#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1	3	0	0	0	0	Many citations in this chapter are erroneous (see my specific comments below). In particular, my papers are cited in many places that are completely out of the study topic, e.g., describing our cyclone activity analysis using Canadian SLP data with citation to our analysis of NH ocean wave height (see my comments #2 and #3 below). I pointed out all these problems in my comments on the first order draft, but little correction is successful so far. (Wang, Xiaolan, Environmen Canada)	Fixed references as raised by reviewer in specific comments.
2	3	0	0	0	0	In general I think this is an excellent chapter. The tables and figures are great, and the discussion of mechanisms and processes are very clear and helpful to the reader. The authors should be congratulated on writing such a good chapter. I have a few general suggestions and some smaller ones. (Lobell, David, Stanford University)	Thanks.
3	3	0	0	0	0	The chapter appears to rely on a few papers that are only submitted and not yet reviewed. This is not unexpected, and not a big deal, except I notice a couple are in climatic change (a painfully slow journal) and figures 3.4 and 3.5 are taken from one such study. this is just to flag this issue although my guess is the CLAs and reviewer editors are already aware of this. (Lobell, David, Stanford University)	Noted. The submitted papers have now been accepted for publication.
4	3	0	0	0	0	I think the authors do a great job of laying out evidence for historical trends, and then discussing projections of future trends, but little is done in terms of saying whether they both paint a consistent picture. This is not quite the same thing as formal D&A, but rather to give a sense of confidence in the projections. If the authors have reasons for not doing this, they should state so. But one possibility would be to add a column or two to table 3.1 to indicate explicitly what shows up in both observations and projections (Lobell, David, Stanford University)	Noted. However, this will now be covered because attribution statements have now been included in the ES. And attribution is already included in Table 3.1.
5	3	0	0	0	0	The authors have not provided an (Lobell, David, Stanford University)	
6	3	0	0	0	0	In FAQ3.2 it would be nice to see a table with recent events that have been in the news, and indications of whether there are scientific studies specific to that event (such as European heat wave), whether the event is likely to become more frequent given the conclusions of this report, and whether there has been an observed trend in related variables. I think this is squarely within the job of synthesis (not new research) and would make a great addition to the report. (Lobell, David, Stanford University)	Reject. The FAQ is intended to answer the question in general - it would be diluted by including too much detail about recent specific events. Some recent specific events are discussed in various places through the Chapter.
7	3	0	0	0	0	This chapter has included one sentence repetitively "earlier shift of spring peak flows in snowmelt and glacier fed rivers (Kundzewicz et al., 2007; Bates et al., 2008). I would like to note that this statement does not reflect health of glacier in anyway shape or form. We have noticed that several glaciers have shown significant retreat without any shift in melting pattern. This needs to be clarified otherwise could cause confusion. (Haritashya, Umesh, University of Dayton)	The statement includes snowmelt, for which there is widespread evidence of earlier melt - and this should be causing changes in spring peak flows.
8	3	0	0	0	0	Deep convection and associated extremes such as convective gusts, tornadoes, or hail cause high annual damage. Although this topic is of high relevance, it is only marginally discussed in SREX, Chap. 3. I suggest including an additional section. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	All these topics are discussed - but there is limited literature, so authors did not consider it worthwhile having a separate section. This would also have led to duplication, because these topics are covered in current sections. However, added a few sentences through Chapter to discuss these small-scale extremes..
9	3	0	0	0	0	In some places, the term "risk" is used for "probability" (e.g., p53, L25). According to the definition in Section 1.1.3.1., "risk" is defined as 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))	Agreed - where possible ensure we avoid use of "risk", except when it is meant to convolve hazard and vulnerability.
10	3	0	0	0	0	This chapter is well organized. Specially, section 3.1.1 which have a good presentation of categories (basic definitions). This information can be used for a better Box 3.1.1. in the SPM. The author have done a great job. The Sahel system has been very well introduced. (Mata, Luis Jose , IMF)	Will include new box with definitions in Section 3.1 which will be based on the material of SOD section 3.1.1.
11	3	0	0	0	0	Uses of the terms 'section' and 'sub-section' have inconsistency and their differneces are not clear: the term 'sub-section' is used for '3.2.1' in P11, L11, though 'section' is used for '3.2.1' in P15, L31. The term 'sub-section' is used for '3.2.3' in P15, L3, though 'section' is used for '3.2.3.1' in P16, L48/P18, L47. (JAPAN)	Noted. Standardise use of "section" and "sub-section".
12	3	0	0	0	0	Overall comment: This chapter has certainly improved since the first version. The uncertainties on anthropogenic impacts on extreme events are better displayed. These uncertainties should find notice also in all other chapters of the report, especially in the summaries. (Schmidt-Thome, Philipp, Geological Survey of Finland)	Thanks.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
13	3	0	0	0	0	Chapter 3 introduces such basic notions as climate extremes, trends and tendencies, variability, etc. The task of writing team of this chapter was very difficult, since it was necessary to introduce basic elements of mathematical statistics in a competent way, with understandable language, as well as to interpret them in terms of climatology. It should be mentioned in this chapter that categories “climate change” and its “impacts” on social and ecological systems cover both changes in probability distributions of key climate parameters (shifts in distribution curves with no changes in their shape) and changes in the shape of distribution curves. One of the manifestations of changes in the shape of a distribution curve is a change in variability, although changes in variability do not always definitely lead to changes in extremes. There can be a situation when a change in the shape of distribution curve leads to an increase in variability (an increase in variance), but at the same time extremes become less frequent (distribution wings become shorter). In Chapter 3, it would be desirable to accurately explain the relationship between variability and extremeness (not treating these terms as synonyms!) and to adequately apply this relationship in Chapter 4 and subsequent chapters. Therefore, in particular in Frequently Asked Questions (FAQ) of Chapter 2, while responding to the question “Does climate become more extreme?” the argument: “Greater variability leads to more extreme climate” should be revised, since in general it is not correct. In this chapter it would be also desirable to emphasize the importance of high quality data of observations and to formulate requirements to data sets and data time series. Meeting those requirements is necessary for the adequate assessment of processes within the climate system. (RUSSIAN FEDERATION)	Noted. Earlier drafts of this chapter did include more discussion on these topics, but much of these discussions was subsequently allocated to other chapters. Need for high quality data is highlighted already (3.2.1).There does not appear to be a FAQ in Chapter 2?
14	3	0	0	0	0	First of all, let me congratulate the authors on what is clearly a very impressive piece of work. That said, I do have (many) comments that I hope the authors will find useful. (Zwiers, Francis, Environment Canada)	Noted.
15	3	0	0	0	0	I am very concerned about this chapter's interpretation of the uncertainty language, and the apparently equivalence that it draws between a confidence assessment of "medium" and a likelihood assessment of "more likely than not". These are not equivalent assessments. Drawing such an equivalence is counter to the intent of the uncertainties guidance paper and it casts doubt on all AR4 assessments that used the term “more likely than not”, suggesting, incorrectly, that those assessments were intended to have a different interpretation than is appropriate to the term "more likely than not". If the author team has information and knowledge that they consider to be reliable and that allows an assessment that the probability of an increase, or decrease, is greater than 50%, then they should say “more likely than not” (which is my understanding of how this assessment was applied in the AR4). If the author team cannot assign a probability because there is not suffient information to quantify uncertainty, but has reasonable confidence in the evidence that leads to a conclusion of a change in direction (either observed or projected), then they should say “medium confidence”. If they feel more comfortable with "medium confidence" in some instances where previously, the AR4 assessed "more likely than not", they can of course, give a different assessment in SREX (giving reasons). For example, they may still have the same information about the direction of change, but may now have understood that the quantification of uncertainty is more difficult than previously judged to be the case. However, they should not, after the fact, implicitly change the AR4 assessment by confusing the interpretation of "medium confidence" with that of "more likely than not". (Zwiers, Francis, Environment Canada)	Agreed. After discussion within the author team, it was agreed that the term "more likely than not" can indeed be considered distinct from "medium confidence" (corresponding to cases with high confidence but low signal to noise ratio). These two terms are not equated anymore. The AR4 assessments cannot be directly compared to our assessments since we follow the new IPCC uncertainty guidelines
16	3	0	0	0	0	Often, the chapter text seems to be insufficiently critical of the papers that it describes (there remains a tendancy to report rather than to assess). Assessments sometimes fail to give reasons, making them difficult to interpret and possibily inviting misinterpretation. This is especially the case for assessments of low and medium confidence. Careful thought needs to be given to which regions are called out as examples, bearing in mind that these are just not examples, but they might well be used by policy makers in the regions that are named. Statistical significance, which is often cited in the chapter as a criterion of assessment, is perhaps given a bit too much weight. (Zwiers, Francis, Environment Canada)	All of the questions raised by the reviewer have been considered in the previous drafts, and will be considered again in the final revision. Space limits severely restrict the amount of detailed criticism we could include. We had to balance the need to assess studies with the stringent space limits placed on us.
17	3	0	0	0	0	Main comment : this chapter does not address the following issues : 1) Sea ice extent summer reduction; 2) Greenland ice sheet with a significant number of outlet glaciers thinning/acceleration and associated increase of iceberg production; 3) Closed Seas, such as Caspian Sea and Black Sea, climatic changes (i.e. water level, ice). These are well documented and could thus be addressed. (International Petroleum Industry Environmental Conservation Association (IPIECA))	Reject. The Chapter has focussed on extremes linked to disasters - we have not tried to assess all aspects of climate change.
18	3	0	0	0	0	The chapter is well organised, well presented and meets the general objectives of the report (GREECE)	Noted.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
19	3	0	0	0	0	The use of standard statistical methods on extremes and the conclusions based on these seems basically sound, and the new modern statistical techniques entering the arena are satisfying. 3.1.2). The report has managed to improve on the distinctions between at least two different interpretations of "uncertainty" compared to AR 4: Confidence a about conclusions, and the degree of statistical significance in individual studies. Whether it is at all possible to convey this distinction to a public debate is another question, also discussed in the report. This will be an important issue in the communication. There is a very sensible discussion of an "extreme" event as something that is outside what is "normal" under the circumstances, as different from what is disastrous in an absolute sense. Such a definition is necessary in order to make room for the varied demands for response to climate events and climate change. The report also lists a number of issues, in particular when it comes to scaling between global climate models, regional models, and local extreme effects, which need to be addressed by research. Here is an obvious cause for more intense cooperation between statistics and climate research. The present report strengthens the arguments for methodological research also on the statistical side. It is necessary that the statistical community supports the very difficult task for climate research to assess the extent and effect of climate change. We therefore strongly support the attempts in the report to use a standardized terminology for different types of uncertainty. (SWEDEN)	Noted.
20	3	0	0	0	0	The inferences on paleoclimate evidence (especially on temperature, precipitation and wind) seem to add quite little truly relevant information in the intended context. If information from past climate records is required, it could perhaps be collected in a specific sub-section, preceded by a short motivation of its relevance in the context. (SWEDEN)	Reject. There is little paleo-evidence about extremes. What we have found is used mainly to point out that extremes did vary prior to human influences on the climate. We agree that paleoinformation does not add a great deal to the report - that is one reason we have restricted the amount of paleoinformation discussed. We also think what little paleo information is included is appropriately placed, to provide some background to discussions of possible modern changes in extremes. Gathering all of this into a single separate section would mean this backgrounding could not be done, without duplication.
21	3	0	0	0	0	In various sections, use is made of quotations, which suggests more a review than assessment of literature. In general, the quotations duplicte parts of the text and would seem unnecessary. (SWEDEN)	Noted. We will examine any quotes, case-by-case, to see if their content could be better expressed.
22	3	0	0	0	0	Temperature extremes focus on absolute threshold based events, such as the low and high temperatures. Another important phenomenon, the surge/cold waves, which occur world-wide as defined as the drastic temperature drops within hours or in a couple of days, should not be totally ignored in the report. (CHINA)	Noted. But little useful literature for a report on climate change.
23	3	0	0	0	0	A balance between observation and projection of the extremes should be considered. In this draft version, the content of the projection occupies a large number of the pages/figures/tables. The content in this chapter focuses too much on model projections, and there is a need to enhance presentation of the observed facts that have higher confidence. (CHINA)	Reject. Projected changes of extremes are much more important for the intended audience of this special report, than are observed changes.
24	3	0	0	0	0	Chapter 3 of SREX is scientifically very sound. The text is extremely detailed. Some chapters are rather unbalanced in the sense that the statistical or the dynamical part dominates. Overall the chapter is not easy readable. Therefore, the thext of the policimakers summary (SPM) has to be short, clear and plausible. In my opinion the figure 3.6 and 3.8 can, in this form, not be used for the SPM. They must be simplified in the sense that averages are produced for much larger regions, even this is difficult or less precise. (Wanner, Heinz, University of Bern)	Figures 3.6 and 3.8 have been simplified for the SPM in order to better convey the overall information but the author team did disagree with the suggestion of providing information on larger regions which would be masking regional variations in the signals.
25	3	0	0	0	0	LOST FIGURE(S) FROM FOD: We were surprised to see that the material from Box 3.1 in the FOD has not really been used by Chapter 1 where it would have served nicely to help frame the report. In particular, FOD Box 3.1 Figures 1 and 2 are not featured. This is a shame, because this fundamental illustration linking changing climate/weather variables and impacts is crucial for this report. While our clear preference would be for Chapter 1 to add this Box to help frame the overall report, a possibility, if Chapter 1 can not be persuaded to include this material, might be to reinclude this figure(s) and a small amount of associated text into section 3.1.2. If this is considered, our previous comments from the FOD remain valid, most importantly, that these two figures could be combined into one: "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)	Noted. New figures will be included in Chapter 1 to overcome this issue. These introductory figures are more appropriate in Chapter 1 than in Chapter 3.
26	3	0	0	0	0	FIGURES: Orlowsky and Seneviratne figures: It should be made clearer in the captions that these figures are an extension and update of the already published results from Tebaldi et al. 2006 used in the AR4. Something like the following text (taken from pg. 28, lines 25-26) could be added to each figure caption: ".......[from Orlowsky and Seneviratne (2011), updating the AR4 assessed results of Tebaldi et al. (2006) to include a larger number of GCMs (23) from the CMIP3 ensemble]". (Stocker, Thomas, IPCC WGI TSU)	Will provide more detailed information; not all figures are updates of T06

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
27	3	0	0	0	0	SUBSECTIONS: We suggest to reintroduce the subsections within each extreme section, separating Observations, Attribution/Causes, and Projections. Some extreme sections are very long, and this will make it much easier for a reader to navigate to their particular interest, eg, observations vs. projections. Reintroducing this sub-sectioning is easily possible for all of the Extremes, most of the Phenomena and some of the Impacts. This additional level of subsections need not necessarily be numbered and certainly does not need to be added to the table of contents. (Stocker, Thomas, IPCC WGI TSU)	Agreed (partly). This would be very helpful for the reader in the long sections (3.3.1, 3.3.2, 3.4.4, 3.5.5) but are not needed in the shorter sections - and would look quite strange in the very short sections such as 3.4.3. Include them in the long sections - but not as numbered sub-sections.
28	3	0	0	0	0	CONSISTENT MAP PROJECTION FOR FIGURES: It would be ideal to see all the global map projection figures (ie, those based on Orlowsky and Seneviratne, McInnes et al., and Kharin et al.) using a common map projection. In order to allow direct comparison with previous global projections given in Chapter 10 of the AR4, we would recommend that the same 'Robinson' map projection is used. This is particularly important for the Orlowsky and Seneviratne figures, which provide an update and extension of the AR4 figures. If reprojection to a common map projection can be achieved without too much extra effort, we think this would be both scientifically and visually very advantageous for the Chapter. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Attempting to use similar projections in the figures.
29	3	0	0	0	0	Generally a very well-organized chapter, but the information is still difficult to grasp for our decision-makers. One reason is may be the representation: we would strongly value something like the graphics with regional information from the FOD -- also as an input to the SPM. (International Federation of Red Cross and Red Crescent Societies (IFRC))	Reject. Chapter has tried to balance the need for scientific rigour with an easily understandable message. Map from FOD was excluded because of space limits and concern about need to ensure consistency between all aspects of the Chapter on regional changes. We think the figures in the curent version, along with tables 3.2 and 3.3 provide more information in a manner that can be used by decision makers, than with the schematic maps referred to by the reviewer.
30	3	0	0	0	0	In addition, it would be good to include more explicit information about what can be provided at smaller scales. This is of course to some extent location- and variable- as well as seaon-dependent (some of that is discussed in box 3.1), but some more discussion would be crucial to allow the management chapters to include some reflection on the opportunities and limits of using this information for decision-making. In practice, based on what we've seen after the AR4, many people will simply be taking the global or regional information and applying it as such to their decision-making context (with sometimes dangerous consequences). Maybe chapter 3 can provide some insights in how far climate science can and cannot go in providing more specific information when decision-makers would request the best possible information in a specific context -- application, location, timescale -- from a specialized climate institute). Chapter 3 might also be able to work with the case study chapters to include more examples at smaller scale science in those specific contexts (also feeding those linkages into the SPM). (International Federation of Red Cross and Red Crescent Societies (IFRC))	This subject is discussed in Box 3.1 (will beome Box 3.2 in final version). Will also include a note about this at the end of the revised ES.
31	3	0	0	0	0	One aspect that we are missing in the report at large is climate information across timescales. There is relatively little discussion of how to interpret climate (change) trends (observations and projections) in light of planning for the coming few years, also in light of information about variability on other timescales, particulalry seasonal and decadal. In our applied programs, this is where we have found a lot of entry points for better us of climate information in disaster risk management (including longer-term change dimensions). One of the key questions is how the information on trends relates to what we know about variability -- and in some cases predictable aspects of that variability. This applies to chapter 3, as well as all of the management chapters (so we'll insert this comment for each of those chapters) (International Federation of Red Cross and Red Crescent Societies (IFRC))	This topic is partly addressed in a case study now transfered to Chapter 9.
32	3	0	0	0	0	Chapter thee: overall …. The chapter should include more scientific explanation of those hydro-climatic events, how they are related to climate change, and their future spatial distribution and magnitude, etc. (Islam, Md. Siarjul, North Sotuh University)	Space restrictions limit detail that can be included.
33	3	0	0	0	0	Regarding Chilean glaciers says “Sollipulli”. It must says “Collipulli”. (CHILE)	Fix this in second last paragraph of 3.5.6.
34	3	0	0	0	0	Editorial : Could you consider replacing all occurrences of "waiting time" by "return period" ? ALL OTHER CHAPTERS already use return period (only). We see no added value in using two names for the same thing. (BELGIUM)	Agree. Will be changed throughout the chapter.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
35	3	0	0	0	0	The context of the provided scenarios should be explained in a clear way. As in the SPM, it should be explained that SRES B1, A2, and A1B are "no climate policy" scenarios. It should also be explained that this range do not cover the full range of the SRES scenarios - at least the top of the A1 family is not included (A1FI), so that the top of the range included here is at least not "an extreme scenario". The consequences of this for the next chapters should be discussed. In particular, it should be taken into account that the current policy objective - 2°C in the Cancun Agreements - would lead to less climate change than B1. How could policymakers take this into account ? Even if very low or very high emissions appear to be unlikely, it is not desirable to suggest so in an implicit manner, by just ignoring the possibility of effective climate policy or, at the other end, complete failure and very high emissions. In addition, using a reduced scenario range without thinking about it may contribute to an important statement that is made in chapter 3 : that for precipitation extremes at least, model uncertainty is more important than scenario uncertainty. Would this mean that even for low scenarios, such as keeping global warming below 2°C, there is little hope for reducing the risk of certain impacts, such as floods ? (BELGIUM)	Reject. This would add considerably to the length of the SREX. These topics are all considered in AR4 and are appropriate for AR5. We have to assume some knowledge on the part of readers, if we are to keep this chapter to length limit.
36	3	0	0	0	0	Le travail réalisé est remarquable par la complétude de la réflexion, par la prise en compte de très nombreuses études publiées ou découvertes après la sortie du rapport du groupe II du GIEC en 2007. Il en résulte un chapitre long, détaillé, rigoureux et fouillé mais dans lequel on a tendance à oublier que l'on traite de l'évolution (fréquence et magnitude) des situations à catastrophe en rapport avec le changement climatique. Si les conclusions générales de l'AR 4 sont la plupart du temps confortées, cette approche par les extrêmes conduit à les relativiser par de multiples considérations d'exceptions et de cas spécifiques qui atténuent leur généralité, limitent assez souvent le degré de confiance (comme dans le déplacement vers le nord des trajectoires des tempêtes des latitudes moyennes) voire corrigent des assertions considérées comme très probables dans l'AR 4 (l'augmentation des sécheresses par exemple). La clé de cette approche pointilliste se trouve de manière claire dans les deux FAQ. Celles-ci mériteraient de figurer en introduction au chapitre plutôt qu'à la fin du texte. Ce qui permettrait aussi de raccourcir, la définition générale d'un climat extrême, peu opératoire pour la suite du texte. Aborder à la fois par les catastrophes (disasters) et par les extrêmes de paramètres physiques est source de confusion. Il suffirait de rappeler qu'une catastrophe peut aussi résulter de multiples causes (en conjonction ou successives) qui peuvent être séparément toutes non extrêmes et sa gravité est fortement fonction de son échelle spatiale (localisation et étendue) et temporelle (durée), de la densité de la population affectée, de sa culture, de son mode de vie, de son état de préparation, de sa gouvernance et de sa situation économique. (BOURRELIER, PAUL-HENRI, AFPCN)	
37	3	0	0	0	0	Suite - Les tableaux sont clairs mais les cartes souvent peu lisibles en raison du format. On gagnerait en lisibilité en présentant sur des feuilles différentes chacun des deux hémisphères ou encore en séparant les Amériques de l'ensemble Europe-Afrique-Asie. L'attribution de la cause (anthropique ou naturelle, effet de serre ou autre) est rarement concluante et, dans le contexte du SREX, de peu d'intérêt (OG2) dès lors que l'on a rappelé que l'exposition et la vulnérabilité sont des facteurs souvent prépondérants dans les catastrophes. On pourrait omettre de le faire systématiquement. L'angle d'approche est essentiellement celui des phénomènes physiques qui peuvent conduire à des catastrophes. Prendre en considération l'accord de plus de 2/3 des modèles (qui constituent un ensemble opportuniste dont l'indépendance n'est pas assurée et dont le nombre pris en compte varie avec les phénomènes et selon les études disponibles) pour juger du degré de confiance dans un résultat est arbitraire. Ces simplifications opératoires affaiblissent les rigoureuses précautions méthodologiques exposées par ailleurs. Plusieurs modèles globaux et régionaux de climat (approche multimodèle) sont utilisés et des résultats de descentes d'échelle pris en compte. Les modèles non hydrostatique, à aire limitée, de maille de 1 à 5 km sont évoqués surtout pour indiquer que leur coût en temps de calcul reste rédhibitoire même pour des simulations sur de courtes durées. A ce propos on peut déplorer que les progrès récents ainsi que les perspectives d'évolution dans la connaissance appliquée à la simulation de la climatologie des phénomènes violents d'échelle locale (orages, tornades, ...) ou régionale ne soient pas traités. (BOURRELIER, PAUL-HENRI, AFPCN)	
38	3	0	0	0	0	Many studies are cited that are based on reanalysis data sets. In many instances the data set is mentioned, but not always. However, this would be important. (Brönnimann, Stefan, University of Bern)	Will carefully revise to add details where appropriate.
39	3	0	0	0	0	Many of the figures are almost illegible (Brönnimann, Stefan, University of Bern)	Agree. Quality of figures has been improved and figures with some fonts have been increased.
40	3	0	0	0	0	Detection of changes in extreme events and attribution of the causes of change to climate change and/or anthropogenic activities should be separated (GERMANY)	In general this has been done, with observed changes being described prior to discussion of causes. See also response to comment 27.
41	3	0	0	0	0	Overall, evidence from Africa and Asia is under-represented in the chapter, and the chapter would benefit from adding additional evidence on impacts in those regions. (UNITED KINGDOM OF GREAT BRITAIN AND NORTHERN IRELAND)	Reject. This reflect the state of the current literature. The author team and several CAs have checked thoroughly for literature for these regions.
42	3	0	0	0	0	The chapter does not deal well with issues of uncertainty. Often, statements are made, but the uncertainty around the evidence linked to those statements only discussed several paragraphs later. (UNITED KINGDOM OF GREAT BRITAIN AND NORTHERN IRELAND)	Reject. The Chapter discusses uncertainty in more detail and with more consistency than has any previous IPCC report. But will address this if specific examples of this are identified.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
43	3	0	0	0	0	Overall - a useful collection of references amplifying messages from AR4 etc. A little repetitive in going through variables one fete another, but this may be unavoidable. Some discussion of EVT and validity under change would be useful up front, as would discussion of changes in extremes occurring (if measured above a threshold particularly) when the mean of a distribution changes. (UNITED KINGDOM OF GREAT BRITAIN AND NORTHERN IRELAND)	Last point is partly addressed in 3.1.6. Other aspects of this question were included in text now moved to Chapter 1.
44	3	0	0	0	0	The chapter seems to be written as if naturally caused and warming caused disasters can be treated separately. It needs to be made clear that what climate change does is to induce greater and greater changes in the natural types of variations and extremes, and that separation is just not possible, with some said to be natural and others human. Thus, on page 2, lines 14-15 where it says that "we can still anticipate a wide variety of natural weather and climate extremes to occur" really misses the point, somehow implying there wil be onew without any human influence (admittedly, we may not now be able to detect the human influence with 95% confidence, but that does not mean there is no human influence and the event is "natural"--everything is being affected, in terms of intensity, frequency, location, duration, etc. (MacCracken, Michael, Climate Institute)	FAQ3.2 addresses this question directly. It is also addressed in other parts of the chapter.
45	3	0	0	0	0	With respect to organization, I think it would be much more helpful to organize the various sections by first providing a global overview of what is happening and various general issues about limits of data, etc., and then for each region (or mega region) covering historical and then projected changes. The typical resource manager looking for information in this chapter that expands on what is said in the SPM is going to care about their region, and right now it is very hard to pull out what is happening by region--and working from past to future would allow an integrated discussion of what has been to what is projecte to be happening. At the very least, I think that there are quite a number of locations where long paragraphs should be broken up and then have a location in bold at the start to make it easier for readers to find relevant materials. (MacCracken, Michael, Climate Institute)	Tables 3.2 and 3.3 are specifically designed to allow a focus on regions. In the main text the focus is nearly always on global aspects first, followed by regional discussions (eg page 23), but wil see if this can be imporved to improve the flow in specific sections.
46	3	0	0	0	0	In general, this chapter seems to me overly long-winded in a number of places. Nice to be encyclopedic, but it does in some places seem so long that the message is obscured. Thus, I'd like to suggest a bit more use of bold when stating findings, and making very sure that the topical sentences of paragraphs explain what is in the paragraph and if a new topic arises, start a new paragraph so the typical reader can better find what they might be looking for. (MacCracken, Michael, Climate Institute)	Reject. Have tried to do all of this throughout Chapter. Will examine how to better structure long paragraphs and consider more use of bold text for important conclusions, in specific instances.
47	3	0	0	0	0	The word "may" needs to be scrubbed, and the appropriate word from the IPCC lexicon used. (MacCracken, Michael, Climate Institute)	Agreed - where an appropriate word from the uncertainty lexicon is suitable. However, "may" may still be appropriate in certain circumstances (such as this sentence).
48	3	0	0	0	0	That the change in the heat index is not alos considered seems an important omission. (MacCracken, Michael, Climate Institute)	Health-related temperature indices that include information on relative humidity are considered in the chapter. Have now mentioned heat index though.
49	3	0	0	0	0	Summarizing changes in extremes over large areas is likely, given the nature of shifting climatic zones, to lead to changes of opposing senses with the large IPCC regions. Phrasing needs to be careful not to lead to the misimpression that this means we cannot say anything or that there is no change occurring--changes in location and type of extremes in particular places can be at least as important in changes in the number or intensity of extremes. (MacCracken, Michael, Climate Institute)	Very difficult with the available literature, and considering the stringent space limits on this Chapter. But have now tried to address this concern eg last paragraph of 3.1.6.
50	3	0	0	0	0	I found it a bit confusing, and think the general reader will find it more confusing, to be using the word "assessment" to describe both this report and findings in specific situations (e.g., page 2, line 55 says "The following assessments …" and means, apparently, the discussion of what is happening with particular variables. I think that internal to the whole assessment report, it would be better to use the word "evaluation" to describe what is being done for each variable. (MacCracken, Michael, Climate Institute)	Reject. SREX is not titled as an assessment; it is a "Special Report". Chapter's use of "assessment" is not confusing.
51	3	0	0	0	0	I was very surprised that there was no simple figure of a Gaussian (or other) distribution of normal conditions and then showing the various types of shifts and how these could lead to quite large fractional changes in the number of excesses of a particul (MacCracken, Michael, Climate Institute)	Figure is being considered for inclusion in Chapter 1. Not appropriate for Chapter 3.
52	3	0	0	0	0	It seems to me that the maps on which all of the projections are produced, including the map showing regional domains, should be equal area maps--not various Mercator like projections that greatly distort the areas of various conditions. Basically, Greenland should be half the size of India, not several times as large. (MacCracken, Michael, Climate Institute)	All figures will use the Robinson projection if possible, which is not equal-area but better suited than Mercator.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
53	3	0	0	0	0	As a general point for this chapter, it really needs to be explained that statistically significant (or not) means with a confidence level of 95% or whatever. It was learned quite clearly during the SAR when the chapter on detection-attirbution used traditional jargon from statistics and this was taken out of context (saying something like the human influence had not been formally detected--when what it meant was that the detection did not yet give a 95% significance level, etc.) and used to critque the SPM statement that the balance of evidence indicates a discernible human influence. The statements are actually perfectly consistent if understood in their contexts (in the chapter, using a statistical test, and in the SPM using a relativel likelihood framing), but that was lost in a storm of skeptic criticism. To avoid confusion, it is absolutely essential to be indicating that phrases like "a statistically not significant trend" (see page 21, lines 5-23) mean that a trend has not YET been identified with 95% statistical confidence (or roughly 20 to 1 odds in its favor)--it deos not mean that changes are not occurring. And in a number of the situations, the problem is that the observational record is not long enough, so the denominator in the analysis is the problem, being quite large and, for short records, also including some of the signal. What really needs to be done is to write clearly and without jargon--not slipping into terminology that means one thing to statisticans and something lese to the general public (as they will be presented quotes out of the chapter). It would also be helpful to indicate, in some way, that alternative explanations, as for example that the changes are due to only natural forcing, have a much lower level of likelihood--which is the standard that is appropriate for the SPM. (MacCracken, Michael, Climate Institute)	We have now included a statement in the ES that low confidence of the existence of a trend does not mean that such a trend does not exist.
54	3	0	0	0	0	As a general principle in presenting the findings, it seems to me that the opening sentence of paragraphs should present the main finding of a paragraph, and then later sentences should provide the regional and other details, etc. In quite a number of paragraphs, the lead sentence is about information that is just not critical (more studies were done by ... with a list of references). The idea is that these chapters should be helpful to the reader seeking information about what is going to happen, and not a progress report about this study and that study, which is fine, but as supporting information. So, please rewrite to put the main conclusions in the leading sentence of paragraphs and it might well also be helpful to put them in italics or bold--the next version needs to make it much easier for the reader to find the key information. (MacCracken, Michael, Climate Institute)	Main conclusions in each section are collected into a final paragraph, as well as in the ES and Table 3.1.
55	3	0	0	0	0	Congratulations again! This version improved in comparison to the FOD. However, I do have some concerns with the new material brought into the chapter from very recent and unpublished papers. To avoid overlap, I will not repeat the comments on this issue nor any other general comments on this chapter which I have contributed to the response by the Dutch Government. My other comments below are generally minor. As before, I have not reviewed the other chapters of this SREX report. Therefore, I will not be able to comment on the consistency between chapters. As indicated in my ZOD comments, this may be more important for the (C)LAs to consider than further shaping the current text. (Klein Tank, Albert, KNMI)	Noted.
56	3	0	0	0	0	Executive summary: This chapter is a terrible potpourri of weather and climate. (Wurzler, Sabine, LANUV NRW)	Noted. Both weather and climate extremes are considered in this chapter.
57	3	0	0	0	0	Reading chapter 3 I found it very difficult that "climate event" is something that I would call a little longer lasting weather event. Furthermore defining "climate extremes" is even more misleading. Reading such a report, I would expect that it deals with changes in extreme events triggered by climate change. Apparently it does not, because most of the phenomena called climate here, are on such a short time scale that it is basically weather. And how to call now climate change induced changes? Climate change induced climate extremes???? Reading chapter 3 gets better after reaching page 8. (Wurzler, Sabine, LANUV NRW)	Do not understand point of comment - both weather and climate extremes are considered in this chapter. For instance, "drought" is a climate extreme, not a weather extreme.
58	3	0	0	0	0	The specific instance resulting in this comment is the paragraph starting p14 l55. I agree with the point it is trying to make but feel that it needs to be made stronger and supported across the SREX. The issue is the response to the reasonable question "Is this (recent) event due to anthropogenic infuence". As implied in the paragraph this is an ill posed question as there is no answer - yes or no are both wrong answers. The only question that can be answered is how anthropogenic influences have changed the event (either in terms of its likelihood or severity). It would be very helpful for both the disciplin and policymakers (the public too) if SREX could take a strong lead in educating people in this mater. I feel this should be mentioned in the exec summary and the SPM. (Brown, Simon, The Met Office Hadly Centre)	Clearly, we agree with the reviewer's comment about the importance of answering this question. But this question is also addressed in FAQ3.2. Since the FAQs will likely be bound with the SPM we feel the question (and its answer) will be raised to the appropriate level of visibility.
59	3	0	0	0	0	The number of figures is highly biased towards a much higher number of figs describing in details the projected changes, while figures relative to present trends are absent. I suggest to show few figures describing what is discussed in this chapter at least for the major changes in Temperature and Precipitation (ITALY)	Reject. For the purposes of the intended audience of this Special Report the projected changes are much more important than past changes. So there is an understandable tendency to focus on the projections.
60	3	0	0	0	0	There is a general problem with the definition of “floods” in the SPM and Chapter 3. Technically, what is meant is river discharge, as this is what is being observed in records and what is being modelled. There are very few models actually simulating changes in flood extent, duration and depth due to climate change. This is also explicitly acknowledged in Chapter 3 (Page 55, Lines 35-36). This needs to be corrected, or at least acknowledged that other processes determine flood occurrence and characteristics, than pure discharge rates (NETHERLANDS)	This issue is addressed in the first paragraph of 3.5.2. But revisions for final version will consider how to improve this discussion.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
61	3	0	0	0	0	A discrepancy exists between the mostly moderate extremes for which statements are included in the chapter and the extremes that cause the major impacts assessed in the other chapters of SREX. This is fine if the changes in the more moderate extremes are used as an indicator for the changes in the more impact-relevant extremes (for which evidence of change is usually more difficult to obtain). However, it needs more emphasis in the introduction to be able to place the findings in context. (NETHERLANDS)	This issue is discussed in 3.1.2 (especially middle paragraph of page 6). In final version this will be discussed in Box 3.1.
62	3	0	0	0	0	Related to the previous point: the text contains a long discussion on the definition of extremes. Simultaneously, it is concluded that the distinction which has been made in the chapter is somewhat arbitrary. For example, one could argue that the percentage area for which extreme conditions apply (e.g. the area hit by a severe drought) is a measure of extremes too. In the present version of the chapter, these type of metrics are left out completely. A more pragmatic choice for defining extremes following the categories used in the other chapters would be helpful. (NETHERLANDS)	Space limits meant that the chapter could not discuss every possible extreme (or the wide variety of extreme definitions). Discussion of various ways to consider extremes now in Box 3.1.
63	3	0	0	0	0	The majority of the figures Ch3 of the SOD are new compared to the FOD. This is of concern because nearly all new figures are from the same two papers which are heavily referenced in the text too. One of these papers (Zwiers et al., 2011) has become available recently, but the other paper (Orlowski and Seneviratne, 2011) is not available yet. Therefore, tracing statements based on this paper back to the source is difficult if not impossible. Note that the use in model simulations of some of the indices of extremes that Orlowski and Seneviratne use in their paper is not straightforward. (NETHERLANDS)	Orlowski and Seneviratne was made available to reviewers of the SOD. It has been accepted for publication. Details of the figure preparation wil be included as supplementary information to allow readers to reproduce them.
64	3	0	0	0	0	The typical time period for which the changes have been assessed is the decadal time scale. Year-to-year changes/variability and (multi)century scale changes (from paleo sources) have been included only a few times. Proposition to elaborate on this focus on recent decades in the introduction. (NETHERLANDS)	Space limits mean that we had to focus on the most important aspects, for the intended readership. This meant only limited space could be spared for paleo observations.
65	3	0	0	0	0	The multi-step attribution described in Ch3 to link observed changes to causes (including human factors) is different from what has been stated on detection/attribution in the WG1 and WG2 reports of AR4. In the WG1 report the multi-step approach was not advocated, whereas it was the norm in the WG2 report. Apparently, the guidance paper on this topic (Hegerl et al., 2010) has changed the way both working groups treat attribution issues. This raises the more general question of stating more explicitly what is new evidence and what are new insights in SREX compared to the AR4 (NETHERLANDS)	Where appropriate the changes since AR4 are noted (eg page 42).
66	3	0	0	0	0	The tables with regional findings are the core of what many policy makers will be looking for, but currently still relatively difficult to read. We suggest to provide this information in a set of graphics as in the first-order draft (and then also use this in the SPM). (NETHERLANDS)	Reject. The tables are more explicit than any schematic graphic could be. We were concerned about difficulties in ensuring that the graphics and tables were compatible.
67	3	0	0	0	0	The chapter should pay more attention to the relative importance of trends and variability in extremes, including variability on decadal and seasonal timescales. This is a key consideration for many decision-makers -- this report should provide some insight in how they have to weigh how to use information on trends in extremes for decisions that play out at a range of timescales. (also in the context of general move towards providing climate information in the context of "climate services", including provision of climate ifnormation across a range of timescales) (NETHERLANDS)	Reject. This seems out of scope of this Chapter.
68	3	0	0	0	0	It would be very helpful if the sub-sections in Sections 3.3 and 3.4 of this chapter would be split up into separate headings (e.g. types of temperature/precipitation observations, and their attribution, and projections). Now the reader has to wade through the text, and it is difficult to identify what evidence/literature is found where. (NETHERLANDS)	Noted. Some of the sections are now very long. Will reintroduce these headings, at least for the longer sections (eg 3.3.1 and 3.3.2).
69	3	0	0	0	0	There is a general problem with the definition of "floods" in this chapter (and the SPM). Technically, what is meant is river discharge, as this is what is being observed in records and what is being modelled. There are very few models actually simulating changes in flood extent, duration and depth due to climate change. This is also explicitly acknowledged in the chapter (Page 55, Lines 35-36). This needs to be corrected, or at least acknowledged that other processes determine flood occurrence and characteristics, than pure discharge rates. (NETHERLANDS)	See response to comment 60.
70	3	0	0	0	0	There could be more attention to observations related to changes in convective weather; particularly wind, hail, extreme rainfall and lightning. Now only hail is discussed to some extent. For impacts all these related observations are important, and ample literature is available. Perhaps this warrants a separate discussion in Section 3.3.2. (NETHERLANDS)	Ample literature does not exist regarding changes in lightening. Wind, hail and extreme rainfall are discussed in as much detail as the literature allows. Some sentences wil be added to final version to imrpove discussion of these small-scale events.
71	3	0	0	0	0	Chapter 3 provides a very detailed and comprehensive assessment of changes in weather and climate extremes. This very rich and dense information is much appreciated, but also comes somewhat at the expense of readability. (NETHERLANDS)	Noted. This revision will work hard to improve readabilty without sacrificing scientific rigour and detail.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
72	3	0	0	0	0	This chapter should address the difference between experiencing more events that would seem extreme today, and more events that will seem extreme when they are experienced. An entire box would probably be useful. The authors of this chapter are well aware that in some cases, te more frequent crossing of (for example) a temperature threshold will occur, while in other cases the threshold for an adverse impact will rise along with the mean temperature. The authors may not realize, however, that some of the sections in other chapters are implicitly based on the assumption that we will see more extreme events relative to the future norm. Surely even in those cases where people can adapt to the new normal, the transition will have impacts during extreme events--for example, a storm surge on top of a higher sea level will destroy buildings designed for lower flood levels. But that is not the same as increased frequency of severe events. (UNITED STATES OF AMERICA)	Earlier versions of this chapter included more on this topic, but this is now expected to be discussed elsewhere in SREX.
73	3	0	0	0	0	It would be nice to see some figures from observations, like one for extreme precipitation. 10 figures seems like not so many to me, particularly as so many are from one study. It might also be nice to start off with a standard plot of how extremes respond to a shift in mean for a normally distributed variable, accompanied by a discussion of how much data is required to pin down the tails. (UNITED STATES OF AMERICA)	Figure prepared for Chapter 1.
74	3	0	0	0	0	References that could be useful in the document: Powell, M. D., and S. D. Aberson, 2001: Accuracy of United States tropical cyclone landfall forecasts in the Atlantic basin 1976-2000. Bull. Amer. Met. Soc., 82, 2749-2767. Powell, M. D., E. Uhlhorn, and J. Kepert, 2009: Estimating maximum surface winds from hurricane reconnaissance aircraft. Weather Forecasting, 24, 868-883. Powell, M. D. and T. A. Reinhold, 2007: Tropical cyclone destructive potential by integrated kinetic energy. Bull. Amer. Meteor. Soc., 87, 513-526. Irish, J. L., and D. T. Resio, 2010: A hydrodynamics-based surge scale for hurricanes. Ocean Engineering, 27, 69-81. (UNITED STATES OF AMERICA)	Reviewer does not indicate how these references would add value to the large number of references (~1000) already cited in this chapter.
75	3	0	0	0	0	General comment: This chapter (and the entire SREX report) is punishingly long and has way too many textbook-like treatments that poorly serve a report where the reader is trying to find information specifically on extremes. The text could benefit from being edited by at least a third. To give just a couple of examples, section 3.2.2.2 is a rambling general discussion on attribution. If this text was better focused specifically on attribution of extremes, the length could be cut at least in half. Section 3.2.2.3 is another over-long general discussion of attribution that has very little to do with extremes. Section 3.2.3.1 goes into way too much detail on general methodology regarding downscaling that has little to do with extremes. And so on throughout Chapter 3. (UNITED STATES OF AMERICA)	We have tried to keep a balance between keeping the length and detail in check, balanced against the demands of reviewers to add even more length and detail (eg see comments 74, 76, 77). Some discussion of methodological issues is necessary, to inform the likely readers of this report. If a reader doesn't need this information they could simply read the ES and tables 3.1-3.3.
76	3	0	0	0	0	It would be useful to restate at the beginning of the chapter what is meant by the term "projection". We assume that it means the change projected by the end of the 21st century under some scenario like A1B. In any case, it needs to be clarified that when statements are made that something will increase, the report is not referring to the next decade or next 20 years but to a longer time scale change. (UNITED STATES OF AMERICA)	Time-scale of projections is spelled out where appropriate (eg caption to table 3.3). This topic is discussed in what wil be Box 3.2 in final version.
77	3	0	0	0	0	The whole chapter could incorporate more paleoclimate data to fortify observations from the instrumental period. This is especially important because in so many cases confidence levels in trends and projections are quite low for extreme events. Although paleoclimate data might not necessarily improve the confidence levels, it will provide a deeper context for understanding possible trends in extreme events. See individual comments below for further elucidation. (UNITED STATES OF AMERICA)	Reject. This request to increase the length of the chapter is contrary to comment 75. It would add length and complexity and reduce readability, but not add illumination.
78	3	0	0	0	0	The figures are in general very difficult to read and comprehend because they incorporate so much data (i.e., Fig. 3.3, 3.4, 3.5, 3.6, 3.7, 3.8, 3.10). Perhaps it would be better to illustrate only selected cases to emphasize where trends are clear (or not). Alternatively, the complex figures as they stand could be retained but augmented with close-ups of particular cases where trends stand out with high confidence. (UNITED STATES OF AMERICA)	Reject. The figures will be clearer in final printed versions.
79	3	0	0	0	0	The choices of locations for placement of tables within the text seem strange and might cause confusion. Reconsider big picture of layout before finalizing. (UNITED STATES OF AMERICA)	We have considered placement very carefully, but this question will be re-addressed as we get closer to a final production version.
80	3	0	0	0	0	In general, more consistent and easily accesible handling of definitions would be conducive to easier reading of this report. We think the scattering of definitions is a major shortcoming in presentation. (UNITED STATES OF AMERICA)	There will be an SREX glossary to overcome this issue.
81	3	0	0	0	0	The adjective "Mediterranean" is used in this chapter as shorthand notation both for the Sea and the region surrounding it. In some cases the meaning is ambiguous. Use of the appropriate noun would remove confusion. (UNITED STATES OF AMERICA)	Agree. "Mediterranean region" will be used instead.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
82	3	0	0	0	0	The overall tone of the chapter is quite negative and seems to be an excuse and explanation on why nothing much can be said. There are indeed many problems in dealing with extremes especially because of there inherently rare nature, the inadequate observations and the rather poor ability of models to simulate variability and extremes. However, the uncertainties cut both ways. While it may not be possible to attribute, one by one, a change or event to human activities, at least in part, by the same token it can not be ruled out either. Yet frequently only the first perspective is mentioned. I found the two FAQs to be especially wishy-washy and rather useless as they do not say anything useful and do not summarize the changes going on that have links to humans, whether proven one by one or not. The fact is that global warming is unequivocal and very likely due to human activities according the AR4, and so how can any event or phenomenon not have a human component, even if small? FAQ 1 does not answer the question, and neither does FAQ 2. All discussion is of the nature of not saying anything and on the limitations of attribution studies and models, especially in sections 3.1 and 3.2. The chapter is unduly long and repetitious, and arises partly from the framing of dealing with variables, phenomena, and impacts separately. Often expectations or understanding is not well spelled out to frame the subsequent discussion. (Trenberth, Kevin, NCAR)	Reject. The chapter is objective and not "negative". It seems useless to keep repeating that any extreme must be being affected by anthropogenic influences, since we would also have to keep saying that we cannot say what the effect has been. As well, there is a lot of difference between concluding that global warming must have some influence on a specific extremes to being able to say if it has a meaningful influence.
83	3	0	0	0	0	Nicely written chapter. The ES does a particularly nice job of balancing climate model results and confidence in those. The explanations are there when needed and clear. (Prather, Michael, University of California, Irvine)	Noted. Thanks.
84	3	0	0	0	0	I am surprised that a very important part of extreme "climate" - that of air quality extremes is not even mentioned. It is one of the more pressing extremes directly related to weather/climate extremes, and is a large government concern. Also it does not fall under the human dimensions side (ie, other chapters). There is a literature out there regrarding the high O3 during the 2003 EU heatwave caused directly by the meteorology. Note refs:RM Doherty, MR Heal et al. (2009), Current and future climate- and air pollution-mediated impacts on human health, Environmental Health, 8:S8 (21 December 2009) Stedman, J.R. (2004) The predicted number of air pollution related deaths in the UK during the August 2003 heatwave, Atmos. Environ., 38:1087-1090. Filleul L., et al. (2006) The Relation Between Temperature, Ozone and Mortality in 9 French Cities During the Heat Wave 2003, Environ Health Perspect, doi:10.1289/ehp.8328. Stott, P.A., D.A. Stone, M.R. Allen (2004) Human contribution to the European heatwave of 2003, Nature 432:610-613. (Prather, Michael, University of California, Irvine)	Noted. This question was considered early in the scoping of the chapter and it was concluded that air pollution was not in scope.
85	3	0	0	0	0	Extreme tropical cyclones have been very well documented. We propose, at least, one passage to be written on tropical cyclones like Katrina. Some materials can be written on tropical cyclone of Gonu that is very important from viewpoint of the affected region. (IRAN, ISLAMIC REPUBLIC OF)	Reject. Focus on Katrina would leave us open to a charge of bias, we believe.
86	3	0	0	0	0	This chapter seems very descriptive. It would be better, some sentences to be written on models and modeling experiment. (IRAN, ISLAMIC REPUBLIC OF)	Most focus is on projections (and other reviewers asked for more on observed change)
87	3	0	0	0	0	Please use same dictation throught report for combined words like heatwave- heat wave (IRAN, ISLAMIC REPUBLIC OF)	Noted. Will standardise this usage.
88	3	0	0	0	0	Use of "threshold" and related terms. In this and other chapters, a number of related terms are used, sometimes synonymously and sometimes differently: climate threshold (which also appears in the glossary), absolute (possibly impact-related) threshold, statistical/probability-based threshold, vulnerability/social (impact-related) threshold, tipping point, critical threshold, critical transition, regime shift. These terms are used to define extreme events or impacts or to characterize non-linear, abrupt, and/or possibly irreversible changes. Where these terms are used, the author team should ensure that the usage is not ambiguous and that it is consistent across chapters. (IPCC WGII TSU)	Noted. Will check usage through chapter to ensure they do not introduce ambiguity.
89	3	0	0	0	0	The chapter uses the term "global warming" in a number of locations. It would be preferable to use "anthropogenic climate change" throughout, as done already in some places in the chapter. (IPCC WGII TSU)	Agreed.
90	3	0	0	0	0	The assessments of observed and projected trends and attribution in sections 3.3 through 3.5 are thorough and effective. To increase the readability of the chapter, it would be helpful to re-introduce the subheadings for observations, attribution, and projections as appropriate within these sections, also so that the chapter can be more easily parsed when used as a reference. (IPCC WGII TSU)	Agreed, at least for longer sections (3.3.1, 3.3.2).
91	3	0	0	0	0	Where the author team evaluates and assigns summary terms for agreement, it often seems that the conceptualization of "agreement" (e.g., as consistency among model results or reanalysis products) is more nearly "consistency" of evidence in the framework of the AR5 Guidance Note on Treatment of Uncertainties. (IPCC WGII TSU)	Reject. The way we have used the uncertainty guidance is clearly explained, and our usage matches the spirit and letter of the guidance.
92	3	0	0	0	0	There are large parts of the text that are tutorial in nature. They should be moved to either an appendix or supplementary material. The text is well written, but very hard for non-experts in statistics to understand or appreciate. The text as is would be ok to keep if it was heavily referenced in the rest of the chapter. I did not find those references. (Stouffer, Ronald, NOAA)	Reject Because of the intended audience it was considered that we did need some tutorial sections, to ensure the reader will understand the basis for the rest of the report. There is no requirement for a reader to read the entire chapter - the "tutorial" text is provided in case they want more information about how the assessments were done.
93	3	0	0	0	0	There is an issue with balance in this subsection. The issues of sampling/variability is missing. (Stouffer, Ronald, NOAA)	Not clear which section reviewer is referring to.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
94	3	1	0	2	0	This is an excellent and very comprehensive chapter. However, nothing is said in the Executive Summary about floods, the climate driven hazard that causes the damages to human beings. See comments below on page 55 and 57. (Barros, Vicente, IPCC WGII TSU)	Reject. Page 3, lines 27-32 discuss floods.
95	3	2	0	4	0	No bold letters in this executive summary…? (GERMANY)	Have now added bolding.
96	3	2	0	4	0	The Executive Summary (ES) contains information about extreme changes in three ways: changes in the frequency of extremes, changes in the magnitudes of extremes, and changes in waiting times. It would be easier to understand if a single framework was adopted for describing changes in extremes at least in the ES. The use of confidence language in the ES is sometimes confusing. (CANADA)	Unfortunately the available literature does not allow us to treat all extremes in a consitent manner as suggested by the reviewer. Do not understand how the use of the confidence language is confusing.
97	3	2	1	0	0	Executive summary: Statements about statistical significance of the changes are made only occasionally; this is inconsistent and should be avoided. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Not clear if reviewer wants more or fewer references to statistical significance. We are trying to avoid the term "significance" if we mean anything other than "statistical significance" and also to use the term "statistical" if we mean "statistical significance". In the ES the occasional use of the term is for very specific purposes and becasue we could not avoid use of the term without predjudicing the accuracy of the very brief statements appropriate for the ES.
98	3	2	1	0	0	Executive Summary: Since this chapter deals with “Changes in Climate Extremes and their Impacts on the Natural Physical Environment”, we may need to start with a simplified definition for an “extreme (climate or weather) event” in the Executive Summary. (Mokssit, Abdalah, Direction de la Météorologie Nationale (DMN))	Definition is in Glossary. Chapter 3 does discuss aspects of this definition, but it is inappropriate for a very short ES.
99	3	2	1	0	0	Executive summary: In this chapter (3), all assessments regarding past or projected changes in extremes are expressed following the new IPCC AR5 uncertainty guidance (Mastrandrea et al., 2010; p.9). Do we need to summarize this new uncertainty guidance and put it in the beginning of the Executive Summary? (Mokssit, Abdalah, Direction de la Météorologie Nationale (DMN))	This should be in Chapter 1.
100	3	2	1	2	1	The bit about "unprecedented events" would apply to an unperturbed climate as well. Regardless of whether the climate is changing, unprecedented, previously unobserved events (records) will occur, simply because the instrumental record is of finite length. Records are set continually, but would also be set (albeit at a bit different pace) in a stationary climate. See, for example, the response to FAQ 3.2, which looks at an unprecedented extreme event (in a given location). The simple analysis presented there suggests that on a global scale, this event was extreme, but not unprecedented, indicating that it could have happened in an unperturbed climate. (Zwiers, Francis, Environment Canada)	The statement this review comment appears to be aimed at simply makes the point that a changing climate can lead to changes in extremes - it no way implies that an unprecedented extreme cannot arise in an unchanging climate. Lines 14/15 clearly indicate that extremes would occur even without climate change.
101	3	2	1	2	2	Replace "an extreme event" with "meteorological extreme events" and "event" with "meteorological event". (Wurzler, Sabine, LANUV NRW)	"weather and climate extremes" included in first line of first section of Chapter (and in first sentence of ES).
102	3	2	1	2	10	A definition of climate event and of weather event is missing here. You give a definition later in chapter 3, but it should also occur here. Such as "Climate is the long term manifestation of either the condition or the course of the weather.It is defined by the statistical collection of weather conditions for a given region during a specified interval of time, usually several decades. Weather events are defined on a much shorter times scale than climate events. Weather represents the state of the atmosphere at a given time and place, with respect to variables such as temperature, moisture, wind velocity, and barometric pressure. Heavy snowfall during one winter or a thunderstorm are examples of weather events." An example of a climate event would be an asset here. (Wurzler, Sabine, LANUV NRW)	Reject. Not appropriate in ES.
103	3	2	1	4	14	In my review of the FOD I asked about the difference between a "weather event" and a "climate event". This question appears to have been ignored. See for example P2 L4 and several other instances in the Executive Summary (including "weather and climate extremes" and "climate or weather variable"). I continue to think that the question is reasonable. In particular, it is very likely to occur to rational policy-makers. The question may have been ignored because it has no answer and the adjectival "weather or climate" has been used simply because it sounds good. Once again I urge the authors either to define the difference or to stop using the double adjective. See also my comment at Ch4 P4 L31. (Cogley, J. Graham, Trent University)	Point is addressed in lines 4-9 on page 6. The reviewer's comment has not been ignored. Inclusion of this discussion is inappropriate for an ES.
104	3	2	1	4	14	Any references to AR4 findings are left out of the Executive summary. This is unfortunate, because this type of information which has been included in the underlying sections (e.g. p30, lines 14-18) is very useful. (Klein Tank, Albert, KNMI)	It was considered that AR4 assessments are out-of-place in the ES, and might lead to confusion with SREX assessments. The AR4 assessments are still discussed in the main text when appropriate.
105	3	2	1	4	14	I was a bit disappointed that the ES was so lacking in quantitative values, for example the page3/L42 on torpical cyclones seems lacking in so useful details compared with the later boldened section (page45/37-48). Also, the scale spent on issues seems out of balance with a long ES section on e.g., landslides compared to a scattered scetion on cyclones. (Prather, Michael, University of California, Irvine)	There is a single 8-line paragraph summarising a variety of physical impacts (not just land-slides). Quantitative values on tropical cyclone changes seem out-of-place in this ES. Readers of the ES can turn to the Chapter if they need more material and detail.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
106	3	2	1	4	14	The readability of the executive summary could be significantly improved by highlighting the key findings in bold, like in the other executive summaries. (NORWAY)	Agreed. Will use bolding.
107	3	2	2	2	2	Add "Changes in climate may lead to changes in weather." (Wurzler, Sabine, LANUV NRW)	Reject. Current wording states this, in more detail, and focussed on extremes.
108	3	2	3	2	3	At start of line, change "can" to "is very likely to"--indeed, saying "is very likely to lead to noticeable changes in" or say "is virtually certain to lead to changes in"--there is no question there will be changes of some kind, and "can" just speaks to potential capability--not indicating that changes will be occurring. (MacCracken, Michael, Climate Institute)	Sentence will be rewritten and "can" deleted.
109	3	2	3	2	4	At the end of the line, change to read "duration of extreme events, quite likely leading to at least some unprecedented extremes." I don't think the matter of whether they have been "previously unobserved" is relevant--perhaps "previously unexperienced" would be, but that is what unprecendented covers. (MacCracken, Michael, Climate Institute)	Agreed. Delete "previousy observed". Sentence has now been slightly reworded.
110	3	2	3	2	10	This passage can be shortened with reference to chapter 1 and 2 (GERMANY)	Reject. Referring to another chapter in the Executive Summary would seem quite strange, and would only save a few words.
111	3	2	3	2	10	This para should represent the main conclusion of 3.1. I don't think it does. There are bold conclusions is 3.1 which do not appear in the summary. In additon this para does not really add any interesting new information. (NETHERLANDS)	First paragraph provides a very small amount of important information without which the rest of the ES might be mis-understood.
112	3	2	3	2	18	First two paragraphs are very detailed for an ES. (Stocker, Thomas, IPCC WGI TSU)	Disagree - these two paragraphs succinctly provide important background ifnormation without which the rest of the ES could be misunderstood by a reader.
113	3	2	3	2	18	It does not hurt to have redundancies in the various chapters of an IPCC report, however at this stage (SOD) I believe this is the last opportunity to clear those issues up. I wonder whether these two first paras should not better go to chapter 2, since chapter 3 as well as chapter 4 ought to work from this very same basis. (Fischlin, Andreas, ETH Zurich)	Reject - see response to comments 110 and 112.
114	3	2	3	4	14	Other than heat waves, there is very little in the Executive Summary about changes that might lead to health impacts (e.g., disease friendly conditions, changes in frost lines, etc.). (MacCracken, Michael, Climate Institute)	Do not understand this comment - SREX is not just about changes leading to health impacts. But in fact much of what is said in the ES is relevant to health impacts - eg concluding that tropical cyclone related rainfall rates are likely to increase.
115	3	2	3	4	14	The whole executive summary should be written so that it represents a clear concise and understandable overview. Remove all redundancy (of which there is a lot!) so that the main findings become clear. Remove overlap between paragraphs. As for the SPM, the exec summ should also highlight what is new compared to the previous assessment, consistent with what the SPM should contain. (NETHERLANDS)	Reject. The ES is about 2000 words. This is about half the length of the typical AR4 ES. There is almost no "redundancy" -the small amount is necessary to provide background information so a reader does not have to read all of Chapter 1 and 2 to avoid misunderstanding this ES. The task of this chapter was not just to highlight what was different from AR4 - this is a special report with a specialised audience, and should not be confused with AR5.
116	3	2	4	2	6	I would suggest rewriting to say "As well, weather and climate events, although not necessarily extreme in a statistical sense, can still lead to extreme conditions or impacst, either by crossing a threshold in a social, ecological, or physical system, or by occurring simultaneously with other impacting events." (MacCracken, Michael, Climate Institute)	Agreed.Sentence rewritten to reflect comment.
117	3	2	4	2	7	Hard to understand : if not stat extreme, = not something rare, how can it become extreme for these "systems"', including physical one ? What is "an extreme impact" if it is not something rare ? The second explanation - combination with other changes - is easy to understand, but the first one looks odd. (BELGIUM)	A moderate El Nino event often leads to extremes in the regions it impacts. So, the El Nino in such a case is not extreme - but its impacts can be.
118	3	2	5	2	5	...a critical threshold - tipping point - in a social… (GREECE)	Reject. Critical threshold expresses this idea sufficiently.
119	3	2	7	2	9	In that "phenomena" is plural, I'd suggest saying "Meteorological phenomena such as tropical cyclones can have extreme impacts, depending on where and when they approach land, even if specific cyclones are not extreme relative to other cyclones." Not only does this switch to plural, but a storm does not have to make landfall to cause problems. (MacCracken, Michael, Climate Institute)	Noted. Sentence rewritten to take this into account.
120	3	2	9	2	13	the last sentence of first paragraph is almost similar with the first sentence of the following paragraph. (GREECE)	No. First of these is about a phenomenon causing extremes in many regions; the second is about extremes arising from natural climate variability (to avoid SREX being accused of simply atributing all extremes to human actions).
121	3	2	10	2	10	Change "may" to "have the potential to" (MacCracken, Michael, Climate Institute)	Reject. Do not see that the extra words clarify the statement. But replace "may" with "could".

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
122	3	2	12	2	12	I am not sure it makes sense to distinguish here weather and climate extremes without giving some explanations. In this respect chapter 4 is even worse and talks about extreme weather and climate events. Since English tends to confound weather and climate languagewise, you need either to clarify for the reader what you mean or drop the distinction entirely (e.g. by writing "Many extreme events are the result of ..."). (Fischlin, Andreas, ETH Zurich)	Reject. This is done on line 4-9 on page 6. Not appropriate for ES.
123	3	2	12	2	13	Many weather... : are there extremes in which climate variability does not play a role ? -> might be better to say something like "natural variablity (...) is a key factor in (...) extremes" (BELGIUM)	This statement is in the ES to avoid SREX being attacked for implying that only human actions cause extremes.
124	3	2	12	2	18	This paragraph needs work to make clear that natural and human caused extremes are not separate, but that what climate change will do is modify the natural tendencies, magnitude, frequencies, etc. Thus, perhaps say "Until the influence of human activiites, extremes in weather and climate such as El Nino events have been the result of natural climate variabilty and what appear to be natural decadeal and multi-decadal variations. As human influences on the climate increase, the patterns, frequencies, intensity and timing of the natural variations will be altered to varying degrees, leading to a changing mix of weather and climate extremes. Because changes in extremes of a climate or weather variable are not always related in a simple way to changes in the mean of the same variable, and in some cases can be of opposite sign (e.g., shifts in storm tracks can lead to increases in precipitation in some regions and decreases elsewhere), quantitatively separating out the natural and human influences is not always possible." Also, with respect to existing text, saying "the next century" literally means the 22nd century, not the rest of this century, as the text appears to be implying. (MacCracken, Michael, Climate Institute)	Reject. Too detailed for ES. Agree that comment on "next century" is incorrect - delete "over the next century". Revise text.
125	3	2	12	2	18	This para should represent the main conclusion of 3.1. I don't think it does. There are bold conclusions is 3.1 which do not appear in the summary. In additon this para does not really add any interesting new information. (NETHERLANDS)	Reject. See 111.
126	3	2	13	2	13	It would be preferable to be more specific about the mentioned "anthropogenic changes." Presumably "anthropogenic changes in the occurrence [intensity and/or frequency] of weather and climate extremes" is meant, and it would be helpful to specify this more directly to avoid ambiguity for the reader. (IPCC WGII TSU)	Reject. Meaning is clear.
127	3	2	14	0	0	There is a switch from conditional voice ("if there were") to something more definite ("can" rather than "could"). Our suggested remedy would be to change to "can." However, the sentence could also be restructured to convey the thought without mentioning ACC at all. (UNITED STATES OF AMERICA)	Agreed. Sentence reworded.
128	3	2	14	0	0	Why would a wide variety of extremes occur? Needs more. In general, the issue of sampling problems seems underdiscussed/highlighted in this chapter. (Stouffer, Ronald, NOAA)	Reject. The expectation that many extremes will occur even without human action has nothing to do with sampling. Extremes have always occurred, even before humans exerted any influence on the climate, and this sentence simply says that this would continue to happen in the future.
129	3	2	14	2	15	Note that this sentence was reworded in the SPM - see SPM page 1, lines 52-53. (Stocker, Thomas, IPCC WGI TSU)	Check that meaning of the ES and SPM sentences is identical.
130	3	2	15	2	15	Delete "to occur". (Cogley, J. Graham, Trent University)	Editorial. Sentence has been slightly reworded.
131	3	2	20	0	0	Note explicitly that "some changes in extremes" in this paragraph generally refers to moderate extremes only. (Klein Tank, Albert, KNMI)	Reject - too much detail at this stage. Sentence is not open to misinterpretation here.
132	3	2	20	0	0	Extremes never occur on a regular schedule, so their frequency, duration, etc. are always changing, on multiple time scales, almost by definition, in the climate system. The preceding paragraph acknowledged that internal variability creates extremes, but not that it also creates trends in extremes. With no ACC, trends in extremes will ALWAYS be up or down. I cannot tell whether the authors are concluding here: (1) simply to report what the directions of these trends were or (2) statistically significant according to some test, (3) to identify trends that are outside the range of expectations from internal variability or natural variability (i.e., "detected" trends), or (4) a trend that is attributable to anthropogenic forcing, and if so what specific anthropogenic forcings. Which interpretation are the authors intending to say? The answer to these questions (throughout the Chapter and SPM) is generally made clear in Table 3.1, but it could be made clearer in the text. (UNITED STATES OF AMERICA)	The detail required to answer these questions is in the main text. We would need a much longer ES if we were to answer all these questions, for each of the observed trends in the extremes. Not appropriate for an ES.
133	3	2	20	2	20	Delete "occurring". (Cogley, J. Graham, Trent University)	Editorial. Sentence has been reworded.
134	3	2	20	2	20	It seems to me that some context is needed here that gives a sens of what the projected changes will be like, with shifting climatic zones, shorter cold seasons and longer and more intense warm/hot seasons, etc. Because of this one can have what seem to be opposing changes in extremes, just in different locations. (MacCracken, Michael, Climate Institute)	Projected changes are discussed in detail through the rest of the ES. Sentence to which the reviewer refers is about observed changes, not projections.
135	3	2	20	2	22	These indices of extreme temperature of unusually warm days and nights and unusually cold days and nights are very easily misunderstood to mean unusually warm/cold days/ nights within respective warm/cold seasons, but this is not necessarily the case given how this index is derived. Suggest a footnote be added here to ensure that this result is properly understood by readers. (CANADA)	Reject. Too detailed for ES.
136	3	2	20	2	22	This sentence seems so important, it should likely be the first one in the paragraph, and it should be bolded or italicized as a key result. (MacCracken, Michael, Climate Institute)	First sentence of paragraph will be bolded - it provides headline for the paragraph.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
137	3	2	20	2	33	The executive summary discusses expected changes in weather phenomena and meteorological variables from future climate projections. However, changes in extremes over recent decades were discussed only for a few phenomena such as extratropical cyclones or high wind speed. To avoid this inconsistency, statements about past changes should include all relevant extremes. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Reject. The audience for SREX is more focussed on projected changes than on what has happened in the past.
138	3	2	20	2	33	Suggest separating into different paragraphs findings that state likelihoods and those that state confidence, for clarity. (SWEDEN)	No. Want to keep all ES statements about observed changes in single paragraph. Focus in Chapter 3, and in the ES, is on the projected changes.
139	3	2	22	0	0	A period shold be inserted after 'for land areas with data'. (JAPAN)	Editorial. Period added.
140	3	2	22	0	0	Add "most" before "land areas with data". (UNITED STATES OF AMERICA)	Agreed. Added.
141	3	2	22	2	22	Insert "sufficient" ahead of "data". (Zwiers, Francis, Environment Canada)	Agreed. Added.
142	3	2	22	2	22	I do not understand why the result for North America is only "likely" rather than "very likely" (MacCracken, Michael, Climate Institute)	That is the Chapter authors' assessment, based on the available literature.
143	3	2	24	2	24	It is not clear how one would interpret a statements to the effect that there is medium or low confidence in trends in some region. This could be interpreted as meaning that there not a lot of confidence in the available trend estimates in that region, either because the data at the locations where the trends are calculated are of poor quality, or because there is lack of confidence in methods, etc. Alternatively, this could be interpreted as meaning that there is insufficient coverage to establish a general trend for the region based on a limited spatial coverage of available trend estimates, and differences from place to place in the directions of those trends. The latter is quite different from the former. I think the basis for these assessment should be briefly stated in the ES. (Zwiers, Francis, Environment Canada)	Too much detail for ES. Details are in Chapter text.
144	3	2	24	2	25	I would think it helpful to explain here that these results are the case in part because the observational record is much less adequate and long as for the other areas. (MacCracken, Michael, Climate Institute)	Too much detail for ES. Details are in Chapter text.
145	3	2	25	2	25	The start of the sentence should be changed to something like "Confidence in reconstructing historical occurrences and in projecting future changes of extremes (including ..." I think it worthwhile to make sure to indicate that we don't have all possible info we need for the past or the future. (MacCracken, Michael, Climate Institute)	Too much detail for ES. Details are in Chapter text.
146	3	2	25	2	26	In talking about warm spells and heat waves, it seems to me important to also be indicating that the relative humidity tends to remain nezr constant, and so the heat index goes up substantially. (MacCracken, Michael, Climate Institute)	There is very little published work on observed trends in humidity in the context of extremes. Consider adding Sherwood and Huber (2010) to Chapter, but probably not for ES.
147	3	2	26	2	26	"the number ... has increased" and "there have been ... increases". (Cogley, J. Graham, Trent University)	Editorial. Changed.
148	3	2	26	2	28	This is a confusing compound statement, which uses an overall assessment of probability 'likely', and a reference to statistically significant trends at the grid point level. It would be more clear to say that there have been increases in more regions than there have been decreases and use the results of a field significance test to give an overall assessment of probability. Also 'e.g.' should be 'i.e.', otherwise the text implies that another percentile could be chosen with the same result, but this wouldn't necessarily be the case. (CANADA)	Reject. Proposed rewrite would be very confusing for readers.
149	3	2	26	2	28	Nouns and verbs do not match singular/plural. (UNITED STATES OF AMERICA)	Agreed Thanks.
150	3	2	27	0	0	What is meant by this "statistcally significant?" Am I to understand that the authors subjectively assess that certain trends are great enough not to be caused by internal variability in the climate system in more than one location out of 20? Or is this a quantitative statement based on a specific hypothesized (possibly incorrect) model of internal variability, simply echoing the words of the author of a paper? If the latter, what is the assessed level of confidence that the variability model is correct? In either case, wouldn't it be important to say whether either of these areas is larger than would be expected from internal variability? And doesn't one of the areas have to be bigger than the other anyway? (UNITED STATES OF AMERICA)	Details of this are in main text - such a discussion would be misplaced in ES.
151	3	2	27	2	28	is "in more regions" equivalent to "more than 50% of area"? Please clarify. (Stocker, Thomas, IPCC WGI TSU)	No, because we only discuss "statistically significant" trends here - so some areas have neither positive or negative significant trends. The statement in the ES is correct.
152	3	2	27	2	33	I would urge making these lines a separate paragraph--they talk about precipitation changes instead of just temperature increases. (MacCracken, Michael, Climate Institute)	Nothing in the paragraph indicates it is just about temperature. We want all the short statements on observed changes to be in a single paragraph.
153	3	2	29	2	29	This statement is confusing. It would be better to make a positive assessment of what can be said, rather than an assessment of the low confidence placed in trends reported in the literature. E.g., "There is little evidence of a trend in tropical cyclone activity, after accounting for observational uncertainty" or something similar. (CANADA)	Reject. Sentence is phrased to allow the use of IPCC uncertainty language. Proposed rewrite would lead to confusion because it avoids the calibrated language.
154	3	2	29	2	30	The real information need for decision makers/resource managers, is how the trend could possibly be. The wording here seems far too negotiated to read usefully. Are the trends generally upward but just not nearly statitsically significant due to variability? Is there an y indication that the trends are negative--should this say that there is no indigation of decreasing trends in tropical cyclones, for example? (MacCracken, Michael, Climate Institute)	See chapter for details. Too much detail for an ES.
155	3	2	29	2	30	Please explain "accounting for past changes in observing capabilities". (NETHERLANDS)	Too much detail for ES. Details are in Chapter text.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
156	3	2	29	2	30	The author team should consider including further (brief) explanation here of why there is low confidence in reported long-term increases in tropical cyclone activity. (IPCC WGII TSU)	Too much detail for ES. Details are in Chapter text.
157	3	2	30	2	31	As written, this sentence literally implies that post-1950 drought intensity/duration exceed pre-1950. We think the writers intend rather to convey the presence of an upward trend during the post-1950 period. If so, the wording needs to change. (UNITED STATES OF AMERICA)	Agreed. Re-worded.
158	3	2	30	2	33	Does the statement of medium confidence apply only to the first clause ("more … droughts") or also to the second ("opposite trends")? If it includes the second, then change to "...but also THAT opposite trends exist." If it doesn't, then break the sentence up and specify a confidence level for the second clause. (UNITED STATES OF AMERICA)	Sentence reworded to avoid this problem.
159	3	2	31	0	0	The word "but" implies an inconsistency of sorts, even though modeling work suggests we should expect trend directions for many water-related variables to vary spatially. (UNITED STATES OF AMERICA)	Reject. We still need to correct the impression that droughts are worsening globally. But sentence reworded for clarity.
160	3	2	32	2	32	When the text says "opposite trends exist" it makes it sound as if there is large disagreement about ultimate trends. Instead, what needs to be said, it would seem, is that climate zones are shifting and this can lead to opposing trends existing "in different regions" (phrase needs to be added). What it seems to me is key to do here is to get across what the expectations are (and why) and what the observations are. (MacCracken, Michael, Climate Institute)	No. This paragraph is just about observed changes, not expected (projected) changes. But sentence reworded for clarity.
161	3	2	32	2	32	Unless referring to specifically named regions (or perhaps if that is region name on the map--and if so initials should be used), there was too much capitalization going on--it shoulc be "central North America and northwestern Australia." This comment could be made generally--I found too much capitalizing of adjectives. (MacCracken, Michael, Climate Institute)	Editorial. Changed capitalization throughout chapter.
162	3	2	32	2	32	What does 'opposite trends' mean? More floods or are the droughts becoming shorter and less in severe? (NORWAY)	Sentence reworded to avoid this problem.
163	3	2	34	0	0	Observed and projected changes in the sea ice summer extent should be addressed (International Petroleum Industry Environmental Conservation Association (IPIECA))	Not an extreme.
164	3	2	35	0	0	Are we correct that the executive summary jumps from a listing of observed historical trends to model-based projections, without any statements about attribution of the observed trends? If so, why is attribution not addressed? (UNITED STATES OF AMERICA)	Short attribution paragraph has now been added to ES.
165	3	2	35	2	38	This paragraph is a general statement that could well be moved up. (MacCracken, Michael, Climate Institute)	Paragraph has now been combined with similar considerations for observed changes and moved above observed changes paragraph.
166	3	2	36	2	36	"depending on" would be clearer than "linked with". (Cogley, J. Graham, Trent University)	Changed.
167	3	2	37	2	38	An important point, economically expressed. (Cogley, J. Graham, Trent University)	Noted. Thanks.
168	3	2	37	2	38	It would be helpful to specify in Executive Summary if "low confidence" in observations similarly "neither implies nor excludes the possibility of changes in this extreme." (IPCC WGII TSU)	Sentence has been broadened to include observations and moved above paragraph listing observed changes.
169	3	2	40	2	40	Insert "the" before "21st". (Cogley, J. Graham, Trent University)	Editorial. Added.
170	3	2	40	2	41	As noted in a general comment for the SPM and chapter 4, I think there needs to be discussion about the significance of choosing 1961-90 as the baseline period--not just this particular period, but also choosing 30 years instead of some other period. For an unchanging climte, choosing a 30-year period might adequately average over the ENSO cycle and occasional volcanic eruptions and a little bit less adequately, over the solar cycle. In a changing climate situation, however, and when the various potential impacts have a range of timescales ranging from perhaps a decade to centuries, it seems to me some consideration needs to be given to the choice that has been made. For example, if the period, say 1900-1970 had been used, so the time before the IPCC has concluded that GHGs are having the primary impact on the temperature record, then basically all conditions would be considered in the highest few percent or so of all conditions (recall, each of the last 20 years has been warmer than any year in the 1900-1970 record). It might then be understood why (for systems that are affected by the annual average temperature) the present situation is so stressful for many existing ecosystems, water systems, etc. While a lot of specific examples could be listed, it seems to me to be essential for there to be discussion about the choice of a 30-year span and the particular one that was chosen--and what these choices mean. (MacCracken, Michael, Climate Institute)	Noted. But this chapter is about changes in extremes, from the current situation. Therefore, the choice of baseline period does not really matter a great deal. But see comment 171 also.
171	3	2	40	2	41	Are the selected projection time horizon (2100) and reference period (1961-1990) generally used throughout the SREX report, or for this chapter only (with the other chapters focussing on decadal trends and the near future)? (Klein Tank, Albert, KNMI)	This reference period is of especial importance for this chapter. But the details of the reference period has now been removed - not needed for ES.
172	3	2	41	2	41	Is 'generally' needed here? - In our reading of chapter 3, the reference period appeared to be consistently 1961-1990. (Stocker, Thomas, IPCC WGI TSU)	See response to #171.
173	3	2	41	2	41	"1961-1990" Clarify for general audience why this period was selected and not the entire period of record (or why you don't use the POR). (UNITED STATES OF AMERICA)	Sentence now deleted from ES. Not needed for ES.
174	3	2	42	2	43	The statement that uncertainty from natural climate variability is large during this time frame could be misinterpreted as meaning that natural climate variability will diminish in future. (Zwiers, Francis, Environment Canada)	Agreed. Re-worded.
175	3	2	42	2	43	"due to natural variability" needs to be changed to something like "due to the interactions of human-induced climate change with natural climate variability." (MacCracken, Michael, Climate Institute)	Agreed. Re-worded.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
176	3	2	42	2	43	Paleoflood data (proxy/paleodata) provide important information to help reduce the large uncertainty. It seems over the last decade or so, modelers have not appreciated the value of this physical data. Whether it is due to a lack of understanding of data, or modelers don't know how to use the data to help calibrate/validate models is unclear, but it likely has limited progress and improvements in models/modeling. (Jarrett, Robert, USGS)	Do not understand point of comment.
177	3	2	42	55	7	As I commented previously, the current performance of the CMIP models regarding the rate of global temperature change as well as the patterns of vertical temperature change in the tropics decrease the confidence in climate model performance (including the performance related to the characteristics of drought). (Knappenberger, Paul, New Hope Environmental Sciences)	Uncertainties and problems with models are considered in reaching assessments regarding projections, through the main text.
178	3	2	43	2	45	this point could be clarified with an additional sentence. Also do the authors mean to imply it is certain? (UNITED STATES OF AMERICA)	Reworded. See 174 & 175.
179	3	2	44	2	45	Elsewhere in the report, there are statements that data from the past holds no value for the future, yet they acknowledge large uncertainties in models. Certianly, models/modeling will improve in time, but they will be limited in not using data (e.g., Milly et al., 2008) and many paleoflood references. (Jarrett, Robert, USGS)	Don't understand the point of the comment.
180	3	2	45	2	45	Replace "For other extremes" by "For the intensities of other extremes, or changes in the occurrence of extremes (in particular, ...)" (MacCracken, Michael, Climate Institute)	No. Extra words do not add clarity.
181	3	2	45	2	46	It wold be easy to avoid the term "scenario uncertainty": use "uncertainty of future emissions" or "scenario choice" (as on p. 3, l. 40) (Brönnimann, Stefan, University of Bern)	Agreed. Re-worded.
182	3	2	46	2	50	This is a rather long and awkward sentence. "The provided assessments" presumably means "In the evaluations provided in this chapter, ucertainty ranges from the direct evaluation of multi-model ensemble projections are modified by taking ..." Then start a new sentence at "the possibility" (MacCracken, Michael, Climate Institute)	Agreed. Re-worded.
183	3	2	47	2	48	How is past performance taken into account? There are almost no detection and attribution studies to provide formal constraints on projections based on past performance, and little research to date that would show how to otherwise usefully constrain projections based on past performance. Thus we are left with a situation where we must assume that projections of change will be less reliable if historical climatology is less well simulated. It would be good to say something like this. (Zwiers, Francis, Environment Canada)	Comment seems to be repeating what is stated in chapter. Seems to detailed for ES.
184	3	2	50	2	51	I think this could be better expressed by avoiding a reference to "confidence" (which is a word with a specific meaning). Perhaps replace "is generally less confident" with "is generally greater". (Zwiers, Francis, Environment Canada)	Agreed. Re-worded.
185	3	2	50	2	51	The phrase 'uncertainty is generally less confident' is confusing. Do the authors mean that the uncertainty range is larger? Or that its size may be larger or smaller but there is less confidence in its magnitude? Particularly in the ES, it would be easier to understand if the authors don't focus on the confidence in the uncertainty estimates. (CANADA)	Re-worded (see 184 also).
186	3	2	50	2	51	It is important to distinguish between the real world and models – models will never provide a complete detailed description. They are designed to reproduce the most essential features of a system, and hence provide the 'big picture'. Due to the vast range of scales, GCMs may never have the capacity to get all the details right. (NORWAY)	True. This is what the statement says - but re-word (see comments 184, 185).
187	3	2	53	2	53	Surely projections of warming are projections of changes in temperature (not of changes in temperature extremes)? (Cogley, J. Graham, Trent University)	Temperature extremes can warm - for instance a warm extreme could become warmer.
188	3	2	53	2	55	This sentence should be rephrased. In the current form it says that models do a reasonable job on temperature extremes, but overestimate the warming. I guess this conclusion it ment to say something different. (NETHERLANDS)	Sentence has been replaced by clearer discussion of the point and shifted into paragraph about how uncertainty is assessed.
189	3	2	53	3	7	This para overlaps with para page 2 lines 20 - 33. (NETHERLANDS)	Page 3 lines 20-33 are about recent observed changes; the paragraph starting at the bottom of page 2 is about projections.
190	3	2	54	2	54	this point needs to be clarified. What warming is overestimated? (UNITED STATES OF AMERICA)	Sentence rewritten for clarity - by including in previous paragraph.
191	3	2	54	2	55	For clarity, change to read "However, simulations of changes in extreme temperatures during the late 20th century suggest that, although models simulate temperature extrmes quite well, they are apparently over estimating the warming influence of grrnhouse gases or underestimating the coling influence of aerosols or natural variations." (MacCracken, Michael, Climate Institute)	This sentence has been deleted and substance has now been included in previous paragraph.
192	3	2	55	0	0	The overestimation concerns all the regions ? Including the Arctic ? (International Petroleum Industry Environmental Conservation Association (IPIECA))	Not sure of relevance of comment.
193	3	2	55	2	55	Don't just focus on the warming of the warmest temperatures in the year; extremes temperatures occur at both ends of the temperature distribution! Models generally under-estimate the observed warming of cold extremes (TNn), and over-estimate the observed warming of warm extremes (TXx) (Zwiers, Francis, Environment Canada)	Sentence has been deleted but substance, including point raised by reviewer, has now been included in discussion in previous paragraph.
194	3	2	55	2	55	The phrase "they may overestimate the warming" is unclear. Does this refer to the mean temperature or to the extremes? If it's the former, it is not thought to be true for climate models in general. If it's the latter, then shouldn't this refer to changes in the frequency of extremes? (CANADA)	See responses to comments 191, 193.
195	3	2	55	2	55	"they may overestimate the warming." Does this mean the mean warming or the increase in maximum temperatures? (UNITED STATES OF AMERICA)	See responses to comments 191, 193, 194.
196	3	2	55	2	55	It is surprising that models simulate temperature extremes well, given their coarse resolution, and limited ability to reproduce the frequency of blocking. Furthermore, we have doubts that any of the CMIP3 GCMs reproduced the European heat wave of 2003, or the cold winters 2009/2010 and 2010/2011. At least, this statement should be backed up with references and 'well' should be defined. (NORWAY)	Sentence has been deleted. See response to comment 193.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
197	3	2	55	2	55	It would be preferable to indicate more precisely the degree to which simulations overestimate the warming. (IPCC WGII TSU)	Too much detail for ES.
198	3	2	55	2	56	How is over-estimation taken into account in the assessments? (Zwiers, Francis, Environment Canada)	Detail is in main text (3.1.5;3.2.3.2; 3.2.3.3; Box 3.1).
199	3	2	56	2	57	Correct the repetition of material in page 2 lines 48-49. (Cogley, J. Graham, Trent University)	See response to 193. Removes duplication.
200	3	2	57	0	0	A "virtually certain" statement needs to be more precise than exists in the current text. When is "into the future"? When does the virtually certain assessment apply? (UNITED STATES OF AMERICA)	Line 40 and line 53 set the time-scale of projections as generally the end of the 21st century. However, the terminology used in this sentence "...the observed increases...will continue into the future.." is very clear - the observed increases will continue, even in the short- to medium-term future. But see response to 201.
201	3	2	57	2	57	This sentence is inconsistent with other text. The sentence says that it is virtually certain the observed increases in the warm extremes of temperature will continue into the future. But on line 20, the Executive Summary states that it is only 'very likely' that these increases have in fact occurred. (CANADA)	Re-worded.
202	3	2	57	2	59	This sentence about warm and cold temperature extremes is ambiguous. Are the "increases" in warm extremes of daily temperature referring to increases in magnitude or increases in frequency? Similarly, are the "decreases" in cold extremes referring to magnitude or frequency? (IPCC WGII TSU)	Added "frequency and magnitude"
203	3	2	58	2	58	Remove the comma. (Cogley, J. Graham, Trent University)	Editorial. Agreed.
204	3	2	59	2	59	"A,B and/or C will increase" strictly reads as either each of A,B and C will increase, or only one of them (A,B or C) will increase. Is this what the sentence is intended to convey? (Global Climate Observing System Steering Committee)	No. Believe that point is clear.
205	3	2	60	0	0	Chap 3, page 2, line 60: It's surprising that the increase in incidence in hottest days is likely to be least in the high northern latitudes, considering that the mean warming is projected to be greatest there. Is this right? (UNITED STATES OF AMERICA)	Yes. This is a function of temperature variability as well as the magnitude of the warming.
206	3	3	1	3	2	It seems quite strange that the high latitudes would be different, so I would think that an explanation is needed.Does this refer to what is happening over just land areas, or does it include the Arctic Ocean, which would be expected to moderate fluctuations? Is it that the measure of variability being used is from the time when sea ice was present, so variability was large, and thus moving by that amount is just unlikely once the ocean becomes open for much of the year? I just think the reader (and I am one of them) will want an explanation given that the actual change in temperature is much larger in the Arctic than elsewhere. (MacCracken, Michael, Climate Institute)	Too much detail for ES. See response to 205.
207	3	3	2	3	3	The appearance, without explanation, of what might be read as a self-contradictory phrase ("moderate extreme") in the executive summary would be unfortunate, potentially lowering the credibility of the report below what it merits. (UNITED STATES OF AMERICA)	Sentence deleted.
208	3	3	2	3	6	We have a number of concerns with these sentences : (1) Need to explain what a moderate extreme is (consistent with explanation on page 6). (2) Need to make clear the link between the projected temperature change of 2-3 degrees and the A2 scenario, (3) Scaling factors will need to be explained to readers of an Executive Summary since this is a technical term that will not be understood by many readers, (4) Sentence 2 is hard to interpret since the the first part of the sentence says that it is likely (P>66%) that warming of 2-3 ° will lead to much larger warming in certain regions or seasons while the bracketed phrase gives a likely (P>66%) range of factors (presumably for different regions) and seasons of 0.5-2.5 degrees. Does the range represent the best estimate of the range of scaling factors for different locations? Or ranges of a single scaling factor derived from different models? And is the scaling between mean warming and warming of extremes? This all needs to be explained if the bracketed phrase is retained. It would also be helpful if 'much' were quantified. (5) Lastly the following sentence says that some regions and seasons extremes might not warm at all. Why then does the range not include negative numbers? (6) Changes to this text to improve clarity should also be considered for where these results appear elsewhere in Chapter 3 (Page 23, and again on page 25). (CANADA)	Delete sentence starting "Moderate (cold and warm).." since the following sentence repeats this, and doesn't use "moderate extremes". Delete the clause with scaling factors from the ES.
209	3	3	3	0	0	What means "moderate seasanal extremes" mentioned only here in the whole SREX ? Without explanations the following sentence « A mean global warming of 2°C or 3°C is likely to lead… for moderate seasonal extremes) » should be modified. (BOURRELIER, PAUL-HENRI, AFPCN)	See response to 208.
210	3	3	4	3	5	According to AR4, the likely range of A2 scenarios (1990-2095) is 2-5.4 degree warming. I.e., overlaps with the 2-3 degrees quoted. (Assumedly, what is written here refers to some specific scenario study. Still, the statement is not really clear.) (SWEDEN)	Delete sentence.
211	3	3	5	0	0	What is meant by scaling factors is not clear even after reading the whole text. How is the range 0.5 to 2.5 impacted by uncertainty in the changes versus issues with sampling? The distnction needs to be clearer. (UNITED STATES OF AMERICA)	See response to 208.
212	3	3	5	0	0	The scale factor of 0.5 to 2.5 does not sound like "likely much larger" since there is a chance of much smaller. Try to reconcile the impression of contradiction with the text (assuming the numbers are correct). (Prather, Michael, University of California, Irvine)	See response to 208.
213	3	3	5	3	6	Discussion of "scaling factors" is far to technical a point for an ES (only specialists would know what this refers to). (Zwiers, Francis, Environment Canada)	See response to 208.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
214	3	3	6	3	6	This statement needs to make clear that this is the case early in the process and becomes less and less likely as temperatures continue to rise (or at least that it takes quite special conditions in these areas--such as very dry areas becoming quite moist, etc.). (MacCracken, Michael, Climate Institute)	Sentence rewritten.
215	3	3	6	3	7	Make it clear that this last sentence is talking about extremes. Also, this sentence needs to make clear the time horizon that is being discussed. I think this is a reasonable thing to say if making a projection of the next few decades, but the implicit time frame is the end of the 21st century (see line 1 of this page) - and I'm not sure that there is any evidence from climate models that would support this last sentence for that time frame. (Zwiers, Francis, Environment Canada)	See response to 214.
216	3	3	6	3	7	If this refers mainly (or in total?) to the observed period, rather that projections for the 21st Century, it would be good if the textwere revised, for clarity. (SWEDEN)	See response to 214.
217	3	3	9	0	0	Why the parentheses? Does it indicate lower confidence? If not, what does it indicate? And does "or" mean "and," or "or?" It if means "or," then the authors are saying that the confidence is attached to the idea that one or the other will increase, but we don't know which. It just seems to be an odd construction, with ambiguous meaning. (UNITED STATES OF AMERICA)	Remove parentheses.
218	3	3	9	3	9	Please clarify: Should "or the proportion" actually be "and the proportion". As written the sentence says that it is likely that either one will increase. (CANADA)	See response to 217. Removing parentheses clarifies meaning.
219	3	3	9	3	12	Unclear which seasons these changes in heavy precipitation are projected for (why is Winter highlighted for northern mid-latitudes?). Please specify. (Stocker, Thomas, IPCC WGI TSU)	Too much detail for ES.
220	3	3	9	3	16	I miss a statement about changes in summer precipitation. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Too much detail for ES.
221	3	3	9	3	16	Given the model physics in the CMIP class of models are by design parameterized to tend to atmospheric stability and thus produce drizzle rather than episodic extreme precipitation events While I applaud the caution for temperature (Chap 3, page 2 lines 53 to 55 " Model projections of changes in temperature extremes are for substantial warming by the end of the 21st century. However, simulations of late 20th century changes in extreme temperatures suggest that although models simulate temperature extremes quite well, they may over-estimate the warming. The following assessments take this possible over-estimation into account, along with the possibility that some processes important for temperature extremes may be missing or be poorly represented in models.", I am surprised that there is not an even more forceful caution in the discussion of model projections of changes in the frequency of heavy precipitation. (Webb, Robert, NOAA)	The uncertainty language used for precip reflects the lower confidence, part of which is derived from the poorer performance of models in simulating heavy precip. The discussion on uncertainty in the Box 3.2 in the final version, discusses the different uncertainty in the different variables.
222	3	3	9	3	16	Also this para overlaps with para page 2 lines 20 - 33. (NETHERLANDS)	No. Para on page 2 is about observed changes; here the text is clearly about projected changes.
223	3	3	11	3	12	Why is central Europe singled out? This phenomenon is seen more broadly in climate models, particularly in subtropical climates. (Zwiers, Francis, Environment Canada)	Example deleted.
224	3	3	11	3	16	Somewhere along here it needs to be explained that these findings are for relatively large areas (i.e., averaging is done over sub-continental size regions) and so even greater extremes can occur over smaller areas (e.g., over Russia in the summer of 2010). It also needs to be said that changes can occur as climate shifts so that some areas may not get decreases in return periods. It should also be noted that shifts in climatic zones can lead to greater problems even if the return period does not decrease because the region may be more vulnerable to the type of extreme. (MacCracken, Michael, Climate Institute)	Most of this is already expressed in this paragraph, with the exception of the shift in climatic zones (which is outside the scope of SREX, except in so far as it is reflected in changes in extremes). Changed wording in paragraph final sentence will indicate that greater warming could be found in some regions.
225	3	3	12	3	13	Suggest the listing of scenarios match, in order, the results, so it's clearer that lower emissions scenarios produce less frequent events (one-in-fifteen year events) and higher emissions scenarios produce more frequent events (one-in-five year). Just reverse the listing of the scenarios. (CANADA)	Changed wording.
226	3	3	14	0	0	Editorial : "more extreme" is a bit odd, as it might be unclear what an extreme scenario is (close to A1FI, which is not included ?); a wording without "extreme", eg. "higher emissions scenarios considered" could be better. Same issue on L38 of same page. (BELGIUM)	Deleted this text.
227	3	3	14	3	14	It's awkward to refer to some types of emission scenarios as being "more extreme" - first because this is a report on extremes (and so using the word in other contexts simply adds to confusion about what we mean by extremes), and secondly because this usage suggests that probabilities are associated with the different members of the SRES family of emissions scenarios - which is explicitly not the case. (Zwiers, Francis, Environment Canada)	Agreed. See response to 226.
228	3	3	14	3	14	Della-Marta et al., 2007b ----> Della-Marta et al., 2007b; Matsueda, 2011 Matsueda, M., 2011: Predictability of Euro-Russian blocking in summer of 2010, Geophys. Res. Lett., 38, L06801, doi:10.1029/2010GL046557. (Kusunoki, Shoji, Meteorological Research Institute (MRI))	This comment must be wrongly placed?
229	3	3	14	3	14	Suggest to not use the term "extreme emission scenario" here and throughout the Chapter. "Extreme" is not well defined and the use of "extreme" might be confusing in the Special Report on … Extreme Events… (BTW, neither SRES A1B nor A2 are "extreme" scenarios within the set of 40 SRES scenarios). Suggest to refer to SRES B1, A1B, and A2 as illustrative low, intermediate, and high-CO2 (or CO2equivalent) emissions scenarios. (Stocker, Thomas, IPCC WGI TSU)	Agreed. See response to 227.
230	3	3	14	3	14	Insert "the" before "21st". (Cogley, J. Graham, Trent University)	Agreed. Changed.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
231	3	3	14	3	16	For the half of the sentence here about projected decreases in waiting times for most regions and for the sentence about increases or no changes in waiting times, what is the associated likelihood or confidence level? Are these also "likely" findings? (IPCC WGII TSU)	Deleted text.
232	3	3	15	0	0	The term "waiting time" is new to me and is not likely to be understood by the intended audience for the executive summary. (UNITED STATES OF AMERICA)	Convert all "waiting time" usages to "return period".
233	3	3	15	3	15	waiting time', this makes it sound as though you would be guaranteed to have a certain event e.g. once every 5 years - and subsequently is a bit misleading. 'Return period' or accompanying explanation of the terminology would assist. (UNITED KINGDOM OF GREAT BRITAIN AND NORTHERN IRELAND)	Noted. See response to 232.
234	3	3	18	0	0	Only "very likely" ? The statement is so general that I doubt that it could be otherwise. This seems "virtually certain", unless you have reasons to believe that there will be no SLR or that for some other reason extreme will not increase accordingly? Or perhaps no likelyhood at all if there is no doubt at all about such statement. (BELGIUM)	The basis for this uncertainty level is explained in 3.5.3.
235	3	3	18	3	18	How can this finding only be "very likely"? Especially given that the rate of sea level rise is probably going to be a good deal greater than AR4 estimates, this finding seems to me to be "virtually certain" and if not, this should be explained. (MacCracken, Michael, Climate Institute)	The basis for this uncertainty level is explained in 3.5.3.
236	3	3	18	3	19	How does a small change in mean sea level result in a large change in extreme sea level? I think this will be interpreted to mean that there will be a large change in the height of extreme sea levels, where I think what this means is that there will be a large change in the the frequency of threshold exceedance for a fixed threshold. The two are very different. (Zwiers, Francis, Environment Canada)	Delete this sentence.
237	3	3	18	3	19	I would suggest starting the sentence "At least in some locations, a relatively …" and drop "in some locations" at the end of the sentence--this qualifier needs to be up front. (MacCracken, Michael, Climate Institute)	See response to 236.
238	3	3	19	3	19	Please clarify: Should "mean sea level" actually be "global mean sea level"? At an individual location, if the mean sea level increases by 10cm, this will lead to an increase in extreme sea level of 10cm, other things being equal? If this is due to changes in winds, gravitational effects, etc then the larger increase in extreme sea level at some locations is not only caused by the small increase in global mean (as stated), but by these other influences too. (CANADA)	See response to 236.
239	3	3	19	3	22	Change "these" at L22, which is 2+ lines away from its noun, to "these wave-height projections". (Cogley, J. Graham, Trent University)	Sentence rewritten.
240	3	3	20	0	0	but the small....these line is not needed as winds is being talked about in line 42-43 of the same page (GARG, AMIT, INDIAN INSTITUTE OF MANAGEMENT AHMEDABAD)	No. The wind section is situated too far from this discussion of sea level. Readers would miss this link.
241	3	3	20	3	20	What makes the "eastern North Sea" (a very small region) worthy of being singled out in the ES? (Zwiers, Francis, Environment Canada)	Deleted this example from ES.
242	3	3	21	3	21	Change "combined with" to "and". (Cogley, J. Graham, Trent University)	Sentence rewritten.
243	3	3	22	0	0	Low confidence in these : concerns the full set of studies or the eastern North Sea Hs increase ? (International Petroleum Industry Environmental Conservation Association (IPIECA))	See response to 241. Delete example.
244	3	3	22	0	0	What is the information provided by this sentence - why is it only "likely" ? (BELGIUM)	Sea level will also contribute, but the sentence points out that changes in storminess will likely overwhelm this in determining the direction of change in wave heights.
245	3	3	22	3	22	Change "means" to "mean" and "positive changes to" to "positive changes in". (Cogley, J. Graham, Trent University)	Sentence rewritten.
246	3	3	23	3	23	It is helpful to be starting to give indications of confidence--should not that also be done in some of the earlier paragraphs? (MacCracken, Michael, Climate Institute)	Uncertainty terminology is used wherever possible. This means that confidence statements are more appropriate here, whereas terms such as "likely" are more appropriate elsewhere.
247	3	3	23	3	25	What is the meaning of this sentence ? Could you rephrase in a clearer way ? Does it simply means that if nothing changes ("all other factors equal"), then following further increases in sea-level, impacts will go on ? Isn't this obvious ? (BELGIUM)	Yes. That is why we express it as "high confidence".
248	3	3	25	3	25	Insert a comma after "sea levels". (Cogley, J. Graham, Trent University)	Agreed.
249	3	3	27	0	0	Rather than speaking of floods, this should be framed as an issue related to river discharge, as this is what is meant in the literature. This is also explicitly acknowledged in the chapter (Page 55, Lines 35-36). Please correct through the chapter, SPM and report. (NETHERLANDS)	Reject. Replacing floods by "river discharge" will confuse the intended readership of this report. However, now use "fluvial" to clarify this.
250	3	3	27	0	0	Chap 3, page 3, line 27: We think it's a little too sweeping to make a blanket statement that there's low confidence for changes in flooding everywhere. In some places, for example the western US, the warming signal (which we have more confidence in than the precipitation signal) changes the hydrology enough to have a significant effect on flooding. I.e., in the historical climate, considerable amounts of winter precipitation were stored as snow in this region; in the future, as more of that snow shifts to rain and the snow melts earlier, there is likely to be more flooding even without any (harder to project) changes in precipitation. At the least, we would qualify the sentence beginning on line 27 by appending ", although this varies by region.". (UNITED STATES OF AMERICA)	Reject. Even in these areas (which are discussed explicitly in the report and even in the ES) a change in precipitation could overwhelm the early melt effect on any flooding. As well, the earier melt does not simply translate to increased flooding.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
251	3	3	27	3	29	It is not clear to me who would be interested in knowing about the changes in the number of floods on a global basis--why is that a metric that should be even considered (I have not seen papers givng a model estimate, etc.). What is suggested is that climatic zones/storm tracks will shift, and this would seem likely to lead to lead to shifts in where floods occur (and maybe when during the year, etc.) rather than a change in number. Why does this sentence then lead the paragraph? And saying regional changes are complex without saying that circulation patterns and storm tracks need to be considered is really creating uncertainty rather than providing insights. Australia seems to have its traditional storm track shifting off to the south of the continent, while the climate changes are opening up the possibility of more tropical cyclones striking the continent in areas not accustomed to large amounts of rain, so, yes, the shifts do create changes in regional likelihood of flooding that need to be explained--just saying they are complex is not adequate. (MacCracken, Michael, Climate Institute)	The intended readership of this Special Report are very interested in whether there will be a global increase in floods. A detailed regional examination of how floods may change cannot fit into a two-page ES.
252	3	3	27	3	32	The second sentence of this paragraph presents a finding of much more interest than the first--it should lead the paragraph (and the sentence that leads should, quite probably, be replaced with a sentence giving more examples of regional changes. (MacCracken, Michael, Climate Institute)	We now start the floods paragraph with a sentence about how changes in temperature and precipitation imply changes in floods, before pointing out that there is still low confidence in projections.
253	3	3	28	3	29	Why are "limited evidence" and "low agreement of projections" italicized? (Cogley, J. Graham, Trent University)	Editorial. Changed.
254	3	3	28	3	29	This is true to a point, e.g., if one only looks at model results. Paleoflood hydrology provides physical data of floods for thousands of years and for the Holocene. I would rate the confidence of paleoflood hydrology as good as it captures substantial effects of climate variability/change during the Holocene and has a good likelihood for helping to assess future effects of change for some, indeterminate time into the future. Certainly, for the immediate planning horizon of 25 to 50 or 75 years. (Jarrett, Robert, USGS)	Reject. Do not see how paleo floods lead to confident projections of future flood changes.
255	3	3	29	3	32	The many causes of floods and changes in their frequency/magnitude include possibly unidentified climate-driven changes in rainfall-runoff response. We think it would be better here to use language that recognizes rain rate as one of multiple drivers. Also, the basis for the anticipation should be stated on this one, we think. We would say "Nevertheless, simple physical reasoning suggests that projected increases in short-term (i.e., daily) and/or long-term (i.e., monthly) rainfall extremes would contribute an increasing tendency to magnitude and/or frequency of rain-generated floods." On a related point, it would be desirable to note that we have little literature and hence low confidence in any estimates of the sensitivity of the rainfall-runoff "response function" itself to climate change. I.e., how, and on what time scales, does the sensitivity of runoff to rainfall evolve as the landscape itself responds to climate change? After all, the landscape consists of soils, vegetation, organic matter, earthworms and all manner of burrowing critters, none of which are indifferent to climate change. Of course, this would need to be addressed in the supporting materials also. (UNITED STATES OF AMERICA)	Sentence has been rewritten.See 252.
256	3	3	30	0	0	Which main regions are concerned ? (International Petroleum Industry Environmental Conservation Association (IPIECA))	There is no simple group of large regions to which this applies.
257	3	3	32	3	32	"Earlier spring peak flows ...": this is both a change and an impact, but it is not a change in an extreme. (Cogley, J. Graham, Trent University)	Noted but as indicated by comment 250, it can have important implications for changes in floods.
258	3	3	34	0	0	Chap 3, page 3, line 34: Similar to my previous comment, I think discussion of changes in drought has more dependence on the region involved than the blanket statement given here suggests. In the southwestern U.S. the warming itself is likely to increase aridity, and then there are likely to be precipitation changes on top of that. The projected temperature changes in this region are not inconsistent in sign, and will drive the system towards increased aridity. (UNITED STATES OF AMERICA)	Reject. The direct impact of warming on drought is limited and would be overwhelmed by precipitation and humidity and cloudiness changes.
259	3	3	34	3	34	The phrase "There is at most medium confidence..." is not useful to readers. This could mean that there might only be "very low confidence". The authors should make a single assessment of the confidence level, rather than using the "at most" qualifier. (CANADA)	Sentence reworded to remove ambiguity.
260	3	3	34	3	35	This first sentence should be moved to be the fourth sentence in the paragraph--give the information that is of higher confidence first. (MacCracken, Michael, Climate Institute)	First sentence of paragraph now reworded.
261	3	3	37	0	0	In the list of regions, the Middle East has been forgotten (International Petroleum Industry Environmental Conservation Association (IPIECA))	No. The Middle East has not been forgotten.
262	3	3	37	3	37	Would it be better to use "Southern Africa" here (as done elsewhere in the chapter) to specify that an area larger than the country of South Africa is intended? (IPCC WGII TSU)	Agreed.
263	3	3	37	3	38	Jargon. Say "reduced precipitation and/or increased evapotranspiration". (Cogley, J. Graham, Trent University)	Agreed.
264	3	3	40	3	40	Observed decline in frequency of dust storms over China for the last 50 years has been reported in many publications, and this has been related to the weakening wind speed and the decreasing frequency of strong winds. This conclusion is robust (Zhang L. and Ren G. 2003. Change in dust storm frequency and the climatic controls in northern China, Acta Meteorologica Sinica, 61(6): 744-750 (in Chinese); Zhou, Z.J. and Zhang, G.C. 2003. Strong dust storm events of northern China over 1954-2002. Bulletin of Sciences, 48(11): 1224-1228 (in Chinese)). (CHINA)	Statement in ES is about projected changes in dust storms, not past changes.
265	3	3	40	3	40	Suggest to provide a short addition to the sentence about the dust projection, to link the dust to the drought statements. (Stocker, Thomas, IPCC WGI TSU)	Have added ", although an increase would be expected in regions where aridity increases."

