Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-1	5	0	0	0	0	The SOD reads much better and informative than the previous two drafts. Congratulations! [Victor Brovkin, Germany]	Noted.
5-2	5	0	0	0	0	In general I would like the see the excellent introduction to uncertainty (5.3.5.2) used more throughout the report to place the observations and paleomodeling results in context. [Michael Neil Evans, United States of America]	Taken into account.
5-3	5	0	0	0	0	Given the excellent discussion of both paleoclimate data-based reconstructions and paleoclimate modeling results throughout this chapter, I was struck that there was no introduction to paleoclimatic modeling. There should be a section (within or shortly after 5.1? or else a paragraph referencing elsewhere in the AR5 report where paleoclimate modeling is discussed) introducing the special topic of paleoclimatic modeling, including uncertainties and how they are assessed; how the use of ensembles and intercomparison projects (CMIP and PMIP figure prominently in this chapter, as they should) is helping to identify uncertainties in individual models. [Michael Neil Evans, United States of America]	Taken into account. A brief introduction to paleoclimate modelling has been added.
5-4	5	0	0	0	0	The executive summary reads like a list of individual research findings rather than a summary of the state of palaeo climate science and its relevance to present and future climate. [Government of Australia]	Taken into account. Some aspects of the executive summary have been revised to stress the relevance of the findings with respect to current and projected changes.
5-5	5	0	0	0	0	There is a lot of discussion about polar paleoclimates in this chapter, however the tropics are the heat engine of the planet and deserve equal treatment at least. [Government of Australia]	Noted. The chapter and executive summary cover aspects of tropical climate changes including tropical SST and monsoon changes.
5-6	5	0	0	0	0	There was little discussion of records from southern Africa in this chapter. That region is important to the southern hemisphere, and there are more than hyrax middens from there. Quite a bit of dune geomorphology evidence is available. [Government of Australia]	Noted. However, due to length constraints, it is not possible to discuss all regions and the choice was made to focus on regional data syntheses that were available with a focus on temperature. Missing information was added in the chapter conclusion (new section 5.9).
5-7	5	0	0	0	0	The Executive Summary has a number of confidence statements related to the Medieval Period (MCA) and also to the mid-Holocene periods (8k and 6.5k years ago). These and others though generally relate to levels in the late-20th century, but this period is never adequately defined. These comparisons ought to also be made to the more recent period (such as 2001-2010). This is particularly relevant for Arctic Sea Ice cover (given the record low in September 2012) and also for the MCA. The chapter will find this difficult to do as there are very few studies that try and bring the proxies up to date and combine with the latest satellite, instrumental and reanalysis datasets. An alternative would be that the comparison up to 2010 is done in Ch. 10. [European Union]	Noted. Reference periods have been more clearly defined throughout the text. Precise comparison of most recent changes and proxy record is however not always feasible.
5-8	5	0	0	0	0	Ch. 5 assesses many new climate model simulations for the last 2000 years and the LGM. This conveys the impression that much new work is being undertaken, but in reality not many new individual proxy reconstructions have been produced since AR4. There is confusion in the chapter about the definition of some of the periods in the past, particularly the LIA, which gets various dates. Table 5.1 is a good attempt to convey a lot of information, and it will be picked over by many after publication. A publication (referred to PAGES 2k) is crucial to a number of conclusions. Some aspects will likely get heavily revised if this doesn't come out. [European Union]	Noted. However, the chapter also refers to many new information for the LGM (e.g. MARGO data synthesis) and 2k (e.g. PAGES2k regional continental reconstructions, which is published) which were not available in AR4. The different past periods refered to in chapter 5 are now introduced in a new table (Table 5.1).
5-9	5	0	0	0	0	This chapter contains no information on changes in permafrost and vegetation cover during previous climate. Yet both play an important role in the climate system. [European Union]	Noted. However due to space limitations it was decided not to cover these aspects. Current changes in frozen soil is covered in chapter 4 and some aspects of changes in vegetation are assessed in chapter 6 in relationship with changes in global biogeochemical cycles.
5-10	5	0	0	0	0	This chapter contains no information on changes in permafrost and vegetation cover during previous climate. Yet both play an important role in the climate system. [Corinne Le Quéré, United Kingdom of Great Britain &	Noted. However due to space limitations it was decided not to cover these aspects. Current changes

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Northern Ireland]	in frozen soil is covered in chapter 4 and some aspects of changes in vegetation are assessed in chapter 6 in relationship with changes in global biogeochemical cycles.
5-11	5	0	1			Consistency in assessment numbers: Because chapter assessments continue to be refined, please check carefully all values (and the uncertainty ranges) carefully between tables, figures, main text, and summary text within your chapter. If numbers are taken from other chapters, please also ensure the latest results are used. Specific examples will be highlighted in our chapter comments. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. Will be taken into account in the final draft.
5-12	5	0	2			Treatment of Uncertainty: please follow the IPCC guidance note carefully; use italics to highlight formal uncertainty assessments; use likelihood in conjunction with high/very high confidence only (except in exceptional cases); if likelihood is given for situations where confidence is less than 'high', we recommend to put confidence in brackets at the end of the sentence rather than combining both confidence and likelihood in text. Please note - usage of the formal terms from the uncertainty guidance note, (egg. "likely", "confidence" etc) should be restricted to the use within statements which report assessment findings. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. Will be taken into account in the final draft.
5-13	5	0	3			Format of Executive Summary (ES): As agreed at the third lead author meeting, we would ask that all chapters follow a consistent style for the ES. 1) The first sentence (or two) of each paragraph should be bolded to highlight the key message, with the subsequent sentences providing the detailed quantitative assessment. 2) Statements should incorporate the IPCC Uncertainty Language 3) Each paragraph must include a traceability to the underlying sections/subsections where the key message was drawn from (to the second level section heading), indicated using square brackets at the end of each paragraph. 3) Paragraphs should be grouped together under subtitles. The use of bullets should be avoided. 4) Finally, because the ES should be short and concise, lengthy textbook or chapeau type introductory text should be avoided. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. Will be taken into account in the final draft.
5-14	5	0	4			Cross-chapter references AR5: suggest to update cross-chapter references to not just refer to Chapter number but to refer to specific section if appropriate. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. Will be taken into account in the final draft.
5-15	5	0	5			References to AR4 and earlier IPCC assessments: be as specific as possible. Writing just AR4 without any reference is not useful to the reader. Please refer to specific chapter where possible. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. Will be taken into account in the final draft.
5-16	5	0	6			Use of acronyms: In order to improve overall readability of the report, we would like to suggest that you please avoid acronyms that are not needed and/or are not used in more than one section of your chapter. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. Will be taken into account in the final draft.
5-17	5	0	7			Personal pronouns: our strong preference is to minimize the usage of personal pronouns, e.g., we/us/our to the extent possible. Exceptions to this would be when the Chapter's assessments conclusions are presented as clear summary statements. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. Will be taken into account in the final draft.
5-18	5	0	8			Please make sure to provide updates of relevant data from your chapter that will be collected in Annex II - Climate System Scenario Tables, to the Annex II Chair. Also, please take the time to critically check all the entries in Annex II that are based on your Chapter assessment or that you are using in your chapter assessment. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. Will be taken into account in the final draft.
5-19	5	0		0		As with many of the other chapters the text here is way too reliant on the use of acronyms which make the whole thing very difficult to read [Peter Clift, United States of America]	Noted. We have limited the use of acronyms and added a table to guide the readers regarding past periods (new Table 5.1).
5-20	5	0		0		I really don't think you can cite all these submitted papers that are not in press [Peter Clift, United States of America]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
5-21	5	0		0		The SOD draft of the chapter is clearly much more mature and comprehensive and the progress from the	Noted. Some aspects of large scale and regional

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						IPCC AR4 is evident. It is generally also more balanced that the FOD, but two main focuses are still dominating (2ka and glacial-interglacial changes), while the Holocene is in the shade. [Olga Solomina, Russia]	Holocene changes are briefly discussed in section 5.5.
5-22	5	0				Comment: A note to acknowledge the work of the Valerie and Michael as Coordinating Lead authors and all those that have contributed to this chapter. It is likely one of the most diverse in the science report for the range of disciplines and science cultures that it incorporates as a consequence of the enormous range of space and time it covers, and that presents special challenges to the review process. Thanks for the chance to comment..Peter [Peter Barrett, New Zealand]	Thank you!
5-23	5	0				Capter 6 (Palaeoclimate) of the AR4 had a section called "Concuding Remarks on Key Uncertainties". A similar Section would be valuable in the current Chapter 5. [Keith Briffa, United Kingdom]	Taken into account. A similar section about conclusions has been added (new section 5.9).
5-24	5	0				The following terms "Dansgaard-Oeschger events", "Heinrich stadials","Bolling and Allerod warmings" and "Marine Isotope Stages" that are commonly used in Chapter 5 deserve also to be included in the Annex III: Glossary in a similar way that other terms such as "8.2 ka", "Younger Dryas", "Little Ice Age" or "Medieval Warm Period" are already included. [Alejandro Cearreta, Spain]	Taken into account. These terms which are specific to our chapter cannot be covered in the glossary. We have therefore added a new table 5.1 to introduce past periods.
5-25	5	0				I joined the IPCC review process as expert in climate reconstruction by documentary records, so my review only cover the chapter 5. After a detailed reading, I would like to congratulate the authors for their impressive work and synthesis capacity. The chapter is beautifully written and in my opinion it provides a very good, complete and accurate revision of the current knowledge of the past climate record and its interpretation. In particular, I think the chapter provides a quite good explicit discussion about the problems of the current temperature reconstructions based in proxy data, one of the key issues to be included in the AR5. I do not miss (to my knowledge) any major reference in the text so I have no particular comments. [GALLEGO DAVID, SPAIN]	Noted. Thank you!
5-26	5	0				Excellent progress made since FOD. This chapter has become very, very good and I wish to congratulate the authors!! However, the chapter still lacks repeatedly in making a clear connection to why some of the issues discussed in a very detailled manner are important in the context of CC. While policy relevance may perhaps be obvious to the specialist, it is not alwyas so obvious unless this is really made very explicit. Therefore texts should be improved throughout with respect to policy relevance. Perhaps certain sections could even be shortened, streamlined, and/or some statements may then even be dropped entirely while applying such a "revision filter" throughout. Although I personally like the material as is, we need to remember, that this is a AR that needs to serve mainly other purposes than just having to serve science. Then, allow me to make also some comments on the structure: I still find it irritating that topic sea level variations comes before temperature variation. Then I find the wording of some sections not sufficiently clear. E.g. the title "Radiative Forcings and Radiative Perturbations from Earth System Feedbacks" may imply for some readers that this topic is discussed also for the present. This may confuse the reader, since other chapters of AR5 WGI appear to be more logical places to contain such material, leaving the reader a bit at a loss what to expect here really. I suggest to consider making the focus on the past more explicit in these titles, unless this chapter is indeed the only one within AR5 where this topic is treated. In the latter case the title may also be improved to reflect this fact (e.g. using phrase "past and present"). Another solution is to rephrase the title and using more specfic terms, e.g. "Past changes in atmospheric composition, solar and volcanic activities" or something similar. I suggest you introduce FAQ 5.3 with a title similar to "How variable was the climate of the past?" or "How unusal is the recent climate change in light of newest findings since AR4?" (or perhaps "What role played the internal variability within the climate system in past and recent climate changes?") [Andreas Fischlin, Switzerland]	Noted. Regarding the main text, several sections have been revised and shortered. The sea level section 5.6 follows the temperature sections (5.3 at hemispheric scales and 5.5 at regional scales). The executive summary statements have been revised. The title of section 5.2 has been changed to "Pre industrial perspective on radiative forcing" for clarity.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-27	5	0				The use of the term "Charney Climate Sensitivity" (CCS) should be coordinated with other chapters. E.g. I don't find this in chapter 12 [Jan Fuglestvedt, Norway]	Taken into account. The revised text now addresses Equilibrum climate sensitivity for consistency with our chapters.
5-28	5	0				Many figures do not present axis labelling. [Government of France]	Noted. Axis labelling has been checked for all figures.
5-29	5	0				The report has captured fairly well the scientific evidences for climate change in the geological records of the Holocene and to some extent in the older Cenozoic times. The major issues discussed in the reviewed chapter of the report are the dynamics of glacial-interglacial climate variability, past changes in sea-level and abrupt climate changes. Although the scientific findings are correctly represented, some of the conclusions based on geological records need to be expressed differently to present the fact. It is important that readers get a balanced understanding of "natural" variability vs a possible "anthropogenic" variability in climate. [Government of India]	Noted. The text has been clarified for statements arising from paleoclimate records or from paleoclimate models.
5-30	5	0				There are multiple proxies to suggest that the global mean sea-level was higher in mid Pliocene than today (see section 5.6.1). The magnitude of sea level during the last interglacial was 6 to 10 m above the present day level (section 5.6.2.2). It may be stated that in further deep time the rise and fall of sea-levels were orders of magnitude higher than the modern times. Although the report is based on a small fraction of the geologic time, it is rightly stated that the current sea level change is not unusually rapid relative to past periods of rapid ice sheet de-glaciation (FAQ 5.2). But the concluding sentence in the same section states that the current rate of mean global sea level change is unusual relative to that observed or estimated over the last two millennia. [Government of India]	Noted. The comparison between current and past sea level changes is restricted to periods for which high resolution and precise age constrains are available.
5-31	5	0				This is also highlighted in the executive summary. A non-specialist will not appreciate how the instrumental record of the past two millennia cannot be directly compared with the archive-based estimation or how the present day instrumental measurements achieve higher resolution compared with that obtained in the past decades or centuries. Such statements, therefore, should be carefully drafted and should be unambiguous in the report. [Government of India]	Noted. The resolution of the past sea level information is clarified in the revised text.
5-32	5	0				Another major issue discussed in the report is about abrupt climate change. The discussion on abrupt warming on timescale of decades during the Dansgaard-Oeschger events is a very good example of how climate extremes were part of natural cycles in our immediate past and before the influence of man. The hyperthermal events of the Early Eocene is the most discussed palaeoclimate event and often considered as an analogue for the present day greenhouse earth. The report suggests the role of sun, volcanic activity and internal variability in climate change but also concludes that the change in global temperature over the past 30 years is due dominantly to anthropogenic factor. The write-up does not justify the second part of the finding. [Government of India]	Noted. The first part of the comment is related to abrupt events, for which our section (5.7) is focused on centennial to millennial time scales. PETM is mentioned in section 5.3. The end of the comment is related to FAQ5.2 (solar variability), which results from the assessments of chapters 5 and 10.
5-33	5	0				It is generally believed that man is influencing climate in a major way. A scientific report of this kind should therefore make a non-specialist clearly distinguish the natural variability in climate prior to human intervention from the one which has occurred since industrialization. The latter again could be due to the combined effect of natural and anthropogenic factors. I am afraid, the sections FAQ 5.1 and FAQ 5.2 give a subtle view that climate related changes in the past few decades are dominantly anthropogenic. [Government of India]	Noted. Sections FAQ5.1 and FAQ5.2 have been revised.
5-34	5	0				The Likelihood Table (Table 1.1) and Confidence figure (1.12) should be repeated in the SPM, TS and each Chapter and the terminology should be applied consistently. As an alternative to repeating the complete table/figure the material should be restated briefly in the SPM, TS, and each chapter. [Government of United States of America]	Noted. The information is found in Chapter 1.
5-35	5	0				There are many acronyms throughout the text. For ease of reading, it is suggested that each section redefine acronyms, with the possibility of including a master table of acronyms. [Government of United States of America]	Noted. Acronyms related to time periods are now introduced in a new table 5.1.
5-36	5	0				Executive Summary of Ch5 could benefit from better consistency in its use of likelihood terminology (e.g. p 5-3 lines 41-47 which states "We have low confidence..."), and more uniform clarity where AR5 findings differ from / improve upon AR4 (e.g. 5-3 lines 5-9. where the AR5 finding pushes the record back to 800 ka from AR4's 650 ka). [Government of United States of America]	Taken into account. This ES statement has been revised.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-37	5	0				The chapter's general tenor is such that actual paleoclimate proxy records are covered in the text in a perfunctory [and sometimes incomplete] way, and paleo-models are overemphasized. Many proxy records are plotted in the figures, though model simulations are also dominant there too. These shortcomings ought to be addressed in some way. [Government of United States of America]	Noted. The source of information arising from paleoclimate records or from simulations has been clarified.
5-38	5	0				Some sections have subsections nicely divided up such that there are no more than 4 or so paragraphs per sub-section. These are easier to follow and better organized than those subsections that go on for many pages. Therefore, it is suggested that an attempt be made to break up the longer sections into more digestible pieces. [Government of United States of America]	Noted.
5-39	5	0				The word “confirm” appears frequently. It is possible to “confirm” why a climate model responds a certain way. However, “confirm” should often be replaced with “indicates” or “supports” when in context with the real world. There are many different types of climate models. Many of the GCMs here used the same version as the future projection and observational period versions to do their paleo simulations, and there should be a way to distinguish these from simplified models. Moreover, there is a hierarchy from box model to EMIC to GCM. Each has strengths and weaknesses, and by appropriately labeling the “climate models” referred to in the text, these will be easier for the reader to follow (and more honest about the confidence in the results). [Government of United States of America]	Taken into account.
5-40	5	0				The organization of the chapter attempts to make an assessment focused on climate processes, mechanisms, and modes of variations an change. While a nice idea, it reads rather choppy with very similar information repeated among the sections and a feeling of disconnect in terms of what all is known about the last interglacial, glacial, and last 2000 years scattered among 5.2-7. For example the regional description paragraphs on page 31-line 46 to page 32-line 34 would nicely follow the global and hemispheric discussion in section 5.3.5, page 22-line 31 to page 24-line 29 [Government of United States of America]	Taken into account. The text structure has been revised.
5-41	5	0				The writing could be tightened to reflect the high level and authorative message of an IPCC assessment rather than conveying information that the authors apparently do not embrace. Use of the terms such as "notions" and "so-called" are inappropriate in the text of a document that is being drafted to inform policy makers. (e.g.,"notion of amplified temperature response", "notion that variations in Earth’s orbital parameters", "notion of a generally warm period", "notion of internal variability", "notion (high confidence) that changes of the AMOC", "notion of irreversibility", "so-called terminations", "so-called elevation-desert effect", "so-called Medieval Climate Anomaly", "so-called far-field". [Government of United States of America]	Taken into account. Terms such as "notions", "so-called" have been avoided in the revised text.
5-42	5	0				On the whole I wonder if the chapter could be organised, or text added at the beginning, to express that there are (at least) two different types of paleoclimate scenarios: 1) those in which there are time-dependent perturbations such as the Milankovitch-scale glacial-interglacial cycles or synoptic time-slice reconstructions (e.g. L. Pliocene warm period) in which at least some of the forcings are known, and allow some inferences of sensitivity. 2) Events or modes of variabiity in which the forcings are not known, but which allow some understanding of unforced background or baseline variability, against which the recent climate change can be compared, thus providing some insight into the question of whether or not the recent record is unusual. So for example forglobal temperature the answer would be yes, for droughts and floods perhaps not. [William Howard, Australia]	Taken into account. The introduction (section 5.1) has been slightly extended to cover this aspect. Chapter 1 has also added background information on paleoclimate modelling.
5-43	5	0				There is still an important new paper by Esper et al., which is not included in the reference section: Jan Esper, Ulf Büntgen, Mauri Timonen, David C. Frank, Variability and extremes of northern Scandinavian summer temperatures over the past two millennia, Global and Planetary Change 88–89 (2012) 1–9 [Kiminori Itoh, Japan]	Accepted.
5-44	5	0				The following paper by Large and Yeager looks extremely important because they measured heat fluxes and compared with SST: Large, W. G., S. G. Yeager, 2012: On the Observed Trends and Changes in Global Sea Surface Temperature and Air–Sea Heat Fluxes (1984–2006). J. Climate, 25, 6123–6135. doi: http://dx.doi.org/10.1175/JCLI-D-11-00148.1. Their conclusion is that the increase in SST during the period	Commt seem misplaced and probably refers to Chapter 3.

