalisation of Table 3.3
1840	3	127	1	0	0	Suggested edits to Figures 3.1 - 3.4 (Overall issues of consistency and readability): It would be more effective to show a world map, with regional information displayed around the map, eg, as in Figure 3.6. Regions are currently not consistent between projection and observation maps. A more effective world map projection should be used - currently Greenland and the Arctic is exaggerated and takes up a lot of unnecessary space. The full range of pictograms should be shown for each region, with some indication used to show where there is no information available (ie, a cross through the pictogram). Some of the large change/medium change arrows appear inconsistent in size, giving the false impression that some changes are bigger/smaller than others. The magnitude of change could be shown, but will require a modification to the arrow design. Consistent colouring should be used for the extreme symbols, ie, Unusually warm days and nights should use the same yellow on red background, and unusually cold days and nights should use the same white/light blue on blue background. Likewise, dry spells should use the same colouring as heavy precipitation. This will make it much easier to visually see which symbols relate to heat, cooling, and precipitation. Figure captions need to be lengthened with a more detailed explanation of the figures (and their basis) given. Should not the tile be "Trends in changes in temp and precip extremes"? (Stocker, Thomas, IPCC WGI TSU)	Maps are being changed substantially for SOD.
1841	3	127	5	0	0	Note that changes refer to the period since about 1950? Some individual symbols seem debateable, including the supposedly inconsistent precipitation change over Europe. What is the source for this result? (Klein Tank, Albert, KNMI)	Maps are being changed substantially for SOD.
1842	3	127	0	127	0	Figure 3.1 – 3.4: Large change versus medium change not clear. (IPCC WGII TSU)	Maps are being changed substantially for SOD.
1843	3	127	0	128	0	The density of information of Figs 3.1 & 3.2 is very large, making them hard to read. The difference between the two darkest purple colours is virtually indistinguishable. (van Oldenborgh, Geert Jan, KNMI)	Maps are being changed substantially for SOD.
1844	3	127	0	130	0	please use an abbreviation list for the used signs(cgi, ala,...) (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	Maps are being changed substantially for SOD.
1845	3	127	0	130	0	Figures 3.1 to 3.4: I like what these figures are trying to achieve, but I wonder if it can be drafted to work better. The arrow direction is hard to see at a glance, and the icons can be very small. Could the icons always be the same size. A more detailed caption is also required. (Whetton, Pennv. CSIRO Marine and Atmospheric Research)	Maps are being changed substantially for SOD.
1846	3	127	0	130	0	Figures 3.1 to 3.5: I would prefer it if the figures could be sorted by region, so that the first figure is figure 3.1, the second figure is figure 3.3 etc. (Koppe, Christina, Deutscher Wetterdienst)	Maps are being changed substantially for SOD.
1847	3	128	0	0	0	I suppose , you are going to present figure 3.1 with 3.2 in a page. If it is, ok, but if no, please use the legend for figure 3.2. (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	Maps are being changed substantially for SOD.
1848	3	130	0	130	0	I suppose , you are going to present figure 3.3 with 3.4 in a page. If it is, ok, but if no, please use the legend for figure 3.4. (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	Maps are being changed substantially for SOD.
1849	3	131	1	131	8	In climatology, it is very much to argue on comparing periods of different lengths. Since no indication regarding the choosing of the 3 intervals from Figure 3.5 is provided, one can suspect that the were based on arbitrarily criteria in order to illustrate a certain, desired, prejudged result. I would either explain the choosing of the intervals or skip the figure. (Cheval, Sorin, National Meteorological Administration)	These intervals are based on the phases of the global temperature curve, warming from 1901-1950, cooling or little change, 1951-1978, strong warming 1979-2003.
1850	3	131	1	0	0	Figure 3.5: Please label x and y axis, and add legend for the black, blue, yellow colours. (Stocker, Thomas, IPCC WGI TSU)	Thanks, but this information is contained in the caption. Will label x and y
1851	3	131	0	131	0	please specify the axis in the figure, and also using different style for lines for better determination of them in white and black print. (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	see response to comment 1850.
1852	3	131	0	131	0	Figure 3.5: A smoothed PDF for each time period could be shown in the figure for a better presentation. (MUJUMDAR, PRADEEP, INDIAN INSTITUTE OF SCIENCE)	This is the raw curve from the data.