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
266	3	3	40	3	40	Suggest "dust storms" rather than "dust activity," to be consistent with 3.5.8. (CANADA)	Agreed.
267	3	3	42	3	42	This statement ("The relatively few studies…") is not true since in recent years, there has been a number of publications about extreme wind speeds from extratropical cyclones based on different regional climate models. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Still "relatively" few studies.
268	3	3	42	3	43	Could also mention that we have low confidence in our understanding of historical changes in wind extremes due to limitations in observations. (Zwiers, Francis, Environment Canada)	This paragraph is focussed on projected changes, not past changes.
269	3	3	42	3	50	We have a problem with the paragraph. Given that GCMs are not producing details of the tropical weather very well (e.g. MJO and the Monsoon), their limited resolution (which means that many TC-relevant relevant processes are not explicitly presented), errors in SST, we would first want to see a convincing evaluation of the models on whether they actually have any skill in predicting the metrics that are under investigation (trend). To have any value, the models also should demonstrate whether they reproduce the geographical, seasonal TC-response, and the empirical relationships with SST, wind shear, etc. Our second concern is the frequency distribution of the wind speeds in the tropics. Winds have one distribution for all times, describing situations with no wind to situations with extreme storms. If present/historical wind speeds follow a Weibull distribution, then the threshold value (10-minute sustained) for a TC is fixed at the value 63 km/h. A future with fewer TC but more frequent intense one would suggest a change in the wind speed distribution, with a reduced area under the curve for winds greater than 63 km/h, but with a thicker upper tail. The area of the curve for winds slower than 63 km/h would furthermore be greater in such a future world. This would be extremely interesting – if the frequency of calm situations were relatively unchanged, that would suggest a more complicated PDF for the winds. Alternatively, there are Weibull shapes that satisfy the change in the areas under the curve, but that would suggest a substantial increase in the number of days with calm (http://en.wikipedia.org/wiki/Weibull_distribution) (NORWAY)	These issues have been debated in considerable detail by the tropcial cyclone community. Our assessments are similar to the assessments by the tropical cyclone community.
270	3	3	42	3	61	These 3 paragraphs could be moved up to line 17, to come between 'precipitation' and 'sea level'. This would then be consistent with the chapter structure: Weather elements - phenomenon - impacts. This would also work better because you would then provide wind and cyclone projections before you talk about extreme waves (which are to some extent a product of the projected changes in wind and storminess). (Stocker, Thomas, IPCC WGI TSU)	Agreed. Move paragraphs.
271	3	3	44	3	46	"tropical cyclone related rainfall rates": I objected to this in the FOD, and it is just as troublesome now. What does it mean? (Cogley, J. Graham, Trent University)	Changed to "heavy rainfall associated with tropical cyclones".
272	3	3	44	3	46	The sentence needs to be rewritten to get rid of two appearances of the useless word "it" and indicate where results are coming from. So, the sentences could be written: Tropical cyclone related rainfall rates are likely to increase with continued warming induced by rising greenhouse gas concentrations; however, model projections indicate that the global frequency of tropical cyclones is unlikely to increase. (MacCracken, Michael, Climate Institute)	Agreed. Rewritten sentence, and split into two..
273	3	3	45	3	46	Why is there a sudden switch to the conditional, as in these two instances of "would increase"? (Cogley, J. Graham, Trent University)	Editorial. Changed.
274	3	3	46	3	48	Uninformative statement of "Likely as not' again -- Why assess reduction versus increase or versus no change since there is an equal chance for any of the three. Suggest you use " The magnitude and even the sign of any anthropogenic influence on number of mid-latitude storms averaged over each hemisphere are uncertain (Webb, Robert, NOAA)	Sentence rewritten.
275	3	3	46	3	48	Why is the result stated as a reduction in the number of mid-latitude storms? (Those in the know will understand why there is an expectation of this, but to other readers, this will not be clear.) Suggest instead just stating that we cannot yet say much about changing frequencies of mid-latitude storms and indicate reasons for expecting a decrease. (CANADA)	Sentence rewritten.
276	3	3	47	3	47	Italicize "as". (Cogley, J. Graham, Trent University)	Editorial. Sentence rewritten.
277	3	3	47	3	48	As a very minor point, "about" appears to have been accidentally deleted (also based on the equivalent sentence in the chapter text) from "about as likely as not." (IPCC WGII TSU)	Sentence partly deleted and rewritten.
278	3	3	49	3	50	I don't understand what "medium confidence" means in this context. In theory, you can count the number of projections that move storm tracks polewards, so naively, there would be a basis for a quantitative projection, that would then have to be downgraded to take various sources of uncertainty into account. If those uncertainties are such that confidence in the quantitative projection becomes medium or lower - then it would be useful to say something like that. (Zwiers, Francis, Environment Canada)	Our usage of the uncertainty language is described in the Chapter.
279	3	3	52	3	52	In mentioning "even the sign of the change" in this sentence, the author team may be unintentionally implying that, where such qualification is not mentioned for low confidence projections, the sign of change is more certain. The author team should consider how low confidence projections are characterized in the SPM to ensure consistency. (IPCC WGII TSU)	Sentence rewritten.
280	3	3	53	3	55	Delete the sentence "Land use changes and aerosols from biomass burning appear to influence monsoons, but these effects are associated with large uncertainties." since this is not expert judgement but expert speculation given the lack of any cited literature. (Webb, Robert, NOAA)	Sentence deleted.
281	3	3	55	0	0	"large uncertainties" - magnitude or sign? (UNITED STATES OF AMERICA)	Sentence deleted.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
282	3	3	57	0	0	Chap 3, page 3, line 57: Since the report calls out changes in the monsoon and ENSO, it should call out changes in the Pacific Decadal Oscillation as well. This affects the climate in significant parts of western North America, including temperature extremes (and precipitation to a lesser extent). There is some evidence that the PDO will be affected by anthropogenic forcing (Meehl, Hu, and Santer 2009, JCLI v. 22, 780-792) although I think you'd have to say that this area has not been well explored. Similarly, I'm a little surprised that neither the NAO nor the AMO are mentioned at this point, even if only to say that while they can affect extremes, there is no clear consensus on what they might do in the future (by my understanding). (UNITED STATES OF AMERICA)	Sentence on other modes now added.
283	3	3	57	3	60	The first sentence needs to be clarified--are the models projecting that a lot of different aspects of ENSO events could change, or are the models giving a wide range of projections for the changes that will occur. The second sentence really only has meaning for the very well informed--it should be rewritten to indicate the types of differences. (MacCracken, Michael, Climate Institute)	Sentence rewritten.
284	3	4	2	4	2	The phrase "higher availability" should be changed to something like "greater presence" (MacCracken, Michael, Climate Institute)	No. There is no "greater presence". But it is now more available (because the glacier covering it has melted).
285	3	4	4	4	5	This current wording begs the question: if a 'likely' increase in heavy precipitation is projected in the Tropics, and shallow landslides are clearly linked with heavy precipitation, why is there only 'low confidence' in projected changes in landslide frequency and magnitude in these regions? Possible rewording to avoid this would be: "...tropical regions, because shallow landslides depend not only on the frequency and intensity of rainfall, but also land use". (Stocker, Thomas, IPCC WGI TSU)	Changed.
286	3	4	6	4	6	"and times of large rock avalanches" should be "and TIMING of large rock avalanches" (Stocker, Thomas, IPCC WGI TSU)	Agreed.
287	3	4	10	0	0	Should also include some information on 1) Greenland current observed mass balance information and outlet glaciers behaviour 2) Closed seas such as Black Sea or Caspian Sea. In addition, there is no mention at this stage of the executive summary of the results of section 3.5.5 on coastal impacts (International Petroleum Industry Environmental Conservation Association (IPIECA))	Too much regional detail for ES. Conclusion of 3.5.5. is noted on lines 23-25 on page 2.
288	3	4	11	4	11	"... based on current knowledge." IPCC and many/most modeling studies may not recognize or acknowledge the benefits of paleoflood hydrology (or the vast wealth of other paleo/proxy data), but they do exist. There needs to be more supportive statements about all types of data as well as better working relationship between data (people) and modelers/modeling. (Jarrett, Robert, USGS)	Not sure what reviewer wants us to do.
289	3	4	11	4	12	Delete "The possibility of the occurrence of". (Cogley, J. Graham, Trent University)	Agreed. Sentence rewritten and moved.
290	3	4	11	4	13	It would be helpful to indicate, if possible, a level of confidence or evidence/agreement summary terms to characterize the conclusion that "the possibility of the occurrence of low-probability high-impact scenarios…cannot be excluded…" (IPCC WGII TSU)	Uncertainty language seems inappropriate here.
291	3	4	13	4	13	Suggest replaciing "Non-linear feedbacks" simply with "Feedbacks" - I'm not sure that the addition of "non-linear" really helps the reader a lot. (Zwiers, Francis, Environment Canada)	Agreed, especially for ES.
292	3	4	13	4	14	"Nonlinear feedbacks" we suggest omitting this from the executive summary, it doesn't seem like useful information for non-specialist readers. (UNITED STATES OF AMERICA)	Agreed. See response to 291.
293	3	5	0	0	0	The whole part 3.1 should be reduced, referring to others chapters for most definitions and general considerations. Some concepts familiar to meteorologists and climatologists, give a sense of excessive and discouraging complexity to the common reader and make him doubt about the results. The use of the new definitions of GIEC for confidence and likelihood make comparisons with the 4e report (2007) delicate. (BOURRELIER, PAUL-HENRI, AFPCN)	Noted. Effort was put into reducing length, but some information cannot be removed. Chapter 3 serves as reference on physical extremes for the other chapter of the report, which is why much material needs to be introduced. Will mention that revision of IPCC definitions for confidence and likelihood means that comparison to AR4 is not straightforward (3.1.5).
294	3	5	1	5	11	Section 3.1 text seems like it could be tightened up and shortened a bit, aided by introduction of a definitions box. Candidates for deletion would be much of the material on page 6; the indices will be seen soon enough, and the technical details (EVT, iid) don't help in understanding subsequent content. The idea of probability vs. threshold basis is repeated unnecessarily. The example of multiple heatwave indices could be shortened considerably. And so on through the section. (UNITED STATES OF AMERICA)	Effort was put into reducing length, but some information cannot be removed. Chapter 3 serves as reference on physical extremes for the other chapters of the report, which is why much material needs to be introduced. Definition box is considered useful, and has been added (based on previous material of Section 3.1). Previous text on multiple heatwave indices has been very significantly shortened.
295	3	5	3	5	40	sugestion: adding some sentence to explain the differences between climatic anomalies/extremes/abrupt/drift (Zhao, Zong-Ci, National Climate Center)	Reject. We are already criticized for having too much textbook material
296	3	5	3	11	1	Section 3.1.1 is now well-balanced. No further comments (Bitner-Gregersen, Elzbieta Maria, Det Norske Veritas AS)	Noted, Thanks.
297	3	5	8	5	8	Suggest "Extremes of weather and climate elements" (cf. Section 3.1.2). Admittedly, the subsequent text discusses both means and extremes. For the purposes of the chapter, the focus nevertheless is on the latter. (SWEDEN)	Agreed. Modified to "Extremes of atmospheric weather and climate variables".
298	3	5	8	5	12	The grouping is not very clear in the way it is presented. Is it based on events or on their impacts? This should be clarified. (GREECE)	We distinguish between events and impacts, this is one purpose of this grouping.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
299	3	5	8	5	53	Is there a distinction between an "element" (line 8) and a "variable" (line 53)? Sometimes some terms are defined and others aren't, or the defintions of related things are given at different places and are hard to find. It's confusing. (UNITED STATES OF AMERICA)	Agree. Will use the term "variable" instead of "element".
300	3	5	9	5	10	Would "atmospheric rivers" be an appropriate class of phenomena to add that influences extreme events? I recently saw a talk on flooding in the US Pacific Northwest that used this concept to good effect in analyzing historical events. (UNITED STATES OF AMERICA)	Reject. The term "atmospheric river" is not well established enough.
301	3	5	9	5	10	The author team should consider redefining the current characterization of these phenomena as "influencing the occurrence of extreme events." While phenomena like monsoons and El Ninos can be considered to fall under such categorization, cyclones can be considered extreme events themselves, in addition to influencing the occurrence of other types of extreme events. (IPCC WGII TSU)	Agreed. Text was modified to "Weather and climate phenomena influencing the occurrence of extremes in climate variables".
302	3	5	23	0	0	It should be emphasied that these phenomena are components of low frequency climatic variability (GREECE)	Reject. Too detailed for purpose of report.
303	3	5	26	0	0	Some references can be added : Rasmusson and Carpenter, 1982; Wang, 1995; Barros et al. 2002; Grimm, 2003; Haylock et al. 2006; Antico 2008; Barrucand et al. 2008; Remon et al. 2011. Rasmusson, E. and T. Carpenter. 1982: Variations in tropical sea surface temperature and surface wind fields associated with the Southern oscillation/El Niño. Monthly Weather Review, 110, 354–384 Wang, B. 1995: Interdecadal changes in El Niño onset in the last four decades, Journal of Climate, 8, 267–285 Barros, V. R., Grimm, A. M. and M. E. Doyle. 2002: Relationship between temperature and circulation in southeastern South America and its influence from El Niño and La Niña events, Journal of Meteorological Society of Japan, 80, 21–32 Grimm, A. M., 2003: The El Niño impact on the summer monsoon in Brazil: Regional processes versus remote influences, Journal of Climate, 16, 263–280. Antico, P. L. 2008: Relationships between autumn precipitation anomalies in southeastern South America and El Niño events classification, International Journal of Climatology 29, 719–727, doi:10.1002/joc.1734 Haylock, M. R., Peterson, T. C., Alves, L. M., Ambrizzi, T., Anunciação, Y. M. T., Baez, J., Barros, V. R., Berlato, M. A., Bidegain, M., Coronel, G., Corradi, V., Garcia, V. J., Grimm, A. M., Karoly, D., Marengo, J. A., Marino, M. B., Moncunill, D. F., Nechet, D., Quintana, J., Rebello, E., Rusticucci, M., Santos, J. L., Trebejo, I. and L. A. Vincent. 2006: Trends in Total and Extreme South American Rainfall in 1960–2000 and Links with Sea Surface Temperature, Journal of Climate, 19, American Meteorological Society, 1490-1512 Barrucand, M., Rusticucci, M. and W. Vargas, 2008: Temperature extremes in the south of South America in relation to Atlantic Ocean surface temperature and Southern Hemisphere circulation, Journal of Geophysical Research, 113, D20111, doi:10.1029/2007JD009026, http://200.16.86.38/uca/common/grupo72/files/Temperature_extremes_in_the_south_of_South_America_in_relation_to_Atlantic_Ocean_surface_temperature_and_Southern_Hemisphere_circulation.pdf Remon, M., Rusticucci, M. and M. Barreiro. 2011: Multidecadal changes in the relationship between extreme temperature events in Uruguay and the general atmospheric circulation, Climate Dynamics, Springer, doi 10.1007/s00382-010-0986-9, http://www.fisica.edu.uy/~barreiro/papers/RenomRusticucciBarreiro2010.pdf (GREECE)	Reject. Can only add very limited number of references at this stage. Adding these references in section 3.1.1. would be unbalanced compared to treatment of other extremes.
304	3	5	28	5	30	Suggest cross-linking to Chapter 7 (Zwiers, Francis, Environment Canada)	Agreed. Added reference to Chapter 7.
305	3	5	28	5	30	The author team might consider also providing a cross-reference to relevant sections of chapter 8 for this sentence. (IPCC WGII TSU)	Agreed. Added reference to Chapter 8. Overall chapter seemed more relevant than single sections within that chapter.
306	3	5	29	5	29	"may" should be changed to "is likely to be" or something similar in the IPCC lexicon. (MacCracken, Michael, Climate Institute)	Reject. Not a formal assessment.
307	3	5	34	5	34	It would be preferable to use the word "assignment" in place of "attribution." (IPCC WGII TSU)	Agreed. Was revised accordingly.
308	3	5	44	5	62	Maybe consider using a different terminology than 'climate event'? This really refers to particular climatic conditions, given that 'climate' refers to typical weather patterns of the PDF describing a weather variable. Although explained on p. 6 L4-9, the term is not well-defined. Alternatively, move this definition to the front, before it is referred to in the text. The phase 'extreme climate event' is equally fuzzy and difficult to use. (NORWAY)	Reject. Climate event is a well defined term. As mentioned by the reviewer, "climate" refers to the overall climatology and "event" to a single occurrence within this climatology.
309	3	5	51	0	0	Another short definition for "an extreme event" can possibly be included : "An unusually high value of a variable, rarity usually specified in terms of return period or exceedence probability" (DEFRA, 2005). DEFRA/ Environment Agency Flood and Coastal Defence R& D Programme. 2005, Joint Probability: Dependence Mapping and Best Practice: Technical report on dependence mapping. R& D Technical Report FD2308/TR1 (GREECE)	Reject. The present definition is the result of several round of reviews and iterations within the author team. The use of the term "rare" was abandoned in the earlier version following review comments to the FOD.
310	3	5	51	5	55	This is regarding the definition of an 'extreme (climate or weather) event'. "Definitions of thresholds vary … during a specified reference period (generally 1961-1990) are often used". The choice of such a reference time period could be questionable. Could the analysis of extreme meteorological and climate events depend on such a choice? Could the reference period be, for instance, a 30-year moving time period (depending on the availability of the data)? Do the pertinent results of extreme events analysis depend (slightly? moderately? heavily?) on the choice of a reference period? (Mokssit, Abdalah, Direction de la Météorologie Nationale (DMN))	Agreed. In response to several SOD review comments on the relevance of the choice of reference period for the definition of extremes (#310, #316, #317, #318), this aspect is now briefly addressed in Box 3.XXX (under heading "choice of reference period"). In addition, the definition now states: "In some circumstances, information from sources other than observations, such as model projections, can be used as a reference". However, there is no clear answer to this question, since it also includes the notion of adaptation.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
311	3	5	51	5	62	"The IPCC SREX" --> "This report"; "SREX Glossary" --> "see Glossary"; "see IPCC SREX Section" --> "see Section…" …etc. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Was revised accordingly.
312	3	5	51	6	2	I suggest for adding clarity to the chapter, to report the definition of extreme event in a separate Box. (ITALY)	Reject. This material is too central for the chapter to be shifted to a box. However, we have now prepared a new box on the scientific aspects of the definitions of extremes (Box 3.XXX) to separate more clearly the core definition from scientific details that are of less interest to practitioners
313	3	5	51	6	9	Very nice discussion (Webb, Robert, NOAA)	Noted, thanks.
314	3	5	52	5	55	Should the definition also mention 10% chance of occurrence in some way? An event that is judged relative to the 10th or 90th percentile may not be very "extreme", but a good part of the literature deals with "extremes" relative to either the 10th or 90th percentile. A further comment is to suggest that "chance of occurrence" be defined more precisely. For example, when speaking of a 1% chance of occurrence, does this refer to events for which the probability of occurrence is 1% in any given year, or does this refer to an event that has a 1% probability of occurrence at some point during a 30-year period?. As written, the definition suggests the latter. (Zwiers, Francis, Environment Canada)	Agreed. Was revised accordingly.
315	3	5	54	5	54	Please clarify: Is this statistic referring to "chance of occurrence *in any given year* within a specified period"? Otherwise this is referring to events with a 1% chance of occurrence in a 30-year period, i.e. one in 3000 year events. (CANADA)	Agreed. Was clarified.
316	3	5	54	5	55	"Definitions of thresholds vary, but values with less than a 5% or 1% or even lower chance of occurrence during a specified reference period (generally 1961-1990) are often used." Using a threshold that is fixed in time has the potential to greatly magnify the role of the climate change, or at least its importance to society and social effects. For instance, the today's developments do not consider what the weather/climate was 100 years ago. Neither will the developments that take place at the end of the 21st century. So gauging future climate extremes based on today's climate is inappropriate. Instead, a time varying threshold should be employed. While it may be particularly useful from a PR standpoint to say that there is projected to be a 10-fold increase in the number of days that exceed some threshold value from today's climate, in the distribution of daily temperatures in the future climate, that temperature may very be not unusual at all (i.e. not "extreme") and thus the society of the future may not even notice it. Societies adapt to the current climate (or climate of the recent past) not to the climate of 100 years ago. So gauging end of this century's changes against the end of the last century's reference climate does not provide a useful metric. (Knappenberger, Paul, New Hope Environmental Sciences)	Agreed. In response to several SOD review comments on the relevance of the choice of reference period for the definition of extremes (#310, #316, #317, #318), this aspect is now briefly addressed in new Box 3.1. In addition, the definition now states: "In some circumstances, information from sources other than observations, such as model projections, can be used as a reference". However, there is no clear answer to this question, since it also includes the notion of adaptation.
317	3	5	55	5	55	Here and at other places is made a point of 1961-1990 as the usual reference period. Many studies use others, namely 1971-2000, etc, due to data availability or just for favouring a more recent reference period. I think it would be worth to make the point that different reference periods do not affect trends (Aguilar, Enric, Universitat Rovira i Virgili)	Reject. The choice of reference period can affect the trend in some cases (see answers to #310/#316 and #318).
318	3	5	55	5	57	"Absolute thresholds (rather than these relative thresholds based on the range of observed values of a variable) can also be used to identify extreme events (e.g., specific critical temperatures for health impacts)." By the same token, absolute thresholds (e.g. specific critical temperature for human health) are also not time invariant (see Davis et al., 2003, Environmental health Perspectives, for an example of how the threshold for a mortality response to extremely high temperatures changes over time). Assuming they are constant will lead to poor projections for the future. (UNITED STATES OF AMERICA)	Agreed. In response to several SOD review comments on the relevance of the choice of reference period for the definition of extremes (#310, #316, #317, #318), this aspect is now briefly addressed in Box 3.1. In addition, the definition now states: "In some circumstances, information from sources other than observations, such as model projections, can be used as a reference". However, there is no clear answer to this question, since it also includes the notion of adaptation. The suggested reference (Davis et al. 2003) is relevant but not included at this late stage, since other SREX chapters address this issue comprehensively (text refers to Chapers 1,2 and 4).
319	3	5	59	5	59	The statement of what is called an extreme may vary in time contradicts the definition given on line 55, which defines extremes relative to a fixed reference period. Also the reference to adaptation here seems out of place, since the previous sentences describe a meteorological definition of extremes, which does not consider impacts. (CANADA)	Reject. This is an important notion as indicated by comments #310, #316, #317 and #318, and by the material on vulnerability and exposure entailed in this report. There is no contradiction: The text referring to the fixed thresholds refers to the common practice in the scientific literature, while the text mentioning adaptation is assessing an additional dimension that is relevant to practicioners and not considered when fixed thresholds are used. The text included in response to comments #310, #316, #317, #318 in the new box 3.1 addresses this apparent contradiction.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
320	3	5	59	5	60	Droughts and floods are phenomena (e.g., like monsoons), not climate variables. A second comment is to ask whether you consider all droughts and floods, or only extreme droughts and floods. (Zwiers, Francis, Environment Canada)	First comment: Agreed; Revised text to "Some climate extremes". Second comment: Referring to "extreme" droughts and floods would not make sense as these are extreme events by definition.
321	3	5	59	5	60	This sentence seems to contradict lines 11-20 on the same page. Droughts and floods are not examples of extremes in climate variables. They are extreme events (extemes in hydrologic variables) caused by extremes in climate variables. (UNITED STATES OF AMERICA)	Comment is not fully correct, as droughts and floods can be induced by series of moderate events as highlighted in text. However, main issue (referring to "droughts" and "floods" as "climate variables", as also noted in comment #321) has been corrected.
322	3	5	60	5	60	It is a general effect that there are extremes on longer time scales, caused by values of the respective parameters which are not rare or extreme on a short term. In this case, it is the aggregation or duration that may cause the adverse effects. Examples are droughts and several other phenomena like river flooding are caused by the occurrence of a time period with instantaneous or short term values of certain parameters not necessarily extreme. (Ulbrich, Uwe, Freie Universitaet Berlin)	Agreed. This text has been clarified in the revised definition.
323	3	5	61	5	62	The text restricts "compound events" to "two.. events occuring simultaneously". It would be more general to refer to them as involving "more than one event" rather than "two events" (Global Climate Observing System Steering Committee)	Agreed. Replaced "Two" with "Two or more".
324	3	6	4	6	9	It is not clear between extreme weather events and extreme climate events. According to this paragraph could we say a two-week-drought is an extreme weather event while a 30-day-drougth is an extreme climate event? (CHINA)	Noted. The distinction between weather and climate extremes is neither so clear-cut nor really critical. Text has been rephrased to avoid providing this impression.
325	3	6	4	6	9	It is a nice idea to give time scales for weather and climate events. Still, the time scales for weather and climate events are somewhat strange, even though you are using the definition of the time scales of a climate event as given by WMO. E.g., weather conditions such as an omega condition, can last for several weeks in a row. Such a weather condition can lead to extreme events such as the heat wave that hit Europe in summer 2003 or the heat wave that hit Russia in 2010 and lasted over more than a month. And there are many more examples of weather conditions that lasted more than two weeks. I would define weather event as something that lasts shorter than a couple of month or a year. Climate event on the other hand is something that should occur at least for a couple of years in a row, at least if it is caused by climate change. It must not necessarily last for the whole year, but occur regularly over several years. Weather is e.g., if we have a strong winter for one, two, or three years. If we have strong winters for more than 10 years, we can start to think of climate. According to WMO you need a statistical evaluation of time spans of at least 30 years to investigate climate phenomena. See my general comment on chapter 3. (Wurzler, Sabine, LANUV NRW)	Noted. The distinction between weather and climate extremes is neither so clear-cut nor really critical. Text has been rephrased to avoid providing this impression.
326	3	6	9	0	0	Very good merging of WX/CX as a continuum - nicely written. (Prather, Michael, University of California, Irvine)	Noted. Thanks. Text was further rephrased to address comments (e.g. #324, #325). Idea of continuum is retained and main message.
327	3	6	9	6	9	Whereas it is easy to understand that it is much easier to handle with one expression instead of using "weather and climate event" all the time, I have the strong feeling that the choice "climate extremes" as abbreviation is rather unlucky. Many of the phenomena that you are dealing with in this report are weather phenomena / extremes. Why not call it "meteorological extremes"? That can be both. (Wurzler, Sabine, LANUV NRW)	Reject. Only reviewer to comment on this point. Climate being the statistics of weather includes meteorological events, but the reverse is not true. "Meteorological events" is also not appropriate because it only refers to atmospheric processes, while some addressed extremes are linked to surface processes (which are encompassed by the term "climate extremes" but not by "meteorological extremes").
328	3	6	15	6	16	I always thought that in statistics pdf meant probability density function--is this something different? (MacCracken, Michael, Climate Institute)	Correct. We decided to only use the terms "distribution function" and "probability density function (pdf)" in the text for the SOD, but this was an oversight. Thanks for catching this.
329	3	6	15	6	21	It also needs to be mentioned that thresholds can depend on other variables as well. Thus, the importance of temperature thresholds can depend on humidity, which is why we have metrics like heat index, discomfort index, etc. I was very surprised tha the discussion in the chapter did not seem to address this issue. (MacCracken, Michael, Climate Institute)	Noted. This notion was already included in the discussion of compound events (Section 3.1.3), and the combined role of humidity and temperature for health indices was already mentioned in the SOD (previously on page 6, lines 39-41; now in Box 3.1). This text was very slightly expanded to include one additional publication by Sherwood and Huber (PNAS, 2010). This text is now referenced in Section 3.1.3.. Note that the cited refs by Diffenbaugh et al. (2007) and Fischer and Schär (2010) use the heat index, but other indices exist such as that used in Sherwood and Huber (2010). Providing more details would lie beyond the scope of the present chapter.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
330	3	6	18	6	18	"Low probability of occurrence" does not characterize extremes well. For a continuous random variable, every value has a low probability of occurrence. An extreme is something that happens either in the upper or lower tail of the distribution, and has the characteristic that there is a low probability of occurrence of a value that great than or equal to that observed, in the case the upper tail, or less than or equal to that observed, in the case of the lower tail. (Zwiers, Francis, Environment Canada)	Agreed. Replaced with "tails of distribution functions".
331	3	6	20	6	21	This sentence is a bit difficult. Isnt' the point of this sentence simply that society has adapted to a certain climate (including the probabilities of extremes in this climate)? (Brönnimann, Stefan, University of Bern)	Reject. The point is that there exists both absolute and relative thresholds. For "non-extreme" regions, these are often the same, which is what this sentence states. However, in a desert, life-threatening temperatures occur regularly and are thus not extreme in a statistical sense, although they are extreme in an absolute sense.
332	3	6	21	6	21	insert space before degree sign (UNITED STATES OF AMERICA)	Agreed. Was changed.
333	3	6	23	0	0	Francis Zwiers usually refers to these type of indices as "place-based indices", which form an alternative to the large-scale indices of, for example, atmospheric circulation. (Klein Tank, Albert, KNMI)	Reject. Extreme indices are not necessarily tied to a given location by design.
334	3	6	23	6	24	The second part of the sentence (from "which can either be based...") basically repeats what is said on lines 11-13...delete? (Stocker, Thomas, IPCC WGI TSU)	Noted. This was not changed because information seems essential, but reference to section is now given to indicate consistency. The sentence is now in Box 3.1.
335	3	6	23	6	27	It should also be mentioned that indices of climate extremes that one might choose to look at can be functions of location (island versus mountaintop, etc.)--the context really matters. I would also note that many types of impact related issues depend on what seem arbitrary thresholds and when they occur (e.g., in US, as I understand it, tomatoes don't set their fruits unless nighttime temperatures are below 70 F at the appropriate time in their life cycle). (MacCracken, Michael, Climate Institute)	Reject. These notions are already included in the text, though with less detail. Text cannot be expanded due to space limitations. Impact-specific information is provided in Chapter 4.
336	3	6	25	6	27	The SREX definition of extremes should recognize the 10% and 90% thresholds in some way given the frequency with which they are used in the extremes literature. (Zwiers, Francis, Environment Canada)	Agreed. This has been changed. Conversely text on extreme indices now also mentions 1% and 5% thresholds.
337	3	6	26	6	27	Suggest changing to "below the 10th or above the 90th percentile." As written the whole distribution is included. (CANADA)	Agreed. Was corrected.
338	3	6	34	6	35	We had not absorbed this definition on first reading, so we didn't know what an "moderate extreme" was when we reached the term later. We suggest having a box where all the pertinent definitions are visible in a prominent location. (UNITED STATES OF AMERICA)	Agreed. This definition is now included in new Box 3.1 which is referred to when moderate extremes are mentioned. Relevant terms cited elsewhere in the chapter are highlighted in bold face.
339	3	6	35	6	36	Does this mean that you downweight research that is based, for example, on analyses of annual maxima but does not use extreme value theory? If so, that would toss out a lot of material. (Zwiers, Francis, Environment Canada)	No, these studies are also considered. Text was modified to remove possibly appearing value judgment. Revised text: "More extreme “extremes” are often investigated using Extreme Value Theory due to sampling issues (see below)".
340	3	6	36	6	38	Say why this is the case. In fact, I don't think this is exactly true; annual extremes are climate indices, and in applied climatology and hydrology, extreme value theory is applied broadly to annual extremes of many kinds of climate and climate impacts data, such as winds, snowload, streamflow - not just temperature and precipitation. (Zwiers, Francis, Environment Canada)	Agreed in view of more recent literature. Text was nuanced more. (See also answer to #341).
341	3	6	37	6	37	Suggest replacing "rarely" with "sometimes" or "infrequently." (CANADA)	Agreed. Was replaced with "also sometimes".

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
342	3	6	43	6	51	Some references can be added for the block maximum approach: Robinson and Tawn, 1997; Martins and Stedinger, 2000; Katz et al. 2002, Morrison and Smith, 2002; Thompson, 2006; Overeem et al. 2009 Robinson, M. E. and J. A. Tawn. 1997: Statistics for extreme sea currents, Applied Statistics, 46, 183-205 Martins, E. S. and J. R., Stedinger. 2000, Generalised maximum-likelihood generalized extreme-value quantile estimators for hydrologic data, Water Resources Research, 36, 737-744. Katz, R.W., Parlange, M.B. and P. Naveau. 2002: Statistics of extremes in hydrology, Advances in Water Resources, 25, 1287-1304. Morrison J.E. and J. A., Smith. 2002: Stochastic modeling of flood peaks using the generalized extreme value distribution, Water Resources Research, 38, 1305, doi:10.1029/ 2001WR000502. Thompson, C.S. 2006: Decadal climate variability of extreme rainfalls in New Zealand, Weather and Climate, 26, 3-20. Overeem, A., Buishand, T. A. and I. Holleman. 2009: Extreme rainfall statistics from weather radar, Geophysical Research Abstracts, 11, EGU2009-4000-1, EGU General Assembly 2009 Some references can be added for the peaks over threshold approach: Smith, 1989; Davison and Smith, 1990; Claps and Laio, 2003; Nadarajah, 2005; Beguería, and Vicente-Serrano, 2006; Acero et al. 2011 Smith, R. 1989: Extreme value analysis of environmental time series: an application to trend detection in ground-level ozone, Statistical Science, 4, 367- 393. Davison, A. and R. Smith. 1990: Models for exceedances over high thresholds (with discussion), Journal of the Royal Statistical Society, Series B, 52, 393- 442. Claps, P. and F. Laio. (2003): Peaks Over Threshold analysis of flood and rainfall frequency curves, Hydrological Risk: recent advances in peak river flow modelling, prediction and real-time forecasting.Assessment of the impacts of land-use and climate changes, Proceedings of the ESF LESC Exploratory Workshop held at Bologna, Italy, http://www.idrologia.polito.it/~claps/Papers/Claps&Laio_Bologna_2003.pdf Nadarajah, S. 2005: Extremes of daily rainfall in west central Florida, Climatic Change, 69(2), 325-342 Beguería, S. and S. M. Vicente-Serrano. 2006: Mapping the hazard of extreme rainfall by peaks over threshold extreme value analysis and spatial regression techniques, Journal of Applied Meteorology, 45(1), 108-124 Acero, F. J., García, A. G. and M. C. Gallego, 2011: Peaks-over-Threshold study of trends in extreme rainfall over the Iberian Peninsula. Journal of Climate, 24, 1089–1105, doi: 10.1175/2010JCLI3627. (GREECE)	Reject. No space and too late to add suggested references. None of the suggested references appear to provide essentially new material and text was already criticized for being too textbook-like.
343	3	6	43	6	58	Section 3.1.2 The two paragraphs need to be rewritten because: 1) EVT is in general an approach for estimation of extreme values (Coles, 2001), not for the evaluation of changes in extremes. On the contrary, an EV analysis estimated the probability of an extreme event based on the assumption that your data are iid (independent and identically distributed). That is why you would use the "iid-test" as a test for changes in extremes. 2) The use of the word parameters - line 48 - is confusing. Normally one would refer to the block maximum approach and the peak over threshold approach as methods to select the extreme values to be modelled. These values are used to estimate the parameters of the pdf using one of many methods (e.g. probability weighted moments or maximum likelihood). (NORWAY)	1) Agreed. Text was revised accordingly. 2) Reject. The argumentation of the reviewer goes in the same direction as our wording.
344	3	6	45	6	46	Less than once is not really rare, can you define more precisely, e.g., in ch. 1 tail events are considered to be be charaterized rarer than the 10th or 90th percentile of the distributionit. (Mechler, Reinhard, INTERNATIONAL INSTITUTE FOR APPLIED SYSTEMS ANALYSIS)	Agreed. Provided following precision: "i.e. generally less than 1-5% of the considered overall sample".
345	3	6	46	6	48	I would temper this a bit - in theory you can go beyond the support of the data - but in practice, extrapolation into the very far tails produces estimates of very long return period values that are very uncertain. (Zwiers, Francis, Environment Canada)	Agreed. Text is now more nuanced.
346	3	6	51	6	51	Deidda and Puliga (2006) provide a thorough investigation into the sensitivity of the POT method on daily precipitation in Sardinia region and adopt a failure to reject method based on goodness of fit test in order to determine the optimal threshold for their region. The POT method was used in order to choose the threshold of SW Australian five stations by Li et al (2005). The authors evaluated the selected thresholds using a goodness of fit test that consists in choosing first a very small threshold raising it until it is verified at a desired significance level. Deidda R, Puliga M, (2006) Sensitivity of goodness –of-fit statistics to rainfall data rounding off. Phusics and Chemistry of the Earth 31: 1240-1251 Li Y, Cai W, Campbell EP, (2005) Statistical modelling of extreme rainfall in southwest western Australia. J Climate 18 (6): 852-863 (GREECE)	Reject. Cannot include so many additional details at this late stage. Moreover, text was already criticized for being too textbook-like.
347	3	6	53	6	58	I would avoid the term "iid-test", which I think statisticians would find a bit naive. The point is not the test, per se, but the fact that people have been investigating whether records are being set more or less frequently than expected in an unperturbed climate. Note that Benestad is not the only person to have worked on this topic. Others include Meehl et al (2009, GRL, L23701, doi:10.1029/2009GL040736) and Trewin and Vermont (2010, Australian Meteorological and Oceanographic Journal, 60, 113-119) (Zwiers, Francis, Environment Canada)	Agreed. Following this and other comments (#347, #348, #350) as well as several comments suggesting to shorten the textbook-like material of this section, this paragraph was significantly shortened and the "iid-test" is now not anymore explicitly mentioned since it is not further referred to in the rest of chapter. Publications by Benestad (2003, 2006) were retained. Suggested references to Meehl et al. (2009b - already in reference list) and Trewin and Vermont (2010) were added to provide a more balanced evaluation of this topic.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
348	3	6	53	6	58	Is this information on the IID-test essential for the Chapter? IID-test does not come up again in the text, and therefore, this paragraph could be regarded as extra 'text-book' style information that could be removed in order to reduce page length. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Following this and other comments (#347, #348, #350) as well as several comments suggesting to shorten the textbook-like material of this section, this paragraph was significantly shortened and "iid-test" is now not anymore specifically mentioned since it is not further referred to in the rest of chapter. Publications by Benestad (2003, 2006) were retained.
349	3	6	56	6	58	A more comprehensive study to be cited here is: Zorita, E., T. F. Stocker, and H. von Storch, 2008: How unusual is the recent series of warm years? Geophysical Research Letters, 35, doi:10.1029/2008GL036228. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Reference was considered useful to provide more balanced review of this topic.
350	3	6	57	6	57	The statement here is essentially useless--17 out of how many stations? Is this a statistically significant fraction of the stations, etc.? (MacCracken, Michael, Climate Institute)	Agreed. Specific reference was removed because too detailed.
351	3	6	63	6	63	Cross reference the location in the text where different types of drought are defined. (Zwiers, Francis, Environment Canada)	Agreed. Reference to Box 3.3 was added.
352	3	6	63	6	63	These two terms for drought are not defined until later. This is another point in favor of a prominent box of most important definitions. (UNITED STATES OF AMERICA)	Noted. These terms are defined in Box 3.3. This reference is now provided. Specific inclusion of these definitions in the box would be superfluous.
353	3	6	63	6	63	The terms "meteorological drought" and "agricultural drought" have not yet been defined in the chapter, and it would be helpful to refer the reader to Box 3.2. (IPCC WGII TSU)	Agreed. Reference to Box 3.3 was added.
354	3	7	1	7	2	The literature on drought is too large to support the statement that these aspects are rarely examined. (Klein Tank, Albert, KNMI)	Agreed. Text was revised; "rarely" was replaced with "not as frequently".
355	3	7	4	7	5	Suggest "there is no precise definition of an extrem", as this should be the point, rather that to arrive at such a definion being "difficult". For clarity. (SWEDEN)	Agreed. Text was modified as suggested.
356	3	7	9	7	9	Note that in all locations affected by frost, there could be extreme dates of last (in spring) or first (in fall) frosts. (Zwiers, Francis, Environment Canada)	Reject. Too detailed for purpose of present text.
357	3	7	12	7	36	This paragraph is seems to be out of context; it is too specific since it considers only temperature extremes. If it serves as an example for extreme indices considering their duration, I suggest to substantially shorten the paragraph. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Agreed. This text was very significantly shortened, also based on comments #293 and especially #294.
358	3	7	12	7	36	This section of text based on Orlowsky and Seneviratne seems out of place here in the introductory pages (much too detailed), and reads very much like an 'add-on'. The section 3.1.2 seems to reach a concise and clear conclusion given in lines 4-10, and then this additional material appears. (Stocker, Thomas, IPCC WGI TSU)	Agreed. This text was very significantly shortened, see also response to #357.
359	3	7	12	7	36	The paper by Orlowski and Seneviratne is not available yet (and was not refered to in the FOD), so I cannot check the details. However, Alexander et al. describe that definition 1 (used in Frich et al.) and definition 2 have been revised by the ETCCDI. Note also that these indices have been defined for studying changes in extremes in the observations rather than in the model projections of future climate. (Klein Tank, Albert, KNMI)	This text was very significantly shortened following several comments (#293, #294, #357, #358) and the indices are not anymore specifically mentioned.
360	3	7	14	7	14	It is suggested to add a new reference by Ting. (Ting Ding, Qian Weihong, 2011: Geographical Patterns and Temporal Variations of Regional Dry-wet Heat Wave Events in China during 1960-2008. ADVANCES IN ATMOSPHERIC SCIENCES, 28(2), 322-337) (CHINA)	This text was very significantly shortened following several comments (#293, #294, #357, #358) and the indices are not anymore specifically mentioned.
361	3	7	24	7	25	I suggest finding some other way to describe the problem with the HWDI index. Lack of statistical robustness is not the right way to characterize the problem because, in statistics, robustness has a specific technical interpretation that refers to something different from problem that is alluded to here. Maybe it would be sufficient to say that "HWDI does not work well (or is not informative) in climates with low temperature variability, such as in the tropics, where its application can produce many zero values." (Zwiers, Francis, Environment Canada)	This text was very significantly shortened following several comments (#293, #294, #357, #358) and the indices are not anymore specifically mentioned.
362	3	7	24	7	25	What does "statistically robust" mean? Why does the fact that the index can be zero for the present day mean that it is not "statistically robust"? Do the authors mean "normally distributed"? (CANADA)	This text was removed (see response to above five comments).
363	3	7	29	7	30	Need to be careful here - the current SREX definition refers to 5% or 1% (as opposed to 10%). Even if the definition were to be adjusted (which I think should be done), it would be prudent to refer to the 90th percentile as only being "moderately extreme". Also, I don't think that statisticians would agree that the 90th percentile is extreme - so I would remove "in a statistical sense". Rather, we are interested in that level because, in many instances, events beyond the 90th percentile begin to be stressful. (Zwiers, Francis, Environment Canada)	Not relevant anymore because referred text has been removed (see response to above comments).
364	3	7	31	7	33	The choice of a metric does not affect the projected changes. I think the point (and it is a very important one, which should be kept in mind throughout this report) is that the choice of a metric can be pivotal in the perception of impact of projected changes, but both the changes and their impact will be what they will be, regardless of the yardstick an analyst chooses to use for their charaterization. (UNITED STATES OF AMERICA)	Agreed. Although text was significantly reduced (see response to above comments), this point is now clearly noted: "projected patterns (in the magnitude but not the sign) of changes"

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
365	3	7	34	7	34	Please clarify: Would the mean or maximum heat wave duration, as just mentioned, not allow us to assess clustering of hot days? (CANADA)	It depends on the definition of hot days. They would not provide an information on the clustering of days with temperature > 90%ile according to considered period. However, this text is not included anymore because of the significant shortening of this section (see response to above comments)
366	3	7	38	8	21	We shoild also think of the extreme events accerelated by some other reasons than the anthropogenic forcing. For example, when we think of the mega citiy's maximum temperature, we need to count the effect of heatwave caused by the concentration of the population to the mega citiy. Around East Asia, when we estimate the temperarture increase around the Tokyo megacity area with in these hundred years, two by three of these values are coming from the urbanization. This measns that the heatwave disaster would be increased aournd the mega city area cousing by the concentratiuon of the population into that area. We can estimate the ratio of such acceleration by adopting urban canory regional model. Ref: (Takayabu, Izuru, Meteorological Research Institute)	Noted. This point is indirectly addressed in the first sentence ("combination of extreme events with amplifying events or conditions").
367	3	7	40	7	50	The occurrence of blizzards (co-occurrence of heavy snow and high wind) can also be included in the discussion of compound events. (CANADA)	Noted. But example not included because no specific text on blizzard in the rest of the chapter.
368	3	7	40	7	50	It would be insightful to work a health related example in here--for example, Hantavirus in US Southwest involves having wet and then dry summers, etc. (MacCracken, Michael, Climate Institute)	Reject. This would belong in chapter 4.
369	3	7	41	7	41	Why do we need amplification? The combination of a 100-year wind extreme and a 100-year precipitation extreme would, under all circumstances, but be rare and damaging, even without mechanisms that amplify impacts. (Zwiers, Francis, Environment Canada)	Yes, but amplification is simply mentioned as a potential additional effect. Slightly revised first sentence to clarify that compound events can be simply two or more extreme events occurring "simultaneously or successively". Further added that "compound events can include causally unrelated events" in following paragraph.
370	3	7	47	0	0	Some references can be added: Svensson and Jones, 2002; DEFRA, 2005 Svensson, C. and D. A. Jones. 2002: Dependence between extreme sea surge, river flow and precipitation in eastern Britain, International Journal of Climatology, 22, 1149-1168 DEFRA/ Environment Agency Flood and Coastal Defence R& D Programme. 2005, Joint Dependence between extreme sea surge, river flow and precipitation: A study in south and west Britain, R& D Technical Report FD2308/TR3 (GREECE)	Agreed. Suggested Svensson and Jones (2002) reference is specifically on this topic and considered a useful complement to Van den Brink et al. (2005). DEFRA report not included because grey literature.
371	3	7	48	7	49	I have two comments here. First, a specific region is again singled out for no apparent reason. I would advise strongly against this. This has implications for how the report will be read and used (we don't want to be telling Central European policy makers that they are particularly at risk when in fact, I suspect many regions are similarly at risk). Second, if possible, it would be better to talk about the current climate than the future climate. I think Ron Stewart, University of Manitoba (ronald_stewart@Umanitoba.ca), would be able to give examples of observations of extreme heavy precipitation events during drought. (Zwiers, Francis, Environment Canada)	Agreed. Reference to region was replaced with "in some regions".
372	3	7	50	7	50	The comment "(due to fire-induced thunderstorms from pyrocumulus clouds (e.g. Tryhorn et al, 2008))" is incorrect in reference to the Tryhorn paper. The study by Tryhorn et al. 2008 found that the bushfire changed land-surface conditions, including the surface albedo, which helped to induce the thunderstorm initiation - it wasn't from pyrocumulus caused by the fires. (AUSTRALIA)	Agreed, but Tryhorn included several references mentioning role of pyrocumulus clouds. Nonetheless, because example was rather anecdotal, reference was removed.
373	3	7	60	8	2	This is regarding the compound (multiple) events: "More generally, the following causes for a correlation between the occurrence of extremes (or their impacts) can be identified: (1) a common external forcing factor for changing the probability of the two events (e.g., regional warming, change in frequency or intensity of El Niño events)." Question: Could La Niña events also be a major factor contributing to and/or leading to the occurrence of a compound event (at least on a regional scale)? (Mokssit, Abdalah, Direction de la Météorologie Nationale (DMN))	Noted. But does not seem critical for argumentation of this section. El Nino is only provided as example. Hence not added.
374	3	8	0	0	0	Section 3.1.4. Feedbacks: Are there any good/classical papers in the scientific literature that describe and illustrate feedbacks (theory, examples, positive feedbacks, negative feedbacks, etc.)? (Mokssit, Abdalah, Direction de la Météorologie Nationale (DMN))	Reject. Feedback is a well defined notion and does not need to be introduced. Was described in previous iPCC report, and we were already criticized for having too much textbook material. But the term "climate feedback" is included in the SREX glossary and this is now indicated in the text
375	3	8	7	8	7	Suggest replacing "was to see changes in" with "was to induce changes in." (CANADA)	Noted. Sentence was slightly revised to "for a changing climate to induce".
376	3	8	10	8	21	This paragraph seems unnecessarily detailed about methods used for analysing compound events. Suggest deleting lines 11-21, keeping only the first two sentences and adding to the end of the second sentence the phrase: "and to explore appropriate methods of analysis (cite references)." (CANADA)	Agreed. Based on this comment and other comments suggesting significant shortening of text in Section 3.1 (#293, #294), this paragraph was reduced as suggested by reviewer, keeping all previous references. One reference suggested in comment #377 was added.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
377	3	8	15	0	0	After Renard and Lang (2007) the following can be added: Favre et al. (2004) use this approach to perform multivariate hydrological frequency analysis. The Burr-Pareto-Logistic (BPL) copula is applied by de Waal and van Gelder (2005) to capture the dependence structure of wave heights and wave periods measured at White Rose, Canada and estimate their joint distribution. Zhang and Singh (2006) derive bivariate distributions of flood peak and volume, and flood volume and duration, using the copula approach. Genest and Favre (2007) provide a review of inference techniques for copula modelling in a hydrologic setting. Zhang and Singh (2007) use the Gumbel - Houggard Copula for trivariate rainfall frequency analysis. Durante and Salvadori (2010) use copulas to construct multivariate probability distributions. Salvadori and de Michele (2011) suggest several strategies in order to estimate the parameters of the selected copulas, according to different criteria and apply the methodology to flood data. Favre, A.C., El Adlouni, S., Perreault, L., Thiémonge N. and B. Bobée. 2004: Multivariate hydrological frequency analysis using copulas, Water Resources Research, 40, W01101, doi:10.1029/2003WR002456 De Waal, D. J. and P. H. A. J. M van Gelder. 2005: Modelling of extreme wave heights and periods through copulas, Extremes, 8, 345-356 Zhang, L. and V. P. Singh. 2006: Bivariate Flood Frequency Analysis Using the Copula Method, Journal of Hydrologic engineering, ASCE, 11(2), 150-154 Genest, C. and A.C. Favre. 2007: Everything you always wanted to know about copula modelling but were afraid to ask, Journal of Hydrologic Engineering, 12, 347-368 Zhang, L. and V. P. Singh. 2007: Gumbel – Houggard copula for trivariate rainfall frequency analysis, Journal of Hydrologic engineering, ASCE, 12(4), 409-419 Durante, F and and G. Salvadori. 2010: On the construction of multivariate extreme value models via copulas, Environmetrics, 21(2), 143-161 Salvadori, G. and C. de Michele. 2011: Estimating strategies for multiparameter Multivariate Extreme Value copulas, Hydrology and Earth System Sciences, 15, 141-150 (GREECE)	Noted, but can only add absolutely essential references at this stage given lack of additional review round, and paragraph was significantly shortened (see also answer to #376). Decided to only include Durante and Salvadori (2010) which provide a general introduction to the topic of copulas.
378	3	8	21	0	0	It should be mentioned that most extreme value analysis methods have a strong limitation of assuming stationarity. To analyse future extreme, non-stationary methods are currently developed and starts to be used as for example : Kysely et al. (2010), Estimating extremes in climate change simulations using the peaks-over-threshold method with a non-stationary threshold Global and Planetary Change 72 55–68 or Kallache, M., M. Vrac, P. Naveau, and P.-A. Michelangeli (2011), Nonstationary probabilistic downscaling of extreme precipitation, J. Geophys. Res., 116, D05113, doi:10.1029/2010JD014892. (International Petroleum Industry Environmental Conservation Association (IPIECA))	Reject. Non-stationarity is considered in several earlier applications of extreme value analysis methods (see e.g. references cited in Kysely et al. 2010). Because textbook material needs to be shortened rather than expanded (e.g. answers to #293 and #294, and also due to overall space constraints), text cannot be included on this topic.
379	3	8	21	0	0	The following can be added: Coles and Tawn (1991, 1994) and Ledford and Tawn (1996, 1997, 1998) present statistical methods to model the behaviour of data which are simultaneously extreme in all components. Heffernan and Tawn (2004) develop an approach for multivariate extreme value data based on modelling the behaviour of data which are extreme in at least one component. Galiatsatou and Prinos (2008) implement different bivariate extreme value techniques to investigate the structure of a spatial process of storm surges in the Dutch part of the North Sea. Naveau et al. (2009) model pairwise dependence of temporal maxima using multivariate extreme value techniques. Ramos et al. (2009) extend the classical pseudopolar treatment of multivariate extremes to develop an asymptotically motivated representation of extremal dependence that also encompasses asymptotic independence. Cooley et al. (2010) present a new parametric model for the angular measure of a multivariate extreme value distribution, which can describe the extremes of random vectors of dimension greater than two. Coles, S. G. and J. A. Tawn. 1991: Modelling extreme multivariate events, Journal of the Royal Statistical Society, Series B, 53, 377-392 Coles, S. G. and J. A. Tawn. 1994: Statistical methods for multivariate extremes: An application to structural design, Applied Statistics, 43, 1-48 Ledford, A. W. and J. A. Tawn. 1996: Statistics for near independence in multivariate extreme values, Biometrika, 83, 171-183 Ledford, A. W. and J. A. Tawn. 1996: Modelling dependence within joint tail regions, Journal of the Royal Statistical Society, Series B, 59, 475-499 Ledford, A. W. and J. A. Tawn. 1998: Concomitant tail behaviour for extremes, Advances in Applied Probability, 30, 197-215 Heffernan, J. E. and J. A. Tawn. 2004: A conditional approach for multivariate extreme values (with discussion), Journal of the Royal Statistical Society, Series B, 66, 497-546 Galiatsatou, P. and P. Prinos. 2008: Statistical models for bivariate extremal analysis of a spatial process, Journal of Hydraulic Research, 46, Extra issue 2, 257-270 Naveau, P., Guillou, A., Cooley, D. and J. Diebolt. 2009: Modelling pairwise dependence of maxima in space, Biometrika, 96, 1-17 Ramos, A. and A. Ledford. 2009: A new class of models for bivariate joint tails, Journal of the Royal Statistical Society, Series B, 71, 219-241. Cooley, D., Davis, R. A., and P. Naveau. 2010: The pairwise beta: A flexible parametric multivariate model for extremes, Journal of Multivariate Analysis, 101, 2103-2117 (GREECE)	Reject. Cannot add that many details that will not be further reviewed, especially on "textbook" material. Suggested publications do not entail essentially new material.
380	3	8	23	8	62	Maybe somewhere in here the coupling of absolute humidity to temperature could be mentioned here, and the explain how change in heat index can be so important. (MacCracken, Michael, Climate Institute)	Noted. Importance of combined effects of humidity and temperature for health are now briefly noted in the text ("Also impacts on human systems or ecosystems (Chapter 4) can be the results of compound events, e.g. in the case of health-related impacts associated with combined temperature and humidity conditions (Box 3.1)". Box 3.1 provides more references on this topic (see also answer to #329). Specific mention of heat index appears superfluous, various indices are used to this effect.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
381	3	8	30	8	40	It might be better to restate this feeback with more emphasis on sensible heat flux or energy balance, and less on temperature. Temperature does not drive evaporation. Rather, temperature and (possibly) "potential evaporation" are both affected by the evaporation deficit during drought. The present text seems to mistake correlation for causation. Additionally, it is not clear to me how insertion of this scientific insight (and other new ones like it), though it might be the most interesting content to us physical scientists, is relevant to this special report, because the connection is never made (as far as I noticed) to the assessment of trends and projections, which, after all, are in the report only as a prelude to discussion of risk management. (UNITED STATES OF AMERICA)	Noted. Text was revised to clarify. It is correct that at one limit of the feedback no more actual evapotranspiration can take place because soil is too dry and hence only potential evaporation is increased. However, these details cannot be discussed due to space limitations.
382	3	8	30	8	50	The mechanism described acts as a positive feedback on temperature anomalies and soil moisture anomalies. But, it does not explain rainfall changes. The first paragraph says that temperature and drought changes mutually reinforce. The latter paragraph refers to a precipitation index, so presumably it is the hydrological definition of drought which is referred to here. But for the mechanism described to induce hydrological drought the temperature increase or decrease in soil moisture would have to reduce the rainfall. If there is literature demonstrating this, than it should be cited. If not, and this refers to soil moisture drought only, then this should be stated clearly, and studies on precipitation changes should not be discussed here. (CANADA)	No, it is increasing evapotranspiration during heatwaves that is the main driving mechanism for the drying in this feedback loop. Higher air temperature means high vapor deficit, which means higher evapotranspiration demand. A positive feedback loop between soil moisture and precipitation is not necessarily required for the feedback loop between droughts and heatwaves and is quite uncertain based on literature. Added a sentence on this point (citing Koster et al. 2004b and Seneviratne et al. 2010) to clarify, after citation of Clark et al. (2011).
383	3	8	37	8	37	A bit too much "enhancing" here. Suggest you say "the enhancement of soil drying with higher temperature". (t's not clear what an "enhanced" temperature would be. (Zwiers, Francis, Environment Canada)	Noted. Text was rephrased.
384	3	8	37	8	40	This example of a positive feedback seems a little confusing to me. Heating leads to drying of the soil which reduces evapotranspiration which causes an increase in sensible heat flux. Wouldn't an increase in sensible heat flux cool the soil, resulting in a negative feedback? (UNITED STATES OF AMERICA)	Reject. Enhanced sensible heat flux cools the soil but warms the air. The feedback loop is with air temperature not with soil temperature.
385	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)	Do not understand this comment, maybe wrong location.
386	3	8	42	8	50	This information is too detailed and specific to be contained in an introductory section. (Stocker, Thomas, IPCC WGI TSU)	Noted. Most of this text has been now removed to make it shorter.
387	3	8	42	8	50	I would be useful here to comment on the relative magnitude of the uncertainty of this feedback and the shift in location of these transitional regions. Clark et al 2011 (already in reference list) usefully shows that even when uncertainty in the magnitude in global warming is almost eliminated the regional uncertianty remains very large. This point is very important as until we improve the modelling of these processes and the shifting of climate regimes the uncertainty in regional projections will not reduce. (Brown, Simon, The Met Office Hadly Centre)	Agreed. Added reference to Clark et al. (2011) regarding uncertainty of feedback. Also added references regarding uncertainties in soil moisture-precipitation feedback which can also play a role.
388	3	8	45	8	48	The Hirshi et al. (2011) result is presented as if it demonstrates a positive feedback, but I'm not convinced that it does. Perhaps drought days are hotter because there is less cloud cover, or because the lack of rainfall is associated with anticyclonic circulation and subsidence. Even if drought days are associated with dry soil and hence a higher Bowen ratio, that still doesn't demonstrate a feedback. We don't think simultaneous correlation is ever a convincing argument for the existence of a feedback. (UNITED STATES OF AMERICA)	Noted. It is correct that correlation can never prove a causal relationship, but the results are consistent with this hypothesis and do not disprove it. An hypothesis can never be proved, it can only be disproved. Nonetheless, this text was removed because it was not essential to the main argumentation and because of the space limitations.
389	3	8	46	8	48	This is a comment on writing style. One way to help the reader is to avoid all acronyms except those that are used frequently. In this case the acronym itself is visually awkward because it contains a percent sign in an unusual place, and it is only reused once. My preference would be to use names rather than acronyms where possible, and to avoid cluttering text that is heavy going in any case with unnecessary definitions, symbols and acronyms. (Zwiers, Francis, Environment Canada)	Comment is not relevant anymore because text was removed (responses to comments #386 and #388)
390	3	8	52	0	0	Chap 3, page 8, line 52: Regarding feedbacks between trends in snow cover and changes in temperature, we would include that this can also affect flooding. Partly because of the changing snow/rain mix (Knowles et al. 2006, JCLI v. 19, p. 4545), and partly because of the importance of rain-on-snow events for causing flooding in some regions, for example, the western U.S. (McCabe, G. J., M. P. Clark, and L. E. Hay, 2007: Rain-on-snow events in the western United States. Bulletin of the American Meteorological Society, 88, (3) p. 319-328.) (UNITED STATES OF AMERICA)	Reject. Do not see how flooding plays a role in the feedback loop.
391	3	8	53	8	53	"Both these feedbacks" we're not sure what two feedbacks are intended here. (UNITED STATES OF AMERICA)	Agreed. Text was clarified.
392	3	8	58	8	61	The release of GHGs with melting permafrost is not likely to be an extreme event but a more gradual process. Suggest deleting the reference to this issue or, alternatively, if the reference to GHG emissions from melting permafrost is kept, then stating what is known about the probability of this event being abrupt. See U.S. CCSP Assessment of Abrupt Climate Change (http://www.globalchange.gov/publications/reports/scientific-assessments/saps/sap3-4). (CANADA)	Agreed. Removed text on melting permafrost. (see also answer to #396)
393	3	8	58	8	62	Feedbacks not covered in Chapter 3: consider referring to the relevant section in Chapter 4. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Added reference to section 4.2.2.1 on feedbacks between drought, fire and climate change.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
394	3	8	58	8	62	This clarification of scope is appreciated. Could a pointer be given to some other chapter/section of the report that does address feedbacks from extremes to climate? If the authors of this section are the only ones who understand this issue, perhaps they can work with authors of subsequent chapters to treat this issue. (UNITED STATES OF AMERICA)	Added link to section 3.1.7 which does briefly mention potential role of Amazon drought as tipping point. But this topic is not addressed in detail in chapter 4 either. Adding detailed material on this at this late stage would be problematic because of lack of additional review.
395	3	8	60	0	0	frozen can be added before lakes (IRAN, ISLAMIC REPUBLIC OF)	Not relevant anymore because referred text has been removed (see responses to commenst #392 and #396).
396	3	8	60	8	62	Why is melting of permafrost and high-latitude lakes considered an extreme event? Is reduced snowpack an extreme event? Is seasonal change in streamflow an extreme event? (UNITED STATES OF AMERICA)	Agreed. Removed text on melting permafrost. (see also answer to #392)
397	3	9	1	9	40	This very important explanation of confidence and likelihood could be put in a box, making it easier to find and breaking up a long stretch of text. (UNITED STATES OF AMERICA)	Reject. Already a definitional box added. Text is early enough in the chapter to be noted.
398	3	9	3	9	18	I think that the use of the word "assessment(s)" here (quite a number of times) will be confusing given the whole report is an assessment; I would urge ues of the word "evaluation(s)" (MacCracken, Michael, Climate Institute)	Reject. Assessment is commonly used to refer to single assessments in IPCC reports.
399	3	9	8	9	8	replace "assessed" with "provided" (Stocker, Thomas, IPCC WGI TSU)	No. "assessed" works better in this context.
400	3	9	8	9	12	Consider adding a footnote with the information on uncertainty ranges etc. (Stocker, Thomas, IPCC WGI TSU)	Reject. Seems too important to be listed in a footnote.
401	3	9	13	9	15	I am very concerned about the remark in parentheses. I think this seemingly innocent line will confuse readers no end by implicitly creating an equivalence between a likelihood assessment and a confidence assessment, which is counter to the intent of the uncertainties guidance paper. In my view, this statement also casts doubt on all AR4 assessments that used the term “more likely than not”, suggesting, incorrectly, that those assessments were intended to have a different interpretation than is appropriate to the term "more likely than not". If the author team has information and knowledge that they consider to be reliable and that allows an assessment that the probability of an increase, or decrease, is greater than 50%, then they should say “more likely than not” (which is my understanding of how this assessment was applied in the AR4). If the author team cannot assign a probability because there is not suffient information to quantify uncertainty, but has reasonable confidence in the evidence that leads to a conclusion of a change in direction (either observed or projected), then they should say “medium confidence”. If they feel more comfortable with "medium confidence" in some instances where previously, the AR4 assessed "more likely than not", they can of course, give a different assessment in SREX (giving reasons). For example, they may still have the same information about the direction of change, but may now have understood that the quantification of uncertainty is more difficult than previously judged to be the case. However, they should not, after the fact, implicitly change the AR4 assessment by confusing the interpretation of "medium confidence" with that of "more likely than not". (Zwiers, Francis, Environment Canada)	Agreed. After discussion within the author team, it was concurred that the term "more likely than not" can indeed be considered distinct from "medium confidence" (corresponding to cases with high confidence but low signal to noise ratio). These two terms are not equated anymore and the remark in parenthesis was thus removed. The AR4 assessments cannot be directly compared to our assessments since we follow the new IPCC uncertainty guidelines.This point is now also noted. (See also answer to #15).
402	3	9	13	9	15	It is indeed extremely useful to highlight how to compare your assessment of uncertainties with the one from AR4 (medium confidence vs. more likely than not). (Stocker, Thomas, IPCC WGI TSU)	Noted, thanks. But this has been changed following review comments #15 and #401 and chapter discussion at LAM4.
403	3	9	17	0	0	Add a table summarizing the confidence and likelihood of assessments (ITALY)	Reject. Does not seem to add information and this is already provided in Box SPM.3. A reference to this box has been added in the text.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
404	3	9	18	9	20	Views of uncertainty of future climate can be grouped in two fundamentally different classes. According to the mainstream view, espoused by IPCC, projections of future climate depend almost entirely upon physically based mathematical models of the climate system. From this perspective, uncertainty results from incomplete understanding of the physics of climate (processes and mechanisms: atmospheric gas cycles, radiative forcing, clouds - particularly interactions with radiation - aerosol effects, and interactions of atmosphere, ocean, sea ice and land), from technical model limitations (resolution and representation of atmospheric layers), or imperfect climate forcing projections (emissions of forcing agents such as greenhouse gases, solar and land-use changes). In this view, quantification of uncertainty can be done by systematically determining the range of climate changes consistent with the aforementioned uncertainties, based on an ensemble of multi-model versions constructed by varying model parameters (Murphy et al., 2004). This is essentially the logic of the IPCC. However, this logic is akin to a sensitivity analysis rather than a true uncertainty assessment. According to the second view, uncertainty is a structural element of nature and climate that cannot be eliminated by building better models. This has its origin in recent studies of chaotic dynamic systems, where even perfect knowledge of the system dynamics does not enable accurate predictions for the distant future and does not eliminate uncertainty. It thus opposes the view that uncertainty about the future climate is reducible through improved understanding of the climate system (Folland, 2007). The pragmatic aim of the second approach is to characterise, or to quantify the uncertainty in probabilistic terms; to this end, long historic data series are analysed via advanced stochastic methods (Koutsoyiannis et al., 2007, 2008). IPCC states: “The confidence assessments are expert-based evaluations which consider the confidence in the tools and data basis (models, data, proxies) used to assess or project changes in a specific element, and the associated level of understanding.” However, this approach has been criticised as lacking mathematical rigour because the probability density function is not conditioned on measured values of the variables (Rougier, 2007). And certainly, the percent of agreement of imperfect multi-model simulations does not ensure a scientifically rigorous assessment of the validity of the results. The position of Kundzewicz et al. (2009) that future climate projections of GCMs are “plausible projections of possible futures” seems more defensible than assignment of hard-to-defend confidence levels. Uncertainty in the projections of IPCC should be discussed more thoroughly than in	Reject. This view is taken into account in the assessment. The uncertainty the reviewer is referring to is essentially the initial-value problem of the projections, i.e. the range within which uncertainties in projections are dominated by initial conditions or the internal variability of the system. However, for many extremes, the system is dominated by the boundary conditions after a period of a few decades. The distinction between uncertainties due to internal variability, model uncertainty and emission projections is addressed in Section 3.2. and Box 3.1. To clarify this, we also added a sentence on this point in this section: "In addition, uncertainty in projections also varies over different time frames for single extremes, because of varying contributions over time of internal climate variability, model uncertainty and emission scenario uncertainty to the overall uncertainty (Box 3.2 and Section 3.2).".
404.2	3	9	18	9	20	past assessments; moreover, an open debate on uncertainty would improve the credibility of all work related to climate change. An in-depth discussion about the uncertainty in the projected climatic forcing and of its propagation to the status of water resources is overdue, despite some advances in this area (New et al., 2007; Vrugt & Robinson, 2007; Vrugt et al., 2008a,b; Lopez et al., 2009). The exchange of opinions between Koutsoyiannis et al. (2009) and Kundzewicz et al. (2008, 2009) is a step towards remedying that situation. References: Murphy J.M., Sexton D.M.H., Barnett D.H., Jones G.S., Webb M.J., Collins M. & Stainforth D.A. 2004. Quantification of modelling uncertainties in a large ensemble of climate change simulations, Nature, 430(7001): 768-772. Folland C.K., Karl T.R., Christy J.R., Clarke R.A., Gruza G.V., Jouzel J., Mann M.E., Oerlemans J., Salinger M.J. & Wang S.W. 2001: Observed Climate Variability and Change. In: Climate Change 2001: The Scientific Basis. Contribution of Working Group I to the Third Assessment Report of the Intergovernmental Panel on Climate Change. Koutsoyiannis, D., A. Efstratiadis, and K. Georgakakos. 2007. Uncertainty assessment of future hydroclimatic predictions: A comparison of probabilistic and scenario-based approaches, J Hydromet., 8(3): 261-281. Koutsoyiannis, D., A. Efstratiadis, N. Mamassis & A. Christofides. 2008. On the credibility of climate predictions, Hydrol. Sci. J., 53(4): 671–684. Koutsoyiannis, D., Montanari, A., Lins, H. F. & Cohn, T. A. (2009) Climate, hydrology and freshwater: towards an interactive incorporation of hydrological experience into climate research. Discussion of “The implications of projected climate change for freshwater resources and their management” by Kundzewicz et al. (2008). Hydrol. Sci. J. 54(2), 394–405. Kundzewicz, Z. W., Mata, L. J., Arnell, N., Döll, P., Jiménez, B., Miller, K., Oki, T., Şen, Z. & Shiklomanov,I. (2008) The implications of projected climate change for freshwater resources and their management. Hydrol. Sci. J. 53(1), 3–10. New, M., Lopez, A., Dessai, S. & Wilby, R. (2007) Challenges in using probabilistic climate change information for impact assessments: an example from the water sector. Phil. Trans. Roy. Soc. A 365, 2117–2131. Rougier, J. (2007) Probabilistic inference for future climate using an ensemble of climate and model evaluations. Climate Change 81, 247–264. Vrugt, J. A. & Robinson, B. A. (2007) Treatment of uncertainty using ensemble methods: comparison of sequential data assimilation and Bayesian model averaging. Water Resour. Res. 43, W00B09, doi:10.1029/2007WR006720. Vrugt, J. A., ter Braak, C. J. F., Clark, M. P., Hyman, J. M. & Robinson, B. A. (2008a) Treatment of input uncertainty in hydrologic modelling: doing hydrology backward with Markov chain Monte Carlo simulation. Water Resour. Res. 44,	
405	3	9	18	9	31	Might it be helpful to include a specific example to illustrate this terminology? (Brown, Simon, The Met Office Hadly Centre)	Noted. But this is included in the notion of lack of understanding.
406	3	9	21	9	21	“low confidence” Low confidence could be due to the fact that model projections disagree, as is the case for the Sahel where the GFDL model projects future drought but the NCAR model says the opposite. (UNITED STATES OF AMERICA)	Agreed. Added the following sentence: "It should be noted that there are some overlaps between these categories, as for instance the lack of evidence can be at the root of the lack of literature and understanding".
407	3	9	26	9	27	It would be helpful here to explicitly recognize that these three categories (lack of literature, evidence, and understanding) are overlapping to some extent. (IPCC WGII TSU)	Agreed. Has been revised accordingly.
408	3	9	29	9	31	This statement is true (and important to note) for the changes in observations too. (Klein Tank, Albert, KNMI)	Agreed. This will be carefully checked for the final draft. Thanks for picking this up.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
409	3	9	42	0	0	Table 3.1: Is it intended that all of the likelihood/confidence terms used in this table can also be found in the corresponding section of the chapter? We note several instances where this is not the case (Winds-observed changes, Monsoon-observed & attribution, El Nino-observed & attribution, Extreme sea level-attribution, Other impacts-observations (Low confidence in landslide trends). (Stocker, Thomas, IPCC WGI TSU)	Table 3.1 is being modified to beter match important assessments in text.
410	3	9	42	0	0	Table 3.1: Extra-T cyclone projections: What is meant by 'Likely IMPACTS on regional cyclone activity'? This is not particularly useful. Would it not be more useful to include here the more specific information regarding 'medium confidence' in a projected poleward shift? (Stocker, Thomas, IPCC WGI TSU)	Modified.
411	3	9	42	0	0	Table 3.1: Flood attribution: The second part of this entry is not clear - are you also saying that there is 'low confidence' in anthropogenic influence on earlier spring peak? From my reading of page 57, lines 21 - 24, it seems that this attribution statement for earlier spring peak should be 'likely'. (Stocker, Thomas, IPCC WGI TSU)	This assessment has been changed to 'There is high confidence that recent high rate of Arctic coastal erosion due to storms will increase because of sea ice retreat, sea-level rise, and permafrost degradation.' and 3.5.7 is cited as the source
412	3	9	42	0	0	Table 3.1: Extreme sea level projections: Not clear where the 'very likely' projection for sea ice retreat has come from. This is not an assessment that has came out of chapter 3. Also, if you are to include this statement on Arctic coastal erosion here, then you need to add 3.5.7 to the first column as this is the corresponding section where this information is assessed. (Stocker, Thomas, IPCC WGI TSU)	Noted. Shortened text slightly by merging the two first paragraphs. Other reviewers did not complain on length of text within this section, hence maybe only an issue for this reviewer.
413	3	9	47	10	24	We don't think it's necessary to spend so much space explaining why certain methods might be problematic. Just mention this concept as one consideration, with reservation, in support of or against a given trend when it seems appropriate later. (UNITED STATES OF AMERICA)	Agreed. Replaced skewness with shape.
414	3	9	49	9	49	I suggest that this sentence talk about changes in the mean, variance and shape of probability distributions. Skewness is a technical term that refers to the 3rd moment of a probability distribution; higher order moments, which also affect the shape of the distribution, might also change. (Zwiers, Francis, Environment Canada)	Such a figure has been provided to Chapter 1. Reference to that figure has been added.
415	3	9	49	9	49	Suggest adding a Figure to illustrate this point (about extremes changing with changes in the mean, variance or skewness of a probability distribution) as has been done in earlier IPCC reports. (CANADA)	Agreed. This was changed. Same change was also done in 3rd sentence. (See also answer to comment #419).
416	3	9	49	9	57	We suggest replacing "caused by changes in the mean.." with "linked to changes in the mean…", as there is no phyical causality implyed here. (BELGIUM)	Yes, but we don't think that the text appears to imply this.
417	3	9	49	9	57	It is not clear from the text whether this is about applying the observed relationship between mean and variance to future projections, but of course this all depends on the mechanisms causing the change. Even if the variance remains unchanged in the recent observations this may be different in the future. (Klein Tank, Albert, KNMI)	Reject. We do include discussion of physical aspects elsewhere (feedbacks, section 3.1.4) and this framework can also be used to assess driving mechanisms; these two aspects (statistics vs physics) are not mutually exclusive. Section 3.1.4 is also referred to in this section.
418	3	9	49	9	57	This statistical view of a climate variable is a helpful and powerful way of looking at a climate variable and is, I think rightly so, the dominant view of this chapter. However it is not very physical, in that it does not look at the problem in terms of the physical processes that are driving/causing the extremes and any changes that might occur. This second view is not explored as often as it should be, and unless it is our progress in improving our confidence in future projections will be slow. So I would request that some comment that more physics process based approaches are needed. This comment could also apply to the following paragraph (Brown, Simon, The Met Office Hadly Centre)	Agreed. This was reworded (see also answer #416).
419	3	9	49	9	57	The opening line suggests that changes in extremes are caused by changes in mean, variance, etc, but this is not a cause. It is merely one facet of this but not a physical cause. Line 53 makes the same error in expression. (UNITED STATES OF AMERICA)	Agreed. Provided text on this point in the new Box 3.1 (which resulted from several review comments on related points).
420	3	9	49	9	57	Again, this applies largely to extremes that are defined to be static in time. If extremes are based on the probability distribution from which they are drawn, then a simple change in the mean (and keeping the same distribution) would produce no change in extremes at all. If however, the variance decreased, then, of course, the occurrence of extremes as defined by the distribution from which they were drawn would decline—even if there was a greater number that exceeded some threshold defined by a different population. (UNITED STATES OF AMERICA)	Agreed. Text was reworded and no causality is now implied. (See answers to comments #416 and #419).
421	3	9	52	9	57	I do not agree with the formulation that changes in extremes were mainly caused by changes in the mean. To my vision, both changes (mean and extremes) are inherently coupled with each other by physical causes. A statistical property, however, could never be presented as an explanation. (BELGIUM)	Agreed. Two publications appear relevant because they provide global perspective on this issue. References were added.
422	3	9	57	9	57	You might also cite Hegerl et al (2004, J. Clim, 17, 3683-3700) who show that simulated changes in extremes do not follow the annual cycle everywhere. See also Kharin et al (2007). These papers might also be included in the list that begins at the end of line 62 of this page. (Zwiers, Francis, Environment Canada)	Agreed. Publication is global. Reference was added.
423	3	9	59	10	4	Clark et al 2006 figs 2 & 3 demonstrate very clearly the complex alterations to the daily temperature distributions that are modeled for the future, with daily temperture distributions for some regions moving from qasi-normal to almost becoming bimodal. (Brown, Simon, The Met Office Hadly Centre)	Agreed. Level of confidence is assessed as high. To substantiate this assessment, additional references documenting global analyses of scaling between changes in mean and extremes have been added (answers to #422 and #423).
424	3	9	60	9	62	The author team might consider characterizing this statement with a level of confidence. (IPCC WGII TSU)	Agreed. Urban locations were mentioned for the observed changes, but global analyses are not available.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
425	3	10	2	10	2	The reference to urban locations seems out of place. Climate models do not generally simulate changes in extremes at urban locations. Supporting references are not seen for this statement. (CANADA)	Agreed. Added publications by Hegerl et al. (2004) and Kharin et al. (2007) which also address changes in precipitation extremes. No publications on scaling of wind extremes are available to our knowledge.
426	3	10	3	10	3	The references in the paragraph concern only the beheviour of extreme temperatures and none for precipitation or wind extremes. It is suggested to add some references about these parameters. (GREECE)	Reject. Studies are cited in reference to 2° target. Sentence was reworded to clarify the content of these studies.
427	3	10	6	10	8	Suggest deleting text beginning with words "since several studies…and ending with references to papers by Allen et al., and Meinshausen et al." This text is unnecessary to the sentence and the specific references provided here are inappropriate since the question about how other impacts, including changes in extremes, scale with changes in global mean temperature is a very fundamental question that predates these recent papers. (CANADA)	Noted. Text was revised to clarify its purpose.
428	3	10	6	10	24	Section 3.1.6. Changes in Extremes and Their Relationship to Changes in (Regional and Global) Mean Climate: The paragraph on the scaling needs to be more clarified. One could make clear the concept of the scaling. Definition? What does it mean when (a) the scaling is near to 1? (b) the scaling is larger than 1? and (c) the scaling is lower than 1? (Mokssit, Abdalah, Direction de la Météorologie Nationale (DMN))	Noted. Indeed very relevant study, however only based on one model. Reference to Clark et al. (2011) was added.
429	3	10	6	10	24	Clark et al 2011 specifically looks at this issue of global tempearture targets affecting regional extremes (or not) so should at least be referenced but probably should be quoted. In addition, this paragraph needs to comment on the large uncertainty that still remians even when a global warming target is set which again Clark et al. 2011 also directly addresses. (Brown, Simon, The Met Office Hadly Centre)	Text was indeed unclear (see also answer to #427). It was reworded.
430	3	10	7	10	7	I'd belive that the studies looking specifically to a two-degree global warming are still quite few, not least those that extend to analyses of extremes. The two-degree target is politically (and scientifically) interesting, and if that is the point being made, then the wording should be adjusted. (SWEDEN)	Figure has been removed because not considered essential for report and text now refers to source paper where all analyses (annual and seasonal) are available.
431	3	10	9	10	9	Although it says here that Figure 3.1 provides scaling "on annual and seasonal" time scales, Figure 3.1 only provides seasonal time scale information. (IPCC WGII TSU)	Yes, this is correct. Was revised accordingly.
432	3	10	17	10	17	Presumably you mean "lack of" consistent "scaling", not "lack of scaling" (i.e., for a given location, the scaling factor varies with the percentile). (Zwiers, Francis, Environment Canada)	Yes, thanks. Was corrected.
433	3	10	18	10	18	"as also" should be "has also" (Lobell, David, Stanford University)	Yes, thanks. Was corrected.
434	3	10	18	10	18	as -> has (Brönnimann, Stefan, University of Bern)	Following feedback from SOD review, it was decided to give less emphasis to this point, which is rather anecdotal. Accordingly, it is not highlighted in bold face anymore.
435	3	10	18	10	20	Having an important finding in bold in the middle of a long paragraph can be very difficult for a reader to come to understand. It seems to me this is just the place to have a discussion of the proverbial Gaussian distribution and the ways in which changes in the Gaussian could affect the occurrence of extremes. I would also note that the particular phrasing here needs to indicate that the possibility of (at least significant) cooling goes down as global average temperature goes up. (MacCracken, Michael, Climate Institute)	Noted. Sentence was modified as suggested in comment #437.
436	3	10	20	10	22	"It has for instance been recently suggested that the decrease in sea ice induced by the mean warming could induce more frequent cold winter extremes over northern continents (Petoukhov and Semenov, 2010)." While this has been suggested, most observations do not bear this out (e.g., Frauenfeld and Davis, 2003; Rohli, 2005; Wrona and Rohli, 2006; Compo et al., 2011). (UNITED STATES OF AMERICA)	Agreed. Text was revised as suggested by reviewer.
437	3	10	21	10	22	There is indeed in Petoukhov and Semenov paper some discussion concerning change of probability of extreme cold winter due to sea-ice melting (probability for the February surface air temperature to be less than –1.5 standard deviation). To account for the specific conditions for which this occurs, I propose to add ", but not systematically," after "could induce". This indeed only occurs when sea-ice cover in Kara and Barents Seas is reduced by a percentage that is comprised between 40 and 80%. The response (obtained through sensitivity experiments performed with one atmospheric climate model) is the reverse for lower or higher decrease of sea-ice, revealing the response to be very non-linear as noted by the authors. The present wording is a little misleading on the exact outcome of the study since it could be interpreted as the possibility of a single direct link between sea ice decrease and increased extreme cold winter occurrence (even with the use of conditional). (PLANTON, Serge, Méto-France)	Noted. This is mostly true, but study by Petoukhov and Semenov suggests one contrary example, although debated. Also possible modification of MOC would be another example though not mentioned here. Hence clarification was not considered necessary.
438	3	10	22	10	24	This paragraph mostly concerns how changes in temperature extremes in individual locations induced by large scale anthropogenic forcing scale with the global mean. But the last two sentences describe an observed instance where one region cooled, and refers to mechanisms. The cited papers have not been read, but it is suspected that these mechanisms relate to other regionally important forcings which have cooled North America, or internal climate variability. That internal climate variability and small-scale responses to local forcings are superposed on the large-scale response to anthropogenic forcings is a separate issue from that of physical mechanisms which will give rise to larger/smaller changes in extremes in some regions as a result of large-scale anthropogenic forcing. (CANADA)	Noted. In principle, this is not dependent on the time frame, but is most relevant to the recent and coming decades.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
439	3	10	22	10	24	It would helpful to indicate the relevant time frame for the described cooling trends. (IPCC WGII TSU)	Reference appears relevant to material of chapter but is now cited in Section 3.1.4. where role of feedbacks is illustrated. A bit too detailed for purpose of Section 3.1.6. (See also answer to #440).
440	3	10	24	0	0	Chap 3, page 10, line 24: At the end of this sentence, we would suggest adding something like "In some regions, increasing irrigation is known to play a role (Lobell, D. B., C. J. Bonfils, L. M. Kueppers, and M. A. Snyder, 2008: Irrigation cooling effect on temperature and heat index extremes. Geophysical Research Letters, v. 35 (9) L09705). (UNITED STATES OF AMERICA)	Agreed. Sentence was deleted because mechanisms are described in 3.3.1.
441	3	10	24	10	24	Either delete this final sentence, or expand to briefly list what these proposed mechanisms are. (Stocker, Thomas, IPCC WGI TSU)	Noted. Publication has been accepted.
442	3	10	26	0	0	Figure 3.1: PLEASE NOTE: Orlowsky and Seneviratne 2011 must be accepted for publication by May 31 2011 for these new, previously unpublished results to remain included and assessed within SREX. (Stocker, Thomas, IPCC WGI TSU)	Figure was removed.
443	3	10	27	10	30	The units in the figure just represent a ratio? This would benefit from a clearer explanation, such as in lines 8-13. (UNITED KINGDOM OF GREAT BRITAIN AND NORTHERN IRELAND)	Figure was removed.
444	3	10	27	10	30	This caption is written too tersely--for those who come to this chapter after reading the SPM, the caption is packed way too tightly. Start with a simple opening sentence without so many interjections. (MacCracken, Michael, Climate Institute)	Figure was removed.
445	3	10	28	10	28	Please don't use shorthand like "10%tile" - this saves very little space, but does make the caption more difficult to read. (Zwiers, Francis, Environment Canada)	Figure was removed.
446	3	10	28	10	29	%ile is cute but odd symbolization spell out the word' percentile' (Webb, Robert, NOAA)	Agreed. Link between the glossary term "climate threshold" and the terms "tipping point" and "critical threshold" has been clarified. "Critical transition" did not seem to need a clarification. The glossary term "abrupt climate change" is now included in the discussion as well as in the title of this section (which deals with a broader topic than only the concept of "surprises").
447	3	10	32	0	0	Section 3.1.7. It would be helpful here to clarify the relationship between "climate threshold," which appears in the glossary, and other similar terms used (tipping point, critical transition, critical threshold). Additionally, the glossary term "abrupt climate change" should be explicitly included in this discussion. This section should also consider consistency with sections 1.2.4 and 8.4.3. (IPCC WGII TSU)	Do not understand this comment, since the wording of this section is very careful. However, revised text additionally notes that confidence in these projections is low (last sentence), and text has been revised to further highlight uncertainties in the literature.
448	3	10	32	11	2	There is an issue of balance in this section. Physical climate models show little if any evidence of tipping points and etc. The transitions are usually smooth with noise (natural variability) set on top of the changes. If the issue of natural variability is what is in view (it can be unclear that one is approaching a tipping point - storm surge over a sea wall, as an example), then the discussion needs to be much clearer. (UNITED STATES OF AMERICA)	Reject. It is an important topic, especially for extremes and disasters. Was little addressed in previous reports. New literature on this was recently published (Lenton et al. 2008, Scheffer et al. 2009).
449	3	10	32	11	2	We don't think this topic needs its own section. These ideas can be mentioned in a few lines when discussing low confidence, lack of literature, lack of evidence, lack of understanding. One can even think of a category below "low confidence" where the change itself has not yet been hypothesized. It would fit very naturally in section 3.1.5. (UNITED STATES OF AMERICA)	Reject. The European 2003 event is a good example of an event induced by a tipping point (soil moisture limitation leading to a shift of climate regime).
450	3	10	34	10	36	Why should surprises only be associated with threshold crossing behaviour? I think it would be helpful to consider "surprises" in a more general setting than in the context of "tipping points" (essentially non-reversible hysteresis behaviour, about which we can only speculate). By any measure, the European 2003 heat wave would have been considered a surprise, but we don't think we associate it with a "tipping point". (Zwiers, Francis, Environment Canada)	Noted. Confidence in this sentence is difficult to evaluate, is likely medium to high given very careful wording ("cannot be excluded"), but is not really meaningul, hence not provided. However, it is important to note that confidence in projections of mentioned tipping points is low (now mentioned in last sentence of section). Nonetheless, as noted in section 3.1.5, low confidence in the potential occurrence of an extreme does not mean that it has a low change of occurrence, only that the understanding is poor. This is why this section is important.
451	3	10	34	10	36	It would be helpful to indicate, if possible, a level of confidence or evidence/agreement summary terms to characterize the conclusion that "the possible future occurrence of low-probability high-impact scenarios…cannot be excluded…" (IPCC WGII TSU)	Agreed. Assessment is now provided in last sentence (low confidence).
452	3	10	34	11	2	I'm concerned about including this material without providing an assessment. The text comes close to an assessment in the sentence that begins on line 61 of page 10, but then vasilates by also including the following sentence about possible early warming "signs", whatever they might be. I think the authors should make an assessment, even if that their assessment were to say that no assessment is possible because of limited evidence and low agreement. (Zwiers, Francis, Environment Canada)	Noted but reviewer does not provide literature that could be used for more balanced assessment. To provide more careful balance, the last sentence now notes that the confidence in these projections is low, and some recent literature that showed some contradictory results on given potential tipping points has been added.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
453	3	10	46	11	2	This review of tipping points is not critical enough. Climate model experiments invariably show, for example, that Arctic sea ice recovers if the forcing is reduced, and the same for the THC. While agreement exists that it is hard to completely rule out the existence of such tipping points, the evidence from climate models is that few if any exist in the climate system, and in most cases responses to forcings are linear. A more balanced assessment should be made, including citations of studies which look for tipping points and find none. (CANADA)	Agreed. This distinction was removed. Reversibility is not mentioned anymore for the two first cases.
454	3	10	49	10	50	Is it well documented that the first mechanism gives a "potentially reversible" and the second "reversible" change? (SWEDEN)	Text was simplified and irreversibility is only mentioned now for the case of the critical bifurcation.
455	3	10	50	10	50	I don't understand the "reversible" here--don't you mean "irreversible"--that is, one can cross it and not be able to go back (like starting melting of an ice sheet, etc. (MacCracken, Michael, Climate Institute)	Noted that term was confusing for readers and was removed. Now only mentions irreversibility.
456	3	10	52	10	52	What is "temporally" irreversible? Does this refer to a longish timescale? (SWEDEN)	Agreed. Reference to AR4 (Meehl et al. 2007b) was added in paragraph.
457	3	10	53	10	53	Chapter 10 AR4 to be cited here , ie, (Meehl et al., 2007). (Stocker, Thomas, IPCC WGI TSU)	Agreed, was modified.
458	3	10	53	10	53	It would be preferable to use the phrase "low-probability high-impact" OR "high-risk" instead of "low-probability high-risks." (IPCC WGII TSU)	Text was revised and full list is not given anymore, only those related to extreme events.
459	3	10	55	10	58	Is this list of the "Lenton et al. Tipping Elements" really needed given that only few are mentioned again just below? Suggest to delete. (Stocker, Thomas, IPCC WGI TSU)	Noted, but this is only citing this specific publication.
460	3	10	58	10	58	Good idea to mention the Sahel as one of the tipping elements of the ECS. (Mata, Luis Jose , IMF)	Agreed. Text was modified as suggested.
461	3	10	61	10	61	can be inferred from' - would be better as 'as suggested by'... (Stocker, Thomas, IPCC WGI TSU)	Agreed. Was revised accordingly.
462	3	10	62	10	62	It would be preferable to use the phrase "low-probability high-impact" OR "high-risk" instead of "high-risk low-probability." (IPCC WGII TSU)	Thanks for useful reference. Was added.
463	3	10	62	11	1	"There is often a lack of agreement between models regarding these high-risk low-probability scenarios, for instance regarding a possible die-back of the Amazon rainforest (Friedlingstein et al., 2006) or the risk of an actual shutdown of the Atlantic thermohaline circulation (e.g., Lenton et al., 2008)." You should add, "or the risk of an ice-free summer Arctic." See Tietsche, S., et al., (2011, Recovery mechanisms of Arctic summer sea ice. Geophysical Research Letters, 38, doi:10.1029/2010GL045698) (UNITED STATES OF AMERICA)	Agreed. Reference was added.
464	3	10	63	11	1	Could cite study by Rahmstorf et al. 2005 when referring to hysteresis behaviour (tipping points) and the potential shutdown of THC (Rahmstorf, S., M. Crucifix, A. Ganopolski, H. Goosse, I. Kamenkovich, R. Knutti, G. Lohmann, R. Marsh, L. A. Mysak, Z. Wang, A. J. Weaver, 2005, Thermohaline circulation hysteresis: a model intercomparison, Geophysical Research Letters, 32, L23605, doi:10.1029/2005GL023655) (Stocker, Thomas, IPCC WGI TSU)	Last sentence removed following comments #466 and #467.
465	3	11	1	11	1	It would be preferable to more explicitly describe the mentioned "early signs." (IPCC WGII TSU)	Agreed. Last sentence removed.
466	3	11	1	11	2	The information provided in the last sentence as such seems small. Suggestion omission, or developing the point that is to be made. (SWEDEN)	Agreed. Last sentence removed.
467	3	11	1	11	2	What are these early signs? Either delete this final sentence, or expand to be more informative. (Stocker, Thomas, IPCC WGI TSU)	Comment is unclear as to what is being asked for here. Declined.
468	3	11	5	0	0	Some difficulties, lack of geographic and temporal data, lack of homogeneity, incertitude, could be stressed in this in this 32. Practical applications of recommended methods seem difficult: when the spatio-temporal scale is precise, the conclusions are less robust; and this is the case for the extremes, which enforces the sense of fragility of conclusions. (BOURRELIER, PAUL-HENRI, AFPCN)	Thanks for the comment, the new reference doesn't add sufficiently to the conclusions to be included at this stage.
469	3	11	5	0	0	In the context of homogeneity tests, I can recommend: J. B. Wijngaard, A. M. G. Klein Tank and G. P. Können, Homogneity of 20th Century European daily temperature and precipitation series, Int. J. Climatol. 23 (2003) 679–692. In that paper the authors have presented a classification scheme according to the quality and homogeneity of time series. (BELGIUM)	thanks for the comment, the new reference doesn't add sufficiently to the conclusions to be included at this stage.
470	3	11	5	19	50	In the context of homogeneity tests, we would like to suggest the following reference : J. B. Wijngaard, A. M. G. Klein Tank and G. P. Können, Homogneity of 20th Century European daily temperature and precipitation series, Int. J. Climatol. 23 (2003) 679–692. In that paper the authors have presented a classification scheme according to the quality and homogeneity of time series. (BELGIUM)	This depends on the variable being analyzed, variance etc. so a general statement would not work.
471	3	11	7	0	0	Section 3.2.1: I miss a statement about the required minimum record length of the time series to separate between natural variability and trends related to climate change. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Agree, added text.
472	3	11	10	11	10	Suggest to add here: "...published mainly since the AR4 and building on the AR4 assessment" (Stocker, Thomas, IPCC WGI TSU)	We have shortened this section already and prefer to keep it as is. It provides context for most of subsequent discussion of observed changes.
473	3	11	12	0	0	One can argue that these issues need not be discussed here at length (to shorten the chapter), because the reader will trust that they are used for expressing confidence levels (as explained). (Klein Tank, Albert, KNMI)	means both, either data exist but are not available (not digitital or digital but country won't release etc. Or the observations were never taken. Discussed in more detail further down in text.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
474	3	11	14	11	14	The phrase "data availability" is ambiguous--does this mean that access cannot be gotten to observations that were taken, or that observations are simply not available because they were not taken. Please clearify for this gives an indication of what the prospects might be for improving the situation in the next assessment. (MacCracken, Michael, Climate Institute)	Extreme Value Analysis is discussed later in text. This just acknowledges that analysis results are influenced by technnique, no mention of the exact technique. Further, the new reference doesn't add sufficiently to the conclusions to be included at this stage.
475	3	11	14	11	17	These lines seem unbalanced as they do not recognise the recent advances in detecting changes in extremes that extreme value analysis has brought. There are a number of references that could be cited (Brown 08, Stott 04, Min 2011) but Christidis et al 2011b comment on the improved detctability of changes in daily Tmax that extreme value analysis provides compared to more traditional methods. This Christidis 2011b is not yet in the reference list - "The role of human activity in the recent warming of extremely warm daytime temperatures" Nikolaos Christidis, Peter A. Stott, Simon J. Brown (http://journals.ametsoc.org/doi/pdf/10.1175/2011JCLI4150.1) (Brown, Simon, The Met Office Hadly Centre)	Made changes for more balance. But Alexander et al is just used as an example here. It is not singled out for criticism.
476	3	11	22	11	38	Need to be careful here to make sure that limitations in datasets are discussed in a balanced way. For example, in lines 30-31, it is said that precipitation stations used in Alexander et al (2006) are not spatially uniform. It is not clear what is meant by "not spatially uniform", but I assume that this means that the underlying station data provide non-uniform spatial coverage. This is an obvious problem with all insitu datasets, and also with some remotely sensed data sets, so singling out Alexander et al seems a bit unfair, and raises questions about why they are being singled out for criticism. (Zwiers, Francis, Environment Canada)	This section discusses instrumental data issues, not models, declined.
477	3	11	24	11	24	What is the highest resolution models can provide a reliable description of local details, given gird box size and time step? The concept of 'skilful scale' needs to be discussed, as it is well-known that models are not able to provide a realistic description on the grid-box level (due to smoothed orography, parameterisation schemes, discretisation and numerical algorithms). (Skillful scale may for instance by of the ~8x8 grid boxes) (NORWAY)	Not sure what is meant here, and not necessarily true. Declined.
478	3	11	26	0	0	Observed time seris from the 1970 - plainly state that these are too short to detect many forms of climate change. (UNITED STATES OF AMERICA)	So noted, no action requested.
479	3	11	31	0	0	Note that strictly speaking Alexander et al is not based on a "global daily dataset", because only the derived indices have been collated not the daily data. The underlying daily data are not available (mainly because of data policy issues). (Klein Tank, Albert, KNMI)	So noted, added some text for clarification.
480	3	11	31	11	35	The text here refers to data sets of 'studies' by Alexander et al. and by Vose et al. If the reason for discussing these studies is that data sets presented by these authors are commonly used by others, then that should be made clear here. (CANADA)	Gives example of other user.
481	3	11	33	0	0	The "used e.g. in Brown et al" does not seem needed. Examples of "commonly used" are not given above in conjunction of other datasets. If Brown et al. has pointed to data gaps in Caesar et al., please reword. (SWEDEN)	Thanks for the comment, the new reference doesn't add sufficiently to the conclusions to be included at this stage.
482	3	11	34	11	35	It is suggested that at the end of "...data gaps in South America, Africa and India", add words "also, the study by QIAN et al. (2011) has data gaps in China."(Qian Weihong, Shan Xiaolong, Zhu Yafen, 2011: Ranking Regional Drought Events in China for 1960-2009. ADVANCES IN ATMOSPHERIC SCIENCES, 28(2), 310-321) (CHINA)	This section discusses instrumental data issues, declined.
483	3	11	40	11	42	Again, paleo/proxy data provide many clues for the assessment of stationarity/non-stationarity issues. (Jarrett, Robert, USGS)	So noted, added brief text in first sentence of paragraph.
484	3	11	40	11	54	It would be useful to mention that homogeneity adjustments appear to have no discernable effect on large area means - e.g., cite papers by Jones and colleagues, or recently for the US, Menne et al (2010, JGR,115, D11108, doi:10.1029/2009JD013094) (Zwiers, Francis, Environment Canada)	They have been assessed, but little done about inhomogeneous data.
485	3	11	40	11	54	This paragraph gives the impression that datasets analyses do not care about homogeneity, due to the difficulties of daily data techniques. I think it is worth mentioning that in most of the references quoted (at least those related to indices) data has undergone a homogeneity assessment)c'd) (Aguilar, Enric, Universitat Rovira i Virgili)	unclear what comment is asking. Appears to be continuation of previous comment already addressed.
486	3	11	40	11	54	(c'd) either by using relative, absolute or visual tests over monthly values or the same indices. Peterson et al 2002 (page 98, line 52) or (Aguilar, Enric, Universitat Rovira i Virgili)	unclear what comment is asking.
487	3	11	40	11	54	(c'd) or Aguilar et al. 2009, 2005 (page 74, line 12) are examples of this. This and other articles related to ETCDDI activities (Aguilar, Enric, Universitat Rovira i Virgili)	unclear what comment is asking.
488	3	11	40	11	54	(c'd)assess homogeneity of the data. (Aguilar, Enric, Universitat Rovira i Virgili)	Thanks for the comment, the new reference doesn't add sufficiently to the conclusions to be included at this stage.
489	3	11	43	11	44	An another example of small buildings and vegetation substantially affecting meteorological measurements is given for rainfall measurements at several Australian gauges in Lavery, B., Kariko, A. and Nicholls, N., 1992. A historical rainfall data set for Australia, Aust. Met. Mag. 40, 33-39. (AUSTRALIA)	Agree
490	3	11	45	11	45	Delete "slight" - the jump can be more than "slight" (see for example, Menne et al, 2010, JGR, 115, D11108, doi:10.1029/2009JD013094) (Zwiers, Francis, Environment Canada)	revised.
491	3	11	48	11	48	Both references refer to daily data, not sub-daily. Provide a reference or omit. (Brönnimann, Stefan, University of Bern)	Thanks for the comment, the new reference doesn't add sufficiently to the conclusions to be included at this stage.
492	3	11	49	0	0	More recent papers on homogenization of daily data exist, e.g. as a result of the COST Homogenization action in Europe, see http://www.homogenisation.org/References.php (Klein Tank, Albert, KNMI)	Thanks for the comment, the new reference doesn't add sufficiently to the conclusions to be included at this stage.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
493	3	11	49	11	49	"Wang et al. 2010" should be added right after "Della-Marta and Wanner, 2006", because this is a very recent study on homogenization and adjustment of daily data (see my comment #15). (Wang, Xiaolan, Environmen Canada)	unclear what comment is asking.
494	3	11	56	11	57	Suggest (?) "Observed tornado occurrence since 1950", as the point is more that there are more observations rather than more tornadoes. (SWEDEN)	true for later period, but not over the long term.
495	3	11	56	11	58	"Tornado occurrence since 1950 in the USA, for instance, displays an increasing trend that mainly reflects increased population density and increased numbers of people in remote areas (Trenberth et al., 2007; Kunkel et al., 2008)." Also changes in observing technology such as Doppler radar and damage assessments have played a large role in the apparent increase. (see NCDC, http://www.ncdc.noaa.gov/oa/climate/severeweather/tornadoes.html) (UNITED STATES OF AMERICA)	Doesn't need to be comprehensive.
496	3	12	1	12	8	This treatment of reanalysis in this paragraph neither as comprehensive nor as up-to-date as it might be. (Global Climate Observing System Steering Committee)	Thanks for the comment, the new reference doesn't add sufficiently to the conclusions to be included at this stage.
497	3	12	1	12	8	Allan et al 2009 go back to 1920 and although this is a regional study it does show low frequency storm variabiltiy is large and that more recent studies on storm chagnges should take this variabiltiy into account. (Brown, Simon, The Met Office Hadly Centre)	Reject. Sentence is an accurate and succinct description of a reanalysis.
498	3	12	3	12	3	The terminology "model-based" should be avoided when referring to reanalyses, as one could equally refer to them as "observation-based". Maybe "reanalyses based on data assimilation" would be better. (Global Climate Observing System Steering Committee)	Thanks for the comment, not peer reviewed and the new reference doesn't add sufficiently to the conclusions to be included at this stage.
499	3	12	6	12	8	For completeness the ACRE project should be mentioned. (http://www.met-acre.org/) as a major activity in delivering historical atmospheric reanalyses. (Brown, Simon, The Met Office Hadly Centre)	Thanks for the comment, the new reference doesn't add sufficiently to the conclusions to be included at this stage.
500	3	12	7	12	7	The "Twentieth Century Reanalysis" has now been published: Compo et al. (2011), QJRMS (Brönnimann, Stefan, University of Bern)	Comment noted, no action, discussed later.
501	3	12	14	0	0	We would argue that this (Knapp et al e.g. in the W. Pac) depicts the uncertainty of any TC metric. In addition, agency estimates can differ from detailed reconstructions or recorded data from extreme events, and new technology thought to increase our "understanding" can be changed by yet newer technology that showed the former to be biased. e.g. Powell and Aberson 2001 (see intensity table in appendix), Powell, Uhlhorn, and Kepert 2009 (SFMR vs GPS sonde estimates of intensity). (I see this is discussed further in later pages). (UNITED STATES OF AMERICA)	So noted, added reference to box.
502	3	12	27	12	27	refer to Box 3.2 when mentioning "agricultural as well as hydrological drought" here (Stocker, Thomas, IPCC WGI TSU)	So noted, added reference to box.
503	3	12	27	12	27	It would be preferable to clearly indicate that definitions for agricultural and hydrological drought are provided in Box 3.2. (IPCC WGII TSU)	disagree; box discusses drought models.
504	3	12	28	12	29	Box 3.2 is not particularly relevant to the context of the sentence starting with "As a consequence…." (CANADA)	Agree, no action requested.
505	3	12	31	12	33	I think this is a bit off topic. Whether we can close water budgets is not an issue for this report. More pertinant would be whether available runoff and stream flow data sets can be used to assess changes in extremes high (and low) flows, and whether there is adequate coverage to assess changes in these extremes regionally. (Zwiers, Francis, Environment Canada)	Thanks for the comment, the new reference doesn't add sufficiently to the conclusions to be included at this stage.
506	3	12	31	12	33	Compared to runoff, observations of evapotranspiration are even more scarce. The lack of evapotranspiration observations has been a critical issue for the validation of model results. It has also contributed significantly to the uncertainties in water budget assessments (e.g., Szeto, K.K., H. Tran, M. MacKay, R. Crawford, and R.E. Stewart: The MAGS Water and Energy Budget Study. J. Hydrometeorology, 9, 96-115). (CANADA)	So noted, precipitation generally includes snow measurements converted to liquid water equivalent.
507	3	12	34	0	0	Snow is also hard to measure. (UNITED STATES OF AMERICA)	Thanks for the comment.
508	3	12	34	12	35	An issue here is not just the coverage of snow data, but also its quality (e.g., whether we have good estimates of snow density, a good understanding of snow aging, and whether we have reliable snow water equivalent estimates). (Zwiers, Francis, Environment Canada)	Will clarify. Text amended slightly.
509	3	12	34	12	36	The statement is vague and somewhat misleading. First the reader does not know what "several physical impacts" are being referred to. The paragraph is about soil moisture so the point that needs to be made is that spring snowpack is a major control of soil moisture in semi-arid regions. Second, in many basins where runoff is stored for hydro-power or water supply, there are good in situ observations of snow water equivalent for use in reservoir management. (Brown, Ross, Environment Canada @ Ouranos)	Disagree, discussion is about the most comprehensive global data set.
510	3	12	42	0	0	There are ample examples on similar lines given in this section (aircraft reconnaissance, tornados, thunderstorms etc.). The last example on rainfall and precipitation can be removed or should be moved somewere in the previous paragraphs (GARG, AMIT, INDIAN INSTITUTE OF MANAGEMENT AHMEDABAD)	not sure which is comment 11 from Albert.
511	3	12	42	12	43	See comment no. 11. (Klein Tank, Albert, KNMI)	Authors don't feel this is causing confusion.
512	3	12	43	12	43	Would "more robust" be more appropriate than "robust". Also, is there some connection between "robust" and the formalised confidence levels (guidance note…)? (SWEDEN)	In many instances the current "observing system" does provide sparse or problematic data, since many areas are under observed or the observations are not of high quality.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
513	3	12	47	12	47	It might be better to write "the data record remain sparse and problematic". The text as it stands might be read as if the current observing system provides data that are sparse and problematic. The current observing system is certainly not perfect, but it is, overall, better than what we had the past, even if data assimilation techniques have to be employed to enable best use to ne made of much of the information provided by a diverse mix of observation types. (Global Climate Observing System Steering Committee)	Figure revised to include NZ.
514	3	12	50	0	0	Figure 3.2: We note here that the regional extent for SAU does not appear to include New Zealand, however, Tables 3.2 and 3.3. refer to this region as South Australia/New Zealand and includes results for New Zealand. The regions shown here are however consistent with the geographical extents used to generate figures 3.6 and 3.8, where New Zealand was not included in the SAU region projected by Kharin. Some clarification is needed. (Stocker, Thomas, IPCC WGI TSU)	So noted. Tables and figures changed to take this into account.
515	3	12	50	0	0	Figure 3.2: See other comment regarding New Zealand. In any case, should not the caption be extended to read "Definitions of regions used in Tables 3.2 and 3.3, and Figures 3.6 and 3.8"? Also, it is not clear why the Arctic (ARC) is shown here, as it is not a region used in the Tables. We also suggest it might be useful to include the abbreviated region names from Figure 3.2 within Tables 3.2 and 3.3 along side the names given in full. (Stocker, Thomas, IPCC WGI TSU)	Disagree prefer map to define regions before Table with results.
516	3	12	50	12	55	Suggest swapping call-outs around, so that the Table comes before the figure that shows the regional definitions. (Stocker, Thomas, IPCC WGI TSU)	Maps projections have been changed to Robinson.
517	3	12	51	12	51	It seems to me that the maps on which all of the projections are produced, including the map showing regional domains, should be equal area maps--not various Mercator like projections that greatly distort the areas of various conditions. Basically, Greenland should be half the size of India, not several times as large. (MacCracken, Michael, Climate Institute)	Agree
518	3	12	53	0	0	Table 3.2: It might be useful to include the abbreviated region names from Figure 3.2 within the Table, eg, W. North America (WMA). (Stocker, Thomas, IPCC WGI TSU)	So noted, will change.
519	3	12	53	12	55	Table 3.2: ANA, WNA & CNA Tmax sections - for these regions you refer to hot days (HDs) but only WD/CD are listed in the top row. Should HD in these instances be changed to WD? (Stocker, Thomas, IPCC WGI TSU)	the title is now removed
520	3	13	1	13	1	Title should be reworded so that it is not phrased as a question. Removing the 'what are' could achieve this. (Stocker, Thomas, IPCC WGI TSU)	the subtitle is deleted, subsequent numberings are changed to reflect this
521	3	13	1	13	1	Suggest to delete this subsection title, i.e., just leave the text without the section title. You would then need to change the following subsection numbering accordingly. The subsection 3.2.2.1 is very short and fits perfectly under the section 3.2.2 title "The Causes Behind the Changes". (Stocker, Thomas, IPCC WGI TSU)	This is correct. We think this statement is important, for this audience (which is not the AR4 audience)
522	3	13	3	13	9	True, but the same reasoning could have opened the chapter for all climate system topics discussed in the entire AR4. (Klein Tank, Albert, KNMI)	words edited as suggested
523	3	13	5	13	6	“forcings such as increased greenhouse gas emissions principally due to the burning of fossil fuels, and land use and land cover changes.” I realize that you are not making an exhaustive list here, but I would suggest adding aerosol emission to this list as they are suggested to have large impacts over local to regional scales—oftentimes exceeding those from greenhouse gas emissions. (UNITED STATES OF AMERICA)	changed to "so external forcings that affect …"
524	3	13	6	13	7	Suggest rephrasing "so changes that affect the mean climate … result in changes in extremes" to increase clarify. For example, sentence could be changed to "so external factors that affect the mean climate." (CANADA)	disagree. It will only be in a very specific case when a change in mean will not cause a change extreme.
525	3	13	6	13	7	It is not necessarily true that changes that affect the mean climate would in general result in changes in extremes. This is only true in such cases where we have a distribution similar to a normal distribution. In these cases it is true when you shift the mean while keeping the variance. (Wurzler, Sabine, LANUV NRW)	disagree. What have been assessed here are related to physical environment rather than impacts of extremes that are assessed in other chapters.
526	3	13	11	13	21	Here we insist on the importance to count the human effect caused by the increase of the population especially in the developing countries. This may affect many disasters related with the extreme in the meteorological phenomena. (Takayabu, Izuru, Meteorological Research Institute)	agreed. "overall increase" is changed to "net increase". Do not believe the current wording implies that warming is the only result of changes in external forcings.
527	3	13	13	13	16	External forcing on the climate can both warm and cool, depending upon the forcing, but this is written implicitly assuming that external forcings warm exclusively. (Zwiers, Francis, Environment Canada)	agreed. Wording revised
528	3	13	15	13	15	Suggest changing "humidity content" to "moisture content" because humidity can imply either relative humidity or specific humidity, and relative humidity is expected to remain near constant under global warming. (CANADA)	agreed, wording changed as suggested.
529	3	13	15	13	16	The statement concerning land-sea contrast and possible impact on monsoon is more speculative than the two previous that have a stronger physical basis, with a clear impact on temperature or precipitation extremes. We know that monsoon is altered by many other factors related to sea-atmosphere or land-atmosphere interactions, and that models fail at projecting a clear impact on monsoons in every regions in spite of the general increase of land-sea contrast in temperature. I propose to add “and to some extent” before “the increased land-sea contrast”. (PLANTON, Serge, Météo-France)	agree. But impacts on circulation has already been mentioned.
530	3	13	16	0	0	These changes can affect not only monsoons, but other large scale patterns, such as NAO and Eastern Atlantic pattern. (GREECE)	wording has been changed to "to some extend"
531	3	13	16	13	16	This statement seems to contrast with the assessment on monsoons - implicitly, this statement expresses quite a bit more certainty concerning the effects of external forcing on monsoons. (Zwiers, Francis, Environment Canada)	reference changed to Hegerl et al. 2007