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						after 1970 is mostly natural. This result seriously affects the explanation of the SST changes in the latter half of the 20th century. [Kiminori Itoh, Japan]	
5-45	5	0				Overall, the chapter is well written, interesting and provides up to date references. [Pascal Kindler, Switzerland]	Thank you!
5-46	5	0				I am surpised that there is only one mention of the Paleocene-Eocene Thermal Maximum (PETM) in the whole chapter (on page 14, lines 31-32), and that is farily dismissive of what we might learn from it. Despite the differences in the background climate state, the response of climate to a massive carbon input to the atmosphere and oceans, and the time taken for atmospheric carbon dioxide to decay back to pre-release levels, can provide some insight into the likely magnitude and longevity of response to anthropogenic disturbance of the Earth system. [Robert Larter, United Kingdom]	Noted. The relevance of the PETM for the long time decay of CO2 perturbations has been briefly mentioned in the revised section 5.3.1
5-47	5	0				Overall this chapter is well written and informative, but the different sections are quite varied in presentation style (and details of the information given, see below). I would like to see an introduction to each section like the one in 5.7 to introduce the reader to the topic focused on. Actually, a brief summary for each section would also be very informative. I think that it should be kept in mind that this chapter will be read by many who are not experts within the included fields, but also those who want to broaden their knowledge, students etc. [Hans Linderholm, Sweden]	Taken into account. A brief introduction to each section has been added. A general conclusion (section 5.9) is also summarizing the main limitations of this assessment.
5-48	5	0				There is a discrepancy in the detail of how the information is presented. As an example section 5.3.5.1 discusses proxy records yielding different temporal resolution, without giving examples of such (which may be informative to the reader). In section 5.6.2.1 however, I feel that there is too much information given on the start and duration of LIG from a large number of sources. A table would possibly suffice in the last example. [Hans Linderholm, Sweden]	Taken into account. Text has been revised to give sufficient details.
5-49	5	0				I would like to see some more discussion about the evolution of the climate in the first millennium CE prior to the Medieval Climate Anomaly, especially with the explicit mentioning of the Roman Warm Period and the Dark Age Cold Period. Since the publication of the IPCC Fourth Assessment Report many more long proxy records with high to medium temporal resolution – reflecting either changes in temperature, precipitation or drought – have been published making it potentially feasible to place the modern global warming into a much longer time perspective than was possible at the time of the IPCC Fourth Assessment Report. This is especially relevant since during the second millennium CE large volcanic eruptions and solar minimums have tended to coincide, making it hard to separate the influence of solar and volcanic forcing, whereas they are better separated during the first millennium CE. A better understanding of the regional to global climate during the first millennium CE is thus important in order to better understand the relative influence of volcanic and solar forcing, respectively, on decadal and longer time-scales. [Fredrik Ljungqvist, Sweden]	Noted. AR5 covers some aspects of climate evolution during the first millennium but no simulation. While this aspect is important, the available litterature does not permit to make a detailed assessment of the relationship between variability and forcings.
5-50	5	0				Related to the comment above is the relative lack of discussion of natural multi-centennial (quasi)oscillations (e.g., the Bond cycles) in the climate system and their possible relationship to long-term changes in solar forcing. An improved understanding of natural multi-centennial climate (quasi)oscillations is important in order to better predict the direction of future natural climate evolution and for investigating if the cause of natural climate variability is likely to reinforce or counteract the anthropogenic global warming. A discussion of natural multi-centennial climate oscillations also places large-scale climate changes, as the Medieval Climate Anomaly and Little Ice Age, in a larger context. At the very least, the limitations of predicting climate from past multi-centennial climate (quasi)oscillations ought to be outlined. [Fredrik Ljungqvist, Sweden]	Noted. Section 5.5 has a brief discussion of multi centennial climate variations.
5-51	5	0				Several recent studies discuss climate cycles, and the following can be mentioned as examples: Wanner, H., Solomina, O., Grosjean, M., Ritz, S. P., and Jetel, M.: Structure and origin of Holocene cold events, Quaternary Sci. Rev., 30, 3109–3123, 2011; Humlum, O., Solheim, J., and Stordahl, K.: Identifying natural contributions to late Holocene climate change, Glob. Planet. Change, 79, 145–156, 2011; Breitenmoser, P., Beer, J., Brönnimann, S., Frank, D., Steinhilber, F., and Wanner, H.: Solar and volcanic fingerprints in tree-ring chronologies over the past 2000 years. Palaeogeogr. Palaeocl., 313–314, 127–139, 2012. Breitenmoser et al. (2012) provide a good presentation of the detection of the DeVries cycle of solar activity in tree-ring records. [Fredrik Ljungqvist, Sweden]	Taken into account.
5-52	5	0				One important reference that I would like to see mentioned in this context of the Holocene Thermal Maximum	Taken into account. A recent publication by Marcott et

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						is Shakun and Carlson (2010) that show, after assessing numerous proxy records, that the warmest conditions during the Holocene occurred in the Northern Hemisphere 8±3.2 ka and in the Southern Hemisphere 7.4±3.7 ka. It could also be of interest to refer to the borehole temperature estimates by Huang et al. (2008) that point to that the earth experienced multi-centennial periods with global mean temperatures of ~1°C above the pre-industrial temperatures. The full reference to Huang et al. (2008) is: Huang, S. P., Pollack, H. N., and Shen, P.-Y.: A late Quaternary climate reconstruction based on borehole heat flux data, borehole temperature data, and the instrumental record, Geophys. Res. Lett., 35, L13703, doi:10.1029/2008GL034187, 2008. [Fredrik Ljungqvist, Sweden]	al (2013) from the same group analysing Holocene temperature variations has been discussed.
5-53	5	0				I have a few concerns with this Chapter as its stands at present (though recognise that it is a major task bringing together the range of paleoclimatic archives): 1) There is often lack of clarity over what time period (and associated temporal resolution of the archives) is being referred to; 2) There is often lack of clarity in whether a single site, wider region or hemispheric or global spatial domains are being considered; 3) There are no perfect proxy climate/environmental records and, from this viewpoint, I often found that what is presented are just lists of what the cited papers said without critical assessment of their reliability and, importantly, a balanced assessment and synthesis of where different records agree or disagree with each other (when covering similar time periods/regions) and (where available) model simulations. [Janice Lough, Australia]	Taken into account. The text has been clarified to express the source of information.
5-54	5	0				I have only provided comments where, I feel, I have some sort of expertise/insights to offer. [Janice Lough, Australia]	Noted.
5-55	5	0				Great text! At the moment, sections vary with respect to writing style, detailedness, etc. Good luck with homogenizing this. Manfred Mudelsee [Manfred Mudelsee, Germany]	Noted. An effort has been dedicated to the improvement of the style throughout sections.
5-56	5	0				Annex II. In your discussion of historical record refer ahead to and check on the pre-industrial abundances of the GHG in Annex II tables (AII.4.1-16) Table AII.1.1: Historical abundances of the Kyoto greenhouse gases (shown in Chapter 8) AII.4: Abundances of the Well Mixed Greenhouse Gases Table AII.4.1: CO2 abundance (ppm) Table AII.4.2: CH4 abundance (ppb) Table AII.4.3: N2O abundance (ppb) Table AII.4.4: SF6 abundance (ppt) Table AII.4.5: CF4 abundance (ppt) Table AII.4.6: C2F6 abundance (ppt) Table AII.4.7: C6F14 abundance (ppt) Table AII.4.8: HFC-23 abundance (ppt) Table AII.4.9: HFC-32 abundance (ppt) Table AII.4.10: HFC-125 abundance (ppt) Table AII.4.11: HFC-134a abundance (ppt) Table AII.4.12: HFC-143a abundance (ppt) Table AII.4.13: HFC-227ea abundance (ppt) Table AII.4.14: HFC-245fa abundance (ppt) Table AII.4.15: HFC-43-10mee abundance (ppt) Table AII.4.16: Montreal Protocol greenhouse gas abundances (ppt) [Michael Prather, United States of America]	Noted.
5-57	5	0				Overall I am a bit puzzled about the structure. Potentially it is to late to make significant changes, but to give an example: The authors discuss the extratropical atmospheric circulation changes for the Glacial-interglacial in section 5.3.2.2 (Page 17) and later in section 5.4.2 they discuss the changes from LGM to Holocene for extratropical modes, which again represent the atmospheric circulation. [Christoph Raible, Switzerland]	Taken into account. The section on atmospheric circulation has been reorganised.
5-58	5	0				Throughout the chapter: There are several places where editing by English native speakers may help to	Noted. However, the "Mid-Holocene" is commonly

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						remove overly complicated / too long sentence construction, incorrect grammar, and lack of appropriate punctuation. I will highlight in my notes sections where such assistance is required. One of the English-speaking authors may want to read through the entire chapter for consistency as well. Also geological conventions need to be used rather than having a mix of "Early to Mid Holocene", "early to mid-Holocene", "Middle Holocene", etc. Stratigraphic subdivisions for intervals that have been officially subdivided are, in chronostratigraphy "Early", "Middle" and "Late", and in lithostratigraphy "Lower", "Middle" and "Upper". "Mid" doe not exist; it is colloquial only. And "mid-" is used only in adjectival use, or in unofficial subdivisions. I don't want to seem pedantic, but conventions exist to promote clarity and remove ambiguity. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	used to refer to the PMIP 6k simulations and has therefore been used in Chapter 5.
5-59	5	0				Throughout the chapter. There is a lot of mention of "wamer temperatures" and 'warming temperatures" (and v.v. for cooling). These are tautologies. To avoid this, best use "Warmer conditions", "Warming", "Increasing temperatures", etc. The tautologies come from the media's sloppy language use (similar to making a sentence saying "also....as well", or to saying "scientists believe", where the latter is not a tautology but is a much favoured saying in the media that in reality has no place in an evidence-based discipline). I strongly argue that scientists should not fall into the trap of becoming equally sloppy in their presentation. In the end it's up to the editors, of course, but I am quite worried that we need to be as careful as possible in the presentation of a report like the IPCC AR5, which some media and bloggers will tease apart to the finest detail they can use to discredit the authors and/or the arguments. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account.
5-60	5	0				This chapter has a reasonable structure that comprehensively covers the major aspects in paleoclimate research. Another highlight in this chapter is the data and figures presented are all coming from the most recent publications or unpublished yet. It will work as an excellent review for people having strong interest in paleoclimate and a lot of concerns about the progress in this field. There is one minor problem I have for the section 'Executive Summary'. It is a little overwhelming for me to read such a long list of summary before I start reading the introduction. As a result, I skipped this part and got back to it after I finished the rest of the chapter. It has nothing to do with the writing, which has been very concise. I am just thinking the reading process may be more fluent if the structure or position for this section can be adjusted. [Ting Wang, United States of America]	Noted. However, we cannot move the Executive Summary.
5-61	5	0				There are several figures where the colours called out in the caption are not the colours on the graph. I will try to list them but a more thorough editorial check is needed on this aspect [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Figure captions have been carefully checked and errors regarding line colors have been corrected.
5-62	5	0				In part of the text there has been an aggressive but inconsistent use of hyphens in multiple word strings. I will try to list the inconsistencies but decisions on style and a thorough copy edit are also needed. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Taken into account.
5-63	5	1	1	52	6	OVERALL COMMENTS: This chapter is shaping up VERY NICELY. It reads well for the most part, and it covers a lot of important concepts effectively; in my opinion, the only weakness is still the weak emphasis on precipitation relative to the discussion of temperature changes, but it's now much better than it was. I particularly like the FAQ section at the end (pages 49-52) which is an excellent resource targeted at important question. Bravo! [Jay Curt Stager, United States of America]	Thank you. The lack of information on past regional precipitation changes is now highlighted in a new conclusion (5.9).
5-64	5	1	2			Not a key comment, but I did baulk at the word "Archives" in the title of this chapter. Paleoclimatology does have its special archives in the form of ice cores etc, but this chapter also includes results from the analysis of modelling, which involves analysis of data extracted from digital archives. But then the same could be said of almost every chapter in this assessment, either because they too deal with modelling results or because most observational studies also involve working with data that have come from archives - paper ones originally in the days before electronic computing. So I wonder whether consideration could be given to replacing "Archives" by "Studies" in the title of this chapter. [Adrian Simmons, United Kingdom]	Noted. However, the title of the chapter was defined earlier and approved and cannot be altered.
5-65	5	1		52		My biggest concern with this Chapter is that it appears to be written in a different style to the other chapters in the report. There is an annoying mixture of American and British English used, inconsistently, and the text has been poorly proof-read. If this was a journal paper I would reject it! I have not flagged all of these below, but	Taken into account. The text has been further proof-read.

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						the Chapter team needs to look at the material carefully. [Peter Burt, United Kingdom]	
5-66	5	1		52		The confidence levels are not italicised in the text (compared to in the other chapters) which immediately makes the message of the text harder to assimilate. [Peter Burt, United Kingdom]	Taken into account. Confidence levels have been italicised in the text.
5-67	5	1		52		Throughout the text, the word 'century' needs a capital 'C', as per in other chapters and correct usage as a proper noun. Instances of hyphens in giving the century information should be removed [Peter Burt, United Kingdom]	Taken into account.
5-68	5	1		52		The Chapter needs a glossary. Keeping track of all the acronmys used was extremely difficult [Peter Burt, United Kingdom]	Rejected. It was decided not to have a chapter glossary but to introduce the specific time period acronyms in a new table 5.1.
5-69	5	1		200		12. This paragraph refers to the entire Chapter 5. Chapter 5 reviews some of the published information on the topic "Paleoclimate Archives". However, the motivation for the reviewed research effort and the logic behind it is more often fraudulent than not, as the respective research frequently follows the pseudo-scientific reasoning that "more corroborating evidence produces a stronger case for the AGW hypothesis". In fact, nothing can be further from the truth, as shown in our my 3. Indeed, no amount of corroborating evidence can prove a hypothesis, while a single piece of contradictory evidence is sufficient to reject a hypothesis. In effect, the only (dubiously) useful result of this research effort is the "general progress of science", resulting from wasteful usage of public money on climate studies, where no real problem requiring study may be found. Even the PhD degrees earned as a result of such research are of dubious (in the very least) value, as we are producing more pseudo-scientists certified as scientists, in addition to the already existing pseudo-scientists. Research based on the AGW hypothesis, known to be wrong, may provide no valid scientific results, as its conclusions are already known before the research even began - these conclusions being "AGW is happening, and we are to blame for it". Additionally, the data interpretation in the publications is frequently done based on the same climate models, which are demonstrably wrong (as shown in my Paragraphs 2 to 8), and therefore constitutes a fraud. [Igor Khmelinskii, Portugal]	Noted. However this comment is difficult to take into consideration due to the lack of any specific statement regarding chapter 5 or suggestions for modifications based on peer reviewed publications.
5-70	5	1				General comment: First of all, the chapter is extremely well written. I have only one comment and this concerns [Sucharita Ghosh, Switzerland]	Noted.
5-71	5	1				the assessment of times when rapid climate change took place: Menendez et al. (2010) address [Sucharita Ghosh, Switzerland]	Noted.
5-72	5	1				rapid change point estimation when the time series observations have long-memory. These authors analyze the GRIP [Sucharita Ghosh, Switzerland]	Noted.
5-73	5	1				oxygen isotope series from Greenland. Their method is based on setting levels on the minimum speed of change in the [Sucharita Ghosh, Switzerland]	Noted.
5-74	5	1				trend curve. This type of analysis is important to identify past rapid change points in various different series [Sucharita Ghosh, Switzerland]	Noted.
5-75	5	1				from differing locations or belonging to various different proxies, to assess whether the series behave differentially. [Sucharita Ghosh, Switzerland]	Noted.
5-76	5	1				Full reference: Menedez, P., Ghosh, S. Beran, J. (2010) On rapid change points under long memory. [Sucharita Ghosh, Switzerland]	Noted.
5-77	5	1				Journal of Statistical Planning and Inference, 140: 3343-3354. [Sucharita Ghosh, Switzerland]	Noted.
5-78	5	1				The discussion of climate sensitivity seems to not follow the expert group decisions taken in Marocco. I thought we had decided against introducing a 'charney sensitivity' as a separate concept but instead assume (and explain) that ECS is without long term feedbacks. Secondly, we had arrived at a cross chapter sensitivity assessment that is carried in a box in chapter 12, and gets fed into by chapters 5, 9, 10. Please coordinate, and we need to all agree on terminology (my notes from the breakout, sent through TSU, say: 'Equilibrium sensitivity - does not map onto Charney CS strictly, so introducing Charney sensitivity was not found helpful.' [Gabriele Hegerl, United Kingdom]	Taken into account. The revised text now discusses equilibrium climate sensitivity.