1853	3	132	1	0	0	Figure 3.6: Suggest to use thicker lines for the plots - they hold the key information but are difficult to see. The red dotted arrows linking the plots to the world map are much to thick and dominant - please re-colour and resize. Caption needs to include explanation for green and blue colours on the figure, and also ALL and ANT need to be explained. (Stocker, Thomas, IPCC WGI TSU)	Figure and caption revised
1854	3	132	0	132	0	The quality of figure for colour print and white and black print(small figures in the figure and their line and font) is not appropriate for IPCC reports. (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	Figure and caption revised
1855	3	132	0	132	0	Figure 3.6: Legend not clear. ALL and ANT not defined (IPCC WGII TSU)	Figure and caption revised

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1856	3	132	0	132	0	Figure 3.6: Would be MUCH more effective with the boxes as pop ups rather than embedded micro-boxes (IPCC WGII TSU)	Figure and caption revised
1857	3	133	1	0	0	Figure 3.7: Can this figure be updated since AR4 with new higher resolution model results? This would significantly improve the value of this figure, and otherwise it is questionable to take up space with old AR4 figures. AR4 and other relevant authors should be contacted and may be able to help update these figures, eg, Meehl, Arblaster, etc. (Stocker, Thomas, IPCC WGI TSU)	Figure will be replaced by figure using more AR4 models. Cannot use post-AR4 models.
1858	3	133	0	133	0	Figure 3.7: Color scale confusing with bluish purple at both ends (IPCC WGII TSU)	Figure replaced
1859	3	133	0	0	0	Note that in some tropical regions, the climatological value of this heatwave index is zero (see also comment 70) (Trewin, Blair, Australian Bureau of Meteorology)	Agreed. Figure replaced by a better, updated figure
1860	3	134	1	0	0	Figure 3.8: Can this figure be updated since AR4 with new higher resolution model results? This would significantly improve the value of this figure, and otherwise it is questionable to take up space with old AR4 figures. AR4 and other relevant authors should be contacted and may be able to help update these figures, eg, Meehl, Arblaster, etc. Caption reads "Changes in extremes..." - only one extreme is shown, ie, precipitation so please be explicit as in Figure 3.7. (Stocker, Thomas, IPCC WGI TSU)	Figure replaced with better graphics and using more of the AR4 models.
1861	3	134	0	0	0	Figure 3.8: A separation into summer and winter precipitation would be helpful to get an estimate about possible changes of the precipitation regimes. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Figure replaced -more focus now on seasonal differences.
1862	3	135	1	0	0	Figure 3.9: This figure is difficult to interpret and is not very user-friendly. We suggest it could be replaced with a new figure based on the same style as Figure 3.6., which gives a better regional breakdown. (Stocker, Thomas, IPCC WGI TSU)	Figure redrafted to make more reader friendly.
1863	3	135	0	135	0	Figure 3.9: Great figure! (IPCC WGII TSU)	Thanks - but see #1863
1864	3	136	1	0	0	Figure 3.10. Units are missing from the legend. The figure might be visually improved if models are recalculated with spatial downgrading. Text above the plots is hardly readable - suggest replacing with a short header, with all other information given in caption. (Stocker, Thomas, IPCC WGI TSU)	these figures redrafted
1865	3	136	48	0	0	Are these model values daily mean wind speeds? (Klein Tank, Albert, KNMI)	yes, 'mean' has been added to the caption
1866	3	136	0	0	0	Figure 3.10: this figure is presented just after a whole discussion that most observed trends in mid- and high latitude wind extremes are negative, and that GCMs cannot be expected to reproduce the small-scale meteorological phenomena leading to wind extremes. Why is then GCM result presented at all? If it is deemed necessary to give some idea, please repeat in short form the arguments made above on the caption. As an extra point against showing this figure, Zahn & von Storch (Nature, 2010, doi:10.1038/nature09388 just showed that polar lows are likely to decrease. These are not resolved by GCMs, invalidating the increase in wind extremes shown over the Arctic in these figures. (van Oldenborgh, Geert Jan, KNMI)	further discussion of the use of these figures has been given.