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
532	3	13	24	13	24	Please cite a specific IPCC chapter, SPM, or synthesis report. (Stocker, Thomas, IPCC WGI TSU)	disagree. This is obvious and does not need to be mentioned here.
533	3	13	24	13	26	The authors should consider adding a statement that this and other statements from AR4 are in the process of being reveavulated for AR5. (UNITED STATES OF AMERICA)	disagree. This is referencing the AR4 assessment. The ongoing AR5 assessment will decide if this AR4 assessment needs to be modified. Not appropriate to consider changing this assessment in SREX.
534	3	13	24	13	26	"Most of the observed increase in global average temperatures is very likely due to the observed increase in anthropogenic greenhouse gas concentrations." This statement has come under increased scrutiny and pressure. It seems that the use of "very likely" is perhaps too strong. Recent papers on the warming influence from black carbon (Ramanathan V., and G. Carmichael, 2009. Global and regional climate changes due to black carbon. Nature GeoScience, 1, 221-227), stratospheric water vapor (Solomon, S., et al. 2010. Contributions of stratospheric water vapor to decadal changes in the rate of global warming. Science, published on-line January 28, 2010), temperature biases (Klotzbach, P. J., et al., 2009. An alternative explanation for differential temperature trends at the surface and in the Lower Troposphere, Journal of Geophysical Research, 114, 10.1029/2009JD011841; McKitrick, R. R., and P. J. Michaels, 2007. Quantifying the influence of anthropogenic surface processes inhomogeneities on gridded global climate data. Journal of Geophysical Research, 112, D24S09, doi:10.1029/2007JD008465) and SST errors (Thompson, D., et al., 2008. A large discontinuity in the mid-twentieth century in observed global-mean surface temperature. Nature, 453, 646-649), combine to potential explain a substantial fraction (bordering on 50%) of the observed warming since the mid-20th century. (Knappenberger, Paul, New Hope Environmental Sciences)	agreed, but space is limited.
535	3	13	27	0	0	other forcings : these would be useful to have a list with Reference (International Petroleum Industry Environmental Conservation Association (IPIECA))	ocean heat content is added but not the references as they were assessed in Hegerl et al. 2007a
536	3	13	30	0	0	Chap 3, page 13, line 30: we know that people are land-centric since we live on land, but oceans cover the majority of the planet. we would add at the end of this sentence something to that effect. For example, "Warming in the oceans, which cover 70% of the planet, has been detected and attributed to human effects as well (Barnett et al. 2005; Pierce et al. 2006)." Full references are: Barnett, T. P., Pierce, D. W., AchutaRao, K., Gleckler, P., Santer, B., Gregory, J., and Washington, W., 2005: Penetration of Human-Induced Warming into the World's Oceans Science, Published online 2 June 2005, doi:10.1126/science.1112418. Pierce, D. W., Barnett, T. P., AchutaRao, K., Gleckler, P., Gregory, J., and Washington, W., 2006: Anthropogenic warming of the oceans: Observations and model results J. Climate, v. 19, p. 1873-1900. (UNITED STATES OF AMERICA)	agreed, but the global-scale detection and attribution do not imply signal is strong enough to be detectable at local scale. The lack of detection and attribution does not imply there is no change regional scale.
537	3	13	32	13	34	This conclusion arises from a very narrow interpretation of how attribution should be done. Indeed, if one considers only what is happening over some small area (e.g., the Arctic) and analyzes the record, there is higher variability and so it is harder to see a signal. However, the global system is interconnected, and there is no way that some region (e.g., the Arctic) can stay isolated from what is happening elsewhere--it just is not possible. Thus, if one has detection-attribution of a human influence at the global scale, there is going to be a human influence on what is happening in a particular region--it may not show up with as high a statistical significance, but it has to be there and weight must be given to this in analyses. And this would be true of all of the forcings--one could not have a solar influence in the Arctic without also having some manifestation at other locations, or vice-versa--a proper analysis would be looking for teleconnnections, consistent patterns inside and outside the region, etc. Just doing an analysis on a small area to see if one can see a global phenomenon might be easy on a statistics student, but makes no sense with our couple golbal system. Thus, this sentece really needs to be clarified and fixed up along with some adjustments to following sentences. (MacCracken, Michael, Climate Institute)	the passage has been reworked.
538	3	13	32	13	49	This paragraph mixes messages on attribution (which has demanding requirements) with those on detection (which as less demanding requirements). It starts off talking about attribution - but then switches to a discussion on detection. I think this is at least somewhat confusing for readers. I suggest discussing detection first, followed by a separate short paragraph on attribution. Regarding detection results on mean surface air (or sea) temperatures, we can detect human influence in surface temperature at the gridbox scale in many places (e.g., work by Braganza and Karoly, see Ch 9, WG1, AR4 for references), but for reasons discussed here and in Ch9, WG1, AR4, we cannot generally attribute causes to effects at anywhere near the grid box scale. (Zwiers, Francis, Environment Canada)	The reference is listed as Van Oldenborgh in the Reference.
539	3	13	38	0	0	Oldenborgh et al. (2009) is not seen in References. (NISHIMORI, Motoki, National Institute for Agri-Environmental Sciences)	"likely" is changed to "perhaps"
540	3	13	39	13	39	The use of "likely" here appears to be casual, not tied to the AR5 Guidance Note on Treatment of Uncertainties, and therefore its use should be avoided. (IPCC WGII TSU)	disagree. Lack of space to cite all papers on subject.
541	3	13	42	13	42	I propose to add a reference to recent papers on the subject of the detection of an anthropogenic influence at small spatial scales with application to France seasonal temperature: "; Ribes et al., 2009, 2010" after "Stott et al., 2010". The corresponding complete references are: "Ribes, A., J.-M. Azaïs, and S. Planton, 2009 : Adaptation of the optimal fingerprint method for climate change detection using a well-conditioned covariance matrix estimate. Climate Dynamics, 33(5), 707-722." and "Ribes, A., J.-M. Azaïs, and S. Planton, 2010 : A method for regional climate change detection using smooth temporal patterns. Climate Dynamics, 35(2-3), 391-406." (PLANTON, Serge, Méto-France)	text has been modified: "in each of 14" is removed.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
542	3	13	45	13	45	Are the domains of "14 Northern Hemispheric sub-continental regions" consistent with Fig. 3.2? If not, more explanation needs to be added for completeness with relevant references. (CANADA)	disagee. The assessment here is about changes in mean. Meehl et al. was assessed in P22 lines 15-20, though a wrong reference was linked. The correct reference is now used.
543	3	13	47	13	49	(Also applies to P. 22, lines 15-20): The discussion here of attributing changes in extremes to human activity should also assess a relevant paper: Meehl, G.A., J.M. Arblaster and C. Tebaldi, 2007: Contributions of natural and anthropogenic forcing to changes in temperature extremes over the U.S. Geophys. Res. Lett., 34, L19709, doi:10.1029/2007GL030948. This paper concludes "most of the changes in [observed] temperature extremes over the U.S. are likely due to human activity". This would seem to be quite relevant to the discussion here. The changes in observed temperature extremes attributed in that paper to human activity (using anthropogenic vs. natural forcings in climate model simulations of 20th century) are decreased frost days, increased growing season length, increased warm nights, and increased heat wave intensity. (UNITED STATES OF AMERICA)	disagree, as US climate extreme index are very different from those discussed in 3.2.2.3.
544	3	13	48	0	0	The US climate extreme index should go in 3.2.2.3? (Klein Tank, Albert, KNMI)	disagree
545	3	13	49	0	0	New Zealand is a very small area with lots of orography - delete from list? (Stouffer, Ronald, NOAA)	sentence has been modified
546	3	13	49	13	49	, in the temperature of Europe (Christidis et al., 2011) and (GREECE)	sentence has been modified but inclusion of "seasonal" seems too detailed for purposes of this paragraph.
547	3	13	49	13	49	The sentence fragment "in Europe (...)" should be elaborated to provide more clarity; something like "in seasonal temperatures over Europe (…)" would be more clear. (CANADA)	The discussion here include AR4 conclusions and new materials since AR4. As Milly et al. (2005) was assessed in AR4, it is not explicitly re-assessed here.
548	3	13	51	14	11	In discussing findings on mean climate that might be relevant for extremes, did the authors consider discussing the results of Milly et al Nature 438, 347-350 (17 November 2005) | doi:10.1038/nature04312, who showed consistency of 20th century mean annual streamflow trends with forced climate-model simulations, and inconsistency with unforced simulations? Doesn't it seem that a regional increase (decrease) in streamflow (=atmospheric water vapor convergence) might be relevant evidence in support of some conclusions about changes in drought risk, and possibly even flood risk at the largest space-time scales? (UNITED STATES OF AMERICA)	"approximately" has been added but references are not. We agree it would nice to have the references but there is a limit in space.
549	3	13	52	13	53	The sentence fragment "increases exponentially" should be "increases approximately exponentially". This is an important introductory sentence explaining physical background so, for completeness, a couple of key references should be cited (in line 53 or in line 55) such as Allen and Ingram (2002) and Trenberth et al. (2003). The full citations of both papers are provided:. Allen, M. R., and W. J. Ingram, 2002: Constraints on future changes in climate and the hydrologic cycle. Nature, 419, 224–232. Trenberth, K. E., A. Dai, R. M. Rasmussen, and D. B. Parsons, 2003: The changing character of precipitation. Bull. Amer. Meteor. Soc., 84, 1205–1217. (CANADA)	agreed. Text modified to cite relevant AR4 text.
550	3	13	52	13	55	"Since moisture condenses out of supersaturated air, it is physically plausible that the distribution of relative humidity would remain constant." Sorry, we don't understand the argument here. Perhaps another sentence or two is needed. See IPCC AR4 for a better discussion of this issue. (UNITED STATES OF AMERICA)	text has been revised.
551	3	13	53	13	55	"Since moisture..... increase in temperature.". This statement is non-sense.. It is true that water vapor condenses, but this is not the reason why the relative humidity remains rather constant even though climate changes. Warmer air can take up more water vapor than cooler air before reaching saturation. Relative humidity is a measurement of the amount of water vapor in a mixture of air and water vapor. It is defined as the partial pressure of water vapor in the air-water mixture, given as a percentage of the saturated vapor pressure under those conditions. The relative humidity of air thus changes not only with respect to the absolute humidity (moisture content) but also temperature and pressure, upon which the saturated vapor pressure depends. (Wurzler, Sabine, LANUV NRW)	sentence modified to indicate that 7% increase is at the temperature of current climate.
552	3	13	54	13	55	The statement "This means that specific humidity increases about 7% for one degree increase in temperature" is ambiguous. Particularly, within what ranges of temperature and relative humidity will this relationship be valid? (CANADA)	text has been modified.
553	3	13	60	13	61	Given that this sentence is an attribution statement, it would be best to avoid use of the word "detected." (IPCC WGII TSU)	sentence modified "by comparing observations with model simulations …"
554	3	13	61	0	0	This is a purely scientific chapter. A small example on how anthropogenic influence has been detected in global surface humidity will help the readers. As has been mentioned that this relationship is a significant advancement since AR4, an example to substantiate it is necessary. (GARG, AMIT, INDIAN INSTITUTE OF MANAGEMENT AHMEDABAD)	wording changed as suggested
555	3	14	1	0	0	Add "all other things being equal" after "expected". (UNITED STATES OF AMERICA)	The Min et al. 2011 is now also cited here.
556	3	14	1	0	0	Chap 3, page 14, lines 1-11: The new Min, Zhang, Zwiers, and Hegerl 2011 paper in Nature (doi:10.1038/nature09763) needs to be referenced and briefly described here. Also, we see that the bibliography still lists this paper is "in press", but it has now appeared. (UNITED STATES OF AMERICA)	the text has now been deleted.
557	3	14	1	14	2	Shifts in precip patterns should be mentioned for context (Held and Soden, JCLI, 2006) (UNITED STATES OF AMERICA)	agreed, lines 2-3 are now been removed.
558	3	14	1	14	4	Precipitation formation depends not only on the water vapor content of the atmosphere but also depends to a very high extent on the aerosol particle population. Cloud dynamics works further in this direction. So it is no miracle to reach higher precipitation rates than from Clausius Clapeyron. (Wurzler, Sabine, LANUV NRW)	sentence deleted

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559	3	14	2	0	0	This sentence hangs. Delete The Netherlands is a very small area. Wind direction matters a lot for P changes. (Stouffer, Ronald, NOAA)	agreed. Lines 2-4 are now removed
560	3	14	3	0	0	Clausius-Clapeyron tells you how the maximum quantity of water in the air varies with T. It tells you nothing on how quickly the water can pour out. I never could understand this statement which is repeated ad nauseam in the literature. (Boucher, Olivier, Met Office)	sentence deleted
561	3	14	3	14	3	Instead of just saying "twice as fast," it would be helpful to indicate the range of rates by which hourly precipitation extremes might increase above what would be assumed from the Clausius-Clapeyron relationship. (IPCC WGII TSU)	sentence deleted
562	3	14	4	14	4	"Haerter and Berg, 2009 and Lenderink and van Meijgaard 2009" do not look relevant here because the former points out other factor's influence (rainfall type change with more convective rains as warming), and the latter is just a reply to the former as it is understood. Refer to page 27 line 61-62 where both are cited. (CANADA)	"latitudinal" is added
563	3	14	5	14	5	The sentence fragment "in the pattern of land precipitation trends" needs to be elaborated, such as "in the latitudinal pattern of land precipitation trends". (CANADA)	agreed. The text has been modified.
564	3	14	6	14	8	Reduction of signal amplitude from averaging across models is part of the story - but certainly not all, because trends simulated by individual models also tend to be smaller than observed. (Zwiers, Francis, Environment Canada)	sentence modified
565	3	14	6	14	8	The sentence fragment "simple averaging of those patterns from model simulation" should be clarified as "simple averaging of those patterns from multiple model simulations". Nevertheless, this sentence, explaining why models underestimate observed changes in precipitation, can be rather confusing. (CANADA)	sentence modified
566	3	14	6	14	8	Unclear what is meant by "Because ....signal". Presumably this is about fingerprint patterns in the D/A formalism, but this is not clear from the text. Like with global temperature, I would imagine that averaging over large regions would give the strongest signal. (Klein Tank, Albert, KNMI)	this section discusses changes in the mean. Min et al. (2011) is cited in extreme precipitation section.
567	3	14	6	14	11	Min at al. 2011 should also be mentioned at this point. (Stocker, Thomas, IPCC WGI TSU)	error corrected
568	3	14	7	14	7	"simulation" should be plural. (Zwiers, Francis, Environment Canada)	"sulphate" is deleted
569	3	14	8	0	0	There are other aerosols than just sulphate aerosols. (Boucher, Olivier, Met Office)	Min et al. (2011) is now cited earlier in paragraph.
570	3	14	9	14	9	Min et al. (2011) should be included in this section. However, it should also be noted that the signal that they detected is weakened when natural and anthropogenic forces are included together. (UNITED STATES OF AMERICA)	wording modified "internal variability of precipitation ,,," to be more specific.
571	3	14	9	14	11	“Internal variability is low in this region.” We're not convinced that internal variability is weak poleward of 55N. Perhaps the sentence could be more specific as to what kind of internal variability is weak. (UNITED STATES OF AMERICA)	the amount of precipitation is bounded by zero, smaller mean precipitation also implies lower precipitation variability
572	3	14	11	0	0	How confident are we that internal variability is low in precipitation in land regions north of 55N. Is this primarily a model inference? Are there observational issues, especially in measuring longer-term variability to investigate possibly natural multidecadal variability? It is stated as if a certainty. (UNITED STATES OF AMERICA)	wording modified.
573	3	14	11	14	11	Insert "in precipitation" after "internal variability". Perhaps explain why this is the case. Since precipitation is bounded below by zero, smaller mean precipitation also implies lower precipitation variability). (Zwiers, Francis, Environment Canada)	Pall et al. (2011) is cited in this section (page 14, line 61 of the SOD)
574	3	14	13	0	0	We recommend citing the attribution work by Pall et al (2011) to highlight the progress in the science of attribution of extreme events to anthropogenic greenhouse gas emissions. This is a useful example as it links climate change to a specific weather extreme. Reference: Pall, P., Aina, T. Stone, D.A., Stott, P.A., Nozawa, T., Hilberts, A.G.J., Lohmann, D. and Allen, M.R. (2011) Anthropogenic greenhouse gas contribution to flood risk in England and Wales in autumn 2000. Nature. 470: 382-385. (World Food Programme (WFP))	It is a good idea but additional table would make the chapter even longer. At global scale these details are in Table 3.1.
575	3	14	15	0	0	Section 3.2.2.3 For the reader who wants to find out the current status of detection and attribution of changes in extremes he will find it quite difficult to find out from this chapters present form. This information probably has significant policymaker interest so would it be helpful if a summary table (another one!) was provided (possibly in this section) of all detection and attributions studies and their conclusions? (Brown, Simon, The Met Office Hadly Centre)	wording modified
576	3	14	15	14	15	The GOOD PRACTICE guidance paper....' (Stocker, Thomas, IPCC WGI TSU)	the paragraph is now in two paragraphs
577	3	14	15	14	24	I think this paragraph should be broken into 2 paragraphs. It discusses two separate aspects of attribution - the types of attribution (down to line 21), and then attribution in the context of extremes (line 21-24). (Zwiers, Francis, Environment Canada)	agreed. Text is modified to reflect this.
578	3	14	15	14	24	Important to note that this is clearly different from D/A as defined in the WG1 contribution to AR4. (Klein Tank, Albert, KNMI)	Disagree. It is correct that independent errors in different variables may be canceled each others out, but not all variables have the same s/n ratio. Thus, adding combining low s/n ratio variable with high s/n ratio variable has a potential to improve detection for low s/n ratio variable, but this may not necessarily result in better detection for all variables.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
579	3	14	33	0	0	Chap 3, page 14, lines 33-36: One of the particularly relevant findings of the work mentioned in the text at this point, but which is not brought out in the text, is that combining multiple variables gives a higher signal to noise ratio for detection and attribution. This is because independent errors in the variables tend to cancel out, allowing better detection and attribution over a limited region (a problem mentioned in the paragraph beginning at Chap 3, page 13, line 32). The sentence starting on Chap 3, page 14, line 33 could be written to better reflect the results of these studies. For example, "GCM-simulated precipitation and temperature have been downscaled as input to hydrological and snow depth models to infer past and future changes in temperature (Bonfils et al. 2008), timing of the peak streamflow (Hidalgo et al. 2009), and snow water equivalent (Pierce et al. 2008) for the western U.S., and thus enabled a detection and attribution analysis on human-induced changes in the combination of these variables at a higher signal-to-noise ratio than for any one variable individually (Barnett et al., 2008)." Full references are: Bonfils, C., Benjamin D. Santer, David W. Pierce, Hugo G. Hidalgo, Govindasamy Bala, Tapash Das, Tim P. Barnett, Daniel R. Cayan, Charles Doutriaux, Andrew W. Wood, Art Mirin, Toru Nozawa, 2008: Detection and Attribution of Temperature Changes in the Mountainous Western United States. J. Climate, v. 21, p. 6404-6424. Hidalgo, H. G., T. Das, M. D. Dettinger, D. R. Cayan, D. W. Pierce, T. P. Barnett, G. Bala, A. Mirin, A. W. Wood, C. Bonfils, B. D. Santer, T. Nozawa, 2009: Detection and attribution of streamflow timing changes to climate change in the western United States. J. Climate, v. 22, p. 3838-3855. Pierce, D. W., Tim P. Barnett, Hugo G. Hidalgo, Tapash Das, Celine Bonfils, Benjamin D. Santer, Govindasamy Bala, Michael D. Dettinger, Daniel R. Cayan, Art Mirin, Andrew W. Wood, Toru Nozawa, 2008: Attribution of Declining Western U.S. Snowpack to Human Effects. J. Climate, v. 21, p. 6425-6444. (UNITED STATES OF AMERICA)	text modified as suggested.
580	3	14	34	14	34	Replace "snow depth models" with "snowpack models" (Brown, Ross, Environment Canada @ Ouranos)	The "not" is not misplaced, it is intended. But sentence has been reworded to avoid misinterpretation.
581	3	14	39	14	39	The "not" here is misplaced. It should be deleted. (CANADA)	reference added
582	3	14	41	0	0	Chap 3, page 14, line 41: A reference to Knowles et al. 2006 is appropriate here, as there is discussion of the changes in spring streamflow without mentioning the important role in the shift of precipitation from snow to rain. For example: "...spring temperature has increased, more precipitation is falling as rain than snow (Knowles et al. 2006), and the timing of spring peak floods...etc". The full reference is: Knowles, N., M. D. Dettinger, and D. R. Cayan, 2006: Trends in snowfall versus rainfall in the western United States. Journal of Climate, 19, 4545-4559. (UNITED STATES OF AMERICA)	agreed, but this would require more text on changes from snow to precipitation etc. So no modification to the text is done
583	3	14	47	0	0	Chap 3, page 14, line 47: it would be good to say "The physical understanding that snow melts earlier, and more precipitation falls as rain rather than snow, increases [NOTE TYPO WITH EXTRA "enhances" AT THIS POINT IN THE DRAFT] our confidence in the assessment." (UNITED STATES OF AMERICA)	no, but we remove "by definition"
584	3	14	55	14	55	"Extreme events are by definition rare": Doe thisnot contradict the introduction? (Brönnimann, Stefan, University of Bern)	wording modified
585	3	14	56	14	56	"Catastrophic" is a loaded word I think - perhaps better to replace "rare and catastrophic" with "high impact". (Zwiers, Francis, Environment Canada)	reference added
586	3	14	59	0	0	Insert Dole et al 2011 as an additional citation after Hegerl et al, 2007 [Dole, R., M. Hoerling, J. Perlwitz, J. Eischeid, P. Pegion, T. Zhang, X.-W. Quan, and D. Murray (2011), Was there a basis for anticipating the 2010 Russian heat wave? Geophys. Res. Lett., doi:10.1029/2010GL046582] (Webb, Robert, NOAA)	reference cited
587	3	14	61	14	61	Could perhaps also cite Zwiers et al (2011), who estimate the effect of external forcing on waiting times for long return period temperature extremes. (Zwiers, Francis, Environment Canada)	Yes - there is a hybrid method - see comment 641. A short paragraph on these methods has been added.
588	3	15	0	0	0	Section 3.2.3.1. Information Sources for Climate Change Projections: In particular, this section describes the two main downscaling approaches, dynamical and statistical (Christensen et al., 2007). Question: Is there any hybrid method based on both dynamical and statistical approaches to predict extreme events? If so, how does it compare with a purely dynamical or statistical approach? (Mokssit, Abdalah, Direction de la Météorologie Nationale (DMN))	It is not possible to introduce new tables/boxes at this stage. Later subsections (3.2.3.2 and 3.2.3.3) deal specifically with uncertainty in the context of projections. All those sources of uncertainty listed in the comment are considered. And all sources of uncertainty are considered in making assessments - e.g., those presented in Table 3.3 (see also Section 3.1.5)
589	3	15	0	19	0	There is a lot of good discussion on uncertainty, which is key for SREX throughout and ch.3. But, it is not easy to follow which uncertainties and to what degree are dealt with. Maybe the types of uncertainties and the way they are assessed in ch.3 could be summarized somewhere in a table or small box discussing these and other uncertainties: data, model, emissions scenario, natural/aleatoric uncertaintiens etc.? (Mechler, Reinhard, INTERNATIONAL INSTITUTE FOR APPLIED SYSTEMS ANALYSIS)	the text goes beyond AR4. Where possible it has been shortened to focus better on extremes.
590	3	15	4	15	5	The fact that much of the discussion in this section is based on the AR4 would suggest there is considerable scope to shorten this section. (Stocker, Thomas, IPCC WGI TSU)	We are talking about development of regional projections here - so this is the correct citation. Other chapters (Randall et al and Meehl et al) are cited later and also now in the first sentence of Section 3.2.3.
591	3	15	5	0	0	Should this reference be to the regional chapter (Christensen et al.) or to the modelling chapters of AR4? (Klein Tank, Albert, KNMI)	References to these simulations are already included. It is not true in general that statistical methods cannot generate extremes larger than observed (see comments 629-631).

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
592	3	15	10	16	39	We firstly anticipate that in Japan, we have already dynamically downscaled by using 5km, 2km resolution RCM. In these very high resolution experiments, some of the drizzle probloem has been overtaken. In this subsection, they compare dynamically downscaling and the statistical downscaling. One of the weakness of the statistical downscaling is that they cannot represent the events that have not been evere observed, but we think that the same thing also has happend to the dynamically downscaling. If the climate changed and the regiona entered to the more tropical climate area, we cannot guarantee the simulated value, because we have never validate such climate condition. We should be careful on that issue. Of course when we get a RCM that applicable whole the world, we would become free from this issue, as will be done in CORDEX. Ref. Kanada, S., M. Nakano and T. Kato, 2010: Climatorogical characteristics of daily precipitation over Japan in the Kakushin regional climate experimants using a non-hydrostatic 5-km mesh model: Comparison with an outer global 20-km-mesh atmospheric climate model, SOLA, 6, 117-120. Nakano, M., S. Kanada, T. Kato and K. Kurihara, 2011: Monthly maximum number of consecutive dry days in Japan and its reproducibility by a 5-km-mesh cloud sysytem resolving regional model. Hydrological Research Letters, accepted. (Takayabu, Izuru, Meteorological Research Institute)	They are now numbered.
593	3	15	13	15	16	Sentence would be clearer if the four sources were numbered following a colon (UNITED STATES OF AMERICA)	The phrase has been deleted as downscaling is defined in the folowing paragraphs. Note that there are examples in which downscaling deteriorate the gcm-simulations
594	3	15	14	0	0	Chap 3, page 15, line 14: I'm a little uncomfortable with the text that says "downscaling of GCM-simulated data using techniques to enhance regional detail" as by my ear it somewhat misses the point of downscaling. I would suggest something more like "downscaling of GCM-simulated data using techniques that better resolve important regional-scale processes". (UNITED STATES OF AMERICA)	This sentence has been deleted in order to shorten text and reduce repetition of AR4.
595	3	15	20	15	20	Significant in what sense? Rather than implicitly invoking what could possibly be statistical significance, maybe it would be better to replace "significant and improving skill" with "considerable improvement". (Zwiers, Francis, Environment Canada)	This sentence has been deleted in order to shorten text and reduce repetition of AR4.
596	3	15	20	15	20	Suggest to delete "state-of-the-art" -- unclear how "state-of-the-art" is defined or what it contains. (Stocker, Thomas, IPCC WGI TSU)	The wording was paraphrased from Randall et al., 2007 and has been changed to more closely repeat the conclusions of that AR4 chapter.
597	3	15	22	15	22	I suggest to change “surprisingly well simulated“ with “reasonably simulated ”, because the performance of the GCMs to simulate extreme events depends on the of type of extremes and region. Good performances have been obtained in generally for extremes of temperature, but some deficiencies have been found in the simulation of precipitation extremes (Kharin, V.V., F.W. Zwiers, and X. Zhang, 2005: Intercomparison of near surface temperature and precipitation extremes in AMIP-2 simulations, reanalyses and observations. J. Clim., 18(24), 5201–5223) (ITALY)	The wording was paraphrased from Randall et al., 2007 and has been changed to more closely repeat the conclusions of that AR4 chapter. In particular, it is now noted that this remark applies particularly to temperature and that there are greater difficulties with precipitation.
598	3	15	22	15	23	This is a very strong, blanket statement. Is it supportable in light of the many low and medium confidence projections cited throughout this chapter? (UNITED STATES OF AMERICA)	This sentence has been deleted.
599	3	15	26	0	0	Add reference to Randall et al. 2007 (UNITED STATES OF AMERICA)	Wording has been changed along the lines suggested.
600	3	15	26	15	26	This is a bit awkwardly worded. Perhaps replace "model parameterization schemes" with "approximations, known as parameterizations, which are used to represent processes that can not be fully resolved in climate models (e.g., ...)". (Zwiers, Francis, Environment Canada)	It was not the intention to imply this emphasis. The wording has been changed accordingly.
601	3	15	27	15	31	These two sentences give greater emphasis on statistical issues (lack of robustness) than on data issues, whereas the latter is the much bigger issue of the two. In a certain sense, extremes value analysis is in a unique position because, while the focus is on things that are rare, the underpinning statistical theory that leads to the GEV distribution, or the Generalized Pareto distribution, is very robust, and seems to work well under a broad range of circumstances. (Zwiers, Francis, Environment Canada)	A sentence on the need to consider scaling as part of validation has been added to the end of the later paragraph which discusses this issue.
602	3	15	27	15	31	In addition to data and the fact that we are working in the tails, an additional issue that has not been well resolved is the "scaling" issue - the discrepancy between the scale that is represented by insitu observations, and that which is represented by model grid box values. This is not just an issue of parameterizations - but is in fact, more fundamental. (Zwiers, Francis, Environment Canada)	Wording changed as suggested.
603	3	15	33	15	33	There is a bit too much "development" in this sentence. Suggest replacing "The development of projections" with "The requirement for projections" - it is the requirement that has driven the development of downscaling approaches. (Zwiers, Francis, Environment Canada)	Where possible the text has been shortened to better focus on extremes rather than general downscaling issues.
604	3	15	33	16	34	The discussion of downscaling methods might be condensed. (UNITED STATES OF AMERICA)	Reference is interesting on general RCM issues but does not specifically discuss extremes. Hence it has not been added.
605	3	15	37	0	0	Chap 3, page 15, line 37: "They are, nonetheless, constrained by the reliability of large-scale information..." Suggest you consider adding a reference to Liang et al. 2008 at the end of this sentence. Full ref is: Liang, X-Z, K. E. Kunkel, G. A. Meehl, R. G. Jones, and J. X. L. Wang, 2008: Regional climate models downscaling analysis of general circulation models present climate biases propagation into future change projections. Geophysical Research Letters, 35, L08709, doi:10.1029/2007GL032849, 2008. (UNITED STATES OF AMERICA)	A 'weaker' statement has been incorporated in the previous paragraph.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
606	3	15	38	15	41	We hope that models will become more useful as they go to higher resolution. We think the statement is too strong and could be eliminated. It's quite common to talk to people who think that the biggest obstacle to accurate climate projections is lack of resolution, and we don't think we should encourage that sentiment. (UNITED STATES OF AMERICA)	This is a nice overview of current RCM issues but not very specific to extremes and does not cover any 'new' issues. So this reference has not been added.
607	3	15	43	17	44	The recent study of Rummukainen, (2010) provides an extensive report of the downscaling concept, the mathematics, the limitations, the potentials and the added value of regional modeling. REFERENCE: Rummukainen, M., (2010) State-of-the-art with regional climate models. Wiley Interdisciplinary Reviews: Climate Change. 1(1), DO - 10.1002/wcc.8 (GREECE)	Kanada et al. 2010 has now been added. But this does not include a simulation of 25 complete years at high resolution. Thus the wording has not been changed.
608	3	15	46	15	49	Recently, non-hydrostatic mesoscale models of 2-5km resolution are coming to be used for climate simulations with periods of 25 years(Kitoh et al., 2009, Kanada et al.,2010). * 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 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)	This has been added.
609	3	15	48	0	0	Add "explicitly" before "resolved". (UNITED STATES OF AMERICA)	No comment on the reliability of these simulations is made. The reference to 'super-high resolution' has been removed.
610	3	15	52	0	0	Garbage in garbage out (Trenberth, Kevin, NCAR)	This issue relates to uncertainty - which is discussed in a later section. It is not clear whether the ability of an RCM to develop its own trends should be considered as an advantage or a disadvantage of dynamical downscaling - which is the issue being discussed here. No change made.
611	3	15	55	0	0	Chap 3, page 15, paragraph starting on line 55: This paragraph does not mention the important point that different RCMs can develop different climate trends even when driven by the same GCM. I suggest adding text along these lines. (UNITED STATES OF AMERICA)	The final part of this sentence has been deleted. A discussion of what is meant by consistency is too detailed for this report - and the issues are not specific to extremes.
612	3	15	57	15	57	It is misleading to use the description 'internally consistent', and in the past, this has been mistaken for 'models being physically consistent'. The dynamical downscaling models are neither internally nor physically consistent – when examining the heat fluxes associated with evaporation between ERA40 and a RCM driven by ERA40, there are systematic differences on large scales. A comparison also reveals that the RCM produces more precipitation. This means that the vertical flow of energy in the RCM is inconsistent with the driving model. The SSTs from the driving GCMs do not respond to changes in the evaporation in a one-way nesting, and upscaling processes are not well-represented. Furthermore, models with a large domain may require spectral nudging in order to produce a large-scale flow that is consistent with the driving GCM. The RCM often has different parameterisation schemes to that of the GCM, which again leads to inconsistent representation in terms of micro-physics. Furthermore, phrase 'internally consistent' is really quite meaningless – we cannot think of anything that is not 'internally consistent', and in most cases this is not really a relevant criterion. RCMs often have serious biases and do not reproduce a number of statistical relationships. The big question is whether the models can represent the important features in real world with skill. (NORWAY)	The volume of output is now an issue for some users. The wording has been changed. The important point here is the availability of point vs area-averaged information. We are not attempting a systematic review of general downscaling issues.
613	3	15	57	15	58	I think the main drawback for users is no longer the computational cost (multi-RCM ensembles of simuluations are now, or will soon be, available for many regions ... ENSEMBLES, NARRCAP, CORDEX,...), so you don't need your own RCM. Rather, the main issues are (i) resolution (still too low) and (ii) biases that are still too large to comfortably drive many impacts models with available RCM output. (Zwiers, Francis, Environment Canada)	changed as suggested.
614	3	15	59	0	0	Add "and GCM" after "RCM" and before "parameterizations". (Stouffer, Ronald, NOAA)	changed as suggested.
615	3	15	59	15	59	RCMs provide area-averaged precipitation". Perhaps change to "precipitation averaged over a grid cell". (UNITED STATES OF AMERICA)	The community still has differing views on this - though the majority view is probably that it is not a matter of concern. But this text has been deleted since it is a general downscaling issue and not specific to extremes.
616	3	15	61	15	62	Why would this be a concern? I see this said in various places - but I'm not sure that I've seen an explanation as to what the problem is. Afterall, the thing that is provided to the RCMs is rather large scale driving information and not the specific conditions for, for example, triggering convection. Moreover, we are happy to drive RCMs with reanalyses without worrying about whether the RCM's physics package corresponds to that in the forecast model that is used for assimilation. (Zwiers, Francis, Environment Canada)	Delete sentence on coupling.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
617	3	15	63	15	63	A sentence or two on why coupling would be important, and what aspect of the ocean should be coupled to the regional atmosphere, would be worth while. I can see arguments for coupling the upper ocean to an RCM (e.g., for TC cyclogensis, or to be able to represent sea ice and its growth, decay and movement - which might be important for lee effects precipitation in coastal regions) but we presumably wouldn't want slow coupled processes to cause the RCM solution to diverge significantly from the driving model on large scales. So there would have to be limitations on the scope of coupling. (Zwiers, Francis, Environment Canada)	A few sentences on UKCP09 have now been added in Section 3.2.3.3 - citing Murphy et al., 2009 since it is not considered appropriate to cite websites.It was not considered appropriate to include the sugegsted example of temperature projections from UKCP09 in Section 3.3.1 since space precludes including country-level projections, except for some of the larger countries.
618	3	16	1	16	2	The importance of uncertainties in the projections of future extremes and the current efforts to quantify those errors are not well represented in the text. The briefest of nods are given to the ENSEMBLES project and UKCP (UK Climate Projections, http://ukclimateprojections.defra.gov.uk/) at p.16 line.37 and p.23 l.57 This might be due to the mistaken notion that UKCP has not been reviewed when it has (http://ukclimateprojections.defra.gov.uk/content/view/1140/689/). Nevertheless the projections are available and are beeing used by decision makers, particularly with regard to moderate extremes (eg hottest/wettest day of year) Though not addressing the more severe extremes surely the uncertainty in the 99th percentile is a minimum guide for the uncertianty of the more severe extremes? It does not seem tenable to me that such a significant advance in the treatment of future prediction uncertainty, particularly for extremes where adaptation is a risk mitigation activity, can be ignored and its omission is a disservice to a large portion of the intended readership of this report. Obvioulsy the UKCP data applies to a very small portion of the globe and undue prominence shouold not be given to the UK. I am not suggesting that, but I am arguing that the scientific developments of UKCP, the integtation of known uncertianties, and the implications it has for regional projections in extremes is applicable worldwide and this is what SREX should be reporting. A practical example of how to refenence UKCP findings would be for paragraph at p.23 line.34-59. A comment could be made that a regional study attempting to account for all known sources of uncertainty predict a range of changes (covering the central 80% of projections) in the hottest day of the year for the southern UK of -2 to +12 DegC for the high emissions scenario (http://ukclimateprojections.defra.gov.uk/images/stories/Presentation_maps/SumWarm_2080_hi.jpg). Now you may have strong missgivings about these numbers but I dont think an IPCC report has the latitude to ignore them. (Brown, Simon, The Met Office Hadly Centre)	This is a nice overview of current RCM issues but not very specific to extremes and does not cover any 'new' issues. So reference not added.
619	3	16	2	16	2	At the end of the sentence additional reference (Rummukainen, 2010). REFERENCE: Rummukainen, M., (2010) State-of-the-art with regional climate models. Wiley Interdisciplinary Reviews: Climate Change. 1(1), DO - 10.1002/wcc.8 (GREECE)	The typo in referring to predictors/predictands has been corrected. Text has been changed as suggested in comment 623. Bias correction is discussed later on (but additional references are not included at this stage).
620	3	16	4	0	0	Chap 3, page 16, line 4: "Statistical downscaling methods use relationships between the large-scale circulation (predictands) and local-scale surface variables (predictors)..." There is a typo in the names here: the predicTORS are the large-scale circulation fields that are used as input to the downscaling scheme, and the predicTANDS are the output of the downscaling scheme (the local-scale surface variables). The text has these reversed. Also, as written, this sounds like it is including only methods that, for example, use 500 hPa height fields or other "large scale circulation" features to produce the downscaled surface fields. There are entire classes of statistical downscaling methods that use instead the same field for the predictor as the predictand. I.e., they do not use relationships between the large-scale circulation and surface variables, they use relationships between the coarse-scale field and the same fine-scale field. For example, the bias correction with spatial disaggregation (BCSD) scheme of Wood et al. 2002 (JGR-Atmos, v107, doi:10.1029/2001jd000659) and Wood et al. 2004 (Climatic Change v. 62 p. 189), or the bias correction with constructed analogues (BCCA) technique of Hidalgo et al. 2008 (Hidalgo HG, Dettinger MD, Cayan DR, 2008: Downscaling with Constructed Analogues: Daily precipitation and temperature fields over the Unites States. California Energy Commission technical report CEC-500-2007-123. 48 pp.). It would be more inclusive of different techniques to say instead something like "Statistical downscaling methods use relationships between large-scale fields (predictors) and local-scale surface variables (predictands)..." (UNITED STATES OF AMERICA)	Corrected.
621	3	16	4	16	4	Please correct "large scale-circulation (predictands)" with "large scale-circulation (predictors)" (ITALY)	Corrected.
622	3	16	5	16	5	Please correct "local-scale surface variables (predictors) " with "large scale-circulation (predictands)" (ITALY)	changed as suggested.
623	3	16	5	16	5	I suggest to change : "and apply these to climate model data" with " and apply these to equivalent large scale fields (predictors) simulated by the global climate models (ITALY)	Nothing is said about the applicability of any of the methods to impacts applications so it is not appropriate to only mention weather generators. Reference not added.
624	3	16	6	16	8	It should also be mentioned that statistical downscaling methods coupled with weather generators can be used to assess changes in impact studies ref: Semenov, 2009: Impacts of climate change on wheat yields in England and Wales. J. R. Soc. Interface, 6, 343-350. (Pavan, Valentina, ARPA Emilia-Romagna)	There is not space to add additional European references.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
625	3	16	9	16	9	Add the following references concerning statistical downscaling models for the simulation of temperature and precipiation: Kostopoulou E, Giannakopoulos C, Anagnostopoulou Chr, Tolika K, Maheras P, Vafiadis M (2007). Simulating maximum and minimum temperatures over Greece: A comparison of three downscaling techniques. Theoretical and Applied Climatology, 90, 65-82 and Tolika K, Maheras P, Flocas H, Vafiadis M, Arseni – Papadimitriou A (2007). Simulation of Seasonal Precipitation and Raindays over Greece: A statistical downscaling technique based on Artificial Neural Networks (ANNs). International Journal of Climatology, 27, 861-881 (GREECE)	There is not space to add additional European references.
626	3	16	14	16	14	Add the following reference on statistical downscaling of extreme precipitation and temperature: Tolika K, Anagnostopoulou Chr, Maheras P, Vafiadis M (2008). Simulation of future changes in extreme rainfall and temperature conditions over the Greek area: A comparison of two statistical downscaling approaches. Global and Planetary Change 63, 132-151 (GREECE)	There is not space to add additional European references.
627	3	16	14	16	14	Please include the follwing reference after Haylock : Tomozeiu R.,Caccciamani C., Pavan V., Morgillo A.,and Busuioc A 2007: "Climate change scenarios for surface temperature in Emilia Romagna (Italy) obtained using statistical downscaling models" Theor. Appl. Climatol. 90, 25–47 (2007) (ITALY)	changed as suggested.
628	3	16	16	16	16	Need to be clear about the kind of "model" being discussed here so that readers understand that a statistical model is being discussed rather than a dynamical model. (Zwiers, Francis, Environment Canada)	There are several analog methods - e.g., see comment 631. Change not made.
629	3	16	17	16	17	There is only one nonlinear method called "The Analog Model" and not "some analog statistical methods". I suggest to re-write the phrase…"In the case of downscaling extremes based on the analog method, one disadvantage is that the model is not able to simulate events with magnitude outside the range of past observation (ITALY)	It's not said that this is a problem with all statistical methods but with SOME analog methods. The text has been changed to clarify this.
630	3	16	17	16	19	This claim, that statistical methods can not produce larger events than observed may be true for some types of downscaling schemes (e.g., those based on regressions or quantile mapping), but in general, this is false. A weather generator will, and should, simulate larger values than observed if the underlying distributional and structural assumptions are satisfied - although it might simulate extremes that are less pronounced than observed, or that occur less frequently than observed, if the tails are not well represented in the statistical model that is used by the weather generator. (Zwiers, Francis, Environment Canada)	See response to comment 630. It is not appropriate to add a new reference on this point.
631	3	16	17	16	19	The problem of predicting new record-breaking events can be solved through a 're-calibration' of the statistical distribution (local quantile mapping) if one knows the shape of the future PDF (see e.g. Benestad, R.E. 'Downscaling Precipitation Extremes: Correction of Analog Models through PDF Predictions', Theor. & Appl. Clim, Volume 100, Issue 1, DOI: 10.1007/s00704-009-0158-1.(2010).) (NORWAY)	Sentence has been deleted in order to shorten text and avoid repetition.
632	3	16	20	16	20	Please include the follwing reference after Hewitson and Crane, 2006: Busioc A.,Tomozeiu R., Cacciamani C., 2008" Statistical downscaling model based on canonical correlation analysis for winter extreme precipitation events in Emilia-Romagna region", Int. J. Climatol., 28, 449-464 (ITALY)	This text has been deleted from this paragraph and this issue is now discussed in Section 3.2.3.2. In line with comment 633, it is now noted that this is also an issue for dynamical models - supported by a reference to Christensen et al., 2007. There is not space in this chapter to discuss stationarity in full - but problems can be reduced by careful selection and use of predictors. Recent studies, e.g., using RCM output as proxy observations indicate that it is not a major issue for some statistical downscaling methods.
633	3	16	20	16	22	This seems to be the most fundamental drawback of statistical methods, but it seems to get lost at the end of a long paragraph that is part of a long discussion of downscaling. (UNITED STATES OF AMERICA)	This text has been deleted from this paragraph and this issue is now discussed in Section 3.2.3.2. It is now noted that this is also an issue for dynamical models - supported by a reference to Christensen et al., 2007.
634	3	16	21	16	21	Delete the word 'statistical' at the beginning of the line – this limitation is not just a caveat for empirical downscaling, but all climate models. Many parameterisation schemes are essentially statistical models too, calibrated on empirical data from the past. Non-stationarities in the parameterisation schemes furthermore represent a more severe problem, as biases feed back to the non-linear computations ('slippery slope'). For empirical downscaling, the effect of non-transient bias will be more limited. This can, however, be reduced if the statistical models in empirical downscaling are based on physics, and can be tested through various types of evaluation (using observations for the past, and models for the past as well as future). (NORWAY)	changed as suggested. (and shifted to 3.2.3.2)
635	3	16	22	0	0	Should it specify : future "mechanistic changes in regional (or global) climate" ? (International Petroleum Industry Environmental Conservation Association (IPIECA))	Disagree with this comment. There are many examples of validation in the published literature. No change made.
636	3	16	23	16	25	While this is true, is it not even more important to note the dearth or absence of evaluations of change-downscaling against observations? (UNITED STATES OF AMERICA)	Reworded.
637	3	16	27	16	28	I think this is too strong a statement. The high frequency output might not be freely available, but I suspect that most groups do archive it. CMIP5 has also asked for a great deal of high frequency output - so I expect that the public offering of high frequency output will change substantially over the next year or so. (Zwiers, Francis, Environment Canada)	Reworded.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
638	3	16	27	16	28	Data is quite often archived at 6-hourly resolution from RCMs. (But less often on higher temporal resolutions.) (SWEDEN)	It is clear that this text applies to non cloud resolving RCMs. An earlier paragraph talks about improvements seen with non-hydrostatic models and includes a citation to Kanada et al 2010. No change made.
639	3	16	27	16	34	Some results （Sasaki et al., 2005; Wakazuki et al., 2008; Sasaki et al.,2008; Kanada et al., 2010) showed that high-resolution RCMs reproduced precipitation for shorter timesteps (1 hourly - 3 hourly) with sufficient accuracy. * Sasaki, H., K. Kurihara, I. Takayabu, 2005: Comparison of climate reproducebility 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 Reserach Institute)	Reference not added but the urban drainage application is now mentioned.
640	3	16	34	16	34	Statistical downscaling to 10-minutes precipitation extremes, based on a 108-year time series (since 1898) of 10-minutes precipitation intensities at Brussels, Belgium, was done by (+ comparison of a set of downscaling methods): Willems P., Vrac M. (2011), 'Statistical precipitation downscaling for small-scale hydrological impact investigations of climate change', Journal of Hydrology, 10.1016/j.jhydrol.2011.02.030. Such downscaling to short-duration precipitation extremes is required for impact analysis on urban drainage (e.g. sewer floods). (Willems, Patrick, Katholieke Universiteit Leuven)	A short addition to the paragraph has been made as suggested including the two references.
641	3	16	35	16	35	A short paragraph could be added for statistico-dynamical methods and cascading techniques that are mentioned p44, l24-27. This includes the method consisting in downscaling RCM projections using non-hydrostatic mesoscale models on selected cases that are chosen to be representative of the occurrence of extreme events. As an example you could mention there Bender et al. 2010 giving the description that is now p44. You could also mention something like "As an example of statistico-dynamical downscaling, Beaulant et al. (2011) have used a two-step approach based first on the selection of representative large scale circulation patterns propitious to high precipitating events in Mediterranean regions using statistical methods, followed by dynamical downscaling of a coupled atmosphere-ocean regional climate model by means of a NH model. After the selection of 21 cases representative of present and future climate conditions under SRES A2 scenario, they show that the method allow to infer some conclusions about the evolution of the geographical location, occurrence and intensity of these intense events.". The complete reference is: Beaulant, A.-L., B. Joly, O. Nuissier, S. Somot, V. Ducrocq, A. Joly, F. Sevault, M. Deque, D. Ricard, 2011: Statistico-dynamical downscaling for Mediterranean heavy precipitation. Quaterly Journal of the Royal Meteorological Society, on line, doi: 10.1002/qj.796. (PLANTON, Serge, Météo-France)	Deleted.
642	3	16	36	16	36	Suggest to delete "For reasons of space" -- it's probably not just space that prevents to expand the assessment to finer scales. While several countries now do have higher resolution information, it will be still be difficult to provide a comprehensive global assessment at those scales. (Stocker, Thomas, IPCC WGI TSU)	Change made.
643	3	16	36	16	36	scale -> scales (Brönnimann, Stefan, University of Bern)	It is not appropriate to include weblinks in this assessment report.
644	3	16	36	16	39	I think it would be helpful to the reader if some pointers were given (reference or web link) in how to find out more about these national assessments eg http://ukclimateprojections.defra.gov.uk/content/view/12/689/ (Brown, Simon, The Met Office Hadly Centre)	It is not possible to include all countries/regions here. Nonetheless, North America (since comparable work has been done in Canada) is now mentioned. Also Austraia.
645	3	16	37	0	0	Chap 3, page 16, line 37: Europe has been admirably foresighted in producing regional climate projections, but it would be incorrect to imply that they are the only ones doing this activity, or that all the regional climate information is being developed at the national scale. For example, there has been a long history of funding these kinds of projections in western North America. It would be significantly more correct to say something like "Several regions, such as Europe and the western United States, have, however, developed their own regional projections, including information about extremes, and a range of other high-resolution information and tools are available from national or regional weather and hydrological services and academic institutions to assist users and decision makers." (UNITED STATES OF AMERICA)	No judgement is made/implied. No change made.
646	3	16	37	16	39	This sounds like a positive statement here, whereas in practice the fact that each country in Europe has developed their own national projections has clear drawbacks for decision makers, because they are generally not consistent at the geographical borders. (Klein Tank, Albert, KNMI)	Space limits strongly restrict the possiblility to include references at that level of detail.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
647	3	16	41	17	44	In respect to uncertainty in climate change projections a small paragraph could be included, describing the potential for stochastic physics and climate modelling included in Palmer and Williams, (2010). REFERENCE: Palmer T.N. and P.D Williams, 2010: Introduction. Stochastic physics and climate modelling. Phil. Trans. R. Soc. A 2008 366, 2419-2425. doi: 10.1098/rsta.2008.0059 (GREECE)	Uncertainties in impact models are assessed in chapter 4.
648	3	16	41	18	59	One major source of "uncertainty" is in the "postprocessing" of GCM outputs to impact metrics, often involving an "impact model," which might be a complex "index" or a hydrologic (or other process) model. One small example of this is given by Milly, P. C. D., Krista A. Dunne, 2011: On the Hydrologic Adjustment of Climate-Model Projections: The Potential Pitfall of Potential Evapotranspiration. Earth Interact., 15, 1–14. The title of this section implies that the analysis of uncertainty stops at the GCM output. We suggest thinking outside of that box, more than the brief mention on page 17, lines 31-36. (UNITED STATES OF AMERICA)	Line 43 was rewritten including a comment about the future emissions changes driven by human activities and represented by multiple emissions scenarios.
649	3	16	43	0	0	Chap 3, page 16, line 43: The climate research community needs to better convey the reality that the largest source of uncertainty for mean global surface temperature by the end of the century arises from humanity's activities (Hawkins and Sutton, BAMS, 2009). I.e., it is what we decide to do. This is a very different kind of uncertainty than that arising from model problems or natural internal climate variability. It would be appropriate to add a sentence at the end of the paragraph that starts on line 43 pointing this out. For example, "By the end of the century, the largest contributor to uncertainty in global temperatures is what measures, if any, humanity takes to address the issue (Hawkins and Sutton, 2009)." (Pierce, David, Scripps Institution of Oceanography/University of California, San Diego)	Line 43 was rewritten including a comment about the future emissions changes driven by human activities and represented by multiple emissions scenarios.
650	3	16	43	16	46	It would be helpful to indicate that, for the first step, uncertainty about future emissions is driven by socioeconomic development and is represented through the use of multiple emissions scenarios. (IPCC WGII TSU)	"Aerosol" was replaced by "Aerosol precursor"
651	3	16	44	16	44	Here, "aerosol precursor emissions" are probably referred to, rather than "aerosol emissions". Albeit soot would be about the latter. (SWEDEN)	"Aerosol" was replaced by "Aerosol precursor"
652	3	16	44	16	44	Replace “and aerosol” by “or aerosols and their precursors” since aerosols are not always directly emitted (“or” is an alternative to “and” that cannot be repeated). (PLANTON, Serge, Méto-France)	"At each step" was removed from the sentence.
653	3	16	45	16	45	This is not “at each step” since there is no direct impact of internal climate variability nor of errors in the model representation of Earth system on the emissions. (PLANTON, Serge, Méto-France)	The sentence includes some of important small-scale processes that models do not represent accurately. Cryospheric processes not relevant to this statement.
654	3	16	52	0	0	Should it also include (e.g. cryospheric processes …) (International Petroleum Industry Environmental Conservation Association (IPIECA))	Agreed. The sentence was modified to clearly mention RCMs.
655	3	16	55	16	57	The structural errors in GCMs propagate to a greater degree to RCMs than empirical-statistical downscaling (ESD) because ESD tie the results directly to observations. Often ESD will focus on anomalies, and can therefore bypass biases in the annual cycle. (NORWAY)	Agreed. The reference was changed.
656	3	16	61	16	61	The authoritative current reference would be Knutson et al., 2010 (Zwiers, Francis, Environment Canada)	Disagree. The fact that models could run at that high resolution, does not mean that model outputs should be delivered to planners at that resolution. It will depend on each sector planner needs.
657	3	16	61	17	5	Assuming that the downscaling is possible to the levels of 1km, it would be an overload of data for planners. This can be related to value addition point given in lines 19-22 on the same page (17) (GARG, AMIT, INDIAN INSTITUTE OF MANAGEMENT AHMEDABAD)	Done
658	3	16	63	16	63	The word "global" should be added before "numerical weather prediction". 1 km horizontal resolution is not far beyond the capabilities of some regional weather forecasting centres. (Global Climate Observing System Steering Committee)	Done
659	3	16	63	16	63	Add “global” before “weather prediction models”. Limited-area NH models are indeed also used for numerical weather prediction with an horizontal resolution of a few km. (PLANTON, Serge, Météo-France)	The sentence was modified as follows: "Since many extreme events, such as those associated with precipitation, occur…
660	3	17	7	0	0	“Since many extreme events occur at rather small temporal and spatial scales where climate simulation skill is currently limited…” This statement may be true for precipitation extremes, but is not entirely correct for temperature extremes, and is also mis-represented in Box 3.1. The space scale of heat waves is on the order of several hundreds of kilometers (e.g. Meehl and Tebaldi, 2004, Science), well within the simulation capabilities of current AOGCMs. Temperature is more uniformly distributed in space compared to precipitation, making the study of present and future heat extremes very feasible with current AOGCMs. (UNITED STATES OF AMERICA)	Paleoclimate records help to assess past observed changes but for future changes their contributions is rather limited and not obvious.
661	3	17	8	17	9	Again, paleo/proxy data provide physical evidence to help answer many such questions. (Jarrett, Robert, USGS)	The text was improved.
662	3	17	19	17	20	Seems to be saying the value of downscaling hasn't been assessed - is this the intended meaning of the statement? (UNITED KINGDOM OF GREAT BRITAIN AND NORTHERN IRELAND)	That particular text was removed.
663	3	17	19	17	21	Has anyone suggested that the statistical models used for statistical downscaling have overfitted the available observations? If so, please provide references and an assessment. As it stands, this statement casts doubt without providing substantiation. (Zwiers, Francis, Environment Canada)	The text was improved.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
664	3	17	21	17	21	Suggest replacing "credible" with "reliable". An overfitted model might generate a credible predictions, but it won't be reliable. (CANADA)	The reference was updated.
665	3	17	25	17	25	An other publication related to the topic is "Déqué et al 2011". This paper updates the results obtained with the PRUDENCE regional climate projections over Europe with those of the ENSEMBLES European project. It concludes in particular that for A1B scenario and 2021-2050 time-slice, RCMs and GCMs used for boundary conditions have similar contribution to the spread of projections of temperature (summer and winter) and of precipitation (winter). For summer precipitation, the contribution of RCMs dominates the spread of results. The complete reference is: Déqué, M., S. Somot, E. Sanchez-Gomez , C. M. Goodess , D. Jacob, G. Lenderink , and O. B. Christensen, 2011: The spread amongst ENSEMBLES regional scenarios: Regional Climate Models, driving General Circulation Models and interannual variability. Accepted in Climate Dynamics, doi: 10.1007/s00382-011-1053-x. (PLANTON, Serge, Méto-France)	Such aspect can already be inferred from the current sentence text.
666	3	17	26	0	0	I suggest to add also the uncertainties that arise from a considerable sensitivity of SDS outputs to the selection of the statistical model and its parameters, as well as of the set of predictors. (Huth R., 2004- Sensitivity of local daily temperature change estimates to the selection of downscaling models and predictors. J Climate 17: 640-652) (ITALY)	Agreed. That concern was deleted.
667	3	17	26	17	26	The concern about domain size needs explanation. Domain size is an issue for RCMs, but it is not apparent to me what the issue is for statistical methods. (Zwiers, Francis, Environment Canada)	Agreed. The comment was included in the revised version of the paragraph.
668	3	17	28	17	29	Apart from highlighting the factors which introduce uncertainty, I think that it could be noted that for many regions of the world no downscaled information exists at all. This is mentioned in the margin of line 43 on p18 only. (Klein Tank, Albert, KNMI)	Agreed. Similar conclusion is arrived at lines 35 and 36
669	3	17	31	17	36	Ho et al (submitted) show that the choice of bias correction method has a significant impact on the future chages that are derived showing that caution is needed when correcting. (Calibration strategies for climate model projections, Chun Kit Ho, David Stephenson, Matthew Collins, Chris Ferro, Simon Brown) (Brown, Simon, The Met Office Hadly Centre)	Space limits strongly restrict the possiblility to include references at that level of detail.
670	3	17	34	0	0	Chap 3, page 17, line 34-36: Should have references to Maurer (2007) and Maurer et al. (2010) here. Full refs are: Maurer, E. P., 2007: Uncertainty in hydrologic impacts of climate change in the Sierra Nevada, California, under two emissions scenarios. Clim Change 82:309-325 Maurer, E.P., H.G. Hidalgo, T. Das, M.D. Dettinger and D.R. Cayan, 2010: The utility of daily large-scale climate data in the assessment of climate change impacts on daily streamflow in California. Hydrol. Earth Syst. Sci., 14, 1125-1138, doi:10.5194/hess-14-1125-2010. (UNITED STATES OF AMERICA)	Space limits strongly restrict the possiblility to include new references.
671	3	17	36	17	36	An other publication could be mentioned before Piani et al.: "Déqué, 2007". In this paper a "quantile-quantile" correction of model projections is proposed and extreme dauly precipitation are analysed. The complete reference is: "Déqué, M., 2007: Frequency of precipitation and temperature extremes over France in an anthropogenic scenario: model results and statistical correction according to observed values. Global and Planetary Change, 57(1-2), 16-26." (PLANTON, Serge, Méto-France)	Paragraph deleted.
672	3	17	39	17	39	It would be best to avoid use of "likely" in this sentence since (presumably) likelihood language per the AR5 Guidance Note is not intended and since these factors apply to future climate changes that could be associated with a range of probabilistic "likelihoods." (IPCC WGII TSU)	Done
673	3	17	48	17	49	Reanalyses could be added to the list of observations, process understanding, a hierarchy of climate models, ... (Global Climate Observing System Steering Committee)	Noted. "possible" was removed.
674	3	17	50	17	50	"possible range of plausible responses"? Suggest omitting either possible or plausible. (UNITED STATES OF AMERICA)	The statement refers to one of the usual ways that multi-member ensembles can be generated.
675	3	17	53	17	53	The statement about different "initial condition" may be misleading since climate models (mostly) rely on changing boundary conditions. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Done
676	3	17	59	17	59	Suggest inserting "available CMIP3" ahead of "GCM simuations" so that is clear which simulations are being discussed. (Zwiers, Francis, Environment Canada)	Such issue will be asessed in AR5. The reference to CMIP5 dataset has been omitted.
677	3	18	1	18	7	It would be helpful to the reader if some sort of comment is made on what improvements we hope to get from CMIP5 such as greater daily diagnostics, diagnostics for storm tracking, etc (Brown, Simon, The Met Office Hadly Centre)	Agreed. The reference to CMIP5 was omitted.
678	3	18	5	18	5	This sentence is a bit dated. Some centers have already completed their CMIP5 simulations, and the machinery that is required to disseminate runs is creaking into action. By the time the SREX report is out, CMIP5 (or at least, the model simulation phase) will largely be a fact. (Zwiers, Francis, Environment Canada)	These issues are taken into account in assessing confidence and likelihood using the approach described in Section 3.1.5. This is now stated at the end of the paragraph and the earlier section referred to.
679	3	18	9	18	18	This overlaps and interferes with the earlier discussion about confidence in section 3.1.5. (Klein Tank, Albert, KNMI)	The sentence was modified as follows: "...or insufficient number of simulations from different models or ..."
680	3	18	11	18	12	It is unclear what is meant by ' insufficient number of simulations'. If this just means insufficient simulations from a given model or groups of models, then this type of uncertainty is due to internal variability, which has already been covered. If this is referring to simulations from an insufficient number of models (i.e. not enough samples of model variability), then this is referring to model uncertainty, which is a separate source of uncertainty which should be mentioned explicitly. (CANADA)	The sentence has been modified to: "... or insuficient understanding of the physical..."