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5-79	5	1				A general comment to chapter 5: There is numerous evidence for a solar influence on climate during the Holocene.This is very shortly mentioned in FAQ 5.1 which says that the sun (among others) played a dominant role in climate change for the last 10000 years. However, this seems to be in contradiction with the newer low TSI estimates that cannot produce large climate changes without feedback effects. I think it would be useful to discuss this apparent contradiction in more detail (e.g. mention that other processes could be important such as e.g. solar UV influence on circulation patterns). [Raimund Muscheler, Sweden]	Taken into account. FAQ5.1 has been revised.
5-80	5	1				This Chapter is greatly improved over the first draft. I have very few comments overall and only suggest changes to the executive summary. [Christian Ohneiser, Netherlands]	Noted.
5-81	5	1				I like the chapter in general. My main point is that a clear distinction should be made between datasets and modeling results discussing the highly variable and anomalous MPWP (3.0-3.3 Ma) and the more extreme and prolonged warming interval in the Early Pliocene (4.0-4.5 Ma) (For recent East Antarctic datasets: Fielding et al., 2012, Glob. Planet. Ch; Passchier, 2011) as it is possible that the climate system operated in a different way (cf. Brierley et al., 2009, Science). My second point would be that somewhere in the text it should be indicated that the data coverage in the Antarctic during the MPWP is very scarce and that we are unable to assess regional variability at this point. [Sandra Passchier, United States of America]	Taken into account. The text has been revised to clarify what arises from data or models and a new section 5.9 has been added regarding key gaps.
5-82	5	2	9	2	9	Suggest another FAQ on the amount of volcanic CO2 emissions compared to CO2 emissions from human activities [Government of Australia]	Noted. However no new FAQ can be added after the review process.
5-83	5	2	20			Orbital forcing is not dominant in the last 2000 years [only withing context of last 10,000 yrs], other processes explain last 2 kyr climate variations [Government of United States of America]	Noted. The role of orbital forcing with respect to other processes for the last 2 kyr climate variations has been clarified.
5-84	5	3	1	5	22	The exectuive summary has become excellent! Congratulations. Notably the comparisons with progress since AR4 are very good. [Andreas Fischlin, Switzerland]	Thank you!
5-85	5	3	1			It might be useful to include the main points of 'Paleoclimate Perspective on Irreversibility in the Climate System' in the executive summary. Even though this is a low confidence concept it will at least give it greater exposure than leaving it to the end of the chapter. [Christian Ohneiser, Netherlands]	Taken into account.
5-86	5	3	5	3	5	Insert the word "likely" between the closed bracket and "exceed" [Giuseppe Cortese, New Zealand]	Noted. This executive summary statement has been revised.
5-87	5	3	5	3	5	Suggest explicitly writing "carbon dioxide (CO2), methane (CH4) and nitrous oxide (N2O)" in Executive Summary. [Government of Australia]	Taken into account.
5-88	5	3	5	3	5	This should be in the SPM. It answers a common question received by government - 'are the greenhouse gas levels we now see unusual'. [Government of Australia]	Taken into account.
5-89	5	3	5	3	9	I am not sure what "range of variability" means here, it seems you simply mean "exceed the range of concentrations" or "is not within the variability". The last sentence just repeats the first one and is not needed. [Olivier Boucher, France]	Taken into account.
5-90	5	3	5	3	17	Perhaps state the current CO2 level in ppm at some point [Rob Wilson, United Kingdom]	Taken into account.
5-91	5	3	6	3	6	Insert "the" before "past" [Robert Larter, United Kingdom]	Taken into account.
5-92	5	3	6			Typo: insert 'the' into "…in ice core records during the past 800,000 years." [Jay Curt Stager, United States of America]	Taken into account.
5-93	5	3	7	3	9	Delete the sentence starting with "New data…", as it repeats what mentioned in the sentence in bold (lines 5-6) [Giuseppe Cortese, New Zealand]	Taken into account.
5-94	5	3	7	3	9	It is confusing that you conclude that the data extend an AR4 claim. Also "new" data is a relative statement. It would be much clearer just to say, "Present-day concentrations likely exceed the natural range of variability for the past 800,000 years." Also, why is it not "very likely?" [Alan Robock, United States of America]	Taken into account.
5-95	5	3	8	3	8	Rate of change of concentrations should also be mentioned [Government of Australia]	Taken into account.

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5-96	5	3	8	3	8	It is recommended to specify time period for “present day” (Line 8 of Page 3) and keep the indicated time periods consistent throughout the whole report. [Government of China]	Taken into account.
5-97	5	3	8	3	8	It would be better if the mean of 1971-2000 is used instead of “present-day”. [Shaowu Wang, Beijing]	Taken into account.
5-98	5	3	8	3	9	The statement can be misinterpreted to mean that the AR4 statement was back to 800,000 years and the new data extends beyond. As such, the authors should consider rewriting the sentence to explain that the AR4 statement was back to 650,00 years ago and in AR5 the additional 150,000 years of data results in an extension back to 800,000 years ago [Government of United States of America]	Taken into account.
5-99	5	3	8	3	9	Better write "... variability back to at least 800,000 years ago ...". [Wilfried Haeberli, Switzerland]	Taken into account.
5-100	5	3	8	3	9	Only "likely" ? AR4 stated that "the atmospheric concentration of CO2 in 2005 exceeds by far the natural range over the last 650,000 years" [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Taken into account.
5-101	5	3	11	3	11	This should be in the SPM. It helps answer a common question received by government - 'do elevated greenhouse gas levels really drive global temperatures?' [Government of Australia]	Taken into account.
5-102	5	3	11	3	11	Insert the word 'past' before the words 'high CO2 climates'. [Government of Australia]	Taken into account.
5-103	5	3	11	3	11	Remove "further." Further than what? Each statement needs to stand on its own. [Alan Robock, United States of America]	Noted, and have replaced with the word "link"
5-104	5	3	11	3	12	This first sentence is vague. "Coupling" is on a specturm- it's not always the same. Therefore, the authors should consider clarifying the text. [Government of United States of America]	Taken into account.
5-105	5	3	11	3	12	“Reconstructions from high-CO2 climates provide further evidence for coupling between atmospheric CO2 concentration and global mean temperature.” The word “coupling” should be reconsidered. “High CO2, high temperature; low CO2, low temperature”—this is “coupling”, but it is not the observation. In this paragraph only Early Eocene and Middle Pliocene are mentioned, both with high CO2 and high temperature; but there is also the Miocene when lower CO2 was accompanied by higher temperature (e.g., Beerling and Royer, 2011, Nature Geoscience). No doubt there is a strong connection between CO2 and temperature, but it remains unclear how the two variables are related with each other, and we are far away from an appropriate understanding of the connection between CO2 and temperature. The use of “coupling” may give a false impression that the scientific question about CO2 effect on climate has been solved. [PINXIAN WANG, China]	Taken into account.
5-106	5	3	11	3	15	Please assess Lariviere et al., 2012 (http://www.nature.com/nature/journal/v486/n7401/full/nature11200.html). This paragraph could stand to be clarified and expanded upon. [Government of United States of America]	Noted and taken into account.
5-107	5	3	12	3	12	Mentioning the current value for CO2 concentration provides context for the following statements [Giuseppe Cortese, New Zealand]	Taken into account
5-108	5	3	12	3	12	Add this sentence before the one starting with "During…": "Current CO2 concentration is 391 ppm." [Giuseppe Cortese, New Zealand]	Taken into account
5-109	5	3	13	3	16	"Associated with" is unclear in these sentences. Need to say whether it is cause, effect, simple coincidence/correlation etc. [Government of Australia]	Taken into account
5-110	5	3	14	3	17	Use "higher temperatures" instead of "warmer temperatures" [Government of Poland]	Taken into account
5-111	5	3	16	3	16	Comment Text: replace "reached" with "exceeded" - extensive precipitation of nahcolite is compelling physical evidence and the figure is a lower limit. [Peter Barrett, New Zealand]	Taken into account
5-112	5	3	16	3	16	Comment Text: For EECO temperatures, high Antarctic margin SSTs in the mid 20s is supported by independent evidence of deep -sea temperatures around 14°C at the time (Cramer et al. 2011 - see comment for p 15 line 27). This in turn suggests the modelled increase in global average SST at 8°C cited is on the low side - perhaps cite 8°C as a minimum value with higher confidence. [Peter Barrett, New Zealand]	Taken into account
5-113	5	3	16	3	16	“low confidence” should be in italics [Government of Australia]	Accepted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-114	5	3	16	3	16	Should "low confidence" be written in italics? [Masa KAGEYAMA, France]	Accepted.
5-115	5	3	16	3	16	"low confidence" should be in italics. [Alan Robock, United States of America]	Accepted.
5-116	5	3	16		23	I still do not think there is sufficient evidence to say that the models underestimate polar amplification. See next comment [Julia Hargreaves, Japan]	Taken into account
5-117	5	3	16			Eocene exact magnitude is not known, but the macrofossils about minimum cold month temperature are pretty well constrained (palms, crocodilian teeth) In this summary, it says 'low' confidence, but later p14,18, it says 'medium confidence'. The latter is probably right, but the conclusions ought to be consistent. [Government of United States of America]	Taken into account
5-118	5	3	16			1000 ppm of CO2 in the Eocene is reasonable but there are a number of estimates that are considerably higher than this number so I would say 1000 ppm or higher [Michael Hren, United States of America]	Taken into account
5-119	5	3	16			(low confidence) should be in italics. [Sandra Passchier, United States of America]	Accepted.
5-120	5	3	17	3	17	Usually the timing of 1750 was used as pre-industrial, but temperature of 1850 was used for no observational data is available in 1750. The proxy data shows that temperatures in 1750 may higher than that in 1850. Therefore, it is suggested that the mean of AD800-1800 is used as pre-industrial valuse for avoiding the impact of decadal to centennial variability. [Lei Huang, China]	Noted. Table 5.1 introduces our definition of pre-industrial, for consistency with CMIP5 simulations.
5-121	5	3	17	3	17	Usually the timing of 1750 was used as pre-industrial, but temperatures of 1850 was used for no observational data is available in 1750.The proxy data shows that temperatures in 1750 may higher than that in 1850. Therefore, it is suggested that the mean of AD800-1800 is used as pre-industrial value for avoiding the impact of decadal to centennial variability. [Shaowu Wang, Beijing]	Rejected. While this alternative definition makes sense, it cannot be applied in practice. Table 5.1 introduces more clearly our definition of pre-industrial, for consistency with CMIP5 simulations.
5-122	5	3	19	3	19	"amplified" temperature response: this statement should be more precise: "amplified" compared to what? [Masa KAGEYAMA, France]	Taken into account. See response to comment 5-126.
5-123	5	3	19			This sentence is not clear (particuarly the phrase, "strengthened the notion"). Also, the authors should consider clarifying the statuement to read something like: "Proxy information has increased confidence that change in atmospheric CO2 concentration result in an amplified temperature response in the Arctic." [Government of United States of America]	Taken into account. See response to comment 5-126.
5-124	5	3	19			Change the title to explicitely state out 'Proxy information has strengthened the notion of amplified temperature response in the Arctic compared to low latitudes during globally warm periods' [Christian Ohneiser, Netherlands]	Taken into account. See response to comment 5-126.
5-125	5	3	21	3	21	Polar amplification means little to a non specialist, maybe keep these two words in brackets right after a one-liner explaining what it is, something like: "... show a strong warming response at high latitudes (polar amplification) for the Arctic..." [Giuseppe Cortese, New Zealand]	Taken into account. See response to comment 5-126.
5-126	5	3	21	3	21	"polar amplification" compared to the global average of temperature changes (as for comment 12, the assertion should be precise, especially in the executive summary. [Masa KAGEYAMA, France]	Accepted. Text revised.
5-127	5	3	21			State 'Current simulations…..' rather than available. [Christian Ohneiser, Netherlands]	Taken into account. Text revised.
5-128	5	3	23	3	25	The term "coupled climate models" should be replaced with "climate models" to be consistent with the other AR5 chapters [Victor Brovkin, Germany]	Accepted. Text revised.
5-129	5	3	23			"underestimation is unclear". Totally unclear? Or just not certain enough to say yet? Consider revising to be consistent with uncertainty language. [Government of United States of America]	Taken into account. See response to comment 5-116
5-130	5	3	23			The following wording is suggested: ..with respect to proxy-based reconstructions, and the reason for this underestimation is unclear for the time being. [Klaus Radunsky, Austria]	Taken into account. See response to comment 5-116
5-131	5	3	25	3	31	In line 26, consider inserting the word "past" before "glacial cycles". Also, the authors should consider reordering the second sentence so reconstructions come before models of different complexity (for better flow of the sentence). Are the terms "leads and lags" (line 28) and "bipolar seesaw" too slangy, or too scientific-	Noted, partly rejected (we did not insert "part" because our statement is also true for future glacial cycles), and partly accepted (the lead and lag section

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						jargon-y without citations or explanations? The authors should also consider inserting the word "record" after CO2 and before "have been revised" in line 29. [Government of United States of America]	has been revised).
5-132	5	3	25	3	31	It is not clear this statement should be assigned "high" confidence as the models' assignment of the proportion of feedback amplification by greenhouse gases gives a *plausible* conclusion but does not add any further data to distinguish between the feedback amplitude of GHGs versus ice sheets, surface properties etc. as feedbacks [William Howard, Australia]	Rejected. The degree of confidence of high because important role of CO2 is supported by numerous lines of evidences.
5-133	5	3	27	3	27	Not clear which "reconstructions" [Government of Poland]	Taken into account.
5-134	5	3	27			"to to negative" should be replaced by "to negative" [Government of France]	Taken into account.
5-135	5	3	28	3	28	"significant"- how much? [Government of Australia]	Taken into account. The text has been revised.
5-136	5	3	29	3	31	Sentence beginning with 'New Antarctic data...' should be in the SPM - this is very relevant to policy makers [Government of Australia]	Noted.
5-137	5	3	29	3	31	The statement "depict simultaneous increases in Antarctic temperature and CO2" overlooks the fact that Pedro et al., have a range of lag (CO2 behind temperature with a range of [-56 to +381] years. This is not so much simultaneous as "indistinguishable form zero" as stated in the body text (Sect 5.3.2.1). [Tasman van Ommen, Australia]	Accepted. Change to "near-simultaneous".
5-138	5	3	29			Should say "near-simultaneous". Pedro at least has a mean +ve lag: simultaneous is within the uncertainty, but is not the most likely. I don't know what the conclusion of Parrenin is, but near would be a sensible qualifier in any case. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted. Change to "near-simultaneous".
5-139	5	3	31	3	31	Remove "due to the bipolar seesaw", as it is not strictly relevant here, and keeps the sentence simpler, with no need to define what the bipolar seesaw is [Giuseppe Cortese, New Zealand]	Taken into account
5-140	5	3	31	3	31	Is the "high confidence" on the timing of Antarctic vs Nothern Hemisphere temperature changes or is it on the mechanism of the bipolar see-saw? I think there is uncertainty in the mechanism, with some fast teleconnections from the tropics to the poles also potentially playing a role in the teleconnections between hemisphere (cf.e.g. Buiron et al, QSR 2012) so I would tend to place the high confidence on the timing (so, earlier in the sentence) and not on the mechanism (which could actually disappear from the sentence). [Masa KAGEYAMA, France]	Taken into account.
5-141	5	3	31	3	31	I do not think that there is enough discussion of skepticism about the role of the AMOC and the bipolar seesaw (e.g. The many papers by Wunsch and group) to evaluate this confidence. Taking these into account, medium confidence may be more appropriate. [Mark Siddall, United Kingdom]	Noted. The text has been revised.
5-142	5	3	33	3	33	You use a technical term "Charney climate sensitivity" in the executive Summary. You can not expect the general reader of the executive summary to be familiar with such terminology! Why not use "equilibrium climate response of the Earth system to perturbations" which gives some information. [Heinz Blatter, Switzerland]	Accepted.
5-143	5	3	33	3	33	Is the "Charney" climate sensitivity the equilibrium climate sensitivity? Please avoid jargon. [Olivier Boucher, France]	Accepted.
5-144	5	3	33	3	33	Is the word "Charney" really needed? [Giuseppe Cortese, New Zealand]	Accepted.
5-145	5	3	33	3	33	Charney'? This is meaningless to a policy maker [Government of Australia]	Accepted.
5-146	5	3	33	3	33	Define Charney climate sensitivity [European Union]	Accepted.
5-147	5	3	33	3	33	Reference should be made to Box 5.1 where the meaning of "Charney climate sensitivity is explained" [Government of Poland]	Accepted.
5-148	5	3	33	3	34	To make the sentence more understandable and explain the abbreviation CCS which is not explained in 5.3.3, add "CCS" to "Charney climate sensitivity" and an explanation "the temperature response to a doubling of the concentration of CO2" so the sentence reads; "New estimates of Charney climate sensitivity (CCS - the	Accepted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						temperature response to doubling of CO2-concentration) using climate reconstructions and simulations for the Last Glacial Maximum (LGM) indicate that values below ……" [Government of NORWAY]	
5-149	5	3	33	3	34	The authors should consider rewriting the statement: 'Proxy information has strengthened the notion of amplified temperature response in the Arctic.' to read: "Since AR4, additional paleoclimate proxy data and climate modeling have advanced the process understanding of hypothesized amplified temperature response in the Arctic." [Government of United States of America]	Taken into account. ES paragraph revised.
5-150	5	3	33	3	34	Inserting 'Charney Climate Sensitivity' as a new term into the assessment report can be confusing when "climate sensitivity" is used elsewhere. Expressing the sensitivity as "values below 1.4°C or above 6°C are very unlikely" may be accurate but will confuse most readers. A suggested revision of the bolded statements could be: 'New estimates using climate reconstructions and simulations for the Last Glacial Maximum (LGM) indicate a climate sensitivity for glacial cooling of less than 1.4°C or greater than 6°C is very unlikely.' [Government of United States of America]	Accepted.
5-151	5	3	33	3	34	As this summary is for executives, will they understand what "Charney climate sensitivity" is. It might be worthwhile to try to formulate this point in a less specialist way. [Adrian Simmons, United Kingdom]	Accepted.
5-152	5	3	33	3	37	It would be better to separate the climate sensitivity estimation for present time from that for the palaeo-climate, for the mechanism of interaction between the ice sheet and atmosphere waries with time. [Lei Huang, China]	Rejected. The impact of ice sheets is taken into account and discussed in 5.3
5-153	5	3	33	3	37	I think this may be too technical for the executive summary [Mark Siddall, United Kingdom]	Accepted.
5-154	5	3	33	3	37	It would be better to separate the climate sensitivity estimation for present time from that for the palaeo-climate, for the mechanism of interaction between the ice sheet and atmosphere varies with time. [Shaowu Wang, Beijing]	Rejected. We explain the importance of estimating the climate sensitivity from the past.
5-155	5	3	33		37	The assessment is relevant for the overall assessment in the box in chapter 12 and also discussed in chapter 10. crosslink and make sure wording is consistent. Note that section 10.8 discusses constraints from palaeoclimate, however, relying to chapter 5 for assessment and discussion focusing there on probabilistic estimates compared to those from other periods). Chapter 10 has a figure showing pdfs, including from palaeoclimate. please crossreference and synchronize with chs 9 10 and 12 [Gabriele Hegerl, United Kingdom]	Accepted.
5-156	5	3	33			I suggest not to use the expression "Charney climate sensitivity" here in the ES since many readers are not familiar with the term. Better to describe what it is, as is done at top of page 12. [Jan Fuglestvedt, Norway]	Accepted.
5-157	5	3	33			Explain the term "Charney climate sensitivity" - the Executive Summary should be understandable for non-specialists. [Wilfried Haeberli, Switzerland]	Accepted.
5-158	5	3	33			The decision was that climate sensitivity should remain as defined in AR4, which is essentially the Charney sensitivity, but the latter expression should not be used. So "Charney" should be removed. Sensitivity estimates that include slow feedbacks are termed Earth System sensitivity (see section 12.5.3, Box. 12.2). [Reto Knutti, Switzerland]	Accepted.
5-159	5	3	33			Jargon alert: readers won't know what "Charney" climate sensitivity is from the outset. Probably better to simply refer to "climate sensitivity" here. [Jay Curt Stager, United States of America]	Accepted.
5-160	5	3	33			Charney Climate Sensitivity: Charney Climate Sensitivity is not used in any other chapters, and should be avoided here too. If it is necessary to be introduced somewhere in the WGI AR5, then it should be within Chapter 12, Box 12.2. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
5-161	5	3	34	3	34	Please give a range of years for the LGM as for the other time periods discussed in the Executive Summary for better orientation of non-specialist readers. [Georg Feulner, Germany]	Accepted. Table 5.1 introduces precisely the different periods
5-162	5	3	34	3	34	remove "(LGM)" There is no need to define an acronym that is not used again in the paragraph. [Alan Robock, United States of America]	Accepted.