1867	3	137	1	0	0	Figure 3.11 - This figure is not quantitative and hardly essential for chapter 3. It may be more appropriate for chapter 4. (Stocker, Thomas, IPCC WGI TSU)	We now include a much simplified version of this figure in the drought box; the revised version highlight the definitional issues of drought
1868	3	137	1	0	0	Figure 3.12 - This figure is not quantitative and hardly essential for chapter 3. It may be more appropriate for inclusion in chapter 4. (Stocker, Thomas, IPCC WGI TSU)	this figure has been removed
1869	3	137	4	137	5	I would replace "positive" and "negative" impact by "reinforcing" and "damping", to avoid a subjective connotation (Van den Hurk, Bart, KNMI)	Agree - changed caption to drought schematic (now in drought box)
1870	3	137	0	0	0	It is nice to see a figure on physical mechanisms. However, a certain imbalance occurs because such a figure is only provided for one of the many extreme events (droughts). What is the justification for discussing the physics of droughts in more details than of other event categories? (Wernli, Heini, ETH Zürich)	Justification is because feedbacks are known to be complex for droughts. Also provides an example of feedbacks.
1871	3	138	1	138	6	Figure 3.12 suggests the climate change can have only negative impacts in coastal zone. Is this the intention of the authors? It is not clear for me why the hatches and the arrows are different, what is the significance of this difference? (Cheval, Sorin, National Meteorological Administration)	this figure has been removed
1872	3	138	1	0	0	Not clear what the different line thicknesses relate to in Figure 3.12 (less or more important relationships?). Also font sizes are inconsistent. (Stocker, Thomas, IPCC WGI TSU)	this figure has been removed
1873	3	138	3	0	0	This is not a very informative figure, unlike fig 3.11 where feedback signs are indicated (Van den Hurk, Bart, KNMI)	this figure has been removed
1874	3	138	0	138	0	Figure 3.12: This figure represents a very interesting idea. It needs more work to make it worth the space. At this point, all of the links are pretty obvious. It is worth thinking about figures like this for other processes. (IPCC WGII TSU)	this figure has been removed
1875	3	138	0	0	0	See comment 11 above: It is not appropriate to label anticyclones, fronts, and cyclones in a general sense as extreme weather events. In this diagram I suggest to label them as weather phenomena and use the extreme event terminology for winds, precipitation etc. (Wernli, Heini, ETH Zürich)	this figure has been removed
1876	3	139	1	0	0	Box 3.1 figs 1 and 2: There does not appear to be any need to show both these figures. The same information from fig 1 is duplicated and expanded upon in fig 2, so it would be sensible to only include fig 2. It is not clear why the top horizontal axis is shifted towards the left, and it should be possible to extend the lower horizontal axis to the right so that the two parts of the figure are aligned. Please also label the vertical axis, and top horizontal axis (climate variance). (Stocker, Thomas, IPCC WGI TSU)	Box donated to Chapter 1
1877	3	139	0	140	0	"%ile" in Figs. 1 and 2 should be replaced either by "%" or by "percentile" (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Box donated to Chapter 1

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1878	3	139	0	140	0	Box 3.1 figure 1 & 2: This material should be in chapter 1 or 2. (IPCC WGII TSU)	Agreed.
1879	3	139	0	0	0	Box 3.1, Figure 1 & 2. I found this figure quite difficult to interpret at first. Suggest changing A and B to I1 and I2; IA2 and IA1 to IC2 and IC1. The captions need more detail. (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Box donated to Chapter 1
1880	3	141	1	0	0	Box 3.3 figs 1 and 2: Are these figures dealing with D&A of mean changes necessary for SREX? Figures from recent BAMS 2009 Hawkins and Sutton paper might be more relevant and useful here. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Delete figures.
1881	3	141	43	141	43	Della-Marta et al. 2007b should read Della-Marta et al. 2007a (Della-Marta, Paul, Partner Reinsurance Company)	Comment does not refer to this page.
1882	3	141	0	141	0	Box 3.3 Figure 1: Why are we seeing a plot of mean temperatures? (IPCC WGII TSU)	Agreed. Delete figures.
1883	3	141	0	0	0	This figure is not about extremes, I suggest to omit it. (Wernli, Heini, ETH Zürich)	Agreed.
1884	3	143	1	0	0	The far right, November 2009 bar could be coloured a bright colour, and/or indicated with an arrow. Can this figure be extended to include more examples from other regions, ie, showing a series of distributions. (Stocker, Thomas, IPCC WGI TSU)	No. Don't need to do this, since bar is very obvious.
1885	3	143	0	143	0	The quality of figure should be improved (Rahimzadeh, Fatemeh, Atmospheric Science and Meteorological Research Center (ASMERC))	See response to comments 1884 and 1886.
1886	3	143	0	0	0	FAQ 3.2, Figure 1: It would be helpful to indicate both the long-term mean and the standard deviation in the Figure (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	NO. Doesn't help interpretation of figure.