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
681	3	18	12	18	12	delete "lack of" (double negation, probably typo) (Neu, Urs, Swiss Academy of Sciences)	The sentence has been modified to: "... or insuficient understanding of the physical..."
682	3	18	12	18	12	"insufficient lack of understanding" should be reduced to "lack of understanding". (PLANTON, Serge, Méto-France)	We have only assessed the published literature (to do anything else would be impossible). We use this term to make the distinction from insufficient evidence.
683	3	18	13	18	13	The category of "insufficient litterature" suggests that analyses are available, but not published. Later in the same paragraph, reference is made to analyses not existing (because of lack of data). Thus, perhaps it is lack of analyses rather than lack of PUBLISHED analyses that is the point to make here? (SWEDEN)	That metric is mentioned in lines 23 and 24
684	3	18	20	18	21	It think this is a bit too much of a simplification. For example, a key metric used for precipitation, both in Chapters 10 and 11 of the WG1 AR4, was agreement in the sign of the projected change. (Zwiers, Francis, Environment Canada)	Space limits strongly restric the level of detail that we can include
685	3	18	20	18	21	We introduce here the project in Japan focusing on estimating all CMIP3 AO-GCM's results. Ref. Seiki, A., Y. N. Takayabu, T. Yasuda, N. Sato, C. Takahashi, K. Yoneyama, and R. Shirooka, 2011: Westerly wind bursts and their relationship with ENSO in CMIP3 models. J. Geophys. Res., 116, D03303. Kosaka, Y., and H. Nakamura, 2010a: Mechanisms of meridional teleconnection observed between a summer monsoon system and a subtropical anticyclone. Part I: The Pacific-Japan pattern. J. Climate, 23, 5085-5108. Kosaka, Y., and H. Nakamura, 2010b: Mechanisms of meridional teleconnection observed between a summer monsoon system and a subtropical anticyclone. Part II: A global survey. J. Climate, 23, 5109-5125. [pdf] Kwon, Y.-O., M. A. Alexander, N. A. Bond, C. Frankignoul, H. Nakamura, B. Qiu, and L. Thompson, 2010: Role of Gulf Stream and Kuroshio-Oyashio systems in large-scale atmosphere-ocean interaction: A review. J. Climate, 23, 3249-3281. Miyasaka, T., and H. Nakamura, 2010: Structure and mechanisms of the Southern Hemisphere summertime subtropical anticyclones. J. Climate, 23, 2115-2130. Nakamura, H., T. Miyasaka, Y. Kosaka, K. Takaya, and M. Honda, 2010: Northern Hemisphere extratropical tropospheric planetary waves and their low-frequency variability: Their vertical structure and interaction with transient eddies and surface thermal contrasts. in "Climate Dynamics: Why Does Climate Vary?" (D. Sun, F. Bryan, Eds.), Geophys. Monogr., 189, 149-179. Nishii, K., H. Nakamura, and Y. J. Orsolini, 2010: Cooling of the wintertime Arctic stratosphere induced by the Western Pacific teleconnection pattern. Geophys. Res. Lett., 37, L13805. Noda, A., 2010: A General Three-Dimensional Transformed Eulerian Mean Formulation. SOLA, 6, 85-88. Ohba, M., D. Nohara, and H. Ueda, 2010: Simulation of Asymmetric ENSO Transition in WCRP CMIP3 Multimodel Experiments. J. Climate, 23, 6051-6067. Ohba, M., and H. Ueda, 2010: A GCM study on effects of continental drift on tropical climate at the early and late Cretaceous. J. Meteor. Soc. Japan, 88, 869-881. Sampe, T., H. Nakamura, A. Goto, and W. Ohfuchi, 2010: Significance of a midlatitude oceanic frontal zone in the formation of a storm track and an eddy-driven westerly jet. J. Climate, 23, 1739-1814. Seiki, A., Y. N. Takayabu, K. Yoneyama, and R. Shirooka, 2010: The impact of trade surges on the Madden-Julian Oscillation under different ENSO conditions. SOLA, 6, 49-52. Takayabu, Y. N., S. Shige, W.-K. Tao, and N. Hirota, 2010: Shallow and deep latent heating modes over tropical oceans observed with TRMM PR Spectral Latent Heating data. J. Climate, 23, 2030-2046. Tanimoto, Y., T. Kajitani, H. Okajima and S.-P. Xie, 2011: A peculiar feature of the seasonal migration of the South American rain band. J. Meteor. Soc. Japan 88, 79-90. (Takayabu, Izuru, Meteorological Research Institute)	Agreed. A comment regarding commonbiases was included.
686	3	18	21	18	25	The shortcoming of this definition of robustness (model agreement) is that it does not take account of possible common biases amongst models. This issue should at least be mentioned. (CANADA)	The sentence was deleted
687	3	18	22	18	22	Was this actually stated in this way in the AR4? The word "robust" is used only a few times in WG1 AR4 Ch 10 (Meehl et al 2007), but not in the way it is used here. It was used often in Chapter 11, but without explicit definition. Note that "robust" is not a calibrated term. (Zwiers, Francis, Environment Canada)	The comment was included in the text. See #686.
688	3	18	25	18	27	There is a subtle but important aspect may be missed here. Different models should of coarse represent the same physics and predict the same 'signal'. Hence, the models will and should not be independent of each other, as all are expected to embed the same core features. But to use them for investigating the uncertainty, it is important that their errors are independent. We think it's fair to say that the uncertainties associated with these models are not well known, and we don't know if they all suffer from the same flaws. (NORWAY)	Yes - expert judgement is also used. Thus model agreement is a necessary but not sufficient condition. A sentence has been added to explain this in the context of the SREX figures.
689	3	18	29	18	30	Model counting to determine "likely" doesn't seem all that cautious. Shouldn't expert judgement assessing all of the uncertainties discussed in 3.2.3.2 come into play as well? (Zwiers, Francis, Environment Canada)	Expert judgement is also used. Thus model agreement is a necessary but not sufficient condition. A sentence has been added to explain this in the context of the SREX figures. There are cases in the assessment where likelihood statements have been 'downgraded' from what the maps indicate based on this expert judgement.
690	3	18	29	18	30	Likely changes indicated when at least 66% of the models agree on the sign of change? This seems much less stringent a criterion than is proposed in the procedure at top of p. 3 of Ch. 3. Therefore this statement should be removed and the procedure not used. One instance I found where it seemed to be used in the report was not in Ch. 3, but in Ch. 4, p. 21, line 38-43. (UNITED STATES OF AMERICA)	Space limits strongly restrict the detail of the discussion included

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
691	3	18	32	18	33	Burke and Brown 2008 also analysed the MME as well as comparing with results from the perturbed physics ensemble. (Brown, Simon, The Met Office Hadly Centre)	A caveat has been added at the end of this sentence noting this challenging issue. It includes citation of Knutti et al., 2010 (already cited elsewhere in the chapter). Space constraints preclude inclusion of the other suggested references.
692	3	18	32	18	35	Some studies had proposed methods to used observational constraints to construct probability distributions. However, it should be noted that recent studies suggested that it is difficult to determine metrics which assess the reliability of projections [Whetton et al., 2007; Abe et al., 2009; Giorgi and Coppola, 2010; Knutti et al., 2010; Raisanen et al., 2010; Shiogama et al., 2011]. Abe, M., H. Shiogama, J. C. Hargreaves, J. D. Annan, T. Nozawa, and S. Emori (2009), Correlation between Inter-Model Similarities in Spatial Pattern for Present and Projected Future Mean Climate, Sola, 5, 133-136. Giorgi, F., and E. Coppola (2010), Does the model regional bias affect the projected regional climate change? An analysis of global model projections, Climatic Change, 100(3-4), 787-795. Knutti, R., R. Furrer, C. Tebaldi, J. Cermak, and G. A. Meehl (2010), Challenges in Combining Projections from Multiple Climate Models, Journal of Climate, 23(10), 2739-2758. Raisanen, J., L. Ruokolainen, and J. Ylhaisi (2010), Weighting of model results for improving best estimates of climate change, Climate Dynamics, 35(2-3), 407-422. Shiogama H., Emori S., Hanasaki N., Abe M., Masutomi Y., Takahashi K., Nozawa T. (2011) Observational constraints indicate risk of drying in the Amazon basin, Nature Communications, 2, Article number 253, doi: 10.1038/ncomms1252. Whetton, P., I. Macadam, J. Bathols, and J. O'Grady (2007), Assessment of the use of current climate patterns to evaluate regional enhanced greenhouse response patterns of climate models, Geophysical Research Letters, 34(14). (Shiogama, Hideo, National Institute for Environmental Studies)	Three sentences have been included on UKCP09. The method is briefly outlined and Murphy et al's 2009 own description of the 'limitations'/assumptions of the approach is included. Space precludes a longer critique.
693	3	18	42	18	42	The statement on Murphy et al (2009) seems rather understated. While there are different views on the result and its utility (there are some very strong views), Murphy et al (2009) employed what is, arguably, the most comprehensive approach to date for quantifying the influence of the cascade of uncertainties that affect downscaled climate change projections. This was done using an extremely complex hierarchical Bayes model, and perhaps represents a bit of a road map for how this might be done in the future. It would presumably be useful if the Chapter expressed a view on the approach. (Zwiers, Francis, Environment Canada)	Space limits strongly restrict the detail of the discussion included
694	3	18	42	18	42	I propose to add one sentence on a recent work that concerns a publication on probabilistic estimates of climate change. After "(Murphy et al., 2009).": "Other estimates of probability density functions of 30 year seasonal mean climate change, at the location of three European cities, show low sensitivity to the choice of weights applied to each of the model of the ENSEMBLES multi-model projections (Déqué and Somot, 2010).". The complete reference is: "Déqué, M., and S. Somot, 2010: Weighted frequency distributions express modelling uncertainties in the ENSEMBLES regional climate experiments. Climate Research, 44(2-3), 195-209.". (PLANTON, Serge, Méto-France)	This sentence has been moved to Section 3.2.3.2. The first part of the paragraph to which it has been moved notes the uncertainties and potential limitations associated with stationarity issues - both in the context of statistical downscaling and parametrization in dynamical models.
695	3	18	42	18	43	"despite being still restricted in terms of geography" Should we add that they're restricted by the stationarity assumption? We would think stationarity is a more severe restriction. (UNITED STATES OF AMERICA)	Disagree. The paragraph only describes the implementation of coordinated modeling activities. It is not for us to decide which is good/bad approach
696	3	18	45	18	59	The rationale for running dynamical downscaling in coordinated simulations that cover the globe could be elaborated, as it may not be clear to all readers why it would not be more effective simply to run a global model with the resolution of the globally complete set of regional downscaling simulations. (Global Climate Observing System Steering Committee)	Agreed. The comment was included in the paragraph.
697	3	18	45	18	59	It is important to point out that ensemble runs with RCMs involve a limited number of driving GCMs, and hence do only subsample the space of uncertainty. Empirical downscaling, on the other hand, have been applied to larger ensembles of GCMs, but even so, these only give a crude estimate of the uncertainties associated with model differences and internal variability. When the same GCMs are used to as boundary conditions to many RCMs, one explores the uncertainties associated with RCM differences, not with internal variations and GCM representations which we believe are more important. (NORWAY)	Space limits strongly restrict the detail of the discussion included
698	3	18	51	18	51	Aroud Japan area, we perform the comparison between the dynamical and the statsitical downscaling and estimete each advantage. Ref. Iizumi, T., M. Nishimori, K. Dairaku, S. A. Adachi, and M. yokozawa, 2011: Evaluation and intercomparison of downscaled daily precipitation indices over Japan in present-day climate: Strengths and wakness of dynamical and bias-correction-type statisitical downscaling methods. Journlal of Geophysics Research, 116, D01111, doi:10.1029/2010JD014513. (Takayabu, Izuru, Meteorological Research Institute)	Space limits strongly restrict the detail of the discussion included
699	3	18	51	18	51	Yokoi, S., and Y. N. Takayabu, 2010: Environmental and external factors in the genesis of tropical cyclone Nargis in April 2008 over the Bay of Bengal. J. Meteor. Soc. Japan, 88, 425-435. (Takayabu, Izuru, Meteorological Research Institute)	Noted. "Natural variability" was replaced by "internal variability"
700	3	18	55	18	55	Natural variability includes the forced response to volcanoes and solar variability, whereas internal variability is variability arising from the dynamics of the climate system itself. Which is meant here? Secondly, it is not surprising that natural variability in regional precipitation is important on multi-annual timescales. It would be expected to be important even on multi-decadal timescales. Even in global mean temperature, internal variability is important on multi-annual timescales. (CANADA)	Reject. This is about relative accuracy - initialisation would also presumably improve global scale simulations.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
701	3	19	5	19	14	There is no discussion here of the fact that the AR4 model runs are projections and were not hind casts or forecasts. None of the models were initialized. It is readily shown that regional climate will not be predictable without such specific initialization of fields to give realistic sea surface temperatures, (even if models do do predictions well yet). The discussion should reflect this. (UNITED STATES OF AMERICA)	Reject. The box does provide the guidance that readers and polictymakers should not assume that all projections of all climate change variables can be made with the same level of confidence, and the factors that control this variation in confidence. This is what is indicated in the title of the Box.
702	3	19	5	19	50	It would be helpful if, as well as arming readers with information about different sources of uncertainty, this box could also give readers guidance on the interpretation of projections of extremes. Also, the box does not really deliver as promised in the title. The first paragraph discusses why we have less confidence in attribution of changes in the mean state on subcontinental scales but doesn't talk about projections. The box then switches to discussion about uncertainties in projections of changes in extremes, and never really resolves how readers show consider projections of changes in extremes - except that they are told to be wary - which is not all that helpful. Projected changes in extremes do have all kinds of problems - but there are also some fundamental physical reasons that give us confidence, at least in qualitative aspects of changes of extremes, and it would be useful to bring some of those qualitative reasons for confidence into the discussion. At the moment, the reader is not getting much guidance. I have a bit the same reaction to 3.2.3.2 as well. (Zwiers, Francis, Environment Canada)	Agreed.
703	3	19	7	19	9	Change "decadal" to "temporal" in line 7 and "decadal" to "multi-decadal" in line 9 (UNITED STATES OF AMERICA)	Agreed. See comment 703.
704	3	19	10	19	10	decadal variations: Do you mean multidecadal? (Brönnimann, Stefan, University of Bern)	Replace "between" with "amongst" - this word may be causing reviewer confusion.
705	3	19	12	19	13	I disgree - I think this is a misinterpretation of WG1 AR4 Figure 9.12. If you look at Fig 9.12 you see that there appear to be biases in some regions (e.g., AMZ, WAF) but otherwise that there is very good consistency. Not being able to reproduce internal variations is not a demonstration of inconsistency because we should not expect to be able to predict internal variations. (Zwiers, Francis, Environment Canada)	See response to 705. Agree with last sentence in comment - add extra sentence to first paragraph on this.
706	3	19	16	19	17	The authors seem to be mixing up inconsistency with internal variability. All we can hope to do is reproduce the statistics of the latter, and judging from Fig 9.12, and also the power spectra of continental mean temperatures that were displayed in WG1 AR4 Ch9, it appears that models do generally reproduce the statistics of internal variability reasonable well. Given the internal variability, there is then little evidence that the observed and simulated changes are inconsistent at the regional scales shown in Fig 9.12, execpt in a few regions where there is some evidence of bias. Another point is that internal variability is not larger at smaller scales - but rather, that there is less opportunity to remove its effects through spatial averaging. (Zwiers, Francis, Environment Canada)	Correct, but misses the point of the Box.
707	3	19	16	19	24	As commented on earlier, this discussion of doing detection-attribution of a global type of change by looking only within some very small domain makes no logical sense. Fine to say that the changes on smaller scales, at least early in this process (so early this century) can get more easily obscured by the inherent variability of the system and the interactions of global change with it, but it does not seem appropriate to me to be saying that a statistical test that is inappropriate for the problem is not showing a result. We clearly know that if one changes the global climate, the various regional parts of the global climate will be changed--the system is interconnected and thi shas to be the case. (MacCracken, Michael, Climate Institute)	See comments 705, 706.
708	3	19	21	19	24	I think this is an incorrect interpretation. Given the internal variability, there appears to be little evidence that the observed and simulated changes are inconsistent at the regional scales shown in Fig 9.12, execpt in a few regions where there is some evidence of bias. (Zwiers, Francis, Environment Canada)	Noted. But does not add to Box, so do not add. Evidence is that precipitation is harder to simulate than temperature.
709	3	19	26	19	31	Kanada et al., 2010 showed that frequency, distribution and intensity of heavy precipitation are well reproduced by a high-resolution RCM comparing with observation. * 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)	Much of the main text is about the last sentence in this comment. The Box addresses just a single issue - does our ability to simulate and predict climate change vary between variables and scales.
710	3	19	28	19	31	The term 'relatively well' is not well-defined, and perhaps not very representative. On p.17, L7-8, the draft states that the climate simulations is currently limited for small temporal and spatial scales on which many extreme events occur. Again, this relates to the question of 'skilful scale'. The question is whether events such as the 2003 European summer heat wave and the cold winters 2009/2010 and 2010/2011 are well represented in these models, events with fairly extensive area. We think that this boils down to the model's ability to reproduce the frequency blocking events, their duration, and severity. We understand that the representation of such phenomena is sensitive to spatial resolution, and that the current GCMs are much too coarse. Hence, the question of whether the models are expected to provide a good representation of heat waves and cold snaps. At least, the evaluation of the models' ability to do reproduce extreme event characteristics and trends must be documented and referred to. (NORWAY)	This is well covered by Knutson et al reference already.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
711	3	19	37	0	0	To illustrate the uncertainty, the recent paper of Grossmann and Morgan (2011) could be mentioned. Grossmann I. and M.G. Morgan (2011) Tropical cyclones, climate change, and scientific uncertainty: what do we know, what does it mean, and what should be done? Climatic Change, DOI 10.1007/s10584-011-0020-1 (International Petroleum Industry Environmental Conservation Association (IPIECA))	Check references.
712	3	19	41	19	42	Please verify if three references are cited correctly. Hawkins and Sutton 2009 examines temperature while Hawkins and Sutton 2011 deals with precipitation so 2011 paper needs to be cited in the 2nd part. (CANADA)	Agreed. Add "generally" in this last sentence?
713	3	19	50	0	0	Not generally true. For example, confidence in changes in precipitation extremes is higher than confidence in changes in the associated seasonal precipitation. (Klein Tank, Albert, KNMI)	The first two changes have been made. The third will be done at the copy editing stage.
714	3	19	55	0	0	Table 3.3: It might be useful to include the abbreviated region names from Figure 3.2 within the Table, eg, W. North America (WMA). The differentiation of R, R, R in the caption is not obvious. You might increase clarity in this table by shading regions and subregions with different background colours. (Stocker, Thomas, IPCC WGI TSU)	Burke and Brown 2008 has been added. Inconsistencies in other modelling studies do not warrant changing the confidence level.
715	3	19	55	0	0	Table 3.3 Row- S Europe & Mediterranean Column- 5. Burke and Brown 2008 should be referenced and their multi-ensemble multi variable analysis indicates that of all the places on the globe the Med has the strongest indication of increased future risk of drought. Medium confidence soed not seen to reflect this clear signal. (Brown, Simon, The Met Office Hadly Centre)	Burke and Brown 2008 has been added. Inconsistencies in other modelling studies do not warrant changing the confidence level.
716	3	19	55	0	0	Table 3.3 Row- S Africa Column- 5. similar comment to the above. Burke and Brown 2008 should be referenced and their multi-ensemble multi variable analysis indicates that Southern Africa has the second strongest indication of increased future risk of drought (Brown, Simon, The Met Office Hadly Centre)	Burke and Brown 2008 has been added. The medium confidence level applies to S Australia and New Zealand.
717	3	19	55	0	0	Table 3.3 Row Whole Australia Column 5. Burke and Brown 2008 would support low confidence in increases in drought not low to medium. Should be referenced. (Brown, Simon, The Met Office Hadly Centre)	All terms used in the table are now explicitly defined in the caption.
718	3	19	59	0	0	In the caption of Table 3.3, both bold G and bold R indicate 'multi-GCM'. A difference between them is not clear. (JAPAN)	All terms used in the table are now explicitly defined in the caption.
719	3	19	59	19	59	“G: multi GCM, R: multi-GCM” Should R be single RCM forced by multiple GCMs? (UNITED STATES OF AMERICA)	Separate descriptions of extremes for various continents and regions have been presented in tables and figures. Separate paragraphs for each continent would add too much length and duplication.
720	3	20	0	34	0	General comment: I would recommend that for the paragraphs regarding the extremes there should be a more clear seperation of the sections regarding each continent and each region. (GREECE)	Not clear what reviewer wants us to do.
721	3	20	1	0	0	Most studies referred are not spécific to extremes. Few new results confirm (or sometimes attenuate) the conclusions of l'AR 4 but many cases show the importance of the spatial variability of trends more or less related to the extremes of temperature or precipitations. (BOURRELIER, PAUL-HENRI, AFPCN)	Thanks for comment, no action requested.
722	3	20	3	0	0	Several studies of new regional temperature series (except Africa and Sud América), globally coherent with global heating and indication that extremes are affected. The conclusions with values are the most robust. (BOURRELIER, PAUL-HENRI, AFPCN)	Added sentence on 2003 and 2010 hot summers and Barriopedro reference.
723	3	20	3	0	0	Section 3.3.1 Extreme seasons have a low profile currently, although the second paragraph mentions them in terms of paleoclimatic work. I think it would be good to expand on this somewhat, particularly in the light of the profile of the Stott 2004 paper and with new papers such as Barriopedro 2011, Christidis 2011a, Jones et al 2008 (Barriopedro et al The Hot Summer of 2010: Redrawing the Temperature Record Map of Europe 10.1126/science.1201224) (Brown, Simon, The Met Office Hadly Centre)	Not clear what this comment is asking, declined.
724	3	20	3	0	0	Section 3.3.1 suffers from not making a clear destinction between anomalous extreme temperature and absolute extreme temperatures. Christidis et al 2011b demonstrates that the changes in anomalous Tmax are much larger for most of the observed globe than actual Tmax hence the difference for Asia between Alexander 2006 and Brown 2008. Obviously spring heatwaves do not kill anyone but climatologically they are no less important in showing climatic extremes are changing. This distinction is currently lost in this section and leads to the weak confidence statement at p21 l61. (Brown, Simon, The Met Office Hadly Centre)	This issue is better to be discussed in Chapter 4.
725	3	20	3	25	16	"A reference which could be included presumably in the temperature section of Ch. 3 is Sherwood and Huber (PNAS, 2010, www.pnas.org/cgi/doi/10.1073/pnas.0913352107), who consider at least the possibility of rising heat stress (via increased wet bulb temperatures) coupled with human health limits (i.e., human body temperature) eventually making large parts of the planet virtually uninhabitable. While important to Ch. 4 impacts, can this study be commented on and assessed from a physical science perspective?" (UNITED STATES OF AMERICA)	This issue is better to be discussed in Chapter 4.
726	3	20	3	25	16	"A reference which could be included presumably in the temperature section of Ch. 3 is Sherwood and Huber (PNAS, 2010, www.pnas.org/cgi/doi/10.1073/pnas.0913352107), who consider at least the possibility of rising heat stress (via increased wet bulb temperatures) coupled with human health limits (i.e., human body temperature) eventually making large parts of the planet virtually uninhabitable. While important to Ch. 4 impacts, can this study be commented on and assessed from a physical science perspective?" (UNITED STATES OF AMERICA)	Authors agreed frost days are not true extremes so little discussion of these in Chapter; heat index is now mentioned in earlier sections, but little available literature.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
727	3	20	5	20	16	I would suggest that this paragraph also mention a few other types of temperature related extremes that are of interest, including the heat index (which is coupled to humidity) and indices such as number of days with minimum temperature belwo freezing (or some other threshold that might be used in evaluating survivability of some pests, etc.). (MacCracken, Michael, Climate Institute)	The definitions of warm and cold extreme would be in SREX Glossary(page 4 and 18).
728	3	20	5	20	19	It is not clear how warm and cold extreme is defined in subsection 3.3.1 (GREECE)	Misunderstanding by reviewer. The context is that the differences between cold extremes and warm extremes are important. No action required.
729	3	20	8	20	9	It would be helpful to indicate more specifically how the mentioned "cold and warm extremes" are different from the maximum and minimum daily temperature. (IPCC WGII TSU)	Adds little ot Chapter. Rejected.
730	3	20	12	20	14	Note that winter cold extremes are also associated with blocking events. See, for example, Sillmann et al (2010, J Climate, submitted - contact Jana Sillmann for a copy - Jana.Sillmann@ec.gc.ca). (Zwiers, Francis, Environment Canada)	Too much detail.
731	3	20	13	0	0	I propose to list blocking first and to add warm air advection: ".....generally caused by atmospheric blocking including quasi-stationary anticyclonic circulation anomalies and warm air advection (Xoplaki….." (Wanner, Heinz, University of Bern)	This sentence is deleted.
732	3	20	18	20	19	This sentence needs to be written more clearly--and needs a comma after "global warming" on line 18. (MacCracken, Michael, Climate Institute)	See #732.
733	3	20	18	20	19	I dont understand this last sentence (Brown, Simon, The Met Office Hadly Centre)	See #732
734	3	20	19	20	19	Suggest deleting "also independently of changes in circulation patterns or surface feedbacks." This description is not correct. For example, the enhanced temperatures in the tropics are closely related to the changes of tropical zonal and meridional circulations, as reported in many studies. Also, please clarify the kinds of surface feedbacks in which are being referred. (CANADA)	Agree, this sentence has been changed.
735	3	20	21	20	21	I think "can" is too strong - suggest replacing with "may be able to". Also, I think this discussion of what can be learnt from the paleo record needs to be scoped in terms of timescale; presumably the paleo record might be able to tell us something about extremes of monthly or seasonal averages - but I would not expect the paleo record to be informative about extremes on the daily timescale. (Zwiers, Francis, Environment Canada)	Added phrase about reconstructions being mainly for Europe.
736	3	20	21	20	30	Overall, this seems thin - and Eurocentric, even if Europe is discussed as an example. (Zwiers, Francis, Environment Canada)	Declined, a detection/attribution study has not been done, this is speculation.
737	3	20	21	20	30	Multiproxy paleotemperature reconstructions exist in several areas of the globe (e.g.: Neukom, R., et al., 2010: Multiproxy summer und winter surface air temperature field reconstructions for Southern South America covering the past centuries. Climate Dynamics, DOI 10.1007/s00382-010-0793-3). I only suggest to add a few words in lines 26 and 27 which indicates that the Grand Solar Minima lead to very cold winters and springs: "The coldest periods within the last five centuries occurred at the end of the late Maunder Minimum in the winter and spring of 1690." (Wanner, Heinz, University of Bern)	Thanks for the comment, but adding such references at this stage do not add sufficiently to the conclusions.
738	3	20	21	20	30	There are more regional historical records showing the temperature change in the past,for example, in China. It is possible to add some previous work in this paragraph. (Chen, Xing, Nanjing University)	Added a sentence and Barriopedro et al. reference.
739	3	20	21	20	30	There is a discrepancy between this paleo information which relates to extremes of seasonal averaged temperatures and the trends in daily extremes indices reported in other paragraphs. One would expect statements on changes in seasonal extremes in the instrumental period too (besides the daily indices). (Klein Tank, Albert, KNMI)	Barriopedro reference discusses this.
740	3	20	22	20	24	Here it is important to mention the heat wave of 2010 in Russia (e.g. Friedrich, 2011: "Analysis of Temperatures and Precipitation recorded at stations in Eastern Europe during the heat wave in summer 2010", submitted to Meteorologische Zeitschrift). (Rapp, Joerg, Deutscher Wetterdienst)	Barriopedro reference sufficient for purposes of this paragraph.
741	3	20	24	0	0	A relevant recent paper on the Russian heat wave and how unusual it was in the context of anthropogenic climate change should be assessed here: Dole, R., M. Hoerling, J. Perlwitz, J. Eischeid, P. Pegion, T. Zhang, X.-W. Quan, T. Xu, and D. Murray, 2011: Was There a Basis for Anticipating the 2010 Russian Heat Wave? Geophys. Res. Lett., in press. (UNITED STATES OF AMERICA)	this is only in the recent instrumental record, not the entire time series. In other words in the recent record only these two events exceeded the +2 SD. This has been clarified.
742	3	20	24	20	26	I'm not sure what to make of this, primarily because I don't know what period is being considered when the authors say that only two events exceed the two standard deviation threshold. In a stationary climate I would expect exceedances at the rate of about 5 per 200 months, assuming that the mean and standard deviation are well estimated and that monthly mean temperature is roughly Gaussian. If I consider the period since 1500 (which contains about 1500 summer months), then I would expect 37 or 38 events, obviously with large uncertainty). If the message is that there have been only two - then isn't there something wrong with the reconstruction (i.e., it is either grossly negatively biased, or the standard deviation is over estimated, or both). Anyway, this is a lot of words to say that this is confusing, probably because of the way it has been reported. (Zwiers, Francis, Environment Canada)	Delete "only" for clarification.
743	3	20	24	20	26	We didn't understand the meaning here. What is "recent?" How many events in the reconstruction exceeded the 2-sigma value? (UNITED STATES OF AMERICA)	This sentence is only designed to put the recent warm periods into some context. Extra detail is not required.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
744	3	20	25	0	0	Since paleoproxies never capture the full variance in the training and validation part of the records, the extremes are typically underestimated in paleoclimate reconstruction and thus comparing an observed departure to the paleoclimate record standard deviation is not all that useful. (Webb, Robert, NOAA)	Sentence adds to understanding of historical variations in temperature extremes - ie the coldest periods were centuries ago.
745	3	20	26	0	0	This sentence hangs. Why does it matter if the coldest period occurred at 1690? (UNITED STATES OF AMERICA)	Too much detail.
746	3	20	27	20	27	There are various literature citations for a volcanic events in 1690 (Briffa et al., 1990, Nature) (UNITED STATES OF AMERICA)	“The recent warming has not exceeded the natural past variability" means “strong natural variability in the basin, possibly exceeding the recent warming". Both of them are correct.
747	3	20	29	20	30	I suggest to review this line because, Camuffo et al (2010) analysing the variability of the temperature from the Mediternanean area taking into account the records limits, underlay that: "Compared with the long -term instrumental records(i.e 1655 onwards), the recent warming has not exceeded the natural past variability …". This is not the same with those reported at the line 29 : "It suggests strong natural variability in the basin, possibly exceeding the recent warming,..". (ITALY)	Paragraph deleted.
748	3	20	32	20	32	The name of this dataset is HadCRUT3 (not CRU/UKMO). (CANADA)	Paragraph deleted.
749	3	20	32	20	43	Should there be some mention of Thompson et al (2008, Nature, 453, doi:10.1038/nature06982) and follow-on papers, noting that this will lead to some minor revision of the global mean surfact temperature trend estimates? (Zwiers, Francis, Environment Canada)	Paragraph deleted.
750	3	20	35	20	36	Suggest adding “trends were also found to be stronger in high latitudes than in low latitudes.” (CANADA)	Paragraph deleted.
751	3	20	37	20	43	Another explanation in the literature is that the temperature records during the period of World War II are biased, due perhaps to the mix of ship observations tilting much more strongly to US ships that were reporting engine intake temperatures. Just looking at the record, the period during World War II looks very strange, and the ranking of years by their anomalies also suggests a problem, and so do the detection-attribution studies from AR4 that show quite good agreement except over oceans during World War II. This alternative (or complementary) explanation needs to be mentioned, and there are articles out there to document this. (MacCracken, Michael, Climate Institute)	Paragraph deleted.
752	3	20	40	20	43	This discussion of the possible (in that the World War II data are suspect) flattening or reversing of the warming, mainly in the Northern Hemisphere, is inadequate, and seriously under-referenced. There needs to be mention of the, I would suggest, likely role of SO2 emissions (not just changes in their amount, but changes in their altitude of emission in that this affects the fraction converted to sulfate and so the overall influence of aerosols) to make clear that this is likely a result of another human influence on the climate and not some sort of natural variation in the Sun or in clouds. Indeed, labeling it as global dimming or brightening I think is not appropriate--the hemispheric temperatures behaved differently, so quite clearly not the same influence was affecting the whole globe. Reference should be made to the TAR and AR4 discussions of detection-attribution and the likely role of sulfates in affecting temperatures through the mid-20th century. (MacCracken, Michael, Climate Institute)	Paragraph deleted.
753	3	20	40	20	43	“It has been suggested that the partial levelling out and/or decrease in some regions from ca. 1945 until the end of the 1970s (and in some regions until the mid-1980s) is due to a so called “dimming” of incoming shortwave radiation in several regions, followed upon by a “brightening” phase, both linked with changes in aerosol concentrations and/or cloud cover (Pinker et al., 2005; Wild et al., 2005; Wild, 2009).” If a decrease in aerosols and/or cloud cover have led to a brightening and thus warming, how does this square with the IPCC’s assertion that greenhouse gases alone likely caused more warming then has been observed? (Knappenberger, Paul, New Hope Environmental Sciences)	Paragraph deleted.
754	3	20	40	20	43	This discussion seems imbalanced, as it doesn't mention other possible explanations or partial explanations, including natural internal variability or even data problems with the SSTs around the 1940s (Thompson, D.W.J., J.J. Kennedy, J.M. Wallace, and P.D. Jones, 2008: A large discontinuity in the mid-twentieth century in observed global-mean surface temperature. Nature, 453(29), doi:10.1038/nature06982. (UNITED STATES OF AMERICA)	Paragraph deleted.
755	3	20	41	0	0	Add "at the ground" after "incoming shortwave radiation". (UNITED STATES OF AMERICA)	Agreed(MRa).
756	3	20	45	0	0	P20 45 Change “warming” to “increase” “in mean temperatures” (UNITED STATES OF AMERICA)	This sentence reports findings from AR$. More details inappropriate here.
757	3	20	46	0	0	Here and elsewhere throughout the report, what is meant by "statistically significant?" Usually the null hypothesis consists of a supposition of no change in a variable, in conjunction with the assumption of an appropriate random-process model for that variable. S.s. indicates failure of at least one but not necessarily both of these assumptions. Are we to understand that the authors of this report (not just the authors of the papers cited) subjectively assess that certain trends are great enough not to be caused by internal variability in the climate system in more than one location out of 20 (or 10 or 100, not stated)? Or is this a quantitative statement based on a specific hypothesized model of internal variability? If the latter, what is the level of confidence that the variability model is correct? It seems some comment on these issues should be part of this report. (UNITED STATES OF AMERICA)	Added reference to Box 3.1.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
758	3	20	46	20	48	We scanned section 3.2.1 for definition of "cold" and "warm" nights, but could not find it. (UNITED STATES OF AMERICA)	Declined, the text is meant to show that the specific variable shows warming.
759	3	20	48	20	51	Here and elsewhere throughout the treatment of temperature extremes, any trend that might be zero-order expected from a general warming are often said to "show warming" or some such language. We suggest this shorthand be removed in the interest of precision. In principle, one could, for example, have more warm nights without having an overall warming. (UNITED STATES OF AMERICA)	agree, changed.
760	3	20	59	0	0	P20 59 Change "less "to "fewer" "cold extremes". This sentence, particularly the placement of the citation, is awkward and should be re-crafted. (UNITED STATES OF AMERICA)	Changed to "negative"
761	3	20	62	0	0	P20 62 "cooling trend in extremes" – What does this mean? (UNITED STATES OF AMERICA)	Thanks for the comment, the new reference doesn't add sufficiently to the conclusions to be included at this stage.
762	3	20	62	20	62	It is suggested to add a new sentence "Since the mid-1980s, the frequency of cold events has also reduced in central and southern Northeast China, eastern North China, the middle and lower reaches of the Yangtze River, and eastern South China（Zhang and Qian, 2011)". (Zhang Zongjie, Qian Weihong, 2011: Identifying Regional Prolonged Low Temperature Events in China. ADVANCES IN ATMOSPHERIC SCIENCES, 28(2), 338-351) (CHINA)	ENSO discussed elsewhere.
763	3	21	1	0	0	What about changes in ENSO frequency and magnitude? (Stouffer, Ronald, NOAA)	Thanks for the comment, the new reference doesn't add sufficiently to the conclusions to be included at this stage.
764	3	21	1	21	1	Shouldn't there also be a Ken Kunkel citation in this list given that his name is very frequently associated with investigation of the "warming hole"? Perhaps cite Kunkel, K.E., X.-Z. Liang, J. Zhu, and Y. Lin, 2006: Can CGCMs simulate the Twentieth Century "warming hole" in the central United States. J. Climate, 19, 4137-153. (Zwiers, Francis, Environment Canada)	Yes, but not specifically directed at the "warming hole". Declined.
765	3	21	1	21	3	This is not an adequate discussion.. As discussed in Chapter 3 of AR4 there are studies showing that the atmospheric circulation has changed in association with internal climate variability like the PDO, ENSO, AMO, etc. (UNITED STATES OF AMERICA)	Including these references would not change conclusions, declined.
766	3	21	5	21	10	Choi et al. (2009) analyzed changes in the extremes of temperature and precipitation in APN region, and many Chinese papers have been published since 2005 (e.g. in a recent special issue of Climatic and Environmental Research, 20 papers analyzing climate extremes in the country were collected). Those publications should be evaluated by those authors from China, and the major findings should be incorporated into the report. (CHINA)	Thanks for the comment, the new reference doesn't add sufficiently to the conclusions to be included at this stage.
767	3	21	5	21	23	There is no reference to quantiles such as Tmax90 and Tmin10, representing warm and cold extremes. There is a study by Maheras et al (2006) concerning trends of these parameters in Greece, following the other studies referring to Eastern Mediterranean: warm extremes are characterised by a negative trend in winter and positive on an annual basis and descending trend in cold extremes annually. (GREECE)	Results are summarized at the end of each major section.
768	3	21	5	21	23	I think it would be much more helpful to the reader to present a summary of the results of the stuides in the first (topic setting sentence) than to give a mine-numbing list of studies that hides the important results. Start the paragraph with the text on line 11, to the effect that: "Studies from around the world indicate that the numbers of unusually warm nights and days are increasing, and the numbers of unusually cold nights and days are decreasing."And put the sentence in bold!! Fine to then go on to provide the other material, which are mainly details, but to hide the key results in a quite long sentence (lines 11-14) in the middle of long paragraph in the middle of a long section of long paragraphs is just not helpful to the reader or a fair evaluation of the state of scientific understanding. I also did not like the phrase "statistically not significant" instead of "not statistically significant" or better yet, "has not yet been shown with 95% confidence." It would also be helpful to find a way to indicate that no other hypothesis than that it is a result of global human-induced climate change has anywhere near a comparable level of confidence. (MacCracken, Michael, Climate Institute)	Including these references would not change conclusions, declined.
769	3	21	5	21	23	There is a recent article discussing temperature and precipitacion indices at the Western Indic Ocean: Vicnent et al (2011), JGR, (Aguilar, Enric, Universitat Rovira i Virgili)	Including these references would not change conclusions, declined.
770	3	21	5	21	23	(c'd) available at http://www.agu.org/journals/pip/jd/2010JD015303-pip.pdf (Aguilar, Enric, Universitat Rovira i Virgili)	Reject. Very obvious point which should be clear to all readers of this report.
771	3	21	5	21	41	A sentence should be inserted, which has indicated once again that trends in different time intervals should not be directly compared. (Rapp, Joerg, Deutscher Wetterdienst)	Including these references would not change conclusions, declined.
772	3	21	8	21	8	Add the following reference concerning extreme precipitation over the Greek area:Tolika K, Anagnostopoulou Chr, Maheras P, Kutiel H (2007). Extreme Precipitations related to Circulation Types for four case studies over the Eastern Mediterranean. Advances in Geosciences, 12, 87-93 (GREECE)	Including these references would not change conclusions, declined.
773	3	21	8	21	8	the eastern Mediterranean including Balkan (Kostopoulou et al., 2009) and Turkey (Kunglitsch et al., 2010) Kostopoulou E, Tolika K, Tegoulias I, Giannakopoulos C, Somot S, Anagnostopoulou C and Maheras P (2009) Evaluation of a Regional Climate Model using in-situ temperature observations over the Balkan Peninsula. Tellus A, 61A, 357-370. (GREECE)	Including these references would not change conclusions, declined.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
774	3	21	8	21	8	It is writen in the report ”…the eastern Mediterranean including Turkey (Kuglitsch et al., 2010)……” It is proposed to modify this sentence to include more relevant studies i.e. “..southeast Europe and the eastern Mediterranean (Nastos and Zerefos, 2008, Kuglitsch et al., 2010, Kioutsioukis et al., 2010)…” These additional references could be also included in table 3.2 References: Kioutsioukis I, Melas D, Zerefos C, 2010, Statistical Assessment of changes in Climate Extremes over Greece (1955-2002). Int. J. Climatol. 30: 1723–1737 Nastos, P.T., Zerefos, C.S., 2008, Decadal changes in extreme daily precipitation totals in Greece. Advances in Geosciences, Vol. 16, pp. 55–62. (GREECE)	Including these references would not change conclusions, declined.
775	3	21	10	0	0	Cite: Choi et al, 2009 for Asia-Pacific Region (ITALY)	Including these references would not change conclusions, declined.
776	3	21	10	21	10	Gallant and Karoly (2010) should be included as a relevant regional study for Australia along with Alexander and Arblaster (2009). Full reference is: Gallant, A. and Karoly, D., 2010. A combined climate extremes index for the Australian region. Journal of Climate, 23, 6153-6165. DOI: 10.1175/2010JCLI3791.1 (AUSTRALIA)	Including these references would not change conclusions, declined.
777	3	21	11	21	14	It is worth to expand the discussion and give more details about the relative importance of these changes. For example, Kioutsioukis et al (2010) found for Greece that extreme minimum temperature indices have been increasing at a faster rate than that of extreme maximum temperatures. These changes are important in a range of applications. References: Kioutsioukis I, Melas D, Zerefos C, 2010, Statistical Assessment of changes in Climate Extremes over Greece (1955-2002). Int. J. Climatol. 30: 1723–1737 (GREECE)	Including these references would not change conclusions, declined.
778	3	21	11	21	14	It is worth to expand the discussion and give more details about the relative importance of these changes. For example, Kioutsioukis et al (2010) found for Greece that extreme minimum temperature indices have been increasing at a faster rate than that of extreme maximum temperatures. These changes are important in a range of applications. References Kioutsioukis I, Melas D, Zerefos C, 2010, Statistical Assessment of changes in Climate Extremes over Greece (1955-2002). Int. J. Climatol. 30: 1723–1737 (Zerefos, Christos, Academy of Athens)	Now in Box 3.1.
779	3	21	12	0	0	Have "unusually cold/warm nights/days" been defined? If so, not prominently. How do they differ from merely "cold/warm nights/days?" (UNITED STATES OF AMERICA)	Accepted
780	3	21	13	21	15	P21 13-15 Change “not significant” to “insignificant” and check parentheses for references. (UNITED STATES OF AMERICA)	This example is simply included to show the spatial variability of trends, especially in small regions.
781	3	21	14	0	0	The changes in Uruguay occur over a relatively small area. Is change real? We assume there is a big signal to noise problem. We also assume the observational record is too short (p22L2-4). If so, delete? (UNITED STATES OF AMERICA)	Context makes it clear that this is about extremes
782	3	21	18	21	18	Is the "less consistent warming tendency" about means or extremes? (SWEDEN)	Sentence deleted.
783	3	21	19	21	23	Is this change point robust? Has it been clearly demonstrated to be inconsistent with internal variability? Only one study is cited here. (CANADA)	Last sentence removed.
784	3	21	19	21	23	This discussion seems imbalanced, as it doesn't mention other possible explanations or partial explanations, including natural internal variability. (UNITED STATES OF AMERICA)	Accepted
785	3	21	19	21	33	The convention on capitalizing of regional names needs to be consistently aplied. On line 33, the text says "western and central Europe" which is, in my view, the propoer way to do this. Yet on earlier lines, the terms "Central and Southern Eastern Europe," "Southeastern U.S." and "Eastern Canada" appear, among other listings. These should be changed to "central and southeastern Europe," "southeastern U.S." and "eastern Canada"--this problem continues in further paragraphs on this page, including lines 32, 47, 53, 54, 60, 61, and so on. (MacCracken, Michael, Climate Institute)	The “abrupt shift” or Jump are implying quick change, whereas here the change is in the direction/magnitude of the trend
786	3	21	20	21	22	P21 20-22 “change point” is sort of a technical term, and I’d suggest using “abrupt shift” or “jump” instead. Also, specify whether the jump is up or down. More important, does the main text discuss whether this could be a spurious artifact of inhomogeneous data rather than a geophysical change? (See my apology above regarding my review of the main text.) (UNITED STATES OF AMERICA)	Agree, sentence removed.
787	3	21	22	21	23	This sounds a bit speculative to me. (Zwiers, Francis, Environment Canada)	Sentence removed.
788	3	21	22	21	23	This suggestion of a relationship to "global dimming/brightening" seems to me obscuring the likelihood that this effect has been caused by the changing amounts, altitudes, and patterns of emissions of SO2, and so is a human-related factor--not some sort of, as seems implied, solar related or other natural result. That the entire role of sulfate is not addressed in the discussion seems to me a serious omission that needs to be corrected. (MacCracken, Michael, Climate Institute)	Added sentence about cold wave declines in USA.
789	3	21	25	21	41	Discussion of recent heatwaves is given, but if this is retained it should for balance be noted that there have also been significant recent cold anomalies - at the beginning and end of 2010 in western Europe, in 2010 over cental and eastern Australia and so on. Global warming remains a residual from summing over warm and cold anomalies, and both warm and cold events should be mentioned to avoid a charge of selective presentation of evidence, however inadvertent this may have been. (Global Climate Observing System Steering Committee)	FAQ3.2 has ben shortened substantially, and focus on Australia reduced.
790	3	21	25	21	41	Some of this regional information provided here on heat waves could be used to make FAQ3.2, currently focusing on Australia only, provide a more global picture of heat waves. (Stocker, Thomas, IPCC WGI TSU)	comment about rh etc. beyond the scope of this section.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
791	3	21	25	21	41	Again, put the key results in the first sentence, and details about the numbers of studies later in the paragraph. I also think that this paragraph needs to mention how relative humidity seems to remain near constant as the temperature goes up, so the absolute humidity rises, and the heat index increases significantly in high humidity regions (indeed, in the US National Assessment from 2000, the increase in the heat index was about twice the increase in temperature, and there were very high levels across the southeastern and south-central US. When talking about heatwaves, etc.--the change in humidity simply has to be discussed. (MacCracken, Michael, Climate Institute)	Thanks for the comments, but adding these references do not add sufficiently to conclusions to warrant inclusion at this stage.
792	3	21	26	21	28	"Alexander et al. (2006) provided an analysis of trends in warm spells mostly in the mid- and high latitudes of the northern hemisphere. The analysis display a tendency towards longer warm spells in much of the region, with the exception of the Southeastern U.S. and Eastern Canada." You would be remiss not to cite the finding of Kaliq et al. (Khaliq, M.N., P. Gachon, A. St-Hilaire, T.B.M.J. Ouarda, and B. Bobeé, 2007. Southern Quebec (Canada) summer-season heat spells over the 1941–2000 period: an assessment of observed changes. Theoretical and Applied Climatology, 88, 83–101) which also provides supporting evidence for a decrease in heat waves in eastern Canada. (UNITED STATES OF AMERICA)	Accepted
793	3	21	27	21	27	"display" --> "displays" (Zwiers, Francis, Environment Canada)	cited earlier.
794	3	21	33	21	41	Barriopiedro et al. 2011 should be cited somewhere in here - DOI: 10.1126/science.1201224 (Stocker, Thomas, IPCC WGI TSU)	added citation to Barriopedro et al.
795	3	21	33	21	41	This paragraph needs to be updated in the light of Barriopedro et al 2011 (Brown, Simon, The Met Office Hadly Centre)	Delete reference to 1.4C etc.
796	3	21	35	21	35	Is the 1.4 K relative to the previous warmest value consistent with the 2.3K above the 1961-1990 mean quoted in Stott et al (2004, 432, 610-614)? (Zwiers, Francis, Environment Canada)	These are examples for supporting more warming in recent years in comparison to last years.
797	3	21	36	21	41	What purpose is served by a list of extreme heat waves, with no indication whether they reflect any change in heat waves? If so, it needs to be made more clear. What does "perhaps" mean? "Other examples of extreme heatwaves" can easily be interpreted as "Other record-breaking heatwaves." Clarify whether this is intended meaning or not. (UNITED STATES OF AMERICA)	Fixed in revised version.
798	3	21	38	21	38	Is there a National Climate Centre reference? (Zwiers, Francis, Environment Canada)	Thanks for the comments, but adding these references do not add sufficiently to conclusions to warrant inclusion at this stage.
799	3	21	38	21	38	Add the following references on the heathave of 2007 over the Balkan Peninsula: Busuioc, A., Dumitrescu, A., Soare, E. and Orzan A.: Summer anomalies in 2007 in the context of extremely hot and dry summers in Romania, Romanina Journal of Meteorology, 9, 1-17, 2007 and K. Tolika, P Maheras, I Tegoulias (2009) Extreme temperatures in Greece during 2007: Could this be a "return to the future"?. Geophysical Research Letters, Vol. 36, L10813, doi: 10.1029/2009GL038538 (GREECE)	Fixed in revised version.
800	3	21	38	21	38	Date for National Climate Centre citation? 2009? (Stocker, Thomas, IPCC WGI TSU)	Agreed, included in second to last sentence of paragraph.
801	3	21	38	21	39	References to the heat wave 2010 in Russia should be added (see above). (Rapp, Joerg, Deutscher Wetterdienst)	Fixed in revised version.
802	3	21	38	21	39	Reference for "National Climate Centre"?; and for the 2010 heatwave in Russia? (Barriopedro et al., Scienceexpress?) (Klein Tank, Albert, KNMI)	Do not understand point of comment.
803	3	21	39	21	41	The 2010 heatwave in Russia was also accompanied by drought. (Global Climate Observing System Steering Committee)	Thanks for the comment, but adding such references at this stage do not add sufficiently to the conclusions.
804	3	21	41	0	0	Cite Baldi et al. (2006), showing that there is an increase in the number of warm speels over Italy, and Europe, in the last 50 years (ITALY)	over the period 1880 to 2005.
805	3	21	43	21	44	It would be preferable to specify the time frame for the described temperature change. (IPCC WGII TSU)	Delete of "Some" accepted. The time period is 1880 to 2005 and included in text
806	3	21	43	21	45	"led to ..... revisions" - delete 'some'. 2 times 'some' in the first sentence sounds very vague. The Della-Marta et al analysis is referring to which time period? (Stocker, Thomas, IPCC WGI TSU)	over the period 1880 to 2005
807	3	21	43	21	45	The period of investigation is missing here. The paper states that the temperature change is determined over the period 1880-2005. (BELGIUM)	over the period 1880 to 2005
808	3	21	43	21	45	The period of investigation is not mentioned here. I have checked in the paper that the temperature changed is determined over the period 1880-2005. (BELGIUM)	Homogenization especially on a regional basis does not always cancel out when averaged up. The Time of Observation bias in the US is a systematic cooling, so when corrected reduces the cool bias in the US temperature record.
809	3	21	43	21	49	This seems thin, Eurocentric, and leaves dangling the question about whether trends in other regions are also too small. Further, this seems to conflict with a large body of work on the impact of homogenization efforts (e.g., Jones and colleagues, Menne et al, 2010 for the US, and many others) indicating that temperature adjustments are made equally in both directions and that they do not have signficant effects on trends for large area means. (Zwiers, Francis, Environment Canada)	over the period 1880 to 2005
810	3	21	44	0	0	+1.6 refers to which period? (Klein Tank, Albert, KNMI)	over the period 1880 to 2005
811	3	21	44	21	44	The +1.6 +/-0.4 K change is for what period? (Zwiers, Francis, Environment Canada)	disagree, authors feel text is not ambiguous.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
812	3	21	51	21	52	"consistent … on the global scale" or "warming climate on the global scale?" The meaning is ambiguous as written. This comment applies elsewhere in the document, too. (UNITED STATES OF AMERICA)	See no need to do this here.
813	3	21	51	22	6	Note where there has been a change in uncertainty from the AR4. (Stocker, Thomas, IPCC WGI TSU)	Disgree, we break the report into Observed, Causes and Projections and summary is appropriate at the end of section.
814	3	21	51	22	6	It seems odd to me that the summary paragraph on observed changes comes before the paragraphs relating to detection and attribution work. Do not the D&A results inform our view of what has changed with regard to extremes? (Brown, Simon, The Met Office Hadly Centre)	Agree. Change to "in most regions"
815	3	21	55	0	0	insert 'in most regions' after '…. In the number of unusually cold days and nights" (Webb, Robert, NOAA)	Changed to "In most regions"
816	3	21	56	0	0	Has the meaning of "on a global scale" in this context been defined? It seems to mean something like "we see this behavior more than the opposite when we step back and look across the globe." But the word "scale" makes me think of an average, such as global mean temperature, which is a very different meaning. We suggest re-wording this statement more precisely. (UNITED STATES OF AMERICA)	Reference only reinforces conclusions and does not add sufficiently to conclusions to be added at this stage.
817	3	21	58	0	0	Table 3.2 (page 119, Tmin and Tmax columns, N. Australia and S. Australia/NZ): Gallant and Karoly (2010) have shown an increase in the area of northern and southern Australia experiencing WD and a decrease in CD, with weaker trends in northwest Australia. Also shown decreases in the area of northern and southern Australia experiencing CN and increases in WN. All results were consistent with the other studies of Alexander et al. 2006 and Trenberth et al. 2007 that have been referenced. Reference should be included with these studies. Full reference: Gallant, A. and Karoly, D., 2010. A combined climate extremes index for the Australian region. Journal of Climate, 23, 6153-6165, DOI: 10.1175/2010JCLI3791.1 (AUSTRALIA)	Disagree, authors made their own assessment and conclusion here.
818	3	21	61	21	63	I dissagree with the use of medium confidence and the lack of literature. This arises from not considering changes in anomalous Tmax (cf Brown 2008 and Christidis 2011b). If you include extremely warm autumn/winter/spring days then the Asian signal is very clear. (Brown, Simon, The Met Office Hadly Centre)	Not clear why we shoul do this?
819	3	21	63	22	2	It would preferable to indicate that confidence assignments are made in Table 3.2 for Africa, even though likelihood assignments are not. (IPCC WGII TSU)	Sentence restructured.
820	3	22	6	22	6	suggest to delete "with some exceptions" -- not needed as the sentence refers to many regions (not all) (Stocker, Thomas, IPCC WGI TSU)	added section headings for clarity
821	3	22	7	0	0	It should be made more visible that the description of observed temperatures ends here while the specification of projected temperature extremes is begun. (Rapp, Joerg, Deutscher Wetterdienst)	Modified to more or less exact statement from Hegerl et al. (2007).
822	3	22	11	0	0	Too many qualifiers in sentence. It reads funny. Change "may have" to "has". (UNITED STATES OF AMERICA)	Modified to more or less exact statement from Hegerl et al. (2007).
823	3	22	11	22	13	This is rather awkwardly articulated. I think you are trying to say that the AR4 assessment was based in part on evidence from formal detection and attribution studies. The criteria used in detection and attribution go well beyond an assessment of statistical significance - see the discussion in Hegerl et al (2007) - so making reference only to statistical significance does rather a disservice. (Zwiers, Francis, Environment Canada)	Accepted
824	3	22	15	22	15	"attributions" --> "the attribution" (Zwiers, Francis, Environment Canada)	Final sentence modified. Remainder of paragraph seems OK and balanced.
825	3	22	15	22	38	This paragraph is unclear, poorly structured, missing key results and should be re-written. A) Discussion of AR4 results (Meehl et al 2007b), lines 17 to 25 should be moved to the previous paragraph which discusses AR4 results. B) why does the paragraph start with regional results rather than global studies eg Zwiers 2011 & Christidis 2011b? C) There is a lack of balance between the Zwiers 2011 and the Christidis 2011b results (nb Christidis 2011b is not in the reference list). D) Zwiers 2011 were unable to sepparate the contribution from natural and anthropogenic forcings. Christidis 2011b were able to do this for anomalous Tmax - an important result yet currently ommited. E) Christidis 2011b show that the resolving the temporal evolution of extremes is important for separrating the natural and anthropogenic components. F) the final sentence is not quite accurate Christidis 2011b only find the anomalous Tmax are overestimated by the models (thier fig 3a - betas > 1) not the actual Tmax (their fig 3b - betas < 1) (Brown, Simon, The Met Office Hadly Centre)	Should be included in references cited.
826	3	22	15	22	38	It seems that this paragraph should at least mention the possibility of anthropogenic urban heat island impacts on extreme temperatures. We think most readers will assume “anthropogenic forcings” are global or regional, not local, in nature. (UNITED STATES OF AMERICA)	agree, single quotes removed.
827	3	22	16	0	0	The phrase "warm nights" has already been used (though I am not sure it has been defined). Why is it put in single quotes here? (UNITED STATES OF AMERICA)	Corrected to Gutowski et al. 2008a.
828	3	22	17	22	18	Check this - the reference is definitely wrong. I'm wondering if this is meant to reference the Meehl et al temperature records paper (Meehl et al, 2010, 36, L23701, doi:10.1029/2009GL040736); even so, the attribution statement that is given here would be somewhat of an over interpretation of that paper because Meehl et al do not use formal D&A techniques (and because of inconsistency between results for cold temperature records and warm temperature records). (Zwiers, Francis, Environment Canada)	Meaning is clear.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
829	3	22	18	0	0	over the US - Everywhere or on average? (UNITED STATES OF AMERICA)	Included because this is what cited assessment looked at.
830	3	22	19	22	21	Is "length of growing season" relevant in the context of extremes? (SWEDEN)	agree, removed.
831	3	22	27	22	28	The abbreviations ANT/ALL should be moved to the caption of Figure 3.3, here they are not relevant (Stocker, Thomas, IPCC WGI TSU)	Comment unclear, no mention of cooling here.
832	3	22	33	0	0	Are these changes consistent with a mean cooling? Are the extreme changes noise? (Stouffer, Ronald, NOAA)	agree, added phrase.
833	3	22	33	22	36	Suggest indicating that these estimates of changes in waiting time are subject to considerable uncertainty. (Zwiers, Francis, Environment Canada)	Check and correct bars in Figure 3.2 (was 3.3).
834	3	22	33	22	37	Are the error bar colors in Figure 3.3 labeled correctly? The conclusions described in this sentence suggest they may not be. For example, do the green bars correspond to annual minimum daily maximum temperature? (IPCC WGII TSU)	These are not exactly the same thing. Cold/warm days are based on percentile thresholds (10th and 90th).
835	3	22	33	22	38	This is a rather complicated section because it combines annual minimum/maximum with daily minimum/maximum temperatures and waiting times. Would it be an option to use Cold/Warm Days/Nights instead? This would also make the terminology consistent with Tables 3.2 and 3.3. But the caption of Figure 3.3 would of course need to be changed accordingly. (Stocker, Thomas, IPCC WGI TSU)	Changed to return period
836	3	22	33	22	41	Suggest changing “waiting times” to “recurrence time” (no one sits around waiting for a heat wave). (UNITED STATES OF AMERICA)	Disagree. Very difficult to simplify these terms without being misleading or even more confusing.
837	3	22	34	22	35	I think the phrases like "extreme annual minimum daily maximum temperature" are simply too complex and condensed for those we want to be able to use this chapter. While it takes more words, I think it much more informative to say something like: "the lowest daily high temperature over aperiod of 30 years" or whatever. But please do try to simplify terms like this. (MacCracken, Michael, Climate Institute)	Changed to return period
838	3	22	35	22	38	The use of "waiting times" has not been explained (or I missed it). It is extremely confusing (and ven wrong) when used in conjunction with return periods. (UNITED KINGDOM OF GREAT BRITAIN AND NORTHERN IRELAND)	included in text.
839	3	22	37	22	38	Note also that models under-estimate observed changes in cold extremes (TNn). (Zwiers, Francis, Environment Canada)	Figure is now improved.
840	3	22	40	0	0	Figure 3.3: The lines linking the plots to the map are still much too dominant and distracting on this figure. The lines should be made much thinner, and should avoid crossing over the plot area. This can be achieved with some minor repositioning of the plots, and by connecting the line in each instance to the top or bottom corner of the plot instead of the centre-point. (Stocker, Thomas, IPCC WGI TSU)	agree.
841	3	22	40	0	0	Figure 3.3, caption: Suggest to add colour information to the brackets, rather than listing them separately: e.g., Annual minimum daily minimum temperature (TN_n; red), …. (Stocker, Thomas, IPCC WGI TSU)	Changed to return period
842	3	22	41	0	0	The term “waiting time” is certainly correct, but “return time” is in common use in extreme value analysis. Note that the return time slightly differs according to the block maximum approach or peaks-over-threshold approach. (BELGIUM)	included in caption.
843	3	22	41	22	45	Again, the terms here are pretty complex--explaining them in simpler terms would be very helpful. The caption also needs to explain what "all" and "ant" are for those who just look at the figures and captions. (MacCracken, Michael, Climate Institute)	Correct the bar descriptions (colour and order) in the caption.
844	3	22	43	22	44	The order of 'annual maximum daily minimum temperature (TNx)' and 'annual minimum daily maximum temperature (TXn)' should be reversed, for the consistency with the text P22, L36. (JAPAN)	Previous sentence (p. 10) has been modified to indicate their conclusions are more regional, not global.
845	3	22	48	22	53	How do the findings Petoukhov and Semenov (2010) (that were previously cited on page 10 of this Chapter) regarding an increase in cold outbreaks effect the confidence that cold extremes should decrease in the future (that is given as virtually certain)? (See also p 23, lines 10-13 and p 23 line 38) (UNITED STATES OF AMERICA)	agree, added phrase.
846	3	22	55	22	57	Perhaps mention that fewer modelling groups calculated indices than saved daily data (during specified time windows) - hence explaining the difference in the number of models used by Meehl et al and Kharin et al. (Zwiers, Francis, Environment Canada)	Accepted
847	3	22	56	22	56	Replace "which was referenced in the AR4" with: 'that provided the basis for extreme projections given in the AR4'. (Stocker, Thomas, IPCC WGI TSU)	Accepted
848	3	22	57	22	57	Insert "indices" after "in several extremes" (Zwiers, Francis, Environment Canada)	"and discussion"
849	3	22	58	22	58	Not sure what is meant by 'and discussion' as noted in the citation. (Stocker, Thomas, IPCC WGI TSU)	disagree, provides context for uncertainty.
850	3	22	59	23	4	References to "stippling" would seem rather too detailed in the text. (SWEDEN)	Decision to include seasonal analysis was motivation. However, this is now just one more reference and doesn't need additional explanation in our text.
851	3	22	60	22	60	What made the update possible, and what aspects of Tebaldi et al were updated? Presumably this has become possible because more modelling groups made daily output available subsequent to the publication of Tebaldi et al - this should be stated so as not to inadvertantly cast aspersions. Note that Table 1 in Orlowsky and Seneviratne lists only 19 models, not 23; some models that, according to the PCMDI website, have daily output available seem to be missing. (Zwiers, Francis, Environment Canada)	unclear what is meant here, declined.
852	3	23	1	0	0	Figure caption - delete. (Stouffer, Ronald, NOAA)	This phrase has been deleted.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
853	3	23	2	23	3	Please insert "minimum" before "...temperatures greater than 20°C)". (Rapp, Joerg, Deutscher Wetterdienst)	The relevant phrase has been deleted
854	3	23	3	23	3	It would be good to provide a scientific reason for using other thresholds than Tebaldi et al., as this change complicates direct comparison. (Stocker, Thomas, IPCC WGI TSU)	Deleted "moderate"
855	3	23	7	0	0	Are "moderate temperature extremes" the same as warm/cold days/nights? If so, could a single term be used consistently throughout? If not, could distinction be made more clear? (UNITED STATES OF AMERICA)	see comment and answer number 845.
856	3	23	10	23	13	How do the findings Petoukhov and Semenov (2010) (that were previously cited on page 10 of this Chapter) regarding an increase in cold outbreaks effect the confidence that cold extremes should decrease in the future (that is given as virtually certain)? (UNITED STATES OF AMERICA)	Bolding is saved for the summary at the end of this section. Authors feel these sentences are sufficient as is.
857	3	23	10	23	15	The very important conclusion on these lines is hidden--it needs to be made more prominent and likely put in bold. I also think the word "assess" on line 11 might better be "conclude" in that a conclusion is being drawn rather than a set of results being examined--for most readers, the key information wanted is the conclusion. (MacCracken, Michael, Climate Institute)	disagree, feel structure is sufficient.
858	3	23	17	23	19	Again, get the key results (lines 27ff) into the first sentence--and try to keep extraneous information out of first sentences of paragraphs (in this case, all the emission scenario names--if this sentence is for some reason kept) (MacCracken, Michael, Climate Institute)	The predictions of return periods are not exact and the ranges shown in Figure 3.5 reflect the uncertainty at regional scales, as does the fact that the strongest assesment here is "likely".
859	3	23	17	23	32	The results of Clark et al 2011 I would think preclude such exact predictions of return periods given at the end of this paragraph. Acknowledment that regional predictions are very uncertian (cf Clark 2011) should be made somewhere in this paragraph. (Brown, Simon, The Met Office Hadly Centre)	Comment is beyond the scope of this assessment. Appropriate for AR5.
860	3	23	17	23	59	How does the fact that for the past 10 to 15 years the rate of increase in global temperature falls well below the mean of the A1B runs of the CMIP3 models impact your assessment of the future utility of the CMIP model projection of temperature changes? It could be that model-contained weather noise (i.e. natural variability) completely explains the observed/projected discrepancy, but model inaccuracies cannot be ruled out. At some point, if global the rate of global temperature increase does not increase, the projections from the CMIP3 models will have to be re-evaluated. I assume that the authors of this Chapter do not feel that the time for re-evaluation is not yet upon us? (Knappenberger, Paul, New Hope Environmental Sciences)	Mid-century projections now noted.
861	3	23	19	23	32	The author team might consider describing here in the chapter text the projections depicted in figure 3.6 for 2050. (IPCC WGII TSU)	This has already been acknowledged in previous paragraphs. But replace "the changes" with "some changes".
862	3	23	22	23	23	Models over-estimate changes in extreme hot daily maximum temperatures (TXx) but under-estimate changes in extreme cold daily minimum temperatures (TNn) during the latter half of the 20th century. The chapter should provide assessments of changes at both ends of the temperature distribution - since both types of changes can have important impacts. (Zwiers, Francis, Environment Canada)	Discussion expanded in attribution section.
863	3	23	22	23	23	“Models overestimate changes in temperature extremes” This sounds like an important statement, We think some elaboration is in order. (UNITED STATES OF AMERICA)	Not clear which is 21.
864	3	23	23	23	23	See comment 21 where the overestimate by models of Tmax changes is not that clear cut. (Brown, Simon, The Met Office Hadly Centre)	Weakening has more stress on low uncertainty(Mra).
865	3	23	23	23	25	is "weakening" the right term to be used here? On line 25 it is stated that "the uncertainty estimates are … reduced"… (Stocker, Thomas, IPCC WGI TSU)	changed to wording
866	3	23	25	0	0	Confusing is that "uncertainty estimates are also reduced" here implies that larger uncertainty exists (Klein Tank, Albert, KNMI)	changed wording
867	3	23	25	23	25	"uncertainty estimates are also reduced to reflect…poor…models" I know what you mean but it is an odd choise of words. The uncertainty is INCREASED so the quality assessment is lowerd (Brown, Simon, The Met Office Hadly Centre)	Projections only for land areas. Explanation of changes in Arctic would require work that has not been done, as far as we are aware.
868	3	23	29	23	30	It needs to be explained why the return period does not change as much in the Arctic as elsewhere. At least mention should be made that, while the temperature has increased the most in this region (such that average conditions are actually likely warmer than any given extreme had been), the variability is not as great--or what is the explanation? And do define if this is referring to land areas or to the ocean-land area of the Arctic together. (MacCracken, Michael, Climate Institute)	Accepted
869	3	23	30	23	30	Delete 'newly computed'. (Stocker, Thomas, IPCC WGI TSU)	added sentence about the Meehl paper since it includes projections.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
870	3	23	34	0	0	A particular aspect of temperature extremes, daily record high and low temperatures, should be called out and mentioned specifically here. Two papers document the recent factor of two increase of daily record high temperatures compared to daily low minimum temperatures, one for the U.S. (Meehl, G.A., C. Tebaldi, G. Walton, D. Easterling, and L. McDaniel, 2009: The relative increase of record high maximum temperatures compared to record low minimum temperatures in the U.S. Geophys. Res. Lett., 36, L23701, doi:10.1029/2009GL040736.) and one for Australia (Trewin, B., and H. Vermont, 2011; Changes in the frequency of record temperatures in Australia, 1957-2007. Australian Meteorological and Oceanographic Journal, 60, 113-119). Meehl et al. 2009 go on to show that this ratio, currently two to one, could increase to 20 to 1 by mid-century and 50 to 1 by the end of the century. The point should also be made that even in a much warmer climate, severe record-setting cold temperatures still occur, but much less frequently than record highs. (UNITED STATES OF AMERICA)	Thanks for the comment, but adding such references at this stage do not add sufficiently to the conclusions.
871	3	23	34	0	0	Another relevant study that was omitted regarding projected temperature extremes that should be assessed here is: Diffenbaugh, N.S., and M. Ashfaq, 2010: Intensification of hot extremes in the United States. Geophs. Res. Lett., 37, L15701, doi:10.1029/2010GL043888. This paper shows that even with a low emission scenario that maintains an average temperature increase below 2C, hot extremes would still increase substantially over the U.S. (UNITED STATES OF AMERICA)	This paragraph, changed.
872	3	23	34	0	0	P23 34 “In the following paragraph”. Do you mean this one (34-59) or really the one that follows (p 24)? (UNITED STATES OF AMERICA)	added a little on the Meehl record paper and the occasional cold record.
873	3	23	34	23	59	I think it would be useful to have a bit better balanced discussion of changes in hot and cold extremes. Cold extremes are mentioned in passing, but moderating cold extremes can also have important impacts. For example, the forest beetle infestations in western North America (see, for example, the discussion in CCSP 3.3) that have caused extensive damage with serious economic, ecosystem and likely forest hydrology implications, have been linked at least in part to the decreasing severity of cold extremes. (Zwiers, Francis, Environment Canada)	Thanks. But we think bolding would not help and breaking up the paragraph would reduce readability.
874	3	23	34	23	59	While I think the details in the first two sentences need to be moved to later in the paragraph (or more briefly mentioned near the start), the overall presentation of results in this paragraph is clearly written and interesting to read--its example should be followed. I do think, however, that it would help to break up this quite long paragraph, perhaps so the regional results are presented in bullets for each region, with some bolding used to help the reader find the region of interest. The tables are helpful in giving a regional breakdown, but unless the text is altered to attract the reader, no one is going to read it (and this applies to most of the chapter). Provide some bolding to help the reader find information for their region, set off different locations with bullets, etc. (MacCracken, Michael, Climate Institute)	see previous answer to comment 874.
875	3	23	34	23	59	This is a very difficult paragraph to digest. Appart from the US region for which likelihood terminology is used the reader is not given an assessment of the robustness of the projected changes. I note (again) that, appart for Australia, no uncertainty in the projections is reported. (Brown, Simon, The Met Office Hadly Centre)	see previous answer to comment 874.
876	3	23	34	23	59	We found this paragraph very hard to read and follow. Try bullet points for the summary of the work. (UNITED STATES OF AMERICA)	The cooling trend is in the observed record, not projections.
877	3	23	35	0	0	This seems to contradict earlier text that suggests central North America and the Eastern US would have more of a cooling trend. Since this would include much of the US, its important to continue to note that these areas may differ from the rest of North America. (UNITED STATES OF AMERICA)	This is discussed sufficiently in previous text on observed, not needed again here.
878	3	23	35	23	41	For the North America temperature projections, an aspect which needs more emphasis is that historical data (since 1900) show substantial areas (in the Southeast US, for example) with no warming or even cooling trends, and that a confident explanation for these has not been found. These data and state of science should have some impact on the confidence/likelihood levels used for future projections in this region. (UNITED STATES OF AMERICA)	Accepted
879	3	23	37	0	0	'hot days and nights' should be 'hot days and warm nights' according to table 3.3. (JAPAN)	See answer to comment 845
880	3	23	38	23	38	“Cold days and cold nights are very likely to become much less frequent” How does this square with the cited results of Petoukhov and Semenov (2010)? (UNITED STATES OF AMERICA)	See answer to comment 845
881	3	23	38	23	38	“Cold days and cold nights are very likely to become much less frequent” How does this square with the cited results of Petoukhov and Semenov (2010)? (UNITED STATES OF AMERICA)	yes, added.
882	3	23	38	23	38	It would be preferable to indicate the "mid-range scenario" more specifically--for example, is A1B intended? (IPCC WGII TSU)	Accepted
883	3	23	42	0	0	Typo: delete "was" (Klein Tank, Albert, KNMI)	Accepted
884	3	23	42	23	42	"was projected" --> "projected". (Zwiers, Francis, Environment Canada)	Accepted
885	3	23	42	23	42	delete 'was'. (Stocker, Thomas, IPCC WGI TSU)	Corrected.
886	3	23	42	23	42	"ensemble was projected"... This sentence does not make sense. (Brönnimann, Stefan, University of Bern)	Thanks for the comment but reference doesn't add sufficiently to conclusions to warrant addition at this stage.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
887	3	23	48	23	53	It is worth mentioning here the study by Zanis et al. (2009) that was based on nine individual RCMs that participated in the EU project PRUDENCE. The study concluded that the future mean changes in ΔT2max and ΔT2min are similar to ΔT2mean, but in general the ΔT2max is slightly higher than ΔT2mean and ΔT2min is slightly lower than ΔT2mean especially over the continental parts of Greece in summer. Reference: Zanis P., I. Kapsomenakis, C. Philandras, K. Douvis, D. Nikolakis, E. Kanelopoulou, C. Zerefos, and C. Repapis, Analysis of an ensemble of present-day and future Regional Climate Simulations for Greece, International Journal of Climatology, 29, 1614-1633,DOI: 10.1002/joc.1809, 2009. (GREECE)	see answer to comment 887.
888	3	23	48	23	53	It is worth mentioning here the study by Zanis et al. (2009) that was based on nine individual RCMs that participated in the EU project PRUDENCE. The study concluded that the future mean changes in ΔT2max and ΔT2min are similar to ΔT2mean, but in general the ΔT2max is slightly higher than ΔT2mean and ΔT2min is slightly lower than ΔT2mean especially over the continental parts of Greece in summer. Reference Zanis P., I. Kapsomenakis, C. Philandras, K. Douvis, D. Nikolakis, E. Kanelopoulou, C. Zerefos, and C. Repapis, Analysis of an ensemble of present-day and future Regional Climate Simulations for Greece, International Journal of Climatology, 29, 1614-1633,DOI: 10.1002/joc.1809, 2009. (Zerefos, Christos, Academy of Athens)	Accepted
889	3	23	49	0	0	For tropical nights, we have Tmin>20°C ? (rather than Tmax) (BELGIUM)	Accepted
890	3	23	49	0	0	The passage "...and tropical nights (Tmax >20°C)..." needs to be corrected to "...(Tmin > 20°C)..." (GERMANY)	Accepted
891	3	23	49	0	0	Tmin>20 instead of Tmax>20 (Wibig, Joanna, University of Lodz)	Accepted
892	3	23	49	23	49	For tropical nights, we have Tmin>20°C ? (BELGIUM)	done
893	3	23	50	23	53	It would be preferable to indicate the relevant emissions scenario for this projection. (IPCC WGII TSU)	clarified in text to "is closer to".
894	3	23	52	23	52	Please clarify the meaning of the sentence fragment: "...entirely within the distribution..." This depends on which percentiles of the distribution are referred to. The 5th-95th? The 0th-100th percentiles of a normal distribution spans from minus infinity to plus infinity. (CANADA)	Caption improved as suggested.
895	3	23	61	0	0	Figure 3.4: As per the general comment above, please extend the citation in the caption to something like: [from Orlowsky and Seneviratne (2011), updating the AR4 assessed results of Tebaldi et al. (2006) to include a larger number of GCMs (23) from the CMIP3 ensemble] (Stocker, Thomas, IPCC WGI TSU)	Caption improved and expanded.
896	3	23	62	23	63	The text of the caption indicates that the units of the plots are in fractions of a day, but a couple of the parts of the figure appear to be not in fractions, but in standard deviations. Please clarify. (MacCracken, Michael, Climate Institute)	Replace with "GCM"
897	3	24	1	24	1	explain "20C3M": 20th Century Climate in Coupled Model (see also caption of Figure 3.5) (Stocker, Thomas, IPCC WGI TSU)	Caption improved as suggested.
898	3	24	6	0	0	Figure 3.5: As per the general comment above, please extend the citation in the caption to something like: [from Orlowsky and Seneviratne (2011), updating the AR4 assessed results of Tebaldi et al. (2006) to include a larger number of GCMs (23) from the CMIP3 ensemble] (Stocker, Thomas, IPCC WGI TSU)	Caption improved and expanded.
899	3	24	7	24	8	The text of the caption indicates that the units of the plots are in fractions of a day, but a couple of the parts of the figure appear to be not in fractions, but in standard deviations. Please clarify. (MacCracken, Michael, Climate Institute)	No. Caption is needed, because we give few details of the figure content in the text.
900	3	24	14	24	14	Figure 3.6: The figure caption is unnecessarily long. Most of this information in the caption is clear from the figure legend, while the last part of the caption even begins interpreting the results. The much shorter caption given with the SPM version of the figure should be sufficient here. Along with shortening the Figure 3.8 caption you will reduce the chapter by half a page length! (Stocker, Thomas, IPCC WGI TSU)	Disagree with statement on many levels, declined.
901	3	24	14	24	58	The projection of future temperature from modeling of IPCC scenarios mentioned here, but should be very careful that all these models only give the increasing in temperature when you put more CO2 in the models. These results may mislead the people.Until now, there is no strong evidence to support CO2 causing temperature increasing as modeling results. (Chen, Xing, Nanjing University)	Disagree. Readers who need to know the details, and there are many details they wil need to know, will be able to work this out from the caption.
902	3	24	15	24	33	This is a very long caption. To make things clear, the sentences in it need to be fairly simple. For example, the opening sentence is 3 lines long and has far too much information stuffed into it. In addition, the descriptions of what is presented take up more than a line to explain. I would also suggest that in including the figure in the report, it be spread over two pages so it is large enough to see what is there (perhaps western and eastern hemispheres on facing pages). (MacCracken, Michael, Climate Institute)	Sentence deleted from caption.
903	3	24	32	0	0	Figure 3.6: The passage "...and by about 1-5 years by late-21st century..." is incorrect and needs to be corrected to "...to 1-5 years by..." (GERMANY)	Sentence deleted from caption.
904	3	24	32	24	32	Should it not say "and to about 1-5 years"? (MacCracken, Michael, Climate Institute)	disagree, means change consistently with.
905	3	24	38	24	41	The meaning of "scale" here is not clear. (UNITED STATES OF AMERICA)	changed, removed "can".
906	3	24	39	24	41	"we can?" or "we could?" DO changes scale? Didn't we read somewhere else in document that they don't? It would be helpful to be consistent and more precise here. (UNITED STATES OF AMERICA)	Replace with "over the 21st century"

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
907	3	24	40	0	0	Is 'virtually certain' the appropriate likelihood here? Isn't there an issue with sampling of the 1961-1990 extremes? The "coming decades" mean 1-3 decades to us. The anthropogenic signal is not that strong. (UNITED STATES OF AMERICA)	Disagree. The change has to be placed in the context of a baseline. Statement would be untrue if baseline moved with the projections.
908	3	24	40	24	41	Misleading formulation: The number of hot extremes does not necessarily decrease if a different reference period than 1961-1990 would be applied. (Rapp, Joerg, Deutscher Wetterdienst)	OK, point taken, changed to remove parentheses.
909	3	24	40	24	43	I strongly dislike this mode of writing, in which two sentences are combined into one through the use of substitutable words that are provided in parentheses. This practice saves very little space, but often makes the text substantially more difficult read because it requires that readers carefully parse the sentence twice. I realize that this is a personal preference, but I was heartened recently to see that I am not the only person who reacts negatively to this mode of presentation - Alan Robock recently expressed the same view in EOS. (Zwiers, Francis, Environment Canada)	That may be true, but elaboration and model critique too detailed. We have spent a lot of space already discussing model issues such as these and described how we lower the assessed confidence based on these issues.
910	3	24	41	24	46	Clark et al 2006 figs 2 & 3 demonstrate very clearly the complex alterations to the daily temperature distributions that are modeled for the future, with daily temperture distributions for some regions moving from qasi-normal to almost becoming bimodal and the role soil moisutre has on affecting the distribution shape. An outcome of this sensitivity that has yet to be mentioned in this temperature section is the need, threrefor to get the cpresent day climatological soil moisture right to get a credible future temperature distribution. If the present day is too wet and the model dries it out in the future then the projected changes are erroneously large. (Brown, Simon, The Met Office Hadly Centre)	disagree, authors feel text is not ambiguous.
911	3	24	48	24	49	The sentence fragment “Other local, mesoscale and regional feedback mechanisms, …” is unclear. Suggest changing it to “Regional scale mechanisms, …”. (CANADA)	Revised text to avoid "may".
912	3	24	48	24	50	There are two occurrences of the word "may" needing to be changed. On line 48, perhaps say "is likely to" and on line 50 say "have the potential to" (MacCracken, Michael, Climate Institute)	changed to evaluate, but additional references not needed here.
913	3	24	52	24	54	This statement is innacurate. Roesch (2006), Raisanen (2007) and Brown and Mote (2009) all used observational data to evaluate (not validate) climate models. There are good quality satellite data on snow cover extent for evaluating climate models (NOAA dataset, Robinson et al., 1993), and sufficient in situ snow depth and SWE obs to evaluate climate models over mid-latitudes of the NH. However, there are major data gaps over the Arctic. You should also be aware that the Roesch (2006) paper included an error in the derivation of snow water equivlalent that invalidated one of his main conclusions that the AR4 climate models overestimated SWE in the spring (Brown and Frei, 2007). Additional refs: Brown, R.D. and A. Frei, 2007: Comment on ‘‘Evaluation of surface albedo and snow cover in AR4 coupled models’’ by A. Roesch, J. Geophys. Res., 112, D22102, doi:10.1029/2006JD008339. Räisänen, J, 2007: Warmer climate: less or more snow? Climate Dyn., 30, 307-319, doi:10.1007/s00382-007-0289-y. Robinson, D.A., K.F. Dewey and R.R. Heim, 1993: Global snow cover monitoring: an update. Bull. Am. Meteorol. Soc., 74, 1689-1696. (Brown, Ross, Environment Canada @ Ouranos)	This sentences only refers to Mediterrainian region.
914	3	24	55	24	57	We think it may be better to say that this is true in most regions, because, in general, we expect atmospheric water vapor content to increase with temperature. (UNITED STATES OF AMERICA)	if RH is enhanced, esp. with warming, means a much higher absolute humidity.
915	3	24	56	24	56	Finally mention of humidity, but should it not be "absolute humidity" instead of "relative humidity" that is enhanced? (MacCracken, Michael, Climate Institute)	Authors disagree, prefer current structure.
916	3	24	59	25	16	I don't find it very helpful to present the summary all at the end---this is the type of information that I think should be leading off the various paragraphs and highlighted throughout the section (in italics or bold). I also think it would be helfpul to actally use the definitons for the IPCC lexicon terms, thus say, for example, "In summary, there is more than a 90% likelihood that there has been an overall decrease in ..." or what ever is appropriate. The IPCC lexicon is nice, but in key spots such as this, it would be very helpful to say what it means. I also did not note anything on the heat index/humidity issue in this summary, and in that it seems likely to be a key issue, it should be mentioned. (MacCracken, Michael, Climate Institute)	seasonal changes are discussed in context with figure 3.4 and 3.5. Over-estimation effects on confidence discussed previously.
917	3	24	59	25	16	How are these conclusions affected by the overestimation of changes in temperature extremes in models? Also, we may have missed it but shouldn’t there be some concise discussion of the seasonality of extremes? If increases in warm days and decreases in cold days are strongest in winter they may be much less scary than if the warm day increases are happening in summer. (UNITED STATES OF AMERICA)	see comment and answer number 845.
918	3	24	62	24	63	As per comment 20 this lack of confidence for Asia only arises when considering absolute temperatures rather than anomaous temperatures (Brown, Simon, The Met Office Hadly Centre)	For precipitation section…..
919	3	25	0	30	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)	There is less certainly in the observational record than in the model projections. And signal is presumably smaller now than in the future. But delete "continue to".
920	3	25	2	25	8	Isn't there a logical inconsistency in saying that it is likely that heatwaves have increased in many (but not all) regions, and then saying it is VERY likely that length, frequency and/or intensity of heatwaves will CONTINUE to increase on the global scale? How can continuation of something that is likely be very likely? (UNITED STATES OF AMERICA)	This is a general assessment statement about warm and cold extremes and not about specific extremes. If taken in toto then we are virtually certain of increases in warm extremes, decreases in cold extremes.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
921	3	25	3	25	16	I am concerned that the subtle balance between different aspects of temperature extremes is not clear in these lines. I agree it is vertually certain that the frequency of future hot/cold events will increase/decrease. I agree that the sign of change for heatwaves length/frequency/intensity is very likely to be +ve. However I think it is very uncertain what the magnitude of these changes will be and we are probably less sure of this than we were at AR4 (cf Clark et al 2011, UKCP projections). The uncertainty of the magnitude of future changes is currently missing from this paragraph. The precision of the changes that are then presented in lines 9 to 16 are not justified in the light of this, particularly given that changes in return period are very non-linear with chagnes in tempearture. (Brown, Simon, The Met Office Hadly Centre)	by the end of this century. Text added.
922	3	25	4	0	0	"virtually certain"s - by when? (UNITED STATES OF AMERICA)	modified.
923	3	25	5	0	0	1961-1990 reference climate is not the present climate (see above)! (Rapp, Joerg, Deutscher Wetterdienst)	Comment is about precipitation, but refers to line numbers in temperature summary, not precipitation section.
924	3	25	6	25	9	This statement has some pitfalls: 1. It does not make sense to consider Central and South America as a whole. This continents extends from near 20°N to 60° S having, in addition, the huge geographic feature of the Andes along the west, which contributes to shape regional climates. Since the global warming trend is not homogeneous, atmospheric circulation patterns may change and consequently precipitation trends may show different and even opposite signs in different regions, even in spite of a general global trend to more heavy precipitations forced by the increased water content. I would be more precise to mention the regions (or region) in which the four quoted papers show a positive trend in extreme rainfall events and also where the reported negative trends in winter (Penalba and Robledo 2010) applied, which by the way also show positive annual trends. More correct would be a more regional focused conclusion like the one for North America (line 3 same page) 2. Negative winter trends in the paper of Penalba and Robledo are in regions that does not have the more important heavy precipitations in winter. 3. Citation has mistakes. It should say Re and Barros, 2009 and Penalba and Robledo 2010. 4. The text has some contradictions with Table 3.2, where more precisely two regions of South America are distinguished with different trends, but both with medium confidence. In this table Penalba and Robledo 2010 are quoted as part of the papers that show increased trends. Again, Robledo is misspelled and Re and Barros 2009 is missing. (Barros, Vicente, IPCC WGII TSU)	this is not the precipitation section.
925	3	25	6	25	9	There is overall low confidence in trends for the whole of Central and South America (Table 3.2). Positive trends in extreme rainfall events are evident in parts of southern South America (Dufek and Ambrizzi, 2008; Marengo et al., 2009b; Re and Ricardo Barros, 2009; Sugahara et al., 2009). But negative trends have been observed in winter extreme precipitation in some regions (Penalba and Robeldo, 2010). (Barros, Vicente, IPCC WGII TSU)	could, but not needed in context here.
926	3	25	8	25	16	One could also add that global mean is a weak constraing on the uncertaianty of the regional chagnes in extreme temperature cf Clark et al 2011 (Brown, Simon, The Met Office Hadly Centre)	deleted.
927	3	25	13	25	15	The meaning of "scaling factor" is not clear in this summary statement. (UNITED STATES OF AMERICA)	No. projections of extremes for other scenarios have received less attention.
928	3	25	14	0	0	Is there any (qualitative) indication for the other SRES scenarios? I assume these are modest differences compared to the differences in global warming, but I have no evidence. (Klein Tank, Albert, KNMI)	Sentence deleted.
929	3	25	15	25	16	According to AR4, the likely range of A2 scenarios (1990-2095) is 2-5.4 degree warming. I.e., overlaps with the 2-3 degrees quoted. (SWEDEN)	This is a general statement.
930	3	25	15	25	16	If this refers mainly (or in total?) to the observed period, rather that projections for the 21st Century, it would be good if the textwere revised, for clarity. (SWEDEN)	Do not understand where and how the reviewer wants us to modify. The reviewer's point is already represented in the current draft, e.g., lines 3-4 page 26, and lines 23-26 page 27.
931	3	25	18	0	0	Inconsistency of measurements make difficult the detection of trends. New studies enlarge the conclusions of l'AR 4 (more intense precipitations) but there are no statistically significant trend for de immense regions. The cartography of cumulated precipitations is fractal as seems to be the cartography of intensity with positive or negative, more or less marked, evolutions according to the region. Are they stable and robust ? At small scale most studies are unique on the sale region. The results for extreme precipitations are not robust, and consequences on floods are still less. (BOURRELIER, PAUL-HENRI, AFPCN)	Checked references for possible inclusio - Trenberhh (2011) is general review of possible links between precip and climate, with little focus on extremes. Does not add to the general information in current draft. No need to add reference. Reject the criticism of "no assessment"- many general assessments are made, and no space for detailed assessment of individual studies.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
932	3	25	18	31	7	There is a comprehensive review of changes in precipitation with climate change that should be taken into account in this section and elsewhere: Trenberth, K. E., 2011: Changes in precipitation with climate change. Climate Research, doi:10.3354/cr00953, in press. It is available (as galley proof) on the following web site: http://www.cgd.ucar.edu/cas/Trenberth/trenberth.papers/SSD%20Trenberth%202nd%20proof.pdf. It is especially relevant to p 27 lines 40 to 60. Also relevant to p 28 l 1-6 is this paper. Trenberth, K. E., and D. J. Shea, 2005: Relationships between precipitation and surface temperature. Geophys. Res. Lttrs., 32, L14703, doi:10.1029/2005GL022760. The seasonality of relationships of precipitation to temperature and the implications for change is not adequately addressed. This issue is also discussed in the above reference. There is no assessment made in this section (or in many others): it is all he says this and someone else says that, and there is no assessment of why the differences exist, the different assumptions, data quality or period, and which is right. This is especially egregious on p 29. (UNITED STATES OF AMERICA)	Section 3.3.2 DOES include information on trends (lines 9-20 page 27) and projections (lines 47-58 page 30) of hail.
933	3	25	18	31	8	Including some information in Section 3.3.2 on trends and projections of high-impact precipitation events such as freezing precipitation will be useful. (CANADA)	Agreed. Modify the wording.
934	3	25	20	0	0	Short-time - vague. It needs defined. Day, season, ...? (UNITED STATES OF AMERICA)	Agreed. Modify the wording.
935	3	25	20	25	20	Suggest replacing "Changes" with "Reductions" (it is hard to imaging that increases would lead to drought). (Zwiers, Francis, Environment Canada)	No.
936	3	25	20	25	33	Do you consider information/statistics on extreme snow cover data? (Rapp, Joerg, Deutscher Wetterdienst)	Not clear what reviewer wants to change here.
937	3	25	20	25	33	Again, if you use percentiles or thresholds that are fixed in time, you will find what appear to be disproportionate changes in heavy precipitation as total rainfall increases unless something really unusual is going on with changes to the shape of the distribution. It seems that under such an approach, the changes in extremes (and their potential impacts) get much more attention that the changes in total precipitation (and their impacts). It is the general nature of precipitation that when the mean increases the increase in the extremes seems greater if you use a fixed-in-time value for the definition. But within the distribution itself, the change in the extremes is much more proportional (see Michaels et al., 2004, International Journal of Climatology, 24, 1873-1882, for a demonstration of this—both theoretical and observational). (Knappenberger, Paul, New Hope Environmental Sciences)	Modified the wording (now only one threshold is kept)
938	3	25	25	25	25	Please clarify why 2 thresholds are given for China. (CANADA)	noted, but the reviewer does not recommend how to modify this section.
939	3	25	26	0	0	Some care has to be taken when we make parametric statistics if rain and snowfall are not separated. The distribution of both variables are entirely different, and so the corresponding extreme value distributions are. In such a case, a two-component extreme value distribution is recommended. (BELGIUM)	The major reason is that rainfall and snowfall are not treated separately in literatures. -- Added this to the text.
940	3	25	26	25	26	Suggest explaining why changes in rainfall and snowfall are not treated separately. Users, particularly in snow prone regions, might ask given that the impacts of extreme snowfall can be serious and can be different from the impacts of extreme rainfall. (Zwiers, Francis, Environment Canada)	Reject. Point is clear in context of observing problems with short term events.
941	3	25	28	25	28	The meaning of "increases in public awareness" seems a bit curious. What is the point being made? As on page 11, lines 55-56? (SWEDEN)	Reject. This is a general point - adding the reference and discussion is too detailed for this report.
942	3	25	32	25	33	Give a reference indicating that biases in precipitation measurements are an issue that affects observations of extreme precipitation amounts. I'm not expert on this issue, but when I have raised this issue with colleagues, the response has been that biases (e.g., due to undercatch and wetting loss) is more of an issue for small precipitation amounts than for large amounts). (Zwiers, Francis, Environment Canada)	Agreed and remove the paragraph. The four references given in the current draft mostly discuss paleo and historical changes in MEAN rainfall, not the changes in precipitation extremes.
943	3	25	35	25	46	The paragraph seems a bit out of context (relevance?), and also builds on only two references (low confidence…) (SWEDEN)	Reject. The study of Büntgen et al. (2011) does NOT discuss extreme precipitation. Also see comment 943.
944	3	25	38	0	0	I suggest to mention the new study by Büntgen et al. (2011) which is unique in the sense it presents a precipitation reconstruction for the last 2400 years: ".....Intertropical Convergence Zone. A 2400 years long tree-ring based reconstruction of spring and early summer (AMJ) precipitation for Europe shows that the actual average was often exceeded, and that exceptional variability is reconstructed for the so-called Migration Period between about AD 250 and 550 (Büntgen et al., 2011). Another study for Europe (Pauling and Paeth, 2007) suggested that....". Reference: Büntgen, U., et al., 2011: 2500 years of European climate variability and human susceptibility. Science, 331, 578-582. (Wanner, Heinz, University of Bern)	Agreed. See also response to comment 943.
945	3	25	44	0	0	We are not sure if/how this sentence fits into this subsection. It seems inclonsistent with the first sentence (P25L20). Delete? (UNITED STATES OF AMERICA)	Not aware of research on paleo extreme daily precip. Paragraph on paleo studies now deleted (see #943)
946	3	25	44	25	44	More recent literature is available for paleo precipitation extremes in Europe, eg, research by Luterbacher. We recommend to use this additional literature to strengthen your assessment. (Stocker, Thomas, IPCC WGI TSU)	Agreed, add "over the second half of the 20th century" to the text.
947	3	25	48	25	60	The time period for which the research was conducted should be mentioned (note that the previous paragraph discusses precipitation changes over the last few hundred years). (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Reword to clarify: (a) change "precpitation intensity" to "the Simple Daily Intensity index"; and (b) add "such as Heavy Rainfall Days (R10)"

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
948	3	25	57	25	60	This sentence should be clarified : "precipitation intensity" - for which aggregation time(s) ? Examples of indices for which no statistically significant change was found? (BELGIUM)	See response to comment 948.
949	3	25	59	25	60	The precipitation indices considered should be specified (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	See response to comment 948.
950	3	25	60	0	0	“precipitation intensity”: for which aggregation time? (BELGIUM)	Accept. Reword the sentence as suggested.
951	3	26	1	26	1	First sentence is awkward. Possible rewording: "Recent studies have updated the assessment of the AR4, with more regional results now being available (Table 3.2)." (Stocker, Thomas, IPCC WGI TSU)	Not sure what reviewer wants to change here, or even if his point is valid. Point does not substantially change the discussion. If we were to discuss in the detail the reviewer appears to want we would have a very long SREX.
952	3	26	3	26	4	Statistical signifance seems to be invoked relatively frequently as an assessment criterion. Certainly, statistical significance is one measure by which trends and other changes can be assessed, but from a physical perspective, the determination of significance can be highly variable from one location to the next (with greater detection power where natural variability is small, and where record length is long). Even if changes are not statistical significant locally, changes that are spatially coherent (over regions larger than the spatial decorrelation length scale of the variable in question) would warrant physical interpretation. (Zwiers, Francis, Environment Canada)	Accept.
953	3	26	4	26	4	Delete final sentence - 'More detailed ....'. This is unnecessary text not needed in a concise report. (Stocker, Thomas, IPCC WGI TSU)	Not clear what reviewer wants to change, or his point.
954	3	26	11	26	13	“Gleason et al. (2008) reported an increasing trend in the area experiencing a much above-normal proportion of heavy daily precipitation from 1950 to 2006;” Gleason et al. relied on fixed-in-time thresholds for this assessment. Comparing the amounts of precipitation that fell in heavy events against the actual annual distribution of precipitation events from which the heavy events were drawn from would likely have shown that the proportion of precipitation falling in heavy events was largely unchanged (see Michaels et al., 2004). (Knappenberger, Paul, New Hope Environmental Sciences)	Corrected. Thanks.
955	3	26	16	26	16	"central plains" should read "Great Plains" (MacCracken, Michael, Climate Institute)	Do not understand the reviewer's point. Surely we cannot discuss trends without some consideration of whether they are stronger than natural variability (which is one interpretation of a significance test in this context).
956	3	26	17	26	22	It would be useful to note that people would very likely notice trends well before their statistical significance can be certified--all this reliance on testing the statistical significance needs context--it is the equivalent of getting DNA confirmation of a suspect that one captured at the scene with the gun in their hand--yes, there are possible explanations that exonerate that person, but there is very strong suspicion. This needs to be explained--not suggesting that trends might not be noticeable until well into the future at a time when statistical confirmation can be achieved. (MacCracken, Michael, Climate Institute)	Accept.
957	3	26	22	26	22	“Overall, the evidence indicates a likely increase in observed heavy precipitation in many regions in North America, despite statistically non-significant trends and some decreases in some subregions (Table 3.2).” Add: “This general increase in heavy precipitation accompanies a general increase in total precipitation in most areas of the country.” I understand that this IPCC report is on extremes, but looking at them in isolation fails to deliver an accurate picture of the changes in climate. In planning and adapting to precipitation changes, knowing what is going on with total precipitation is also quite important (Knappenberger, Paul, New Hope Environmental Sciences)	Agreed. This paragraph is significantly modified and now more consistent with Table 3.2.
958	3	26	24	26	24	The basis for the low confidence should be made clear and readers should not have to go to Table 3.2 to find out. The subsequent sentences indicate some disagreement but also imply a scarcity of data. (CANADA)	Agreed. This paragraph is significantly modified and now more consistent with Table 3.2.
959	3	26	24	26	27	Some additional discussion supporting this assessment would be useful. Do you have low confidence in the available trend estimates themselves (e.g., because of data quality or methods used), or is confidence low because the available published trends may not represent the region well as a whole? (Zwiers, Francis, Environment Canada)	Reject. Not relevant.
960	3	26	24	26	27	What is assumed about deforestation in the scenarios for South America? This clarification is needed because in Ch. 4, tropical deforestation is claimed to decrease local precipitation. That is a separate issue addressed in other comments later on for Ch. 4, but given the emphasis deforestation is given in Ch. 4, the assumptions in the scenario about deforestation need to be clarified here in Ch. 3. (UNITED STATES OF AMERICA)	Not sure which studies are meant. Some are already cited, others are too old (pre-AR4).Too many European references already.
961	3	26	29	26	42	In relation to heavy precipitation in Europe, would expect to see studies by C. Frei, and C. Schär cited here. We recommend to use this additional literature to strengthen your assessment. (Stocker, Thomas, IPCC WGI TSU)	Noted. However,reject since the suggested references would not significantly enhance the current draft and because of space limit also.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
962	3	26	34	26	38	There are some contradicting results for Greece. Although there is an overall consensus that the climate is getting drier (e.g. Nastos and Zerefos, 2008, Kioutsioukis et al., 2010) there are deviating results about the frequency and intensity of extreme precipitation. For example, Kioutsioukis et al. (2010) found that extreme precipitation has been decreasing while it extreme precipitation events exhibit increased variability without following any coherent regional pattern. On the other hand, Nastos and Zerefos (2007) found increasing trends in the percentages of the number of wet days exceeding specified thresholds (such as 10, 20, 30 and 50 mm) in Athens. References: Nastos, P.T., Zerefos, C.S., 2007. On extreme daily precipitation totals at Athens, Greece. Adv. Geosci. 10, 1–8. Nastos, P.T., Zerefos, C.S., 2008. Decadal changes in extreme daily precipitation in Greece. Adv. Geosci. 16, 55–62. (GREECE)	See response to comment 962.
963	3	26	34	26	38	There are some contradicting results for Greece. Although there is an overall consensus that the climate is getting drier (e.g. Nastos and Zerefos, 2008, Kioutsioukis et al., 2010) there are deviating results about the frequency and intensity of extreme precipitation. For example, Kioutsioukis et al. (2010) found that extreme precipitation has been decreasing while it extreme precipitation events exhibit increased variability without following any coherent regional pattern. On the other hand, Nastos and Zerefos (2007) found increasing trends in the percentages of the number of wet days exceeding specified thresholds (such as 10, 20, 30 and 50 mm) in Athens. References Nastos, P.T., Zerefos, C.S., 2007. On extreme daily precipitation totals at Athens, Greece. Adv. Geosci. 10, 1–8. Nastos, P.T., Zerefos, C.S., 2008. Decadal changes in extreme daily precipitation in Greece. Adv. Geosci. 16, 55–62. (Zerefos, Christos, Academy of Athens)	Thanks. Remove Ntegeka and Willems (2008).
964	3	26	36	0	0	It is not correct that increasing trends in the percentiles are observed in (Ntegeka and Willems, 2008). They have never used percentiles, and in addition, they have introduced a new method (the quantile perturbation approach) for assessing trends in extremes. The major conclusion in that paper is the presence of multi-decadal oscillations in rainfall extremes. Increasing trend are not reported there. (BELGIUM)	Accepted.
965	3	26	41	26	41	"associated with" would be better wording than "caused by" (Global Climate Observing System Steering Committee)	Agreed and modified the text as suggested. "Low tomedium confidence" in both continents.
966	3	26	44	0	0	Why "low confidence" for Asia and "low to medium confidence" for Africa in line 62. My judgement based on the papers cited would be reverse. (Klein Tank, Albert, KNMI)	Noted, but two references are already cited for China, and adding the suggested references would not enhance the assessment significantly.
967	3	26	44	26	55	Two more recent references should be included for regional trends of precipitation extremes in China and (Su, B., T. Jiang, and M. Gemmer, 2007: Spatial and temporal variation of extreme precipitation over the Yangtze River Basin. Quaternary International, 186 (2008), 22-31; Gemmer, M., T. Fischer, T. Jiang, B. Su, and L. Liu, 2011: Trends of Precipitation Extremes in the Zhujiang River Basin, South China. Journal of Climate, 24 (3), 750-761) (CHINA)	Thanks. Add assessment of Gallant and Karoly (2010) to text and Table 3.2, also to the reference list.
968	3	26	57	26	59	Gallant and Karoly (2010) have shown that there have been reductions in the area of southeast Australia experiencing heavy rain days when mean rainfall has reduced but NOT in southwest Australia, where it has not changed. Also, the statement that there is a lack of literature on trends in northern Australia heavy precipitation is erroneous. Gallant and Karoly (2010) looked at how much of northeast and northwest Australia have shown changes in the proportion of annual/seasonal rainfall stemming from heavy rain days. Specifically, there were statistically significant increases from 1911–2008 and 1957–2008 in the northwest of the continent. Full reference: Gallant, A. and Karoly, D., 2010. A combined climate extremes index for the Australian region. Journal of Climate, 23, 6153-6165, DOI: 10.1175/2010JCLI3791.1 (AUSTRALIA)	Accepted.
969	3	26	58	0	0	Table 3.2 (page 199, column 4 - Heavy precipitation, N. Australia and S. Australia/NZ): Following from the previous comment, remove "Insufficient studies for assessment" from the box N. Australia. Should be replaced with "Low to Medium confidence (??): Increases in the proportion of total rainfall stemming from heavy precipitation events (Gallant and Karoly, 2010)" or similar. Full reference: Gallant, A. and Karoly, D., 2010. A combined climate extremes index for the Australian region. Journal of Climate, 23, 6153-6165, DOI: 10.1175/2010JCLI3791.1 (AUSTRALIA)	Deleted "lack of literature" and added assessment of Gallant and Karoly (2010).
970	3	26	58	26	59	Do you mean "lack of (sufficient) literature"? If there is no literature, what is the basis for the "low confidence" assessment? (Zwiers, Francis, Environment Canada)	Checked the AR4 and did NOT find such assessment. The text is corrected/deleted.
971	3	27	4	27	4	Specify Chapter in IPCC AR4 WGI which "reported a decrease in heavy precipitation in east Africa..." (Stocker, Thomas, IPCC WGI TSU)	Cannot find papers. Cannot add much detail on this aspect.
972	3	27	9	27	20	This sub-section bout hail can be much more detailed including the three above cited papers and the studies reported in the bibliography of Eccel et al. 2011 (ITALY)	OK.
973	3	27	10	27	10	"environment" --> "environmental" (Zwiers, Francis, Environment Canada)	OK.
974	3	27	10	27	10	Should read: "...changes in environmental conditions conducive to hail are used to infer changes in hail occurrence". (Stocker, Thomas, IPCC WGI TSU)	Here we mean the estimation of hail occurrence from radiosonde data is assoicated with uncertainty. Modified the text.
975	3	27	11	27	12	The assessment that reanalyses or radiosonde data are associated with high uncertainties should be supported with references. Also, it is not clear if the authors regard both, or just the latter, as being uncertain. (Zwiers, Francis, Environment Canada)	Here we mean the estimation of hail occurrence from radiosonde data is associated with uncertainty. Modified the text.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
976	3	27	12	27	12	Is it the radiosonde data themselves that are"associated with high uncertainty" or the estimation of hail occurrence from (relatively sparse) radiosonde data that is the main source of uncertainty? If this is a criticism of the quality of radiosonde data, then a reference should be given. (Global Climate Observing System Steering Committee)	Deleted this sentence.
977	3	27	13	0	0	There is no assessment in this discussion. Delete or make an assessment. (Stouffer, Ronald, NOAA)	Reject. Kunz and Kuttmeier (2009) already cited in the current draft.
978	3	27	13	27	13	Include: According to the study of Kunz (2007) for a test region in Germany, several convective parameters such as the convective available potential energy are directly related to hailstorm occurrence in the past. Therefore, changes in thunderstorm potential can be estimated from changes in stability. 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))	Delete DeRubertis (2006) and Changnon and Changnon(2000).
979	3	27	14	27	14	Why is it important to tell readers that Changnon and Changnon described five types of variation? (Zwiers, Francis, Environment Canada)	Apply to severe thunderstorms.
980	3	27	14	27	16	Does the statement about strong variability but no strong trend apply to severe thunderstorms or to hail associated with such events? (CANADA)	This report does not cite unpublished analysis.
981	3	27	17	27	18	Unpublished DWD analysis show other hail trends during other periods (e.g. decreasing between 1950 und 2010). (Rapp, Joerg, Deutscher Wetterdienst)	Reject. Vinet (2001) provides climatology of frequency and intensity of hail over France, but NOT the trends.
982	3	27	20	0	0	Add a comment on the results by about hail trends in France as discussed in Vinet, F., 2001: Climatology of hail in France. Atmospheric Research 56, 309-323 (ITALY)	Searched the journal online, but did not find the paper. Seems too local for SREX because of space limits.
983	3	27	20	0	0	Add a comment on hail climatology in North Italy as discussed in Eccel, E., P. Cau, K. Riemann-Campeb and F. Biasiolic, 2011: Quantitative hail monitoring in an alpine area: 35-year climatology and links with atmospheric variables. International journal of climatology DOI: 10.1002/joc.2291 (ITALY)	See response to the next comment (984).
984	3	27	25	27	26	Perhaps rrends are also more consistent in winter in other regions, as well as in Europe? Isn't this finding one that is expected? (Zwiers, Francis, Environment Canada)	Text modified as suggested.
985	3	27	25	27	26	Nothing was said about seasonal changes for most regions. Therefore, suggest this sentence be rephrased as follows: "In particular, many regions.....and, where seasonal changes have been assessed, variations between seasons are evident." (CANADA)	Delete "whole".
986	3	27	26	27	27	The statement that this assessment applies to the whole continent does imply some expert judgement since data coverage suitable for extremes analysis, for example, in HadEX, is largely confined to the US and a narrow strip in Canada along the US-Canada border. (Zwiers, Francis, Environment Canada)	Improve readability by putting summaries in bold. Summary paragraph moved to end of section.
987	3	27	28	27	29	It should be made more visible that the description of observed precipitation ends here while the specification of projected precipitation extremes is begun. (Rapp, Joerg, Deutscher Wetterdienst)	Reject due to space limit.
988	3	27	41	27	44	You could also mention Kharin et al (2007), who show broad consistency with Clausiun-Clapeyron for long return period extremes. In particular, see their Figure 16 and associated discussion. (Zwiers, Francis, Environment Canada)	Agreed. Text modified.
989	3	27	42	27	44	This is a very strong statement. Is it supported by equally strong evidence? No reference is made to the modeled warmer world, so presumably it is being stated with reference to the real world. Please clarify. (UNITED STATES OF AMERICA)	Accept. Text modified.
990	3	27	43	27	63	The thermodynamic constraint using the Clausius-Clapeyron equation holds true only over regions with sufficient water supply such as wet lands or oceans. Over other land surfaces (e.g., arid regions), this relation is not a sound estimate for extreme precipitation. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Accepted and delete the word "may".
991	3	27	44	27	45	The word "may" is used twice in the same phrase of the sentence--both need to go, replaced by the IPCC lexicon--otherwise, the sentence means nothing as "may" can be interpreted as from 1 to 99%. Also see lines 50-51 where "may" is again used twice. (MacCracken, Michael, Climate Institute)	Accepted. Modify the text for consistency, i.e., add "at many regions" after "the observed increase in extreme precipitation".
992	3	27	45	0	0	The unequivocal reference here to "the observed increase in extreme precipitation" seems to be at odds with more moderate assessments stated just a couple paragraphs earlier. (UNITED STATES OF AMERICA)	Text modified to clarify that it is in climate model projections (Pall 2007).
993	3	27	51	27	52	Are we talking about the real world or models here? (UNITED STATES OF AMERICA)	Uncertainties of projections of heavy precipitation due to coarse resolution is pointed out in the current draft, e.g. lines 42-43 page 30.
994	3	27	56	27	57	It is interesting it is suggested that models are not simulating the full range of rainfall extremes (I'd guess due to limited resolution) whereas earlier it was siad that models over-predicted temperature extremes. Interesting--might be related; might be worth connecting, and at least mentioning that lack of resolution can be a real problem, expecially in orgraphically varied terrain. (MacCracken, Michael, Climate Institute)	Accept. Text modified.
995	3	27	58	27	62	Consistency between this sentence and page 14, lines 2-4, should be ensured. Also, instead of just saying "twice as fast," it would be helpful to indicate the range of rates by which hourly precipitation extremes might increase above what would be assumed from the Clausius-Clapeyron relationship. (IPCC WGII TSU)	Do not understand the reviewer's point. Detection discussed in 3.2.2.
996	3	28	1	0	0	Is detection defined someplace? (UNITED STATES OF AMERICA)	Accept. Add Pall et al. (2011) after '(Min et al. 2011)'. Also, need to update the status of Pall et al. (2011) in the reference list: Nature, Volume: 470, Pages: 382–385 Date published: (17 February 2011) DOI: doi:10.1038/nature09762