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5-163	5	3	34			Climate sensitivities do not include internal feedbacks such as vegetation, which can make sensitivity higher (see refs such as Bhatt et al.) [Government of United States of America]	Rejected, Here we discuss only the equilibrium climate sensitivity.
5-164	5	3	34			In order to improve clarity the following wording is suggested: .. Indicate that values for the Charney climate sensitivity below … In addition it is sugegsted to include in the glossary an explanation of the term "Charney climate sensitivity". [Klaus Radunsky, Austria]	Taken into account.
5-165	5	3	35	3	35	The position of continents have changed throughout the Cenozoic (i.e. closure of Tethys Sea), coupled with orogenic events and the formation of the Ismus of Panama. One would assume climate sensitivity values (1.4-6 deg. C) are valid for the LGM-present interval. Thus, is the reference to preceding 65 million period needed, especially given that the configuration of continents, mountain systems, and ocean basins were notably different? [Government of Canada]	Noted. Past changes in topography are taken into account when diagnosing climate sensitivity.
5-166	5	3	35	3	35	Add comma after "assymetries" [Alan Robock, United States of America]	Accepted. Although the senstence is now modified.
5-167	5	3	35	3	37	Is what is meant here that there is low confidence in the statement that sensitivity estimates for colder conditions are not direcntly applicable to warmer conditions ? In which case why bring forward this low confidence statement to ES ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Text has been revised for clarification
5-168	5	3	37	3	37	again, on which part of the paragraph is the "low confidence?" [Masa KAGEYAMA, France]	Taken into account. Text has been revised for clarification
5-169	5	3	38	3	38	The authors should consider inserting a new finding based on material presented in section 5.3.2.1: "There is high confidence that orbital forcing is the only external driver of glacial cycles. Based on current understanding of glacial inceptions during the late Quaternary, it is very unlikely that orbital forcing on its own would trigger a glacial inception before the end of the next millennium, and only if atmospheric CO2 content were below the pre-industrial level, would a glaciation be possible within the next several thousand years." [Government of United States of America]	Noted. Taken into acccount during revision
5-170	5	3	41	3	45	SL 10 m above present for mid-Pliocene is MINIMUM SL, NOT a best estimate [~20 meters is best]. The text should be revised accordingly. Also, the early Pliocene [~4-4.5Ma] sea level was probably HIGHER than mid-Pliocene SL, so wording here should be changed; it sounds like Pliocene and LIG SL may have been the same, which is not a correct message to convey. [Government of United States of America]	Taken into account. Text has been revised.
5-171	5	3	42	3	43	"~10 m (±10 m)". This seems an odd uncertainty range given that there is "high confidence" that sea levels were higher than modern during the warm intervals (previous sentence). It might be better just to remove this sentence, so that we have high confidence that it was higher than modern, but likely less than 20m. [Daniel Lunt, United Kingdom]	Taken into account. Text has been revised.
5-172	5	3	42	3	43	Might be good to check the plioceen statements for consistency with the Last interglacial sea level statements. For the LIG, it is said that we have "high confidence that during the last interglacial period, global mean sea level was between 6 and 10 m higher than present.". If this is really the case, then maybe it could be said that Pliocene interglacial sea levels were at least 6m above modern? [Daniel Lunt, United Kingdom]	Taken into account. Text has been revised.
5-173	5	3	43	3	43	"~10 m (+/- 10 m)" is an error surely [Christopher Hollis, New Zealand]	Taken into account. Text has been revised.
5-174	5	3	43	3	43	Remove "~" If you have a +/- after the number, there is no need to say it is approximate. [Alan Robock, United States of America]	Accepted. Revised text addresses this issue and acknowledges that multiple lines of evidence indicate MPWP sealevel is greater than present, and a revised confidence estimate now reflects assymetric bounds, wth the statement that "that sea-level was very unlikely to have exceeded +25m.
5-175	5	3	43	3	44	Comment text: See comment on p19 line 38 for detail. Section 5.6.1 talks of peak Pliocene level and concludes that this was 10 m (+10 m) with medium confidence but without explanatory reasoning. I have argued for a range between 10 and 21 m (includes 3 m for low-mid latitude ice caps and steric effect). A rise of less than 6m (as the current estimate allows with medium confidence) is inconsistent with 2-3°C rise in global average temperature, and high confidence in a 6-10 m rise in SL during the LIG. [Peter Barrett, New	Taken into account. Text has been revised.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Zealand]	
5-176	5	3	43			Is it intentional that the range of SL change includes 0 / no change? [Government of United States of America]	Taken into account. Text has been revised.
5-177	5	3	44	3	45	I was confused why you mention the low confidence of >20m of sea level rise. Surely the use of "best estimates" in previous sentence shows this. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Text has been revised.
5-178	5	3	44			I am not sure you mean that you have low confidence that sea level was >20 m. To me low confidence means this is a statement you on balance think likely but don't have much evidence for. You are using it here in the opposite sense, to say something you think is unlikely. Given that you have already said you have medium confidence that sea level is 10+/-10, it fllows that you think 20 m is unlikely. My suggestion: extend the previous sentence, adding "of the Pliocene, and was unlikely to have been higher than 20 m above present (medium confidence)". [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Text has been revised.
5-179	5	3	47			The word "only" could be removed as it gives the impression that the East Antarctic Ice Sheet does not provide an important contribution. Due to its large size, however, even a small reduction (percentage wise) in the volume of the East Antarctic Ice Sheet could have large sea level effects. In addition, the relative contributions of the different ice sheets to sea level rise are poorly known for the Pliocene. [Sandra Passchier, United States of America]	Taken into account. Text has been revised.
5-180	5	3	49	3	50	Comment Text: These lines understate what the modelling papers have reported on p40 lines 4-6, and these are supported with geological data. Consider rewording " The Greenland and West Antarctic Ice Sheets lost most of their ice at times during the last few million years that were 2 to 3.5°C warmer than today". From the text of 5.6.1. I'd have though you were justified in saying "It is very likely that the Greenland Ice Sheet was strongly reduced.." [Peter Barrett, New Zealand]	Taken into account. Text has been revised.
5-181	5	3	49	3	50	All the statements under this heading continue the above mid-Pliocene discussion on the stability of the Greenland and West Antarctic ice sheets. The two headings ("There is high confidence..." and "The Greenland and West Antarctic...") can be merged, which will avoid some repetition, and allow a slight shortening of the text. [Giuseppe Cortese, New Zealand]	Taken into account. Text has been revised.
5-182	5	3	49	3	50	The authors should consider deleting the word "have" in have retreated. Consider rephrasing - do you mean to say the 2 ice sheets had smaller land ice mass during past warm periods? [Government of United States of America]	Noted and taken into account in the final structure of the ES statements.
5-183	5	3	49			I would say " The ice sheets were smaller during periods..." Using "have retreated" makes comprehension of the sentence difficult to this native English speaker. [Julia Hargreaves, Japan]	Noted and taken into account.
5-184	5	3	50	3	51	GIS reduced, WAIS absent: I am not clear why you have highlighted a difference between them, as section 5.6.1 does not make any such statement, referring instead to near-complete deglaciation of both (models) and periodic deglaciation of WAIS (data) [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Text has been revised.
5-185	5	3	52	3	52	Change "that West" to "that the West" [Alan Robock, United States of America]	Noted and taken into account in the final draft of the ES statements.
5-186	5	3	52	3	55	Revise to: Ice sheet modeling suggests that the West Antarctic ice sheet is very sensitive to subsurface southern ocean warming and implies with low confidence that a large part of the West Antarctic ice sheet will eventually retreat if the atmospheric CO2 concentration stays above ~400 ppm for several millennia. [Christopher Hollis, New Zealand]	Taken into account. Text has been revised.
5-187	5	3	52	5	55	These sentences indicate projection (not paleo observations or modelling) and they may belong in Chapter 13 [Mark Siddall, United Kingdom]	Noted but the ES statement provides a paleoclimate perspective on a potential CO2 threshold for GIS and WAIS stability relevant to future projections, it is not a projection in itself. Chapter 13 will summarise Chapter 5 sea-level and ice sheet assessments relavent to projections.

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5-188	5	3	52			Typo: insert 'the' into "…simulation suggests that the West Antarctic..." [Jay Curt Stager, United States of America]	Accepted.
5-189	5	3	53	3	53	Comment text: A key result of Pollard and DeConto's 2009 modelling was to simulate the disappearance of WAIS in hundreds of years at 400 ppm atmospheric CO2 levels.Their model suggested to me there should be a least medium confidence that most of WAIS would be lost - not just retreat - in centuries rathan than millenia [Peter Barrett, New Zealand]	Rejected. Pollard and DeConto 2009 do not simulate WAIS collapse in hundreds of years. Nevertheless we have reassessed the likelihood of WAIS collapse as "medium confidence" on the various lines of evidence.
5-190	5	3	53	3	53	Check grammar. What is the subject of verb "imply"? [Olivier Boucher, France]	Noted.
5-191	5	3	53			It's not clear as to what you have low confidence in: the timescale (of several millenia) or the eventual retreat [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted and taken into account.
5-192	5	3	55			Page 47, line 25 uses a value of 420 ppm for this statement not 400. Make consistent. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Taken into account.
5-193	5	3		4		The Executive Summary might give the impression LIG and mid Pliocene sea levels might have been the same [near 10m above present]. This is not supported by the underlying text of the chapter or the scientific literature, more broadly. Pliocene SL was probably 3-4 times higher than LIG [Government of United States of America]	Taken into account - covered in Section 5.6.2.1
5-194	5	4	1	4	1	Please give a range of years for the last interglacial period for better orientation. [Georg Feulner, Germany]	Taken into account - covered in Section 5.6.2.1
5-195	5	4	1	4	1	Insert: "~125,000 years ago" [Christopher Hollis, New Zealand]	Taken into accont.
5-196	5	4	1	4	10	"There is high confidence that during the last interglacial period, global mean sea level was between 6 and 10 m higher than present." is an extremely strong statement given that the finding is primarily based on "only two tectonically stable far-field areas have been studied, Australia and the Seychelles islands", Greenland is unlikely to contribute more than 2m and the unexplained sea level contribution is inconsistent with previous estimates for Antarctica. Discounting the LIG sea level data from much of the rest of the world seems a bit cavalier. The discussion on pages 42 to 44 suggest the data are much more complicated than high confidence merits. At a minumum, it is suggested that the authors rewrite this to state: There is medium confidence that during the last interglacial period, global mean sea level was between 6 and 10 m higher than present." The implication for future doubled CO2 climate is extremely important since the implication is that the equilibrium response to the persistence of a past 1-2 °C warming could be 6 to 10m of sea level rise. From SPM page 13 {For RCP4.5, 6.0 and 8.5, global mean surface air temperatures are projected to at least likely exceed 2°C warming with respect to preindustrial by 2100, and about as likely as not to be above 2°C warming for RCP2.6}. [Government of United States of America]	Noted. The ES and section have been revised.
5-197	5	4	1	4	10	This paragraph ends in a sentence that - on the face of it - seems to conflict with Figure 1.15f. In that figure, the ice-sheet contribution to sea level from Antarctica is indicated for MIS5.5, and it is nearly zero. I think that Fig. 5.15 may need a more precise caption to reflect exactly what is plotted, rather than Antarctic contributions in a general sense. This confusion is potentially misleading if pulled out of context, so it must be sorted out. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account.
5-198	5	4	1	5	10	Great chapter, great improvement. Wish I could review in detail, but I can't. Instead, I'll hope you can help chap 13 with their LIG text and discussion. Please refer to my comments 1-7 (get from TSU) focused on chap 13. They are also focused on your chapter. The WGI needs to agree on what to say, and what confidence to put on things. Chap 13 doesn't agree with you, and ignores the implications for the future that the LIG has as well. Please help them get it right - huge interest among policy-makers. [Jonathan Overpeck, United States of America]	Thank you! Noted.
5-199	5	4	1	10		It should be noted in this summary (remember some readers may read only the summary and not the full text) and other discussions of the last interglacial that the forcing for this relative warming, however apportioned, does not include higher-than-present greenhouse gases, as these were similar to pre-industrial (as noted later in the chapter on Page 21) [William Howard, Australia]	Accepted. Text revised.
5-200	5	4	2	4	2	Please consider to remove the word "change". It will make the sentence more correct and precise, so the	Accepted. Text revised.