1887	3	147	32	147	32	An advantage of GEV... There now exist methods to model the parameters of the GPD as functions of covariates, so this advantage is not true anymore, see Yee et al Extremes (2007) Vector generalized linear and additive extreme value models 10:1-19 DOI 10.1007/s10687-007-0032-4. A possible advantage of GEV methods is that the tricky topic of threshold selection in the GPD is avoided. (Della-Marta, Paul, Partner Reinsurance Company)	Comment refers to different page (page 14). Paragraph is being revised.
1888	3	160	12	160	12	Another source of uncertainty in assessing changes in either observed or modelled climate extremes is the statistical uncertainty associated with the application of analysis methods. For example Della-Marta and Pinto (2009) showed that by using appropriate confidence interval calculation techniques that the future changes in extreme value distribution for all return periods of storm vorticity are statistically significant. They indicate that the future changes are perhaps unrealistically large and as such can offer a starting point to investigate further the cause behind the changes. They advocate the appropriate use of statistical uncertainty calculation techniques alongside methods to explore the initial condition, scenario and model uncertainties: P. M. Della-Marta, J. .G, Pinto. Statistical uncertainty of changes in winter storms over the North Atlantic and Europe in an ensemble of transient climate simulations Geophysical Research Letters, 36:L14703, doi:10.1029/2009GL038557, 2009. (Della-Marta, Paul, Partner Reinsurance Company)	Comment refers to different page - cannot identify correct page.
1889	3	161	63	161	63	Related to the comment above (3), maybe it is worth expanding in a paragraph a few studies which have considered the statistical uncertainty associated with future changes in extremes. e.g. Della-Marta and Pinto (2009), Brown, Ceaser and Ferro (2007). Frei et al (2006) (Della-Marta, Paul, Partner Reinsurance Company)	Comment refers to different page - cannot identify correct page.
1890	3	168	46	168	46	On line 2 of page 165 references are made to paper describing the mechanisms of temperature change. Maybe it is appropriate to make a reference back to the observed changes in this region to show consistency between projected regional changes and those that have been observed. In this case add a reference to Della-Marta et al (2007a) and add "consistent with observed changes over the last century (Della-Marta et al. (2007a))" after the reference to Fisher and Schaefer 2009. (Della-Marta, Paul, Partner Reinsurance Company)	Comment refers to different page - cannot identify correct page.
1891	3	176	2	176	2	Maybe point out that Wang 2009b show that the 1990s displayed unprecedented storminess in the north sea region and not the Atlantic region as a whole. (Della-Marta, Paul, Partner Reinsurance Company)	Comment refers to different page - cannot identify correct page.
1892	3	194	13	194	15	Maybe add a reference to Della-Marta and Pinto (2009) see full reference in comment 2. The main inovation in this study is that extreme value analysis techniques are applied to future scenarios of the ECHAM5 model. Unlike previous studies which measure the simple changes in cyclone frequency above a fixed threshold (Donat et al 2009,2010, Pinto et al 2009), Della-marta and Pinto (2009) Return Periods (RPs) of North Atlantic storms' minimum central pressure (CP) and maximum vorticity (VOR) remain unchanged by 2100 for both the A1B and A2 scenarios compared to the present climate. Whereas shortened RPs for VOR of all intensities are detected for the area between British Isles/North-Sea/western Europe as early as 2040. However, they caution that, the changes in storm VOR RP may be unrealistically large: a present day 50 (20) year event becomes approximately a 9 (5.5) year event in both A1B and A2 scenarios by 2100. (Della-Marta, Paul, Partner Reinsurance Company)	Comment refers to different page - cannot identify correct page.
1893	3	251	0	251	0	Table 3.2: Please add some references in northern and southern Europe heat wave changes: Beniston, M., 2004, Della-Marta 2007a and b, Klein tank and Koennen 2004 (Della-Marta, Paul, Partner Reinsurance Company)	More references have been added
1894	3	0	35	0	0	The following result may serve as reference: "by the end of this century, the Fertile Crescent of Middle East will lose its current shape and may disappear altogether."(Akio Kitoh, Akiyo Yatagai and Pinhas Alpert: First super-high-resolution model projection that the ancient "Fertile Crescent" will disappear in this century, Hydrological Research Letters, Vol. 2, pp.1-4, (2008)) (Nakaegawa, Toshiyuki, Meteorological Research Institute)	No page/line identified for comment.
1895	3	0	40	0	0	"of secondary importance" should be "of third importance" (Nakaegawa, Toshiyuki, Meteorological Research Institute)	No page/line identified for comment.