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
997	3	28	2	28	6	This needs to be nuanced a bit further given Pall et al (2011), who considered a fall event. (Zwiers, Francis, Environment Canada)	Accept. Text modified.
998	3	28	3	28	6	It would be helpful to clarify the mentioned "50% chance of detecting anthropogenic influence"--does this mean there is a 50% chance of an anthropogenic influence or 50% chance that an anthropogenic influence can be detected? (IPCC WGII TSU)	Accepted.
999	3	28	4	28	4	Should this say "For example, there is now about …"? Otherwise, not very clear. (MacCracken, Michael, Climate Institute)	Agreed. After discussion within the author team, it was concurred that the term "more likely than not" can indeed be considered distinct from "medium confidence" (corresponding to cases with high confidence but low signal to noise ratio). These two terms are not equated anymore. The AR4 assessments cannot be directly compared to our assessments since we follow the new IPCC uncertainty guidelines. (YL) After discussion within the author team, it was concurred that the term "more likely than not" can indeed be considered distinct from "medium confidence" (corresponding to cases with high confidence but low signal to noise ratio). These two terms are not equated anymore. The AR4 assessments cannot be directly compared to our assessments since we follow the new IPCC guidelines.
1000	3	28	11	28	11	I disagree - I think this does modify the AR4 assessment. An assessment of "medium confidence" should not be regarded as being the equivalent of a likelihood assessment of "more likely than not". See my previous comment concerning page 9, line 13 (3.1.5). (Zwiers, Francis, Environment Canada)	Comparison with AR$ deleted. See #1000.
1001	3	28	11	28	11	Suggest replacing "does not modify" with "consistent with." This is not just an editorial suggestion but a change that reinforces that this Special Report confirms the results of the AR4. (CANADA)	Done
1002	3	28	11	28	12	The author team might consider characterizing this statement with summary terms for evidence and agreement. (IPCC WGII TSU)	Reject. "or" is from AR4. Statement does not equate these two terms - in fact it is the reverse.
1003	3	28	15	31	1	Twice in this section a text passage can be found which suggests that the frequency of heavy precipitation is identical to the proportion of total rainfall from heavy falls (in both cases the word "or" is used). This is not necessarily true. Since the threshold defining heavy precipitation is generally determined using data from the control period, it is well possible that this threshold is exceeded more frequently in the projection time period although the proportion of total rainfall from heavy falls does not change due to a simultaneous increase in the total amount of rainfall (GERMANY)	Agree, but the current draft does point out the large uncertainties in projections of changes in heavy precipitation. And it is clear from earlier sections that such uncertainties have been used in assessing over confidence. (section 3.1.5)
1004	3	28	20	28	49	It should be noted that the CMIP models have projected a faster rate of warming than has occurred since the beginning of the SRES projected scenarios. Also, there is still much debate as to whether or not the CMIP models are correctly projecting the vertical warming patterns in the tropics (e.g. Douglass et al., 2007, International Journal of Climatology; Santer et al., 2008, International Journal of Climatology; Christy et al., 2010, Remote Sensing). Together, at the current time, this should cast some decrease in the confidence that you have in the model precipitation regimes (and their changes). (Knappenberger, Paul, New Hope Environmental Sciences)	Do not understand the reviewer's point.
1005	3	28	20	28	59	The same thing as above should be careful when citing the projections from IPCC modeling results. (Chen, Xing, Nanjing University)	Agreed. Delete "leading to increased robustness of the projected changes".
1006	3	28	23	28	23	I'm not sure that I would characterize the outcome of this additional work as resulting in greater robustness. Uncertainty has been more fully explored - but the assessment of uncertainty is still subject to large caveats given the limitations of the representation of precipitation producing processes in global and regional models. (Zwiers, Francis, Environment Canada)	No. Seems appropriate placing in this section.
1007	3	28	25	28	26	Suggest to move up this information on the expansion to a larger number of GCMs in the study Orlowsky and Seneviratne, 2011. This is important information in support of updating the Tebaldi et al. study and should be highlighted the first time the study by Orlowsky and Seneviratne, 2011, is mentioned in the Chapter (section 3.1, in the context of Figure 3.1) (Stocker, Thomas, IPCC WGI TSU)	Accepted.
1008	3	28	31	28	31	Delete: "All three indices were also considered in Tebaldi et al. (2006). This is already clear from the fact that Orlowsky and Seneviratne 2011 is an update and extension of the previous work. (Stocker, Thomas, IPCC WGI TSU)	Accepted. Replaced "extreme" with "stronger".
1009	3	28	48	28	48	Replace "extreme emissions scenarios" with 'high-CO2 emissions scenarios'. (Stocker, Thomas, IPCC WGI TSU)	Citation extended and improved.
1010	3	28	51	0	0	Figure 3.7: As per the general comment above, please extend the citation in the caption to something like: [from Orlowsky and Seneviratne (2011), updating the AR4 assessed results of Tebaldi et al. (2006) to include a larger number of GCMs (23) from the CMIP3 ensemble] (Stocker, Thomas, IPCC WGI TSU)	Citation extended and improved.
1011	3	28	52	28	53	It is really not clear what the units are here--changes in standard deviations, in fraction, what? (MacCracken, Michael, Climate Institute)	Citation extended and improved.
1012	3	28	59	0	0	Figure 3.8: The figure caption is unnecessarily long. Most of this information in the caption is clear from the figure legend, while the last part of the caption even begins interpreting the results. The much shorter caption given with the SPM version of the figure should be sufficient here. (Stocker, Thomas, IPCC WGI TSU)	Figure improved

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1013	3	28	59	0	0	Figure 3.8: Suggest to use consistent Y-axes for all bar plots on panel a) to improve cross-region comparability. This then makes this panel also structurally consistent with panel b) as well as with both panels a) and b) of Figure 3.6. (Stocker, Thomas, IPCC WGI TSU)	No. needs to stand alone.
1014	3	28	59	29	19	The legend of figure 3.8 is very long. It should be integrated in the manuscript text - so far as possible. (Rapp, Joerg, Deutscher Wetterdienst)	Figure is being improved for readability.
1015	3	28	60	28	60	For readability, this figure should be spread across two facing pages. (MacCracken, Michael, Climate Institute)	Delete "Orlowsky and Seneviratne (2011)".
1016	3	29	21	29	23	Shouldn't this be the other way around? Orlowsky and Seneviratne confirm previous results, given that this study is not yet published? It's a bit odd to say that earlier studies confirm results from a subsequent study. (Stocker, Thomas, IPCC WGI TSU)	Accepted.
1017	3	29	27	29	27	Delete: "and drought" - keep the focus on precipitation. (Stocker, Thomas, IPCC WGI TSU)	Reject. Do not see relevance of comment.
1018	3	29	28	0	0	Aerosols changes matter for rainfall changes over India. Delete this sentence. (UNITED STATES OF AMERICA)	Accepted.
1019	3	29	31	29	31	Delete: "and less severe drought" - keep the focus on precipitation (Stocker, Thomas, IPCC WGI TSU)	Accepted.
1020	3	29	38	29	39	Delete final sentence - 'In addition ....'. This is unnecessary text not needed in a concise report. (Stocker, Thomas, IPCC WGI TSU)	Accepted. Add explanation as suggested.
1021	3	29	41	29	41	Suggest providing an explanation about why high resolution is important for studies of precipitation extremes. It is believed that this is not because the forced signal is likely to have spatial structure on a fine spatial scale, but that the physical processes responsible for extreme precipitation require high spatial resolution to resolve them. (CANADA)	Reject due to both space limit and insignificant enhancement to the current draft by suggested addition of Wehner et al. (2010).
1022	3	29	41	30	12	This paragraph starts out by saying that three approaches have been used to obtain high resolution projections of changes in extremes, but it does not appear to provide an assessment of the three approaches. Presumably it should. An additional paper that could be assessed in this context would be Wehner et al (2010, Clim Dynamics, 34, 241–247, DOI 10.1007/s00382-009-0656-y), who study how the intensity of precipitation extremes change with resolution in a given climate model. (Zwiers, Francis, Environment Canada)	Rejected due to space limit.
1023	3	29	41	30	12	Quasi-multi-model ensemble or multi-SST ensemble approach with high-spatial resolution model was used for future change in the 5-day rainfall total for South America in Kitoh et al. (2011). Kitoh, A., S. Kusunoki and T. Nakaegawa, 2011: Climate change projections over South America in the late twenty-first century with the 20-km and 60-km mesh MRI-AGCM. J. Geophys. Res, VOL. 116, D06105, 21 PP., doi:10.1029/2010JD014920. Quantification of uncertainty in future climate projection may be essential even for high-spatial projections. (Nakaegawa, Toshiyuki, Meteorological Research Institute)	Accepted.
1024	3	29	43	29	47	Delete sentence - "Kamiguchi et al. (2006) is an example of studies that employed the first approach." Reword the next sentence to: "Based on the Meteorological Research Institute and Japan Meteorological Agency (MRI-JMA) 45 20-km horizontal grid AGCM, heavy precipitation was projected to increase substantially in south Asia, the Amazon, and west Africa, with increased dry spell persistence projected in South Africa, southern Australia, and the Amazon at the end of the 21st century (Kamiguchi et al. 2006)" (Stocker, Thomas, IPCC WGI TSU)	Reject. Provides a good specific example of a high-resolution GCM use.
1025	3	29	44	29	44	Unless there is a specific reason for mentioning the GCM in question, suggest leaving it out, as in other cases, such detail is not added. For clarity. (SWEDEN)	Reject, no space for such details.
1026	3	29	45	29	45	Define "time slice mode" (UNITED STATES OF AMERICA)	Text modified significantly and now is consistent with Table 3.2.
1027	3	29	49	29	49	Here and in Table 3.2, it's not clear why the Goswami et al study in science is not discussed. Is it because it was prior to the AR4? It seems like the table in general is heavy on references to Alexander's global analysis when the text includes many references for specific regions. (Lobell, David, Stanford University)	Frei et al. 2006 is cited. Previous studies generally not cited because previous to AR4.
1028	3	29	52	29	59	In relation to heavy precipitation in central Europe, would expect to see studies by C. Frei, and C. Schär cited here. (Stocker, Thomas, IPCC WGI TSU)	Accepted.
1029	3	29	56	29	56	"Low confidence," assuming it is being used her per the AR5 Guidance Note on Treatment of Uncertainties, should be italicized. (IPCC WGII TSU)	Delete "compared with the uncertainties"
1030	3	29	61	29	61	It would be helpful to clarify what "uncertainties" are meant here. (IPCC WGII TSU)	Accepted.
1031	3	29	62	29	62	Should read: "....the high-CO2 A2 emission scenario". (Stocker, Thomas, IPCC WGI TSU)	Accepted.
1032	3	30	9	30	9	Not sure what you mean with "for warm or rainy season in Japan"? It would be more useful to actually give months here, eg, "for June through to September". (Stocker, Thomas, IPCC WGI TSU)	The possibility that shifts in climate zones might lead to regiaonal changes that do not equate to a global average change is now discussed in section 3.1.6. Nevertheless, the question as to whether there has been any sort of goblally-consistent change is often asked and needs to be answered.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1033	3	30	14	30	16	This discussion of the fraction of areas experiencing one outcome or the other would, it seems to me, only be appropriate if the situation being faced were changes under equilibrium conditions where there was an expectation that every region could possibly go up or down. That is not the situation we are in--we are in a situation where climatic zones are shifting and where land masses are not equally spread by latitude and where area does not contract at higher latitudes (and maybe also where there were no mountain chains anchoring particular conditions, etc.). The conclusion should thus not be about relative sizes of areas, but in consistency between model projections and observations--indicating that some areas are expected to get drier and some wetter, some to have more heavy precipitation and some not. For example, in the US, heaviest precipitation (other than tropical cyclones) is in convective fronts. Well, the locations of these is going to shift as the Arctic warms, with its less cold air not able to push back as hard against the moist air from the Gulf of Mexico and warm Atlantic, etc.--so the fronts will occur further to the north (on average). As the seasonal swing shifts further north, some regions will get more extreme rains and some less--this is what is expected. The present text sort of implies that every location is expected to experience the same outcome and so there is less confidence in the result because this is not happening. Well, the expectation is what is wrong--not the result, as likelihoods vary by location. And confidence in our projections is not from a more robust accounting of the relative size of the various areas, but of how well the models and observations are in accord. (MacCracken, Michael, Climate Institute)	Likely is weaker assessment than in AR4. Already includes consideration of issues regarding model simulation of extremes. Regarding changes in hurricane-related precipitation rates Knutson (2010; Nature Geoscience) indicates an increase in a warmer climate.
1034	3	30	14	30	16	Re the likely increase in heavy precipitation in the tropics in particular, O'Gorman and Schneider (PNAS 2009) note that "In the tropics, preciipitation extremes are not simulated reliably and do not change consistently among climate models." Is likely still justified in light of this? This is relevant to assessing tropical heavy precip change as likely elsewhere up the chain in the report, including the section and chapter summaris and Summary for Policymakers. On the other hand, there is a consistent signal among existing models of an increase in hurricane-related precipitation rates in a warmer climate (Knutson et al, GRL, 2010) (UNITED STATES OF AMERICA)	Reject. AR4 used same construction.
1035	3	30	14	30	17	We don't understand the use of the conjunction "or" here. The statement seems to say that it is likely that at least one of these two things is likely. What is the assessment for the two elements separately? (This comment flows to other locations in the document.) (UNITED STATES OF AMERICA)	Yes. As discussed in 3.1.5 the ability to simulate extremes is one factor used in assessments of confidence.
1036	3	30	14	30	18	I agree with the assessment. Presumably this also accounts for concerns about the representation of the details of precipitation producing processes in models (Dai, for one, has been quite vocal on this - see, for example, Dai 2006, JClim; Sun et al. 2006, 2007). (Zwiers, Francis, Environment Canada)	Paragraph deleted and content now just included in final summary, bolded, paragraph.
1037	3	30	14	30	18	It seems that this summary would be better placed after the following paragraph discussing precipitation uncertainties and before discussing the hail projections (i.e., after lines 20-45, before the paragraph starting on line 47). (Stocker, Thomas, IPCC WGI TSU)	Paragraph deleted and content now just included in final summary, bolded, paragraph.
1038	3	30	14	30	18	Having summary statements in bold would help the reader (MacCracken, Michael, Climate Institute)	Comparison with AR4 assessment now deleted.
1039	3	30	17	30	17	What is the meaning of "more robust"? Does it imply "high(er) confidence" level? (SWEDEN)	Comparison with AR4 assessment now deleted.
1040	3	30	17	30	17	It is not clear what is meant by robust in this context and whether its use is justified here. Does robust mean that the forecast is more likely to be correct? Since this is effectively a single probabilistic forecast (P>66% that precip extremes will increase), this will be hard to verify. If extremes precip does not increase is the 'likely' statement more correct than the 'very likely' statement? This is hard to say based on a single forecast. Does it mean that the assessment of probability is based on more evidence? This is true, but this is bound to be the case given that this report was published later and assesses more recent literature than the AR4. By this measure all forecasts considered in this report are more robust than those in the AR4. Or does 'robust' imply that the probability assessment is a better reflection of the available literature than that which was made in the AR4? It is doubtful whether the authors mean to imply that they are better at assessing the evidence than the AR4 authors. In summary, it is suggested that 'but more robust' be deleted. (CANADA)	Comparison with AR4 assessment now deleted.
1041	3	30	17	30	18	If indeed the result is more robust, then why is there not a change here indicated in the level of confidence? This really needs to be done. (MacCracken, Michael, Climate Institute)	Comparison with AR4 assessment now deleted.
1042	3	30	18	30	18	Rather than saying "based on more numerous lines of evidence" you should be more specific, eg, based on a larger range of model ensembles etc, or more detailed process understanding etc. (Stocker, Thomas, IPCC WGI TSU)	Accept. Remove the name of the RCM.
1043	3	30	27	30	27	Unless there is a specific reason for mentioning the RCM in question, suggest leaving it out, as in other cases, such detail is not added. For clarity. (SWEDEN)	Reject. Sentence does not comment on "all statistical downscaling schemes".
1044	3	30	29	30	32	I think this needs to be better nuanced - an evaluation of two statistical downscaling schemes presumably does not provide an adequate basis for making statements about all statistical downscaling schemes, given the large variety of approaches that are possible. (Zwiers, Francis, Environment Canada)	Reject. Cannot see relevance of ENSO impact on this part of the chapter.
1045	3	30	36	30	37	You could also cite Zhang et al (2010, ENSO influence on extremes) in this context as an example for North America. (Zwiers, Francis, Environment Canada)	The reviewer does not provide relevant references to enhance the assessment of importance of natural variablity for understanding the trends in the observations.
1046	3	30	36	30	37	This implies that natural variability is also important for understanding the recent trends. This is somewhat underexposed in the section where the trends in the observations are discussed. (Klein Tank, Albert, KNMI)	Kitoh et al. (2009) is already cited. Model resolution already cited as a posisble cuase of uncertainty.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1047	3	30	43	30	45	Kitoh et al. (2011,JGR, see above) pointed out that the magnitude of changes in precipitation extremes depend on spatial resolution; higher spatial resolution models shows larger magnitude of changes. This may be a possible uncertainty to be addressed. (Nakaegawa, Toshiyuki, Meteorological Research Institute)	OK.
1048	3	30	47	30	47	"Low," assuming "low confidence" is being used here per the AR5 Guidance Note on Treatment of Uncertainties, should be italicized. (IPCC WGII TSU)	Do not understand comment. Presumably reviewer has indicated incorrect page/line.
1049	3	30	49	0	0	No assessment in these sentences. Delete or make an assessment. (UNITED STATES OF AMERICA)	Nevertheless we need to cite the study because it is one of very few that posisbly address hail.
1050	3	30	52	30	58	CAPE is not represented well in models. The paper cited do not contain comparison of projected values and observations in so called reference period, so the confidence in the projection is low and it should be clearly described in the text. (Wibig, Joanna, University of Lodz)	See comment 1033.
1051	3	30	60	30	62	Again, it is just not proper to be suggesting that human activities are affecting the climate by there being more areas experiencing heavier precipitation or not--I don't think anyone has said that every location is projected to experience heavier precipitation. The projection has been that a greater fraction of rains are in the heavier categor, not specifying this happens at every fixed location--these are different statements. With shifting climate zones, different locations have different expectations and what needs to be looked at are how simulations and observations are comparing, with some with heavier precipitation and some not. (MacCracken, Michael, Climate Institute)	If reader is not interested in details she can skip to final paragraph. Have now re-introduced subsection headings to help reader.
1052	3	30	60	31	7	This chapter is 5 pages long, basically with a lot of long paragraphs and not a lot of help throughout on the key findings, etc. Having to wade through 5 pages is just too much. The paragraphs need to have informative topical sentences that present interesting results and put in italics or bold so the material even gets a glance. (MacCracken, Michael, Climate Institute)	Delete "may".
1053	3	30	61	30	62	Saying there is "medium confidence" that something "may" happen should be avoided, as "may" can cover a very low probability of occurrence. The sentence also refers only to changes, not the size or magnitude of the changes. Can "may" be deleted? Otherwise it might be considered that we have high confidence that a change (of some sign or other, and perhaps very small) has occured due to anthropogenic influence. It would be surprising if anthropogenic influence would have no consequence whatsoever. (Global Climate Observing System Steering Committee)	Delete "may".
1054	3	30	62	30	63	Assigning confidence to a statement that changes in extreme preciptation at global scale MAY HAVE BEEN anthropogenic is not a very useful assessment. One could also say they may not have been. We suggest deleting or rephrasing. Other similar instances may be present in the report draft. (UNITED STATES OF AMERICA)	Accepted.
1055	3	30	63	30	63	Delete "may". Assigning 'Medium confidence' already includes an element of uncertainty. (Stocker, Thomas, IPCC WGI TSU)	Accepted.
1056	3	30	63	30	63	Is "anthropogenically related" the right term to use? Perhaps "anthropogenically influenced" would be better. (Stocker, Thomas, IPCC WGI TSU)	Not relevant to precipitation (section 3.3.2).
1057	3	31	0	34	0	Section 3.3.3: I miss specifications about measurement/averaging intervals in the whole section (gusts, 1-min mean, 10-min mean, daily mean, daily maximum...?). (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Accpeted.
1058	3	31	2	31	3	Bolded concluding statements for the other extremes do not cite tables or figures. Delete the citation here to be consistent. (Stocker, Thomas, IPCC WGI TSU)	Done.
1059	3	31	3	31	3	Delete "Some" (Stocker, Thomas, IPCC WGI TSU)	RP events are not moving. Time slices are implied here.
1060	3	31	3	31	7	Talk of RP events moving from 1 in 20 to 1 in 5 should be avoided as it implies a trend, which invalidates EV theory! Better to use AEP. (UNITED KINGDOM OF GREAT BRITAIN AND NORTHERN IRELAND)	See comments 1033.
1061	3	31	4	31	7	Again, not everywhere is projected to have the same result--and not every region is expected to have an increase in either global average or extreme precipitation. As climate zones shift, so do expectations, with some likely getting more, more intense events, and some less. The phrasing here implies that everywhere shoudl be having the same trend in outcomes, and this is just not correct, given the climatic zones, orographic features, locations of continents with respect to climate zones, and more. So, this needs rephrasing--and a re-evaluation of the relative likelihood and whether it has changed from the AR4. (MacCracken, Michael, Climate Institute)	Rewording as suggested.
1062	3	31	5	31	5	This is awkwardly expressed; "could likely" suggests that the "likely" assessment is conditional on something. Maybe it would be better to say "It is likely that ....". (Zwiers, Francis, Environment Canada)	Rewording as suggested.
1063	3	31	5	31	5	Replace "could" with 'will'. Using the term 'likely' already includes an element of uncertainty. (Stocker, Thomas, IPCC WGI TSU)	We are aware of these limitations and include a sentence stating the limitations of the wind data and models.
1064	3	31	9	0	0	Nothing really crédible on wind. Many instrumental problems impair past series and global models give only values of mean wind. Indications on pressure are not better. (BOURRELIER, PAUL-HENRI, AFPCN)	Each chapter subsection follows a structure that deals with observed trends then projected changes. As well, doing what the reviewer asks is probably not scientifically feasible, at the moment.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1065	3	31	9	34	14	I think it would really help if the section opened with a general description of the large scale changes in atmospheric circulation and the shits in climate zones, giving an indication of the types of changes that can be experienced in general weather types. There is some of this done on page 32, lines 33 ff, but what is needed is an overview up near the start of the section. (MacCracken, Michael, Climate Institute)	A sentence has been added to describe extreme wind metrics.
1066	3	31	11	31	25	It would be helpful to discuss the terms of mean and gust wind speed in the first paragraph (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Reject. The current sentence is self-obviously correct and does not require any change.
1067	3	31	12	31	13	We would rather use the word "might" than "can," because there are strong arguments that negative feedbacks though BL processes reduce the importance of wind speed. Higher windspeeds enhance fluxes, which then reduce gradients of the fluxed entity. (UNITED STATES OF AMERICA)	This paragraph provides context regarding circulation changes that have occurred on long time scales. We have included some extra key references but note that a more extensive treatment of paleo-climatic changes is beyond the scope of this chapter.
1068	3	31	27	31	37	The paragraph seems a bit out of context (relevance?), and also builds on only ONE reference (low confidence…) (SWEDEN)	We have revised this paragraph to include some extra key references but note that a more extensive treatment of paleo-climatic changes is beyond the scope of this chapter.
1069	3	31	27	31	37	The addition of this paragraph covering information from paleo-records is very good. However, it seems to be based on a single review paper by Wanner et al. Suggest to provide and assess more of the original references to strengthen the assessment based on multiple lines of independent evidence. (Stocker, Thomas, IPCC WGI TSU)	Extra references have been cited and comparative language used where appropriate to convey the level of certainty in the evidence.
1070	3	31	27	31	37	The paleo discussion is good, although I think it needs language with more caveat phrases on how certain we are about the climate inferences from the paleo evidence. (UNITED STATES OF AMERICA)	Since this is a relatively new development, we believe that before talking about extremes of wind in the past, we may need some background information on mean changes
1071	3	31	27	31	37	This discussion of circulation changes in past climates is interesting but the relevance to extreme wind events should be clarified. (UNITED STATES OF AMERICA)	We think that the rewording of this paragraph makes it clear that we are referring to changes from 6000 BP to the present.
1072	3	31	32	31	32	Suggest a clarification of the relative measure of ENSO activity. Is it higher than 1850, or higher than the present day? Or do the authors mean that ENSO activity increased over the period from 6000 yr BP to 1850? (CANADA)	This paragraph has been shortened and broken up into several smaller paragraphs.
1073	3	31	39	31	63	Having such long paragraphs is like a sleeping pill for the reader--the text needs to be livened up with results highlighted, etc. (MacCracken, Michael, Climate Institute)	Unfortunately inclusion of an additional map is not possible due to length constraints. However, the discussion has been rearranged to improve clarity.
1074	3	31	39	32	17	These results are very difficult to digest. A map pointing at the regions with cited decline and increase would be recommendable, even if it does not cover every single definition of mean/extreme or season or time period considered. The text should be ordered according to some criterion, for example regions where both long term means (no matter if computed from 1 min data) and extremes change in the wame direction. As noted before, the trends in the means and in the extremes needn't change in the same way, even if the same dataset and the same time period is considered (which may otherwise be another reason for seemingly contradicting results). (Ulbrich, Uwe, Freie Universitaet Berlin)	This paragraph has been rewritten to strengthen the synthesis of the information provided.
1075	3	31	39	32	17	In contrast to the paleo-evidence section before, which cites a single review paper, this section reads more like a review, reporting results from many individual studies without clear synthesis/assessment of all the information. Suggest to put some emphasis during revisions on strengthening this synthesis part of the assessment. (Stocker, Thomas, IPCC WGI TSU)	The references cited that used the reanalyses, either NCEP, ECMWF or the NARR describe in some way the quality of the reanalyses by comparing with observations for the regions where the study was focusing on
1076	3	31	39	32	17	This paragraph would benefit from more assessment of overall conclusions. Suggest the inclusion of an assessment of the quality of reanalysis extreme wind trends, or include statements whether these are likely to be more or less reliable than station measurements. Include any literature that examines the reliability of reanalysis trends in extreme wind speeds. (CANADA)	This paragraph has been rewritten to emphasise the consistency between the findings of these two studies.
1077	3	31	49	31	51	Here is an example where the spatial structure of results is suggestive of an underlying physical cause, even if individual results are not statistically significant. (Zwiers, Francis, Environment Canada)	Done.
1078	3	31	50	31	50	Delete "(NARR)" - Acronym is not used again in SREX. (Stocker, Thomas, IPCC WGI TSU)	Added.
1079	3	31	56	31	58	You may want to add that: Nissen et al. 2010 (Nissen, K.M., G. C. Leckebusch, J. G. Pinto, D. Renggli, S. Ulbrich, and U. Ulbrich, 2010: Cyclones causing wind storms in the Mediterranean: characteristics, trends and links to large-scale patterns. Nat. Hazards Earth Syst. Sci., 10, 1379–1391.) investigated exteme wind events in ERA40 data and found a significant decrease in the southern (adjacent North Africa) part of the western Mediterranean, while an increase was observed in the area around the Gulf of Genoa (their Fig. 12, bottom). They related these changes to the corresponding effect the NAO exerts on extreme wind events. (Ulbrich, Uwe, Freie Universitaet Berlin)	Reference to this comparison has been removed.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1080	3	31	58	31	60	Can we credibly compare grid box values with observations on this scale, even for the reanalyses? By reporting this finding, the authors are implicitly making the assessment that such comparisons are reasonable. (Zwiers, Francis, Environment Canada)	References to comparisons of opposite trends in anemometer and wind data have been removed. The following statement at the end of the paragraph is made instead 'Some studies report the opposite trends observed in some areas between anemometer winds and reanalysis data sets (e.g. McVicar, 2007; Smits et al, 2005; Vautard et al, 2010) however comparisons of surface anemometer data, at 10 m or lower with reanalysis-derived 10 m data that do not resolve complex surface features is problematical.'
1081	3	31	58	31	63	Overall, I think comparing winds from reanalyses with observations is a difficult proposition, particularly for surface (2 m or 10 m) winds, which are very strongly affected by local surface properties and small scale topographic features. Note that surface winds are not likely prognostic, either in climate models or in the NWP models used in reanalysis. (Zwiers, Francis, Environment Canada)	The references to NCEP and ERA reanalyses have been removed in this instance. So don't need citation.
1082	3	31	60	31	60	References should be given for the reanalyses. Not necessarily here if they are given somewhere earlier in the report, but this should be checked. (Global Climate Observing System Steering Committee)	Reference to Troccoli was removed due to not making publication deadline.
1083	3	32	1	32	17	The NCEP and other reanalyses are far from homogeneous and this applies especially to major discontinuities across 1978-79 as the transition to the satellite era began. The references here to 1975-2006 on lines 3 and 8 make no sense. (UNITED STATES OF AMERICA)	2-m refers to 2-metre (SI abbreviation for minute is min).
1084	3	32	3	0	0	2 m wind data refers to 2 meters or 2-min averaging time ? (International Petroleum Industry Environmental Conservation Association (IPIECA))	10-m refers to 10-metre (SI abbreviation for minute is min).
1085	3	32	6	0	0	10-m wind refers to 10 meters height or 10-min averaging time ? (International Petroleum Industry Environmental Conservation Association (IPIECA))	The reference to the study by Tokinaka and Xie has been removed.
1086	3	32	14	32	17	Shouldn't we be very sceptical of ship-based anemometer data? (Zwiers, Francis, Environment Canada)	corrected.
1087	3	32	17	0	0	"geostrophic" instead of "geostropic" (Simiu, Emil, National Institute of Standards and Technology)	The reference to the study by Tokinaka and Xie has been removed.
1088	3	32	17	0	0	A reference is missing for the wind trends of Australia and south east Asia (International Petroleum Industry Environmental Conservation Association (IPIECA))	We have rewritten and reordered this paragraph to reduce overlap.
1089	3	32	19	32	56	This section is much about cyclones, which are later discussed in own sections. Of course, high wind speeds and storms are related, but there is some unuseful overlap here. (SWEDEN)	This paragraph has been rewritten and the term storminess is no longer used to avoid the possibility of different definitions.
1090	3	32	20	32	20	Is there a technical definition for 'storminess' that could be included here in (...). Is storminess the same as wind intensity? (Stocker, Thomas, IPCC WGI TSU)	See response #1090
1091	3	32	21	32	21	How is "storminess" defined? (UNITED STATES OF AMERICA)	This paragraph has been rewritten and shortened and so these details are no longer reported.
1092	3	32	26	32	26	Replace "unprecedented since 1874" with ""unprecedented in winter since 1874". For summer time, the highest peak is around 1880 (see Fig. 4 in Wang et al. 2009c) (Wang, Xiaolan, Environmen Canada)	Added.
1093	3	32	26	32	27	Wang et al (2009c) point out that trends for the 20th century were different for the summer and winter season. In the abstract they state that "winter and summer storminess conditions have undergone very different longterm variability and trends". This statement is more precise than the one given, and it is also clear from their study that there were no large century long trends. For the reanalysis period, Leckebusch et al (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.) find an increasing trend of storm event severity over the North Atlantic, which is quantified with a storm severity index. (Ulbrich, Uwe, Freie Universitaet Berlin)	This paragraph has been rewritten for brevity. Since the topic is on how storm activity changes can be used to infer wind changes, we feel that it is appropriate to combine all phenomena in the one paragraph.
1094	3	32	28	32	31	The whole previous para deals with extratropical cyclones. The sentences on the other phenomena should be clearly separated. (Ulbrich, Uwe, Freie Universitaet Berlin)	This has been done.
1095	3	32	33	32	49	Please clarify which of the statements refer to mean winds and which refer to extreme winds! (Ulbrich, Uwe, Freie Universitaet Berlin)	This sentence has been changed and now cites chapter 9 instead of the SPM to avoid the ambiguity.
1096	3	32	34	32	36	This sentence is misleading. Observed (present day) climate variability is typically of a different time scale than a nonlinear climate change, assumed to be scaling with forcing. A first order approach is to add internal variability and nonlinear change for obtaining a guess of possible future developments. (Ulbrich, Uwe, Freie Universitaet Berlin)	Changed to 'Canadian west coast'.
1097	3	32	37	32	38	For what areas of the world is this statement true? (Ulbrich, Uwe, Freie Universitaet Berlin)	The results of this study have been cited.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1098	3	32	48	32	49	(essentially same comment as my comment 10): Nissen et al. 2010 (Nissen, K.M., G. C. Leckebusch, J. G. Pinto, D. Renggli, S. Ulbrich, and U. Ulbrich, 2010: Cyclones causing wind storms in the Mediterranean: characteristics, trends and links to large-scale patterns. Nat. Hazards Earth Syst. Sci., 10, 1379–1391.) investigated exteme wind events in ERA40 data and found a significant decrease in the southern (adjacent North Africa) part of the western Mediterranean, while an increase was observed in the area around the Gulf of Genoa (their Fig. 12, bottom). They related these changes to the corresponding effect the NAO exerts on extreme wind events. (Ulbrich, Uwe, Freie Universitaet Berlin)	This paragraph has been shortened.
1099	3	32	51	33	26	This paragraph should be shortened substantially since it includes only a few studies and, as stated here, the confidence in GCM extreme wind speed is low (A statement I fully agree). (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	This reference has been added.
1100	3	32	51	33	26	We would suggest adding a synopsis of the work of Zahn and von Storch (2010, Nature) which showed projections of a decrease in polar lows (and associated wind speeds). (UNITED STATES OF AMERICA)	This has been done.
1101	3	32	53	32	53	Please cite a specific IPCC chapter. (Stocker, Thomas, IPCC WGI TSU)	This change has been made.
1102	3	32	54	32	54	"Likely" should be italicized, since it is calibrated uncertainty language. (IPCC WGII TSU)	Wording has been changed.
1103	3	32	58	32	58	"agreement between models of a ..." -- "agreement on" (Stocker, Thomas, IPCC WGI TSU)	10-m refers to 10-metre (SI abbreviation for minute is min).
1104	3	32	60	0	0	10 m winds refers to 10 meters height or 10-min averaging time ? (International Petroleum Industry Environmental Conservation Association (IPIECA))	This paragraph has been shortened.
1105	3	33	1	33	17	This rather lengthy discussion of Figure 3.9 could be shortened to focus on the key patterns illustrated in the figure. (Stocker, Thomas, IPCC WGI TSU)	The regional information has been combined into a shorter 'regional paragraph'.
1106	3	33	1	33	63	The regional information contained in lines 1 to 26, should be combined with the regional information given from line 38 onwards, into a single, shortened section. (Stocker, Thomas, IPCC WGI TSU)	We have rewritten and reordered this paragraph so that only the very general changes are discussed.
1107	3	33	8	33	9	We don't see this point from examining Fig. 3.9 (UNITED STATES OF AMERICA)	Thanks, this paper is now cited.
1108	3	33	11	33	11	An increase of storm severity in A1B and A2 scenarios carried out with a specific model (ECHAM5/OM1) is also reported by 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.) (Ulbrich, Uwe, Freie Universitaet Berlin)	We agree that multi-model agreement does not lead to increased confidence if we are aware that the models are all failing to resolve particular phenomena. We have rewritten this paragraph to better reflect this point.
1109	3	33	18	33	26	This paragraph is written in a misleading way. Indeed, agreement of the sign of signals of the models provides some confidence in the signals, assuming that these signals are related to the same phenomena. For mid-latitudes, it is apparent that there are regional increases in winter wind storm risks under rising green house forcing, while at a hemispheric scale there is a decrease of the zonal mean meridional temperature gradient in the lower troposphere. Second, as the models models at their current resolution are unable to resolve small scale phenomena such as tropical cyclones, tornadoes and mesoscale convective complexes, a common signal in the regions most influenced by these phenomena cannot represent the same degree of confidence. Third, the sentence refering to Diffenbaugh does not make sense in this context. (Ulbrich, Uwe, Freie Universitaet Berlin)	10-m refers to 10-metre (SI abbreviation for minute is min).
1110	3	33	30	0	0	10 m mean wind refers to 10 meters height ? What is the averaging time 6hr, daily ? (International Petroleum Industry Environmental Conservation Association (IPIECA))	It has now been published online.
1111	3	33	30	0	0	Figure 3.9: PLEASE NOTE: McInnes et al. must be accepted for publication by May 31 2011 for these new, previously unpublished results to remain included and assessed within SREX. (Stocker, Thomas, IPCC WGI TSU)	This figure has been redrafted on a different projection for consistency with other figures in the chapter and so this point has been taken care of.
1112	3	33	30	0	0	Figure 3.9: suggest to remove the grid lines on all four panels -- this is the only figure in Chapter 3 that has grid lines (Stocker, Thomas, IPCC WGI TSU)	The GCM model findings are now cited in the paragraph that discusses global results (including Figure 3.9) and the regional studies are discussed in a separate shortened paragraph.
1113	3	33	38	34	17	I suggest to separate between the results derived from RCMs or GCMs in the whole paragraph (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Since there has not been a dedicated discussion on winds in previous IPCC reports we felt that inclusion of figures on mean and 99th percentile provided a good basis for subsequent discussion and regional information about the changes arising from circulation changes in GCMs. We realise that engineers may require information more extreme winds than are discussed but these are not generally available at this time due to GCMs being unable to resolve the phenomena that are responsible for such extremes. Therefore, no change.
1114	3	33	43	33	43	Is "daily mean wind speed" what is really needed--I would imagine what architects and builders might want is the peak one-minute winds or hourly winds, etc.--how is daily mean wind an extreme condition? Cannot frontal wind speeds or some other measure be provided? Same question applied to Figure 3.9. (MacCracken, Michael, Climate Institute)	Text on RCMs has been shortened and reworded so comment is no longer relevant.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1115	3	33	49	33	49	What is meant by the wording "daily maximum wind speeds ... Become more frequent"? The daily maximum wind speed has daily frequency by definition. (Global Climate Observing System Steering Committee)	Text on RCMs has been shortened and reworded so comment is no longer relevant.
1116	3	33	58	33	58	Change this into "10-year gust wind speed" (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	No changes to current day demographics were modelled.
1117	3	33	60	33	63	Did Pinto et al, provide any insight into what proportion of these increases were due to changes in population density vs. Increasing extreme winds? (Stocker, Thomas, IPCC WGI TSU)	Unable to obtain a copy of this paper. Premature to include here?
1118	3	34	5	34	5	Metrics measuring wave activity may also provide indicators about climate trends. The wave activity is a function of wind. There is a study by Ebeling & Stein (2011, Bullit. Seism. Soc. Am., vol 101 – perhaps some earlier ones too?) looking at the wave activity picked up as seismic activity. (NORWAY)	The concluding comment has been rewritten to explicitly mention low confidence on observed trends as well as on the causes of trends.
1119	3	34	6	34	11	The first part of this concluding bolded statement describes an observed declining trend in mean wind speed in the Northern Hemisphere, but without applying any confidence, or level of agreement/evidence to this trend. Table 3.1, states "Low confidence" in observed trends, but it is not clear how this links to the information here in the bolded conclusion, because here low confidence is only given in relation to the attribution of the causes of this trend. It is also questionable whether the second sentence, based on 'one study' should be elevated to this level of prominence. (Stocker, Thomas, IPCC WGI TSU)	We believe that the low confidence level to be the best choice. Some studies and their consistency may suggest a higher confidence, but the fact that few observations and more reanalyses based results are available may suggest to be careful about assigning the levels of confidence.
1120	3	34	6	34	14	Of course, rating the results as "low confidence" can always be seen as justified in general terms as we still need to raise our confidence and understanding. However, with respect to the studies over the Atlantic and Europe, my impression from the text (and figure 3.9) is a fairly consistent picture of quite a number of studies pointing at an understanding of regional present day variability from the variations in the large scale patterns, in particular the NAO, and at an increase in storm risk in this region under rising greenhouse gas concentrations. It is, however, not so clear if this increase is explained by a change in the patterns, in particular the NAO. Still, this doesn't detract much from the degree of confidence I feel should be assigned to the signal in this region. (Ulbrich, Uwe, Freie Universitaet Berlin)	The conclusions have been reworded. Not sure what reviewer means by "not helpful".
1121	3	34	6	34	14	The conclusions here are not helpful and it makes no sense to me to do this assessment of wind without discussing dynamics and changes in circulation. (Trenberth, Kevin, NCAR)	This sentence has been removed.
1122	3	34	8	34	9	It is too specific for a short summary to refer to one study only and to explicitly mention the increasing surface roughness. Furthermore,the RCM results discussed above are not summarized appropriately in this short paragraph. (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Yes and this sentence has been amended to reflect this.
1123	3	34	10	0	0	Does this imply that there is low confidence in the trends themselves too? (Klein Tank, Albert, KNMI)	Conclusions at end of Section 3.4.1
1124	3	34	19	0	0	No conclusion on monsoons et still less on their extremes. (BOURRELIER, PAUL-HENRI, AFPCN)	Some shortening of section. However monsoons cover a large part of the world. So section needed to be reasonably comprehensive.
1125	3	34	19	37	31	The assessment (p37l25-31) is that uncertainty is too large to make an assessment. There are 4 whole pages and several figures to reach this weak conclusion. Shorten by at least half. I note that the ENSO section (3.4.2) is about 2 pages to reach a slightly stronger conclusion.... (UNITED STATES OF AMERICA)	We consider that the text is clear and non exclusive to El Nino, since some models of variability of monsoon are not linked directly to El Nino. There are also discussions to aerosols in the text of the section. Much of the text is about problems with observing and modelling the monsoons, and the lack of consistency between models.
1126	3	34	19	37	31	The context for this discussion is poor. The biggest changes in monsoons are with ENSO but ENSO is done separately. Also there is no discussion of the critical regional role of aerosols, and the expectations are not given to frame the discussion. The findings are often dependent on the time frame which is not well spelled out (e.g. l 41) or which do not stand up (l 48). The whole of this section should be shortened a lot (esp pp 35-36) and the material from p 36 line 53 to the end retained. The models do monsoons poorly, they fail to simulate MJOs, tropical storms, there is a double ITCZ, etc: all the transients are poorly done. The simulation results are a function of scenario especially wrt aerosols (not discussed). The discussion is also redundant with the precipitation section. (UNITED STATES OF AMERICA)	Done.
1127	3	34	21	34	23	Suggest limiting the sentence to the first fragment ending at "...not well understood," as the remainder of the sentence is unclear and inconclusive. (CANADA)	Seasonal rainfall can also be extreme. Section 3.3.2 is clear that it only looks at short-term extremes, and it is appropriate to examine, where possible, extremes in seasonal rainfall. As well, in a sense monsoons are extreme themselves - and changes can lead to changes in extreme rainfalls
1128	3	34	21	34	30	The monsoon section addresses total and seasonal rainfall, with extremes in 3.3.2. So why is it in this report? And are statistical correlations between monsoon circulation and precipitation extremes believed to be stationary and therefore useful for projecting extremes? (UNITED STATES OF AMERICA)	Phrase has been deleted.
1129	3	34	22	34	23	This seems rather nebulous. Wouldn't the statement that "a variey of extremes ... may occur more or less frequently ... as a consequence of climate change" be true everywhere, and not just in monsoon regions? (Zwiers, Francis, Environment Canada)	Sentence reworded.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1130	3	34	23	34	24	The author team should consider if it makes sense to describe inhabitants having a "most important variable." (IPCC WGII TSU)	Done.
1131	3	34	32	34	34	This sentence should be clarified as to the sense of time (i.e., ITCZ moved progressively southward between x and y years BP) (UNITED STATES OF AMERICA)	Section already being attacked for being too long. Cannot add more references.
1132	3	34	32	34	36	The addition of this paragraph covering information from paleo-records is very good. However, it seems to be based on a single review paper by Wanner et al. Suggest to provide and assess more of the original references to strengthen the assessment based on multiple lines of independent evidence. (Stocker, Thomas, IPCC WGI TSU)	See #1132
1133	3	34	32	34	36	There are many paleoclimate references that could be added here: i.e., Weldeab et al., GRL, 2007; Liu et al, 2007, Quat Science; Timm et al., 2010, JCLI; Niedermeyer et al., 2010, QSR; Cook et al., 2010, Science; Wang et al., 2010, Earth Sci Rev; and many others). Some of these records show abrupt events that could relevant to the documentation of extremes. (UNITED STATES OF AMERICA)	It would be present day, the kind of past minus present climate (control runs in some models)
1134	3	34	34	34	47	Provide clarification on the relative measure of monsoon rainfall. This sentence describes a trend in monsoon rainfall in most regions over the Holocene, and an absolute change in South America ('in South America the monsoon was weaker'). Weaker than when? Preindustrial, present-day? (CANADA)	We will do as the reviewer suggests
1135	3	35	3	35	5	Suggest deleting this sentence, as the sentence "Over the longer term, however, Zhou et al. (2008b; 2008a) and …" is a duplication described above on page 34, para-5, line 39-41. (CANADA)	Statement revised.
1136	3	35	4	35	4	What is meant by "weakening trend"? A weakening positive or negative trend? This statement should be conform with the "decreasing long-term trend in north African summer monsoon rainfall", L36 on this page (Kunz, Michael, Karlsruhe Institute of Technology (KIT))	Do not accept that bullet points would improve clarity.
1137	3	35	7	35	18	This paragraph covers a lot of regions--it seems to me that when such content is available, bullets should be used to separately give the results for each region, making the results easier to spot. (MacCracken, Michael, Climate Institute)	We will do as the reviewer suggests
1138	3	35	20	35	20	Suggest inserting "in part," ahead of "because". I would think that this would not be the only reason for difficulty. (Zwiers, Francis, Environment Canada)	Seems rather speculative. But see #1138.
1139	3	35	23	35	26	I think it would be useful to mention that part of the reason for the problems is likely limited resolution and that models with higher resolution are rapidly becoming available so the problmes here may well be resolvable. (MacCracken, Michael, Climate Institute)	We will do as the reviewer suggests
1140	3	35	25	35	25	Sea Surface Temperature should be spelt out here in full where it is used for the first time, and not on line 29. (Stocker, Thomas, IPCC WGI TSU)	We will do as the reviewer suggests
1141	3	35	30	35	30	Regional behaviour should be specified. Suggest the sentence fragment: "The trend is strongly linked to …" could be changed to "The trend in east Asian monsoon is strongly linked to …". (CANADA)	We would say that NAM and SAM may affect monsoon, but monsoons may also affect the functioning of NAM and SAM
1142	3	35	47	35	48	Please clarify: Do the NAM and SAM really influence monsoons? Or, do the monsoons influence the NAM and SAM? (CANADA)	Regional monsoons refer to the geographical distribution of monsoon areas around the world. In lines 55 and 56 we refer to monsoon as a regional circulation, that includes all features (rainfall, winds)
1143	3	35	49	35	56	Some terms here do not seem clear: what is "regional monsoon", etc. Does "monsoon" on lines 55 and 56 refer to strength or circulation or development or what? Some clarification would help. (MacCracken, Michael, Climate Institute)	Due to the space limitation we refer to studies by Meehl, Lau and Silva Diason the role of aerosols on monsoons
1144	3	35	56	35	57	The link between aerosols and changes in monsoons should be explained. (CANADA)	We will do as the reviewer suggests
1145	3	35	63	35	63	Suggest adding the following result between the 1st and the 2nd sentences: "Held and Soden (2006) demonstrate that an increase of the hydrological cycle is accompanied by a global weakening of the large-scale circulation." (The reference has already been listed in the Chapter.) (CANADA)	We will do as the reviewer suggests
1146	3	36	2	36	2	Please cite specific IPCC chapter. (Stocker, Thomas, IPCC WGI TSU)	We will do as the reviewer suggests.
1147	3	36	3	36	3	Suggest replacing "may become" with "will become". Isn't there wide agreement on this? See for example, Fig 10.9 in WG1 AR4. (Zwiers, Francis, Environment Canada)	We will do as the reviewer suggests
1148	3	36	3	36	3	Suggest replacing "hypothesis" with "projection"(it's more than a hypothesis). (Zwiers, Francis, Environment Canada)	We will review the text. Changes in circulation are related to changes in rainfall, and this may have to do with SST anomalies. The "complicating..." statement will be removed.
1149	3	36	10	36	11	Please clarify why SSTs are "complicating the picture further." GCMs would be expected to resolve such interactions with SSTs, and this is also to be expected based on what is written earlier in the paragraph about monsoons being driven by the land-sea temperature contrast. (CANADA)	We do not understand the comment.
1150	3	36	14	36	15	Good analysis of model projections (global warming scenarios) for the Sahel system (drying, intensification of rain and frequent extreme events) (Mata, Luis Jose , IMF)	Space limits preclude this. Monsoons section is already attacked for being too lengthy.
1151	3	36	14	36	16	This statement is so contradictory that it requires further elucidation. Reference for example Held et al., PNAS, 2006. (UNITED STATES OF AMERICA)	We will do as the reviewer suggests
1152	3	36	22	36	22	Suggest deleting "signficant" - your expert judgement that this is an important change is perhaps sufficient. Make clear what period and what forcing scenario is being discussed. (Zwiers, Francis, Environment Canada)	We have deleted those wrods

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1153	3	36	23	36	23	It would be helpful to clarify what is meant by "excess and deficient." (IPCC WGII TSU)	No, in fact it is not as recent as we thought, since it is from 2009. The "recent" word is because of the data of publication. We will correct that.
1154	3	36	24	36	24	Does "A more recent study" imply a quality assessment - i.e., that, even though only a single model is used, this result might be more reliable than that of Kripalani et al? I think this is how this may be interpreted by readers. (Zwiers, Francis, Environment Canada)	Due to space limitations, we believe that the references to Lau et al 2007 and Held and Soden 2006 would help the reader to understand the physical reasons for such changes. Nevetheless, we will see a way to reorganize the text to add this new information withou extending too much
1155	3	36	25	36	27	[and also lines 43-47] Might it be possible to explain the reasons for the types of changes. One would normally expect that land warming would lead to a stronger monsoon, but greater vertical stabilization caused by the radiative effects of the higher CO2 concentration is a countervailing influence--and perhaps also the higher albedo caused by the higher temperatures/intensified rate of evaporation led to an increased tendency to descending air to make up for the heat deficit by adiabatic heating as happens over desert areas. At least give some sense that there possible explanations (which is why we need models). (MacCracken, Michael, Climate Institute)	We will do as the reviewer suggests
1156	3	36	27	36	27	Please lest these 'key areas' of South Asia. (Stocker, Thomas, IPCC WGI TSU)	We will use south Asia
1157	3	36	27	36	27	The author team should clarify what is meant by "key areas of south Asian." (IPCC WGII TSU)	We will do as the reviewer suggests
1158	3	36	31	36	31	As Taiwan is a Province of China, it must be cited as "Taiwan province of China". (CHINA)	We will do as the reviewer suggests
1159	3	36	32	36	32	Suggest adding a cross reference to WG1 AR4 Ch 10, where the "El-Nino-like" mean state change is assessed. (Zwiers, Francis, Environment Canada)	We will do as the reviewer suggests
1160	3	36	53	37	23	This is a very long paragraph; it would be generally helpful to readers if paragraphs could be kept to bite-sized chunks. (Zwiers, Francis, Environment Canada)	we will do as the reviewer suggests
1161	3	36	53	37	23	This is a very long paragraph. I'd suggest breaking it up a bit--and also giving some sense about the potential for improvement in the estimates over time--indicate what will it take. (MacCracken, Michael, Climate Institute)	We will do as the reviewer suggests
1162	3	36	55	36	55	I would suggest changing "the model reporesentation" to "the limits in the model reporesentation" (MacCracken, Michael, Climate Institute)	We will include the correct reference
1163	3	36	57	36	58	"Kharin and Zwiers (2007)" should be "Kharin et al (2007)" (Zwiers, Francis, Environment Canada)	The lack of consistency is taken into account in the assessment of confidence/uncertainty.
1164	3	36	58	36	60	The Kharin and Zwiers finding is another reason to question the use of "likely" for projected increases in extreme precipitation in the tropics in particular. (UNITED STATES OF AMERICA)	I would say that it is not just the model resolution, but also the model representation or processes at such high resolution. This also includes the representation of topography and land use
1165	3	36	59	36	60	Is it limits in model resolution or in model processes? With limited resolution, the processes have to be adjusted for the larger resolution, and so problems can be blamed on processes when really what needs to be done is have better spatial resolution. I raise this point as it seems to me that one implies a limit in understanding about atmospheric behavior and the other explanation suggests the major problem is in limited computer resources (and these limits are quite rapidly seeming to be overcome). (MacCracken, Michael, Climate Institute)	Paragraph has been separated into several shorter rewritten paragraphs, so comment is not relevant.
1166	3	37	15	0	0	Insert a paragraph break after (Christensen et al, 2007). The sentences that follow (lines 15 to 23) are expert judgement at best (Webb, Robert, NOAA)	See comment 1144
1167	3	37	19	37	19	Explain why aerosols from biomass burning are of particular significance to the issue of changes in monsoons. (CANADA)	Do not see how this would enhance the conclusions stated here.
1168	3	37	25	37	31	This perspective could be enhanced by bringing in paleoclimate data (UNITED STATES OF AMERICA)	We will do as the reviewer suggests
1169	3	37	27	37	27	Delete "since" (Zwiers, Francis, Environment Canada)	We will revise the text.
1170	3	37	30	37	31	This final statement reads very vague relative other concluding statement in chapter 3. Can the word "may" be removed, and a statement on the level of evidence/agreement or confidence be used instead? (Stocker, Thomas, IPCC WGI TSU)	We will revise the text and include suggestions of the reviewer
1171	3	37	31	37	31	Add "in monsoon regions" after 'increase in extreme precipitation' if that is what is meant. Otherwise, this sentence seems to contradict the preceding one that says there is low confidence in projects of changes in monsoons. (CANADA)	Do not understand reviewer's point here.
1172	3	37	33	0	0	Apart a trend of a move of center of El Nino toward the center of Pacific, other results are inconsistent. We have to wait for tropical cyclones as for extra-tropical tempests. (BOURRELIER, PAUL-HENRI, AFPCN)	Reject. Difficult to understand the point of the reviewer's various comments. Reviewer does not indicate which conclusions on pg 39 are wrong; nor does he provide references demonstrating how they are wrong. The conclusions are consistent with other recent peer-reviewed examinations of changes in ENSO.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1173	3	37	33	39	41	The AR4 has a nice introduction to ENSO with patterns of SLP, temperature and precipitation that should be referred to. Trenberth, K. E., and D. P. Stepaniak, 2001: Indices of El Niño evolution. J. Climate., 14, 1697-1701. discusses robust changes in ENSO evolution that are not addressed here. Prior to 1976 the EN events developed first in the east and subsequently have developed first in the central Pacific. This relates to the so-called new type of ENSO event, that is not new. Most models (all) do not simulate ENSO adequately so can can one do any useful attribution? (p 38 l 43-44). So p 39 lines 14 to 22 makes much of the rest of the discussion irrelevant. The El Nino like changes or otherwise depends on the definition of ENSO and it is not correct to define it just as the interannual variability. Regardless of whether the SST or SO indices have changed, there are strong signals of change in ENSO in precipitation: both heavy rains and drought that are not discussed. The conclusions p 39 are wrong wrt effects of ENSO. (Trenberth, Kevin, NCAR)	Agreed. replace "extremes" with "phases" where appropriate.
1174	3	37	42	37	42	Suggest to avoid using "extremes" when referring to the El Nino and La Nina episodes here. Potential for confusion with extremes discussion. (Stocker, Thomas, IPCC WGI TSU)	Reject. Rather old reference; should have been cited already in TAR and AR4.
1175	3	37	48	0	0	Chap 3, page 37, line 48: Should include here a reference to Gershunov, A., and T. P. Barnett, 1998a: ENSO influence on interseasonal extreme rainfall and temperature frequencies in the contiguous United States: Observations and model results. Journal of Climate, 11, (7)1575-1586. (UNITED STATES OF AMERICA)	Reject. Already cite six more recent papers on this topic - this section cannot cite all papers ever written on this topic.
1176	3	37	48	0	0	A relevant paper omitted here that documents the patterns of temperature and precipitation extremes over the U.S. associated with ENSO is: Meehl, G.A., C. Tebaldi, H. Teng, and T. Peterson, 2007: Current and future U.S. weather extremes and El Niño. Geophys. Res. Lett., 34, L20704, doi:10.1029/2007GL031027. (UNITED STATES OF AMERICA)	Fossil coral evidence already mentioned in this paragraph. Agree to replace "through quite anomalous climate periods" with "in very different background climate states."
1177	3	38	2	38	10	I think it would be useful to add phrases about how the information is derived (e.g., from coral corings, etc.). I think it is helpful to be presenting this information. On line 9, I would think that "anomalous" should be changed to "varied" or somethig similar (or some other way to indicate climatic conditions different than those we have now)--saying they are anomalous just does not seem right. (MacCracken, Michael, Climate Institute)	Replace "LGM" in line 8 with "last glacial interval".
1178	3	38	2	38	10	This section could be improved and contains some inaccuracies. For example, the statement about the mid-Holocene is incomplete (cf., Chiang, 2009, JCLI) and the statement about the LGM is correct (no true LGM (18-23 kyr BP) corals were recovered in the Tudhope study (glacial interval is more correct). (UNITED STATES OF AMERICA)	See response to comment 1177 - will replace "anomalous".
1179	3	38	8	38	10	This last statement about paleoevidence and ENSO seems to imply that LGM (or more general cold phases) represent "anomalous climate periods". However, on paleo-timescales warm, not cold periods are the exception. (Stocker, Thomas, IPCC WGI TSU)	Reference included.
1180	3	38	31	38	31	Along with a tendency of recent El Nino episodes to be centred more in the central equatorial Pacific, Lee and McPhaden (2010) have shown that central Pacific El Nino's have been increasing in intensity since 1982. Full reference: Lee, T. and McPhaden, M. 2010: Increasing intensity of El Nino in the central-equatorial Pacific, Geophysical Research Letters, 37, L14603, DOI:10.1029/2010GL044007 (AUSTRALIA)	Page cited by reviwer is incorrect - he presumably means page 37. Do not understand reviewer's point. The term "Southern Oscillation" is so well-known we surely do not have to cite Walker (1924) every time it is introduced.
1181	3	38	36	38	36	I think the term "Southern Oscillation" is correctly attributed to Walker (1924; see http://www.walker-institute.ac.uk/about/sir_gilbert.htm) (Zwiers, Francis, Environment Canada)	Do not understand review comment. Old reference.
1182	3	38	46	38	49	a useful perspective is provided by Cane, 2005, EPSL (UNITED STATES OF AMERICA)	The sentence is included to point out how variable (and therefore how uncertain) the model response to changes in CO2 is - not to predict ENSO changes.
1183	3	38	55	38	58	Should this be included? I'm not sure that studying what a model does at 16x CO2 would tell us anything credible about what might happen over the next century or so. (Zwiers, Francis, Environment Canada)	Section already cites a lot of evidence that the models do not do a consistent or good job in simulating ENSO. This is the basis for the conclusion that there is low confidence in these projections. Do not need more references to make the same point.
1184	3	39	2	39	3	How well is the location of SST variability during ENSO simulated in model control runs. There is also a question about internal modulation of various features of ENSO: How large a sample does one need to take from a particular model before the sign of change in this phenomenon in that model can be determined with confidence? Wittenberg (GRL, 2009 doi:10.1029/2009GL038710) shows some good examples of long-term internal modulation of ENSO in climate models which should serve as a cautionary note on interpreting simulated changes in ENSO in various model simulations. You could even use Fig. 1 from that paper in the report. (UNITED STATES OF AMERICA)	The words "tend to project" do not mean "good agreement amongst models". What the paragraph states is that models tend to project more central Pacific episodes, but there is considerable differences between models even in this projection.
1185	3	39	2	39	12	Suggest being more clear earlier in the paragraph about the extent of model consistency in this projection. This paragraph seems inconsistent at times. It begins by saying models tend to project (indicating good agreement among models in this respect) a change in El Ninos centred on the central equatorial Pacific and later say a "majority of models' project this - this is consistent. But then later it is stated that there is considerable uncertainty about this result with only 4/11 models producing statistically significant changes. (CANADA)	Reject. These papers should have been cited in AR4.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1186	3	39	14	0	0	Chap 3, page 39, lines 14-22: In this summary of ENSO behavior in models, really should reference AchutaRao and Sperber (2002, 2006). Full refs are: AchudaRao, K., and K. R. Sperber, 2002: Simulation of the El NIno Southern Oscillation: Results from the Coupled Model Intercomparison Project. Climate Dynamics, 19, 191-209. AchutaRao, K., and Sperber, K. R., 2006: ENSO simulation in coupled ocean-atmosphere models: are the current models better? Climate Dynamics, 27, 1-15. (UNITED STATES OF AMERICA)	Agreed.Delete "wavetrains observed in the".
1187	3	39	21	39	21	need to explain "typical wavetrains"? (Stocker, Thomas, IPCC WGI TSU)	Reject. These papers were presumably cited in AR4.
1188	3	39	24	0	0	Perhaps a more consistent change to El Nino-related phenomena (other than magnitude) is in the teleconnection patterns over the Northern Hemisphere related to changes in the base state midlatitude circulation in a warming climate: Meehl, G.A., and H. Teng, 2007: Multi-model changes in El Niño teleconnections over North America in a warmer climate. Cli. Dyn., 29, 779—790, DOI 10.1007/s00382-007-0268-3. This relates directly to changes in the patterns of temperature and precipitation extremes over the U.S. in future El Nino events: Meehl, G.A., C. Tebaldi, H. Teng, and T. Peterson, 2007: Current and future U.S. weather extremes and El Niño. Geophys. Res. Lett., 34, L20704, doi:10.1029/2007GL031027. Those changes include an eastward and northward shift of the teleconnection pattern of frost days, intense precipitation, and heat waves over the U.S. during a typical El Nino event in the future. (UNITED STATES OF AMERICA)	Agreed. Changed text as suggested by reviewer.
1189	3	39	24	39	25	"Position" provides an awkward description of the assessment at the time of the AR4 (this sounds like one side of a political argument). It would be better to say something like, "There was no consistency in projections of changes in ENSO variability or frequency at the time of the AR4 (Meehl et al., 2007), and this situation remains unchanged." (Zwiers, Francis, Environment Canada)	Tudhope et al already cited in appropriate place in section.
1190	3	39	24	39	25	The Tudhope et al study (2001, Science) provides useful context for potential changes in ENSO as a consequence of changes in the background state of climate. (UNITED STATES OF AMERICA)	Reviewer makes same point as is in current draft. So the point of his comment is unclear.
1191	3	39	27	39	28	This seems to be a strong statement in the face of mixed evidence. Ultimately there will be some change in ENSO, but it appears there is no basis for saying anything about the potential size or direction of the change, so I'm not sure that it is useful to make this statement. (Zwiers, Francis, Environment Canada)	Remove direct quotes.
1192	3	39	28	39	35	Use of quotations fits less well in an assessment context. (SWEDEN)	Summary of observed change now added.
1193	3	39	37	39	41	This summary covers the projections only, whereas elsewhere observations have been included in the summary. (Klein Tank, Albert, KNMI)	Reject. Seven of the 11 models predicted this change. The reviewer has confused direction of change with statistical significance of the projected change.
1194	3	39	39	39	41	This summary statement does not seem to be supported by line 2-12 on this page. 4/11 models is not 'most GCMS.' (CANADA)	Reject. This is about different patterns of climate variations, not impacts.
1195	3	39	41	39	41	This sounds like it is trying to say something about climate impacts, so should you replace "climate variations" with "climatic impacts"? (Zwiers, Francis, Environment Canada)	Reject - insufficient space to do this.
1196	3	39	45	40	2	I would suggest having separate paragraphs for the various modes of variability rather than jamming them into one paragraph. Giving a sense of the regions affected in some prominent way would be helpful as well. (MacCracken, Michael, Climate Institute)	Reject - insufficint space to include all modes of variability.
1197	3	39	45	40	43	Should also AO be mentioned? (SWEDEN)	Reject. Insufficient space to discuss all modes of variability.
1198	3	39	46	0	0	"There are also other modes not discussed here" p 40 l 30-38: these studies are not evaluated and critiqued: they need to be, where is the assessment? The summary fails to account for the need to initialize models to do this task well. (UNITED STATES OF AMERICA)	Agreed.
1199	3	39	46	39	46	IOC should be outside of the citation. (Stocker, Thomas, IPCC WGI TSU)	Agreed. Replaced with wording suggested by reviewer.
1200	3	39	53	39	54	Rephrase to "The SAM is the largest mode of SH extratropical variability and refers to north-south shifts in atmospheric mass between middle and high latitudes and plays an important role in climate variability in these latitudes." I don't think it has been shown that the SAM is the most important pattern at the middle and high latitudes individually. (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Reject. The point is not sufficiently important (or controversial) to require a lot of extra papers to be included.
1201	3	39	58	39	59	Hendon et al 2007 only discusses SAM impacts on Australia. Need to add references on other countries mentioned here. (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Reject. See response to comment 1202.
1202	3	39	58	39	59	Suggest the inclusion of Kell et al. (2006), as it is relevant here too. (CANADA)	Reject. We do not have space for an extended discussion of what causes the IOD.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1203	3	39	59	39	62	There is an alternative line of evidence that suggests the IOD is not a physical atmosphere/ocean phenomenon and is the result of stochastic SST variability or perhaps stochastic variations combined with an influence from ENSO. Furthermore, it seems the dipole mechanism does not act to influence Australian rainfall, instead it occurs primarily through influences in the eastern Pacific Basin. Specifically, Dommenget and Latif (2001), Dommenget (2007) and Dommenget and Jansen (2009) convincingly show that the IOD is the result of EOF analysis being applied to red noise. Furthermore, Allan et al. (2001) show its links to ENSO. Finally, Ummenhofer et al. (2009) showed that the western pole of the IOD does not act to regulate Australian precipitation and the influences were mostly from the east of the Basin. There should be a recognition here that new evidence questions the existence of the IOD as a physical climate mode. Full references for the above evidence are as follows: Allan, R., D. Chambers, W. Drosdowsky, H. H. Hendon, M. Latif, N. Nicholls, I. N. Smith, R. Stone, and Y. Tourre (2001), Is there an Indian Ocean dipole, and is it independent of the El Nino - Southern Oscillation?, CLIVAR Exchanges, 6, 18-22. Dommenget, D. (2007), Evaluating EOF modes against a stochastic null hypothesis, Clim. Dynam., 28, 517-531. Dommenget, D., and M. Latif (2001), A cautionary note on the interpretation of EOFs, J. Clim., 15, 216-225. Dommenget, D., and M. Jansen (2009), Predictions of Indian Ocean SST indices with a simple statistical model: a null hypothesis, J. Clim., 22, 4930-4938. Ummenhofer, C. C., A. Sen Gupta, A. S. Taschetto, and M. H. England (2009), Modulation of Australian precipitation by meridional gradients in east Indian Ocean sea surface temperatures, J. Clim., 22, 5597-5610. (AUSTRALIA)	Do not understand comment. There is no quotation on page 40.
1204	3	40	0	0	0	Section 3.4.4 comment: Use of citations does not fit very well in an assessment. Much of the content of the made quotation is also already given in the running text. (SWEDEN)	Reject. Do not have the space for such details.
1205	3	40	1	0	0	Chap 3, page 40, line 1-2: Suggest modifying this sentence to include the findings of Gershunov and Barnett 1998b on how the PDO modulates ENSO's teleconnections over North America. For example: "Variations in the PDO modulate ENSO's teleconnection over North America (Gershunov and Barnett, 1998b) and have been related to precipitation extremes... etc". Full reference is: Gershunov, A., and T. P. Barnett, 1998b: Interdecadal modulation of ENSO teleconnections. Bulletin of the American Meteorological Society, 79, (12)1715-2725. (UNITED STATES OF AMERICA)	Agreed. Include Trouet et al reference.
1206	3	40	3	0	0	It would make sense to mention that long-term reconstructions of the NAO index exist, and suggest that the NAO index showed distinct values during warm and cold periods: "Multidecadal scale reconstructions of the NAO index show positive (negative) values during warm (cold) episodes over Europe, such as the warm Medieval Climate Anomaly (MCA) and the cool Little Ice Age (LIA; Trouet et al., 2009). In addition the MCA-LIA difference pattern shows a "La Niña-like" state (Mann et al., 2009). References: Trouet, V. et al., 2009: Persistent positive North Atlantic Oscillation mode dominated the Medieval Climate Anomaly. Science, 324, 78-89. / Mann, M.E., et al., 2009: Global signatures and dynamical origins of the Little Ice Age and Medieval Climate Anomaly. Science, 326, 1256-1260. (Wanner, Heinz, University of Bern)	Agreed.
1207	3	40	4	0	0	These studies should be discussed, evaluated, critiqued and ultimately, assessed. "the NAO has been strongly negative the past 2 years" (UNITED STATES OF AMERICA)	Noted.
1208	3	40	4	40	6	I partly agree with the growing trend of the NAO. A while ago, I have checked for myself that there is no trend in the summer NAO, while there is an increasing trend in the winter NAO. (BELGIUM)	No known literature indicating this.
1209	3	40	4	40	6	The barometric pressure gives a measure of the atmospheric mass above the observation point. A trend in the NAO implies a systematic shift in the mean sea level pressure readings at Iceland and the Azores, which should be manifested as a permanent redistribution of mass. Are there any physical reason to think that this will happen? (NORWAY)	Reject. This is a very short section - and there is not space for a comprehensive chapter-length discussion on the NAO and SAM. Nor do we need to add all literature on this topic to the already much too long list of references.
1210	3	40	4	40	16	The discussion of the causes of trends in the annular modes could be made more concise, and include more references. At present the chapter cites only one study from 2010 as evidence for an effect of GHGs on the NAO, and no studies on the influence of ozone and GHGs on the SAM. Studies finding a link between GHG increases and an increase in the NAO (or closely-related NAM): Fyfe, J.C., G.J. Boer, and G.M. Flato, 1999: The Arctic and Antarctic Oscillations and their projected changes under global warming. Geophys. Res. Lett., 26, 1601–1604. Paeth, H., A. Hense, R. Glowienka-Hense, and R. Voss, 1999: The North Atlantic Oscillation as an indicator for greenhouse-gas induced regional climate change. Clim. Dyn., 15, 953–960. Shindell, D.T., R.L. Miller, G.A. Schmidt, and L. Pandolfo, 1999: Simulation of recent northern winter climate trends by greenhouse-gas forcing. Nature, 399, 452–455. Gillett, N.P., H.F. Graf, and T.J. Osborn, 2003a: Climate change and the North Atlantic Oscillation. In: The North Atlantic Oscillation: Climate Significance and Environmental Impact [Hurrell, Y.K.J., G. Ottersen, and M. Visbeck (eds.)]. Geophysical Monograph Vol. 134, American Geophysical Union, Washington, DC, pp. 193-209. Gillett, N.P., et al., 2002b: How linear is the Arctic Oscillation response to greenhouse gases? J. Geophys. Res., 107, doi: 10.1029/2001JD000589. Osborn, T.J., 2004: Simulating the winter North Atlantic Oscillation: the roles of internal variability and greenhouse gas forcing. Clim. Dyn., 22, 605–623. Rauthe, M., A. Hense, and H. Paeth, 2004: A model intercomparison study of climate change-signals in extratropical circulation. Int. J. Climatol., 24, 643–662. Studies on the roles of ozone and GHGs in forcing the SAM: Sexton, D.M.H., D.P. Rowell, C.K. Folland, and D.J. Karoly, 2001: Detection of anthropogenic climate change using an atmospheric GCM. Clim. Dyn., 17, 669–685. Gillett, N.P., and D.W.J. Thompson, 2003: Simulation of recent Southern Hemisphere climate change. Science, 302, 273–275. Marshall, G.J., et al., 2004: Causes of exceptional atmospheric circulation changes in the Southern Hemisphere. Geophys. Res. Lett., 31, L14205, doi:10.1029/2004GL019952.	Agreed.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1210.2	3	40	4	40	16	Shindell, D.T., and G.A. Schmidt, 2004: Southern Hemisphere climate response to ozone changes and greenhouse gas increases. Geophys. Res. Lett., 31, L18209, doi:10.1029/2004GL020724. Miller, R.L., G.A. Schmidt, and D.T. Shindell, 2006: Forced variations in the annular modes in the 20th century IPCC AR4 simulations. J. Geophys. Res., 111, D18101, doi:10.1029/2005JD006323. Arblaster, J.M., and G.A. Meehl, 2006: Contributions of external forcing to Southern Annular Mode trends. J. Clim., 19, 2896–2905. Karpechko, A.Y., Gillett, N.P., Marshall, G.J. and Scaife, A.A., 2008. Stratospheric influence on circulation changes in the Southern Hemisphere troposphere in coupled climate models. Geophysical Research Letters, 35(20). Son, S.W., Gerber, E.P., Perlwitz, J., Polvani, L.M., Gillett, N.P., Seo, K.H., Eyring, V., Shepherd, T.G., Waugh, D., Akiyoshi, H., Austin, J., Baumgaertner, A., Bekki, S., Braesicke, P., Bruhl, C., Butchart, N., Chipperfield, M.P., Cugnet, D., Dameris, M., Dhomse, S., Frith, S., Garny, H., Garcia, R., Hardiman, S.C., Jockel, P., Lamarque, J.F., Mancini, E., Marchand, M., Michou, M., Nakamura, T., Morgenstern, O., Pitari, G., Plummer, D.A., Pyle, J., Rozanov, E., Scinocca, J.F., Shibata, K., Smale, D., Teyssedre, H., Tian, W. and Yamashita, Y., 2010. Impact of stratospheric ozone on Southern Hemisphere circulation change: A multimodel assessment. Journal of Geophysical Research-Atmospheres, 115. Son, S.W., Polvani, L.M., Waugh, D.W., Akiyoshi, H., Garcia, R., Kinnison, D., Pawson, S., Rozanov, E., Shepherd, T.G. and Shibata, K., 2008. The impact of stratospheric ozone recovery on the Southern Hemisphere westerly jet. Science, 320(5882): 1486-1489. Son, S.W., Tandon, N.F., Polvani, L.M. and Waugh, D.W., 2009. Ozone hole and Southern Hemisphere climate change. Geophysical Research Letters, 36. (CANADA)	
1211	3	40	5	40	5	This is summarized from AR4, but should be updated given the two recent winters with negative NAO (Brönnimann, Stefan, University of Bern)	Agreed. Move Trenberth reference so that it is not being cited as evidence of recent changes in NAO.
1212	3	40	5	40	6	Trenberth et al (2007) is an inappropriate reference for "in the last five years" considering the reference itself is almost 5 years old (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Agreed. Thanks.
1213	3	40	12	0	0	Presumably you mean "stratospheric ozone" here. (Boucher, Olivier, Met Office)	Agreed. Add wording and reference.
1214	3	40	16	0	0	Chap 3, page 40, line 16-17: Should acknowledge the work of Meehl, Hu, and Santer JCLI 2009 here. For example, at the end of this paragraph, add a sentence such as: "However, some evidence suggests that the PDO may be affected by anthropogenic forcing (Meehl et al., 2009)." The full reference is: Meehl, G. A., A. Hu, and B. D. Santer, 2009: The mid-1970s climate shift in the Pacific and the relative roles of forced versus inherent decadal variability. Journal of Climate, 22, 780-792. (UNITED STATES OF AMERICA)	Agreed. Added Arblaster et al. and extra wording.
1215	3	40	29	40	30	Clarify here that the impact of stratospheric ozone on the SAM is mostly in DJF while increasing GHGs dominate SAM projections in other seasons (eg. Miller et al 2006; Polvani et al. GRL 2011). Recent studies suggest a near-cancellation of the competing effects of ozone and GHGs on the SAM in DJF (Polvani et al. 2011; Son et al. JGR, 2010), while Arblaster et al., GRL (2010) show that future SAM trends are correlated with climate sensitivity in all seasons. Ref: Arblaster, Meehl, Karoly, 2011: Future climate change in the Southern Hemisphere: competing effects of ozone and greenhouse gases, Geophys. Res. Lett., 38, L02701, doi:10.1029/2010GL045384 (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Deleted "likely" to avoid confusion with calibrated uncertainty language.
1216	3	40	30	40	30	"Likely" should be italicized, since it is calibrated uncertainty language. (IPCC WGII TSU)	Noted. But do not need to include older work when newer reference also cited.
1217	3	40	30	40	33	A tendency for an increase of the NAO under increasing greenhouse gas focing was already noted in parts or AR4 WG1 CH10 on page 706, in particular referring to Stephenson et al. 2006. (Ulbrich, Uwe, Freie Universitaet Berlin)	Space limits preclude a detailed discussion. We are already about 30% too long.
1218	3	40	30	40	38	These studies should be discussed, evaluated, critiqued and ultimately, assessed. "these studies are not evaluated and critiqued" (UNITED STATES OF AMERICA)	Agreed - included "including the NAO, SAM, and IOD".
1219	3	40	40	40	40	spell out what the natural modes are (UNITED STATES OF AMERICA)	Agreed. See response to comment 1219.
1220	3	40	40	40	43	"changes in the modes" -- we suggest to be more specfic about which modes this includes. (Stocker, Thomas, IPCC WGI TSU)	Agreed. See response to comment 1215.
1221	3	40	40	40	43	We have high confidence in an increasing trend in the SAM under increasing GHGs as it is one of the most robust and consistent responses of models. Only in DJF is confidence lower due to the competing effect of ozone. This statement should be reworded to reflect the seasonality in our confidence in SAM projections. (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Noted. Check that statements in this section (especially summary) and Table 3.1 agree.
1222	3	40	40	40	43	It might be helpful, in this section summary, to indicate the other assessments provided in Table 3.1 ("likely" trends in NAO and SAM and "likely" anthropogenic influence). For example, it seems that the likely assignment for trends is not explicitly mentioned in this section (e.g., is it just the AR4 assignment, which hasn't changed?), and it might be informative to comment on the likelihood assignment for attribution, which seems to be maintained from the AR4, also considering further publications since then. Also, the term "likely" on line 40 should be italicized if it is calibrated uncertainty language; otherwise, its usage should be avoided. (IPCC WGII TSU)	Agreed. Thanks.
1223	3	40	41	0	0	Idem "stratospheric ozone" (Boucher, Olivier, Met Office)	Thanks. We have added the reference to the statement that no regional trends have yet been detected.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1224	3	40	45	0	0	Section 3.4.4: Observed changes in severe land-falling tropical cyclones over eastern Australia have been documented by Callaghan and Power (2010) over a much longer period than has been done previously using historical information sources, providing one of the longest reliable records of tropical cyclone activity in the world. Interannual variability is linked to ENSO and decadal changes are partly linked to decadal in ENSO. There is a downard trend over the period 1872-2010 and some but not all of this is linked to a downward trend in the SOI. Ref: Callaghan, J. and S. Power, 2010: Variability and decline in the number of severe tropical cyclones making land-fall over eastern Australia since the late nineteenth century. Climate Dynamics , DOI 10.1007/s00382-010-0883-2. (Arblaster, Julie, NCAR; Australian Bureau of Meteorology)	Thanks. We have added the reference to the statement that no regional trends have yet been detected.
1225	3	40	45	0	0	An additional reference for this section could include Callaghan and Power (2010) Variability and decline in the number of severe tropical cyclones making land-fall over eastern Australia since the late nineteenth century, Springer, DOI 10.1007/s00382-010-0883-2 (AUSTRALIA)	The section has been restructured and shortened to improve readability. We've added subsections to help break up the main points better.
1226	3	40	45	45	35	This is a very long chapter with many long paragraphs and no highlights along the way--it is very hard to wade through when what one wants is a good summary of the findings either near the start or as bolded or italicized sentences at the heads of paragraphs through the section. (MacCracken, Michael, Climate Institute)	Thanks for these comments. We agree that if the environment of the storms has changed, then the behavior and characteristics of TCs could be expected to change in concert. However, as we have made a point to demonstrate, the literature shows that there is not necessarily a prima facie expection for this (e.g., the climate-dependent TC genesis SST threshold, or potential intensity's dependence on relative versus absolute SST). So a blanket statement that a changing environment will tacitly change all TC metrics of interest would in fact be naive and not defensible within the larger body of literature. Furthermore, while the arguments for water budget changes under global warming may follow one-to-one and monotonic arguments, when patterns of change and circulation changes are taken into account, the question of how any single event like Katrina or Ivan was influenced has to be considered probabilistically. It is entirely possible that global warming could lead to Katrina being weaker than it would have been since it's entirely possible that the deconvolved circulation changes attributable to global warming acted to increase shear, for example, in Katrina's environment.
1227	3	40	45	45	48	The whole framing of this section could be improved. The fact is that the environment for these storms has changed (increased SSTs and different patterns, increase water vapor), so how can they not be affected. Yes the data are poor, and the GCMs don't include realistic TS, so there are uncertainties. But the uncertainties work both ways. There are comprehensive discussions of the framing of this question in Trenberth, K. E., C. A. Davis and J. Fasullo, 2007: Water and energy budgets of hurricanes: Case studies of Ivan and Katrina . J. Geophys. Res., 112, D23106, doi:10.1029/2006JD008303. and Trenberth, K. E., and J. Fasullo, 2007: Water and energy budgets of hurricanes and implications for climate change. J. Geophys. Res., 112, D23107, doi:10.1029/2006JD008304. and for the Atlantic Trenberth, K. E., and J. Fasullo, 2008: The energy budgets of Atlantic hurricanes and changes from 1970. Geochemistry, Geophysics, Geosystems, 9, doi:10.1029/2007GC001847. These do give attribution for changes in precipitation for Katrina and more generally. See also Trenberth (2011) for changes in precipitation (ref given for precip section). (Trenberth, Kevin, NCAR)	The text has been modified. We now simply say "poleward".
1228	3	40	60	40	60	Please avoid this style of writing, where readers are required to parse sentences twice in order to properly understand both meanings. (Zwiers, Francis, Environment Canada)	We feel that "tropical progenitor" is a succinct and understandable way to convey the meaning we intended.
1229	3	40	61	40	61	need to explain "tropical progenitors"? (Stocker, Thomas, IPCC WGI TSU)	The reviewer is perhaps overstating things here. We have changed the text to include Cat3 storms, but it is groundless to say that the main points of Landsea's and Pielke's earlier papers are "no longer the case". It is also not defensible to state that "Cat5 storms tend to be smaller than Cat3 storms". In fact, the pathway to windfield expansion is largely due to eyewall replacement, which is much more prevalent in Cat4-5 storms (see e.g., Kossin and Sitkowski, 2009). We have modified the text about storm size to best reflect the relevant aspects of storm size to this report.
1230	3	41	3	0	0	This is no longer the case. Larger but less intense storms pose a bigger storm surge threat (e.g. Katrina at landfall, Hurricane Ike). Cat 5 storms tend to be smaller so a Cat 3 storm can pose an equal threat (e.g. Katrina at landfall as a Cat 3 with the same Integrated kinetic energy as at max intensity the day before as a Cat 5 (Powell and Reinhold 2007). (UNITED STATES OF AMERICA)	Agreed. This point was addressed in this text, but we have modified it to be more clear about this, and have added the reference.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1231	3	41	9	0	0	There is a almost no relationship between the areal inundation and the SS category of a storm. The size of the wind field plays a much bigger role e.g. Irish and Resio 2010 (UNITED STATES OF AMERICA)	Yes. The text has been modified to be more clear.
1232	3	41	11	41	11	It would be helpful to clarify what is meant by "human-induced climate variability"--are human-induced changes in climate variability meant? (IPCC WGII TSU)	Thanks. We feel that the point has been made adequately and we have to be very judicious about adding new references at this late stage. This newer citation could possibly be added into the upcoming AR5.
1233	3	41	16	0	0	To illustrate the challenge, the recent paper of Grossmann and Morgan (2011) could also be mentioned here. Grossmann I. and M.G. Morgan (2011) Tropical cyclones, climate change, and scientific uncertainty: what do we know, what does it mean, and what should be done? Climatic Change, DOI 10.1007/s10584-011-0020-1 (International Petroleum Industry Environmental Conservation Association (IPIECA))	We have added and modified the text addressing storm size, which we feel will convey the necessary point in the context of this report. The Landsea et al. (2004) reference is listed in the draft we have, so we're unclear were the mix-up might be.
1234	3	41	17	41	18	We would include integrated kinetic energy as another important metric. Its much more meaningful than ACE or power dissipation, which are useless without including some measure of wind field size. Powell Kepert and Uhlhorn (2009) have re-examined several significant Atlantic basin TCs and found that the most intense storms tended to maintain their intensity when reinvestigated with more modern intensity estimation methods. You might also include Powell, Kepert and Uhlhorn 2009 since we find a bias in the method that Landsea 2004 used for storms of Cat 4 and lower intensity. Note: Landsea et al 2004 (line 17) is not listed in the references. (UNITED STATES OF AMERICA)	The text has been modified and shortened by moving the Landsea reference to the more general previous sentence about frequency trends.
1235	3	41	30	41	33	The Emanual (2010) result is for a much shorter time frame and is a much smaller trend than observed even over that shorter time frame. This doesn't come through the way the text is written. The following adaption of the Kossin et al. (2010) concluding sentence on this issue could be added at the end of the paragraph: "Thus there is evidence that both physical processes and data heterogeneity may have contributed to the observed trends in short-duration storms over the 20th century, although current model estimates suggest that physical processes have contributed less to the trend than data heterogeneity." (UNITED STATES OF AMERICA)	Thanks. We have added the Callaghan and Power reference. Since it states a similar conclusion to that of Harper et al, we've chosen to omit Harper et al here in favor of the more recent paper.
1236	3	41	37	41	37	Two more references that could be added here (both for the Australia region) are: Harper, B. A., S. A. Stroud, M. McCormack, and S. West, 2008: A review of historical tropical cyclone intensity in northwestern Australia and implications for climate change trend analysis. Aust. Met. Mag., 57, 121–141. and Calaghan and Power (Clim. Dynamics, 2010, 10.1007/s00382-010-0883-2). (UNITED STATES OF AMERICA)	Good suggestion, thanks.
1237	3	41	41	41	41	Suggest to move "whereas" to just before "intensity estimation" (Stocker, Thomas, IPCC WGI TSU)	Noted, thanks.
1238	3	41	44	0	0	This is illustrated in Powell, Kepert, and Uhlhorn 2009 (UNITED STATES OF AMERICA)	Noted, thanks.
1239	3	41	53	0	0	Power dissipation or ACE as defined in no way measured total energy consumption or actual accumulated energy by a tropical cyclone because these metrics have only intensity and no wind field information. (UNITED STATES OF AMERICA)	Done, thanks.
1240	3	41	59	41	59	Insert "tropical" before "Atlantic". (Zwiers, Francis, Environment Canada)	We refer to this as a decline since 2005, not a trend. To increase clarity, the text has been modified slightly to avoid this mistaken interpretation. There is no evidence to support the notion of "compensation" between the NATL and the rest of the tropics (see, e.g., Frank and Young 2007)
1241	3	41	60	41	63	The discussion of five year trends (since 2005) seems odd in a discussion about climate trends. Better replace by the discussion of long-term trends: "There have been no trends in accumulated tropical cyclone energy (ACE) globally or hemispherically during the satellite observation period (Maue 2009). However, regionally there is a significant upward trend of ACE in the North Atlantic, which is compensated for by a decrease in other basins." (Neu, Urs, Swiss Academy of Sciences)	The change has been made.
1242	3	41	61	0	0	"40-year low point as estimated by Maue (2009)." (UNITED STATES OF AMERICA)	Noted, thanks. Text has been added that briefly addresses this.
1243	3	42	0	0	0	p42. and General Comment: Climate scientists and modelers should be asking whether the size of tropical cyclones has shown any changes over the past 30-40 years and whether TC size might increase in a warmer climate. Larger (but perhaps less intense) TCs would have much more of a damage impact than slightly more intense but smaller TCs. In just the past few years there have been several huge TCs in the Atlantic basin, including Hurricanes Isabel in 2003, Ike in 2008, and this years Igor that dwarf other storms in the H*Wind database. Perhaps this is related to elevated Atlantic SSts providing conditions for large storms that had previously been limited to the W. Pacific basin. (UNITED STATES OF AMERICA)	It's not clear what the reviewer is asking for here, since it seems that we have provided this in this section. Half of the two sentences are a summary of the tentative inferences and what the studies mean as a whole.
1244	3	42	8	42	14	This section could be expanded with further detail on tentative inferences reached from paleoclimate data. Perhaps add one sentence on what the studies mean in whole. (UNITED STATES OF AMERICA)	Done, thanks.
1245	3	42	16	42	16	"Likely" on this line should be italicized, assuming it is being used as calibrated uncertainty language. (IPCC WGII TSU)	Done, thanks.
1246	3	42	16	42	31	The direct quoting here from the AR4 and CCSP is not needed and makes this section unnecessarily long. You should rather provide only a brief assessment of what was concluded in the AR4 and CCSP. This also applies on lines 45 - 47. (Stocker, Thomas, IPCC WGI TSU)	Done, thanks.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1247	3	42	24	42	24	Suggest to cite CCSP report consistently as Kunkel et al. 2008. Or is it necessary to highlight the US CCSP SAP 3.3 prominently here? (Stocker, Thomas, IPCC WGI TSU)	No. A REPORT implies something quite different to Knutson et al..
1248	3	42	31	42	31	The WMO product is a rather different type of 'assessment' than the AR4 or CCSP. It would be more appropriate here to reword as: ".....the most recent REPORT by the World M........." (Stocker, Thomas, IPCC WGI TSU)	Agreed. The text has been modified to be more clear.
1249	3	42	37	42	38	the present assessment…WMO report' this conclusion is confusing. AR4 concluded (line 18 of this page) that there is observational evidence for an increase in t.c. activity..since 1970' Knutson et al., (line 33) 'does not conclude that it is likely that t.c. numbers...have increased over the past 100 years'. Surely these are opposite, not identical conclusions? (UNITED KINGDOM OF GREAT BRITAIN AND NORTHERN IRELAND)	Agreed. The text that originally had direct quoting has been substantially pared down.
1250	3	42	45	42	47	The direct quoting here from the CCSP reports could be avoided (Stocker, Thomas, IPCC WGI TSU)	Agreed, thanks. The text has been modified to be more precise.
1251	3	42	52	0	0	Do you mean to imply that the fractional changes would be largest in the upper quantiles, or that the change would be more detectable in the upper quantiles. We agree with the former, but we're not sure why the latter would apply. This could use some clarification here. (UNITED STATES OF AMERICA)	Both are correct, but one can be argued from simple logic (a gradient cannot increase indefinitely), and the other perhaps requires more evidence (that global warming is not expected to lead to large gradients). We feel that the message is conveyed adequately in the present text.
1252	3	42	62	0	0	We suggest changing "continuously increasing SST gradients" to "large changes in SST gradients" (UNITED STATES OF AMERICA)	Done, thanks.
1253	3	43	17	43	17	Insert "concentrations" after "increasing greenhouse gas". (Zwiers, Francis, Environment Canada)	Done, thanks.
1254	3	43	17	43	17	Change to "greenhouse gas concentrations" (MacCracken, Michael, Climate Institute)	It is not an expectation when the SST increase due to greenhouse gas concentration changes is considered. In the last revision, we added text that specifically targets this issue, and we feel that the point is being adequately and correctly conveyed. Please review the references cited in that new text for a more complete explanation of why there is no prima facie expectation that climate change will increase the size of the TC genesis region.
1255	3	43	19	43	20	There is not just an expectation, but it is documented that the number of TCs varies with the warm SST area (Benestad, R.E. 'On Tropical Cyclone Frequency and the Warm Pool Area' Nat. Hazards Earth Syst. Sci., 9, 635-645, 2009). (NORWAY)	Good suggestion. We've added text to address this.
1256	3	43	21	43	23	Since a logic reason for this statement seems not straightforward, an explanation might be added (e.g. from Knutson et al. 2008, already in ref.): "... because this threshold is related to the temperature needed for a saturated air parcel near the surface to be sufficiently buoyant to rise to the tropopause, and therefore depends on the actual temperatures in the troposphere". (Neu, Urs, Swiss Academy of Sciences)	Thanks, changed to greenhouse gas forcing.
1257	3	43	22	43	22	Do you mean "greenhouse gas" or "anthropogenic" forcing rather than "CO2" forcing? (Zwiers, Francis, Environment Canada)	These comments are valid, but are out of place here. This section deals with observed changes and GCMs are not the subject until later.The relationship between SST and TC genesis, as stated in the text and in the response to comment 1255 above, varies according to the cause of the SST changes. That's the main point here.
1258	3	43	24	43	27	It can be demonstrated that the number of TCs varies with seasonal changes in the warm SST area, and that the number of TCs in the north Atlantic is systematically influenced by ENSO. The question is whether the GCMs are able to capture these features. Furthermore, seeing their limited ability to reproduce the MJO and monsoon characteristics (p. 37, L1-13) – so far, we have not seen any discussion or reference to any evaluation of whether the GCMs have skill in predicting such aspects. (NORWAY)	The text has been changed. But a likelihood statement here is not necessary. Instead the statement now states that the effect is not manifest.
1259	3	43	25	43	25	Need to replace "may" to give indication of likelihood. (MacCracken, Michael, Climate Institute)	Thanks for the comment. But we feel that this is a level of detail that is not needed to convey the points, and in fact could obfuscate the intended message since latitudinal shifts have little to do with the basic idea of a non-stationary genesis threshold. This is also not a modeling section and the suggested additions would be out of place here.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1260	3	43	27	0	0	II 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 atmiddle 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)	Inserted "global". Mean is inferred and is explicitly stated with mean intensity.
1261	3	43	35	0	0	insert "global mean" before "tropical cyclone characteristics" (UNITED STATES OF AMERICA)	Thanks for the comment. But we feel that we've adequately made the point about climate-dependent thresholds. Note that we also address PI changes and relative vs absolute SST in a previous paragraph. We would further suggest that it is not useful to form correlations with SST (or PI) and TC intensity, as PI merely sets an upperbound on intensity.
1262	3	43	39	43	42	Along these same lines, Michaels et al. (GRL, 2006, doi:10.1029/2006GL025757) documented a SST threshold for the formation of major (Cat. 3 or higher) hurricanes in the Atlantic. However, Michaels et al. (2006) found no relationship between local SST and storm intensity above this threshold, indicating that while local SST warming may slightly increase the number of intense tropical cyclones in the Atlantic, the average intensity of major hurricanes would likely be unaffected. (Knappenberger, Paul, New Hope Environmental Sciences)	Noted, thanks.
1263	3	43	39	43	42	Michaels et al. (2005, Journal of Climate), point out that the analysis of Knutson and Tuleya (2004) was based on assumptions that were not manifest in the real world, and that a reconsideration of the assumptions made the detection of an anthropogenic influence on Atlantic hurricane behavior unlikely during this century. A rebuttal to the comments of Michaels et al. was provided in Knutson and Tuleya (2005, Journal of Climate). (Knappenberger, Paul, New Hope Environmental Sciences)	We've removed the word "informal".
1264	3	43	41	43	41	What's an "informal comparison" -- please reword or explain. (Stocker, Thomas, IPCC WGI TSU)	Agreed. We've added a sentence that specifically makes this point.
1265	3	43	43	43	45	The phrasing here needs a bit of adjustemnt. While one may not get a clear signal until after 2050, that does not mean that there will not be an increasing likelihood through the next few decades, just that it cannot be rigorously determined as statistically significant. Somehow it needs to be made clearer that likelihoods will be continually changing even if statistical significance is not acheived for some time. (MacCracken, Michael, Climate Institute)	Thanks for these insights. We feel that a discussion of specific characteristics of the models is not ideally inserted here, but we have added a sentence to the text that mentions the caveats and points the reader to another section that deals more explicitly with this.
1266	3	43	43	43	45	RCMs have higher spatial resolution, and may reproduce more realistic looking TCs. But this does not mean that they are able to provide a reliable account about the global TC characteristics. This hinges on the GCMs ability capture the essential changes in the ambient environment, as the RCM discussed here involves a one-way nesting, where upscaling processes are not well-represented. Hence, the effect of TCs on the large-scale energy flow is incompletely represented. At least, the question about whether the surface latent heat flux and precipitation rates differ in the RCM and GCM must be addressed. We expect the effect of coarse spatial resolution in driving GCMs and misrepresentation of SSTs to limit the reliability of the modelling of Tcs. One interesting question would otherwise be: why would the GCMs provide a good description of the ambient environment TCs when they fail to reproduce the MJO and the monsoon? A recent paper provides a sceptical view on the models' ability to reproduce local details (Oreskes et al, 2010, Philosophy of Science. Vol 77) (NORWAY)	Done, thanks.
1267	3	43	54	43	54	It would be helpful to italicize "more likely than not" on this line to indicate use of calibrated uncertainty language. (IPCC WGII TSU)	The text has been removed or pared down.
1268	3	43	58	43	62	Is the direct quoting here from Knutson et al necessary? Suggest to add a brief assessment of what was concluded rather than quoting. (Stocker, Thomas, IPCC WGI TSU)	Done, thanks.
1269	3	44	5	44	5	Italicize 'low confidence' (Stocker, Thomas, IPCC WGI TSU)	Done, thanks.
1270	3	44	5	44	5	"Low confidence" should be italicized. (IPCC WGII TSU)	The text has been pared down.
1271	3	44	8	44	14	Is the direct quoting here from Meehl et al. et al necessary? Suggest to add a brief assessment of what was concluded rather than quoting. (Stocker, Thomas, IPCC WGI TSU)	The text has been modified.
1272	3	44	9	44	9	It would be helpful to indicate that "likely" on this line is calibrated uncertainty language by un-italicizing it within the italicized quote. (IPCC WGII TSU)	Noted, thank you. As an assessment report, which is not intended as a literature review, we do not necessarily include all possible references, particularly when they are all generally making a similar point.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1273	3	44	19	44	19	Chapter 3, Page 44, Line 19: Zhao et al., 2009 ----> Zhao et al., 2009; Murakami and Sugi, 2010; Walsh et al., 2010 Murakami, H., and M. Sugi, 2010: Effect of model resolution on tropical cyclone climate projections. SOLA , 6, 73-76, doi:10.2151/sola.2010-019. Walsh, K., S. Lavender, H. Murakami, E. Scoccimarro, L-.P. Caron, and M. Ghantous, 2010: The tropical cyclone climate model intercomparison project. Hurricanes and climate change, (Eds., J.B. Elsner, R. E. Hodges, J. C. Malmstadt, and K. N. Scheitlin), Springer, 1-24. (Kusunoki, Shoji, Meteorological Research Institute (MRI))	Thanks for the comment. We had perhaps tried to lump too many papers into one parenthetical list. We've modified the text to be more precise in how we're referencing these papers.
1274	3	44	31	44	34	A correction here: Gualdi et al did not find an intensity increase, but they only used a ~110km grid resolution model, so perhaps this is not surprising, as we can infer from Bengtsson et al's study looking at model of progressively higher resolution. Also Sugi et al. 2009 did not examine intensity in their study. You could mention in this paragraph that a greater number of models show a global decrease in tropical storm frequency, though many of those studies use relatively coarse resolution models that do not simulate very intense tropical cyclones. (UNITED STATES OF AMERICA)	Thank you for the suggestion, but this paper deals more with a technical aspect of the models themselves and not so much how TC intensity is changing. While the paper is relevant to the discussion in general, we feel it is too specific to justify adding it here.
1275	3	44	33	44	33	Chapter 3, Page 44, Line 33: Bender et al., 2010 ----> Bender et al., 2010; Murakami and Sugi, 2010 Murakami, H., and M. Sugi, 2010: Effect of model resolution on tropical cyclone climate projections. SOLA , 6, 73-76, doi:10.2151/sola.2010-019 (Kusunoki, Shoji, Meteorological Research Institute (MRI))	Thanks, this has been addressed (see response to comment 1265).
1276	3	44	33	44	34	The phrasing needs adjustment here--it sort of implies that there will not be changes until the end of the century, when what will really happen is that there will be changes through the century even though statistical significance is not achieved until late in the century. (MacCracken, Michael, Climate Institute)	Thanks for these insights. The general model characteristics and caveats are found in another section of this chapter, and we have added some text related to this (see response to comment 1266). Also, note that this is an assessment report based on existing literature, and performing new analyses to the extent suggested would go beyond the intended scope.
1277	3	44	34	44	37	Given that GCMs are not producing details of the tropical weather very well (see previous comments), their limited resolution (which means that many TC-relevant relevant processes are not explicitly presented), errors in SST, we would first want to see a convincing evaluation of the models on whether they actually have any skill in predicting the metrics that are under investigation (trend). To have any value, the models also should demonstrate whether they reproduce the geographical, seasonal TC-response, and the empirical relationships with SST, wind shear, etc. We are also concerned about the frequency distribution of the wind speeds in the tropics. Winds have one distribution for all times, describing situations with no wind to situations with extreme storms. If present/historical wind speeds follow a Weibull distribution, then the threshold value (10-minute sustained) for a TC is fixed at the value 63 km/h. A future with fewer TC but more frequent intense one would suggest a change in the wind speed distribution, with a reduced area under the curve for winds greater than 63 km/h, but with a thicker upper tail. The area of the curve for winds slower than 63 km/h would furthermore be greater in such a future world. This would be extremely interesting – if the frequency of calm situations were relatively unchanged, that would suggest a more complicated PDF for the winds. Alternatively, there are Weibull shapes that satisfy the change in the areas under the curve, but that would suggest a substantial increase in the number of days with calm. (NORWAY)	Agreed. The text has been modified.
1278	3	44	37	44	37	"...cyclone intensity changes" would be better as "...cyclone intensity increase" (Stocker, Thomas, IPCC WGI TSU)	Thanks, this is from the same source. The text has been modified to say "maximum wind speed" (which is generally understood to be synonymous with "intensity" in TC research).
1279	3	44	37	44	38	Please check if the statement on cyclone intensity change "(between 2 and 11%)" is from a different source than the one of Knutson et al (2010) that is recalled on p45, lines 9-10: "increase in mean tropical cyclone maximum wind speed (+2 to +11% globally)". If this is the source, the precision on "maximum wind speed" should be added because "intensity" is too vague. Whatever the source, the reference should be added. (PLANTON, Serge, Météo-France)	Agreed. The text has been pared down to eliminate repetition.
1280	3	44	37	44	44	Part of this assessment is repeated on page 45, lines 8-35. Please avoid unnecessary duplication, either here or on page 45. (Stocker, Thomas, IPCC WGI TSU)	This type of behavior is typical when individual members are compared to the ensemble mean, and it's not clear that this needs to be singled out in this particular case. We have added text that explicitly states that the results discussed here are based on the 18-model ensemble-mean fields. The details of the study, including the behavior of the 4 individual models that Bender et al chose to single out, can be readily found in their paper.
1281	3	44	39	44	42	Regarding Bender et al., you should elaborate that the increase in Cat 4-5 numbers was not simulated for all four of the individual models they downscaled (one showed a decrease), but it was simulated using average climate change conditions from 18 CMIP3 models. (UNITED STATES OF AMERICA)	Noted, thank you. We've added the Murakami et al. (2011) reference, but did not go into the regional aspects. We will consider including these details in the AR5 Ch.14, which deals specifically with regional aspects.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1282	3	44	49	44	49	Chapter 3, Page 57, Line 49: (Emanuel et al., 2008). ----> (Emanuel et al., 2008). Murakami and Wang (2010) and Murakami et al. (2011) suggested that the physical mechanisms of the future changes in tropical cyclone frequency are different basin by basin. Murakami, H., and B. Wang, 2010: Future change of North Atlantic tropical cyclone tracks: Projection by a 20-km-mesh global atmospheric model. J. Climate, 23, 2699-2721, doi:10.1175/2010JCLI3338.1. Murakami, H., B. Wang, and A. Kitoh, 2011: Future change of western North Pacific typhoons: Projections by a 20-km-mesh global atmospheric model, J. Climate, 24, 1154-1169, doi:10.1175/2010JCLI3723.1. (Kusunoki, Shoji, Meteorological Research Institute (MRI))	Thank you for the feedback.
1283	3	44	56	45	6	I like the discussion of mechanisms included here. (MacCracken, Michael, Climate Institute)	Thanks for the suggestion, but it would be very difficult to add a figure at this late stage of the report. We do feel that we have conveyed the salient information in the text.
1284	3	45	0	0	0	I highly reccomend to included a figure describing the changes in tropical cyclones present trends (ITALY)	Please see our response to comment 1255.
1285	3	45	2	45	2	Should add that the relationship between number of TCs and warm SST area has also been explored, suggesting a non-linear relationship between these (Benestad, R.E. 'On Tropical Cyclone Frequency and the Warm Pool Area' Nat. Hazards Earth Syst. Sci., 9, 635-645, 2009), but that it is only one of many factors affecting the TC-statistics. The non-linear relationship and strong influence from other factors can explain why trends have been so hard to detect in the past. (NORWAY)	This is a valid point, and certainly an interesting one, but given our discussion about the non-stationarity of TC genesis thresholds, we would only be able to speculate with no solid physical grounds or citations to reference. This would stray a bit too far from a literature assessment report.
1286	3	45	8	45	25	I was surprised not to see mention that tropical cyclones might develop in new areas, such as the South Atlantic where an occasional storm has now been observed--global totals are not the key issue, what matters is what is appening in the various basins, and if new areas start experiencing their occurrence. (MacCracken, Michael, Climate Institute)	Thanks, we've made appropriate modifications to the text.
1287	3	45	10	45	11	Assuming "likely" on both of these lines is being used as calibrated uncertainty language per the AR5 Guidance Note on Treatment of Uncertainties, it should be italicized. (IPCC WGII TSU)	After giving this careful thought, we've concluded that this is not an equivalent statement in terms of perception, even though it may be true in a formal logic setting. To say that something is unlikely to increase does not properly emphasize the point that there is a substantial likelihood of a decrease. The perception of "no increase" may be that it will remain the same. We feel that it is more informative and truer to the intended meaning to say "likely to decrease or remain the same".
1288	3	45	11	45	11	This could be stated equivalently as "it is likely that overall global frequency will not increase". (Zwiers, Francis, Environment Canada)	We've changed it to "more likely than not". This is also in better alignment with Knutson et al (2010).
1289	3	45	12	45	12	Do you mean "medium confidence" or "more likely than not" (these two assessments are not equivalent). (Zwiers, Francis, Environment Canada)	Agreed, thanks. This is a very good point. The text has been modified to better convey the intended message.
1290	3	45	12	45	13	The assessment "medium confidence that the frequency of the most intense storms (e.g., Saffir-Simpson Category 4-5) will increase in some ocean basins" seems a little weaker than the projection assessment of Knutson et al (2010) as it doesn't say something like "increase substantially". The statement leaves open the question of how large an increase. In fact, it seems little different from a projection one could make with just a randomly varying climate. For example, even if there is no systematic climate change, but just random variability, and taking the 1981-2010 period as the baseline, we might expect that the frequency of cat 4-5 storms in some basins would be higher during 2081-2100 than during 1981-2010 (and in some basins would be lower). The question is how much higher would be frequency be in some basin? Large enough to be detectable compared to natural variability for example? Perhaps the meaning of the author's assessment statement could be further clarified. (UNITED STATES OF AMERICA)	We feel that this is a bit overly specific. Also note that a tropical storm is a tropical cyclone (with intensity between 35 and 65 kt). Perhaps tropical depression was meant? Either way, this is a very specific statement that doesn't necessarily add to the report. Also note that we do have a brief mention of the mechanism of rainfall rate increases.
1291	3	45	20	45	20	It should be mentioned that there can be huge amounts of precipitation from tropical storms that do not develop into tropical cyclones--and the warmer waters also increase the potential for this occurrence. (MacCracken, Michael, Climate Institute)	Agreed. The material from redundant paragraphs has now been considerably distilled down.
1292	3	45	27	45	35	Am not sure that this paragraph is needed. It seems to duplicate much of the same information given in the above paragraph and in the bolded paragraph below. Consider deleting this paragraph, and shifting anything crucial from this paragraph (ie, the projected frequency information lines 32-33) into the preceding paragraph. (Stocker, Thomas, IPCC WGI TSU)	The models are not effective at projecting such specific hazards and impacts. We do state this lack of confidence in this section.
1293	3	45	27	45	35	The section does not address storm surges. This issue should be mentioned, at least referring to where in the report it is discussed. (MacCracken, Michael, Climate Institute)	It certainly could, but this is not the best place to introduce a discussion about model weaknesses. We do explicitly mention this above, and point the reader to other sections that deal with model uncertainty.
1294	3	45	27	45	35	Does the current performance of the CMIP models regarding the patterns of vertical temperature change in the tropics decrease the confidence in climate model performance (including the performance related to the characteristics of tropical cyclones)? (UNITED STATES OF AMERICA)	The change has been made.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1295	3	45	37	45	37	...that any OBSERVED long-term increases in ....' (Stocker, Thomas, IPCC WGI TSU)	Thanks for these suggestions and comments. The two statements are separated, in a general sense, as 1) past changes and 2) causes for the changes, which is aligned with the original subsectioned format of the chapter. The TC section has now been sub-sectioned again, which should help to better disinguish the context of the two statements. We are reassessing the contributions to the ES and SPM based on your suggestion. Regarding our assessment of low confidence, your point that we might have medium confidence of no trend is a good one, but our statement specifically targets increases. We are using "robust" here in the usual sense; that it stands up to scrutiny in a consistent way.
1296	3	45	37	45	41	There are two main assessment statements about past changes in TCs: 1) "there is low confidence that any reported long-term increase in tropical cyclone activity are robust after accounting for past changes in observing capabilities." 2) "The uncertainties in the historical tropical cyclone records, the incomplete understanding of the physical mechanisms linking tropical cyclone metrics to climate change, and the degree of tropical cyclone variability provide only low confidence for the attribution ofany detectable changes in tropical cyclone activity to anthropogenic influences." I think statement #2 is more general and more useful than the first, so I would propose the authors highlight it in the SPM, etc more than the first statement. The first statement is more confusing, as we don't know what the terms "robust" and "long-term" mean in this context. If it means that we have low confidence that we've identified the trend that one would measure if we had "perfect" data so that we should have low confidence in all reported trends, this may be too strong a criticism of the data. Don't we have some at least medium confidence in some reported TC trends such the hurricane landfalling record for the US from 1900 on? Granted , this trend is estimated as ~zero, but don't we have some confidence in this estimate? Where we really have low confidence is in the attribution of any observed changes in TC activity to anthropogenic forcing (i.e., statement 2). (UNITED STATES OF AMERICA)	This is a good question. We chose to provide a summary assessment of the likelihood of increases because that is most probably the a priori expectation that readers will bring to this. But note that we do in fact provide a likelihood of decreasing TC frequency in the main assessment statements of the text. We have addressed the comment about slow changes in comment 1265.
1297	3	45	37	45	48	While there is not confidence in the increases, are there any indications of decreases--it would be helfpul even to give a sense of what is happening or of expectations, even if the changes might happen slowly. I would also suggest that the summary be separated into two paragraphs, one dealing with the past, and one with the future. (MacCracken, Michael, Climate Institute)	We disagree. Please see the response to comment 1288.
1298	3	45	44	45	45	This could be stated equivalently as "it is likely that overall global frequency will not increase". (Zwiers, Francis, Environment Canada)	They come from a suite of modeling studies and are meant to represent a range from those results. More detail on where these values came from can be found in the tables of the supplementary material of Knutson et al. (2010)
1299	3	45	45	0	0	+2 to +11%; how exact are these numbers? (Klein Tank, Albert, KNMI)	Agreed. The material from redundant paragraphs has now been considerably distilled down and we are using "more likely than not".
1300	3	45	46	45	47	The repetition of the previous assessment can probably be avoided. Note my previous comments that medium confidence and more likely than not assessments are not equivalent. (Zwiers, Francis, Environment Canada)	No, this would require a substantial introduction of new text. It would also represent a significantly more specific interpretation of the models than was applied by Knutson et al. (2010), which would be difficult to justify.
1301	3	45	48	45	48	Is it possible, without expanding the summary too much, to be more specific here than 'in some basins'? (Stocker, Thomas, IPCC WGI TSU)	We have not included much on cyclone frequency trends over continental regions since there is relatively few publication addressing the continents. As the continents are on the fringe of the stormtracks the results becomes more dependent on stormtracking methodology than in the center of the stormtracks.The first paper sugested by the reviewer only looks at ocean regions (Atlantic and Pacific) so there is not information to obtain from that paper on continental cyclone has The second paper is already cited in the manuscript, but have little on continental trends. We have however included specific trends for parts of Europe were we cite the Donat et al., 2010 paper.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1302	3	45	50	49	22	Section 3.4.5: More information on the trends and future projections of extratropical cyclones frequency over continental regions (where they matter) would be useful. Reference: Mesquita, M.d.S., Kvamstø, N.G., Sorteberg, A. and Atkinson, D.E., 2008. Climatological Properties of Summertime Extra-Tropical Storm Tracks in the Northern Hemisphere. Tellus A, 1-13. Also, if available, some information on the seasonal dependence of cyclone trends and projections are also useful. Reference: Ulbrich, U., G.C. Leckebusch, and J.G. Pinto, 2009: Extra-tropical cyclones in the present and future climate: a review. Theoretical and Applied Climatology, 96(1-2), 117-131 (CANADA)	Due to the limitation on number of pages for this chapter we have decided to not go into any details on the cyclone development, but we have added the conversion from tropical to an extratropical system to the sentence in p. 45 line 52.
1303	3	45	52	45	55	A further option for development is the conversion from tropical to an extratropical system (see e.g. Jones, 2010, "The downstream impact of tropical cyclones on a developing baroclinic wave in idealized scenarios of extratropical transition", QUARTERLY JOURNAL OF THE ROYAL METEOROLOGICAL SOCIETY, Volume: 136, Issue: 648, pages: 617-637, Part: Part A). (Rapp, Joerg, Deutscher Wetterdienst)	Due to page constraints. We have not expanded this into a separate paragraph. We have not been able to find peer review papers supporting the the argument of less cold dense air favor warmer moist air from lower latitudes to push further northward. The main argument related to less cold dense air from the north is that it reduces the thermal contrasts of the cold arctic air and the warmer mid latitude air and therefore may reduce the intensification of the cyclones. The second argument about the role of seaice we think is captured in the text. (page 46. Line 6). We have also added the Bader et al, 2011 reference which is a review of sea ice and extratropical cyclone realtionships
1304	3	45	63	46	9	This issue of why things are changing merits a separate paragraph. It would help for the chapter to have a global overview of the shifts in circulation that are expected. An aspect not mentioned here is that with a much warmer Arctic, the air coming out of that region is not as cold (dense) and so there will be less pressure exerted on the northern edge of the extratropical storm belt, letting the warmer moist air from lower latitudes to push forther northward, and the collision of air masses is occurring at different latitudes (especially over North America). It might also be mentioned that the warmer conditions in high latitudes caused by the delayed refreezing of sea ice creates locations of energy injection into the atmosphere that can deflect/disrupt the polar vortex, allowing cold air to pur out differently than it has in past winters, thus further altering the character and locations of storms. (MacCracken, Michael, Climate Institute)	This is noted, and we have added that the low and high level pole to equator temperature gradients may change in oposite directions.
1305	3	46	3	46	3	Note that the projected change in the Northern Hemisphere zonal mean meridional temperature gradient in winter is different in the lower troposphere (where it is projected to decrease due to surface warming, mainly associated with a retreat of polar sea ice) and in the upper troposphere (where it is projected to increase). (Ulbrich, Uwe, Freie Universitaet Berlin)	The sentence has been changed to make the distinction between changes in total activity and latitudibal changes (which may give strong regional changes) is more clear.
1306	3	46	6	46	9	“unclear to what extent…” The distinction between a latitudinal shift and a change in activity is not clear. A latitudintal shift will be important for the regional distribution of storminess, so I think it should “count” just as much as a change in intensity (UNITED STATES OF AMERICA)	Should be too not two.
1307	3	46	15	46	15	assume 'too' should be 'two' (UNITED KINGDOM OF GREAT BRITAIN AND NORTHERN IRELAND)	Noted. Text added.
1308	3	46	16	46	16	"Dynamical core" should be defined (UNITED STATES OF AMERICA)	Noted and changed
1309	3	46	20	46	21	…, but progress is being made, e.g., by using…' (Stocker, Thomas, IPCC WGI TSU)	We partly agree. Several reviewers of the first order draft have asked for a paleo perspective. We have added some text to conclude what we mean the paleo studies mean in whole: ... "Thus the information gained from paleoclimatic proxies to put the last 100 years of extratropical cyclone variability in context is limited." ...
1310	3	46	20	46	27	The information content here seems very small. Relevance? (SWEDEN)	A sentence on this has been added.
1311	3	46	20	46	27	this section could be expanded with further detail on any potential inferences reached from paleoclimate data. Perhaps add one sentence on what the studies mean in whole. (UNITED STATES OF AMERICA)	Done
1312	3	46	47	46	48	move citations to Wang et al and Raible et al to the end of the sentence. (Stocker, Thomas, IPCC WGI TSU)	Unfortunately we are reluctant to add new references for relatively small regions at this stage, but the papers results for the Atlantic of the paper is now cited
1313	3	46	49	46	49	Add at the end of this sentence 'and in a study of climate variability over the Baltic Sea area.' Reference: Lehmann A., Getzlaff K. and Harlaß J., 2011: Detailed assessment of climate variability in the Baltic Sea area for the period 1958 to 2009. Climate Research, 46, 185-196. (Pavan, Valentina, ARPA Emilia-Romagna)	This is noted and text is added to reflect this.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1314	3	46	51	46	51	These inconsistencies can be produced from the different sensitivities of the identification schemes used. They can also be produced from different definitions of an "extreme cyclone", as climate change signals have been found to differ between different cyclone intensities. Here are a few references for this effect: Leckebusch, G.C., M. Donat, U. Ulbrich and J. Pinto, 2008: Mid-latitude cyclones and storms in an Ensemble of European AOGCMs under ACC. CLIVAR Exchanges (ISSN No: 1026 - 0471), 46 (Vol. 13, No. 3), 3-5. http://eprints.soton.ac.uk/55670/01/Exch_46_final3.pdf, see also Pinto, J. G., Spangehl, T., Ulbrich, U., Speth, P., 2006. Assessment of winter cyclone activity in a transient ECHAM4-OPYC3 GHG experiment. Meteorologische Zeitschrift, 15, 1-13. 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. Leckebusch, G.C., B. Koffi, U. Ulbrich, J. Pinto, T. Spangehl, S. Zacharias, 2006: Analysis of frequency and intensity of winter storm events in Europe on synoptic and regional scales from a multi-model perspective. Climate Research, 31, 59-74 A review of recent results is given in 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)	Sentence changed
1315	3	46	53	46	55	This is another example of a dual duty sentence, where the meaning changes upon substitution of the words in parentheses; this writing style makes text difficult to understand, and should be avoided. (Zwiers, Francis, Environment Canada)	Noted
1316	3	46	53	46	57	Page 39, line 49 seems to draw a strong connection between NAO changes and cyclone activity changes, but here the text seems to be more nuanced. Should that nuancing be carried across to p39? (Zwiers, Francis, Environment Canada)	We feel that this concern is dealt with by not stating that the links are intermittent, but that they may be. We have stated this more clearly by adding that the statistical intermittency does not necessarily mean that the underlying physical processes responsible for creating the connection act only intermittently.
1317	3	46	53	46	57	I tend to take analyses of things that appear to be intermittent with a grain of salt. Such analyses are typically based on moving window analyses, which implies that the statistical tools that are being used have limited detection power due to small record lengths. Consequently, much of the apparent intermittency may be the result of sampling variability that affects the outcome of low-power tests. The fact that detection occurs only intermittently under such circumstances does not necessarily mean that the underlying physical processes responsible for creating the connection act only intermittently. (Zwiers, Francis, Environment Canada)	Noted.
1318	3	46	61	0	0	Presumably the conclusion is that large multidecadal variability exists, rather than that the reanalysis data are wrong. Note that the multidecadal variability described here will also have affected the reported trends in precipitation extremes over the same period. Include a similar comment in that section? (Klein Tank, Albert, KNMI)	Due to page limitations we have decided to not include regional mid and high latitude cyclones with smaller sizes and shorter life cycles than typical synoptic extratropical cyclones. This is now clarifed in the first paragraph of the extratropical cyclone chapter: "It should be noted that regionalized smaller scale mid latitude circulation phenomena such as polar lows and mesoscale cyclones are not treated in this chapter. " Thus, mediterranean cyclones have unfortunately been excluded.
1319	3	47	0	0	0	There are no references to recent studies dealing with changes in Mediterranean cyclones, such as Flocas et al (2010), Nissen et al (2010), Raible et al (2010). More specifically, Flocas et al (2010) demonstrated a statistically significant negative trend for winter cyclonic tracks entering Eastern Mediterranean, mostly attributed to decrease in baroclinicity while a positive trend was found in September and November associated with SST increase. Nissen et al related a third of the trends of cyclones causing wind storms in the Mediterranean with the NAP variability. Raible et al (2010) presented for the future a decrease in the number of Western Mediterranean cyclones by 10% while no significant change is found in Eastern Mediterranean. References: 1) Flocas, H.A., I. Simmonds, J. Kouroutzoglou, K. Keay, M. Hatzaki, D. N. Asimakopoulos, V. Bricolas, 2010: On cyclonic tracks over the Eastern Mediterranean. J. Climate, 23, 5243-5257 doi: 10.1175/2010JCLI3426.1 2) Raible, C., Ziv, B, Saaroni, GA, Wild M, 2010: Winter synoptic-scale variability over the Mediterranean basin under future climate conditionsas simulated by the ECHAM5. Clim. Dynamics, 35, 473-488. (GREECE)	changed to US West Coast (which is the term for the westernmost coastal states of the United States.)
1320	3	47	10	47	12	Was the entire US west coast affected including the Alaska panhandle, and the west coast of the coterminus US south of the 49th parallel? If so, was the Canadian coast between these two areas also affected? (Zwiers, Francis, Environment Canada)	Noted and changed
1321	3	47	19	47	22	The work cited here is not in the "Wang et al. 2006a" listed in the References; rather it is in "Wang et al. 2006b", which should be added in the Reference section, replacing the "Wang et al. 2006b" listed there (see my comment #3). (Wang, Xiaolan, Environmen Canada)	Noted and changed to Wang et al., 2006
1322	3	47	27	47	27	The "Wang et al. 2009b" should be replaced by "Wang et al. 2006a", because only the former is not about cyclone activity in reanalysis data, but the latter is. This correction will result in the "Wang et al. 2009b" listed in the References is not cited anywhere in this Chapter and thus should be deleted (see my comment #6). (Wang, Xiaolan, Environmen Canada)	Noted and changed to Wang et al., 2006