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						sentence should read; "Global-mean annual temperature for this period……." [Government of NORWAY]	
5-201	5	4	2			10 meters seems high for the LIG sea level [Government of United States of America]	Noted.
5-202	5	4	5	4	7	McKay et al. (2011) estimate 0.4 ± 0.3 m of global thermal expansion; assuming the uncertainty is a 95% confidence interval (unclear from their manuscript), this represents a significant level of thermal expansion. [Robert Kopp, United States]	Taken into account
5-203	5	4	6	4	6	In order for this sentence to not contradict the statement "data syntheses suggest maximum global warmth of 1-2 degC above pre-industrial temperature", most of the mentioned warming must have occurred on land. Is this the case? [Giuseppe Cortese, New Zealand]	Taken into account. Covered in Section 5.3.4
5-204	5	4	9	5	9	"the West and/or East Antarctic" ... Better "West, and potentially East Antarctic" since nobody really expects the "or" to be East without West as well! [Tasman van Ommen, Australia]	Accepted. ES statement changed accordingly.
5-205	5	4	12	4	12	Insert value in brackets (after the word "change"), in order to be able to compare it with the 25 cm mentioned below [Giuseppe Cortese, New Zealand]	Noted.
5-206	5	4	12	4	14	No rate is cited in the bold text, yet a 25 cm magnitude of sea level change is listed in the example. Rates (i.e. ratio between measurements) and magnitudes (size of change) are not the same. Clarification is needed. [Government of Canada]	Taken into account. Clarified in the revised text
5-207	5	4	12	4	14	The first sentence is a statement, the second a documentation, but it is not possible to compare the 25 cm for the oscillations during the last few thousand years with the current global sea level changes since a figure for the latter is lacking. We suggest that you change the first sentence to; "The rate of current global sea level change xx cm during the last yy years is unusually high in the context of the past millennium (or millennia)". [Government of NORWAY]	Noted. Clarified in revised text.
5-208	5	4	12	4	14	Medium confidence seems too strong a statement given the lack of a global SL curve for this period. Also, the statement should add "global" because regional SL curves show large oscillations, as do glacier records implying SL variability [Government of United States of America]	Noted. The revised text and ES account for regional vs global variations.
5-209	5	4	12	4	14	Oscillation may imply a fixed frequency, maybe variations would be more appropriate. The sentence compares the RATE of change today with the total range of variation in the past, but I don't see how that implies that the rate of change today is bigger. To make that conclusion the rate today should be compared to a rate (in cm/century for example) in the past. [Reto Knutti, Switzerland]	Taken into account.
5-210	5	4	13	4	13	Is it possible to better specify what "few" means (i.e., provide an approximate number)? [Giuseppe Cortese, New Zealand]	Taken into account. Text has been revised to provide more accurate statements.
5-211	5	4	14			This number of 25cm seems low, and in the follow up section, it doesn't seem to be consistent. The 8k event would have been 25 cm within a year, for instance, and Heinrich events bigger. Periods of fast melting (early Holocene), likely did exceed this number. This doesn't seem consistent with later write up and should be revised accordingly. [Government of United States of America]	Taken into account. Text has been revised to provide more accurate statements.
5-212	5	4	18	4	18	In addition to paleoclimate simulations and other proxy-based studies, several studies using pollen-climate calibration sets show that a cooling trend has actually prevailed during the last ca. 5-4,000 years in western Canada and Scandinavia (Neoglacial cooling). Long-term late Holocene cooling in the NH is mentioned on page 30, line 56 though given that it is a prominent feature in (NH) Holocene climate history, elaboration is warranted. See, for example: Antonsson et al. 2008: Anticyclonic atmospheric circulation as an analogue for the warm and dry mid-Holocene summer climate in central Scandinavia. Climate of the Past 4, 215-24. Seppä, H. and Birks, H.J.B. 2001: July mean temperature and annual precipitation trends during the Holocene in the Fennoscandian tree-line area: pollen-based climate reconstructions. Holocene 11, 527-539. Seppä et al. 2009: Last nine-thousand years of temperature variability in Northern Europe. Climate of the Past 5, 523-35. Heusser CJ et al., 1985. Late-Quaternary climatic change on the American North Pacific Coast. Nature, 315: 485-487. [Government of Canada]	Accepted. Revised section 5.5 also addresses multi-millennial Holocene trends.

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5-213	5	4	18	4	18	"reversed": is this the right English word? What is actually meant here? "compensated"? "more than compensated"? [Masa KAGEYAMA, France]	Reversed is probably the best word, a cooling trend has stopped and a warming rend has come in its place. Will consider clarity in revision.
5-214	5	4	18	4	20	What is "long-term" in this context? Has the NH not warmed during the 19th century? [Olivier Boucher, France]	Taken into account. Text has been clarified.
5-215	5	4	18	4	20	High confidence? I would call this medium confidence, for the general reasons described in section 5.3.5.2: limited observational network, especially in the tropics and at high latitudes (especially the Southern Oceans) [Michael Neil Evans, United States of America]	Rejected because the statement is explicit about the restrictions (mid to high latitudes, northern hemisphere)
5-216	5	4	19	4	19	Qualify (cooling/warming?) this trend: is this the warming or cooling (should be this one) trend mentioned in the title? [Giuseppe Cortese, New Zealand]	Rejected. Text is clear about cooling trend.
5-217	5	4	19	4	19	Most importantly, discuss the level of confidence of (and evidence for) the opposite trend (the one observed during the 20th century). [Giuseppe Cortese, New Zealand]	Rejected. Chapter 5 does not assess the level of confidence for 20th century trends based on instrumental data (this is covered in Chapter 2)
5-218	5	4	19	4	19	Long-term is an expression that creates ambiguity... By reading this sentence one gets the feeling that nature is pushing, in a long-term fashion, towards a cooling. It may do, but only if one observes "just" the last 2000 years (which suddenly become very short-term if compared to glacial-interglacial shifts). [Giuseppe Cortese, New Zealand]	Taken into account. The statement has been revised.
5-219	5	4	19	4	20	Section 5.5.1 does not attribute a systematic preindustrial long-term cooling trend to orbital forcing at the continental scale throughout the last 2000 years. High confidence on this issue appears only valid for the Arctic (see the first lines of page 5-31) although even there some additional forcing such as volcanic activity (see for instance Miller, G. H., et al. (2012), Abrupt onset of the Little Ice Age triggered by volcanism and sustained by sea-ice/ocean eedbacks, Geophys. Res. Lett., 39, L02708, doi:10.1029/2011GL050168.) also played a role. [Hugues Goosse, Belgium]	Taken into account. The statement has been revised.
5-220	5	4	19	4	20	Are the long-term temperature variations over the last 2000 years soley due to orbital forcing? [Janice Lough, Australia]	Taken into account. The statement has been revised.
5-221	5	4	22	4	22	define 'ka' [Government of Australia]	Accepted.
5-222	5	4	22	4	22	Please consider to replace "reduced compared to" with "less than" so the sentence reads; "Summer sea ice cover between 8 ka and 6,5 ka was less than late 20th Century levels both in the Arctic Ocean and along East Greenland". [Government of NORWAY]	Accepted.
5-223	5	4	22	4	25	What is the late 20-th century? Give the years. However, the key issue is how unusual the years 2007-2012 are. A comparison to this period should be added. [European Union]	Accepted.
5-224	5	4	22	4	25	What is the late 20-th century? Why do you have to be vague? Give the years. The key issue is not that period anyway. The key period is how unusual the years 2007-2012 are. This is the period you should be comparing with. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-225	5	4	22			The word "reduced" implies a negative trend even though the ice cover experienced all sorts of variations from then until now. Why not use a purely factual expression such as "Summer sea ice cover between 8 ka and 6.5 ka was less than that in the late 20th century"? [Government of France]	Accepted
5-226	5	4	22			Typo: no capital letter on "Century" (20th century) [Jay Curt Stager, United States of America]	Accepted
5-227	5	4	23	4	23	For sea ice reduction at early to mid-Holocene, could you add an explanation (as is done in the SPM). [SYLVIE JOUSSAUME, France]	Taken into account in the revised text
5-228	5	4	25	4	25	But they are not anomalous in a slightly longer perspective, as mentioned in the title (referring to 8-6 ka). [Giuseppe Cortese, New Zealand]	Rejected. Less clear evidence fo enable an executive summary statement.

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5-229	5	4	25	4	25	It would also be interesting to add a similar comment (and supporting evidence) for the other major sea ice field on Earth: the one around Antarctica. [Giuseppe Cortese, New Zealand]	Rejected. Less clear evidence fo enable an executive summary statement.
5-230	5	4	27	1	28	"The current near-global recession of glacier length is unusual in the context of the last 2 millennia [medium confidence]" - I would not include this in the ex sum - we know very few about the glacier length during the warm periods of the last 2ka and there are some evidences that during the MCA the glaciers were smaller than now in several regions. [Olga Solomina, Russia]	Taken into account.
5-231	5	4	27	4	29	Aren't some glaciers and ice sheet margins retreating due to ocean temperatures, not only surface air temperature? [Government of United States of America]	Noted
5-232	5	4	27	4	29	"recession of glacier length" - either recession or length changes [Olga Solomina, Russia]	Taken into account.
5-233	5	4	29	4	29	include larger in area so it can not be misunderstood as larger (in rate) [European Union]	Taken into account.
5-234	5	4	29			Better write " ... in some regions ..." - the information is by far not good enough to make a statement about "most" regions. The term "retreat" is correct but relates to the length and not to the volume or size of a glacier. Therefore correctly write " ... are longer ..." (instead of larger). This is a fundamentally important aspect which must be treated more seriously in the text: the length change of glaciers is a heavily delayed response to climate change. With other words: looking at a glacier tongue or margin (and this is usually done in paleoglacier research) means to look many years or even several decades back in time with respect to climatic conditons. Make reference in the text to the overview paper by Solomina et al. (2009): Solomina, O., Haeberli, W., Kull, C. and Wiles, G. (2008): Historical and Holocene glacier-climate relations: general concepts and overview. Global and Planetary Change 60, 1-9. This paper explains the glaciological background which is often neglected in interpretations of former length changes. The term "now" is also unacceptable (sorry): "now" or "present-day" and similar terms are frequently used in the scientific literature but do not at all mean "now" (2012?) but often something highly unprecise like "at the end of the 20th century", "in recent years" or "at the time of mapping" (whenever this was).To be strict and precise is absolutely essential in comparison with times of rapid if not accelerating change. [Wilfried Haeberli, Switzerland]	Taken into account.
5-235	5	4	29			It is not clear what is implied by the word "still" here. Would replacing it by "even" convey what is intended. Or is "still" meant to convey that the result still holds here in AR5 as it did in AR4? [Adrian Simmons, United Kingdom]	Accepted
5-236	5	4	30	4	31	Using the confidence scale and the likelihood scale for two identically framed statements is strange. Is medium confidence supposed to be the same thing as 'likely'? Please maintain consistency here. [Gavin Schmidt, United States of America]	Taken into account. Addressed by using likelihood language together with a confidence attribute to clarify differing levels of confidence between the two periods considered.
5-237	5	4	31	4	38	This summary point mixes the likelihood and confidence langauges. Why do some ES points have confidence words in brackets at the end and some don't? What do 'medium evidence' and 'medium agreement' mean? They seem very odd words to use. Perhaps this ES point should be split into the two. The final sentence should be a separate point in itself, as it worth stressing separately. In other words there isn't just the proxy evidence there is our modelling of the system and our reconstruction of the forcing histories. All three are independent of each other and they combine in a mutually supportative way. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	See 5-236
5-238	5	4	34	4	34	medium "evidence" or "confidence"? [Government of Australia]	See 5-236
5-239	5	4	34			"medium evidence" and "medium agreement" are new terms. This seems inconsistent with the usual "likely" and "confidence" terms used when speaking about uncertainty. Evidence + Agreement = Some level of confidence according to Figure 1.12 [Government of United States of America]	See 5-236
5-240	5	4	35	4	36	There is nothing like "true uncertainty" and the phrase "and the published uncertainty ranges may underestimate the true uncertainty." reads very awkwardly. Uncertainty is not identical to variability or some error estimates (the latter being in the cases of true measurement errors a valid proxy for uncertainty). I know that equating those terms is commonly done, but I belive a mistake done by many does not excuse it. Please correct the text accordingly. [Andreas Fischlin, Switzerland]	Noted, but this sentence has been removed from the revised text.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-241	5	4	36	4	36	give the main forcings [Government of Australia]	Accepted.
5-242	5	4	36	4	36	To improve understandability we suggest that you consider to replace "Simulations forced by natural and anthropogenic radiative changes……" with "Model simulations with forcing of both natural and anthropogenic radiative changes……." [Government of NORWAY]	Taken into account. Text reworded.
5-243	5	4	36	4	36	“may” seems a little vague. Re-word. We know quite clearly that the published uncertainties are underestimates of the true uncertainty. We just don’t know by how much for a variety of factors. [Rob Wilson, United Kingdom]	Noted, but this sentence has been removed from the revised text.
5-244	5	4	36	4	38	on the model-data comparisons for the last millenium. Maybe a word on the uncertainty in the forcings should be added here? [Masa KAGEYAMA, France]	Noted. We already say "broad uncertainties" and this covers all sources: forcing, reconstruction, internal variability.
5-245	5	4	37	4	37	what about SH and global temperatures? [Government of Australia]	Noted, but limited space to expand the ES. Covered in the main text.
5-246	5	4	37	4	37	Please, insert “relatively” before consistent. As is noted in the main text, the models do not capture the timing and amplitude of the Medieval Climate Anomaly that well and have an overall tendency to underestimate centennial scale variability. [Fredrik Ljungqvist, Sweden]	Accepted. The ES statement has been modified taking this into account. Instead of ' radiative changes during the last millennium are consistent' now the text reads ' radiative changes during the last millennium are in general consistent'.
5-247	5	4	40	4	40	include mean period of MCA (950 CE - 1250 CE) (page 5-22, line 49) [European Union]	Noted. A Table has been included in the chapter with the reference time intervals used for various relevant periods and acronyms used in the text.
5-248	5	4	40	4	41	The authors should consider deleting 'In contrast to the late 20th century'. The instrumental record does not show a pattern of higher temperatures that were consistent across seasons and regions for the 20th century. In fact the interesting climate science is why higher temperatures have been inconsistent seasonally and regionally. A potential re-drafting could read: "There is high confidence that the Medieval Climate Anomaly was not characterized by a pattern of higher temperatures that were consistent across seasons and regions" [Government of United States of America]	Accepted. The statement has been modified taken this into account. The comparison to the 20th century is however kept, based on the assesement of Section 5.5.1 and Fig. 5.12
5-249	5	4	40	4	42	Proxy data are just not reliable enough to make such statements. I would say there is not even low confidence for this claim. Local evidence points in the opposite direction, i.e. MWP at least as warm or warmer than the current warm period, see http://www.co2science.org/data/mwp/description.php. Why would so many local studies show a medieval warm period that is at least as warm as it is today while at the same time NH multiproxy reconstructions (with all its problems) would show the opposite? I repeat an advise of McIntyre here to look more at complete ecological analyses. Two examples are Mukhtar M. Naurzbaev, Malcolm K. Hughes, Eugene A. Vaganov, 2004. Tree-ring growth curves as sources of climatic information, Quaternary Research 62, 126-133 and Millar, C.I., J.C. King, R.D. Westfall, H.A. Alden, and D.L. Delany. 2006. Late Holocene forest dynamics, volcanism, and climate change at Whitewing Mountain and San Joaquin Ridge, Mono County, Sierra Nevada, CA, USA. Quaternary Research 66 (2006): 273-287. Millar finds: "The paleo climate modeled for Whitewing during the Medieval period was significantly warmer and slightly drier than present (Table 4). Medieval mean annual minimum temperature was warmer than current by 3.2°C, with large differences in winter (+3.5°C, January) and summer (+4.0°C, July). Mean annual maximum temperature was also greater in the Medieval period (+2.3°C), with greater differences in winter (+3.2°C, January) than summer (+2.6°C, July)." Note that the research area of Millar is close to the oft used bristlecone pines from the White Mountains. [Marcel Crok, The Netherlands]	This comment has been considered. The statement has been modified to reflect the assessment in Sections 5.3.5 and 5.5.1 and Fig. 5.12. Fig. 5.12 shows a comparison, including uncertainties, of the temperatures various continental regions between the MCA and present.
5-250	5	4	40	4	45	This paragraph would be more useful to policy makers if it was in plain English - especially the bolded sentence. [Government of Australia]	Noted. The statements have been changed, also trying to make them more clear.
5-251	5	4	40	4	45	This is a good and well written conclusion. The implication that the period 1981-2010 was characterized by consistently higher temperature anomalies spatially and seasonally could be made more explicit. [European Union]	Noted. This has been taken into account in the new version.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-252	5	4	40	4	45	Can this be reworded? Does it mean there is asynchroneity in the timing of MCA warmth? There is evidence for solar and volcanic forcing as well as internal processes [Government of United States of America]	Noted. The statements have been changed, also trying to make them more clear regarding the timing of events.
5-253	5	4	40	4	45	The assignment of "High" confidence to the statement that the "Medieval Climate Anomaly was not characterized by a pattern of higher temperatures that were consistent across seasons and regions", is belied by the later caveat about uncertainties in reconstructed temperatures. There is still very low overall global coverage of palaeclimate records (especially over the ocean and Southern Hemisphere) over this time period. So this statement should be assigned medium or low confidence. [William Howard, Australia]	Noted. See 5.5.1 and Figure 5.12 for evidence on various continents and regions. The phrasing of the statement has been modified also to account for these uncertainties. The high confidence stems from the assessment in Fig 5.12, including reconstruction uncertainty from each continental region.
5-254	5	4	40	4	45	This is a good and well written conclusion. The implication of it is that the period 1981-2010 was characterized by consistently higher temperature anomalies spatially and seasonally. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. This has been taken into account in the new version.
5-255	5	4	40	4	45	I do not agree that we with “high confidence” can state that the Medieval Climate Anomaly was not characterized by geographically coherent warm conditions. Firstly, we have too little data and much of the data have a high noise level. Secondly, dating uncertainties in many proxy types – as well as the sparse coverage – makes it close to impossible to compare any 30-year (or 50-year) period during the Medieval Climate Anomaly with the last 30 (or 50) years on a larger geographic scale. I would, at most, give “medium confidence” to the statement. [Fredrik Ljungqvist, Sweden]	Noted. See 5.5.1 and Figure 5.12 for evidence on various continents and regions. The phrasing of the statement has been modified also to account for these uncertainties.
5-256	5	4	40	4	45	Make it clear whether talking globally or just NH. [Janice Lough, Australia]	Accepted. The paragraph explicitly addresses continental scales now.
5-257	5	4	40	4	45	I find this paragraph too vague. The bit "was not characterized by a pattern of higher temperatures that were consistent across seasons and regions" is ambiguous, and gramatically convoluted. I think this should be simplified to bring out the true meaning. I would make a suggestion, were it not that I can't figure out exactly what the intended meaning would be.. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Noted. The statements have been changed, also trying to make them more clear.
5-258	5	4	40		41	the Medieval Climate Anomaly was characterized by a warm climate condition firstly, rather than by a pattern of higher temperature that were not consistent across seasons and regions. [Jingyun Zheng, China]	Accepted. The paragraph has been modified introducing first an statement as suggested.
5-259	5	4	40			This is grammatically wrong: as written it suggests that we don't have confidence in the consistent pattern of the late 20th century. It should be worded: "There is high confidence that the MCA, in contrast to the late 20th century, was not characterised..." [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted. The statement has been modified taken this into account.
5-260	5	4	42	4	42	Seems to contradict previous comment No. 1 [Janice Lough, Australia]	Noted. The statement has been reprased trying to make it clear and less contradictory.
5-261	5	4	43	4	43	Add the abbreviations MCA and LIA [Government of NORWAY]	Accepted.
5-262	5	4	47	4	48	The discussion of North American megadrought in section 5.5.5 is limited to the southwestern United States. Hence, the statement "In most regions, proxy information from the last millennium provides evidence of megadroughts of greater magnitude and longer duration than observed during the 20th century" is not fully supported by data presented in the report. [Government of Canada]	Taken into account. ES has been partly rewritten.
5-263	5	4	47	4	50	It might be worth mentioning N American MCA megadroughts, which are well-documented [Government of United States of America]	Taken into account. ES has been partly rewritten. Rather than focussing on N America, the sentence reads: During the last millennium, there is high confidence for droughts of greater magnitude and longer duration than observed since the 20th century in many regions.
5-264	5	4	47	4	50	Is this really true for "most regions" - rather only those for which we have robust reconstructions. Also why treat (here and elsewhere) "megadroughts" as something different from "megafloods"? Extended dry and extended wet periods would be better terminology. And wetter conditions mentioned here but "paleofloods"	Taken into account. ES has been partly rewritten.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						described in next point (page 4, lines 52-55. [Janice Lough, Australia]	
5-265	5	4	49	4	49	Spell out LIA [Government of Australia]	Done.
5-266	5	4	49	4	49	Change "LIA" to "Little Ice Age" It has not been previously defined, and these statements need to stand on their own without acronyms. [Alan Robock, United States of America]	Done.
5-267	5	4	49			“LIA” is used but not defined. [Julia Hargreaves, Japan]	Done.
5-268	5	4	49			Acronym "LIA" is not defined. It can be introduced in line 43, where there is a reference to the Little Ice Age. [Adrian Simmons, United Kingdom]	Done.
5-269	5	4	49			Should mention East Africa in here as well, because it was also wet then and this is an important part of the world. I suggest "… and wetter conditions prevailed in East Africa and in the South American…" [Jay Curt Stager, United States of America]	Taken into account. ES has been partly rewritten. The sentence now reads: There is medium confidence that more megadroughts occurred in monsoon Asia and wetter conditions prevailed in arid Central Asia and the South American monsoon region during .....
5-270	5	4	52	4	53	Using the phrase "past centuries' is unclear. Consider changing to: 'past four or more centuries' -- Figure 5.14 indicates 4 centuries at a minimum and up to 7 centuries.. [Government of United States of America]	Taken into account in the revised text
5-271	5	4	52	4	55	palaeo-floods. Is there any impact of man in this statement (through the building of dams, river management, for instance?) [Masa KAGEYAMA, France]	Rejected. ES does not include evidence on this issues. Within section 5.5.5 there is information provided.
5-272	5	4	52			I wondered why you chose to highlight the first sentence rather than the second. Either way shows bias. You might want to not highlight either [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Taken into account. ES has been partly rewritten.
5-273	5	4	52			Clarification needed: large floods are reconstructed for WHICH "past centuries?" The last millennium or the whole Holocene? [Jay Curt Stager, United States of America]	Taken into account. ES has been partly rewritten.
5-274	5	4	55	4	55	This sentence is the opposite of the title, and makes the observation inconclusive (discharges higher compared to previous centuries in some regions, but not in others). Interesting for the regional variability, but the "exception" might be brought forward in the title (example: "...and eastern Asia (high confidence), but are comparable etcetc in other regions (Near East, etcetc". [Giuseppe Cortese, New Zealand]	Taken into account in the revised text
5-275	5	4	55	4	55	If the suggestion above is adopted, a few lines should probably be added (maybe on the evidence used for these statements, or on the "regional flavour" of such climatic events). [Giuseppe Cortese, New Zealand]	Rejected. Due to space limitation of the ES, no information can be provided.
5-276	5	4		5		The subjects raised in this part of the executive summary are fascinating, but seem somewhat ecclectic. Could they be framed and arranged in a way that makes their context clearer ans show why these findings are important and what is their policy relevance? [Jochen Harnisch, Germany]	Taken into account. ES has been partly rewritten.
5-277	5	5	1	5	1	The term "hydroclimate" is unclear. It could be replaced with "rainfall patterns" or "precipitation patterns" as in tropicis and substropics droughs are associated with precipitation and not with the temperature changes [Victor Brovkin, Germany]	Accepted. Term changed to "precipitation patterns".
5-278	5	5	1	5	1	A hyphen is not needed for North Atlantic; what regions are meant exactly? [Government of United States of America]	Hyphen deleted, detailed regions to be found in 5.7
5-279	5	5	1	5	5	Make it clear what time scales these statements are referring to. [Janice Lough, Australia]	Comment unclear. The length of glacial stadials is very well known.
5-280	5	5	1	5	5	Switch the first and second sentences. The bold statement should be the actual science, not how it compares to AR4. [Alan Robock, United States of America]	Accepted. Sentence changed.
5-281	5	5	2	5	5	North Atlantic abrupt events and simultaneous hydrological changes in the tropics. I think proxy evidence shows simultaneous changes, but it is hard from the proxy only to state there is causality (i.e.North Atlantic events causing ITCZ/monsoon changes). Model experiments (including my own, cf. Kageyama et al, CP, 2009, Marzin et al, submitted to CP) have shown that this could be the case but do not actually rule out a	Noted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						tropic to northern extratropics connection. So we should be careful with the "causality" link here. [Masa KAGEYAMA, France]	
5-282	5	5	3	5	5	Here the statement that"…North Atlantic cooling…affected global monsoon systems" is correct. However, this paragraph, together with the entire Section "5.7", gives again a false impression that the abrupt climate changes originate only from the high-latitudes, and the high- and low-latitudes teleconnection is a one-way street. As a scientific report, it should be indicated here is growing evidence for abrupt climate changes of low-latitude origin (see Chiang, 2009, Annu. Rev. Earth Planet. Sci.) and for tropical Pacific influence on North Atlantic climate (e.g., Turney et al., 2004, Nature). [PINXIAN WANG, China]	Noted. There is definitely evidence for abrupt climate change in the tropics on millennial timescales. However, there is no clear indication that this variability is actually generated in the tropics, whereas there is very clear evidence from model experiments that tropical abrupt changes associated e.g. with DO events can be triggered by extratropical mechanisms.
5-283	5	5	5	5	38	No comments on the text related to my experties [Valter Maggi, Italy]	Noted.
5-284	5	5	5			"(albedo) of snow itself may also be changing": Is it incresing or decreasing? By how much? [Government of United States of America]	This comment seems to be unrelated to the text. Maybe it was misplaced.
5-285	5	5	9	5	11	a new reconstruction of ENSO... give a confidence rating here? Based on my reading of sections 5.3.5.2 and 5.4.1, I would revise this sentence to: "Accumulating evidence for ENSO variance through the mid to late Holocene from tropical temperature and hydrometeorological proxy data suggests there is a large range of natural variability in ENSO, and no clear response of ENSO amplitude to external forcing. (medium confidence)" [Michael Neil Evans, United States of America]	Text has been revised to include confidence statement.
5-286	5	5	9	5	11	My interpretation of the new reconstruction (Cobb et al in press) is that they present evidence for ENSO variability occurring throughout the Holocene - not "high levels of variability"; indeed they suggest that 20th century ENSO variability is unusually high. [Janice Lough, Australia]	Taken into account, and text revised for clarification.
5-287	5	5	9	5	11	Clarification suggested: perhaps worth mentioning why this revision is important enough for a general audience to warrant mentioning it here. [Jay Curt Stager, United States of America]	Noted. It is an important change in the scientific consensus which needs to be mentioned.
5-288	5	5	9	5	11	From the text « A new reconstruction from the equatorial Pacific and modelling experiments show high levels of variability in El Niño-Southern Oscillation (ENSO) behaviour throughout the entire Holocene". This is not completly exact since only the coral reconstruction is from the "entire Holocene" whereas cited model experiments (5.4.1) come only from 6k. It's important since the cited paleo data indicate variability of ENSO that should be compare with transient models and not with a snapshot at 6k. [Bruno TURCQ, France]	Noted. There is no current modeling paper looking at various transient model simulations throughout the Holocene and potential ENSO changes.The text has been reformulated to be consistent.
5-289	5	5	9	5	11	Moreover, as in other parts of the report it seems that the newest is the best. This bias is of course due to the fact that this AR5 report only complete AR4 with the newest studies but it also could be interpreted that this new record is outlined BECAUSE it is more coherent with the model results [Bruno TURCQ, France]	Noted. In fact, the new record is NOT coherent with the latest model results, as stated in the revised ES statement.
5-290	5	5	9	5	11	This statement appears to be based on a single study/reconstruction. Do you consider this robust enough to be elevated to the executive summary, and therefore, potentially the TS/SPM? [Thomas Stocker/ WGI TSU, Switzerland]	Noted. However, the Cobb study presents the most comprehensive ENSO-resolving dataset from the main center of action of ENSO. Together her dataset includes about 1000 years of monthly resolved d18O, which is highly correlated with Nino 3.4 SST (typical correlation >0.85). Most other datasets that were for instance cited in AR4, are in ENSO-teleconnected regions and do not even resolve interannual variability (except for 2 short coral segments from the Western Pacific) and have difficulties distinguishing background changes from ENSO variance changes.Their present-day correlation with ENSO SST anomalies, if present, is considerably lower than for the Cobb dataset. It is fair to say that the Cobb data present an unprecedented and the most accurante view into ENSO variance changes, at least for the segments analysed.
5-291	5	5	10	5	11	The new reconstruction does not really weaken the evidence assessed in AR4, it conflicts or disproves.	Accepted. Text changed accordingly.

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						Therefore, a suggested re-write of the sentence could be: "These new results contrast with the previous finding in AR4 for reduced ENSO variance during the early-to-mid Holocene." [Government of United States of America]	
5-292	5	5	13	5	14	Please describe the weak effect (decrease or increase in frequency, variance, amplitude?). The reader is left wondering what to do with this. [Andreas Fischlin, Switzerland]	Taken into account in the revised text.
5-293	5	5	14	5	14	ENSO statistics - term is too vague - frequency, variance? [Christopher Hollis, New Zealand]	Taken into account in the revised text.
5-294	5	5	15	5	15	ENSO variance is unrelated to many other phenomena. Explaining why one would want to test whether ENSO is related/unrelated to solar variability would be important to mention here. [Giuseppe Cortese, New Zealand]	Rejected.This would be beyond the scope of the executive summary, which just summarizes the key points from the main text.
5-295	5	5	23	5	23	This is generally a very good ES. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Thank you!
5-296	5	5	24			Should there be a sentence on abrupt climate changes in the executive summary? [Government of Australia]	Taken int account. ES statement on abrupt change has been added.
5-297	5	5	24			We noted that there is currently no mention of abrupt climate changes in the executive summary, which means that two entire sections (5.7 and 5.8) are not covered in the executive summary. [Thomas Stocker/ WGI TSU, Switzerland]	Taken int account. ES statement on abrupt change has been added.
5-298	5	5	25	5	25	so called' comes accross as perjorative. How about 'which are often called terminations (ref)' or similar [Mark Siddall, United Kingdom]	Taken into account.
5-299	5	5	52		55	It may appear strange that the bold text refers to regions where paleofloods were "worse", while the remaining non-highlighted text refers to regions where this is not true. You could consider using sentences from 5.5.5 for the bold text, e.g.: In general, paleoflood discharges larger than those recorded during the 20th century are documented, although in some regions (e.g., Near East, India, central North America) modern large floods are comparable or surpass in magnitude and/or frequency historical floods. The remaining of the text (not in bold) may then provide details, such as the time period for this data. [Philippe Marbaix, Belgium]	Taken into account in the revised text.
5-300	5	6	1	6	1	Text on figure is too small to read in parts B and C. I also feel that the figures should be renumbered because box 5.1 on box 5.2 are essentially different figures not part of a single figure [Peter Clift, United States of America]	Noted.
5-301	5	6	1	6	51	While section 5.1 is helpful to understand the scope and the structure of the chapter easy to understand, the meaning of the title 5.2 has remained unclear. See also my comment on entire chapter. [Andreas Fischlin, Switzerland]	Taken into account. The title of 5.2 has been modified.
5-302	5	6	5	6	5	Please cite that chapter properly, i.e. Jansen et al., 2007. Later you cite it properly, and you need to make the connection clear here. Jansen, E., Overpeck, J., Briffa, K. R., Duplessy, J.-C., Joos, F., Masson-Delmotte, V., Olago, D., Otto-Bliesner, B., Peltier, W. R., Rahmstorf, S., Ramesh, R., Raynaud, D., Rind, D., Solomina, O., Villalba, R., & Zhang, D., 2007. Paleoclimate. In: Solomon, S., Qin, D., Manning, M., Chen, Z., Marquis, M., Averyt, K. B., Tignor, M., & Miller, H. L. (eds.). Climate change 2007: The Physical Science Basis. Contribution of Working Group I to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change (IPCC). Cambridge University Press: Cambridge, UK and New York, NY, USA. 433-497. (http://www.ipcc.ch/) Ja071 [Andreas Fischlin, Switzerland]	Taken into account.
5-303	5	6	7	6	7	It would be useful to provide a definition of "Earth-system"; what elements does it encompass? [Janice Lough, Australia]	Taken into account. Definitions have been added in Box 5.1
5-304	5	6	8	6	8	The sentence states that paleoclimate data are useful to learn about the response to solar, volcanic and orbital forcing, but does not mention changes in atmospheric composition which is confusing. [Georg Feulner, Germany]	Taken into account.

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5-305	5	6	8	6	10	"which acted in the past and will affect future climate evolution" sounds confusing -as though the past forcing will affect future climate evolution. Please clarify. Also, the authors should consider inserting the word "comparatively" between "evaluated from" and "short instrumental records" in line 10. [Government of United States of America]	Taken into account.
5-306	5	6	8			Are GHG not included b/c they are considered to be a feedback? Some of the feedbacks that GCMs use as forcings (GHG, topography, etc.) should at least be spelled out here. [Government of United States of America]	Noted. Chapter 1 now has an additional paragraph about paleoclimate modelling (boundary conditions etc).
5-307	5	6	9	6	9	A reader of an AR is not interested in studying a scientific topic. Therefore a statement such as "allows the study of Earth-system feedbacks" is a turn-off. Perhaps you could write: "allows to understand Earth-system feedbacks"? [Andreas Fischlin, Switzerland]	Taken into account.
5-308	5	6	9	6	10	There is a conflict with Fig.1 in Box 5.1 where in the caption it says that "….response to external forcings on timescales of hours to millions of years" [Government of Poland]	Taken into account. However the introduction sentence aimed at highlighting the added value of paleoclimate records with respect to instrumental data, with a focus on long time scales.
5-309	5	6	15			What is "transient modeling" as opposed to "more comprehensive modeling"? This warrants a deeper explanation. [Government of United States of America]	Taken into account. The text has been revised.
5-310	5	6	17	6	17	complexity --> complexities? [Masa KAGEYAMA, France]	Noted.
5-311	5	6	19			There is overlap with Ch. 6 here- Is Chapellez the only ice core author? - First two Exec Summary points are paleo-CO2, CH4; most of first 15 pages are paleo [Government of United States of America]	Taken into account. The overlap with chapter 6 has been clarified.
5-312	5	6	19			There is overlap with Ch. 6, p. 15-16 Abrupt Glacial Events=paleo too; page 6-20- are exponents meant in "Reaction" part? [Government of United States of America]	Taken into account. The overlap with chapter 6 has been clarified.
5-313	5	6	19			There is overlap with Ch. 10 here: D&A-10-17 - No AMO Paleorecords? General: Considering the conclusion about anthropogenic forcing, there is remarkably LITTLE paleo-citations to defend it; ie, 10-36-38 sea ice NO Paleo-sea ice mention, such as Early Holocene; in contrast, 44-30 on droughts is more conservative based on paleo-data [Government of United States of America]	Taken into account. Consistency with chapter 10 has been revised. Chapter 5 refers to AMO and sea ice.
5-314	5	6	21	6	24	The authors should consider the following re-write: "The chapter proceeds from evidence for changes in atmospheric composition and external climate forcings, including solar and volcanic activity (Section 5.2, FAQ 5.1), to consideration of global and hemispheric responses. Several open debates are covered, including the temporal relationship between atmospheric CO2 content and temperature during glacial-to-interglacial transitions (so-called terminations; [Section 5.3.2]), paleoclimate constraints on climate sensitivity (Section 5.3.3) and the evolution of surface temperatures during the past 2000 years (Section 5.3.5)." {This revision puts the sections in order} [Government of United States of America]	Taken into account.
5-315	5	6	25	6	25	Insert "glacial" before the word "terminations" [Giuseppe Cortese, New Zealand]	Taken into account.
5-316	5	6	33	6	33	"polar amplification" of global temperature changes [Masa KAGEYAMA, France]	Taken into account.
5-317	5	6	33	6	33	Write "polar amplification *of temperature changes* is addressed". [Manfred Mudelsee, Germany]	Taken into account.
5-318	5	6	35	6	41	The authors should consider moving this section to precede lines 21-26. [Government of United States of America]	Taken into account.
5-319	5	6	44	6	46	Awkward formulations. "Will finally" does not make sense in a final product such as an AR. "Will" is all that is needed. Please rephrase that all text becomes clear and mor concise. [Andreas Fischlin, Switzerland]	Taken into account.
5-320	5	6	44			Is "finally archived" what is really meant? Or does the writer mean "permanently archived"? There is a difference in meaning and "finally archived" is not typically used in the community. [Government of United States of America]	Taken into account.
5-321	5	6	44			"Eventually" would be better than "finally", though if the data will be archiived by the time the assessment is published, "will finally" can be replaced by "are". [Adrian Simmons, United Kingdom]	Taken into account.