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1323	3	47	27	47	27	Wang et al. 2009b wrong reference (Brönnimann, Stefan, University of Bern)	This is correct, previous studies is meant to be the pre AR4 study of Fyfe, 2003. We have change the text to clarify this.
1324	3	47	40	47	40	Not sure what previous studies menas; I thought Pezza et al. used ERA40 not NCEP (Brönnimann, Stefan, University of Bern)	The use of bold in sentences is not allowed inside the text
1325	3	47	48	47	60	This is a helpfully organized paragraph--it might help to bold the first and last sentences. (MacCracken, Michael, Climate Institute)	Noted and changed
1326	3	47	49	47	49	"The degree of agreement" would be better as: 'The level of agreement'. (Stocker, Thomas, IPCC WGI TSU)	Noted and changed
1327	3	47	49	47	51	In this sentence, agreement is described as "high" and evidence as "robust." Since the terms "high" and "robust" are calibrated uncertainty language (per the AR5 Guidance Note on Treatment of Uncertainties), they should be italicized. Additionally, the author team should consider carefully whether agreement as conceptualized here is more nearly "consistency" of evidence in the framework of the AR5 Guidance Note on Treatment of Uncertainties. (IPCC WGII TSU)	Noted. Robust replaces strong.
1328	3	47	57	47	57	In this line agreement is described as "high" and evidence as "strong." If calibrated uncertainty language is intended (per the AR5 Guidance Note on Treatment of Uncertainties), "high" should be italicized, and "robust" or another calibrated term (also italicized) should be used in place of "strong." (IPCC WGII TSU)	Noted
1329	3	47	59	47	59	In this line, agreement is described as "low." Assuming calibrated uncertainty language is intended (per the AR5 Guidance Note on Treatment of Uncertainties), "low" should be italicized. (IPCC WGII TSU)	Noted. Text and references has been added in p. 48, line 16 and p 46 line 6.
1330	3	47	62	48	21	The discussion here gives very little attention to the idea that jet streams and stormtracks can shift poleward because of stratospheric cooling. This is particularly relevant for the Southern Hemisphere since studies like Thompson and Solomon (2002 DOI: 10.1126/science.1069270) show that ozone depletion in the South Polar vortex has probably been responsible for a poleward shift of the SH jet over the late 20th century. The same idea works for global warming, as CO2 cools the stratosphere while warming the troposphere, which leads to a meridional temperature gradient at the tropopause level and a stronger jet. This is an important concept as the document makes lots of references to a reduction in the pole-to-equator temperature gradient due to global warming, but the temperature gradient in the UTLS region actually gets stronger. Chen and Held (GRL 2007) give an explanation for why the poleward shift of the jet and eddy activity goes all the way to the surface. Lorenz and DeWeaver (2007) also argue for a strong role for CO2-induced stratospheric cooling in causing poleward jet shifts. (UNITED STATES OF AMERICA)	Role of ozone changes noted in text.
1331	3	47	62	48	21	One reason why this is an issue is that the section makes lots of references to anthropogenic versus natural cause for stormtrack shifts, but it's not clear if the authors include ozone depletion in their definition of anthropogenic effects. (UNITED STATES OF AMERICA)	Reference added
1332	3	48	4	48	5	Suggest the inclusion of Gillett et al. (2009) (detection analysis of SLP trends in all seasons) as another reference. (CANADA)	Due to limited space we have not gone in detail into the relationship between the stormtracks and Hadley cell widening, and to which extent the Hadley cell widening is influencing the stormtrack shift or if the widening is just a simultaneous response to the mid latitude tropospherich height changes, but we now cite papers which refer to this (e.g. Son et al, 2010).
1333	3	48	4	48	15	An additional influence on the poleward shift of stormtracks might be a poleward extension of the Hadley Cell and a resulting shift of the jet, which is a common feature in GCMs (e.g. Lu, J., A. Deser, T. Reichler, 2007: Lu, J., C. Deser, and T. Reichler, 2009: Cause of the widening of the tropical belt since 1958. Geophys. Res. Lett., 36, L03803; Hu, Y. and Q. Fu, 2007: Observed poleward expansion of the Hadley circulation since 1979. Atmos. Chem. Phys., 7, 5229-5236) and has been detected in observations (Seidel, D.J., Q. Fu, W. J. Randel, and T. J. Reichler, 2008: Widening of the tropical belt in a changing climate, Nature Geoscience 1, 21-24; Johanson, C. M., Q. Fu, 2009: Hadley Cell Widening: Model Simulations versus Observations. Journal of Climate, 22, 2713-2725) (Neu, Urs, Swiss Academy of Sciences)	This is added.
1334	3	48	5	48	5	Add: Gillett, N. P., and P. A. Stott (2009), Attribution of anthropogenic influence on seasonal sea level pressure, Geophys. Res. Lett., 36, L23709, doi:10.1029/2009GL041269. (Brönnimann, Stefan, University of Bern)	level of confidence is assigned
1335	3	48	17	48	18	The author team should consider whether it would be preferable to assign a level of confidence instead of the likelihood term "about as likely as not" as is done elsewhere in the chapter. (IPCC WGII TSU)	Not sure we understand the comment. This sentence ends the discussion about atribution of observed trends. Before we move on with the disscussion on future changes.
1336	3	48	17	48	21	It is a little confusing to have a summary in the middle of a long section. It would be useful to perhaps break the section into parts and then have summary statements like this in bold. (MacCracken, Michael, Climate Institute)	It implies that multiple-step detection has been done in some studies (eg., Wang et al, 2009) but no detection is found in other studies(Gillette and Stott, 2009)
1337	3	48	18	0	0	This implies that also there has not been a multiple-step detection described in 3.2.2.3? (Klein Tank, Albert, KNMI)	The sentence is rephrased.

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1338	3	48	21	48	21	"enhances our confidence in this assessment" is unclear about the exact statement here considered. If this is the one on lines 17 and 18 that is "about as likely as not", it is difficult to understand that "indirect evidence" or "increasing physical understanding" can enhance this confidence. (PLANTON, Serge, Méto-France)	This is noted in the paragraph below: "It should be noted that other studies indicate that the poleward shift is less clear when models including a full stratosphere and ozone recovery are used (Son et al., 2008)" The Scaife paper is not yet published, but we have added references to the Huebener et al., 2007, Morgenstern et al., 2010 papers to empahsis this.
1339	3	48	26	48	26	There is agrowing evidence that the general shift polewards of the storm track may be an systematic bias in the AR4 models due to their common deficincy of not having a well resolved stratosphere. see Climate Change and Stratosphere-Troposphere Interaction. A.A. Scaife, et al 2011.Clim. Dyn., accepted. (Brown, Simon, The Met Office Hadly Centre)	Done
1340	3	48	28	48	28	Define "upper tropospheric storm track" for readers. (Zwiers, Francis, Environment Canada)	Due to a mistake in the reference handling the two other studies (O'Gorman, 2010; Wu et al., 2010) was missing in the SOD .Tthey are now added. The northern hemisphere term is deleted.
1341	3	48	28	48	29	"northern hemisphere poleward shift" This is misleading, as Lorenz and DeWeaver (2007) found stronger jet shifts in the Southern Hemisphere. One reason this is important is that the Southern Hemisphere is where the recovery of the ozone hole may be important for stormtrack shifts. (UNITED STATES OF AMERICA)	Due to a mistake in the reference handling the two other studies (O'Gorman, 2010; Wu et al., 2010) was missing in the SOD final version they are now added.
1342	3	48	28	48	30	Suggest the inclusion of more references, as the sentence implies multiple references "...post-AR4 studies...". (CANADA)	References to two other studies (Huebener et al., 2007, Morgenstern et al., 2010) was missing in the SOD final version. In Morgenstern et al., 2010 ((CCMVal-2 runs) most models had a close to T42 resoulution and around 40-70 levels, thus the resolution is comparable to several AR4 models. The Huebener et al., 2007 paper is a coupled model study which reproduce the southward stormtrack response in the uncoupled models.
1343	3	48	29	48	32	I'm not sure how much weight I would put on this. Middle atmosphere models typically have lower horizontal resolution than models with somewhat lower lids. Also, my understanding is that most models used in CCMVal (which is the context for the Son et al paper) are not coupled to ocean models. (Zwiers, Francis, Environment Canada)	Due to a mistake in the reference handling the McDonald study was missing in the SOD final version it is now included.
1344	3	48	42	48	42	McDonald 2010 is another study that indicates a south/shoutheast shift in the earstern end of the North Atlantic storm track. This is attributed to a local minimum in ocean warming in the central North Atlantic and subsequent local changes in baroclinicity and southward shift of the jet. More cyclones are found with deeper central pressures but this increase may be due to the background mean sea level pressure. McDonald also show that storm change results are very dependant on sample size and individual ensemble members can have considerably different future chagnes. Ruth E. McDonald Understanding the impact of climate change on northern hemisphere extra-tropical cyclones.Climate Dynamics DOI: 10.1007/s00382-010-0916-x: (Brown, Simon, The Met Office Hadly Centre)	Due to page limitations and concerns that the report gets too Eurocentric we have decided to not include regional mid and high latitude cyclones with smaller sizes and shorter life cycles that typical synoptic extratropical cyclones. Thus, mediterranean cyclones and polar lows have been excluded.
1345	3	48	44	48	48	Add reference to Lionello P. and F. Giorgi, 2008: Future changes in cyclone climatology over Europe as inferred from a regional climate simulation. Clim Dyn, 30, 657–671. In this paper it is shown that, over the Mediterranean region, projections for the period 2071-2100, under the A2 scenario conditions, include a significant decrease in synoptic activity, measured in terms of cyclones frequency, for the months from September to November and from April to May. Under B2 scenario, projections suggest the occurrence of a significant decrease only in the month of November. (Pavan, Valentina, ARPA Emilia-Romagna)	We can not add new figures at this stage.The reason for this is that the report is not going out for a third review, so the scientfic community will not be able to comment on the new figures.
1346	3	49	0	0	0	I highly reccomend to included a figure describing the changes in extratropical cyclones present trends (ITALY)	After discussion in the author team, the two terms do not equate anymore so assesment changes to medium confidence.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1347	3	49	10	49	20	This summary distinguishes between "more likely than not" and "medium confidence" (which I think should be done if confidence is sufficiently high to allow an assessment that the likelihood of a given outcome is greater than 50%). See comment concerning page 9, line 13 (3.1.5). (Zwiers, Francis, Environment Canada)	The first paper is not addressing extratropical cyclones which is the topic of this section, rather it is about a mesocale penomena (polar lows). This is now clarifed in the first paragraph of the extratropical cyclone chapter: "It should be noted that regionalized smaller scale mid latitude circulation phenomena such as polar lows and mesoscale cyclones are not treated in this chapter. We feel the essence of the second paper is already in the manuscript: Page 32: "More recent studies confirm these findings and indicate that storminess in this region exhibits strong inter-decadal variability (Alexandersson et al., 2000; Allan et al., 2009; Wang et al., 2009c). The latter half of the 20th century was punctuated by a peak in storminess around 1990 which according to Wang et al. (2009c) is unprecedented since 1874. However, no long-term trends were detected in storminess over this time period (Barring and von Storch, 2004; Barring and Fortuniak, 2009)" and page 46: "It should be noted that there is some suggestion that the reanalyses cover a time period which starts with relatively low cyclonic activity in northern coastal Europe in the 1960s and reaches a maximum in the 1990s. Long term European storminess proxies show no clear trends over the last century. "
1348	3	49	10	49	20	There are two recent papers of note concerning extratropical circulation and storminess, the results of which should be interwoven into this section on extratropical storms and which very well may impact the overall summary. They are: Zahn, M., and H. von Storch. 2010. Decreased frequency of North Atlantic polar lows associated with future climate warming. Nature, 467, doi:10.1038/nature09388. "[I]n projections for the end of the twenty-first century, we found a significantly lower number of polar lows and a northward shift of their mean genesis region in response to elevated atmospheric greenhouse gas concentration. This change can be related to changes in the North Atlantic sea surface temperature and mid-troposphere temperature; the latter is found to rise faster than the former so that the resulting stability is increased, hindering the formation or intensification of polar lows. Our results provide a rare example of a climate change effect in which a type of extreme weather is likely to decrease, rather than increase." Compo, G.P., et al., 2011. The Twentieth Century Reanalysis Project. Quarterly Journal of Royal Meteorological Society, 137, 1-28. "It is anticipated that the 20CR dataset will be a valuable resource to the climate research community for both model validations and diagnostic studies. Some surprising results are already evident. For instance, the long-term trends of indices representing the North Atlantic Oscillation, the tropical Pacific Walker Circulation, and the Pacific–North American pattern are weak or non-existent over the full period of record. The long-term trends of zonally averaged precipitation minus evaporation also differ in character from those in climate model simulations of the twentieth century." (UNITED STATES OF AMERICA)	Done
1349	3	49	11	49	12	In this sentence, agreement is described as "high" and evidence as "robust." Since the terms "high" and "robust" are calibrated uncertainty language (per the AR5 Guidance Note on Treatment of Uncertainties), they should be italicized. (IPCC WGII TSU)	Sentence removed
1350	3	49	12	49	12	Perhaps better to use "confidence" expression than one of "robust". (SWEDEN)	Noted and changed
1351	3	49	17	49	17	"..medium degree of agreement' would be better as: '..medium level of agreement'. (Stocker, Thomas, IPCC WGI TSU)	Done
1352	3	49	17	49	17	In this line, agreement is described as "medium." Assuming calibrated uncertainty language is intended (per the AR5 Guidance Note on Treatment of Uncertainties), "medium" should be italicized. (IPCC WGII TSU)	In the same paragraph we do note that regional changes may be substantial and IPCC AR4 simulations show some regions with medium degree of agreement (like the Atlantic-European region). However, we feel that there is still uncertainties related to the lack of a well resolved stratosphere, uncertainties in analysis techniques, choice of physical quantities and different atmospheric vertical levels to represent cyclone activity to conclude that there is medium confidence in general for the regional projections.
1353	3	49	19	49	20	I disagree with the statement about low confidence (seemingly) assigned to all regions. Several (multi-model) studies point at an increase of Atlantic-European storminess. See 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 Also note that there is no real lack of consensus on projected extreme wind speed changes for this region. (Ulbrich, Uwe, Freie Universitaet Berlin)	This structure was decided after ZOD, and cannot be changed now

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1354	3	49	23	55	18	While I like the way that the situations in various regions are presented distinctly, I think it is unfortunate to separate what is happening in each region in the past and projected for the future. I think that an integrated treatment would be most useful. (MacCracken, Michael, Climate Institute)	This impression should not be given. Hence we add a clarifying statement: "The parameterization of potential evapotranspiration as empirically dependent on air temperature in PSDI calculations can lead to misleading results when temperature changes are not associated with respective changes in evaporative demand."
1355	3	49	23	55	18	The drought section posits excess evaporative demand (not consistently named in the report, but commonly known as potential evapotranspiration, Ep, in the scientific literature) as a factor in drought, but is uncritical of parameterizations for Ep that rely largely on empirical relations to temperature. By far the most widely used example is Thornthwaite's Ep, integral to the PDSI (self-calibrated or not); it is also the foundation of the SPEI (Vicente-Serrano et al 2010). References in the report to PDSI and SPEI and to studies that used them (notably AR4/Trenberth 2007; and the Burke et al 2006 attribution analysis) should be re-examined. (This comment does not apply when Penman-Monteith Ep is used in PDSI.) The report (e.g., page 53, lines 6-8) may further encourage such temperature-causes-evaporation thinking. Indeed, the summary paragraph of Box 3.2 begins "In summary, drought indices often integrate temperature…" even though the word "temperature" did not appear anywhere else in the Box. But energy, not temperature, drives evapotranspiration. In assessing the literature, it is imperative that the serious shortcoming of emprical temperature-based methods be considered and that the affected studies be discounted accordingly. This is a first-order issue, because (page 51, line 40) the recent-decades PDSI trends are governed by trend of temperature, not precipitation. Relevant references on the problem with temperature-based Ep models are *** Burke et al. (2006), cited in the report (which nevertheless uses PDSI for historical analysis); *** Milly, P. C. D., Krista A. Dunne, 2011: On the Hydrologic Adjustment of Climate-Model Projections: The Potential Pitfall of Potential Evapotranspiration. Earth Interact., 15, 1–14. doi: 10.1175/2010EI363.1; ***Shaw, S. B. and Riha, S. J. , Assessing temperature-based PET equations under a changing climate in temperate, deciduous forests. Hydrological Processes, doi: 10.1002/hyp.7913, http://onlinelibrary.wiley.com/doi/10.1002/hyp.7913/abstract (UNITED STATES OF AMERICA)	There is no space for these more "subtle" and detailed issues. The limitation of models is mentioned in the report.
1356	3	49	23	55	18	The drought section makes reference to stand-alone simulations of soil moisture (Sheffield and Wood, 2008a) and streamflow (Hirabayashi et al., 2008b; Feyen and Dankers, 2009) as evidence for/against drought trends (historical or projected). Mention needs to be made of (1) the subtle but possibly critical absence of atmospheric feedbacks in such simulations, as well as (2) the lack of "validation" of the (effective) Ep trends in such simulations, wherein radiation, it appears, may have been parameterized by empirical relations from historical data. In a changing climate, it is critical to assess the stationarity of such empirical relations. This is an issue analogous to the use of Thornthwaite's Ep in the PDSI. Like PDSI, these simulations will get the precipitation forcing of drought right, but will they get the energy forcing right? (UNITED STATES OF AMERICA)	This does not contradict our more general assessment. Relevant for a concrete drought are the actual water balances and not statistical moments. Pre AR4 citattion if not crucial are to be avoided at this stage.
1357	3	49	23	55	18	If it is necessary to pool met, ag/sm, and hydro droughts in the assessment, rather than to assess them separately, a more prominent acknowledgment of this strategy and its limitations should be made. A good case can be made for that strategy, because changes in drought characteristics for the coming century (defined with respect to a fixed historical climate) appear to be related mainly to the big regional shifts, positive and negative, in the long-term MEAN water and energy balances, and not as much to changing variability. This is evident from review of drought graphics in various papers. Indeed, it surprising that the effect of a change in observed/observable components of the mean water balance (precipitation, runoff/streamflow) on drought is touched upon only occasionally in the drought section (page 53, lines 6-8; and references to Orlowsky and Seneviratne 2011 submitted, which, in contrast to most other cited literature, apparently does not address extremes and should be discussed separately), but then featured in the SPM figure. Chapter 3 should put forth mean hydrologic changes as a major factor in the detection, attribution, and projection of drought under a changing climate; global patterns of mean change in precipitation and runoff were major aspects of AR4 WGI report, and the regional feature most common to the reported drought projections are explicable largely by changes in the mean water fluxes and availability; Milly et al. 2005 might be a relevant citation in this regard. (Milly, P.C.D., Dunne, K.A., and Vecchia, A.V., 2005, Global pattern of trends in streamflow and water availability in a changing climate: Nature, v. 438, no. 7066, p. 347-350.) (UNITED STATES OF AMERICA)	Disagree; it is defined as meteorological drought and as such valuable. Space is too limited for an in-depth discussion. What is relevant for impacts in different sectors should not a priori be defined in chapter 3.
1358	3	49	23	55	18	The drought section needs to focus on measures of drought that are directly relevant to impacts (soil moisture, groundwater, streamflow), to the exclusion of precipitation, because the latter leaves out trends in energy balance and Ep (unrelated to P-drought feedbacks). In particular, SPI and especially CDD (Alexander et al 2006) do not seem useful here, and could well create artificial "uncertainty" in the assessment. Perhaps they could be put in the section on precipitation extremes, as a highly relevant causative factor for droughts. (UNITED STATES OF AMERICA)	We refer to the Table where dryness appears in the text first. Dryness is seen as a more general term. Cf. Also comment 1357

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1359	3	49	23	55	18	The introduction of "dryness" into the drought section was rather subtle and escaped this reader completely until he did a search after reading the related table. "Dryness" is only vaguely defined by the statement "In this chapter we often use the term "dryness" instead of "drought" as a more general qualifier." Subsequent usage of the word in the text seems to imply a downward trend in soil moisture, i.e., a change in the mean. The issues of means and extremes needs to be more cleanly articulated and separated in this report. Changes in the mean are suggestive of changes in extremes, but they are not the same, nor is one a generalization of the other. (UNITED STATES OF AMERICA)	The shift of climate zones, as affecting many elements discussed in this chapter, is now discussed at the beginning (Section 3.1).
1360	3	49	24	49	24	It would be useful here to have a summary of the changes in the character of the climate here--perhaps a shift of the climate zones. (MacCracken, Michael, Climate Institute)	We mention that already "not unprecedented", unclear what other studies are meant
1361	3	49	25	0	0	The increases of droughts since 1970 have precedents (result post AR4) in the past centuries. Large incertitude remains in the trends at global scale. Some studies can give more affirmative conclusions on certain régions or basins. (BOURRELIER, PAUL-HENRI, AFPCN)	Due to space limitations we cannot include this reference
1362	3	49	25	0	0	This section could include consideration of Frederiksen et al (2011) 'Changes and Projections in Australian Winter Rainfall and Circulation: Anthropogenic Forcing and Internal Variability', The International Journal of Climate Change: Impacts and Responses, Vol 2 no. 3 (AUSTRALIA)	This comment remains unclear.
1363	3	49	25	0	0	Global observed (since 1950) and projected (to 2100) trend in extreme event type: Observed: Medium confidence of more intense and longer droughts in some regions of the world but evidence of the opposite trend in others. Projected: Low to medium confidence in projected increases of duration and intensity of hydrological drought. (World Food Programme (WFP))	see comment 1363
1364	3	49	25	0	0	Observed (since 1950) and projected (to 2100) trend in extreme event type in example region: Observed: Low to medium confidence of more intense and longer droughts due to inter-annual variability in the Sahel. Projected: Low to medium confidence in projected increases of duration and intensity of hydrological drought. (World Food Programme (WFP))	This is consistent with our assessment (we don't assign high confidence) but such a broad statement is not needed and confidence varies between regions also
1365	3	49	25	0	0	Observed and projected trend in extreme event type at scale of risk management in example region: Definitional problems and lack of data due to deterioration of weather stations do not allow for high confidence levels in observation of drought changes. Moreover, the inability of models to include all the factors that likely influence droughts preclude stronger confidence than medium in projections. (World Food Programme (WFP))	The attribution section is clearly separated from the observed changes section. Introduced subsection headings to clarify.
1366	3	49	25	55	18	There is a worthy attempt at defined the observed changes in drought in various places in this section, There is also an attempt at defining future changes. The text needs to be clearer that detection of drought changes in the 20th century is not the same as attribution. These observed changes may or may not be related to the future changes (i.e forced by GHG or aerosol changes). (UNITED STATES OF AMERICA)	Disagree: pre-conditioning can be caused by low earlier precipitation (outside an averaging window) but also by extraction of water by humans (e.g. declining ground-water levels), or vegetation, or by irrigation
1367	3	49	27	49	31	It seems confusing to speak of pre-conditioning in this way, and we suggest deletion. It has already been noted that drought is related to precipitation deficit on a time scale much longer than a day. Why don't we conceptually include the "preconditioning" within the time averaging? Apparently the authors are drawing a line somewhere in temporal dimension. Where is that line, and why is it used? Perhaps it has something to do with seasonality? Anyway, this is not made clear. The preconditiong concept might make more sense for floods, where two distinct time scales are involved, but it doesn't help me conceptualize drought changes. Furthermore, we don't see that this preconditioning concept is used later at all. (UNITED STATES OF AMERICA)	Indeed this general definition leaves open the reference period because this is not standardized between studies.
1368	3	49	27	49	31	"Drought is generally 'a period of abnormally dry weather…'" Then what is normal? Drought depends on the definition of the normal time period, and this fact is not well communicated. When climate changes, normal changes. One end-member approach to definition is to anchor "normal" in the past. Another is to make it a moving 30-year window, ending or even centered on the time of the event. The defintion of an impact-relevant "normal" will be somewhere in between these end members with the time lag depending on the time constants of the affected players and systems. This report is based upon the definition that generally results in the biggest changes in droughts (and other extremes), and, consequently, in risks giving a biased viewpoint to an audience concerned with impacts. At the very least, summary statements about changes in extremes must reference the "normal" period for the statement, because "normal" is part of the definition. The poor understanding of changes in extremes held in the general public, and its resultant correlation with personal ideology, can be traced partly to our collective failure to communicate this kind of information precisely and clearly. (UNITED STATES OF AMERICA)	We changed this to "by enhanced radiation, vapor pressure deficit or wind speed" (the main physical drivers of evaporative demand)
1369	3	49	27	49	31	This sentence uses "temperature or radiation" to describe potential evaporation, I think. Reference to temperature tends to perpetuate the erroneous concept that increases in temperature cause increases in evaporation. (UNITED STATES OF AMERICA)	We remove the specific references for the general statements and refer to Box 3.3 now.
1370	3	49	27	49	32	The opening sentences of this paragraph are very broken up--need to have a clear message in first sentence, not littered with lots of extra information. I am surprised, for example, that references are included for some pretty general ideas and notions--specific studies on such ideas can be included in later sentences. (MacCracken, Michael, Climate Institute)	We refer to Box 3.3 now, following comment 1370
1371	3	49	30	49	31	add references for the "preconditioning" (Stocker, Thomas, IPCC WGI TSU)	Space is too limited for citing pre AR4 textbook.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1372	3	49	31	49	31	Section 3.5.1 The textbook: L.M. Tallaksen & H.A.J. van Lanen (Eds) (2004), Hydrological Drought Processes and Estimation Methods for Streamflow and Groundwater. Developments in Water Sciences 48. Elsevier Science Publisher, the Netherlands, pp. 579 provides a comprehensive overview of hydroclimatological drought generating processes and estimation methods, including a description of drought indicators, drought in different climates, space-time variability, recent drought events and impact of climate change. We suggest to add a reference to this textbook, e.g. ....can contribute to the emergence of soil moisture and hydrological drought (Tallaksen &van Lanen, 2004; Box 3.2) (NORWAY)	Instead of temperature we now mention radiation, wind speed, vapor pressure deficit (positively related with temperature)
1373	3	49	38	49	38	Should atmospheric moisture (e.g. humidity) be mentioned as a driver of evaporative demand? This has also been shown to be important. Studies that show this include: Donohue, R. J., T. R. McVicar, and M. L. Roderick (2010), Assessing the ability of potential evaporation formulations to capture the dynamics in evaporative demand within a changing climate, J. Hydrol., doi:10.1016/j.jhydrol.2010.03.020. Monteith, J. L. (1965), Evaporation and environment, Academic Press Inc., New York, U.S.A. Morton, F. I. (1983), Operational estimates of areal evapotranspiration and their significance to the science and practice of hydrology, J. Hydrol., 66, 1-76. Penman, H. L. (1948), Natural evaporation from open water, bare soil, and grass, paper presented at Proc. Roy. Soc., London. (AUSTRALIA)	ok
1374	3	49	45	49	45	Need to spell out Palmer Drought Severity Index and refer to Box 3.2 (Stocker, Thomas, IPCC WGI TSU)	ok, "however" removed for this logical implication
1375	3	49	45	49	46	The phrasing of this section intimates that AR4 suggested that recent (post 1970) droughts were unprecedented. If that was the case, AR4 should be quoted for clarity - if not the 'However' should be removed. (UNITED KINGDOM OF GREAT BRITAIN AND NORTHERN IRELAND)	This is the assessment of AR4, which we repeat as a baseline here. Do not understand what reviewer wants done.
1376	3	49	45	49	52	I don't understand the message of this paragraph. It says that the value of a particular metric (change in area with PDSI below -3) mentioned in the first sentence is not unprecedented, but it does not support that statement. It merely makes qualitative reference to the fact that big droughts have occurred in the past. Furthermore, isn't this touching on attribution, which is to be brought up later, after box 3.2? (UNITED STATES OF AMERICA)	Reject. Too many papers cited already.
1377	3	49	45	49	52	It seems appropriate to consider citing the Fawcett et al. (2011) paper on megadroughts in the southwestern United States (Nature 470: 518-521). (IPCC WGII TSU)	This is already in AR4
1378	3	49	46	49	47	Please cite references in relation to these mega droughts in Europe, North America and Australia. (Stocker, Thomas, IPCC WGI TSU)	We do not see where we imply this.
1379	3	49	46	49	47	Just because megadroughts have occurred in some locations in the past doesn't mean current global trends are consistent with internal variability. Proper synthesis is needed to reach this conclusion. Is the variability in droughts in models wrong? (CANADA)	This detailed link is not needed in the section (space limitation)
1380	3	49	47	0	0	A modeling study of megadroughts has linked them (in North America and South Asia) to decadal variability of tropical Pacific SSTs: Meehl, G. A., and A. Hu, 2006: Megadroughts in the Indian monsoon region and southwest North America and a mechanism for associated multi-decadal Pacific sea surface temperature anomalies. Journal of Climate, 19, 1605–1623. (UNITED STATES OF AMERICA)	Deleted to avoid the potential confusion (was included to highlight compound events)
1381	3	49	50	49	51	What is the point of mentioning very warm air temperatures here? Is it empirical support for the "temperature-drives-evaporation" concept? More likely the warmth is a consequence of the absence of evaporative cooling and of anomalous incoming solar radiation. I suggest deletion from the drought section. Perhaps this and some related remarks belong in the heatwave section of the report. (UNITED STATES OF AMERICA)	Droughts are longer in time-scale, hence this reasoning of "unprecedentedness" is more appropriate with drought
1382	3	49	52	0	0	The question whether events are unprecedented also applies to other variables, e.g. recent extreme precipitation amounts or recent temperature extremes. These extremes at a particular location may have been experienced before too. This rationelle was not noted in the sections where these variables were described, because the point is not whether individual events are unprecedented, but whether there is a shift in probabilities. (Klein Tank, Albert, KNMI)	The shift of climate zones, as affecting many elements discussed in this chapter, is now discussed at the beginning (Section 3.1).
1383	3	49	53	49	53	It seems to me that there needs to be a discussion about the relationship between increased drought and climate shifts. Shifts in climate zones will lead to some regions becoming drier and drier. Calling such changes drought is rather misleading--the Sahara Desert is not having a drought--that is its climate. A similar situation seems likely to be occuurring in southern Australia and southwestern North America--the regions are being said to be in drought. That may well be unrealistically optimistic thinking--what is happening is a climate shift to a new state where what will be unusual is the wet year, not the drought year. This could be discussed in an opening paragraph on the shifting of climatic zones, etc. (MacCracken, Michael, Climate Institute)	Noted. Reason for including box on drought comes from complexity of drought and its definition. Definitional issues for tropical cyclones or heatwave-related quantities do not appear as difficult. A short introductory paragraph was added at the beginning of this box to highlight the motivation for providing more background information on this specific extreme.
1384	3	49	55	0	0	Box 3.2: This box is rather technical and includes partly textbook-style material. It would be good to shorten and focus more on what is relevant in terms of the assessment provided in the main text. As it currently stands, it's not clear from reading the box why drought gets special treatment. Metrics for tropical cyclones, or heat-wave related quantities could have been equally valid. (Stocker, Thomas, IPCC WGI TSU)	Reject. This is a new definition, and this box provides the background for the notions introduced in the definition.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1385	3	49	60	50	5	You could probably refer to the Glossary instead of repeating the Drought definition here? (Stocker, Thomas, IPCC WGI TSU)	Reject. Deserts are not in hydrological imbalance. The text additionally notes "Drought should not be confounded with aridity, which describes the general characteristic of an arid climate". In the revisions a parenthesis was added to note that deserts are examples of arid climate.
1386	3	49	61	49	62	By this definition, it seems that deserts are in drought, instead of this being their normal climate. (MacCracken, Michael, Climate Institute)	No. This refers to ACTUAL evapotranspiration excesses. Text has been clarified.
1387	3	50	2	50	3	Does this mean to refer to POTENTIAL evapotranspiration excesses? Is this documented to be a STRONG effect? I.e., does variance in potential ET explain a substantial part of the variance of water storage? Certainly there is a feedback, but does it really affect storage all that much, especially since ET is usually water-supply-limited in these situations? I suggest deletion. (UNITED STATES OF AMERICA)	No, definition is by length, see text.
1388	3	50	4	50	5	Is there any consideration of spatial scale when it comes to defining 'megadrought'? (Stocker, Thomas, IPCC WGI TSU)	Socio-economic drought does not belong to the scope of our chapter, but to chapter 4. But term "socio-economic drought" is indeed an important notion and is now mentioned in relation to water scarcity (this sentence refers the reader to chapter 4).
1389	3	50	7	50	15	In many scientific texts, droughts are categorized into four types that 4th type i.e., socio-economic drought can be very important to the public. (IRAN, ISLAMIC REPUBLIC OF)	Agreed. Corrected.
1390	3	50	8	50	9	Insert "a" before "deficit" (twice - once on each line). (Zwiers, Francis, Environment Canada)	Reject. Can only add a handful references at this stage. This reference does not seem critical to the argumentation, as the already cited publication by Heim Jr 2002 is very well established and comprehensive.
1391	3	50	11	50	11	Box 3.2 Add reference (e.g. Heim Jr, 2002; Tallaksen & van Lanen, 2004) (NORWAY)	Add "through soil mechanical processes".
1392	3	50	14	50	14	Suggest explaining how soil moisture deficits affect building infrastructure since it is not obvious (although presumably it is through loss of soil stability). (CANADA)	Agreed. Was changed as suggested.
1393	3	50	17	50	17	The term "overuse" seems unnecessarily judgmental, and we would suggest changing it simply to "use." (UNITED STATES OF AMERICA)	Sentence was rephrased and effect on evapotranspiration was also mentioned. No mention of direction of feedback (positive vs negative) in revised version given lack of literature on this topic.
1394	3	50	18	50	19	Avoid this speculation, which can be argued the other way, too. Water use for irrigation might also provide a negative feedback. Water is more available to the atmosphere when it is put onto agricultural fields than when it is deep in the ground. (UNITED STATES OF AMERICA)	Reject. Can only add a handful references at this stage. Reference does not seem critical to argumentation.
1395	3	50	22	50	22	Box 3.2 Add reference (e.g. Tallaksen & van Lanen, 2004; Dai, 2011) (NORWAY)	Actual or potential evapo(transpi)ration is commmonly considered in drought indicators as indicated in the following paragraph. Also, several studies have highlighted that changes in evapotranspiration play a non-negligible role in projected changes in soil moisture drought. However, it is correct that precipitation is generally found to be the dominant driver. In addition, in very dry conditions, evapotranspiration indeed becomes limited by soil moisture. Three sentences were added to clarify these points.
1396	3	50	29	50	30	What is the evidence that evapotranspiration excess is a main driver of of drought? In fact, during a drought, ET is usually in deficit, due to shortage of available water. Perhaps the statement means to refer to potential ET. But Ep is at most a minor postive feedback on drought, with initiation being the job of precipitation. (UNITED STATES OF AMERICA)	Agreed, text was unclear. Sentence was replaced with two sentences which clarify the respective contribution of precipitation and evapotranspiration to simulated changes in drought. Precipitation is the dominant driver, but changes in evapotranspiration play a non-negligible role.
1397	3	50	30	50	31	We don't understand what this sentence is saying, even within the context of the surrounding text. (UNITED STATES OF AMERICA)	Agreed. Can only add very few references at this stage, but this publication is relevant and important.
1398	3	50	33	50	33	Box 3.2 Suggest to add reference to Fleig, A.K., Tallaksen, L.M., Hisdal, H. & Hannah, D.M. (2011) Regional hydrological drought in north-western Europe: linking a new Regional Drought Area Index with weather types. Hydrological Processes 25, 1163-1179, doi: 10.1002/hyp.7644. where the results show that the hydrological response time (i.e. the time over which Weather Types influence drought development) vary markedly (45-210 days) between regions according to basin storage properties. We suggest to add a reference to this paper, e.g. .....drought forcing (e.g. Begueria et al. 2010; Fleig et al., 2011) (NORWAY)	Reject. The reference is correctly cited and listed. It is Wang A.H. et al. 2009a (see reference list).
1399	3	50	34	50	34	There is no "Wang et al. 2009a" listed in the References. This needs to be added in the References (see my comment #8). (Wang, Xiaolan, Environmen Canada)	All arrows are now in the same color.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1400	3	50	43	0	0	Box 3.2, Figure 1: Is there any meaning associated with the red and black arrows? (Stocker, Thomas, IPCC WGI TSU)	Publication seems too detailed for purpose of text. In addition, inclusion of new material is limited to the minimum.
1401	3	50	43	50	44	I would like to draw your attention to the recent paper by Burke and Brown 2010 which I think is relevant to the discussion of drought indicators here but also to the wider impact/adaptation brief of this report. This paper was in response to requests from users for more relevant drought metrics with which impact and adaptation policies could be developed namely area-severity-frequency curves for an administrative region. The high data demands of these metrics resulted in a "drought generator" trained on RCM data. The paper also presents area-severity-frequency predictions for the UK regions and for the 12-month index used current predictions indicate both greater and lesser degrees of drought are possible in the future. - ie very uncertian. This highlights the challenge of providing relevant drought metrics for adaptation planning. Reference:"Regional drought over the UK and changes in the future Eleanor J. Burke and Simon J. Brown (2010), Journal of Hydrology Volume 394, Issues 3-4, 26 November 2010, Pages 471-485" (Brown, Simon, The Met Office Hadly Centre)	These references are important and will be documented in the main text of Section 3.5.1. However, the purpose of the present section is to address indicators to assess drought in present and future climate. Paleoclimate proxies are not directly relevant in this context. To clarify this point, we replaced the term "proxies" with "indicators", and the text refers to the main text of Section 3.5.1 regarding paleoclimate proxies. Lake sediments were added in the list of paleoclimate proxies.
1402	3	50	47	55	8	Lake sediments also serve as important drought indicators and have yielded important records: Nelson et al., 2011, PNAS; Fawcett et al., 2011, Nature; Shanahan, 2009, Science). These should be profiled and summarized in this section. (UNITED STATES OF AMERICA)	This section is not on methods but rather on indices themselves - we cannot add paragraphs in this last revision.
1403	3	50	47	55	8	A paragraph should be added focusing on paleoclimate results (see lake sediment citations as well as others: e.g., Woodhouse et al., 2010, PNAS and others) (UNITED STATES OF AMERICA)	Brackets were removed.
1404	3	50	49	50	50	Suggest removing the brackets in the sentence that begins "These proxies ...". (Zwiers, Francis, Environment Canada)	Droughts following these definitions are (moderate) extremes. 5% is a commonly used threshold for moderate extremes as noted elsewhere in the text and in the main definition of extreme weather and climate events in the glossary.
1405	3	50	56	50	59	It might be worth noting that by these definitions, extreme conditions (drought or wet conditions combined) occur about 5% of the time. That is, not all droughts are extremes. (Zwiers, Francis, Environment Canada)	"or more" added.
1406	3	50	60	50	60	I suggest to introduce also the time scale of 24 months in the SPI computation, being the time scale that reveals long term condition of drougts. (ITALY)	Agreed. Text was clarified. Indicated link through cloud cover.
1407	3	51	1	51	3	These sentences need some additional explanation to make the meaning clear. The link between 'periods without rain' and higher radiation forcing should be made explicit. (CANADA)	The comment from the reviewer is incorrect for water-rich environments which are also affected by droughts. However, text now notes that this is only true if evapotranspiration is not limited by very dry soil moisture conditions.
1408	3	51	1	51	3	Contrary to this statement, periods without rain are bound to have NEGATIVE ET anomalies, unless they are in a very water-rich environment. Generally, increase in energy availability is outweighed by the decrease in water availability. (UNITED STATES OF AMERICA)	Agreed that this sentence may confuse the reader. Removed since not essential to argumentation.
1409	3	51	3	51	5	This remark is hard to interpret. (UNITED STATES OF AMERICA)	We mean actual ET. Actual ET is a forcing for the surface water balance (from the point of view of the soil moisture reservoir). The notion of driver depends on the considered entity. This will be clarified in the text.
1410	3	51	7	51	7	Here we think the authors mean potential ET rather than ET itself. In PDSI, the ET is not a forcing; the Thornthwaite PET is the forcing. (UNITED STATES OF AMERICA)	yes, that's correct - explained now.
1411	3	51	10	51	11	It would be helpful to explain briefly what the PPEA and SPEI are, rather than just spell out the acronyms. My quick reading from the original sources is that PPEA is an accumulated difference between precipitation and an estimate of PET, and SPEI is a variation on PDSI-Thornthwaite in which a more flexible time scale is introduced. (UNITED STATES OF AMERICA)	Agreed. Was changed as suggested.
1412	3	51	15	51	15	Insert "land surface" ahead of "model underlying the PDSI.... ". (Zwiers, Francis, Environment Canada)	Reject. See answer to #1399.
1413	3	51	21	51	22	There is no "Wang et al. 2009a" listed in the References. This needs to be added in the References (see my comment #8). (Wang, Xiaolan, Environmen Canada)	References have been moved to first sentence of this paragraph and last sentence has been removed.
1414	3	51	21	51	22	Last sentence of this paragraph could be deleted, listing of references is not needed (Stocker, Thomas, IPCC WGI TSU)	Reject. Can only add a few references. Suggested additional reference is not critical to argumentation (textbook).
1415	3	51	24	51	24	Box 3.2 Add reference (e.g. Heim Jr, 2002; Tallaksen & van Lanen, 2004; Vidal et al. 2010; Dai, 2011) (NORWAY)	Agreed. Was added.
1416	3	51	26	51	26	It would also be appropriate to cite Burke et al (2006) here. (Zwiers, Francis, Environment Canada)	Reject. Publication is only for a very small region.
1417	3	51	26	51	26	After reference Dai, 2011 add (Koutroulis et al., 2010). REFERENCE: Koutroulis A. G., Vrochidou A., Tsanis I.K., 2010: Spatial and temporal characteristics of droughts for the island of Crete. Journal of Hydrometeorology, (in press).DOI: 10.1175/2010JHM1252.1 (GREECE)	Agreed. Summary statement was bolded.
1418	3	51	30	51	33	Emphasize in bold the summary of this Box as per the other variables in the text (ITALY)	As highlighted in the text, it is not easy to define what is the most "correct" index. But the validity of respective indices for the addressed questions needs obviously to be evaluated. Text was revised to include this point.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1419	3	51	32	51	33	We disagree that the solution to possibility of disagreement is to compute an answer many ways and (seemingly implied) to make confidence proportional to agreement among methods. Rather, we think that the solution is to figure out the sources of the disagreement and to select indices on the basis of their impact-relevance and physical credibility. (UNITED STATES OF AMERICA)	Structural changes are impossible at this stage (no more review)
1420	3	51	38	51	53	It might be good to distinguish the meteorological or potential drought from the hydrological and agricultural drought. If, as suggested above, this chapter is divided into two parts,treating observed and projected changes and impacts respectively, the problem with drought can also be solved. (CHINA)	ok
1421	3	51	39	51	39	Replace "PDSI model" with "PDSI proxy" to be consistent with other parts of the text. (Zwiers, Francis, Environment Canada)	ok
1422	3	51	44	51	44	trends --> trend (Stocker, Thomas, IPCC WGI TSU)	changed to "moistening trend"
1423	3	51	44	51	44	The term "wetting trends" seems rather unconventional. (MacCracken, Michael, Climate Institute)	Change to moistening trend. Climate zone shift are discussed now in 3.1
1424	3	51	55	51	57	Is there any evidence of meridional structure in the tendency towards moistening, with a stronger tendency in the north than in the south? Also, could one say moistening, instead of less dryness? (Zwiers, Francis, Environment Canada)	The shift of climate zones, as affecting many elements discussed in this chapter, is now discussed at the beginning (Section 3.1).
1425	3	51	55	51	57	I like the approach of identifying the region of the changes right at the start of the paragraph. However, this sentence really seems to fail to recognize the shifts of climatic zones that is occurring, making it sort of seem like we are at equilibrium and this or that change is occurring here or there. The overall global circulation is a coherent system and this needs to be explained. The tendencies to dryness are really shifts in circulation occurring. (MacCracken, Michael, Climate Institute)	this is for Chapter 4
1426	3	51	55	55	18	Only changes of drought are described,while the impacts are not emphasized. Impacts of drought relates not only the water/moisture, but also the reduction of crop production, shortage of drinking water etc. (CHINA)	ok deleted.
1427	3	51	59	51	59	The "Wang et al. 2009c" listed in the References has nothing to do with this topic and thus should not be cited here. By comparing the First and Second Order Drafts, I think this is meant to cite the "Wang et al. 2006c: Control of dust emmissions ..." that was listed in the First Order Draft, but deleted by mistake in this second order draft? It whould now be added (see my commnet #11 and #13). (Wang, Xiaolan, Environmen Canada)	we add: "with sub-regional exceptions"
1428	3	51	62	51	63	“Recent regional trends towards more severe drought conditions were identified over southern and western Canada, Alaska and Mexico.” St. George (2007, Streamflow in the Winnipeg River Basin, Canada: Trends, Extremes and Climate Linkages. Journal of Hydrology, 332, pp., 396-411) reported and increasing trend in stream flow in the Winnipeg of south central Canada and noted that “The results of this study show that hydrological trends in the Winnipeg River basin during the 20th century are different from those observed on other Canadian rivers, and imply that projections made for the rivers in the Canadian prairies may not be valid for this watershed.” Therefore, the situation in southern Canada is more is more complex than the statement above implies. (UNITED STATES OF AMERICA)	ok: Dai 2011 added.
1429	3	51	63	51	63	The sentence requires supporting references. (CANADA)	see comment 1428, 1429
1430	3	51	63	51	63	Could "southern and western Canada" be made more specific?. This is a large region. This sentence also requires supporting references. (CANADA)	Dryness is used as general term as explain in Box 3.3
1431	3	52	2	52	2	It really seems strange to say there are" increase in dryness" rather than saying that the evaporation is going up and soil mositure is, as a result, decreased. (MacCracken, Michael, Climate Institute)	The regional differentiation is being done already.
1432	3	52	2	52	8	Doing averages and totals across Europe does not make much sense to me--different types of changes are expected in southern Europe versus northern, and summing the changes and talking about the region as a whole seems to me to not be very useful. I'd also note that it should be mentioned that really extreme conditions can occur on smaller scales than are often discussed, as happened in 2010 in Russia. (MacCracken, Michael, Climate Institute)	This is only a very specific region, and cannot be included here
1433	3	52	2	52	22	In this section on European drought trends, you should include a discussion of Li et al. (2007, Evaluation of Intergovernmental Panel on Climate Change Fourth Assessment soil moisture simulations for the second half of the twentieth century, Journal of Geophysical Research, 112, D06106, doi:10.1029/2006JD007455). Li et al. (2007) noted the poor performance of climate models in simulating trends in surface moisture (towards increasing wetness) in the region of Russian and the Ukraine: “To explore the summer drying issue for the second half of the 20th century, we analyzed the linear trend of soil moisture for Ukraine and Russia. Observations from both regions show increases in summer for the period from 1958–1999 that were larger than most trends in the model simulations. Only two out of 25 model realizations show trends comparable to those of observations. These two trends, however, are due to internal model variability rather than a result of external forcing.” (UNITED STATES OF AMERICA)	Still our statement is correct from Stahl et al. (2010)
1434	3	52	18	52	19	Trends in LOW streamflows are very sensitive to non-climatic human influences on the landscape and along the stream, because they represent very small fluxes of water, and are therefore unreliable indicators of climate change. (UNITED STATES OF AMERICA)	Citation cannot be added for space limitations. Good that it is consistent

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1435	3	52	19	52	19	Section 3.5.1 In the paper Wilson, D., Hisdal, H., Lawrence, D. (2010) Has streamflow changed in the Nordic countries? – Recent trends and comparisons to hydrological projections. Journal of Hydrology, 394, 334-346, 151 streamflow records from the five Nordic countries are analysed for the periods 1920-2005, 1941-2005, 1961-200. For all periods it is found that trends towards increasing streamflows dominate the annual values and winter and spring seasons. There is a clear signal towards earlier spring snowmelt floods and a tendency towards more severe summer droughts in southern and eastern Norway. The streamflow trends result from both changes in temperature and precipitation, but the temperature induced signal is stronger than the precipitation influences. This is evident since the observed trends in winter and spring, are greater than the annual trend. A comparison with available streamflow projections showed that changes expected because of increased temperatures were reflected in the historical trends (e.g. earlier snowmelt floods), whereas changes anticipated due to increases in precipitation are not (e.g. increased streamflow in the autumn and increased rain flood magnitudes). The conclusions in this paper correspond well with the conclusions of chapter 3, and referring to the paper would strengthen the scientific basis of the report. The following addition is suggested: .....secondary low flows in summer. In a study by (Wilson et al., 2011) no change in summer streamflow droughts were found for the Nordic region except for the south-eastern part of Norway were a tendency towards more severe summer droughts were found. (NORWAY)	Perhaps but this is a general point - don't need to add all heatwaves.
1436	3	52	19	52	22	Is the European 2010 heatwave relevant here also? (Stocker, Thomas, IPCC WGI TSU)	This is not an intermediate level, but characterizes sub-regional variablity in confidence (clarified now in the text)
1437	3	52	24	52	24	Does "low to medium confidence" imply that confidence is being assessed on a finer than 5-point scale by inserting a level between low and medium? If so, how would one describe the level between low and medium? I think it would be best if the authors did not sit on the fence between low and medium. The same comment applies to line 41 of this page. (Zwiers, Francis, Environment Canada)	agreed.
1438	3	52	24	52	24	It would be best to unitalicize "to." (IPCC WGII TSU)	Yes, this additional single event does not change the assessement. But it is mentioned now and cited.
1439	3	52	24	52	27	Considering drought of 2010 is this statement still robust? Lewis, S.L., P.M. Brando, O.L.Phillips, G.M.F. van der Heijden, Daniel Nepstad, 2010; 'The 2010 Amazon Drought', Science, 331, pg 554 (UNITED KINGDOM OF GREAT BRITAIN AND NORTHERN IRELAND)	see comment 1437
1440	3	52	24	52	31	Qualitatively all of the information presented seems to point in the same direction, so perhaps medium confidence would be appropriate? (Zwiers, Francis, Environment Canada)	ok, it is a combination of all these aspects.
1441	3	52	24	52	41	Overall, it is uncertain how readers should interpret assessments of "trends" in a region. If a collection of trends in a region is assessed to have low o confidence, for example, does this mean that the data are poor, that methods are questionable, or that it is not known whether the trends that are calculated from available data are representative of the entire region. Some additional discussion concerning the basis of such assessments would be useful. (Zwiers, Francis, Environment Canada)	Insufficient pages available to do this.
1442	3	52	24	52	41	I think it would help in these paragarphs (and others) about the general mechanisms involved and the general types of climatic changes being expected--as context for the specific discussions. "Asia and Africa" is a huge area, and different types of changes can be expected--explaining mechanisms would help explain why different changes are occurring in different places. (MacCracken, Michael, Climate Institute)	cf. 1439
1443	3	52	25	52	27	A new paper on the 2010 Amazon drought and its impact should be added: Lewis et al., 2011, Science) (UNITED STATES OF AMERICA)	But it has helped interpreting the precip estimates which are longer (clarified now)
1444	3	52	25	52	27	Contrary to the implication, GRACE has not been flying for 100 years. (UNITED STATES OF AMERICA)	medium added as in Table 3.2
1445	3	52	35	52	35	The confidence statement is made for Asia as a whole, but what is the confidence for the statement about E Asia specifically. I imagine this will be of greater interest now than when the authors wrote this section, given the issues with winter snowfall this year in China (Lobell, David, Stanford University)	thanks
1446	3	52	36	52	37	Good mention of the relation of Sahel and interannual variability. (Mata, Luis Jose , IMF)	we split the paragraph, and Africa is mentioned explicitely at the end now
1447	3	52	40	52	41	This summary for Africa could be misread as a summary for the whole paragraph, which talks about both Africa and Asia. (Zwiers, Francis, Environment Canada)	Gallant and Karoly used for Table 3.2.
1448	3	52	43	0	0	Table 3.2 (page 119, column 5, Dryness: N. Australia and S. Australia/NZ): For the box for N. Australia, change to "Decrease in dryness (SM, PDSI) in northwest during 20th Century (Sheffield and Wood, 2008a; Gallant and Karoly, 2010; Dai, 2011)" or similar. For the S. Australia box - include Gallant and Karoly (2010). However, they show that the increases in the fractional area of southern Australia experiencing dryness (from soil moisture) since the mid-20th century are only small and not signficiant. There has been little change from 1911–2008. Full reference: Gallant, A. and Karoly, D., 2010. A combined climate extremes index for the Australian region. Journal of Climate, 23, 6153-6165, DOI: 10.1175/2010JCLI3791.1 (AUSTRALIA)	ok
1449	3	52	43	52	43	Delete "only" (Stocker, Thomas, IPCC WGI TSU)	See #1448.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1450	3	52	43	52	45	Gallant and Karoly (2010) study should be included in this discussion. They looked at the area of Australia where there has been a frequency of months above/below the 90th/10th percentile of monthly soil moisture. From 1911–2008 there has been an increase in the proportion of Australia experiencing extreme wetter soil moisture conditions of 2.2% per decade. This mainly stemmed from increases in northwest Australia. The proportion of northwest Australia experiencing extreme moisture surplus conditions has increased by 3.9% per decade from 1911–2008. There have been few changes in the area of southern Australia experiencing extreme dryness calculated from soil moisture since 1911, but decreases since the mid-1990s. Full reference: Gallant, A. and Karoly, D., 2010. A combined climate extremes index for the Australian region. Journal of Climate, 23, 6153-6165, DOI: 10.1175/2010JCLI3791.1 (AUSTRALIA)	Specific years can't be included for each region due to space limitations.
1451	3	52	43	52	50	2010 was in contrast to what is discussed here. This could perhaps be acknowledged, even if only to remind a possible lay reader that it's only one year. (Global Climate Observing System Steering Committee)	Cant be added due to space limitations
1452	3	52	43	52	50	The widely reported drought in SE Australia, which received attention in the scientific literature in attempts to link it to atmospheric circulation change, should be discussed here, as it seems unusual in the context of long term records. (CANADA)	Can't be added due to restrictions
1453	3	52	43	52	50	Another reference for long-term Australian drought is Bureau of Meteorology 2008, Long-term rainfall deficiencies continue in southern Australia while wet conditions dominate the north, Special Climate Statement 16. (Trewin, Blair, Australian Bureau of Meteorology)	change to "quasi-globally distributed"
1454	3	52	48	52	48	What are "global" eddy flux observations? (Zwiers, Francis, Environment Canada)	ok
1455	3	52	52	52	52	Please cite the relevant AR4 chapter, and also cite the 'one study' referred to here. (Stocker, Thomas, IPCC WGI TSU)	ok
1456	3	52	52	52	52	revise text to "following the assessment [of observed changes provided in] the AR4" (Stocker, Thomas, IPCC WGI TSU)	ok
1457	3	52	52	52	53	If this is true that the AR4 assessment (of drought) was largely based on one study, then this should also be stated where the conclusions from the AR4 are presented on page 49 lines 45-46, along with an assessment of the quality of that study. Also, that study should be referenced at the end of this first sentence on line 52. (CANADA)	Very small scale - cannot include all such regions in a global assessment.
1458	3	52	56	52	56	South-west Western Australia should be included as an area that has experienced more intense and longer droughts. (AUSTRALIA)	It was decided to have the attribution after the assessment of the observations: this is now made clear by including subsections.
1459	3	52	59	52	62	This is a very helpful summary--I would suggest starting the overall discussion with this summary finding. (MacCracken, Michael, Climate Institute)	This is an assessment of the attribution not the overall trends
1460	3	52	60	52	61	There seems to be a contradiction here. On the one hand you say that the AR4 detection and attribution of drought trends was based on multiple lines of evidence, while on line 52, you say that the AR4 assessment of observed drought trends was based largely on one study. Please clarify. (Stocker, Thomas, IPCC WGI TSU)	Do not need extra references that do not add different viewpoints to assessment.
1461	3	53	1	0	0	A modeling study of megadroughts has linked them (in North America and South Asia) to decadal variability of tropical Pacific SSTs: Meehl, G. A., and A. Hu, 2006: Megadroughts in the Indian monsoon region and southwest North America and a mechanism for associated multi-decadal Pacific sea surface temperature anomalies. Journal of Climate, 19, 1605–1623. (UNITED STATES OF AMERICA)	ok, "and model resolution of orographic and other effects" added
1462	3	53	1	53	20	I would think that orograhy should be mentioned as an influence, and there will also be interest in the timing thorugh the year of dry periods, etc. I think that limited model resolution is a contributor to the uncertainty about what is likely to happen. (MacCracken, Michael, Climate Institute)	But they are of the same sign, which is the basis of the statement
1463	3	53	8	53	8	Simulated precipitation trends are smaller than those observed (Xhang et al., 2007). (CANADA)	Deleted reference to single drought but kept reference to Seager at al.
1464	3	53	11	53	14	This statement about the 2005/6 drought implies that this is a caveat on the previous discussion, but this is just one event. Is this really inconsistent with models, accounting for internal variability? As written the text implies that it might be - please clarify. (CANADA)	ok
1465	3	53	14	53	14	"streamflow drought" should be 'hydrological drought' (Stocker, Thomas, IPCC WGI TSU)	Reference added
1466	3	53	15	53	16	Where it states that this result is "still debated" no references are provided. Where is the debate occurring then? If not in the scientific literature then this sentence should be deleted. (CANADA)	ok
1467	3	53	16	53	16	"streamflow drought" should be 'hydrological drought' (Stocker, Thomas, IPCC WGI TSU)	agree but here is no problem. The medium confidence is an additional assessment compared to AR4
1468	3	53	17	53	20	Again, "medium confidence" and "more likely than not" should not be considered to be equivalent. See also my comment concerning page 9, line 13 (3.1.5). (Zwiers, Francis, Environment Canada)	pg 51 is on the observation of change, while pg 53 is on the attribution
1469	3	53	18	53	20	This seems to contradict the statement based on Sheffield and Wood on pg 51, line 40-43. (CANADA)	cf. 1469
1470	3	53	18	53	20	Implicit in this statement is the discounting of previously acknowledged decreases in droughts in some regions. This seems like a regression to the AR4 increase in droughts from page 52, line 60. (UNITED STATES OF AMERICA)	This is not the case! Rather see comment 1468
1471	3	53	19	53	20	To understand that the SREX assessment of 'medium confidence' is not a change from the AR4 assessment, the reader has to remember that 'medium confidence' is considered, by the authors of this report, equivalent to the AR4's 'more likely than not'. Suggest adding this explanation here to the end of line 20. (CANADA)	In the literature different indices are used which address different aspects of drought. It is hard to qualify them as faulty.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1472	3	53	29	53	36	We think this assessment is failing to upweight robust findings about mean fluxes and downweight studies with faulty and/or questionale methods. Differences in time scales and differences in meteorological vs. soil-moisture/agricultural vs. hydrologic droughts are relatively minor distractions in the context of contemporary climate change, because the drought trends appear to be driven, conisitent with a first-order, conservative assumption (albeit with some noted exceptions), by a change in the mean water balance. (UNITED STATES OF AMERICA)	It is rather background information that is implicit in the assessment
1473	3	53	35	53	40	These kinds of findings need to be provided greater visibility. (MacCracken, Michael, Climate Institute)	Reference and discussion added.
1474	3	53	35	53	40	I think the conclusions drawn from Blenkinsop and Fowler 2007b need to be modified in the light of Burke et al 2010. Burke et al 2010 sampled a wider set of modelling uncertainty and show the future change in 3, 6, 12 and 18 month drought and soil moisture drought severity ranging from wetter than present to increased dryness (their fig 8) though the the wetness increase in 3 & 6 month drought being much smaller than the longer periods. They also show the UK area average ensemble mean change in severity is negative and does not include zero for 3 & 6 month drought and only just for the longer periods (thier fig 9). Reference is: An extreme value analysis of UK drought and projections of change in the future Eleanor J. Burke, Richard H.J. Perry and Simon J. Brown Journal of Hydrology Volume 388, Issues 1-2, 25 June 2010, Pages 131-143 (Brown, Simon, The Met Office Hadly Centre)	text revised
1475	3	53	36	53	36	"indicate" --> "indicates" (Zwiers, Francis, Environment Canada)	Fix reference.
1476	3	53	36	53	36	I think this is referencing the wrong Blenkinsop, S., and H.J. Fowler, 2007. The right one, which is not in the reference list, is: "Changes in drought frequency, severity and duration for the British Isles projected by the PRUDENCE regional climate models S. Blenkinsop *, H.J. Fowler doi:10.1016/j.jhydrol.2007.05.003" (Brown, Simon, The Met Office Hadly Centre)	Given the uncertainties of land surface models, in particular with the parameterization of the soil, we disagree with this assessment
1477	3	53	42	53	48	SPI gives the smallest drought increase, not surprisingly, because it unrealistically ignores increases in evaporative demand. PPEA should also be ignored, because it goes too far in the other direction, erroneously equating a unit increase in Ep to a unit decrease in P, when in fact they are not equivalent: the absence of 1 mm of rain is more effective than the addition of 1 mm of Ep, because the latter will not generally remove 1 mm of water from the soil, thanks to water limiteation of actual E. We would argue that the soil moisture (here reflected in the SMA) from the climate models is the best indicator we have for soil-moisture drought. It is based on an internally consistent surface energy balance. All the other metrics are simply offlne attempts to reproduce the climate model soil moisture, and they are all distorted by their failure to do all the physics in the climate model. Incidentally, the patterns of SMA shown by Burke and Brown 2008 are largely consistent with published projections of shifts in mean water balance, suggesting that the distinction between soil-moisture drought and hydrologic drought is not a big deal. (UNITED STATES OF AMERICA)	It is not the same, and on the long-term simply the deeper water will be depleted, while the CO2 effect reduces the losses. Not to mix this up, we do not include the statement
1478	3	53	42	53	48	Even the climate-model soil moisture anomaly (SMA) has problems as an index of water stress for ecosystems and for land-atmosphere fluxes, potentially exaggerating drought stress. As we've pointed out [Milly, P. C. D. 1997, Sensitivity of greenhouse summer dryness to changes in plant rooting characteristics, Geophys. Res. Lett., 24(3), 269–271, doi:10.1029/96GL03968], midlatitude summer dryness results largely from a radiatively forced increase of water and energy fluxes without a proportionate increase in plant rooting depth, which is held fixed in most models. If plants were allowed to adapt in models (and perhaps if they do in nature), the reduction in effective SMA can be largely reversed. This should be included as a speculative biophysical feedback, alongside CO2 fertilization/stomatal response. (UNITED STATES OF AMERICA)	No. Difficult for reader to understand unless we add these few words which help explain the differences.
1479	3	53	45	53	45	Delete "based solely on precipitation" - this is already clear from the explanation of SPI given earlier in the chapter. (Stocker, Thomas, IPCC WGI TSU)	The overall global assessment is still valuable (and better than talking about mean global changes)
1480	3	53	45	53	48	I don't think that the fractional area is a very useful measure--what matters is what islikely to happen where. Or perhaps it might be of interest to have the fraction of some area used for agriculture or of forests, etc., but to take large areas with mixes of land covers and give fractions does not seem very useful to me. (MacCracken, Michael, Climate Institute)	The regional patterns of both of them
1481	3	53	57	53	59	It is not clear in the sentence beginning at the end of line 57 ("These results...are consistent with other studies") which results are being referred to: the results where 2 indices agree or the results where the 2 indices disagree. (CANADA)	it is already mentioned prominently
1482	3	53	61	53	61	The Dai 2011 paper should be addressed because it contains a particularly grim drought forecast and was widely profiled in the press (Wiley Interdisciplinary Reviews: Climate Change Volume 2, Issue 1, pages 45–65, January/February 2011) (UNITED STATES OF AMERICA)	removed
1483	3	53	61	54	7	reference to specific figures in Sheffield and Wood needed? (Stocker, Thomas, IPCC WGI TSU)	ok, as comment 1485
1484	3	54	3	54	3	To reiterate an earlier comment - emissions scenarios should not be characterized as extreme, since none was considered to be more or less likely than any other by SRES. (Zwiers, Francis, Environment Canada)	ok
1485	3	54	3	54	3	"extreme emission scenarios" would be better as: 'high-CO2 emission scenarios' (Stocker, Thomas, IPCC WGI TSU)	too specific, cannot be included
1486	3	54	9	54	14	Hadjinicolaou et al (2010) analyzed data from regional climate model (RCM) simulations and found that the annual number of consecutive dry days shows a statistically significant increase (of 9 days) in Limassol, Cyprus. This reference could be included in table 3.3 (GREECE)	too specific, cannot be included