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5-322	5	6	48	6	49	Can information be given as to whether the quoted ages are median ages or 1-sigma or 2-sigma ranges? At the very least, this information should be added to the text to provide clarification about what the calendar ages actually represent. [Government of Canada]	Age ranges for the time intervals being covered are provided in the new Table 5.1
5-323	5	6	49	6	49	Give an indication what is "present" in age values (e.g., 1950 CE). [Manfred Mudelsee, Germany]	Taken into account.
5-324	5	6	51			Section 5.2: excellent [Manfred Mudelsee, Germany]	Thank you!
5-325	5	6	53	9	5	Section 5.2.1: This section does nowhere discuss the relative relevance of the external forcings discussed in detail and in isolation from each other. I suggest a small new section at the beginning does that or refers at least to the place where that happens (should it come later in the chapter). [Andreas Fischlin, Switzerland]	Noted. Radiative forcing is discussed extensively in chapter 8, the relative contributions are illustrated in the figure of the FAQ 5.1
5-326	5	6	55	7	1	Please ensure hanging headers of this type do not appear in the final version of the text. [Peter Burt, United Kingdom]	Noted.
5-327	5	6		28		In general, I found the organisation of within the subsections of section 5.2 and withink section 5.3 difficult to follow, because we constantly jump from one period to another. Maybe some lines should be added in the introduction to introduce the climates/periods which will be focussed on and why. [Masa KAGEYAMA, France]	Taken into account. The text has been rewritten to better guide the readers.
5-328	5	6		48		Related to comment 51: some references seem to be pertaining to AR4 rather than AR5. Some sections contain many of those, while others seem to have chosen to only include new (post-AR4) references. Maybe this should be homogenized throughout the chapter? [Masa KAGEYAMA, France]	Noted.
5-329	5	6				On the whole, this is an outstanding review of the recent paleoclimate literature relevant to the internal dynamics and external forcing of climate change. My two suggestions regard 1) expanding the geographical coverage to include recent high-resolution paleoclimate research in Latin America (e.g., Bernal et al 2011; Metcalfe et al 2010; Seager et al 2009; Woodhouse et al 2010), and 2) diluting the tendency to refer to paleoclimatic research published by the contributing authors themselves or at the institutions to which the authors belong. I would also note that the 'dipole-like' gradient in climate variability from southwestern into northwestern North America appears to be part of a larger latitudinal pattern with three nodes extending from Trade Wind easterlies over Mesoamerica across the Southwest and into the Pacific Northwest/Northern Rockies (e.g., Stahle et al. 2011). Bernal, J.P., Lachniet, M.S., McCullogh, M., Mortimer, G., Morales, P., and Cienfuegos, E., 2011, A speleothem record of Holocene climate variability from southwestern Mexico. Quaternary Research 75:104–113. Metcalfe, S.E, Jones, M.D, Davies, S.J, Noren, A and Mackenzie, A., 2010. Climate variability over the last two millennia in the North American Monsoon region, recorded in laminated lake sediments from Laguna de Juanacatlán, Mexico. The Holocene 20, 1195-1206 Seager, R., M. Ting, M. Davis, M. Cane, N. Naik, J. Nakamura, C. Li, E. Cook, and D.W. Stahle, 2009. Mexican drought: An observational, modeling and tree ring study of variability and climate change. Atmosfera 22(1):1-31. Stahle, D. W., D.J. Burnette, J. Villanueva Diaz, R.R. Heim, Jr., F.K. Fye, J. Cerano Paredes, R. R. Acuna-Soto, M.K. Cleaveland, 2011. Atlantic and Pacific influences on Mesoamerican climate over the past millennium. Climate Dynamics DOI 10.1007/s00382-011-1205-z. Woodhouse, C.A., D.M. Meko, G.M. MacDonald, D.W. Stahle and E.R. Cook, 2010. A 1,200-year perspective of 21st century drought in southwestern North America. Proceedings of the National Academy of Science 107(50):21283-21288. [David Stahle, United States of America]	Noted.
5-330	5	7	1	7	1	Orbital' here should be 'astronomical' - see Lunt et al in press, CPD, on the LIG and definitions from Berger [Mark Siddall, United Kingdom]	Taken into account. Both terms are used interchangeably in the literature. The sentence

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							clarifying our terminology use has been moved to the beginning of this subsection.
5-331	5	7	2	7	4	Why not here referring more specifically to the FAQ 6.1 of Jansen et al., 2007 (Frequently Asked Question 6.1 What Caused the Ice Ages and Other Important Climate Changes Before the Industrial Era?) [Andreas Fischlin, Switzerland]	Noted. Reference to AR4, Box 6.1 most appropriate for this subsection.
5-332	5	7	5	7	39	The whole paragraph does not seems to belong to a paleoclimate chapter [Gunnar Myhre, Norway]	Noted. Provides some basic information to better understand the paleo part.
5-333	5	7	6	7	8	This sentence should come at the beginning of this section. [Andreas Fischlin, Switzerland]	Taken into account. See response to comment 5-330.
5-334	5	7	7	7	7	Historically, the expression "orbital parameters" included tilt/obliquity. Therefore, write "the parameters determining Earth's orbit" instead of "Earth orbital parameters". [Manfred Mudelsee, Germany]	Taken into account in the revised version.
5-335	5	7	10	7	13	Cite the paper by Tzedakis et al. 2012 (Nature Geoscience doi:10.1038/ngeo1358) [Yueh-Hsin Lo, Taiwan]	Accepted. Citation added.
5-336	5	7	10			“no consensus on exactly how it influences” This can be a bit misleading. Parts of the process are well understood, and could be outlined. Carbon cycle feedbacks may not be as well understood, but (for instance) high summertime insolation over an ice sheet leading to melting... well understood. [Government of United States of America]	Taken into account in the revised version
5-337	5	7	11	7	13	Servonnat et al (CP, 2011) could also cited here (the reference is actually already in the reference list) [Masa KAGEYAMA, France]	Taken into account. Orbital forcing section now references Section 5.5 where this aspect is addressed.
5-338	5	7	12	7	12	"millennual trends" in what? [Janice Lough, Australia]	Taken into account.
5-339	5	7	12	7	13	The paper of Kaufmann et al. (Science, 2009) also addresses this issue. [Government of France]	Taken into account. Orbital forcing section now references Section 5.5 where this aspect is addressed.
5-340	5	7	12			Variations in insolation in a single millennium cannot explain millennial trends. Do you mean multimillennial orbital variations, or centennial climate trends? [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Text revised to clarify multi-millennial trends.
5-341	5	7	13	7	13	(e.g. Wanner et al., 2008). While I agree that this paper has to be cited here, there are many more invoking similar ideas. [Andreas Fischlin, Switzerland]	Accepted. See responses to comments 5-337 and 5-339.
5-342	5	7	15	8	6	Section 5.2.1.2: While this section discusses in great detail TSI variations, it does not make clear whether recent warming could possibly have been caused by solar forcing nor does it refer a reader to another section where this is discussed. In particular LBB (Figure 5.1b, Lean et al., 1995b) could be cherry-picked to make this argument (sceptics). This section should either assess this explicitly and clearly or then refer to another place in AR5 where this is discussed. It could also be addressed easily in FAQ 5.3 as I have suggested to add. And BTW, the referring to section 5.3.5 on p. 8, line 3 is IMHO too broad and not good enough. [Andreas Fischlin, Switzerland]	Taken into account. A sentence refering to FAQ 5.1 is added to the introduction.
5-343	5	7	15	8	6	When report discussed the solar activity effect on climate, besides the effect of TSI, UV and galactic cosmic ray, the space weather (the solar proton, relativistic electron flux , HCS, Interplanet Magnetic Field (IMF)) also can impact on tropospheric circulation by changing the atmosperic cloud microphysics, which has been reviewed by Tinsley, Burns, Zhou (2007, Advances in Space Research) and Tinsley (2008, Reports on Progress in Physics). So in this part instead of involving the TSI UV effect, the whole space weather effect, not only the galactic cosmic ray effect, should be mentioned, especially the space weather effect on the tropospheric circulation. [Limin Zhou, China]	Noted. Since in chapter 5 the main emphasis is on paleodata this should be dealt with in chapter 8.
5-344	5	7	15	8	22	Good discussion of solar forcing. The phrase on line 29 of page 7 is very important. Similarly for lines 41-51. One could perhaps state even more strongly the limitations on what we know of solar forcing (TSI and UV SSI separately) for past centuries and millenia. [Robert Kandel, France]	Noted. Due to very limited space a more extend discussionis not possible.
5-345	5	7	15	8	22	Good discussion of solar forcing. The phrase on line 29 of page 7 is very important. Similarly for lines 41-51.	Noted. Some changes made.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						One could perhaps state even more strongly the limitations on what we know of solar forcing (TSI and UV SSI separately) for past centuries and millenia. [Robert Kandel, France]	
5-346	5	7	17	7	18	move citation (Kopp and Lean 2011) at the end of the sentence for easier readability [Masa KAGEYAMA, France]	Noted. The first two sentences have been removed.
5-347	5	7	17	7	20	1365-1360 = 5 W/m2 difference. This seems large in 1000y anomaly context. In line 20, is this what figure 5.1 is? Lower solar energy input for the global radiative energy budget? [Government of United States of America]	Noted. The first two sentences have been removed.
5-348	5	7	17	7	39	I feel that the first part of the Solar Forcing can be shortened significantly. For example, I suggest to remove the information regarding the observed reduction in Total Solar Irradiance during 2008. [Eugenia M. Gayo, Chile]	Accepted.
5-349	5	7	17	7	39	At end of opening sentence, consider citing: Milankovitch, M. 1920. Theorie Mathematique des Phenomenes Thermiques produits par la Radiation Solaire. Gauthier-Villars Paris. [Government of United States of America]	Rejected. This reference is in French and not easily accessible.
5-350	5	7	17	7	41	This is a very nice section, but it does not present information from Paleoarchives so it's unclear if it is best palced in this chapter. The authors could delete the entire paragraph and rewirte the opening sentence of the next paragraph. [Government of United States of America]	Noted. It does provide some background information which is required to understand the approaches applyed to derive paleoinformation from archives.
5-351	5	7	18	7	18	You've given a Ref for the new TSI value. Can you given a reference for the one of 1365.5? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	So far the absolute value of TSI was not well constrainedand is also constantly changing. See e.g. Frohlich, C., and J. Lean (2004), Solar radiative output and its variability: evidence and mechanisms, Astronomy and Astrophysics Review, 12(4), 273-320.
5-352	5	7	19	7	19	"8.3.1.1.1" should be corrected to "8.4.1.1.1". [Georg Feulner, Germany]	Accepted.
5-353	5	7	19			Sorry to be picky but Fig 5.1 does not give any value for TSI as it presents only anomalies [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Taken into account.
5-354	5	7	21	7	21	"the lower TSI absolute value" --> "this lower TSI absolute value" [Masa KAGEYAMA, France]	Taken into account. Sentence removed.
5-355	5	7	23	7	23	Change 'records' to 'measurements'. [European Union]	Accepted.
5-356	5	7	23	7	23	Change 'records' to 'measurements'. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-357	5	7	24	7	26	Add data about changes in air temperature resulting from changes in TSI (0.1%) in view of the direct impact of changes in TSI on the Earth's surface. [Lei Huang, China]	Taken into account. Reference to solar Box added.
5-358	5	7	25	7	25	SSI variations over the entire UV range vary widely in themselves, so the statement that the changes are "several percent for the ultra-violet (UV) part" is rather imprecise. [Georg Feulner, Germany]	Accepted. Statement rephrased.
5-359	5	7	25	7	25	Solar forcing is discussed in Section 8.4, not 8.3. [Georg Feulner, Germany]	Accepted.
5-360	5	7	26	7	26	"SSI" is defined earlier as the "spectral (wavelength dependent) solar irradiance" but in line 26 seems to be only referring to the UV part. This should be clarified. [Masa KAGEYAMA, France]	Taken into account. Sentence changed.
5-361	5	7	27			Gray et al (2010) is an excellent review paper but not a specific reference for SSI influences; I suggest replace by Haigh J D (1996) The impact of solar variability on climate. Science, 272, 981-984. Alternatively you could use Gray et al (2010) in place of nearly all of the references in this section! [Joanna Haigh, United Kingdom]	Accepted.
5-362	5	7	28	7	29	Make sure the terms "sunspots, faculae, magnetic network" are all in the glossary. [Andreas Fischlin, Switzerland]	Noted.
5-363	5	7	29	7	31	What is the possible cause for models not to successfully reproduce the measured TSI minimum in 2008? A brief explanation is needed considering that this paragraph was started with a new lower absolute value for	Noted but this is beyond the scope of Chapter 5.

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						TSI determined during the 2008 minimum. [Lei Huang, China]	
5-364	5	7	33	7	34	It is true, but may be misleading the readers. Increased sunspot numbers mean intensified solar activities, thus enhanced irradiance although sunspot itself reduces irradiance. This explanation needs to be modified. [Lei Huang, China]	Taken into account. Sentence rephrased.
5-365	5	7	35	7	35	solar cycle maximum --> solar cycle maxima [Masa KAGEYAMA, France]	Accepted.
5-366	5	7	39	7	39	Solar forcing is discussed in Section 8.4, not 8.3. [Georg Feulner, Germany]	Accepted.
5-367	5	7	42	7	42	"Multi-decadal trend" reads strange because this is just a 34-yr interval. Replace by "long-term trend". [Manfred Mudelsee, Germany]	Accepted.
5-368	5	7	43	7	43	It is unclear, what 'no longer available means'. How long was it availabe? [Government of Germany]	Taken into account. Sentence changed.
5-369	5	7	43	7	43	the sentence should read: "….various magnetic features of the Sun…." [Government of Poland]	Taken into account.
5-370	5	7	43			The authors should consider deleting: "is no longer available". Was there ever sufficiently detailed information on the various magnetic features in pre-satellite times as early as 400 years ago? [Government of United States of America]	Taken into account. Sentence modified.
5-371	5	7	43			Statement that detailed information about magnetic features "is no longer available" needs careful clarification [Government of United States of America]	Taken into account. Sentence modified.
5-372	5	7	43			"is no longer available" what does this mean? That it was available at some time? [Government of United States of America]	Taken into account. Sentence modified.
5-373	5	7	44	7	45	Please quantify 'past millennia' so we are clear of the period being referred to. [Peter Burt, United Kingdom]	Taken into account. Sentence modified.
5-374	5	7	48	7	48	"8.3.1" should read "8.4.1". [Georg Feulner, Germany]	Accepted.
5-375	5	7	49	7	49	Change "their respective geochemical cycles" to "their respective geochemical cycles and transport pathways" [Government of Australia]	Accepted.
5-376	5	7	49	7	49	Citation of Pedro et al., Clim Past, 7, 2011 is incorrect. Correct citation is Pedro et al., EPSL, 2012. Full reference to be added to the reference list as follows: [Tasman van Ommen, Australia]	Accepted.
5-377	5	7	49	7	49	J.B. Pedro, J.R. McConnell, T.D. van Ommen, D. Fink, M.A.J. Curran, A.M. Smith, K.J. Simon, A.D. Moy, S.B. Das, Solar and climate influences on ice core 10Be records from Antarctica and Greenland during the neutron monitor era, Earth and Planetary Science Letters, Volumes 355–356, 15 November 2012, Pages 174-186, ISSN 0012-821X, 10.1016/j.epsl.2012.08.038. [Tasman van Ommen, Australia]	Taken into account.
5-378	5	7	50	7	50	Write "Correcting for these *not exactly quantified* non-solar components ...". [Manfred Mudelsee, Germany]	Accepted.
5-379	5	7	50	7	51	"Correcting for these non-solar components increases the uncertainty of the reconstructions (grey band in Figure 5.1c)." This is not really correct (it could imply that the uncertainties are smaller without correction). The correction is necessary and does not increase the uncertainty. However, uncertainties in the correction contribute to the overall uncertainty. Suggestion: "Uncertainties in correcting for non-solar components adds to the overall errors of the reconstruction". [Raimund Muscheler, Sweden]	Accepted.
5-380	5	7	53	7	55	Make reference to figure 5.1d. [Rob Wilson, United Kingdom]	Accepted.
5-381	5	7	54	7	55	The reference in the text (Stuiver and Brazunias) is different from the reference in Fig 1 caption (Torrence and Compo for the same result). I guess one refers to the dataset and the other to the statistical method but anyway you need consistency. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Stuiver reference added to the figure caption
5-382	5	7	54	7	55	Surely better would be ""has claimed to reveal periodicities of…", as they seem far from convincing or persistent on Fig 5.1d, and surely cannot be considered robust until the same methods are used on the new data shown in Figure 5.1c. I think it is dangerous to propagate these periodicities, which are much misused, as certain. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted. Fig. 5.1d shows the results of the wavelet analysis of the TSI data presented in Fig. 5.1c. These periodicites are well-defined and certain, but highly variable in amplitude. Adjustment made in the figure