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1487	3	54	9	54	14	Hadjinicolaou et al (2010) analyzed data from regional climate model (RCM) simulations and found that the annual number of consecutive dry days shows a statistically significant increase (of 9 days) in Limassol, Cyprus. This reference could be included in table 3.3 (Zerefos, Christos, Academy of Athens)	Reference added.
1488	3	54	9	54	22	CCD projections in South America projected with multi-SST ensemble aproach using 20- and 60-km grid versions of an AGCM are found in Kitoh et al. (2011), where uncertainty in CCD is quantified using the multi-SST ensemble. (Nakaegawa, Toshiyuki, Meteorological Research Institute)	too specific, cannot be included
1489	3	54	14	54	14	before "For North America…" you can add "Bias adjusted monthly precipitation estimates from an ensemble of ten regional climate models used to quantify the influence of global warming to drought conditions over the period 2010 – 2100 for the region of Crete Island in eastern Mediterranean (Koutroulis et al., 2010). Projections indicate that extremely prolonged dry conditions may cover 25% of the island during the rest of the 21st century, under A1B SRES emission scenario." REFERENCE: Koutroulis A. G., Vrochidou A., Tsanis I.K., 2010: Spatial and temporal characteristics of droughts for the island of Crete. Journal of Hydrometeorology, (in press).DOI: 10.1175/2010JHM1252.1 Island in eastern Mediterranean (Koutroulis et al., 2010). Projections indicate that extremely prolonged dry conditions may cover 25% of the island during the rest of the 21st century, under A1B SRES emission scenario." REFERENCE: Koutroulis A. G., Vrochidou A., Tsanis I.K., 2010: Spatial and temporal characteristics of droughts for the island of Crete. Journal of Hydrometeorology, (in press).DOI: 10.1175/2010JHM1252.1 (GREECE)	the following sentence explains it with references
1490	3	54	14	54	14	A reference is required for this statement about changes in North America. (CANADA)	Added.
1491	3	54	14	54	15	A change in the climate physically changes many of the factors affecting floods (e.g. precipitation, snow cover, soil moisture content, sea level, glacial lake conditions) and thus may consequently change the characteristics of floods. It should be added vegetation (Barros, Vicente, IPCC WGII TSU)	ok
1492	3	54	17	0	0	Chap 3, page 54, line 17: "...with more frequent multi-year drought in the American southwest". Would be good to add a reference to Cayan et al. 2010 here in addition to the Seager et a. 2007 reference, since Cayan et al. looked at hydrological drought while Seager et al. looked at meteorological drought. Full reference is:Cayan, D. R., T. Das, D. W. Pierce, T. P. Barnett, M. Tyree, and A. Gershunov, 2010: Future dryness in the southwest U.S. and the hydrology of the early 21st century drought. Proceedings of the National Academy of Sciences, 107, (50)21271-21276. (UNITED STATES OF AMERICA)	ok
1493	3	54	25	54	25	"streamflow drought" should be 'hydrological drought' (Stocker, Thomas, IPCC WGI TSU)	no, this concerns different time-slices and also mean changes and extremes are not really comparable
1494	3	54	25	54	25	Since runoff equals precipitation minus evaporation over the long term, does the projection of increased streamflow drought in North America contradict the projection of "increase in precipitation minus evaporation" given on line 13 of P.53? (CANADA)	ok
1495	3	54	30	54	30	"streamflow drought" should be 'hydrological drought' (Stocker, Thomas, IPCC WGI TSU)	caption improved
1496	3	54	32	0	0	Figure 3.10: As per the general comment above, please extend the citation in the caption to something like: [from Orlowsky and Seneviratne (2011), updating the AR4 assessed results of Tebaldi et al. (2006) to include a larger number of GCMs (23) from the CMIP3 ensemble] (Stocker, Thomas, IPCC WGI TSU)	yes, indices derived from modelled data are consistent with indices from observed data.
1497	3	54	39	54	40	The meaning of "satisfactory simulation of drought indices" is not clear. It sounds like a model of a model, with no observations. (UNITED STATES OF AMERICA)	This is only part of the world and one mechanism. We do not have complete understanding
1498	3	54	39	54	50	“lack of complete knowledge” It seems odd that in all the discussion of Mediterannean drought we can’t find a mention of the NAO. We thought Jim Hurrell’s science paper from the 1990s made a good case that the trend towards the high phase of the NAO was at least partly responsible for a long-term drought in Spain if not other parts of the Mediterranean. (UNITED STATES OF AMERICA)	see comment 1360
1499	3	54	39	54	50	The Mediterranean drying is also consistent with fig. 1 of the AR4 SPM, which shows poleward shifts of the midlatitude rain bands. The work of Held and Soden and others shows that this is consistent with the change in P-E expected due to the nonlinearity of the Clasius – Clayperon equation. Lorenz and DeWeaver (JClim 2007) present evidence that the poleward shift of the jet streams and stormtracks in the models also contributes to the shifts in P-E, which is consistent with the impact of the NAO. (UNITED STATES OF AMERICA)	"insufficient knowledge"
1500	3	54	43	54	43	rephrase "lack of complete knowledge" -- would "sufficient knowledge" work? (Stocker, Thomas, IPCC WGI TSU)	Checked Biasutti and Sobel paper. Paper summarises research that supports this sentence.
1501	3	54	45	54	46	Please check the sentence that begins "For example...". I'm wondering if the correct paper has been cited, because my quick skim through Biasutti and Sobel (2009) didn't turn up anything to support the statement that is made here. (Zwiers, Francis, Environment Canada)	Too much detail; space limitations.
1502	3	54	46	54	50	For the precipitation change in Australia, it would seem to me the changing types of precipitation regime should, I would think, be presented. As I understand it, the traditional storm track is shifting southward off the southern coast of Australia and being replaced by an increasing likelihood that tropical cyclones will come and on occasion dump very large amounts of precipitation. It is not just total precipitation that counts--it is the character, timing, etc. Providing an indication that such issues matter would help provide insight. (MacCracken, Michael, Climate Institute)	can't add other confirmative paper on this specialised topic for a small region.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1503	3	54	47	54	48	A recent study that supports the sentence "Another example is illustrated with the relationship of rainfall in southern Australia with sea surface temperatures (SSTs) around northern Australia." is Smith and Timbal (2010). Like the Nicholls (2009) paper, they also show that changes in north Australian SSTs are inconsistent with the drying trend in southern Australia. Full reference: Smith, I. and Timbal, B., 2010: Links between tropical indices and southern Australian rainfall, International Journal of Climatology, DOI: 10.1002/joc.2251 (AUSTRALIA)	In general, these causes may have either a positive impact on flood occurrence or a negative impact. The meaning of this sentence is not restricted to "bad" impacts nor "good" impacts. This sentence is a scientifically neutral statement.
1504	3	54	59	54	60	Not necessarily. As it is said in the same page 54 in line 11, floods are affected between other things by the soil character and status (frozen or not,…). It is known from long ago, that rivers with a high fraction of their catchment area with frozen soil have lower infiltration and a more step peak during the melting season. Thus, a warming trend may in some cases tend to reduce this effect and favor a smoother peak. (Barros, Vicente, IPCC WGII TSU)	FLOODS
1505	3	55	0	58	0	Pakistan flood, as a very disastrous extreme event and as one of the most recent widespread floods, could be notified and some brief information can be presented in this section. (IRAN, ISLAMIC REPUBLIC OF)	too specific, cannot be included
1506	3	55	5	55	6	Section 3.5.1 Drought propagation through a comparison between meteorological and hydrological drought was studied for Denmark (regional scale) in Hisdal, H. & Tallaksen, L.M. (2003) Estimation of regional meteorological and hydrological drought characteristics. Journal of Hydrology, 281, 230-247 It is suggested to change/add as follows: ....of the issue at the catchment and regional scales (e.g. Hisdal & Tallaksen, 2003; Tallaksen et al., 2009), but these need to be further studied and extended to the continental scale. (NORWAY)	spotted removed
1507	3	55	7	0	0	Chap 3, page 55, line 7: "...better understanding of the spotted maps of hydrological droughts...". What does "spotted" mean in this sentence? (UNITED STATES OF AMERICA)	spotted removed
1508	3	55	7	55	7	what is meant by "spotted maps"? (Stocker, Thomas, IPCC WGI TSU)	see comment 1360
1509	3	55	10	55	12	This phrasing makes it sound as if just random places are having random changes. I think it would be much more informative to say something like: In summary, as climatic zones shift, there will be regions that tend to get more precipitation (e.g., as the tropical zone expands, and in polar regions as warming occurs) and others that will tend to get less (e.g., the equatorward edge of the mid-latitudes as the subtropics expand poleward). Consistent with this general tendency, regions such as ... have bcome drier since the mid-20th century whereas regions such as .... have become wetter; a few other regions have shown opposing changes, suggesting that natural variability is continuing to play a noticeable role. Such trends are expected to continue as climate change continues to intensify and climatic zones shift further through the 21st century. In particular, more frequent droughts are projected to occur [give locations] and wetter conditions in [give locations]." Or at least something like this. (MacCracken, Michael, Climate Institute)	this is given our assessment that precipitation is much harder to simulate then e.g. temperature, and drought is a complex phenomenon
1510	3	55	10	55	18	Despite the problems that have been documented, the assessment of the projections seems to be somewhat on the conservative side. (Zwiers, Francis, Environment Canada)	this is given our assessment that precipitation is much harder to simulate then e.g. temperature, and drought is a complex phenomenon
1511	3	55	10	55	18	It feels that the degree of scientific caution applied in the drought section is higher than say for the temperature section 3.3.1. The uncertianty in modelling the right processes is having a direct affect on toning down the confidence statment for predictions to medium. This may be right but it does not seem that the temperature section is exhibiting the same degree of caution. I must admit from a personal perspective, fig 8 of Burke and Brown 2008 suggests to me that there is higher confidence than just medium for the Mediterranean and Southern Africa. (Brown, Simon, The Met Office Hadly Centre)	ok, deleted
1512	3	55	12	55	13	This conclusion that new studies have improved understanding of the mechanisms leading to drought is not well supported by the text in this section which said rather little on this subject (in Box 3.2 about drought drivers for example, or on page 53 lines 1-20). (CANADA)	Noted. Necessary modification is made. What the authors meant is the same as what you pointed out here. "Global" did not intend to mean "globally same direction". Rather, "global scale" change indicates a global pattern of changes which vary amoung regions.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1513	3	55	14	55	21	The number of analyses based on stream gauge records for rivers in other parts of the world is limited. Available (limited) analyses for Asia suggest the following changes: the annual flood maxima of the lower Yangtze region show an upward trend over the last 40 years (Jiang et al., 2008), the likelihood for extreme floods in the Mekong river has increased during the second half of the 20th 16 century (Delgado et al., 2009), and both upward and downward trends are identified over the last four decades in four selected river basins of the northwestern Himalaya (Bhutiyani et al., 2008).In the Amazon region in South America, the 2009 flood set record highs in the 106 years of data for the Rio Negro at the Manaus gauge site in July 2009 (Marengo, 2011). However, such analyses cover only limited parts of the world. Evidence in the scientific literature from the other parts of the world, and for other river basins, appears to be very limited. For example, Conway et al. (2009) concluded that robust identification of hydrological change was severely limited by data limitations and other reasons for sub-Saharan Africa. Di Baldassarre et al. (2010) found no evidence that the magnitude of African floods has increased during the 20th century. The above analysis indicates that research subsequent to the AR4 still does not show clear and widespread evidence of observed changes in the magnitude/frequency of floods at the global level based on instrumental records, and there is thus low confidence regarding the magnitude and even the sign of these trends. 1. The approach to flood trends is missing the point that since the global warming trend is not homogeneous, atmospheric circulation patterns may change and consequently precipitation trends may show different and even opposite signs in different regions. Therefore, looking at a general global trend of floods does not make too much sense, neither from the scientific point of view, nor from the impacts on nature and human society. What matters is considering local and regional cases or at least the greater and more populated basins where people is adapted to certain river maximum flows that when surpassed creates severe damages and social suffering. (Barros, Vicente, IPCC WGII TSU)	This comment was misplaced, and is similar to a comment 1557. Because 1557 was place in the right place, the reply is written there.
1514	3	55	14	55	21	2. The paragraph quotes positive trends or extreme positive records to big rivers with huge catchments like the Negro in Amazon basin and Yangtze and in other important rivers in Asia. In addition, positive trends are reported for part of Europe in the former paragraph. It is mention that robust identification of hydrological change was severely limited by data limitations, but it seems that the search for literature were not comprehensive for developing regions. For instance, for the Plata basin that extends over 3 M Km2 with a discharge of 23,000 m3/s, there are evidences in the literature of important changes in the more extreme flows of their main rivers after 1970. The monthly discharges that surpasses the three times the standard deviation over the media at Corrientes over the Parana River which is the more important tributary of the Plata, were 16 in the period 1904-2000, of which only 4 took place before 1970 (Camilloni and Barros 2003, their Table 2). For the Paraguay River at Asunción for the same period, of the 16 monthly discharges that surpassed two times the standard deviation over the media, only 6 occurred before 1970 (Barros et al 2004, their Table 1). Finally, for the Uruguay River at Paso de los Libres, out of the 18 highest monthly discharges between 1909 and 2000, only 5 took place before 1970 (Camilloni 2005, her table 3.3) References: 1) I. Camilloni and V. Barros 2003. Extreme discharge events in the Paraná River and their climate forcing. J. of Hydrology, 278, 94-106 2)V. Barros, L Chamorro, G. Coronel and J. Báez, 2004. The major discharge events in the Paraguay River; Magnitudes, source regions and climate forcings. J Hydrometeorology 2004 5, 1061-1070 3) I, Camilloni, 2005. Variabilidad y tendencias hidrológicas en la Cuenca del Plata. In El Cambio Climático en el Río de la Plata. Eds. V. Barros, A. Menéndez y G, Nagy. CIMA. Buenos Aires 200 pp. (Barros, Vicente, IPCC WGII TSU)	Noted. Thank you for the comment. The assessment does not intend to make a novel conclusion, which is different from the objectives of journal papers. Assessment is usually conservative. "Not surprising" reflects the nature of assessment.
1515	3	55	20	0	0	The conclusions on floods are not surprising taking in account the accumulation of incertitude from different types of models (climatic, scaling down, hydrologic). (BOURRELIER, PAUL-HENRI, AFPCN)	Disagree. As is written in the assessment, there is no compelling evidence that the risk (magnitude/frequency) of spring floods in high latitudes has increased or will increase. Milly et al. is a good paper, but is not the only paper. There are a number of papers on both past changes and future projections. The assessment is based on a number of papers The AR4 also considered the paper by Milly et al., but did not fully support it in terms of widespread observed trends.
1516	3	55	20	58	30	We are very surprised that increased risk of spring floods in high latitudes in not mentioned in the summary. Global warming is known to lead to more winter precipitation in high latitudes. When that increased amount of snow melts, the risk of flooding would increase. Milly et al. have a series of papers that discuss this subject. This seems pretty robust. (UNITED STATES OF AMERICA)	Noted. The comment is appreciated, but the total length of the text should be limited. Therefore, it is difficult to put more information on paleoclimate data here. Please understand this is not a textbook nor a full review paper.
1517	3	55	20	58	30	A short section on paleoclimate data should be added (e.g., Baker, 2008, Geomorphology) (UNITED STATES OF AMERICA)	Agree.
1518	3	55	22	55	23	The author team should cite the SREX glossary as well. (IPCC WGII TSU)	Modification is made following the suggestion, but the start of new paragraph could be slightly different from the suggestion.
1519	3	55	22	55	35	The opening text on the topic of floods is quite well done. I would only suggest starting a new paragraph at line 35 where the subject matter being covered changes. (MacCracken, Michael, Climate Institute)	The same as 1518.
1520	3	55	23	55	23	Should the AMS definition, or a variant, become part of the SREX glossary? (Zwiers, Francis, Environment Canada)	Now cite SREX glossary instead.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1521	3	55	23	55	23	Please cite the AMS glossary correctly, ie, normal (Author/year) format. (Stocker, Thomas, IPCC WGI TSU)	Thank you for the comment. The Lead Authors discussed this matter, and determined that the current description is fine for this section.
1522	3	55	24	55	24	Section 3.5.2 deals with floods, and it is stated here that this term includes coastal floods. But coastal floods are not discussed in this section, but rather in 3.5.5., where they are referred to as "inundations" not "floods". The term coastal floods is used again in Chapter 4. There is scope for some tidying up here. (Global Climate Observing System Steering Committee)	The comment was understood. Because this kind of classification is not pure science nor mathematics, however, it is difficult to avoid some overlappings. For example, there can be an urban + coastal flooding. River flood and flash flood may be difficult to distinguish for some cases. There is no pefect way to clasify floods.
1523	3	55	24	55	24	Is 'urban flood' not covered by the terms flash flood, sewer flood, or pluvial flood? (Stocker, Thomas, IPCC WGI TSU)	Modified.
1524	3	55	25	55	25	Not clear what you mean with "...a combination of previous types"? Please clarify. (Stocker, Thomas, IPCC WGI TSU)	Noted. Necessary modification is made through this section (see also the response to 1584). Methodological issues regarding GCM, RCM and statistical downscaling are also in Section 3.2.3. Also, the assessment on floods written in the summary reflects the uncertainity of projections including what this comment argued. In addition, the text never mentioned that the hydrological simulations are or should be carried out by the "offline" mode. There is still no answer for whether "offline" simulation is better or "online" simulation directly using GCM runoff is better. Some references in this section relied on "online" simulation. Researchers in a practical field of science tend to use "offline" and "downscale" which ignore the energy balance. Some of them claimed in this commentary sheet that GCM/RCM outputs alone are not too much effective because of their biases and coase resolution, for example for hydrological simulations in mountainous areas. On the other hand, some "pure" scientists argued that the energy and mass balance should be kept. The description of this section tries to reconcile those two aspects under the current level of scientific knowledge, focusing on floods.
1525	3	55	36	55	37	This statement implicitly supports two practices that are widely practiced without careful examination: (1) ignoring runoff/discharge produced by GCMs and instead using "offline" simulation of runoff and river discharge based on other model outputs, and (2) using inappropriate temperature-based parameterizations of potential evapotranspiration, unconstrained by energy balance, to "improve" the runoff outputs of the climate models. It should also be kept in mind that such simulations still rely on coarse-resolution precipitation information, perhaps with unsubstantiated downscaling methods. (UNITED STATES OF AMERICA)	The comment was understood. However, it is the common structure through the entire Chapter that the introduction for each extreme should contain the brief explanation on the methodology/tools for projections. Thus, the current order is kept.
1526	3	55	36	55	43	These sentences "River discharge ... described later in this subsection" are misplaced here in the introduction to the flood section. Suggest this text is shifted to page 57, line 55 where flood projection and uncertainties are discussed. The final line "more details...." could then be deleted. (Stocker, Thomas, IPCC WGI TSU)	Some modification is made through this section to avoid misunderstanding. By the way, some references on flood changes in this section were based on results of GCM/RCM runoff where energy budget is closed. Thus, this criticism does not apply to all the references and all the sources for the assessment. See also the response to 1525.
1527	3	55	38	55	40	This conclusion could come about, even if it were not true, as a result of looking at a set of analyses in which a methodological bias (e.g., inappropriate representation of energy-availability trends) was common to all. (UNITED STATES OF AMERICA)	This sentence does not intend to deny the importance of downscaling for snow- or glacier-dominated rivers. This short sentence only indicates the importance of temperature for snow- or glacier-dominated rivers, while precipitation is generally considered important for floods of rivers. Importance of downscaling for snow- or glacier-dominated rivers are described later in this section, as indicated in this paragraph. Through the section, some more description on downscaling/bias-correction is added.
1528	3	55	41	55	42	For better snow and glacier melt info and emphasis needs to be made to the local model. Downscaling of GCM and RCM is important for most of the high altitude glaciated mountains in order to get quantitative measurements. We have noticed that simple GCM/RCM is not too effective. This statement could be modified, especialy pertaining to snow and glacier fed rivers in the high mountains. (Haritashya, Umesh, University of Dayton)	Modified. (Delete "sub").
1529	3	55	43	0	0	The term 'subsection' is used here, but is not used any other place. (JAPAN)	Agree. Modified.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1530	3	55	46	55	47	Delete this final sentence. This is an assessed finding that is appropriately given on page 56, lines 50-51. It does not need to be repeated here. (Stocker, Thomas, IPCC WGI TSU)	The authors appreciate the comment, but this fact is not necessary to be added here, although the fact is interesting in general.
1531	3	55	49	0	0	To our knowledge, the longest hydrological record ever are the water levels of the Nile river, measured at Roda Island near Cairo, and covers the period 622—1284 AD. (BELGIUM)	The authors appreciate the comment, but this fact is not necessary to be added here, although the fact is interesting in general.
1532	3	55	49	0	0	To my knowledge, the longest hydrological record ever are the water levels of the Nile river, measured at Roda Island near Cairo, and covers the period 622—1284 AD. (BELGIUM)	The authors appreciated the comment, but this comment is more relevant for the sections for precipitation and atmospheric phenomena. For example, Emori and Brown (2005) and Sugiyama et al. made a classification of causes for heavy precipitation between dynamical one and humidity-increase. Such an aspect is written in the sections of precipitation and other atmospheric phenomena, not in this section. For the projections of floods, the suggested mechanism was automaticlly included, because projected precipitation is used for river discharge simulations.
1533	3	55	49	55	51	I think it somewhere needs to be mentioned that as the storm tracks shift, the precipitation will be falling onto regions that have not experienced the heavier precipitation, and so the geography and land and river forms will not be suited to the new conditions, and floods would seem to be more likely even without an intensification of precipitation systems. (MacCracken, Michael, Climate Institute)	Noted, but deglacial floods are the out of scope of this section. Deglacial floods is a very interesting topic in the entire climate science, but this section is focused on what people are worrying to suffer in the coming few decades or a century. Deglacial floods would not change the assessment which policy makers and people want to have, within the scope of this section and SREX. Some glacier-related issues within the scope of this Chapter are in 3.5.6.
1534	3	55	49	55	62	Would be good to see more recent literature on paleo-floods assessed here. In relation to geological indicators, there are recent papers on deglacial floods for example. (Stocker, Thomas, IPCC WGI TSU)	Appreciate the comment. Slightly modified.. It is also possible, however, that paleoflood studies would not perfectly cover the entire world if the funding for it was huge.
1535	3	55	51	55	51	"… this can be overcome partly…" It is substantially overcome when paleoflood studies are done. That such relatively little funding goes towards paleoflood hydrology vs modeling is a major hinderance to better understand the regional/country-wide effects of climate variability and potential future changes. (Jarrett, Robert, USGS)	Noted. Because of several previous suggestions that pointed out "too much European literature", the number of references for Europe was reduced. With these suggested references, the conclusion/assessment of this section would not be different. Please understand this assessment is not a full review of existing papers.
1536	3	55	52	56	2	In this passage some results from e.g. the RIMAX-Project should added, e.g. Grünewald et al. (Trends in flood risk of the River Werra (Germany) over the past 500 years, Hydrological Sciences Journal - Journal des Sciences Hydrologiques 51(2006)5 : Special issue, Historical Hydrology, S. 818-833, 0262-6667 ; Mudelsee, M., Börngen, M., Tetzlaff, G., Grünewald, U. (2004): Extreme floods in central Europe over the past 500 years: Role of cyclone pathway "Zugstrasse Vb", Journal of geophysical research, 109(2004)23, 0148-0227; Mudelsee, Manfred, Börngen, Michael, Tetzlaff, Gerd, Grünewald, Uwe (2003): No upward trends in the occurence of extreme floods in central Europe, Nature : international weekly journal of science, 425(2003)6954, S. 166-169, 0028-0836; Dostal, P.; Imbery, F.; Bürger, J.; Seidel, J. (2010): Regional determination of historical heavy rain for reconstruction of extreme flood events. In: Kropp, J.P.; Schellnhuber, H.J. (eds.): In Extremis - Extremes, Trends and Correlations in Hydrology and Climate. Springer, Berlin, 91-101; Bürger K, Dostal P, Seidel J, Imbery F, Barriendos M, Mayer H, Glaser R, 2006: Hydrometeorological reconstruction of the 1824 flood event in the Neckar River basin (southwest Germany). In: Hydrological Sciences Journal,; 51 (5) : 864-877) (GERMANY)	Modified.
1537	3	55	54	55	54	Suggest changing text to "… flood records suggest that (1) flood magnitude and frequency can be sensitive to subtle alterations…" (UNITED STATES OF AMERICA)	Appreciate the comment. The text is modified. (the same as 1537)
1538	3	55	54	55	54	"… frequency are very sensitive…" Certainly, this may be true for the two referenced papers, but other studies show low sensitivity. I'd suggest providing a little balance with this sentence. (Jarrett, Robert, USGS)	Modification is made. By the way, this sentence originally intended to show a scientifically positive aspect of using paleo data.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1539	3	55	60	55	61	"… sometimes claimed to be…" Why is such a negative term used only for paleoflood data/analyses? Certainly, much of the modeling results "claim to be…", but such terminology is not used for modeling/modelers. Rather biased and needs revision as necessary throughout the report. (Jarrett, Robert, USGS)	Noted. See 1536, as well. This paragraph does not intend to make a full review of palefloods in Europe or in any other regions. Rather, this paragraph intends to show an overall introduction to paleo flood studies with some examples, and make it useful of the assessment, within the limited length of text. The argument of this paragraph is the limited spatial coverage of paleo-flood data avaiable so far in scientific literature. Good examples may be found in limited rivers in Europe, but the point is that limited rivers in Europe is a very small portion of the world.
1540	3	56	1	56	2	Certainly paleoflood data are not available as we'd like, but perhaps a more realistically interpretation is that the use of paleoflood data by modelers has not been used may be a better statement. This could change with a more balanced funding of different approaches to address climate change understandings and the report's goals/methods. (Jarrett, Robert, USGS)	Noted, but the conclusion of AR4 introduced here includes the paper by Milly et al. (2002). SREX generally begins with the conclusion/outcome of AR4 which implicitly contains references cited in AR4. In addition, later in thise section, Milly et al. (2002) is described as an example of detection-attribution study, because it is more relevant to describe it there within the limited length of the section. Also, see the response to 1516 on the Milly's paper.
1541	3	56	1	56	2	For European floods I suggest to consider the most recent and best referenced literature and to replace the sentence "However, the currently available…." by a more precise statement: "Extreme catastrophic flood events in Europe for the last 1000 years are reported for the Main in July 1342 (Tetzlaff et al., 2003), the Lower Rhine in February 1374 (Herget and Meurs, 2010), and for the High Rhine Basin in August 1480 (Wetter et al., accepted). Interestingly a unique "flood disaster gap" was observed in many areas of Europe between about 1877 and 1998 (Wetter et al., accepted)." / References: Tetzlaff, G., M. Börngen, A. Raabe, and M. Mudelsee, 2003: Meteorological-hydrological conditions of the Elbe flood in August 2002 in relation to those of the Main flood in July 1342. Geophysical Research Abstracts, 5, 10085. / Herget, J., and H. Meurs, 2010: Reconstructing peak discharges for historic flood levels in the city of Cologne, Germany. Global and Planetary Change, 70, 108-116. / Wetter, O., C. Pfister, R. Weingartner, T. Reist, J. Trösch, and J. Luterbacher, accepted: The largest floods in the High Rhine Basin since 1268 assessed from documentary and instrumental evidence. Hydrological Science Journal, accepted. (Wanner, Heinz, University of Bern)	The description here is "currently available … data is limited." It never denies the potential of paleo data studies. Rather, the description presents the current status of research. By the way, this relatively rich amount of description on paleo floods as a section for floods in an IPCC report will be benefitial not only for paleo researchers but also for modelers and policy makers for further encouragement of paleo-flood studies, although the text does not intend to be policy-prescriptive.
1542	3	56	4	56	5	At the time of AR4, evidence, albeit weak, existed for a 20th-century increase in flooding on the time-space scales that are resolvable by GCMs (Milly et al, 2002, in Nature); to ensure that this study has been considered here it would be good to cite it and state the deficiencies that cause it to be discounted; otherwise, the reader might have the impression that it has been overlooked. (UNITED STATES OF AMERICA)	This is an introduction to the major assessment on floods by the AR4. Thus, this paragraph shows that the description in AR4 was limited, which is worth to describe here in SREX that begins with the outcomes of AR4.
1543	3	56	6	56	9	Citation and equivocal interpretation of a single event (Trenberth 2007 reference to European flooding of 2002) doesn't seem informative. (UNITED STATES OF AMERICA)	Modified as suggested.
1544	3	56	8	56	9	Please give information about the analyzed time period of the 'trend' referred to here. (Stocker, Thomas, IPCC WGI TSU)	Modified as suggested.
1545	3	56	10	56	10	Italicize 'high confidence' (Stocker, Thomas, IPCC WGI TSU)	Modified as suggested.
1546	3	56	10	56	10	It would be helpful to italicize "high confidence" on this line to indicate use of calibrated uncertainty language. (IPCC WGII TSU)	Modified as suggested.
1547	3	56	11	0	0	"change in TIMING of flow peak" ? (UNITED STATES OF AMERICA)	Noted. Tthis paper did not describe floods relevant for this section.
1548	3	56	14	56	16	Consider mentioning rain-on-snow (ROS) as a contributing factor to snowmelt floods. The paper by Rennert et al. (2009) analysed ROS in the daily output of a fully coupled GCM under a future climate change scenario and found "an increase in the frequency and areal extent of [ROS[events for many parts of the Arctic over the next 50 yr and that expanded regions of permafrost become vulnerable to ROS". (Brown, Ross, Environment Canada @ Ouranos)	Modified as suggested.
1549	3	56	14	56	16	Shouldn't this say "affects PRECIPITATION TYPE (i.e., RAIN/SNOW partitioning), snowmelt or ice cover" (UNITED STATES OF AMERICA)	Noted. The comment is very appreciated, but space is limited for putting all the requested references. In particular, it was strongly pointed out by previous suggestions that references for Europe are too many. Thus, we deleted many references for Europe. Please understand that, without this reference, the conclusion is not different.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1550	3	56	18	56	18	Section 3.5.2 See comment on p55 l5-6: It is suggested to add: …(Shiklomanov et al., 2007). The same is seen for the Nordic countries (Wilson et al., 2011) were no trends are seen in the magnitude of autumn floods nor spring snowmelt floods, but a clear tendency towards earlier snowmelt is found except in Iceland. Lindström and ……. (NORWAY)	Noted. After revisiting the references for North America, it is still difficult to make a better elaborated statement without carrying out a new study. The difference between the statement for North America and for Europe simply comes from the number of countries. Integration of the knowledge from various papers is a role of assessment, but carrying out a new study is out of the scope of assessment. It is necessary to conduct a new study for making an elaborated statement for North America.
1551	3	56	22	56	29	Suggest some elaboration of the findings from the 5 cited studies about changes in floods in North America be provided beyond the simple statement that "there is no compelling evidence for change in the magnitude/frequency of floods". There should be more consistency in reporting on work for different continents to the extent the literature allows. The remainder of the paragraph discusses individual countries within Europe and concludes by saying a continental-scale assessment of change is difficult due to geographic differences in reported changes. Is this the case for North America as well? (CANADA)	Noted. The comment is appreciated. Because geographical balance is necessary, however, it is impossible to refer to all the European references. This text is a result after a long text for Europe was shortened. Please understand this is not a full review.
1552	3	56	25	56	27	References to flood impact studies in Belgium are: (impacts of 24 AR4 RCM runs on daily low flow extremes of 67 catchments of the Scheldt basin studied) Boukhris O., Willems P., Vanneuville 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); and Baguis P., Roulin E., Willems P., Ntegeka V. (2010), 'Climate change and hydrological extremes in Belgian catchments', Hydrol. Earth Syst. Sci. Discuss., 7, 5033-5078, doi:10.5194/hessd-7-5033-2010 (Willems, Patrick, Katholieke Universiteit Leuven)	The comment is apprecited. Because geographical balance is necessary, however, it is impossible to refer to all the European references. This text is a result after a long text for Europe was shortened.
1553	3	56	27	56	27	Section 3.5.2 It is suggested to add the Wilson et al.reference as well, e.g: ……..in the UK ( ) and in the Nordic countries (Wilson et al., 2011). (NORWAY)	Noted; some necessary modification is made to this section to highlight that our description focuses on "climate-driven" changes in floods. On the other hand, the suggested aspect (= land-use) is described later in this section, according to the general structure common to a section for each extreme. Also, many cited studies in this paragraph tried to minimize the impact of land use change. Thus, it is inappropriate to address land use change in this paragraph, particularly in the last part of the paragraph. Also, this is a paragraph for instrumental records rather than paleo records.
1554	3	56	29	56	29	"… not seen in the reported changes." Also need to note that many European rivers have had substantial land-use changes over the past 100+ years (e.g., regulated flows of varying degrees). This is true for a number of countries. Hoever, in working in many different river that have had substantial disturbance, one can usually find an undisturbed reach of the stream where paleoflood reconstructions can be made. (UNITED STATES OF AMERICA)	Deleted.
1555	3	56	31	56	31	Delete this first sentence. It is repeated again on line 37. (Stocker, Thomas, IPCC WGI TSU)	No. Fits better where it is lcated.
1556	3	56	31	56	41	The influence of water resource management needs to be mentioned already within this paragraph. Suggest to move the sentences beginning on line 61 to appear in this paragraph. (Stocker, Thomas, IPCC WGI TSU)	Following the comment, some references are added, and a sentence is added.
1557	3	56	31	56	41	Camilloni and Barros (2003) and Camilloni (2005) explored the extreme discharge events (monthly discharge anomalies larger than three times the standard deviation) in the Paraná and Uruguay rivers of La Plata basin in South America during the 20th century. Their results show in both cases a remarkable increase in the frequency of these events during te second half of the century in comparison with the first one. Similar results were found by Barros et al (2008) for the Paraguay River. These results are consistent with the positive rainfall trends in Southeastern South America (e.g.Barros et al 2008). These references are not cited. Camilloni, I. and V. Barros.2003. Extreme discharge events in the Paraná River and their climate forcing. Journal of Hydrology 278, 94-106. Camilloni, I. 2005. Hydrological variability and trends in La Plata Basin. 2005. In: Cliamte Change in La Plata River (In Spanish, El Cambio Climático en el Río de la Plata). V.Barros, A. Menéndez and G.Nagy (eds.). Ed. CIMA, Buenos Aires, 21-31. Barros,V.;L.Chamorro; G.Coronel and J.Baez, 2004. Themajordischarge events inthe Paraguay River: magnitudes, source regions and climateforcing. J. of Hydrometeorology 5, 1161-1170. (Camilloni, Ines, University of Buenos Aires/CONICET)	modified.
1558	3	56	46	56	46	"...lack of literature and evidence" would be better as: '...limited literature and evidence'. (Stocker, Thomas, IPCC WGI TSU)	deleted.
1559	3	56	51	56	51	"likely to imply past changes" -- are you saying here that "past changes are likely" or that the "information available likely indicates past changes"? Please clarify. (Stocker, Thomas, IPCC WGI TSU)	Modified.
1560	3	56	53	56	53	Suggest being specific that Bates et al. 2008 refers to the IPCC Technical Paper on Water. (CANADA)	Modified.
1561	3	56	54	56	54	The end of this sentence, stating that cause-and-effect was not discussed either in the AR4 or in Bates et al (2008), is awkwardly constructed. (Zwiers, Francis, Environment Canada)	Modified to avoid misunderstanding.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1562	3	56	58	56	59	"… change is nonetheless clearly detected…" There are many other studies of US rivers showing change is not clearly detected or detected at all. Rephrase as necessary. (Jarrett, Robert, USGS)	Modified.
1563	3	56	59	0	0	Suggest changing "regimes" to "timing" (UNITED STATES OF AMERICA)	Noted. Insufficient space for more attention on these points.
1564	3	56	61	57	2	These are good points that need more attention. (MacCracken, Michael, Climate Institute)	Noted. The relevant description is shown in the last paragraph of p. 57 with several references. Section 3.2.3 also describes the aspects pointed out. In addition, detailed implications to water resource management should be out of focus for this Chapter that deals with natural physical phenomena.
1565	3	57	0	58	0	The IPCC findings regarding the importance of GCM, downscaling and hydrologic model used on the uncertainty of the hydrologic projection make sense, yet do not lead to conclusions regarding their water resources implications. The same is true for the concluding paragraph of Box 3.1 Variations in Confidence in Projections of Climate Change: Mean vs. Extremes, Variables, Scale that underscores the increasing uncertainty at smaller scales and the decreasing confidence when variables other than temperature are considered. Of particular concern to water resources management is the uncertainty of the projected precipitation at scales below the spatial GCM resolution (~105 km2), as GCMs "give a broad, large-scale view of the evolution of global climate" (Kundzewicz et al., 2009). Therefore, to study the impact of climate change on the water resources and their management, the projected precipitation (the principal hydrological forcing) must be commonly downscaled to basin areas less than 105 km2. Downscaling, in essence, filters (statistically or dynamically) the uncertain projected data, adding to the uncertainty surrounding the model results. According to Kundzewicz et al. (2009), "quantitative projections in river flow at the basin scale remain largely uncertain". Estimating the impact of the climate on the groundwater recharge in a basin is an example of uncertainty propagation, if the recharge is derived from the hydrological balance, as is often the case: it involves the projected precipitation and the modelled ET based on projected climate, surface runoff, infiltration and storages under a changing climate. It is thus obvious that uncertainty propagates (additional uncertainty derives from land-use changes due to a rapidly evolving demographic and economic environment that modify the physical behaviour of the basin's surface through changes in its albedo and flow characteristics). References: Kundzewicz, Z. W., Mata, L. J., Arnell, N. W., Döll, P., Jimenez, B., Miller, K., Oki, T. & Şen, Z. (2009) Water and climate projections. Reply to "Climate, hydrology and freshwater: towards an interactive incorporation of hydrological experience into climate research" by Koutsoyiannis et al. (2009). Hydrol. Sci. J. 54(2), 406–415. (GREECE)	Noted. This study is an interesting study, but a single study which focuses on a few catchments in the same region does not solve the general question here. On the other hand, to cover all regional details, the space is not enough. It might be possible that the volume 2 of AR5 WG2 will cover these regional details because the volume will consist of regional chapters. Please also understand that previous reviews made us reduce the number of European literature to keep a geographical balance in the assessment.
1566	3	57	4	57	13	In the paper Lawrence, D & Haddeland, I. (2011) Uncertainty in hydrological modelling of climate change impacts in four Norwegian catchments. Hydrology Research (accepted) it is shown that 1) in catchments with a dominating snowmelt flood, annual maximum floods are expected to become smaller whereas in catchments with dominating rain floods, annual maximum floods are expected to increase. The paper focuses on sources for uncertainty related to the dominating flood generating process and a reference to the paper would add value to the chapter. A reference is suggested in the last bracket of the paragraph: (....Dankers and Feyen, 2009; Lawrence and Haddeland, 2011) (NORWAY)	Necessary modification is made through the section. Also, see the response to 1513.
1567	3	57	6	57	7	According to the preceding comment, this conclusion should be changed since it does not make to much sense to consider floods as a whole over the global scale. It should be replaced by a conclusion stressing the regional differences and accounting for the large regions of the world where there were important changes in the frequency of extreme floods. (Barros, Vicente, IPCC WGII TSU)	Modified to indicate that some other parts of the world have a similar phenomenon. A reference is added.
1568	3	57	8	57	8	Earlier break-up of river ice has also been reported for rivers in Arctic Canada. Please contact Dr. Terry Prowse of Environment Canada (terry.prowse@ec.gc.ca) for the most appropriate references for work he and colleagues have undertaken that provide evidence of this. (CANADA)	See the response to 1553. Please understand that it is impossible to refer to all the countries with adequate examples.
1569	3	57	8	57	8	Section 3.5.2 It is suggested to add the Nordic countries, e.g: ……..in the Canada ( ) and in the Nordic countries except Iceland (Wilson et al., 2011), along with …… (NORWAY)	Modified.
1570	3	57	12	57	12	delete "(i.e." (Stocker, Thomas, IPCC WGI TSU)	Examined through the paragraph and modified if necessary.
1571	3	57	15	57	24	There seems to be an excessive substitution of "warming" for "climate change" in this paragraph. In some places ("observed warming" and reference to spring flow peaks), the reference to "warming" might be appropriate. But changes in water fluxes, are not simple results of temperature changes. Rather, both temperature and water-flux changes are results of a radiative perturbation in the energy balance of the climate system. Use of "warming" in lines 17 and 19 seems simplistic and inappropriate. (UNITED STATES OF AMERICA)	Modified.
1572	3	57	17	57	17	It would be helpful to italicize "limited evidence" on this line to indicate use of calibrated uncertainty language (per the AR5 Guidance Note on Treatment of Uncertainties). (IPCC WGII TSU)	Modified.
1573	3	57	27	57	27	replace "introduced in the AR4" with "assessed in the AR4" or "considered in the AR4" (Stocker, Thomas, IPCC WGI TSU)	Noted. They are not duplications. First one is the outcome of AR4. Second one is an introduction to studies after AR4.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1574	3	57	34	0	0	duplicates line 26 - could paragraphs by merged or have their scope clarified? (BELGIUM)	This paper is relevant for a paragraph for river-basin scale simulations. However, there are already many examples and the addition of this paper would not change the conclusion.
1575	3	57	34	0	0	In this context, we suggest : Baguis P., Roulin E., Willems P., Ntegeka V. (2010), 'Climate change and hydrological extremes in Belgian catchments', Hydrol. Earth Syst. Sci. Discuss.7, 5033-5078; doi:10.5194/hessd-7-5033-2010. The paper was peer reviewed, adapted and submitted again. It should be accepted and published soon. (BELGIUM)	This paper is relevant for a paragraph for river-basin scale simulations. However, there are already many examples and the addition of this paper would not change the conclusion.
1576	3	57	34	0	0	In this context, I can refer to: Baguis P., Roulin E., Willems P., Ntegeka V. (2010), 'Climate change and hydrological extremes in Belgian catchments', Hydrol. Earth Syst. Sci. Discuss.7, 5033-5078; doi:10.5194/hessd-7-5033-2010 (BELGIUM)	Because various original papers cited in this review paper were already referred to in this section, this review paper is not added here.
1577	3	57	35	0	0	The following review for flood projections in Europe may be a good citation: Assessing river flood risk and adaptation in Europe—review of projections for the future. Zbigniew W. Kundzewicz, Nicola Lugeri, Rutger Dankers, Yukiko Hirabayashi, Petra Döll, Iwona Pińskwar, Tomasz Dysarz, Stefan Hochrainer and Piotr MatczakMITIGATION AND ADAPTATION STRATEGIES FOR GLOBAL CHANGE Volume 15, Number 7, 641-656, DOI: 10.1007/s11027-010-9213-6. (Nakaegawa, Toshiyuki, Meteorological Research Institute)	Modified into "northeastern".
1578	3	57	39	57	41	Dankers and Feyen 2009 refers to "northeastern Europe" to define the region with decreased flood probability - it looks unclear that it applies to the north of Europe as a whole, eg. including Norway. This paper also conclude that "[Outside northeastern Europe], we find a consistent tendency toward a higher flood hazard in the majority of the model experiments in several major European rivers" : changes are contrasted, with increases and decreases, even in a single paper. Could this be better reflected in the text? (BELGIUM)	There are already many examples, and the addition of this paper does not change the conclusion. Please understand that the number of examples has to be limited.
1579	3	57	46	57	47	Section 3.5.2 It is suggested to add: …UK catchments ( ), catchments in Norway (Lawrence and Haddeland, 2011) and catchments in continental…….. (NORWAY)	Added.
1580	3	57	49	0	0	Kitoh et al. (2011,JGR, see above) projects change in river discharges for major river basin in South America and focused on monthly time scale. (Nakaegawa, Toshiyuki, Meteorological Research Institute)	There are already many examples, while the addition of this paper does not change the conclusion. Please understand that the number of examples must be limited. Also, the focus of the paper is different from floods.
1581	3	57	49	0	0	The following reference for projections in the Middle East may be a cited: 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)	Added.
1582	3	57	50	57	50	A very recent reference for Africa is: Taye M.T., Ntegeka V., Ogiramoi N.P., Willems P. (2011), ‘Assessment of climate change impact on hydrological extremes in two source regions of the Nile River Basin’, Hydrology and Earth System Sciences (Hydrol. Earth Syst. Sci.), 15, 209-222, doi: 10.5194/hess-15-209-2011 (Willems, Patrick, Katholieke Universiteit Leuven)	What is "recently recognized" has a narrower/specific meaning than what is suggested by this comment. What is "recently recongized" does not only refer to general uncertainties in hydrological cycle of GCM that have been recognized, but refers to uncertainties in hydrological projections under a certain emission scenario with a set of certain downscaling methods.
1583	3	57	56	57	56	"It has been recently recognized.." Since the first use of GCM models several decades ago and their linkage to GCM-output used in hydrologic models, it has been widely known that the models need improvement and continue to need to be enhanced to assess the extreme complexities in nature and feedbacks. Rephrase as necessary. (Jarrett, Robert, USGS)	A sentence is added in this paragraph to refer to this issue. The suggested issue is a very important issue for large-scale water budget calculation based on climate model outputs. However, it is also true that researchers in practical fields of science or practitioners tend to use "offline" and "downscale" which ignore the energy balance, in particular for flood simulations of relatively small catchments and snow and ice simulations in mountainous areas. Some of them claimed in this commentary sheet that GCM/RCM outputs alone are not too much effective because of their biases and coase resolution, for example for hydrological simulations of mountainous areas. It seems energy closure is not necessary for them. On the other hand, some "pure" scientists argued that the energy and mass balance should be kept. What should be done for getting a scientifically-resonable and practically-relevant hydrological projections before a perfect climate model appears in far future is very difficult. The current text tries to reconcile those various aspects for making an adequate assessment.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1584	3	57	56	58	4	This paragraph shows no awareness of the errors ("uncertainties") introduced when selected output variables (usually temperature and precipitation) are taken from a climate model, while others (net radiation, potential evapotranspiration) are ignored and instead replaced (usually implicitly/unwittingly) by largely empirical and frequently implicit/hidden assumptions of no change or non-energy-conserving empirical relations with temperature. Extensive comments on this issue have been provided in connection with the drought section, and will not be repeated here. Bias correction and downscaling, which receive so much attention in the literature, possibly are the least of our problems in this regard. One counterexample serves to illustrate the fallacy of attributing confidence to consensus (Milly, P. C. D., Krista A. Dunne, 2011: On the Hydrologic Adjustment of Climate-Model Projections: The Potential Pitfall of Potential Evapotranspiration. Earth Interact., 15, 1–14.). (UNITED STATES OF AMERICA)	See the response to 1580 and 1581.
1585	3	58	1	0	0	same as above (Nakaegawa, Toshiyuki, Meteorological Research Institute)	Section 3.5.3-7 describe those issues as much as possible, within the limitation of text length, and within the framework of SREX. Section 3.5.3-8 describe those issues as much as possible, within the limitation of text length, and within the framework of SREX.
1586	3	58	1	70	30	Section 3.5.3 Extreme Sea Level we think the text is missing important papers and ideas on alpine glaciers contributing to recent (last 10 years especially since 2003 see Lyman May 20, 2010 Nature for references] and 21st century sea level rise – Meier 2007 Science, Berthier January 17 2010 Nature Geoscience, Hock et al. 2009 GRL are a good start on Alaskan- Canadian and Antarctic island glaciers. There is an important idea among some glaciologists that alpine glaciers have been and will play a larger role in sea level. Likewise, the mass balance of the two big ice sheets Greenland and Antarctic ice sheets is much better known and decreasing contributing to recent SL rise [van den Broeke 2009 Science, Rignot 2008 Nature, respectively]. The text does cite Veliconga, but there are many other papers on ice sheet contribution to SL. We raise this simply due to balance – the text goes into some detail about steric effects, decadal and spatial variability, storms etc, but not to the same extent for glacial contributions. Page 62. We noticed line 20 the glacial sea level here is given as 120 meters but earlier in section 3.5.3 the value 130 was used – the latter is probably closer to the best estimate, though the Barbados reefs and oxygen isotopic ice volume method have meters of error and the age of the peak "LGM" is also subjective, accepted as 22,000 years ago but Peltier and Fairbank's 2005-06 Barbados reef dating showed it to be 22-26 ka or so, which was the conservative age range used correctly in section 3.5.3. Also, the idea that catastrophic collapse of portions of west Antarctic or smaller ice shelves can occur might be mentioned since the theme is extreme events. 3.5.6. Glacier, geomorphological and geological impacts. p. 66, line 14: This is an unusual grouping of topics and doesn't really synthesize all the processes p. 66, line 16: Why start with mountains? Many glaciers are in lowlands. Yes high mountain chains are at plate boundaries but there is disconnect between climate hazareds and tectonic ones. p. 66, line 21-22: This gives the impression earthquakes take a long time, but if ever there was a quick event an earthquake qualifies. But why are these even discussed in a climate document? Later in this section page 66, line 32, the text on climate impacts on glaciers is better. Page 66 line 40 section on	This section is described within the framework described in 3.2 which explains the methodology of projections. In addition, the limited text length is used for information useful to adaptation communities. Also, description on observed paleo-flood appeared already in an earlier part of this section.
1586.2	3	58	1	70	30	glacial lake floods is confusing. First, large ice-dammed proglacial lakes are of course believed to have triggered major climate reversals like the Younger Dryas – this is a huge topic but not really critical here, since we don't have immense glacial lakes like those during the last deglaciation [when SL was rising 50 mm/yr]. In contrast to proglacial lakes, we do nowadays have jokulhlaups, like the one in southern Iceland in the 1990s. These are floods lasting a day or so caused by catastrophic release of water trapped below an outlet glacier or small ice cap. Richard Alley wrote a nice review paper on jokulhlaups a few years ago. There are also immense amounts of freshwater stored in lakes below Antarctic just being investigated. But we think the text is addressing another class of lakes, those behind moraines. Please clarify. The next page on landslides, debris flows seems more comprehensive. But page 68 seems to jump around from climate influence on glaciers, landslides, seismicity, isostatic adjustment, volcaniclastic activity, and other topics. So we recommend careful rereading of these sections, refocusing the text. Page 69 line 5. Many other factors influence these impacts…" This makes no sense, reflecting my confusion at the outset of the section. Test needs a little more focus and clarity on just what topics are being covered here. We recommend focus on geomorphological processes as they may be affected by climate change, this offers an array of surface processes like erosion, debris flows, lake outbursts etc. But leave out tectonics and glaciers [cover glaciers elsewhere under sea level, water resources]. We noticed later the section on sand dunes and dust storm, these are geomorphological-earth surface processes, perhaps unite them with surface processes in 3.5.6. Section 3.5.7. "High-Latitude Changes." Our take is this section seems pretty short considering the climate impacts being researched up there, both on land and in the Arctic Ocean and surrounding seas. (UNITED STATES OF AMERICA)	
1587	3	58	6	58	6	Increasingly, there are regional paleoflood studies (early 1990s to present) that provide projections of flood magnitude/frequency relations where climate variability has not resulted in changes in regional flood magnitude . Consider clarifying that modeling can not make such projections with much reliability. (Jarrett, Robert, USGS)	This is a summary paragraph leading to the final assessment of this section. Thus, limited repetition is necessary.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1588	3	58	7	58	7	Section 3.5.2 This line is a copy of line 45 p. 57 and reformulation should be considered. (NORWAY)	"low confidence" indicates "limited evidence" and/or "low agreement" in the published literature. The target of this assessment is not a limited number of specific basins. Papers on paleofloods do not significantly change this assessment. In addition, this assessment does not refer to the potential of any scientific methodology. Rather, the assessment should describe whether future projections of floods are available in a huge number of rivers all over the world or not. Possibility of paleo-flood research was already described earlier in this section.
1589	3	58	9	58	10	"... low confidence..." True, if one only considers model results. We suggest stating that paleoflood reconstructions have/can provide a moderate/high confidence in providing long-term data on the mangnitude and frequency of flooding, particularly for the largest flood for a given stream for calibrating/validating model projections. (UNITED STATES OF AMERICA)	Modification is made to this sentence and this paragraph.
1590	3	58	12	58	13	This sentence needs references to support the claim. (UNITED STATES OF AMERICA)	Modified.
1591	3	58	15	58	16	This is awkwardly stated, and potentially misleading because extreme precipitation (long return period events) is generally projected to increase, even in some areas where mean precipitation is projected to decrease. That is, increases in extreme precipitation are not necessarily linked to changes in mean precipitation. (Zwiers, Francis, Environment Canada)	P.57 described the observed changes. On the other hand, the text here described the future changes that seem to be larger and clearer than observed changes. Thus, naturally, there is a difference.
1592	3	58	20	58	20	Benefit from elucidation of factors responsible for shifts in spring peak flows in snowmelt and glacier-fed rivers other than anthropogenic influence, as it is an unexplained leap to suggest that anthropogenic influence has 'likely' been linked to such (pg 57 line 22), and then that earlier shifts are 'very likely'. (UNITED KINGDOM OF GREAT BRITAIN AND NORTHERN IRELAND)	Modification is made for clarification.
1593	3	58	24	0	0	Is the problem really that "the sign of change" at the world level is not known, or more that the changes are likely to be contrasted, with + and - areas, - as shown e.g. for Europe (even if the precise location of these regions remains unclear) ? (BELGIUM)	Please see the response to 1589.
1594	3	58	24	58	25	"... low confidence..." True, if one only considers model results. We suggest stating that paleoflood reconstructions have/can provide a moderate/high confidence in providing long-term data on the mangnitude and frequency of flooding, particularly for the largest flood for a given stream for calibrating/validating model projections. (UNITED STATES OF AMERICA)	Extreme low flows are so-called hydrological droughts which are discussed in 3.5.1. Eventually, future flood decrease shown in Dankers and Feyen (2009) and Hirabayashi et al (2008) for northeastern Europe is probably due to this kind of mechanism, although they did not much discuss it
1595	3	58	24	58	30	What about extreme low flows (the far opposite extreme to floods). For example, small basins at low elevations that have mixed nival/pluvial regimes in the current climate will transition to pluvial dominant flow regimes in the warmer climate of the future. In regions where precipitation is received predominantly in winter, this could result in very strongly reduced summer flows due to the elimination of snow storage. (Zwiers, Francis, Environment Canada)	Flood changes due to storm track changes and circulation changes are included here as a part of simulated flood changes due to precipitation and atmospheric circulation changes. More detailed discussions on this aspect (i.e. precipitation and atmospheric changes) are written in the section for precipitation and sections 3.4.1-3.4.5. This section should not refer to the causes of changes in precipitation that should be described in 3.4.1.-3.4.5.
1596	3	58	24	58	30	It seems to me the issues of shifting storm tracks and the changing underlying geography are important factors to be considering. (MacCracken, Michael, Climate Institute)	Modification is made in P. 56 for clarification. P. 58 is also modified. P. 56, lines 57-58 intended to describe the lack of a specific type of studies, for example, a statistical detection and attribution study of the relation between anthropogenic greenhouse gas increases and the changes in floods at a large scale. On the other hand, the description in P. 58 shows what simple physical reasoning suggests. Also, what is not established in P. 56 is a link from anthropogenic climate change to floods, rather than a link from precipitation to runoff.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1597	3	58	26	58	26	Reconcile this statement with earlier statements. If one cannot establish a direct link between trends in extreme precipitation and floods (lines 57-58, P. 56) , how can one conclude that "Nevertheless, an increase in magnitude and/or frequency of rain-generated floods is anticipated …. where ... rainfall extremes are projected to increase"? (CANADA)	P is a parameter which is easily obtained, but E is not a parameter/information which is easily obtained, although E is calculated in climate models and is available CMIP3/5 database. This may be partly because E is not easy to observe. Simulated E is easy to obtain, but actual E and historical E is difficult to obtain. Thus, P-E is not a good parameter to be mentioned here. Readers are not only limited to climate scientists. In addition, P-E could be a good measure for drought and water storage, but might not be good measure for floods. When big flood disasters occur, peopel do not care much about E. What they care about is P.
1598	3	58	28	0	0	For long-term time scale, it is more appropriate to consider P-ET than P alone. Patterns of change in long-term mean discharge (Milly et al 2005 in nature), which equals change in long-term mean P-ET, are one zeroth-order measure of potential changes in monthly to seasonal extremes of P-ET. I.e., areas of increased water supply, absent changes in rain/snow, are good candidates for locations of increased flood risk on large rivers. Furthermore, such a measure can be derived directly from the climate model, so it is immune to the methodological pitfalls in postprocessing of climate model outputs. (UNITED STATES OF AMERICA)	Not sure what point is being made here. No change
1599	3	58	33	0	0	Studies show that the trends for the highest levels are more accentuated than the evolution of the mean level which is deprived of doubt. + 0,80 m in 2100 is considered as the most plausible. But it is relative to the mean not the extreme. (BOURRELIER, PAUL-HENRI, AFPCN)	Have included a reference to this paper in the introductory paragraph
1600	3	58	33	60	41	Yin et al. have a paper showing the agreement (or lack) amoung the CMIP3 models for patterns of SLR. Two conclusions seem important to highlight here. One the patterns are not consistent model to model. Two, the magnitude of the local changes can be large relative to the global mean change. This means the local changes could be quite large (even without land ice melt) but the locations of the large rises are not well known...however several model indicate the NE coast of the US will be a area of relatively large SLR. (UNITED STATES OF AMERICA)	We acknowledge the possibility of such events but that the literature is too limited to allow an assessment at this time
1601	3	58	33	60	41	Why isn't the potential for rapid sea level rise considered as an extreme event? For example, loss of the Ross Ice Shelf in Antarctica could lead to very rapid rises in global sea levels, say more than 1 meter in a short period of time. While it's not considered likely, it's a possibility, along with other factors that could lead to rapid destabilization of ice sheets. I do note that other factors associated with more gradual sea level rise are covered. I imagine there was a reason this subject was left out. (UNITED STATES OF AMERICA)	These potential consequences of climate change have now been mentioned.
1602	3	58	35	0	0	There is complicated interaction between sea level and wave height. For example, mean sea level rise may increase or decrease astronomical tide range on a very mild coast. Wave height in surf zone increases for extreme sea levels. (Kawai, Hiroyasu, Port and Airport Research Institute)	these are mentioned in this section so no change required
1603	3	58	35	58	44	It seems to me that the issues of gravitational variations that contribute to regional patterns in sea level rise and isostatic effects need to be mentioned. (MacCracken, Michael, Climate Institute)	tsunamis have now been mentioned
1604	3	58	35	58	44	Tsunamis should be mentioned as an example of extreme sea level, even though they are not a climatic phenomenon. Tsunamis do provide useful context, especially given the two large events that have occurred in the last 5 years. (UNITED STATES OF AMERICA)	a sentence describing the various definitions used has been added to the first paragraph
1605	3	58	35	58	44	Should provide context for description of extreme sea levels - they can be defined as storm related highest values, highest daily mean values, highest annual and interannual monthly means, etc.. (UNITED STATES OF AMERICA)	explanation in text is sufficient and clear
1606	3	58	36	0	0	"Wave setup" - Please define. (UNITED STATES OF AMERICA)	these have been mentioned
1607	3	58	41	0	0	Winds changes are argueably the most important factor over the 20th century. Need to be mentioned here. (UNITED STATES OF AMERICA)	Made this change
1608	3	58	42	58	44	“In addition, rapid melting of ice sheets will lead to non-uniform rates of sea level rise across the globe due to adjustments in the Earth’s gravitational field (e.g., Mitrovica et al., 2010).” This should be changed to… “If rapid melting of ice sheets occur, it will lead…” (Knappenberger, Paul, New Hope Environmental Sciences)	Made this change
1609	3	58	43	58	43	It might be better to say "...rapid melting of ice sheets WOULD lead to non-uniform….." given that Chapter 3 don't actually assess or discuss the likelihood of rapid ice sheet collapse. (Stocker, Thomas, IPCC WGI TSU)	Changed to Clark's period
1610	3	58	47	58	47	The LGM is defined as 19 to 23 kyr BP (Clark et al., Science, 2009) (UNITED STATES OF AMERICA)	text is clear that it is since 18th century.
1611	3	58	53	0	0	What time frame? (Stouffer, Ronald, NOAA)	Corrected.
1612	3	58	56	58	58	Quote from IPCC (Bindhoff 2007) of 0.17 (0.12 to .22 mm/yr) for 20'th century appears wrong - should be 1.7mm/yr (1.2 to 2.2 mm/yr) (UNITED STATES OF AMERICA)	this paper has been cited.
1613	3	58	59	58	60	Further evidence also worth citing from Rignot et al. 2011 - GEOPHYSICAL RESEARCH LETTERS, VOL. 38, L05503, 5 PP., doi:10.1029/2011GL046583. "increasing evidence" is probably an overstatement based on two papers, so 'There is evidence that....' would be more appropriate. (Stocker, Thomas, IPCC WGI TSU)	this paper has been cited.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1614	3	58	60	0	0	Greenland mass loss has also been shown more recently by other papers such as Sorensen et al. (2010) Mass balance of the Greenland ice sheet– a study of ICESat data, surface density and firn compaction modelling, The Cryosphere Discuss., 4, 2103–2141, 2010 (International Petroleum Industry Environmental Conservation Association (IPIECA))	this paper has been cited.
1615	3	58	60	58	60	The new study by Rignot et al, 2011, GRL for acceleration should be cited. (UNITED STATES OF AMERICA)	reference to attribution of extreme sea level rise is only mentioned in Table SPM.2 following on from the attribution of mean sea level rise in Hegerl et al. (2007) so have not changed this but have added the Hegerl et al reference for the attribution of mean sea level rise
1616	3	58	63	58	63	Avoid citing entire IPCC WG reports other than for very general work of the IPCC. In this case, for specific results about sea level rise, the appropriate chapter of the WGI report should be cited. (CANADA)	increasing. This has been amended
1617	3	59	2	59	2	"..trend in extreme sea levels...." - increasing or decreasing trend? (Stocker, Thomas, IPCC WGI TSU)	this reference has been added
1618	3	59	6	59	26	Cite the Timmerman et al,, 2010, JCLI study on the effect of winds on SL (UNITED STATES OF AMERICA)	It is not clear that this is relevant in this context.
1619	3	59	19	59	20	Insert sentence after Merrifield (2007) that global extreme sea level patterns can be monitored using the average of the top 2% largest daily averaged sea levels and then fitting a GEV analyses to indicate above average extremes on an annual basis (Merrifield et al (2010), "Sea Level Variations", Chapter 3, Section h, in State of the Climate 2009, BAMS, Volume 91, July 2010. (UNITED STATES OF AMERICA)	This study is cited in the introductory paragraph of this section.
1620	3	59	36	59	43	In this paragraph it is important to mention the new results produced in terms of sea level rise scenarios for the Mediterranean obtained coupling a full AOGCM with a detailed high resolution model of the Mediterranean by Tsimplis et al (2008). Reference: Tsimplis, M. N., M. Marcos, and S. Somot, 2008: 21st century Mediterranean sea level rise: Steric and atmospheric pressure contributions from a regional model. Global and Planetary Change, 63, 105-111. (Pavan, Valentina, ARPA Emilia-Romagna)	This will be addressed comprehensively in the AR5. Here we are focussing on extreme sea level changes and provide a brief review of the projected slr to provide context
1621	3	59	36	59	52	We would like to see more guidance on future sea level changes. (SWEDEN)	Amended
1622	3	59	38	59	38	"An additional allowance … was made for …" -- suggest to reword to "An additional contribution … was taken into account for ..." (Stocker, Thomas, IPCC WGI TSU)	this has been made
1623	3	59	38	59	38	It would be preferable to avoid use of "confidence" here for clarity--"90% uncertainty range," for example, would avoid confusion with the calibrated confidence uncertainty metric. (IPCC WGII TSU)	Amended
1624	3	59	44	59	48	I think the correct year for Grinstead et al is 2010. (Zwiers, Francis, Environment Canada)	this sentence has been reworded
1625	3	59	44	59	48	Suggest providing more clarity on methods. This paragraph begins by noting that these studies use alternative approaches to projecting future SLR to those based on future emission scenarios. It is therefore confusing to have one of the results identified as being linked to the A1B scenario. (CANADA)	We agree that a review of post AR4 studies is a major task and is beyond the scope of the current assessment. We have however retained our review of the statistical models with more discussion on the caveats of these models .
1626	3	59	44	59	52	A post-AR4 review of global mean sea level rise is a major task. If it is taken on here, at a minimum it should consider also the following references: Joughin, I., Smith, B. E., and D. M. Holland, 2010. Sensitivity of 21st Century Sea Level to Ocean-induced Thinning of Pine Island Glacier, Antarctica. Geophysical Research Letters, 37, L20502, doi:10.1029/2010GL044819. Nick, F. M., et al., 2009. Large-scale Changes in Greenland Outlet Glacier DynamicsTtriggered at the Terminus. Nature Geoscience, DOI:10.1038, published on-line January 11, 2009. Schoof, C., 2010. Ice-sheet acceleration driven by melt supply variability. Nature 468, 803-805. Wada, Y., et al. 2010. Global Depletion of Groundwater Resources. Geophysical Research Letters, 37, L20402, doi:10.1029/2010GL044571. (UNITED STATES OF AMERICA)	this paper has been cited.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1627	3	59	44	59	52	RE: Section on recent papers on mean sea level rise. While you cite a string of post-AR4 papers which projected higher levels of mean sea level rise by the end of the century, all the while, the rate of sea levels rise has slowed in recent years (http://sealevel.colorado.edu/index.php) and many of the mechanism that are linked to an accelerated sea level rise (e.g., rapid loss of Antarctica's Pine Island glacier, rapid increased glacial flow rates in Greenland as a result of basal lubrication) have shown to be unlikely contributors. Such papers include: Joughin, I., Smith, B. E., and D. M. Holland, 2010. Sensitivity of 21st Century Sea Level to Ocean-induced Thinning of Pine Island Glacier, Antarctica. Geophysical Research Letters, 37, L20502, doi:10.1029/2010GL044819. Nick, F. M., et al., 2009. Large-scale Changes in Greenland Outlet Glacier DynamicsTtriggered at the Terminus. Nature Geoscience, DOI:10.1038, published on-line January 11, 2009. Schoof, C., 2010. Ice-sheet acceleration driven by melt supply variability. Nature 468, 803-805. And further, a sizeable proportion of the recent rise in sea level (including a rising contribution) has been linked to groundwater extraction (Wada, Y., et al. 2010. Global Depletion of Groundwater Resources. Geophysical Research Letters, 37, L20402, doi:10.1029/2010GL044571)—a process largely unrelated to anthropogenic climate change and yet one that was overlooked by analyses such as Rahmstorf (2007). I would suggest that this SREX report is not the proper place to discuss mean sea level rise estimates. Thus I suggest the removal of this entire paragraph. If mean sea level rise estimates are going to be discussed, then a much more thorough review needs to take place than simply listing a few papers which project higher sea level rise estimates than those presented in AR4—the situation is far more complicated than those papers suggest. And as future sea level rise is an extreme important and much followed topic with the potential for an incredible amount of press coverage, I would suggest that until a more complete and detailed review can take place, that the topic not be addressed. It is probably best left for the AR5. (Knappenberger, Paul, New Hope Environmental Sciences)	this suggested rewording has been adopted and Rahmstorf upper value corrected to 1.4
1628	3	59	44	59	52	The new study by Rignot et al, 2011, GRL for sea level by year 2050 should be mentioned here. (UNITED STATES OF AMERICA)	See comment above
1629	3	59	46	59	48	Suggest rewording here to make it clearer that the Grinsted et al. projection is the only one listed here that is for a single scenario (A1B), while the others all cover a range of scenarios. Also would make sense to order these from lowest to highest minimum rise. Possible rewording: "These alternative approaches yield projections of sea level rise under a range of SRES scenarios by 2100 of 0.47 -1.00 m (Horten et al. 2008), 0.50 - 1.20'?' m (Rahmstorf 2007), 0.75 - 1.90 m (Vermeer and Rahmstorf 2009), and 0.90 - 1.30 m (A1B scenario only, Grinstead et al. 2009). (Stocker, Thomas, IPCC WGI TSU)	emission scenarios have been included
1630	3	59	46	59	48	It would be preferable to specify the relevant emissions scenarios for all of the provided ranges. (IPCC WGII TSU)	this has been corrected
1631	3	59	47	59	47	Is the Rahmstorf 2007 projected range not 0.50 to 1.40 m? (Stocker, Thomas, IPCC WGI TSU)	I don't think this is inconsistent with what is said in lines 48-50 'However as noted by Cazenave and Llovel (2010), future rates of sea level rise may be less closely associated with global mean temperature if ice sheet dynamics play a larger role in the future' which implies they could be much larger. However, perhaps the comment is relating to the next sentence about the Pfeffer study. If so, it should be noted that the Pfeffer study is dealing with Greenland and Antarctic outlet glaciers and how they could contribute to more rapid sea ice contribution in the future.
1632	3	59	48	59	50	While the contribution of glaciers and ice caps will decrease with diminishing ice mass, the contribution of ice sheets might strongly increase in future. (Neu, Urs, Swiss Academy of Sciences)	This refers to Rignot et al 2011 which is now cited
1633	3	59	50	59	52	You might want to check this new article sugesting greater contribution from Greenland ice sheet to the sea level (http://www.agu.org/news/press/pr_archives/2011/2011-09.shtml). (Haritashya, Umesh, University of Dayton)	Not added - does not add sufficiently to justify adding another reference
1634	3	59	51	0	0	Another paper illustrates this fact : Lowe and Gregory (2010) A sea of uncertainty, Nature, Vol.4, April 2010, doi:10.1038/climate.2010.30 (International Petroleum Industry Environmental Conservation Association (IPIECA))	The Pfeffer study was a perturbed physics experiment. i.e. they perturbed parameters in a glacier model to see what combination of parameters could yield different levels of sea level rise, therefore no accounting was taken of emission scenarios
1635	3	59	51	59	52	The author team should specify the relevant emissions scenarios for this estimate. (IPCC WGII TSU)	the paragraph referred to on page 32 is not comparing model projections and observations for the current era. Rather it is dealing with different approaches to inferring trends in storms from observations and the strong role of climate variability. nevertheless, the first sentence has been modified
1636	3	59	54	59	55	"New studies, whose focus is on quantifying the effect of storminess changes on storm surge, have been carried out over the northern European region since the AR4 and mostly find an increase along the North Sea coastline. This is consistent with increased storminess and wind speed as indicated by most models across this region in Figure 3.9." Well, the "models" may have "indicated" "increased storminess and wind speed", but according to the findings you all reviewed in Chapter 3, page 32, lines 19-28, the observations don't support such a trend. It needs to be stated explicitly that there is difference between model projections and observations for the current era. (UNITED STATES OF AMERICA)	This sentence has been modified

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1637	3	59	54	60	4	The opening sentence of this paragraph seems to overstate the findings reported in this paragraph. Three studies are discussed of which two find some evidence for increased storm surges along specific regions of the North Sea coastline. (CANADA)	made this correction
1638	3	59	58	59	58	change "run" to "runs" (Stocker, Thomas, IPCC WGI TSU)	this change has been made
1639	3	59	59	59	59	More logical to swap order and to write as "between 1961-1990 and 2071-2100" (Stocker, Thomas, IPCC WGI TSU)	changed 'concatenated' to 'joined'
1640	3	59	63	59	63	Suggest not many readers will know what "concatenated" means. (CANADA)	made this change
1641	3	59	63	60	1	change "ensemble of A1B simulations" to "ensemble of simulations using the SRES A1B emission scenario"; delete "model" before "periods" (Stocker, Thomas, IPCC WGI TSU)	The relevant recent refereed literature has been referenced including a new paper by Church et al, 2011 that updates the Rahmstorf et al 2007 paper. The statement regarding uncertainty about the relative contribution of variability and long term trend to this signal is still valid.
1642	3	59	65	59	69	While the IPCC seems keen on listing the rate of sea level rise during different periods with the idea that the rate of sea level rise during the most recent period is greater than over the long term, it would be remiss not to point out that the rate of sea level rise during the satellite era has been declining during the past several years (http://sealevel.colorado.edu/index.php). Thus while in the AR4, the IPCC stated that “Whether the faster rate of increase during the latter period reflected decadal variability or an increase in the longer term trend was not clear.” The recent observed slowdown in the rate of sea level rise is strong indication that natural variability played a large role in the recent upswing in the observed rate of sea level rise. (Knappenberger, Paul, New Hope Environmental Sciences)	this sentence has been reworded to increase clarity
1643	3	60	7	60	9	unclear what is meant by "...an increase...that were 20 to 35% of the upper end of the A1FI scenario" -- please clarify. (Stocker, Thomas, IPCC WGI TSU)	This paper is now cited
1644	3	60	14	0	0	There is similar research for Japanese Bays. Kawai et al. (2006): Improvement of Stochastic Typhoon Model for the Purpose of Simulating Typhoons and Storm Surges under Global Warming, Proc. of the 30th International Conference on Coastal Engineering. The paper compares the changes in the extreme storm surges with the mean sea level rise. I will provide a copy of the manuscript (file name is 'c3-p60-stochastic.pdf' (Kawai, Hiroyasu, Port and Airport Research Institute)	this change has been made
1645	3	60	15	60	16	Cross link to the subsection on tropical cyclones. (Zwiers, Francis, Environment Canada)	this change has been made
1646	3	60	15	60	16	The statement that "current GCMs are unable to realistically represent tropical cyclones" is consistent with the assessment provided in section 3.4.4 of the Chapter. Suggest to refer to Chapter 3 here for more details. (Stocker, Thomas, IPCC WGI TSU)	Have included reference to similar work in Japan
1647	3	60	16	60	19	Presumably alternative approaches have been used in many places, not just Australia. (Zwiers, Francis, Environment Canada)	The summary section has been shortened
1648	3	60	28	60	41	Section: 3.5.3. Extreme Sea Levels. The summary of this section could be substantially improved in a more concise manner. (Mokssit, Abdalah, Direction de la Météorologie Nationale (DMN))	The summary section has been shortened and mention of the North Sea studies has been removed.
1649	3	60	31	60	34	The statement about evidence for an increase in extreme sea levels in the North Sea due to an increase in storminess seems overstated (as on page 59 line 54-55). Also, in this concluding paragraph, the fact that only a few studies projecting changes in extreme sea levels have been done since the AR4 should be noted, with the majority of those few focusing on Europe. (CANADA)	this change has been made
1650	3	60	35	60	37	delete "has been found"; sentence states that "Other studies ....find that..." (Stocker, Thomas, IPCC WGI TSU)	have reworded to extreme sea levels
1651	3	60	36	60	36	what does the term "total water level" mean here? This term is not introduced conceptually in the previous text in 3.5.3 (UNITED STATES OF AMERICA)	This is mentioned in the introductory paragraph
1652	3	60	38	0	0	Somewhere in the discussion, (maybe doesn’t need to be in summary) you should mention that not every location on the globe is very likely to experience a local sea level rise by 2100, as there are some regions where local uplift of the terrain may exceed the climate change-induced change in local sea level there, etc. (UNITED STATES OF AMERICA)	We take this as a comment. No change
1653	3	60	43	0	0	Some conclusions, but taking in account the incertitude on wind, no robust result. (BOURRELIER, PAUL-HENRI, AFPCN)	noted. Thanks.
1654	3	60	43	62	37	Section 3.5.4 Waves has now a well-balanced text. No further comments (Eide, Lars Ingolf, Det Norske Veritas)	Have added a sentence to the introduction to mention this and refer to the relevant sections
1655	3	60	43	62	37	I was surprised that the issue of sea ice retreat allowing generation of wintertime waves was not mentioned in this section (it comes up a bit later in chapter text). That is, that sea ice can suppress wave generation seems to me a worthwhile point to mention in this section. (MacCracken, Michael, Climate Institute)	this comment is not relevant to the ocean waves section
1656	3	60	43	62	37	Add and discuss new references on the European summer 2010 heatwave:Barriopedro et al, Science Express, 17 March 2010 and Dole et al, GRL, in press (UNITED STATES OF AMERICA)	this reference has been added and discussed
1657	3	60	43	62	37	Add and discuss new reference on trends in waves over the last 23 years: Young et al, Science Express, 24 March 2011 (UNITED STATES OF AMERICA)	noted. Thanks.
1658	3	60	45	62	37	Section 3.5.4 has now the well-balanced text. Changes introduced are fine. No further comments (Bitner-Gregersen, Elzbieta Maria, Det Norske Veritas AS)	The sentence discussing the Dodet study has been moved to before the sentence discussing the Weisse and Gunther study

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1659	3	60	53	60	53	Dodet et al. is is focused on the eastern atlantic. This sentence should be moved to the relevant paragraph discussing trends in waves patterns in the last 50 years (i.e. p. 61, lines 1 to 32); (MODARESSI, HORMOZ, BRGM)	Have changed 'relative' to 'mean'
1660	3	60	56	60	56	Is 'relative sea level' different from 'mean sea level'? According to the SREX glossary it is the same thing (although glossary could be wrong!). This is the only time in chapter 3 that 'relative sea level' is used, so for consistency it would be good if 'mean sea level' was used here instead. (Stocker, Thomas, IPCC WGI TSU)	Due to constraints on space,we have elected to include only the Le Cozannet et al 2011 paper since the results of it are consistent with Dupuis etal and the Dupuis et al paper pre-dates the AR4
1661	3	61	1	61	32	Since a discussion on climate variability influence on sea wave parameters is included, a discussion on waves periods trends could be introduced : "Dupuis et al. (2006) investigated the relationship between sea wave parameters and teleconnection patterns. They analysed a 20-year time series from a waverider buoy moored at 26 meters depth off of Biscarosse in the Bay of Biscay and found a positive correlation between the NAO and the wave period, but could not relate the NAO to sea wave heights. Dupuis, H., D. Michel, and A. Sottolichio, (2006): Wave climate evolution in the Bay of Biscay over two decades. J. Mar. Syst, 63, 105-114. (MODARESSI, HORMOZ, BRGM)	Have included this reference.
1662	3	61	1	61	32	Since a discussion on climate variability influence on sea wave parameters is included, the conclusions of Le Cozannet et al. 2011 could be added: this analysis shows that "most of the increase in annual mean sea-wave height since the 1970s in the Bay of Biscay has occurred because the relative frequency of occurrence of persistent observable sea states is evolving over time: from 1970 to 2001, energetic northwest swell becomes more frequent than low-energy intermediate sea states. Moreover, anomalies of the relative frequency of occurrence of observable sea states are related to large-scale recurring pressure anomalies: principally, the Northern Atlantic Oscillation (NAO) but also (during winters) the east Atlantic (EA) pattern, as well other teleconnection patterns of the Northern Hemisphere." Le Cozannet, Gonéri, Sophie Lecacheux, Etienne Delvallee, Nicolas Desramaut, Carlos Oliveros, Rodrigo Pedreros, 2011: Teleconnection Pattern Influence on Sea-Wave Climate in the Bay of Biscay. J. Climate, 24, 641–652. doi: 10.1175/2010JCLI3589.1. (MODARESSI, HORMOZ, BRGM)	The paragraph has been split in two
1663	3	61	1	61	32	This is a very long paragraph. (MacCracken, Michael, Climate Institute)	Usefull to retain VOS - but explained in text here
1664	3	61	4	61	4	Delete 'VOS' - this abbreviation is not used again in SREX. (Stocker, Thomas, IPCC WGI TSU)	done
1665	3	61	19	61	20	Cross link to the subsection on tropical cyclones. (Zwiers, Francis, Environment Canada)	Have clarified the regions
1666	3	61	22	0	0	These positive trends concern the US west coast ? This is not clear as one of the paper cited (Adams et al, 2008) concerns South California. (International Petroleum Industry Environmental Conservation Association (IPIECA))	Details available in referece already cited.
1667	3	61	34	61	42	This requires a reference for C-ERA-40 (would that be Sterl and Caires, 2005, Int J Climatol, 25, 963–977?). Also, I think it would be useful to have some assessment of the sources of the trends - there are likely inhomogeneities in both the reanalysis data and observational data. (Zwiers, Francis, Environment Canada)	this reference has been added
1668	3	61	36	61	37	The "Wang et al. 2006a" can be cited right after "renalysis products", because this study shows problems of temporal homogeneity in renalysis products (ERA40 and NCEP) for the SH. (Wang, Xiaolan, Environmen Canada)	details already in references cited
1669	3	61	39	61	39	The corrected ERA-40 reanalysis is (C-ERA-40), please provide reference or relevant explanation for this data set (how does it differ from the ERA40). (CHINA)	This has been done
1670	3	61	47	61	48	Please cite references in relation to this link to increasing number of hurricanes. (Stocker, Thomas, IPCC WGI TSU)	This sentence has been reworded to clarify more specifically what was said in Meehl et al (2007)
1671	3	61	56	61	58	If increases in wave height were projected in some places due to projected shifts in storm tracks, wouldn't that mean that decreases in wave height would have been projected elsewhere? (Zwiers, Francis, Environment Canada)	This sentence has been reworded to clarify more specifically what was said in Meehl et al (2007)
1672	3	61	56	61	58	It is stated that there will be an increase in extreme wave height for many regions due to a projected northward (should this be poleward?) movement of storm tracks. But if this is simply the case, there will equally be many regions where there is a decrease in extreme wave height, and this should be pointed out. If a more poleward storm track is intrinsically associated with more extreme wave heights than a storm track located nearer the equator, this should be pointed out. (Global Climate Observing System Steering Committee)	low confidence now italicised
1673	3	61	60	61	60	It would be helpful to italicize "low confidence" on this line to indicate use of calibrated uncertainty language. (IPCC WGII TSU)	this change has been made
1674	3	62	2	62	2	More logical to swap order and to write as "between 1979-2004 and 2075-2100" (Stocker, Thomas, IPCC WGI TSU)	It was a spatial average of the average of the top 10 values over a 25 year time slice. This has been clarified
1675	3	62	3	62	3	top ten values' - over what time period? Per year? (Stocker, Thomas, IPCC WGI TSU)	It was a spatial average of the average of the top 10 values over a 25 year time slice. This has been clarified
1676	3	62	3	62	5	Do you think the average of the top 10 values is a robust measure of extremes? What period is used to identify the top ten values - is this the 10 largest values in each of the 26-year periods considered by these authors, or the top ten values per year, or something else? (Zwiers, Francis, Environment Canada)	The study attributes the increase in the extreme wave height to an increase in strong tropical cyclones while changes in Hs largely reflect changes in wind patterns which would reflect the movement of storm tracks over the winter months as well.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1677	3	62	5	62	5	Is there a reasonable explanation for the apparent change in shape of the wave height distribution in the Indian Ocean? I infer a change in shape because the text indicates that the 66th percentile decreased (i.e., SWH decreased), but that the most extreme values increased. Would this be a plausible result of fewer TCs overall, but intensification of the strongest TCs? (See subsection on TCs). If so, it might be worth making that linkage. (Zwiers, Francis, Environment Canada)	this has been done
1678	3	62	9	62	9	Make clear that this is evidence for positive PROJECTED trends. (Zwiers, Francis, Environment Canada)	this sentence has been reworded so as not to imply that this is Hemer's assessment of the previous studies
1679	3	62	12	62	25	Do you agree with Hemer's assessment? (Zwiers, Francis, Environment Canada)	The first sentence of the summary makes the connection between new studies and previous studies on wave trends. The third sentence has now been removed
1680	3	62	27	62	37	Summary quick to state that there is only one study detecting link between SWH in northern high latitudes but does not state reinforcement of statistically significant trends in SWH in mid-latitudes in post AR4 studies (pg 61, lines 1-9) which is an important finding. (UNITED KINGDOM OF GREAT BRITAIN AND NORTHERN IRELAND)	This text has been amended to say 'There is low confidence that there has been an anthropogenic influence on extreme wave heights due to insufficient number of studies at this time. '
1681	3	62	29	62	32	I don't think this conveys quite the right impression of the state of the science. There has been only one study, that I am aware of, that has asked the question as to whether external forcing has caused a change in SWH. Saying that "Only one study has detected ..." could mean that there are several studies that have attempted detection and attribution, and that only one has succeeded, which is not the case. I think it would be better to say that confidence is low because there is an insuffienct number of studies. (Zwiers, Francis, Environment Canada)	The summary has been modified to reflect this
1682	3	62	29	62	32	If there is only one available study, then this is limited evidence 'insufficient literature', and no level of confidence should be assigned (and the results of this study should not be elevated to the concluding bold statement). (Stocker, Thomas, IPCC WGI TSU)	Indeed. We interpret this as a comment as this point is made in the summary comments of this section.
1683	3	62	39	0	0	Anthropic pressure can generate very important impacts ; the phenomenons are very complex. It is impossible to characterise one evolution of coastal line. (BOURRELIER, PAUL-HENRI, AFPCN)	this has been done
1684	3	62	41	62	41	It would be best to refer to "disaster risk management" here for consistency with the glossary and the rest of the report. (IPCC WGII TSU)	This has now been mentioned
1685	3	62	41	62	42	Salt water intrusion in coastal aquifers could also be mentionned (MODARESSI, HORMOZ, BRGM)	large scale has been inserted
1686	3	62	47	62	47	Modes of LARGE SCALE variability such as ENSO (Stocker, Thomas, IPCC WGI TSU)	even has been inserted
1687	3	62	49	62	49	Insert "even" before "in the absence of ...". (Zwiers, Francis, Environment Canada)	the issue this point is making is that rising sea levels will in general cause recession of a sandy shoreline all other factors being equal. The controversy around the Bruun rule is to do with the amount of recession that may occur since for many (most) shorelines, it is complicated by more factors than are accounted for in the Bruun rule. therefore I have elected to instead cite Ranasinghe and Stive (2009) who discuss the issues and the debate over the use of the Bruun rule.
1688	3	62	51	62	51	This sentence should be justified by a publication. If it is justified by Bruun 1962, 1983, 1988, then, the Cooper and Pilkey discussion on its applicability should be also mentionned; Bruun P (1962) Sea level rise as a cause of shore erosion. J Waterw Harb Div 88:117 Bruun, P., 1983. Review of conditions for use of the Bruun Rule of erosion. Coastal Engineering 7, 77– 89. Bruun, P., 1988. The Bruun Rule of erosion by sea-level rise: a discussion of large-scale two- and three-dimensional usages. Journal of Coastal Research 4, 627–648. Cooper J. A. G.,Pilkey O. H. (2004) - Sea-level rise and shoreline retreat; time to abandon the Bruun rule. Global Planet. Change, 43, p. 157-171. (MODARESSI, HORMOZ, BRGM)	The 6th dot point now reads '6. The loss of natural protective structures such as coral reefs (e.g., Sheppard et al., 2005; Gravelle and Mimura, 2008) due to increased ocean temperatures (Hoegh-Guldberg, 1999) and ocean acidification (Bongaerts et al., 2010) or the reduction of permafrost or sea ice in mid and high latitudes, which exposes soft shores to the effects of waves and severe storms (see 3.5.7, Manson and Solomon, 2007).'
1689	3	62	59	62	59	Mention the potential degrading role of ocean acidification on reefs (UNITED STATES OF AMERICA)	while this is now mentioned in the opening paragraph, the impacts are mainly human and ecosystem which is dealt with in chapter 4
1690	3	63	0	63	0	The relevant discussion does not mention among the implications of climate change the impact of sea level rise on coastal aquifers. While sea level rise alone may not cause a significant change in seawater intrusion in coastal aquifers, the combination of sea level rise and reduced recharge may increase seawater intrusion markedly, especially if the slope of the aquifer base is small. The implications for the water supply of coastal regions, particularly of small islands in arid or semi-arid climates that are located far from the mainland, would be significant. (GREECE)	have modified a sentence to now read 'For example, reduced protection from high waves during severe storms could occur as a result of depleted mangrove forests or the degradation of coral reefs (e.g., Gravelle and Mimura, 2008) or loss of sea ice or permafrost (e.g. Manson and Solomon, 2007); '
1691	3	63	1	63	18	It would seem the situation in the Arctic could be mentioned here. (MacCracken, Michael, Climate Institute)	this has been done
1692	3	63	8	63	8	remove 3rd use of 'may' -> '....factors which affect disaster responses.' (Stocker, Thomas, IPCC WGI TSU)	changed 'risks of climate change' to and these will amplify the risks brought about by climate change

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1693	3	63	11	63	12	Do the authors mean the loss of ecosystem services amplifies the "risks of climate change"? As stated, this would mean the loss of those services is expected to change the frequency or magnitude of specific climatic changes. If what is meant is the risk of hazards or negative impacts associated with climate change, then rephrasing is required. In general, it may be required to search for this phrase "risks of climate change" and replace with phrasing consistent with definitions for key terms from Chapter 1 and elsewhere. (CANADA)	have added 'or falling'
1694	3	63	14	63	14	Post-glacial rebound can also cause some regions to fall (if I recall, this is happening in places along Canada's east coast). Material beneath the crust that is advected into the area under the section that is rising must come from somewhere ... (Zwiers, Francis, Environment Canada)	Range of values of SLR is now cited
1695	3	63	20	63	20	Page 58, line 47 says 130 m rather than the 120 m stated here. (Zwiers, Francis, Environment Canada)	Cross-cited section 3.5.3 for discussion of this.
1696	3	63	20	63	20	On page 58, line 47, you quote a different source and report 130 m. Please be consistent. (Stocker, Thomas, IPCC WGI TSU)	This would duplicate what has been written in 3.5.3 and so instead a reference to this section has been included
1697	3	63	20	63	23	It would seem worthwhile to devote a bit more text to the changes of sea level over the Holocene, etc. (MacCracken, Michael, Climate Institute)	a reference to Forbes et al (2004) has been added
1698	3	63	25	63	45	In Canada, seasonal reductions in sea-ice have been associated with coastal erosion, for example, along the north shore of the Gulf of St. Lawrence. See references in Canadian adaptation report available from http://adaptation.nrcan.gc.ca/assess/2007/ (Zwiers, Francis, Environment Canada)	The citation has been moved to this sentence rather than the one following.
1699	3	63	29	63	32	Please cite references in relation to this Caribbean sentence. (Stocker, Thomas, IPCC WGI TSU)	The explanation of the Webb and Kench study has been expanded and an additional reference added
1700	3	63	36	63	41	This is an interesting finding that is contrary to popular impression and very policy relevant. It requires further elucidation or explanation of the observed trends. (UNITED STATES OF AMERICA)	have added per 'decade' after 0.1 to 5.6 hectares
1701	3	63	39	63	39	I don't understand how a range of hectares is a rate of growth of islands--over what time. And is this some percentage of island areas? What is presumably needed is the rate of linear accretion into the ocean or something comparable across islands. (MacCracken, Michael, Climate Institute)	the Webb and Kench study has been explained in more detail to address these points
1702	3	63	41	63	41	I don't quite understand what "net lagoonward migration of islands" means--does this mean filling in of the lagoons? And is this a result of a natural process or development? (MacCracken, Michael, Climate Institute)	This study is now cited.
1703	3	63	42	63	42	Re: Section on observed coastal changes. You should probably add the work of Dawson and Smithers (2010. Shoreline and beach volume change between 1967 and 2007 at Raine Island, Great Barrier Reef, Australia. Global and Planetary Change, 72, 141–154.) which found a small accretion of beach are to a coral island in the Great Barrier Reef. They report “Despite the recent concern that Raine Island is rapidly eroding, our data demonstrate net island growth (6% area, 4% volume) between 1967 and 2007. Perceptions of erosion probably reflect large morphological changes arising from seasonal, inter-annual and inter-decadal patterns of sediment redistribution rather than net loss from the island’s sediment budget.” (UNITED STATES OF AMERICA)	We take this as a comment. No change
1704	3	63	50	0	0	Same considerations for small islands. (BOURRELIER, PAUL-HENRI, AFPCN)	Reject. The point of this box is to address a comment on an earlier draft which requested more information on extremes for small island states
1705	3	63	50	65	2	BOX 3.3: Recommend this Box on small islands focus on issues related to their exposure to changes in extremes affecting the marine environment, rather than also attempting to provide a summary of trends and projections for as many variables as possible (e.g. temperature and precip extremes). One option would be to delete the first couple of paragraphs and start the Box on page 64 lines 23. (CANADA)	The introductory paragraph has been modified to reiterate the particular potential impacts of sea level rise for small islands.
1706	3	63	50	65	2	It might be a useful to add a paragraph profiling a few observed climate extremes related to sea level rise on small islands (i.e., sea level rise, erosion, groundwater penetration by seawater). The section in general seems to pay too little attention to the issue of sea level rise. (UNITED STATES OF AMERICA)	have reworded with 'Small Island States' which is also consistent with Chapter 4
1707	3	63	52	63	52	Sorry to be pedantic, but small islands can exist in lakes also, so they do not necessarily have a maritime climate. (Global Climate Observing System Steering Committee)	Made this change
1708	3	63	58	63	58	resolve' - 'represent' (Stocker, Thomas, IPCC WGI TSU)	Made this change
1709	3	64	17	64	17	will -> would (Stocker, Thomas, IPCC WGI TSU)	Removed 'In the Pacific'
1710	3	64	23	64	24	Wouldn't this be true more or less everywhere? (Zwiers, Francis, Environment Canada)	Reject. This construction works OK here and shortens the description.
1711	3	64	27	64	28	Another example of the annoying practice of constructing double sentences with replacable words. (Zwiers, Francis, Environment Canada)	Made this change
1712	3	64	38	64	38	"Anthropogenic-induced" is redundant; "anthropogenic" would be preferable. (IPCC WGII TSU)	This paragraph has been rewritten to eliminate repetition. The changes in ocean circulation and biological cycling have been mentioned.
1713	3	64	43	64	55	Oceanic oxygen changes are not just expected from warming-driven solubility changes, but also from changes in ocean circulation (ventilation) and changes in the biological cycling of organic material -- see details in Keeling et al. 2010 and references therein. BTW, there seems to be some repetition in this section. (Stocker, Thomas, IPCC WGI TSU)	this change has been made
1714	3	64	48	64	48	Should refer the reader here to Chapter 4 (Section 4.3.5.5) which gives more details on coral bleaching. (Stocker, Thomas, IPCC WGI TSU)	This paragraph has been rewritten to shorten and eliminate repetition. The sentence in question has been removed.
1715	3	64	51	64	52	this statement is confusing (in this context) and needs clarification (UNITED STATES OF AMERICA)	this change has been made

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1716	3	64	57	64	59	Please italicize the 3 confidence terms used here. (Stocker, Thomas, IPCC WGI TSU)	this change has been made
1717	3	64	57	64	59	Confidence assignments (per the AR5 Guidance Note on Treatment of Uncertainties) in these sentences should be italicized. (IPCC WGII TSU)	Here we are reporting confidence in the sign of the change. To quote values would be placing unwarranted focus on the only two available studies (one for Pacific and one for Caribbean) therefore no change.
1718	3	64	59	64	59	Quantify the projected temperature increases, ie, how much? (Stocker, Thomas, IPCC WGI TSU)	have added 'across the small island regions considered here' to clarify that we don't mean the regions where anthropogenic warming has been detected.
1719	3	64	59	64	60	It is stated here that there is insufficient evidence to assess trends in rainfall. This is to be compared with page 57 of Chapter 3, lines 19 and 20, where it is stated that anthropogenic warming has detectably influenced components of the hydrological cycle such as precipitation. The two statements could be reconcilable, but if so this should be explained. (Global Climate Observing System Steering Committee)	this change has been made
1720	3	64	62	64	62	risks' should be 'impacts'. (Stocker, Thomas, IPCC WGI TSU)	changed 'risks' to 'impacts'
1721	3	64	62	64	62	The phrase 'risks of climate change' is not used properly here since it would seem what is meant is the need to understand the consequences or hazards associated with climate change. (CANADA)	OK this sentence has been removed
1722	3	64	62	65	2	I think this assessment of the consequences of not doing science is beyond the scope of this report. The task here is to assess the science, not the information needs for adaptation. (Zwiers, Francis, Environment Canada)	This sentence has been deleted
1723	3	65	1	0	0	These comments on the societal consequences of our lack in knowledge have not been provided in earlier summary paragraphs (e.g. where tropical cyclones have been discussed). Either delete or provide elsewhere too. (Klein Tank, Albert, KNMI)	this has been done earlier in box. Does not seem appropriate in this summary.
1724	3	65	2	65	2	It would be useful at the end here to refer the reader to Chapter 9 (Case study 9.2.9) which deals with adaptation issues for Small Island Developing States. (Stocker, Thomas, IPCC WGI TSU)	Have rephrased to say that 'The AR4, stated with very high confidence that the impact of climate change on coasts is exacerbated by increased pressures on the physical environment arising from human settlements in the coastal zone (Nicholls et al., 2007).
1725	3	65	7	0	0	Human pressure exacerbated on coasts is not relevant to the subject of the chapter. Should be displaced to an another chapter. (BOURRELIER, PAUL-HENRI, AFPCN)	I have removed the words 'consistent with that assessment'
1726	3	65	7	65	10	I find the second part of this paragraph inconsistent with the very high confidence statement given in the AR4, rather than 'consistent' as stated by the author. Some rewording is needed for clarity. Please also cite a specific AR4 chapter (line 7). (Stocker, Thomas, IPCC WGI TSU)	Nicholls et al (2007) has been added
1727	3	65	7	65	10	The relevant AR4 chapter citation for this paragraph should be provided. (IPCC WGII TSU)	the word risks has been replaced with hazards
1728	3	65	12	65	12	Although this sentence is an exact replication of the wording from the AR4, the use of the word 'risks' here is not appropriate. Impacts would be a better word or hazards. (CANADA)	In previous reviews,it has been requested not to discuss programs
1729	3	65	18	65	40	The programm for european assessment for shoreline Erosion (Eurosion, 2004, www.eurosion.org) could be mentionned here (line 22) (MODARESSI, HORMOZ, BRGM)	this reference has been added
1730	3	65	18	65	40	It could be added that: 'In France, Vinchon et al. (2009) evaluated the vulnerability of various coastal systems, defined by their geomorphological patterns and their exposure to hydrodynamical forcing". Vinchon C., Aubier S., Balouin Y. et al. (2009). Anticipate response of climate change on coastal risks at regional scale in Aquitaine and Languedoc Roussillon (France). Ocean & Coastal Management 52 (2009) 47–56. (MODARESSI, HORMOZ, BRGM)	changed 'reviewed' to "considered".
1731	3	65	20	65	20	The sentence that begins with "Two types of studies are reviewed" misrepresents the purpose of this report. The task is to do an assessment, not to provide a review. (Zwiers, Francis, Environment Canada)	have reworded to use neutral language
1732	3	65	26	65	26	I suggest using neurtral language rather than expressions that indicate judgements of the severity of impacts ("already experience problems"). Instead of saying that problems are being experienced, one could simply say that large parts of the coasts of Great Britain are experiencing phenomena such as sediment starvation, etc. (Zwiers, Francis, Environment Canada)	added 'since 1954'
1733	3	65	28	65	30	The relevant time frame for this reported increase should be mentioned. (IPCC WGII TSU)	this reference was added to touch on regions that were identified as requiring more attention in earlier reviews. Have left in but reference only land area vulnerable since this is a physical impact
1734	3	65	37	65	40	This final sentence strays beyond the scope of Chapter 3, and should be deleted. (Stocker, Thomas, IPCC WGI TSU)	The relevance of this study was that it calculated return period maps and inundation extent on a seasonal basis. The application to bird breeding grounds was just an example of the application and so I have removed reference to this.
1735	3	65	44	65	46	Does this belong in Chapter 4? The main consideration seems to have been an ecosystem impact. (Zwiers, Francis, Environment Canada)	Agreed, I have rewritten this sentence to only cite the relevant aspects of the physical modelling

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1736	3	65	44	65	46	assessment of "species vulnerability" and "bird breeding cycles" (Bernier et al.) belongs in Chapter 4. (Stocker, Thomas, IPCC WGI TSU)	The use of a monetary unit was a metric chosen by the authors for evaluation. it is not central to the methodological findings that come out of applying a monte-carlo approach to yield more information on extreme impacts in this study. Therefore have deleted '(in monetary terms)'
1737	3	65	50	65	54	Does discussion of research that evaluates monetary losses belong in this chapter? (Zwiers, Francis, Environment Canada)	replaced 'assumed a plausible' with 'constructed a'
1738	3	65	51	65	51	what's a "plausible probability distribution to the range of future sea level rise estimates" -- please clarify. (Stocker, Thomas, IPCC WGI TSU)	The use of a monetary unit was a metric chosen by the authors for evaluation. The paper uses a Monte-Carlo approach to estimate impacts and show that the stochastic approach better accounts for extremes. The use of monetary units to express the results is not central and therefore reference to it has been deleted i.e. '(in monetary terms)'
1739	3	65	52	65	54	discussions of losses is beyond the scope of Chapter 3 --> belongs in Chapter 4. (Stocker, Thomas, IPCC WGI TSU)	the sentence now reads 'Hunter (2010) combined sea-level extremes evaluated from observations with projections of sea level rise to 2100 and showed that, for example, planning levels in Sydney, Australia would need to be increased respectively by 0.3m and 0.45 m to allow for an 80% and 30% likelihood of flooding.'
1740	3	65	54	65	56	You describe what Hunter (2010) did, but not their results. Please extend this sentence or if the results from Hunter (2010) are not relevant, delete this sentence. (Stocker, Thomas, IPCC WGI TSU)	These lines have been deleted
1741	3	65	56	65	59	If the methods from Callaghan et al. have not yet been applied in a climate change context, is it necessary these three lines regarding this study are included? (Stocker, Thomas, IPCC WGI TSU)	reworded to 'Along a section of the southeast coast of the U.K. '
1742	3	65	62	65	62	"UK East Anglia Coast" -- is this a good enough and widely understood description of the region of interest? (Stocker, Thomas, IPCC WGI TSU)	reworded to 'On the basis of modelling the 25 year beach response to various climate change scenarios'
1743	3	66	1	66	1	Please be more specific about the scenarios here: "On the basis of modelling various climate change scenarios over the next 25 years..." (Stocker, Thomas, IPCC WGI TSU)	reworded to Coelho et al. (2009) concluded that a stormier wave climate led to higher rates of beach erosion than mean sea level rise along a stretch of the Portuguese coast
1744	3	66	2	66	2	"less important" in what sense? (Zwiers, Francis, Environment Canada)	the ES states 'There is high confidence that locations currently experiencing adverse impacts such as coastal erosion and inundation will continue to do so in the future due to increasing sea levels, all other contributing factors being equal.'
1745	3	66	6	0	0	Results from this section are not mentioned in the executive summary (International Petroleum Industry Environmental Conservation Association (IPIECA))	Done
1746	3	66	6	66	6	Change "...the coast are low in regional...." to '...the coast are limited in regional...' (Stocker, Thomas, IPCC WGI TSU)	this part of the statement assessing quality has been removed. The low confidence in the influence of anthropogenic climate change is due to insufficient evidence
1747	3	66	6	66	8	I think this assessment needs work. There are probably many more regional studies than have been considered. Also, the rather broadbrush assessment of the quality of data and methods is worrisome (where are the supporting evaluations of data quality and methods?). (Zwiers, Francis, Environment Canada)	We feel that there is insufficient evidence to suggest that anthropogenic climate change is a major cause of observed changes in the literature that was surveyed. With regard to the Nicholls et al assessment that 'there is very high confidence that the impact of climate change is exacerbated by increasing human induced pressures' we note that although such an assessment is conceivable if one considers for example the issue of sediment starvation due to dam construction exacerbating coastal erosion and such like. However, literature along these lines has not been reviewed in this instance and so such an assessment cannot be made.
1748	3	66	6	66	12	Some clarification required as to the difference between exacerbation and cause. Essentially, has the message changed from AR4? pg 65 line 7 - 'very high confidence ... impact of climate change on coasts exacerbated by increasing human-induced pressures' Summary - 'low confidence that anthropogenic cc has been a major cause of observed changes'. (UNITED KINGDOM OF GREAT BRITAIN AND NORTHERN IRELAND)	Seems like grey literature - not clear it would add suefully to section.
1749	3	66	8	66	8	There is actually higher confidence that sediment supply is a major driver for shoreline movements. There are GIS tools to represent this (Rosati and Klaus, 1999). Rosati, J.D. and N.C. Kraus, 1999: Sediment Budget Analysis System (SBAS). US Army Corps of Engineers, Coastal Engineering Technical Note IV-20, 14p. (MODARESSI, HORMOZ, BRGM)	this clause has been deleted.
1750	3	66	8	66	8	Change "...causes is also low...." to '...causes is also limited..' (Stocker, Thomas, IPCC WGI TSU)	Comment noted.

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1751	3	66	10	66	12	I think this is reasonable - albeit perhaps stated with too high a level of confidence given the rather cautious approach in many other parts of this chapter and the limited supporting coverage in the text. (Zwiers, Francis, Environment Canada)	Due to constraints on space, it is not possible to add this additional material
1752	3	66	13	0	0	Should this section also include information on observed and future expected changes of closed seas such as the Caspian Sea and Black Sea. This could be with an additional Box 3.4 equivalent to 3.3 for small islands. Several studies have been undertaken, for example, for the Caspian Sea on both observed and future changes using GCM/RCMs : Arpe, K., Leroy, S.A.G. (2007), The Caspian Sea Level forced by the atmospheric circulation, as observed and modeled. Quat. Int., 173-174, 144-152; Elguindi N, Giorgi F (2007), Simulating future Caspian sea level changes using regional climate model outputs. Climate Dynamics, 28:365-379; Elguindi N. et al. (2009), Climate change evolution of the hydrological balance of the Mediterranean, Black and Caspian Seas: Impact of climate model resolution. Climate Dynamics, doi : 10.1007/s0382-009-0715-4; Renssen H. et al (2007), Simulating long-term Caspian Sea level changes: The impact of Holocene and future climate conditions. Earth and Planetary Science Letters, 261, 685-693 (International Petroleum Industry Environmental Conservation Association (IPIECA))	Agree. No action
1753	3	66	14	0	0	The link between catastrophic phenomenons and climatic evolutions (essentially only temperature) is evident but can be appreciated only locally. (BOURRELIER, PAUL-HENRI, AFPCN)	The text has been modified to make it clear that the discussion is about possible modulations of seismicity and that the impacts may be seen at a very long time scale.
1754	3	66	14	69	13	We found this subsection very weak and much too long. It seems to be mainly conjecture with little evidence of climate impacts on the process in view. Permafrost and glacier retreat are notable exception to this generalization. (UNITED STATES OF AMERICA)	The suggestion has been considered but it is difficult to make the title right because so many diverse topics have been covered. Nothing has been changed.
1755	3	66	14	69	13	It seems that 3.5.6 is only about alpine areas (including permafrost) and ice sheets, so the title is a bit inappropriate: there's no treatment of geology or geomorphology outside mountains and ice sheets, and permafrost appears in the title of a different section. Text could be reduced considerably, with focus on support of the section summary. (UNITED STATES OF AMERICA)	Agree. These sections are slightly different from other sections in that it is the imapcts of changes in climate rather than impacts of changes in climate extremes are assessed. A reason for this is that the gradiual change in the climate can lead to extreme impacts on the natural physical environment. This is now explained in Section 3.1.1.
1756	3	66	14	70	2	A general remark about Ch3 sections 3.5.6 and 3.5.7 is that while they are thorough and impressive reviews they are mostly (but not quite entirely) about the impacts of change rather than about the impacts of changes in extremes. But perhaps this is inevitable when, as so often, the relevant probability distribution functions are unknown, even for a reference period. (Cogley, J. Graham, Trent University)	agreed, text modified.
1757	3	66	16	66	16	"..and flash floods..." should be replaced with "...and flooding...". There is no reason to focus on flash floods here, and the more general term 'flooding' incorporates a larger range of events, such as hyper-concentrated flows, GLOFs etc, which don't normally fall into the category of 'flash floods'. (Stocker, Thomas, IPCC WGI TSU)	agreed, text modified.
1758	3	66	17	66	17	"Changes in mountain glaciers affect these processes...." should be reworded as: 'Change in the cryosphere affect these processes....". This way, permafrost is also included. (Stocker, Thomas, IPCC WGI TSU)	It is well established that ice-mass wastage following the end of the last glaciation led to increased levels of seismicity. We have added relevant references, also modified text to reflect and point out that climate change signal might be small.
1759	3	66	21	0	0	increased seismicity and volcanic activity - One needs more evidence than is given in the text in the rest of the subsection. What are the mechanisms? How large are the climate signal relative to the other factors (noise)? (UNITED STATES OF AMERICA)	agreed. We have modified the text to make it clear that changes in seismicity and volcanism are at very long time scale.
1760	3	66	21	66	22	The possibility that seismicity and volcanism might be CONSEQUENCES of glacier mass loss and the other drivers will strike many readers as very odd. The mechanism is explained reasonably enough later, but it needs to be introduced briefly here as well. (Cogley, J. Graham, Trent University)	text deleted
1761	3	66	22	66	23	Change "phenomenon" to "phenomena" and "are simulated" to "is simulated". (Cogley, J. Graham, Trent University)	text deleted
1762	3	66	24	66	25	Please provide reference for this statement concerning GCM's. (Stocker, Thomas, IPCC WGI TSU)	text modified and correct reference cited
1763	3	66	27	66	28	Please cite a specific AR4 chapter here. (Stocker, Thomas, IPCC WGI TSU)	We agree it would be nice to include those references but the space is really limited. Rather, we referenced some overview papers here.
1764	3	66	32	66	38	You might want to add this reference (3. Umesh K. Haritashya, Michael P. Bishop, John F. Shroder Jr., Andrew B. G. Bush and Henry N. N. Bulley (2009) Space-based assessment of glacier fluctuations in the Wakhan Pamir, Afghanistan. Climatic Change, Vol. 94, pp 5 – 18.) this was first ever detailed study done for glaciers in Afghanistan which is dealing with acute shortage of freshwater on top of unstable geopolitical situation. Also, this new Scherler et al article is worth mentioning, since this shows spatial varition in glacier response due to climate change (http://www.nature.com/ngeo/journal/v4/n3/full/ngeo1068.html). (Haritashya, Umesh, University of Dayton)	after the last glaciation. We have modified the text.
1765	3	66	33	66	34	Maximum extent over what period of record? (Zwiers, Francis, Environment Canada)	Leclercq et al 2011 is used to replace Oerlemans 2005.

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1766	3	66	34	66	34	Oerlemans 2005: A better reference would be Oerlemans, J., M.B. Dyurgerov and R.S. van de Wal, 2007, Reconstructing the glacier contribution to sea-level rise back to 1850, The Cryosphere, 1(1), 59–65. A still better reference would be Leclercq, P.W., J. Oerlemans and J.G. Cogley, 2011, Estimating the glacier contribution to sea-level rise for the period 1800-2005, Surveys of Geophysics, in press (a preprint of which has been sent to C. Huggel). (Cogley, J. Graham, Trent University)	text modified.
1767	3	66	35	66	35	"many parts of the world" should be strengthened to "most regions of the world". There are very few exceptions, particularly over longer term time-scales. (Stocker, Thomas, IPCC WGI TSU)	agree, text modified.
1768	3	66	48	66	48	"frequency" would be a more focussed term than "occurrence". (Cogley, J. Graham, Trent University)	A clarification is added that it refers to North America
1769	3	66	48	66	50	Does this statement from Clague and Evans refer to a particular region or is it a global statement? (Stocker, Thomas, IPCC WGI TSU)	edited
1770	3	66	50	66	50	Remove the comma after "small". (Cogley, J. Graham, Trent University)	A new sentence and two references have been added that takes this aspect into account.
1771	3	66	50	66	52	No GLOF event since 1998 should aprtly be credited to the measuresd taken by local governments, NGOs, other agencies and also scientific communities. (Haritashya, Umesh, University of Dayton)	This paper does not focus on high mountain regions and is not added.
1772	3	66	58	0	0	Please add Italian author: Reichenbach P., Carrara A. & Guzzetti F., editors (2002) Assessing and Mapping Landslide Hazards and Risk. Natural Hazards and Earth Systems Sciences, Vol. 2: 1-2, 82 p., http://www.nat-hazards-earth-syst-sci.net/special_issue10.html. (de Jong, Carmen, University of Savoy)	agree, text modified.
1773	3	67	3	67	3	could rephrase to: "...especially those that happened before regular satellite observations and seismic monitoring became available." - Seismic monitoring has been very important for establishing the exact timing of many recent landslides so is worth mentioning here. (Stocker, Thomas, IPCC WGI TSU)	agree, text modified, and additional reference added
1774	3	67	4	67	5	I would change the wording here to: "...an apparent increase in large rock avalanches and rockfalls during the past two decades......" or alternatively "....an apparent increase in rock instabilities.....". Focus should not just be on the large events because much interesting information is coming from the documentation of smaller rockfalls. The Fischer et al. 2011b should be moved and cited after "European Alps". Also in relation to the European Alps you should also cite the recent, very convincing study from Ravanel and Deline, 'Climate influence on rockfalls in high-Alpine steep rockwalls' (2010, The Holocene, published online, doi:10.1177/0959683610374887) (Stocker, Thomas, IPCC WGI TSU)	reference not added due to space limitation.
1775	3	67	11	0	0	The number of events has increased in the last decades in the Eastern Italian Alps (Marchi, L.,Tecca, P.R. (2006) Some Observations on the Use of Data from Historical Documents in Debris-Flow Studies Natural)Hazards 38: 301–320 (de Jong, Carmen, University of Savoy)	text modified
1776	3	67	13	67	13	The comma after "climate" in L12 requires a comma after "yield". Insert "of" after "frequency". (Cogley, J. Graham, Trent University)	a few words on the impacts of reduction in glacier are added.
1777	3	67	16	0	0	Is any of the discussion likely related to climate change? (UNITED STATES OF AMERICA)	reference not added due to space limitation.
1778	3	67	16	67	28	Haritashya et al. 2006 (7. Umesh K. Haritashya, Pratap Singh, Naresh Kumar and Yatveer Singh (2006) Hydrological importance of an unusual hazard in mountainous basin: Flood and landslide. Hydrological Processes, Vol. 20, No. 14, pp 3147 – 3154.) shows how high rainfall created artifical moraine and landslide dammed lake in front of the glacier terminus, and bursting of that lake created havoc both in terms of landslides, debris flows and flood in the downstream region. Several such event occurs in Himalayas but rarely gets reported. (Haritashya, Umesh, University of Dayton)	edited
1779	3	67	18	67	18	"largest," --> "largest" (Zwiers, Francis, Environment Canada)	edited
1780	3	67	20	67	20	"averaging", not "averaged". (Cogley, J. Graham, Trent University)	edited
1781	3	67	21	67	21	Insert "and" before "are thought". (Cogley, J. Graham, Trent University)	text deleted
1782	3	67	26	67	28	This last sentence in relation to the Bering Glacier appears badly out of place here. It would be better moved to appear on page 68, line 7, where the glacier - seismicity link is further discussed. (Stocker, Thomas, IPCC WGI TSU)	text deleted.
1783	3	67	26	67	28	How glacier mass loss can "modulate" seismicity (i.e. by unloading the lithosphere) needs to be explained. (Cogley, J. Graham, Trent University)	text modified but reference not added due to limitation in space.
1784	3	67	30	67	31	You should extend this sentence to better describe the physical mechanism linking warming and slope stability, eg, "Warming and degradation of permafrost affects slope stability, through a reduction in the shear strength of ice filled rock discontinuities (Davies et al. 2001)". {The effect of rise in mean annual temperature on the stability of rock slopes containing ice-filled discontinuities, Permafrost Periglacial Processes, 12 (1), 69-77} (Stocker, Thomas, IPCC WGI TSU)	edited
1785	3	67	32	67	32	"triggered", not "trigging". (Cogley, J. Graham, Trent University)	edited
1786	3	67	44	67	44	"inn" --> "in" (Zwiers, Francis, Environment Canada)	edited
1787	3	67	44	67	44	"in", not "inn". (Cogley, J. Graham, Trent University)	Outside scope of SREX.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1788	3	67	55	67	55	Since you discuss potential temperature and precipitation impacts of avalanches and debris flow from melting glaciers, it seems remiss not to discuss the potential impact of warming from black carbon deposition onto the glacial surface (as well as aerosol impacts on precipitation patterns). In regions such as the Himalayas, black carbon deposition has been suggested to have played a major role (Menon et al., 2010, Atmospheric Chemistry and Physics). (UNITED STATES OF AMERICA)	A short explanation has been added. More details can also be found in the previous paragraph.
1789	3	67	57	0	0	Their incidence may increase - Why? (UNITED STATES OF AMERICA)	text modified
1790	3	67	58	67	58	Reword: "Landslide susceptibility is also enhanced by glacier dynamics, which may......." (Stocker, Thomas, IPCC WGI TSU)	A clarification has been added.
1791	3	67	58	67	58	I do not understand "glacier flows". How might they destabilize or oversteepen slopes? (Cogley, J. Graham, Trent University)	text modified
1792	3	67	62	67	62	"....or magnitude of severe rainstorms (see Section 3.3.2) could cause......" (Stocker, Thomas, IPCC WGI TSU)	text modified
1793	3	68	8	68	8	Delete "a" before "wastage". (Cogley, J. Graham, Trent University)	text modified
1794	3	68	10	68	10	Delete "of" after "facilitating". (Cogley, J. Graham, Trent University)	text modified
1795	3	68	13	0	0	Is this statement backed by evidence or is it conjecture? Again on lines 43-45 - same comment. One could argue the other side of the argument. The weight of the ice should make the region more unstable. The fact that it is stable means we have no idea... (UNITED STATES OF AMERICA)	text modified
1796	3	68	13	0	0	Is this statement backed by evidence or is it conjecture? Again on lines 43-45 - same comment. One could argue the other side of the argument. The weight of the ice should make the region more unstable. The fact that it is stable means we have no idea... (UNITED STATES OF AMERICA)	agree. Text modified
1797	3	68	16	68	16	"...that are related to glacier retreat and/or permafrost degradation are...." (Stocker, Thomas, IPCC WGI TSU)	attribution paragraph deleted.
1798	3	68	16	68	17	I think this is a rather strong multi-step attribution statement. Perhaps something like this can be said about the ensemble of such events (perhaps using "more likely than not"?), but as written, the reader would associate "likely" with each of the phenomena listed, plus others that he/she might imagine - I think this is inapporiate. These phenomena occur regionally and locally, at scales where attribution of the causes of temperature changes remains difficult (see, for example, the discussion in WG1 AR4 Ch 9 on the limitations of attribution at regional scales). Should confidence language be used here? (Zwiers, Francis, Environment Canada)	see #1798
1799	3	68	26	68	27	The literature used as the basis of this likelihood statement regarding the formation of new, unstable lakes should be cited. (Stocker, Thomas, IPCC WGI TSU)	No. Text modified
1800	3	68	27	68	27	Should confidence language be used here? (Zwiers, Francis, Environment Canada)	agree. Text modified.
1801	3	68	30	68	31	Suggested rewording: "The zone of warm permafrost (mean annual rock temperature ~- 2 to 0°C), which is more susceptible to slope failures than cold permafrost or completely thawed permafrost (Davies et al. 2001), may rise in elevation......." (Stocker, Thomas, IPCC WGI TSU)	agree, text modified.
1802	3	68	32	68	32	What does raising the elevation of this zone of warm permafrost mean for the hazard? I think you need to add a sentence here saying something like: "This will shift the zone of enhanced instability and landslide initiation, towards higher elevation slopes which in many regions are already significantly steeper, and therefore predisposed to failure (eg, Southern Alps New Zealand - Allen et al. 2011)." (Stocker, Thomas, IPCC WGI TSU)	agree, text modified
1803	3	68	35	68	35	Delete the second "increase". (Cogley, J. Graham, Trent University)	agree, text modified
1804	3	68	40	68	40	Delete "by" (at end of line). (Cogley, J. Graham, Trent University)	second half of sentence deleted.
1805	3	68	40	68	41	For the latter half of this sentence, wouldn't it be better to cite a paper describing projections of temperature extremes and cross-link to previous sections that assessed those projections? (Zwiers, Francis, Environment Canada)	second half of sentence deleted.
1806	3	68	40	68	41	The last part of the sentence should be reworded to; "....and warm extremes are projected to increase in most land areas of the World (See section 3.2.1)." (Stocker, Thomas, IPCC WGI TSU)	This aspect has now be added.
1807	3	68	43	0	0	Increased volcanic activity associated with ice loss may be real, but loss of ice caps from glaciated volcanoes could in fact make them safer. One issue now is that magma-ice interactions are explosive and generate lahars (volcanic mudflows). Loss of ice could decrease the number of lahars. (UNITED STATES OF AMERICA)	agree, text modified
1808	3	68	45	68	45	Delete "a" before "timescales". (Cogley, J. Graham, Trent University)	agree, text modified
1809	3	68	50	68	50	Do not hyphenate "ice unloading". Either change the semicolon to a comma or change "being" to "is". (Cogley, J. Graham, Trent University)	agree, text modified
1810	3	68	51	68	53	Ice thinning will make some ice-covered volcanic eruptions more explosive, but by the same token, ice thinning will make some eruptions less explosive, by removing or reducing ice cover from volcanoes that are currently prone to explosive eruption. (Global Climate Observing System Steering Committee)	agree, text modified.
1811	3	68	57	68	59	"such events" should be changed to "the rainfall rates within tropical cyclones". Also end the sentence with this elaboration: "though the number of tropical cyclones is projected to decrease globally (see section 3.3.4)." (UNITED STATES OF AMERICA)	agree, text modified
1812	3	68	58	68	59	The reference here to tropical cyclones is not precise enough. You need to be consistent with the wording from Section 3.4.4, and say that it is " tropical cyclone-related rainfall rates will 'likely' increase". The current wording you use, could be misinterpreted as saying that cyclone frequency will increase. Note - the citation should be 3.4.4 and not 3.4.5. (Stocker, Thomas, IPCC WGI TSU)	agree, text modified

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1813	3	68	58	68	59	Clarification needed that 'events' relate to rainfall rates as could be interpreted as cyclone frequency, which counters earlier comments. (UNITED KINGDOM OF GREAT BRITAIN AND NORTHERN IRELAND)	agree, text modified
1814	3	68	59	68	60	Delete the commas after "future" and "explosive", and delete "volcanic". (Cogley, J. Graham, Trent University)	agree, text modified
1815	3	68	60	68	60	Cite (section 3.3.2) in relation to increased extreme precipitation events. (Stocker, Thomas, IPCC WGI TSU)	ok agreed
1816	3	68	62	69	2	These last two sentences in relation to Pinatubo are badly out of place here. These are 'observations' and should be moved to page 67, line 26, to follow on logically from the Hurricane Mitch related volcanic debris flow. (Stocker, Thomas, IPCC WGI TSU)	text modified
1817	3	69	6	69	6	Delete "the availability of" and change "rare" to "rarely available". (Cogley, J. Graham, Trent University)	text modified
1818	3	69	7	69	7	Delete "quantitatively" (or move it to follow "trends"). (Cogley, J. Graham, Trent University)	This assessment is because of the influence of other factors on the landslides.
1819	3	69	8	0	0	I do not understand the reasoning for "low confidence". Remember that the reported changes in extreme precipitation (p30 and exec. summary) had the "likely" status (which implies "high confidence"?). (Klein Tank, Albert, KNMI)	Comment does not negate the assessment.
1820	3	69	8	69	10	“There is low confidence in projected changes in the magnitude and frequency of shallow landslides in temperate and tropical regions, as they depend mainly on frequency and intensities of rainfall events and anthropogenic land-use.” As I noted previously, aerosol deposition also likely plays a significant role in some regions. (Knappenberger, Paul, New Hope Environmental Sciences)	Now start with a general assessment of high confidence that some climate changes will affect some high mountain impacts, and increased use of uncertainty terminology through summary.
1821	3	69	10	69	10	Is a likelihood assessment appropriate here? (Zwiers, Francis, Environment Canada)	No; Arctic coastal erosion is included. Retain current title. Also need current title to differentiate with 3.4.6.
1822	3	69	15	0	0	Section 3.5.7: Because this section deals exclusively with permafrost-related impacts, a more appropriately title would be: "High-Latitude Changes in Permafrost" or "High-latitude Permafrost Change". But then, by including the Qinghai-Tibetan Plateau you include regions which are not in the High Latitudes. Some thought needs to be given to the most appropriate title. (Stocker, Thomas, IPCC WGI TSU)	Section 3.4.6 describes permafrost in high mountains, so there is no redundancy.
1823	3	69	15	0	0	Partly redundant with the preceding sub section. (BOURRELIER, PAUL-HENRI, AFPCN)	The justification is now provided in Section 3.1.1
1824	3	69	15	69	58	It would be beneficial for the authors to insert comments to justify the relevance of this section. Given that this document is to focus on climate extremes, there is very little in the discussion about high latitude permafrost that refers to climate extremes (except for coastal erosion) but rather focusses on longer-term changes. There has been documentation of impact of extreme warm years (e.g. 1998) on active layer conditions such as Atkinson et al. (2006, also discusses impacts on entire cryosphere), Smith et al. (2009, 2010) or occurrence of active layer detachments (slope instability) that occurs in response to warm conditions or impacts of wildfires (e.g. Lewkowicz and Harris 2005a,b, Lewkowicz 2007, Lipovsky et al. 2006) or changes to active layer thickness and ground temperature in response to fire which can be related to climate extremes (eg. Smith et al 2008; Viereck et al. 2008) . References: Atkinson, D.E., Brown, R., Alt, B., Agnew, T., Bourgeois, J., Burgess, M., Duguay, C., Henry, G., Jeffers, S., Koerner, R., Lewkowicz, A.G., McCourt, S., Melling, H., Sharp, M., Smith, S., Walker, A., Wilson, K., Wolfe, S., Woo, M.-k., and Young, K. 2006. Canadian cryospheric response to an anomalous warm summer: a synthesis of the Climate Change Action Fund Project "The state of the Arctic Cryosphere during the extreme warm summer of 1998". Atmosphere-Ocean, 44(4): 347-375. Smith, S.L., Romanovsky, V.E., Lewkowicz, A.G., Burn, C.R., Allard, M., Clow, G.D., Yoshikawa, K., and Throop, J. 2010. Thermal state of permafrost in North America - A contribution to the International Polar Year. Permafrost and Periglacial Processes, 21: 117-135. Smith, S.L., Wolfe, S.A., Riseborough, D.W., and Nixon, F.M. 2009. Active-layer characteristics and summer climatic indices, Mackenzie Valley, Northwest Territories, Canada. Permafrost and Periglacial Processes, 20(2): 201-220. Lipovsky, P.S., Coates, J., Lewkowicz, A.G., and Trochim, E. 2006. Active-layer detachments following the summer 2004 forest fires near Dawson City, Yukon. In Yukon Exploration and Geology 2005. Yukon Geological Survey. pp. 175-194. Lewkowicz, A.G., and Harris, C. 2005a. Frequency and Magnitude of Active-layer	It clear now that discussion in 3.5.6 is inclusively for mountain permafrost. Calculation of heat into melting fronzen soils is very prelimarily.
1824.2	3	69	15	69	58	Detachment Failures in Discontinuous and Continuous Permafrost, Northern Canada. In Permafrost and Periglacial Processes. John Wiley & Sons Ltd., pp. p. 115-130. Lewkowicz, A.G., and Harris, C. 2005b. Morphology and geotechnique of active-layer detachment failures in discontinuous and continuous permafrost, northern Canada. Geomorphology, 69: 275-297. Lewkowicz, A.G. 2007. Dynamics of active-layer detachment failures, Fosheim Peninsula, Ellesmere Island, Nunavut, Canada. Permafrost and Periglacial Process, 18: 89-103. Lipovsky, P.S., Coates, J., Lewkowicz, A.G., and Trochim, E. 2006. Active-layer detachments following the summer 2004 forest fires near Dawson City, Yukon. In Yukon Exploration and Geology 2005. Yukon Geological Survey. pp. 175-194. Viereck, L.A., Werdin-Pfisterer, N.R., Adams, P.C., and Yoshikawa, K. 2008. Effect of wildfire and fireline construction on the annual depth of thaw in a black spruce permafrost forest in Interior Alaska: A 36-year record of recovery. In Ninth International Conference on Permafrost. Edited by D.L. Kane and K.M. Hinkel. Fairbanks Alaska. Institute of Northern Engineering, University of Alaska Fairbanks, Vol.2, pp. 1845-1850. Smith, S.L., Burgess, M.M., and Riseborough, D.W. 2008. Ground temperature and thaw settlement in frozen peatlands along the Norman Wells pipeline corridor, NWT Canada: 22 years of monitoring. In Ninth International Conference on Permafrost. Edited by D.L. Kane and K.M. Hinkel. Fairbanks Alaska. Institute of Northern Engineering, University of Alaska Fairbanks, Vol.2, pp. 1665-1670. (CANADA)	

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1825	3	69	15	70	2	There is some overlap with the summary in 3.5.6. Merge discussion of permafrost into this section. Is there an estimate of how much heat is going into melting frozen soils? (UNITED STATES OF AMERICA)	this is now discussed
1826	3	69	15	70	2	There is no discussion of the potential for catastrophic methane or C release from permafrost (cf., Schaefer et al. ref.). This is a scenario that policy-makers are often aware of and interested in, and some cogent discussion should be added. Notice that this issue is touched upon on p. 8, but is not currently assessed in this chapter. (UNITED STATES OF AMERICA)	Not clear what reviewer wants changed. No attempt here to attribute trends/changes in permafrost to either natural or anthropogenic climate change.
1827	3	69	15	70	2	Just a general subjective impression here: there seems to be little appreciation for the possibility of large interdecadal internal variability in the arctic, which could invalidate a simple extrapolation of historical trends that apparently forms a large part of the basis for the conclusions here. I do acknowledge/speculate that the permafrost is something of an integrator and might not be so sensitive to the ups and downs, but I don't get the sense that this has been thought through carefully. (UNITED STATES OF AMERICA)	edited
1828	3	69	17	69	17	Insert "the" before "Arctic" and "Subarctic". (Cogley, J. Graham, Trent University)	updated the references
1829	3	69	17	69	58	Most of the literature cited here dates to 2006 or earlier. There must be new literature, particularly given the effort during IPY. (Zwiers, Francis, Environment Canada)	reject. Term is important and explained in parentheses.
1830	3	69	23	69	23	This is the only instance of "pedogenic" in the chapter, so it might just as well be omitted. (Cogley, J. Graham, Trent University)	This has been modified to indicate that the 3C increase is at the permafrost table level, and discussion on changes in permafrost temperature at greater depth (10-20m) is also added
1831	3	69	25	69	25	I continue to find "temperature at the top of the permafrost" an unsettling quantity. The top of the permafrost is either the ground surface, in which case it would be clearer to speak of the surface temperature, or the frost table, in which case its temperature is a constant (0 deg C) by definition. So the only way I can interpret this is to suppose that it means "mean ground surface temperatures in permafrost regions have increased by up to 3 deg C ...". Then at line 27 and elsewhere in this paragraph, "permafrost temperature" must refer to the temperature PROFILE as a function of depth. (Cogley, J. Graham, Trent University)	This is now cited
1832	3	69	25	69	28	This section should mention recent work in analysing the IPY 2007-09 measurements : Romanovsky et al. (2010) Permafrost Thermal State in the Polar Northern Hemisphere during the International Polar Year 2007–2009: a Synthesis, Permafrost and Periglac. Process. 21: 106–116 (International Petroleum Industry Environmental Conservation Association (IPIECA))	References has been updated, though not all papers mentioned here are added because of space limits.
1833	3	69	25	69	31	Suggest this section be updated with references from more recent publications. Much has been done in polar regions since the AR4, particularly research associated with the International Polar Year. Some of the results of this research have been recently published and provide updated information on trends. In particular, there is new information for North America and Russia. In addition some explanation should be provided for the differeence in rate of increase in permafrost temperatures between discontinuous and continuous permafrost (largely related to latent heat requirements to thaw ice-rich permafrost as temperatures approach 0°C) and Romanovsky et al 2010a provides a discussion relevant to this. Publications include: Smith, S.L., Romanovsky, V.E., Lewkowicz, A.G., Burn, C.R., Allard, M., Clow, G.D., Yoshikawa, K., and Throop, J. 2010. Thermal state of permafrost in North America - A contribution to the International Polar Year. Permafrost and Periglacial Processes, 21: 117-135. Romanovsky, V.E., Smith, S.L., and Christiansen, H.H. 2010a. Permafrost thermal state in the polar Northern Hemisphere during the International Polar Year 2007-2009: a synthesis. Permafrost and Periglacial Processes, 21: 106-116. Romanovsky, V.E., Drozdov, D.S., Oberman, N.G., Malkova, G.V., Kholodov, A.L., Marchenko, S.S., Moskalenko, N.G., Sergeev, D.O., Ukrainsteva, D.G., Abramov, A.A., and Vasiliev, A.A. 2010b. Thermal state of permafrost in Russia. Permafrost and Periglacial Processs, 21: 106-116. Christiansen, H.H., Etzelmuller, B., Isaken, K., Juliussen, H., Farbot, H., Humlum, O., Johansson, M., Ingeman-Neilsen, T., Kristensen, L., Hjort, J., Holmlund, P., Sannel, A.B.K., Sigsgaard, C., Akerman, J., Foged, N., Blikra, L.H., Pernosky, M.A., and Odegard, R. 2010. Thermal state of permafrost in the Nordic area during the IPY 2007-2009. Permafrost and Periglacial Processes, 21: 156-181. Zhao, L., Wu, Q., Marchenko, S.S., and Sharkhuu, N. 2010. Thermal state of permafrost and active layer in Central Asia during the International Polar Year. Permafrost and Periglacial Processes, 21: 198-207. Burn, C.R., and Zhang, Y. 2009. Permafrost and climate change at Herschel Island (Qikiqtaruq), Yukon Territory, Canada. Journal Geophysical Research, 114(F02001): 16. Osterkamp, T.E. 2008. Thermal state of permafrost in Alaska during the fourth quarter of the twentieth century. In Proceedings Ninth International Conference on Permafrost. Edited by D.L. Kane and K.M. Hinkel. Fairbanks. Institute of Northern Engineering, University of Alaska Fairbanks, Vol.2, pp. 1333-1338. Taylor, A.E., Wang, K., Smith, S.L., Burgess, M.M., and Judge, A.S. 2006. Canadian Arctic Permafrost Observatories: detecting contemporary climate change through inversion of subsurface temperature time-series. Journal of Geophysical Research, 111(B02411, doi:10.1029/2004JB003208): 14. (CANADA)	edited, text modified
1834	3	69	28	69	28	Do not capitalize "interior". (Cogley, J. Graham, Trent University)	Agree, text modified. References updated.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1835	3	69	33	69	37	Note that in some areas there are less obvious trends in active layer although there are increases observed during extreme warm years. There has also been research examining the role of organic matter etc. in controlling the response of the active layer to variations in climate. There is also updated information on active layer changes through time for North American that should be cited as Brown (2000) is now out of date. Publications include: Smith, S.L., Wolfe, S.A., Riseborough, D.W., and Nixon, F.M. 2009. Active-layer characteristics and summer climatic indices, Mackenzie Valley, Northwest Territories, Canada. Permafrost and Periglacial Processes, 20(2): 201-220. Smith, S.L., Romanovsky, V.E., Lewkowicz, A.G., Burn, C.R., Allard, M., Clow, G.D., Yoshikawa, K., and Throop, J. 2010. Thermal state of permafrost in North America - A contribution to the International Polar Year. Permafrost and Periglacial Processes, 21: 117-135. Woo, M.-k., Mollinga, M., and Smith, S.L. 2007. Climate warming and active layer thaw in the boreal and tundra environments of the Mackenzie Valley. Canadian Journal Earth Sciences, 44: 733-743. Burn, C.R., and Kokelj, S.V. 2009. The environment and permafrost of the Mackenzie Delta area. Permafrost and Periglacial Processes, 20(2): 83-105. Nelson, F.E., Shiklomanov, N.I., and Streletskiy, D.A. 2008. A permafrost observatory at Barrow, Alaska: long-term observations of active-layer thickness and permafrost temperature. In Ninth International Conference on Permafrost. Edited by D.L. Kane and K.M. Hinkel. Fairbanks Alaska. Institute of Northern Engineering, University of Alaska Fairbanks, Vol.2, pp. 1267-1272. Nelson, F.E., Shiklomanov, N.I., Hinkel, K.M., and Brown, J. 2008. Decadal results from the Circumpolar Active Layer Monitoring (CALM) Program. In Ninth International Conference on Permafrost. Edited by D.L. Kane and K.M. Hinkel. Fairbanks Alaska. Institute of Northern Engineering, University of Alaska Fairbanks, Vol.2, pp. 1273-1280. Streletskiy, D.A., Shiklomanov, N.I., Nelson, F.E., and Klene, A.E. 2008. Thirteen years of observations at Alaskan CALM sites: long-term active layer and ground surface temperature trends. In Ninth International Conference on Permafrost. Edited by D.L. Kane and K.M. Hinkel. Fairbanks, Alaska. Institute of Northern Engineering, University of Alaska Fairbanks, Vol.2, pp. 1727-1732. (CANADA)	edited
1836	3	69	36	69	36	"and 2000". (Cogley, J. Graham, Trent University)	edited
1837	3	69	37	69	37	Insert "the" before "North American". (Cogley, J. Graham, Trent University)	Agree, text modified. Vallee and Payette reference added.
1838	3	69	37	69	39	This discussion on thermokarst development would benefit from inclusion of information from other regions. There are a number of recent publications documenting thermokarst development in Canada and also collapse and subsidence of frozen peatlands. Vallée, S., and Payette, S. 2007. Collapse of permafrost mounds along a subarctic river over the last 100 years (northern Québec). Geomorphology, 90: 162-170. Payette, S., Delwaide, A., Caccianiga, M., and Beauchemin, M. 2004. Accelerated thawing of subarctic peatland permafrost over the last 50 years. Geophysical Research Letters, 31(L18208). Smith, S.L., Burgess, M.M., and Riseborough, D.W. 2008. Ground temperature and thaw settlement in frozen peatlands along the Norman Wells pipeline corridor, NWT Canada: 22 years of monitoring. In Ninth International Conference on Permafrost. Edited by D.L. Kane and K.M. Hinkel. Fairbanks Alaska. Institute of Northern Engineering, University of Alaska Fairbanks, Vol.2, pp. 1665-1670. Fortier, R., and Aubé-Maurice, B. 2008. Fast permafrost degradation near Umiujaq in Nunavik (Canada) since 1957 assessed from time-lapse aerial and satellite photographs. In Ninth International Conference on Permafrost. Edited by D.L. Kane and K.M. Hinkel. Fairbanks. Institute of Northern Engineering, University of Alaska Fairbanks, Vol.1, pp. 457-462. Beilman, D.W., and Robinson, S.D. 2003. Peatland permafrost thaw and landform type along a climate gradient. In Proceedings Eighth International Conference on Permafrost. Edited by M. Phillips, S.M. Springman, and L.U. Arenson. Zurich, Switzerland. A.A. Balkema, Vol.1, pp. 61-65. Kershaw, G.P. 2003. Permafrost landform degradation over more than half a century, Macmillan/Caribou Pass region, NWT/Yukon, Canada. In Proceedings of 8th International Conference on Permafrost. Edited by M. Phillips, S.M. Springman, and L.U. Arenson. Zurich Switzerland. July 2003. A.A. Balkema, Lisse, pp. p. 543-548. Camill, P. 2005. Permafrost thaw accelerates in boreal peatlands during late-20th century climate warming. Climate Change, Vol. 68: p. 135-152. (CANADA)	Agree, text modified. Lantz and Kokelj reference added.
1839	3	69	37	69	39	Recent studies on thaw slumps adjacent to tundra lakes, including observed increases in occurrence in the Mackenzie Delta region, may also be relevant to this discussion on thermokarst: Kokelj, S.V., Lantz, T.C., Kanigan, J., Smith, S.L., and Coutts, R. 2009. Origin and polycyclic behaviour of tundra thaw slumps, Mackenzie Delta region, Northwest Territories, Canada. Permafrost and Periglacial Processes, 20(2): 173-184. Lantz, T.C., and Kokelj, S.V. 2008. Increasing rates of retrogressive thaw slump activity in the Mackenzie Delta region, N.W.T., Canada. Geophysical Research Letters, 35(L06502): 5. (CANADA)	Agree, however we are limited by the available literature and space limits.
1840	3	69	42	69	45	This is a very, very limited discussion of a very important impact that is happening now--causing villages to be relocated, etc. This determines much more coverage--as much of the erosion occurs during storms, etc. (MacCracken, Michael, Climate Institute)	There are other factors which are discussed in the following paragraph.
1841	3	69	42	69	45	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)	This is now reflected in the text. Taylor et al reference added.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1842	3	69	47	69	50	Taylor et al. (2006) found that for high arctic sites, changes in snow cover could counteract changes in air temperature with respect to impacts on the ground thermal regime. E.g. increases in air temperature may not necessarily lead to increases in ground temperature if there is a decrease in snow cover. Taylor, A.E., Wang, K., Smith, S.L., Burgess, M.M., and Judge, A.S. 2006. Canadian Arctic Permafrost Observatories: detecting contemporary climate change through inversion of subsurface temperature time-series. Journal of Geophysical Research, 111(B02411, doi:10.1029/2004JB003208): 14. (CANADA)	Earlier snowfall in winter does not necessarily result in early or later snowmelt in spring. In general, snowmelt is a very rapid process and its cooling effect on ground is very limited (see Zhang, 2005).
1843	3	69	48	69	51	If snow is falling earlier in autumn, and the thaw season is getting longer, then spring snowmelt must be happening much earlier. Is this correct? If so, it is very surprising and needs to be explained. (Cogley, J. Graham, Trent University)	The reference is deleted. The text is updated with new references
1844	3	69	51	69	53	The elimination of 90% of permafrost by 2100 is a startling projection and needs to be discussed in more detail. As a minimum, "near-surface" should be defined; at a guess, I conjecture that the projection is that 90% of permafrost that is thinner than some number of metres will disappear. If this is correct, say so. Mention also the anticipated end-of-summer active-layer depths that will prevail in regions where the permafrost survives, and the percentage of ALL permafrost that will have been lost when the 90% of thin permafrost has disappeared. At page 92 line 60, complete the Lawrence and Slater reference. (Cogley, J. Graham, Trent University)	We have added the following text with reference: "The projected permafrost degradation may result in significant ancient carbon currently frozen in permafrost to be released into the atmosphere, providing a positive feedback to climate system (Schaefer et al., 2011) ."
1845	3	69	51	69	53	add a citation to the new Schaefer et al 2011 Tellus paper and discuss results and implications (UNITED STATES OF AMERICA)	agree. The text has been modified (see also response to #1844)
1846	3	69	51	69	53	As we recall, the vertical domain of the soil in the cited study was much shallower in extent than most permafrost in the real world. We think the implication that 90% of permafrost area will be lost in this century is not a realistic interpretation of that study. It will take a long time to melt all that ice. The concern for the coming century is the deepening of the active layer. (UNITED STATES OF AMERICA)	No. Seems to be corrrect placement.
1847	3	69	51	69	56	The points made here likely merit attention much earlier in the section. (MacCracken, Michael, Climate Institute)	The sentence is deleted
1848	3	69	52	69	52	"will" is too definitive - suggest replacing "will" with "may". (Zwiers, Francis, Environment Canada)	The Lawrence and Slater (2005) is removed. Text modified. See also response to #1844.
1849	3	69	52	69	53	It is suggested that Lawrence and Slater (2005) not be quoted. There was much criticism of this article as there were a number of issues related to the approach used and the results presented were misleading. There was a lack of accounting of the factors that will affect the rate at which permafrost thaws. Also, loss of near-surface permafrost is poor terminology and what is really meant is an increase in thaw depth. Issues with Lawrence and Slater were clearly documented in Burn and Nelson (2006) which also had input from a number of permafrost scientists. In addition others have produced papers (e.g. Yi et al. 2007, Delisle 2007)in response to Lawrence and Slater to show the importance of other factors and also more realistic predictions of changes in thaw depth (see also Woo et al. 2007). These and other studies do not project the fast or widespread loss of permafrost which also needs to be considered in the wording used for the conclusion of this section. Refereces: Burn, C.R., and Nelson, F.E. 2006. Comment on "A projection of sever near-surface permafrost degradation during the 21st century" by David M. Lawrence and Andrew G. Slater. Geophysical Research Letters, 33(L21503 doi:10.1029/2006GL027077). Delisle, G. 2007. Near-surface permafrost degradation: How severe during the 21st century? Geophysical Research Letters, 34(9): L09503. Yi, S., Woo, M.-k., and Arain, M.A. 2007. Impacts of peat and vegetation on permafrost degradation under climate warming. Geophysical Research Letters, 34(L16504): 5. Woo, M.-k., Mollinga, M., and Smith, S.L. 2007. Climate warming and active layer thaw in the boreal and tundra environments of the Mackenzie Valley. Canadian Journal Earth Sciences, 44: 733-743. (CANADA)	Sea ice impact on coastal erosion discussed in previous paragraph
1850	3	69	53	0	0	P. 69, line 53: Another factor for coastal erosion is related to rapid sea ice loss, such as that observed over the last several years, that can rapidly warm up the adjacent land, with the enhanced warming signal penetrating 1500km inland, and this enhanced terrestrial warming increases permafrost vulnerability and can lead to rapid thaw of permafrost (Lawrence, D.M., A.G. Slater, R.A. Tomas, M.M. Holland, and C. Deser, 2008b: Accelerated Arctic land warming and permafrost degradation during rapid sea ice loss. Geophys. Res. Lett., 35, L11506, doi:10.1029/2008GL033985). (UNITED STATES OF AMERICA)	sentence deleted.
1851	3	69	53	69	54	Are you happy to make such an unequivocal statement ("is responsible")? (Zwiers, Francis, Environment Canada)	reference and sentence deleted.
1852	3	69	54	69	54	Suggest reviewing Atkinson et al. (2006) - we do not believe that this article discusses coastal erosion. Other publications that may be more relevant are: Solomon, S.M. 2005. Spatial and temporal variability of shoreline change in the Beaufort-Mackenzie region, northwest territories, Canada. Geo-Marine Letters, 25: 127-137. Atkinson, D.E. 2005. Observed storminess patterns and trends in the circum-Arctic coastal regime. Geo-Marine Letters, 25: 98-109. Manson, G.K., and Solomon, S.M. 2007. Past and future forcing of Beaufort Sea coastal change. Atmosphere-Ocean, 45(2): 107-122. Manson, G.K., Solomon, S.M., Forbes, D.L., Atkinson, D.E., and Cramery, M. 2005. Spatial variability of factors influencing coastal change in the Western Canadian Arctic. Geo-Marine Letters, 25(138-145). (CANADA)	text modified
1853	3	69	60	69	61	Please clarify: Is is certain that this should not be increase in thaw depth rather than increase in permafrost thawing? (CANADA)	text modified, see also response to #1844

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1854	3	69	60	69	63	Statements regarding the projected widespread loss of permafrost in subarctic and arctic would appear to be largely based on Lawrence and Slater (2005) which as mentioned above received much criticism. Delisle (2007) for eg. (citation in earlier comments) has probably produced more realistic projections and states that permafrost will still exist at depth in subarctic and will largely still be present in Arctic environments north of 70°N. The authors should consider revising this statement as well as indicating that the rate of increase in permafrost temperatures is spatially variable. Also it is important to indicate that there will not always be significant physical impacts related to thawing permafrost. For example, greater impacts such as thaw settlement or reductions in strength would be expected in ice-rich soils compare to those with little ice or bedrock. Some modifications to Table 3.1 may also be required. (CANADA)	this has been changed to confidence language
1855	3	69	60	70	2	Is likelihood language appropriate in all of these instances? Also, much of the support for these statements appears to be implicit in other projections, such as of continuing temperature change - but that hasn't been discussed explicitly. (Zwiers, Francis, Environment Canada)	delete text.
1856	3	69	61	69	61	...that it has had physical impacts' - This is too vague. Please be more specific. (Stocker, Thomas, IPCC WGI TSU)	"more storminess" is now removed. Sentence revised.
1857	3	70	1	70	1	"...contribution from MORE STORMINESS...." seems to be too vague to appear in a bolded section conclusion. It would be better to be more specific, and refer to the "polewards shift in storm tracks" as assessed in Section 3.4.5. (Stocker, Thomas, IPCC WGI TSU)	disagree. Only sand and dust storms are discussed in this section.
1858	3	70	5	0	0	Large, if not total, incertitude on sand and dust tempests as a result of incertitude on wind. A sub-section on tornados lack and should be corrected with the abundant studies in USA, even if they are not conclusive for the evolution. (BOURRELIER, PAUL-HENRI, AFPCN)	disagree. This section discuss impacts (of weather and climate) on physical systems.
1859	3	70	5	70	57	Section 3.5.8 Sand and Dust Storms: Suggest this section be integrated into Section 3.4.6 on storms. (CANADA)	Disagree. But we also shorten the section where possible.
1860	3	70	5	70	57	With such limited conclusions, perhaps this section could be shortened. (UNITED STATES OF AMERICA)	agree. Text modified.
1861	3	70	11	70	12	Not only North but also South America (Amazon) - important to be included because one of the Case Studies in Chapter 9 deals with this topic (Stocker, Thomas, IPCC WGI TSU)	disagree, waves are addressed somewhere else in the report.
1862	3	70	11	70	16	Suggest to add: Waves are also a very important factor for marine safety (Eide, Lars Ingolf, Det Norske Veritas)	agree, text revised
1863	3	70	12	0	0	A number of GCMs (better to call them climate models) have a representation of dust aerosols, and can predict to some extent "dust storms". Very few if any at all can predict "sand storm". (Boucher, Olivier, Met Office)	agree, text revised
1864	3	70	12	70	12	Most GCMs likely have dust parameterizations of some kind - but that's a far cry from simulating sand and dust storms. (Zwiers, Francis, Environment Canada)	text revised, see response to comment #1863
1865	3	70	12	70	12	Is this statement really true? (UNITED STATES OF AMERICA)	agree, cross-reference added
1866	3	70	13	70	14	Cross-reference to previous discussions of uncertainties in soil moisture, etc. (Zwiers, Francis, Environment Canada)	agree, text revised
1867	3	70	13	70	14	Doesn't vegetation factor in also? It seems much more important than soil moisture. Where there is no vegetation, soils are very dry. (UNITED STATES OF AMERICA)	agree. Text modified. See response to comment #1863
1868	3	70	14	70	15	Most GCMs likely have dust parameterizations of some kind - but that's a far cry from simulating sand and dust storms. (Zwiers, Francis, Environment Canada)	agree, but the authors are not aware of formal publication on dust storm changes in those regions.
1869	3	70	17	0	0	In the second paragraph, the frequency of sand storms also increased in the North African and West Asian countries. For example Egypt used to have sand storms only during the months of March and April (Khamaseen Winds) while Sudan used to have them during June and July before the rainy season, now they are frequent all year round with an increase in their frequency intensity and duration. In 2010, Syria and Iraq registered more than 150 days of sand storms. (El Mallah, Fatma, League of Arab States)	agree, text revised
1870	3	70	17	70	17	"strongest dust sources" perhaps better worded as "largest dust sources" (Stocker, Thomas, IPCC WGI TSU)	Noted, but paleo is not discussed here.
1871	3	70	17	70	24	There are a number of paleo studies on the Sahel that include discussion of dust events: Mulitza et al, 2010, Nature; Kröpelin et al., 2008, Science). Also, for North America, see Neff et al., 2008, Nature Geoscience. (UNITED STATES OF AMERICA)	thanks
1872	3	70	18	70	21	Good introduction of the Sahel relation with regional dust sources. (Mata, Luis Jose , IMF)	not the authors are aware of
1873	3	70	19	70	20	Are there not more recent references? (Zwiers, Francis, Environment Canada)	China, text is modified
1874	3	70	19	70	20	which region does the statement "but there seems to also be an increase in more recent years" refer to? (Stocker, Thomas, IPCC WGI TSU)	The Gulev amd Grigorieva (2004) discussed changes in wind, not dust transport
1875	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 reflecetd in this paragraph (Eide, Lars Ingolf, Det Norske Veritas)	agreed, text is modified
1876	3	70	27	70	27	Why only the desert? (Zwiers, Francis, Environment Canada)	text is modified to reflect the fact that the discussion was about prior-1990s.
1877	3	70	30	70	30	Is drought still persistent in the Sahel? (Zwiers, Francis, Environment Canada)	text revised
1878	3	70	32	70	33	Some discussion about what this means would be useful, because on the face it is, this seems to be a contradiction. Also, did Zhong (1999) really report on desert expansion through to year 2000!? (Zwiers, Francis, Environment Canada)	text revised, see also response to #1878
1879	3	70	33	70	33	reword as "1960-2000, corresponding with a decrease in dust storm frequency". (Stocker, Thomas, IPCC WGI TSU)	text revised
1880	3	70	35	70	35	These citations should appear along with Gong et al. at the end of the sentence. (Stocker, Thomas, IPCC WGI TSU)	reference deleted in revised text

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1881	3	70	36	70	36	The "Wang et al. 2006b" listed in the References is not about "changes in wind". I think this was meant to cite the "Wang et al. 2006c", which should be added in the References, as suggested in my comment #11 (Wang, Xiaolan, Environmen Canada)	wang et al. 2009b was wrongly linked here due to formatting, and has now been deleted.
1882	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)	text deleted
1883	3	70	38	70	38	This sentence attributes causes for an observed change, but without acknowledging that there might be uncertainty in those attributions. The next sentence, beginning on line 39, does the same. This is inapproriate for an assessment. (Zwiers, Francis, Environment Canada)	text revised
1884	3	70	42	70	43	The last part of this sentence is not worded well. Possible rewording: "...is not complete; for example, the relative importance of the various factors affecting dust storm frequency (as outline above) is uncertain". (Stocker, Thomas, IPCC WGI TSU)	text revised
1885	3	70	43	0	0	Which potential factors are affecting dust frequency in China ? Are these different from those mentioned for the other regions at lines 27-29. Is there a Reference for this region of China ? (International Petroleum Industry Environmental Conservation Association (IPIECA))	agreed, but this is out of the scope of this section
1886	3	70	45	0	0	In the fourth paragraph, there is no mention that sand storms also caused increased desertification in the arid and semi arid North African and West Asian countries, as well as an increase in sand dunes encroachment. (El Mallah, Fatma, League of Arab States)	text revised
1887	3	70	49	0	0	What "reactivated" means, please ? (International Petroleum Industry Environmental Conservation Association (IPIECA))	this comment is out of place
1888	3	70	50	70	50	Is the trend og 0.059m/yr for mean SWH? (Eide, Lars Ingolf, Det Norske Veritas)	the assessment provides what is the current state, but does not make recommendation on what future studies should be (as a review paper would do).
1889	3	70	54	0	0	In the fifth paragraph, the “low confidence “in projecting future dust storm changes is because there is not enough evidence based profound studies on dust in storm source regions. The report should stress the need of such studies. (El Mallah, Fatma, League of Arab States)	text has been revised.
1890	3	70	54	70	57	The assessment here seems to be appropriately cautious, but the supporting text seems a bit too bold in places. (Zwiers, Francis, Environment Canada)	text revised
1891	3	70	54	70	57	Suggest to start the conclusions by stating the "low confidence in projected future dust storm changes" and then provide the details for how you arrive at this assessment. (Stocker, Thomas, IPCC WGI TSU)	This can be speculated in a research paper but our assessment has to be based on existing literatures.
1892	3	70	54	70	57	It would be helpful if even tendencies in particular regions could be indicated. For example, with Asia north of the Himalayas drying, is it not likely that there will be more dust lofted? What about in other dry/arid regions that now generate dust? (MacCracken, Michael, Climate Institute)	these are out of scope of this section
1893	3	70	58	0	0	Should sections included here on both climate changes affecting sea ice and the Greenland ice sheet. For sea ice, there are numerous published studies on observed and projected changes (e.g : Kwok et al. JGR 2009; Lindsay, 2009; Journal of Climate; Feng et al, 2011, Climate Dynamic; Stroeve, GRL 2007; ...). For Greenland, this is also the case concerning both the Greenland mass balance (Sorensen et al. 2010, mentioned before) and the current regimes of the outlet glaciers : Straneo, Science, 2010; Rignot, Science, 2010; Joughin, Journal of Glaciology, 2010; Thomas, Journal of Glaciology, 2009) (International Petroleum Industry Environmental Conservation Association (IPIECA))	Reject. The FAQ is attempting to show that what seems to be a simple question (and one that is often posed but then answered incorrectly) is in fact quite difficult to answer. Simplifying it will lead to exactly the same issue that it is trying to point out should be avoided - getting a simple but wrong answer to a complex question. Much of the "technical" aspect of the FAQ has been included at the insistence of other reviewers.
1894	3	71	1	71	4	This FAQ was long and lacked clarity. Is there a way to reformulate it so it is not so technical and involved? Is there a better way to ask the question so it can be answered more simply? (UNITED STATES OF AMERICA)	Reject. Previous reviews have been very supportive of our approach, while providing useful comments to improve the FAQ. The approaches of using the number of record-breaking events, and the CEI, are included at the insistence of reviewers of the FOD. The comments on the first approach, mentioned by the reviewer, is not specifically attacking any papers - simply pointing out that an increase in one extreme might be offset by a decrease in another extreme, leaving the question as to whether extremes are becoming more frequent as moot. There is no simple way forward - that is the point of the FAQ.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1895	3	71	1	72	4	Overall, I still find the response to this question unsatisfying. The response provides a number of reasons why the question is difficult to answer. It then considers three metrics that have been proposed to overcome these problems, but it finds that all three are lacking. In the first of these cases, which is based on counting the occurrences of record breaking events, it could be inferred that published literature (e.g., the Meehl et al paper on the frequency with which records are broken) is being criticized. If so, then there should be a supporting critical discussion in the main text. In the second case, an operational product is criticised without naming the agency. In that case, I don't think anyone would really expect that it would be possible to develop a comprehensive all encompasing index of extremes given the complexity of the climate system, so I think this is somewhat off the mark as well. The third approach, I think, has at least been discussed more extensively in the impacts literature, so one could point at various supporting papers. In the end, there is not much of a useful statement about a reasonable way forward, which is unfortunate. (Zwiers, Francis, Environment Canada)	Noted. Question of including FAQs is a decision for the Co-Chairs. But see response to 1898.
1896	3	71	1	73	9	There is no need of FAQs. They have already been answered in the report. Even if FAQs are needed, there are many more of them which can be included (GARG, AMIT, INDIAN INSTITUTE OF MANAGEMENT AHMEDABAD)	Noted. Decision for Co-Chairs.
1897	3	71	1	73	52	As already suggested, FAQs should be integrated as annexes to SPM (NUSSBAUM, Roland, Mission Risques Naturels)	Reject. The public would expect "yes/no" and they would be wrong - that is the point of the FAQ. Previous reviewers were very supportive of the FAQ, but provided useful input to improve it.
1898	3	71	3	0	0	FAQ 3.1: This reads as a somewhat typical 'scientist' response. The public would expect 'yes'/'no' and then specification. The current response is quite academic. This would be an opportunity to give results of assessment - robust/less robust/unknown. Suggest to rewrite substantially. (Stocker, Thomas, IPCC WGI TSU)	This comment is off topic, for this FAQ.
1899	3	71	5	71	9	It would be helpful to indicate that, with climate zones shifting, the expectation is that there will be some areas shifting in some directions, some in others. The discussion in the chapter has been mainly as if there is no consistent expectation. (MacCracken, Michael, Climate Institute)	Reject. Ths FAQ has been strongly supported by previous reviewers, and is not very long relative to FAQs in other IPCC assessments. Much of the length is the result of suggested additions from previous reviews.
1900	3	71	11	71	33	Given that this paragraph is in an FAQ, it would be preferable to make it more concise. (IPCC WGII TSU)	No, here region is not defined - it could be much smaller than sub-continental.
1901	3	71	13	71	13	I think it needs to be said that the regions are basically sub-continental in size, so it is possible to get a range of outcomes. (MacCracken, Michael, Climate Institute)	Agreed. Will fix. Use "mean daily maximum temperature" and "mean daily minimum temperature".
1902	3	71	16	71	17	This is true for the averages of Tmin and Tmax, but for the absolute values in a year that are used in the present example the changes may be different (in fact this is expressed in the index ETR, but there is no recent global overview of trends in this index available). (Klein Tank, Albert, KNMI)	Off topic for this FAQ.
1903	3	71	42	71	43	The problem with a large area index is that location matters--and what would be interesting is to get a sense of how the observed changes compare to expectations from models. (MacCracken, Michael, Climate Institute)	Off topic for this FAQ.
1904	3	71	54	71	58	Another reason for problems along the coasts is sea ice retreat--might be mentioned. (MacCracken, Michael, Climate Institute)	Defined in glossary and discussed further in 3.1.
1905	3	72	11	72	11	"climate and weather extremes": what is the difference between a climate extreme and a weather extreme? See comment at Ch3 P2 L4. (Cogley, J. Graham, Trent University)	Agreed. Revised these words.
1906	3	72	15	72	18	The presentation seems a bit awkward, with a "For instance" followed by a "For example". (Zwiers, Francis, Environment Canada)	Have focussed on extremes that may be related to disasters.
1907	3	72	20	0	0	Could mention here the low 2007 summer sea ice extent (International Petroleum Industry Environmental Conservation Association (IPIECA))	Agreed.
1908	3	72	20	72	21	Should mention also the European summer of 2010. (Stocker, Thomas, IPCC WGI TSU)	Noted. Final sentence in paragraph revised for clarity.
1909	3	72	30	72	32	It might be preferable to indicate more explicitly that, while an extreme warm event as rare as the 2009 event could have occurred previously, it is ostensibly now more probable. (IPCC WGII TSU)	See response to comment 1909
1910	3	72	31	72	32	Something wrong with sentence structure here - "before the effects ... were much less pronounced". (Zwiers, Francis, Environment Canada)	See response to comment 1909.
1911	3	72	32	72	32	less' should be 'more' (UNITED KINGDOM OF GREAT BRITAIN AND NORTHERN IRELAND)	Agreed. Replaced with "the".
1912	3	72	35	72	35	Poor style, starting a sentence with "So,". (Zwiers, Francis, Environment Canada)	Agreed. Thanks.
1913	3	72	35	72	35	missing word 'summer' and has an unnecessary comma (UNITED KINGDOM OF GREAT BRITAIN AND NORTHERN IRELAND)	See response to 1907.
1914	3	72	40	0	0	Will the current observed reduction in Arctic sea ice extent not been useful to illustrate the impact of GHG increase ? (International Petroleum Industry Environmental Conservation Association (IPIECA))	Agreed. Delete figure since it has only sparse information that is already explained in text

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1915	3	72	50	0	0	FAQ 3.2, Figure 1: This figure in its current form contains only sparse information that is already more than adequately explained in the text without the need for the figure. We wonder if this figure could be extended to show similar information for other heat waves events, eg, Europe 2003 and 2010, perhaps as a series of 3 panels (e.g. by adding information provided on page 21, lines 25-41). In addition, it would be more useful for the std deviations to be added to the plot. The local temperature data might also be supplemented with regional data. (Stocker, Thomas, IPCC WGI TSU)	"Frequency" is standard terminology for histogam axis. But see response to 1915.
1916	3	72	50	0	0	FAQ 3.2, Figure 1: y-axis shows "frequency", but of what? Suggest to expand the axis annotation to be more specific. In the x-axis, (C) should be changed to (oC) (Stocker, Thomas, IPCC WGI TSU)	See response to 1915.
1917	3	72	51	0	0	FAQ 3.2, Figure 1: I think it would be useful if the November 2009 temperature anomaly was highlighted in red (or another colour) to distinguish its difference from the rest of the distribution. (AUSTRALIA)	Already too many references for small regions
1918	3	74	0	0	0	Add "Baldi, M., G.Dalu, G. Maracchi, M. Pasqui, F. Cesarone, 2006: Heat waves in the Mediterranean: a local feature or a larger scale effect? International Journal Of Climatolology, 26: 1477–1487 (ITALY)	Already too many references for small regions
1919	3	74	0	0	0	I suggest to add the following references about New Zealand precipitation trends: "Ummenhofer CC, Sen Gupta A and England MH., 2009: Causes of late Twentieth Century New Zealand precipitation trends. Journal of Climate, 22, 3-19"; Ummenhofer CC and England MH., 2007: Interannual extremes in New Zealand precipitation linked to modes of Southern Hemisphere climate variability. Journal of Climate, 20, 5418-5440" (ITALY)	Already too many references. Not clear why this should be added.
1920	3	74	0	0	0	Regarding trends in the southern hemisphere I believe the following study should be cited: Ummenhofer CC. (2010). Southern Hemisphere regional precipitation and climate variability: Extremes, trends, and prediction. Bulletin of the Australian Meteorological and Oceanographic Society, 23, 48-50. (ITALY)	Have done this.
1921	3	74	0	110	0	We did not attempt to reconcile the references, but spot checks suggest this (understandably) remains to be done. (UNITED STATES OF AMERICA)	Add reference and fix both references.
1922	3	79	27	79	28	There are two different Christidis et al 2011 referenced in the chapter but only 1 here in the reference list. The missing one (which I have referenced as Christidis 2011b) is "The role of human activity in the recent warming of extremely warm daytime temperatures" Nikolaos Christidis, Peter A. Stott, Simon J. Brown (http://journals.ametsoc.org/doi/pdf/10.1175/2011JCLI4150.1) (Brown, Simon, The Met Office Hadly Centre)	Reference deleted.
1923	3	92	60	92	61	Complete the Lawrence and Slater (2005) reference (cited at page 69 line 52). (Cogley, J. Graham, Trent University)	Wang et al (2006) references have been fixed.
1924	3	107	60	107	62	The "Wang et al. 2006b" listed here is not cited anywhere in Chapter 3; it should be replaced by "Wang, X. L., H. Wan, and V. R. Swail, 2006b: Observed Changes in Cyclone Activity in Canada and Their Relationships to Major Circulation Regimes. J. Climate, 19 (No. 6), 896–915. DOI:10.1175/JCLI3664.1", because this is the work cited on page 47, line 22 but not listed in this section (see my comment #2). (Wang, Xiaolan, Environmen Canada)	Reference has been deleted
1925	3	107	62	107	62	This paper "Wang, X.M., Z.J. Zhou, and Z.B. Dong, 2006c: Control of dust emissions by geomorphic conditions, wind environments and land use in northern China: An examination based on dust storm frequency from 1960 to 2003. Geomorphology, 81(3-4), 292-308" should be added back right after this line. (Wang, Xiaolan, Environmen Canada)	Wang et al papers fixed.
1926	3	108	1	108	1	The "Wang et al. 2009a" paper cited on pages 50 (line 34) and 51 (lines 21-22) should be added right before this line. (Wang, Xiaolan, Environmen Canada)	Reference deleted.
1927	3	108	1	108	2	This reference should be deleted because it is not cited anywhere in this chapter after the correct mentioned in my comment #5. (Wang, Xiaolan, Environmen Canada)	No. Not clear what extra reference adds to chapter
1928	3	108	9	108	9	This recent publication "Wang, X.L., H. Chen, Y. Wu, Y. Feng, and Q. Pu, 2010: New techniques for detection and adjustment of shifts in daily precipitation data series. J. Appl. Meteor. Climatol. 49 (No. 12), 2416-2436. DOI: 10.1175/2010JAMC2376.1" should be added right before this line. It should be cited on page 11 (see my comment #14). (Wang, Xiaolan, Environmen Canada)	As stated in the TC section text, we consider activity to encompass all TC-related metrics, so intensity is inferred in this statement.
1929	3	112	0	0	0	Table 3.1 : row 7, column 3 : what about the current observed changes in TC intensity ? Only activity is mentioned in the table. (International Petroleum Industry Environmental Conservation Association (IPIECA))	Second paper now added.
1930	3	112	0	0	0	Table 3.1 comment: "Medium confidence of past trends towards more frequent central equatorial Pacific ..." The text cites only one paper (Yeh et al. 2009). Is this sufficient evidence? (Brönnimann, Stefan, University of Bern)	Sentence is OK. We have confidence there will be an impact but low confidence that we can predict the geographical patterns of this.
1931	3	112	0	0	0	Table 3.1 extratropical cyclones projected changes (bottom right box). It is odd having "likely impacts" but not saying what the likely impacts would be. There is not even a sign of a change given for any metric. The reduction in storms (next sentence) implies a sign of a change, but there the likelihood is only "as likely as not" (UNITED STATES OF AMERICA)	Attribution has been weakened now, relative to AR4, for NAO. SAM attribution is now linked to ozone.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1932	3	112	0	0	0	Table 3.1 columns 3 and 4. "Likely anthropogenic influence on identified trends in NAO and SAM." If anthropogenic influence has already been detected in observations, why is there "low confidence" in projections of future changes in the NAO and SAM? In the case of the SAM, Solomon and Thompson (Science 2002) argue that the the observed poleward shift of the Southern Hemisphere jet and stormtrack, which projects onto the high phase of the SAM, is due to loss of polar stratospheric ozone. If this is the anthropogenic effect referred to in column 3, it should be distinguished from the anthropogenic effect of greenhouse gas increases. In the case of the NAO, the discussion is very brief, mainly citing the AR4. In light of more recent data continuing to weaken the positive trend signal, we suggest that the authors reconsider whether "likely" is still justified (and also taking into account the new guidance on the determination of "likely", with high confidence needed, etc). Also a recent relevant reference to examine (though not directly addressing the NAO) is: Gillett, N. P. and P. A. Stott (2009), Attribution of anthropogenic influence on seasonal sea level pressure , Geophys. Res. Lett. , 36 , L23709, doi:10.1029/2009GL041269. (UNITED STATES OF AMERICA)	text has been changed to medium confidence
1933	3	112	0	0	0	Table 3.1. Projection box for extratropical cyclones. As a very minor point, "about" appears to have been accidentally deleted (also based on the equivalent sentence in the chapter text) from "about as likely as not." (IPCC WGII TSU)	Summary statements have been changed based on comments and responses in drought section
1934	3	112	0	113	0	Many comments on the drought text would likely affect the summary in Table 3.1. (UNITED STATES OF AMERICA)	Reject. Is not consistent with projections. Uncertainty is large in this region (already dry now).
1935	3	113	0	0	0	Table 3.1 : row 2, column 5 : Middle East should also be mentioned in the regions to be affected by droughts (International Petroleum Industry Environmental Conservation Association (IPIECA))	Reject. Not an extreme related to disasters.
1936	3	113	0	0	0	Table 3.1 should include a section on Arctic sea ice changes. This could also maybe consider the Greenland ice sheet and the Caspian Sea in the coastal impact section. (International Petroleum Industry Environmental Conservation Association (IPIECA))	Summary statements in table revised in the light of review comments on text.
1937	3	113	0	0	0	Table 3.1, Other Impacts section: Note that increases in thaw will not result in significant physical impacts everywhere as material properties are important. (CANADA)	Statement revised.
1938	3	113	0	0	0	In Table, it seems very conservative to suggest that it is only "likely" that there will be an increase in the extreme high water with sea level rise. With sea level rising many tens of centimeters, it seems inconceivable that extreme high water will not increase. Also, in the rightmost column for floods, the order of the two findings should be reversed. (MacCracken, Michael, Climate Institute)	Summary statements revised, but this was not promoted. Not sufficiently clearly related to extremes and disasters.
1939	3	113	0	0	0	Table 3.1. For the attribution of floods, second part (spring peak) what is the confidence/likelihood level? The robust finding on timing of spring flood peak on p. 57 might deserve elevation to the SPM. (UNITED STATES OF AMERICA)	Summary statements revised
1940	3	113	0	0	0	Table 3.1. Attribution box for floods. The way this finding is constructed, it is ambiguous for the reader if "low confidence" also applies to "anthropogenic influence on earlier spring peak in snow-dominated regions," which in the chapter text is more nearly assigned "high confidence" and "likely." (IPCC WGII TSU)	disagree, authors prefer refs in table.
1941	3	114	0	0	0	In Table 3.2, I think it would be very helpful to take out the references to specific papers and instead give a pointer to the subsection in the text where the issue is discussed. (MacCracken, Michael, Climate Institute)	Now use numbers for regions.
1942	3	114	0	0	0	Table 3.2. To maximize ease of interpretation for the reader, it would be helpful to clearly present the acronym used in Figure 3.2 for each region/sub-region described in the table. (IPCC WGII TSU)	Fixed abbreviations
1943	3	114	0	0	0	Table 3.2. Abbreviations used: In the Tmax header, WD and CD are defined. However, in the boxes themselves, HD is sometimes used instead of WD, presumably synonymously. It would be preferable to use only the defined acronym abbreviations in the regional entries. (IPCC WGII TSU)	so noted, coordination between chapts. 3 and 4 is occurring.
1944	3	114	0	3	126	Tables 3.2 and 3.3: Many of the regional subsections of Section 4.5 also include material on observed trends or future projections of extremes. Consistency is vital between such material in chapter4 and regional trend/projection material in Tables 3.2 and 3.3. I've pointed out some problems if this nature with Section 4.5.7 but also recommend careful checking and consultation between Ch 3 and Ch4 authors on the other regional subsections of Section 4.5 compared to Tables 3.2 and 3.3. (Wratt, David, NIWA)	So noted, will change for final print version.
1945	3	114	0	119	0	the table 3.2 is hard to read. It would be much better if you repeat the first row (titles row) on each page (ITALY)	see #1934.
1946	3	114	0	119	0	Many comments on the drought text would likely affect the summary in Table 3.2. (UNITED STATES OF AMERICA)	PDSI is an index that characterizes extremes as well as mean, same with other "dryness" indicators.
1947	3	114	0	126	0	Table 3.2, for the first time I can see in the document, states implicitly ("precipitation extremes, including dryness") that "dryness" is an extreme, and implies that it is only a meteorological extreme. Apparently (although not stated in the definition) "dryness" is proportional to negative soil moisture and proportional conceptually to PDSI. These variables do not describe extremes, but rather means, and they are not exclusively functions only of precipitation and temperature. I understand the use of CDD as a measure of a low-precipitation exteme--meteorological drought--and it has been used frequently in the text; arguably it should appear in the section on precipitation extremes. However, in addition to CDD, both soil moisture and PDSI are now used in this table to characterize "dryness" or "precipitation extreme." I don't understand this. It is at odds with definitions of extremes for the report. (UNITED STATES OF AMERICA)	text modified, removed speculation on dimming etc.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1948	3	115	0	0	0	Table 3.2. Central Europe. Too much emphasis on this "change point" notion. What is the definition of "change point" here? How do we know whether it is a forced response, natural variability, or a data problem, or some combination of these? The physical explanation mentioned in the main text seemed imbalanced, as it doesn't mention other possible explanations or partial explanations, including natural internal variability or even data problems with the SSTs around the 1940s (Thompson, D.W.J., J.J. Kennedy, J.M. Wallace, and P.D. Jones, 2008: A large discontinuity in the mid-twentieth century in observed global-mean surface temperature. Nature, 453(29), doi:10.1038/nature06982. (UNITED STATES OF AMERICA)	unclear what comment means, declined.
1949	3	116	0	0	0	Table 3.2; S Europe and Mediterranean; Medium confidence; Good et al., 2008 Good P., Giannakopoulos C., Flocas H., Tolika K., Anagnostopoulou C., and Maheras P., 2008. Significant changes in the regional climate of the Aegean during 1961-2000. International Journal of Climatology 28, 1735-1749. (GREECE)	thank you for the comment but the new reference doesn't add sufficiently to conclusions to be included at this stage.
1950	3	116	0	116	0	Add reference in Table 3.2 to the results on trends in mean temperature, and extremes over Northern Italy reported by Tomozeiu et al (2006). Reference: Tomozeiu R., Pavan V., Cacciamani C., Amici M., 2006: Observed temperature changes in Emilia-Romagna: mean values and extremes. Climate Research, 31, 217-225. (Pavan, Valentina, ARPA Emilia-Romagna)	thank you for the comment but the new reference doesn't add sufficiently to conclusions to be included at this stage.
1951	3	116	1	0	0	Please, cite Baldi et al., 2006 on the column Tmax relative to S. Europe and Mediterranean (ITALY)	clarified.
1952	3	118	0	0	0	Table 3.2. All Asia, dryness box. The text seems garbled here. At any rate it is not comprehensible. (UNITED STATES OF AMERICA)	No. Sufficient papers in table
1953	3	119	0	0	0	Table 3.2, should include in the “Tibetan Plateau” section reference to You et al., 2008 (page 109, line 36) (Aguilar, Enric, Universitat Rovira i Virgili)	Disagree. References are very important for traceability of the assessment. No change made.
1954	3	120	0	0	0	In Table 3.3, I would take out the references--they clutter the presentation--and instead include a pointer to the subsection where the issue is addressed. (MacCracken, Michael, Climate Institute)	Some possible causes of this cooling tendency in c North America and E US are discussed in Section 3.3.1. They include aerosols - whose influence is not expected to persist. The projections are as robust/consistent over these regions as other regions of North America. Table 3.2 makes it clear that parts of these regions experience warming, while other parts experience cooling - thus the cooling is not large-scale. Having reviewed the observed and modelled evidence again, it is not considered appropriate to change the likelihood statements or confidence levels.
1955	3	120	0	0	0	Table 3.3. It is interesting that the temperature increase projections are "very likely" for some regions (e.g., in central and eastern North America) where cooling trends have been observed since 1900 (Table 3.2). Until the reason for the negative trends in this region is better understood, is it wise to maintain such likelihood levels for the 21st century warming there? (UNITED STATES OF AMERICA)	All terms used in the table are now explicitly defined in the caption.
1956	3	120	0	0	0	Table 3.3. To maximize ease of interpretation for the reader, it would be helpful to clearly present the acronym used in Figure 3.2 for each region/sub-region described in the table. (IPCC WGII TSU)	Abbreviations fixed.
1957	3	120	0	0	0	Table 3.3. Abbreviations used: In the Tmax header, WD and CD are defined. However, in the boxes themselves, HD is sometimes used instead of WD, presumably synonymously. It would be preferable to use only the defined acronym abbreviations in the regional entries. (IPCC WGII TSU)	All terms used in the table are now explicitly defined in the caption.
1958	3	120	0	0	0	Table 3.3. Abbreviations used: In the table, "R" (bold, underlined) is used (e.g., for Europe), even though it is not explicitly defined in the table legend. Please make the symbols used consistent--for example, should "R" (underlined) in the table legend also be bold (but then "R" (underlined) also appears in the table in a few places, e.g., for Central Europe)? Or is the reader supposed to interpret "R" (bold, underlined) as representing both multi-GCM ("R," bold) and mulit-RCM ("R," underlined)? (IPCC WGII TSU)	this will be addressed at the copy editing stage.
1959	3	120	0	126	0	the table 3.3 is hard to read. It would be much better if you repeat the first row (titles row) on each page (ITALY)	PDSI has been deleted from the column heading since PDSI projections are not assessed in the table. Dai 2011 is cited in Section 3.5.1 as supporting the evidence assessed in the table.
1960	3	120	0	126	0	The heading of Table 3.3 lists PDSI in the dryness column, but I don't see reference to PDSI in any of the entries. I don't understand. PDSI was used in the text to support detection and attribution, and it is abandoned in the content, but not the heading, of the projection table. Is it being abandoned because Dai 2011 shows such a gloomy future that it can't possibly be true? If so, shouldn't one say so explicitly, and shouldn't one wonder about the reliability of other PDSI findings? Shouldn't this assessment assess Dai 2011? (UNITED STATES OF AMERICA)	All terms used in the table are now explicitly defined in the caption.
1961	3	120	3	0	0	In the caption of Table 3.3, both bold G and bold R indicate 'multi-GCM'. A difference between them is not clear. (JAPAN)	All terms used in the table are now explicitly defined in the caption.
1962	3	120	3	120	3	the caption does not make very clear the difference between a bold R and an underlined R. might want to spell out that both are using regional models, one with multiple gcms driving a single regional model and the other with multiple regional models (if that is the case) (Lobell, David, Stanford University)	All terms used in the table are now explicitly defined in the caption. A few sentences describing the table have been added at the end of Section 3.2.3.3, i.e., where the table is provided.
1963	3	120	3	120	3	Abbreviations used for modeling evidence: The abbreviations used in the table and defined on this line should be explained more fully, ideally in section 3.2.3.1. It is not completely clear how the sources of modeling evidence listed here relate to the extended description in 3.2.3.1. (IPCC WGII TSU)	This wording is not used in the table.

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1964	3	120	5	120	6	The order of 'annual maximum daily minimum temperature (TNx)' and 'annual minimum daily maximum temperature (TXn)' should be reversed, for the consistency with the text P22, L36. (JAPAN)	All terms used in the table are now explicitly defined in the caption.
1965	3	122	126	0	0	The bold 'R's with under bar are used in the table, but is not explained in the caption. (JAPAN)	This reference deals with observations and is not appropriate to include in this table on projections.
1966	3	126	0	0	0	Table 3.3, should include in the “Tibetan Plateau” section reference to You et al., 2008 (page 109, line 36) (Aguilar, Enric, Universitat Rovira i Virgili)	Noted. The article was made available for the SOD review through the NJ Lite library. Article is now accepted. Furthermore detailed description is also provided.
1967	3	127	0	0	0	Figures: 5 Figures of this chapter are from an as yet unpublished paper (Orlowsky and Seneviratne, 2011). It was not readily apparent to us whether this paper was made available to reviewers through the government review process in the form of additional supplemental material for reviewers. Essential referenced material that is not publicly available to reviewers should be made readily accessible to reviewers during the review process. (CANADA)	Figure deleted.
1968	3	127	0	0	0	Figure 3.1. The meaning of stippling should be added. (PLANTON, Serge, Météo-France)	Figure deleted
1969	3	127	0	0	0	Figure 3.1. This figure will be unintelligible to the vast majority of the target audience. I'm not sure I understand it myself. (UNITED STATES OF AMERICA)	Agreed. Figure deleted
1970	3	127	0	0	0	Figure 3.1. It would be beneficial for the reader to spell out the months indicated by DJF and JJA at least in the figure legend. Also, the current DJF and JJA figure labels could be interpreted as y-axis labels instead of top and bottom panel labels. Ideally each map could be clearly labeled with the relevant months and percentile. Also, it would be helpful to label the color scale bar with "scaling factor" or another appropriate label. Additionally, does the stippling indicate anything, such as a degree of model agreement? Finally, it be best to provide some context or brief description to define the parenthetical "A2 vs 20C3M" for the reader, as done for example in Figure 3.4. (IPCC WGII TSU)	Agreed. All captions will be revised.
1971	3	127	0	136	0	many of the figures are difficult to understand for a general reader -- partly due to the lack of information in the captions (International Federation of Red Cross and Red Crescent Societies (IFRC))	Noted. Article is now accepted. Detailed documentation is provided on the computation of these figures.
1972	3	127	0	136	0	Figures: There are 9 Figures suggested for this chapter (not including Figure 3.2. which just shows regional areas). Of the 9, 5 are from one single paper which has not yet been accepted for publication: Orlowsky and Seneviratne, 2011. We are concerned that this represents a large vulnerability for the IPCC. We appreciate the work done to update the Figures from Tebaldi at al., 2006 but in order to ensure the robustness of these Figures we would suggest that the author team for Chapter 3 engage an additional expert to be a contributing author who would be willing to reproduce these Figures using the methods documented in the referenced paper. (CANADA)	Agreed. Replaced %ile with "percentile". The “D” actually was “[]”, but we agree it was hard to read. It is now omitted (replaced?). Further improvements will be considered.
1973	3	127	1	127	10	I assume that in this figure, the change in the global mean is in the demominator (this should be said). Columns and rows should be labeled in the figure, as well as described in the caption. Some readers will find the obscure reference in the caption to "A2 vs 20C3M" to be confusing. Avoid using short hand in the caption such as "10%tile", which only makes the caption more difficult to read. Label the colour bar as "Scaling factor" rather than with a character that I can only guess is a "D". Make it clear in the caption and the text exactly what the percentiles represent (I'm guessing, based on the caption, that these are percentiles of daily departures from monthly means, that have then been pooled for all days within a given season, but perhaps they are percentiles of something else). (Zwiers, Francis, Environment Canada)	Agreed. This appendix is now provided.
1974	3	127	1	127	10	I think it would be good to include, as an appendix or supplementary material, a precise step-by-step description of how these diagrams were produced, together with a list of models and simulations used. (Zwiers, Francis, Environment Canada)	Reject. Figures on comprehensive model analyses are not available from other publications except Tebaldi et al. (2006). Orlowsky and Seneviratne (2011) is an update on this article, providing an analysis for the whole CMIP3 ensemble, and for the seasonal as well as annual time scales.
1975	3	127	136	0	0	General comment on the figures: Several figures are from the same paper (Orlowsky and Senevirante, 2011). I would recommend a larger variety of paper from which the figures should be inclued. (GREECE)	Agreed. Caption revised
1976	3	129	0	0	0	Figure 3.3. This figure is hard to understand. The phrase "see text for more details" doesn't help--the chapter is over 100 pages long, and anyway the figure caption should contain all information needed to understand the figure. Do cryptic "all" and "ant" refer to climate experiments with all known and anthro-only estimated forcings? I would like to see similar results for an unforced experiment, as a point of reference. Why is the same variable denoted by two different names (waiting time and return value), neither of which is defined? It might be possible for a layperson to comprehend a figure of this sort, but it will take much more work on presentation to make that possible. (UNITED STATES OF AMERICA)	Corrected.
1977	3	129	0	0	0	Figure 3.3. It appears that the error bar colors are not labeled correctly in the figure caption. The description in the chapter text, as well as in Zwiers et al. 2011, suggests that the green bars should correspond to annual minimum daily maximum temperature (TXn). The caption lists TXn as the blue bars instead, and TNx as the green bars. Additionally, the values represented by the blue and pink bars seem to be reversed in Zwiers et al. (2011) compared to in Figure 3.3. Also, it would be helpful for the reader to define ALL and ANT in the figure legend (in addition to the description already provided in the chapter text). (IPCC WGII TSU)	Caption revised. The stippling will be enlarged.
1978	3	130	0	0	0	Fig. 3.4 What are the units? I assume it is units of standard deviations for %WD and %CD, correct? What is the unit for %Tmax>30 ? It is hard to see the stippling on some of these plots. (UNITED STATES OF AMERICA)	Caption and figure revised

#	Ch	From Page	From Line	To Page	To Line	Comment	Response
1979	3	130	0	131	0	Figures 3.4 and 3.5. What is "SD?" According to the figure caption, one can only infer that it is change, expressed as a fraction, but then values could not exceed 1 in magnitude. What is the variable (denoted "O" or "D"?) in the third column? Terms "cold day" and "warm day" should be defined again in caption for convenience of reader. Aside from all this, the caption does little for me to understand what exactly these pictures show. I have difficulty imagining most laymen or even impact people understanding this figure. (UNITED STATES OF AMERICA)	Agreed. The figures and caption will be revised accordingly. The stippling points will be enlarged.
1980	3	130	1	130	10	Additonal details are required in the caption, including precise definitions of the indices, and reference to a table providing a list of models and simulations used. Better labelling is required for columns and colour scales. Units should be stated, and units implied by labelling and colour bars should correspond. For example, the right hand column suggests a projected change expressed as a percentage, but the colour scale seems to indicate fractional change (on the scale 0-1) instead. I can't guess whether the scale for warm days and cold days is expressed on the fractional or percentage scales. Stippling was impossible to see in the hard copy that I printed on my colour laser printer. (Zwiers, Francis, Environment Canada)	Caption and figure revised. Stippling points clearer.
1981	3	131	0	0	0	Fig. 3.5 What are the units? I assume it is units of standard deviations for %WN and %CN, correct? What is the unit for %Tmin>20 ? It is hard to see the stippling on some of these plots. (UNITED STATES OF AMERICA)	Caption and figure revised
1982	3	131	1	131	10	Similar comments apply here as for previous figures of this kind. (Zwiers, Francis, Environment Canada)	Caption and figure revised
1983	3	132	0	0	0	Figure 3.6. The plots on the figure are very small and the displayed numbers cannot be recognised. (GREECE)	Noted. Will be checked.
1984	3	132	0	0	0	Figure 3.3 and 3.6 seem to be related; I much prefer 3.6, even though it is more intimidating at first glance. For the sake of the reader, could one or both of these figures be redesigned so that they are more closely related, and so that similar graphic elements could be used for both? And could regions in 3.2, 3,3, 3.6 and everywhere else be defined consistently? (UNITED STATES OF AMERICA)	Reject. Too late to be added at this stage. Information on cold nights is also provided in Fig. 3.5
1985	3	132	1	132	24	Should there be a corresponding figure for TNn? (Zwiers, Francis, Environment Canada)	Noted. Stippling points will be enlarged.
1986	3	133	0	0	0	Fig 3.7 Hard to see the stippling on these, so hard to interpret the results. (UNITED STATES OF AMERICA)	Caption and figure revised
1987	3	133	1	133	10	Similar comments apply here as for previous figures of this kind. (Zwiers, Francis, Environment Canada)	Noted. Will be checked.
1988	3	134	0	0	0	Figure 3.8. Similarly to Fig 3.6. (GREECE)	TCs are not well simulated, so other methods than AOGCMs are required to project changes. These approaches are discussed in detail in 3.4.4.
1989	3	135	0	0	0	Fig. 3.9. It's not clear how useful this figure is for extreme winds discussion. It shows reductions of 99th percentile daily winds throughout most of the tropics from GCMs. But the GCMs don't simulate intense tropical cyclones, which are projected by higher resolution models to increase in intensity in some tropical regions. Therefore TCs are presumably not included in the picture for Fig. 3.9, so what extremes is this referring to. Also, do the global models have any predictive ability for the 99th percentile winds, say on an interannual time scale? If so, this should be noted, and if not, this should be given as a caveat with regard to the associated projections. (UNITED STATES OF AMERICA)	This figure has been redrafted on a different projection for consistency with other figures in the chapter and the bold grey stippling changed to red.
1990	3	135	0	0	0	Figure 3.9. It is very hard to distinguish the "fine black" stippling from the "bold grey" stippling. For example, are the slightly larger, more "smudged" marks the "bold grey stippling"? Could two stippling symbols be used instead--e.g.., fine points versus small open circles? (IPCC WGII TSU)	Reject. Soil moisture is a measure related to agricultural drought. Will clarify in the text.
1991	3	136	0	0	0	Figure 3.10. (Also Figure SPM.2) Average soil moisture is not an extreme. A different metric is needed for agricultural drought. (UNITED STATES OF AMERICA)	Caption and figure revised
1992	3	136	1	136	10	Similar comments apply here as for previous figures of this kind. (Zwiers, Francis, Environment Canada)	Reject. Only critical comment on this figure. Is complementary to drought box.
1993	3	137	0	0	0	We recommend deleting this figure, which is oversimplified, ambiguous, and not supportive of the main thrust of the document. (UNITED STATES OF AMERICA)	Figures have been revised and improved.
1994	3	148	0	285	0	I do not have specific remarks to the chapter but general opinion concerning the issue. First of all I would like to underline the unusual high importance of this chapter in the entire Report. It was perfectly decided that these 3 elements have been studied in details (temperature, precipitation and wind). I have only negatice opinion as to observed extremes. There is no sufficient knowledge about already observed extremes. Some examples from the literature can not give general overview. There is no studies concerning the global scale. However, such ones are prepared for the projected changes of extremes!!! And more, all examples and figures refer to the projections only. There are attached 10 huge figures, no one with observed extremes. Some figures are rather poor quality/resolution and are not quite well readable (e.g. Fig. 3.3). (POLAND)	Figures have been revised and improved.