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							caption.
5-383	5	7	55	7	55	This list of periodicities is repeated in the caption of Fig 5.1 Surely it only needs to be there once. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-384	5	7	55	7	55	"but" (end of line): where is the contradiction? [Masa KAGEYAMA, France]	Noted. If one characterizes a time series one generally assumes that it is composed of sinusoidal waves with well-defined periods, amplitudes and phases. Here the amplitudes are highly variable.
5-385	5	7	55	7	56	....bit WITH varying amplitudes.... [Rob Wilson, United Kingdom]	Accepted.
5-386	5	7	56	7	56	Consider to use recent TSI reconstruction by Steinhilber et al. (2012): Steinhilber, F., J. Beer, and C. Frlich (2009), Total solar irradiance during the Holocene, Geophys. Res. Lett., 36(L19704). [Takuro Kobashi, Japan]	Noted. To be consistent with the input data used for the PMIP3-CMIP5 simulations we prefer to keep these data, but added a reference to the new data.
5-387	5	7	56	8	3	I think this statement needs revisions. There is the new Shapiro et al. (2011, J. Astronomy & Astrophysics), which is mentioned later in the chapter (see page 27, line 1-2). This forcing needs to be discussed also in this paragraph, e.g., "Most of the recent reconstructions show a ...(0.24%). Still, there is a new sprectrally-resolved reconstruction which shows a signifcnat invcrease in amplitude (0.44%, Shapiro et al. 2011). [Christoph Raible, Switzerland]	Taken into account. Text modified.
5-388	5	8	3	8	3	remove parentheses around "section 5.3.5" [Masa KAGEYAMA, France]	Accepted.
5-389	5	8	5	8	6	Although chapt.5 (p.8 L5) refers 8.3.1.5. for effects of GCR on clouds, no such subsection was found. It should be Section 7.4.5. [Government of Japan]	Accepted.
5-390	5	8	5	8	6	Chapter 7 discuss the cosmic ray influence on clouds most thoroughly, Chapter 8 has a summary of this in section 8.4.1.5 [Gunnar Myhre, Norway]	Accepted.
5-391	5	8	6	8	6	There is no Section 8.3.1.5. Galactic cosmic rays are discussed in Sections 7.4.5 and 8.4.1.5. [Eimear Dunne, Finland]	Accepted.
5-392	5	8	6	8	6	"8.3.1.5" should read "8.4.1.5". [Georg Feulner, Germany]	Accepted.
5-393	5	8	6	8	6	Section 8.4.1.5 and 7.4.5 [Gavin Schmidt, United States of America]	Accepted.
5-394	5	8	9	8	9	The greenhouse gas forcings over the past 1-2 millennia should be shown alongside the natural forcings of Fig 5.1. These are especially relevant to the millennial simulations of several figures later in this chapter. [Government of Australia]	Noted. Chapter 5 has provided updated information on volcanic and solar forcing. Greenhouse gas forcing during the last millennium is mentioned in chapters 6, 8 and 9.
5-395	5	8	9	8	22	Figure 5.1 needs reordering as 5.1b is the first component referred to in the main text; also I cannot see any brown squares for Antarctica. [Janice Lough, Australia]	Noted. Since three panels are dedicated to solar forcing we prefer to keep those together.
5-396	5	8	9			Figure 5.1 caption: there are no brown squares on panel a [Government of United States of America]	Taken into account. Figure corrected.
5-397	5	8	13			The caption refers to brown squares but none ar visible on the plot. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Figure corrected.
5-398	5	8	13			These are not TSI (parts b and c) but TSI anomalies. The y-axis should be re-labelled. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-399	5	8	14	8	16	"All records have been used for PMIP" - do you mean people have used each record or that they used the average? If the former then no change needed. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted. The former is right.
5-400	5	8	16			Figure 5.1 caption: please explain here the yellow band to right of panel c [Government of United States of America]	Taken into account. An explanation added.
5-401	5	8	17	8	17	Figure 5.1: Addition of 11-year cycle: no details on the amplitude (which is furthermore not time-constant) is	Taken into account. An explanation added.

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						given. [Manfred Mudelsee, Germany]	
5-402	5	8	17			Figure 5.1 caption: Consider adding "shown by dashed white lines" after "preferred periodicities" [Government of United States of America]	Accepted.
5-403	5	8	21	8	22	"Varying amplitude and persistence" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted. These periodicities can disappear completely for certain periods but they come back afterwards with the same periodicities in a similar way as the 11-y sunspot cycle disappears during the Maunder minimum. Insofar they are persistent.
5-404	5	8	22	8	22	Figure 5.1: No colour bar for wavelet amplitudes is given. [Manfred Mudelsee, Germany]	Accepted. Added.
5-405	5	8	22	8	22	Figure 5.1: No colour bar for wavelet amplitudes is given. [Manfred Mudelsee, Germany]	Duplicate of 5-404
5-406	5	8	26	8	27	Impacts of aerosols on climate. Should their impact on cloud formation be mentionned here? [Masa KAGEYAMA, France]	Noted. However the cloud - aerosol interactions are addressed specifically in chapter 7.
5-407	5	8	27	8	27	Chapter 7 has no discussion of volcanic eruptions. [Gunnar Myhre, Norway]	Taken into account.
5-408	5	8	28	8	28	Check usage of "last" versus "past" (also throughout whole text). [Manfred Mudelsee, Germany]	Taken into account
5-409	5	8	30			“chemical records” is too broad. Specifically, the reconstructions use sulfate deposition [Government of United States of America]	Taken into account
5-410	5	8	33	8	33	"reconstructions of spatial distribution" --> "reconstruction of the spatial distribution" [Masa KAGEYAMA, France]	Taken into account
5-411	5	8	35	8	38	The stament “The timing and relative magnitude of large stratospheric volcanic aerosol injections is generally consistent amongst these and earlier studies (Jansen et al., 2007) for the last 700 years, the 1783 CE Laki (Iceland) eruption being a notable exception.” - needs clarification. What is the evindence for a large stratospheric volcanic aerosol injection from Laki? [Government of Iceland]	Noted. The text has been reformulated to refer to the debate on the fraction of stratospheric injection by the Laki.
5-412	5	8	36			"amongst" should be "among" for consistency of style through the chapter. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account
5-413	5	8	37			Gao, Robock and Ammann have a 'version 2' of their data that moves a 1762 volcano to 1783. http://climate.envsci.rutgers.edu/IVI2/#Version2; Also, if the modern Ey... eruption is indicative, ash may have been a very important constituent of the Laki eruption. It isn't in the forcing data sets, and GCMs aren't modeling this yet either. [Government of United States of America]	Taken into account.
5-414	5	8	38	8	38	Some rewording may be required " since a new paper (Plummer et al., 2012) has refined the timing of volcanic events, including importantly the timing of the largest eruption of the last millennium in terms of volcanic forcing (1450s). This work dates this eruption commonly regarded as Kuwae at 1458. The paper notes evidence that suggests two eruptions in the 1450s. Preferable would be to say "...is generallly consistent amongst these and earlier sudies (Jansen et al., 2007) for the last 700 years with some notable exceptions. The 1783 CE Laki (Iceland) eruption and, recently a redating of the large Kuwae eruption from ~1453 to 1458CE (Plummer et al., 2012)." [Tasman van Ommen, Australia]	Taken into account
5-415	5	8	38	8	38	Note that paper is now accepted in Climate of the Past but the extant citation in CPD is: C. T. Plummer, M. A. J. Curran, T. D. van Ommen, S. O. Rasmussen,A. D. Moy, T. R. Vance, H. B. Clausen, B. M. Vinther, and P. A. Mayewski, An independently dated 2000-yr volcanic record from Law Dome, East Antarctica, including a new perspective on the dating of the c. 1450s eruption of Kuwae,Vanuatu, Clim. Past Discuss., 8, 1567–1590, 2012 [Tasman van Ommen, Australia]	Taken into account
5-416	5	8	42	8	42	Change "et al" to "et al." [Alan Robock, United States of America]	Taken into account
5-417	5	8	42	8	42	Hegerl et al (2006) point after al. is missing [Olga Solomina, Russia]	Taken into account

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5-418	5	8	43	8	43	It is a bit odd that the reference to Fig. 5.1a comes after the one to 5.1b [Hans Linderholm, Sweden]	Noted but the order of figure panels has not been modified.
5-419	5	8	45			There is uncertainty in optical depth. It seems like Reff/Veff uncertainty might deserve more mention here. We only have Pinatubo (and to a much lesser extent Chichon) to estimate the size distribution of particles. (Sato et al 1993 is a ref) Even if the mass of the injection is known, or at least constrained from sulfate deposition, the particle size will impact the AOD, length of greater AOD, as well as the radiative forcing. (Lacis et al 1992) [Government of United States of America]	Taken into account.
5-420	5	8	47	8	47	For consistency with previous comment, (e.g. Kuwae, 1458CE, or to reconcile whatever Timmreck uses, "(e.g. The 1450s Kuwae eruption)" [Tasman van Ommen, Australia]	Taken into account.
5-421	5	8	52			I thought you were using US spelling conventions (paleo rather than palaeo) but now I hit sulphur. This is a UK English spelling although IUPAC recommends sulfur and sulfate for all. If changed this needs a global edit. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	AR5 is expected to use English spelling (and therefore sulphur) with the exception of "paleoclimate"based on the scoping documents.
5-422	5	8	54	8	56	The Laki study is interesting, but is this worth adding here. It is still an issue under discussion. The effects of Laki have been well studied across Europe. Most of the effects are direct effects on crops from sulphur near the surface. [European Union]	Taken into account
5-423	5	8	54	8	56	The Laki study is interesting, but is this worth adding here. It is still an issue under discussion. The effects of Laki have been well studied across Europe. Most of the effects are direct effects on crops from sulphur near the surface. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account
5-424	5	8	55	8	56	The statement on the debate is obscure (no argument is provided). The text should be more self-contained. [Government of France]	Taken into account
5-425	5	8	56	8	56	Schmidt et al. (2012) is now in press. [Alan Robock, United States of America]	Noted.
5-426	5	8	56			This is one of a number of references listed as "in press". I presume all "in press" items that are included will be in press or published prior to finalizing this draft? [Michael Hren, United States of America]	Noted. Papers in press prior to March 15th, 2013 are allowed for citation in the final chapter text.
5-427	5	9	4	9	4	Insert 'the' before 'last' [Peter Burt, United Kingdom]	Taken into account.
5-428	5	9	5	9	5	insert 'as' before 'climate' [Peter Burt, United Kingdom]	Taken into account.
5-429	5	9	7			Section 5.2.2. This is an odd combo- GHGs and dust. Especially when you're really only talking about 3 GHGs. [Government of United States of America]	Noted.
5-430	5	9	9	9	57	This entire section needs to be sorted out by a native English speaker. It has pretty poor English, and this introduces scope for misinterpretation. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account.
5-431	5	9	11	9	11	Explain "low-pass filtered" [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account.
5-432	5	9	11	9	20	It should be added the recent paper by Ahn et al. GRL 2012. They show a high resolution CO2 record showing that CO2 concentrations increased abruptly during D-O warming events 8 and 9. Additionally, the sharp CO2 increase occurred synchronous or slightly after the Antarctic temperature rise. Ref.: Ahn et al., 2012, Abrupt change in atmospheric CO2 during the last ice age. GRL, L18711, doi:10.1029/2012GL053018. [Maria Fernanda Sanchez Goñi, France]	Taken into account.
5-433	5	9	11			Jargon alert: readers won't know what "low-pass filtered" means. Define/explain or omit. [Jay Curt Stager, United States of America]	Taken into account.
5-434	5	9	13	9	13	Please give reference to a figure or table where the new data for CH4 and N2O can be seen. [Government of Germany]	Rejected. The text describes precisely the range of CH4 and N2O variations.
5-435	5	9	15		16	This is a very important piece of information as it defines the uncertainty/variability in the natural ranges - which may be applicable today. It is part of the uncertainty in today's natural (when coupled with an understanding of how lifetimes may have changed recently). These numbers should be in the ES: "Centennial	Taken into account.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						variations of up to 10 ppm CO2, 40 ppb CH4 and 10 ppb N2O occur throughout the late Holocene. [Michael Prather, United States of America]	
5-436	5	9	18			"concentration" should be "concentrations". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account.
5-437	5	9	19	9	20	The last sentence is confusing - what is meant by long term trends? Orbital variations dominate the 800 kyr record with the mid-Bruhnes shift being clear and well-studied [Government of United States of America]	Taken into account. The ambiguous sentence was removed.
5-438	5	9	19	9	20	What kind of long-term trend does the CO2 record reveal in addition to glacial–interglacial cycles, increase or decrease? Better to make it clear. [Lei Huang, China]	Taken into account. The ambiguous sentence was removed.
5-439	5	9	19	9	20	It is in my view not clear whether that long-term CO2 trend is real; longer series than EDC would be required to prove this. Word down and write "CO2 record may reveal subtle long-term ...". [Manfred Mudelsee, Germany]	Taken into account. The ambiguous sentence was removed.
5-440	5	9	19			"to a range geological records" -> "to a range of geological records" [Government of France]	Taken into account.
5-441	5	9	19			Consider rephrasing this sentence: during interglacials prior to 420 ka back to ~ 800 ka, why not use MIS terminology for these interglacials? [Government of United States of America]	Taken into account.
5-442	5	9	22	5	22	stay witihin natural limits' this is true in the Quaternary but very confusing for the novice reader when earlier periods are also mentioned in the chapter. It is a contradictory statement without reference to 'the last 800 000 years' [Mark Siddall, United Kingdom]	Taken into account.
5-443	5	9	22	9	22	Comment Text: Add after "The GHG concentrations.." "in ice cores from pre-industrial times.." so as to exclude post 1940 GHG concentrations, which of course exceed 300 ppm of CO2. [Peter Barrett, New Zealand]	Taken into account.
5-444	5	9	22			"The GHG concentrations stay….." Need to specify in this sentence during what time they stayed within the limits, because it could be used as an assessment that the current concentrations do so (which would be wrong I know, but it's open to misinterpretation because it's not sufficiently specific). [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account.
5-445	5	9	23			The authors should consider presenting the CH4 and N2O data if they're going to be discussed. [Government of United States of America]	Rejected. The text describes precisely the range of CH4 and N2O variations.
5-446	5	9	24	9	24	Comment text: The sentence beginning "The occurrence.. Is awkwardly phrased. Pehaps words like "No peak approaching the size and duration could be found in records from the last 22,000 yr.. [Peter Barrett, New Zealand]	Taken into account.
5-447	5	9	24	9	24	delete comma before first 'and' [Peter Burt, United Kingdom]	Taken into account.
5-448	5	9	24	9	27	Why is 22,000 yr? Probably better to be 21,000 or 20,000 yr from the point of paleoclimatological view. [Lei Huang, China]	Taken into account.
5-449	5	9	24		25	The occurrence of a peak similar …to the anthropogenic rise can be excluded to have occurred during …" Delete : "to have occurred". [Government of France]	Taken into account.
5-450	5	9	25	9	25	Add the values of the anthropogenic rise for CO2 and CH4 in brackets, if possible. [Lei Huang, China]	Taken into account.
5-451	5	9	25			"…can be excluded to have occurred ….". This is pretty sloppy English, and it does not help to make a point. Please rephrase/simplify to be clearer. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account.
5-452	5	9	26			Jargon alert: readers won't know what "low-pass filtered" means. Define/explain or omit. [Jay Curt Stager, United States of America]	Taken into account.
5-453	5	9	28	9	28	Why with "medium confidence"?. It is hard to imagine natural processes which would produce such rates of change simultaneously for CO2, CH4 and N2O, as encountered in the 20th century. [Government of Poland]	Taken into account.
5-454	5	9	28	9	28	Is there high confidence on the first part of the sentence? [Masa KAGEYAMA, France]	Taken into account.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-455	5	9	28	9	29	I think you need to explain why you only have mediunm confidence in the last 800 kyr. Aadd a sentence: "The lower resolution of data for earlier time periods makes it impossible to state that there could not be higher values between data points but it is unlikely (medium confidence). [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Taken into account.
5-456	5	9	28			Consider inserting "anthropogenic" after the word "observed" and before "rates were not encountered". [Government of United States of America]	Taken into account.
5-457	5	9	28			"Observed…" When? Do you mesan "Currently observed…"? Or if not, then please specify as needed. I think this is too vague as it is now. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account.
5-458	5	9	28			"…also not during the last 800 kyr". Do you mean "and neither….."? [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account.
5-459	5	9	29	9	29	Write out 800kyr in full [Government of Australia]	Taken into account.
5-460	5	9	31	9	36	I feel that this paragraph could be strengthened with a figure. Otherwise it should be rewritten so that a figure is not expected (feels like it is taken directly from a review paper) [Hans Linderholm, Sweden]	Taken into account.
5-461	5	9	33	9	33	"are linked to" suggests causality, which might not be what the authors actually meant. I would suggest the more neutral "occur at the same time as". A link would call for a mechanism, which is not given here [Masa KAGEYAMA, France]	Taken into account.
5-462	5	9	34	9	34	same as comment 32 for "are connected to" [Masa KAGEYAMA, France]	Taken into account.
5-463	5	9	38	9	45	Perhaps not a necessary paragraph : this paragraph/sentence justs reports a speculation, and no assessment of reliability. [Government of France]	Taken into account.
5-464	5	9	38	9	53	These two paragraphs try to explain mechanisms of CO2 and CH4 during the LGM and the Holocene which are detaily discussed in the chapter 6, sections 6.2.2. and 6.2.3. What is an added value of repeating this discussion in the chapter 5? It makes more sense to add on the periods and processes not covered in the chapter 6. [Victor Brovkin, Germany]	Taken into account. The whole paragraph was removed as this is described in chapter 6
5-465	5	9	40	5	40	"d13CO2" is a non-standard expression. [Manfred Mudelsee, Germany]	Taken into account. The whole paragraph was removed as this is described in chapter 6
5-466	5	9	40			Insert "glacial" before "terminations"? [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account. The whole paragraph was removed as this is described in chapter 6
5-467	5	9	44	9	45	Consider revising the sentence to read: "Relative importance of ocean processes…. As drivers of Holocene CO2 is also discussed in Chapter 6" [Government of United States of America]	Taken into account. The whole paragraph was removed as this is described in chapter 6
5-468	5	9	44			insert "and" before "peat". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account. The whole paragraph was removed as this is described in chapter 6
5-469	5	9	44			"land use changes and peat uptake" (add "and") [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Taken into account. The whole paragraph was removed as this is described in chapter 6
5-470	5	9	47			section on CH4. Include recent paper by Sapart, Nature, 2012 [European Union]	Taken into account. The whole paragraph was removed as this is described in chapter 6
5-471	5	9	50	9	50	The (referenced) Petrenko study utilised 14C for the first time to establish that marine clathrate destabilisation was not responsible for the YD termination CH4 increase. Thus use of the word "stable" is wrong: the Petrenko study utilised radiocarbon measurements. [Government of Australia]	Taken into account. The whole paragraph was removed as this is described in chapter 6
5-472	5	9	50		51	The CH4 clathrate statement is important here and for projections (that do not use it) - please put in ES. [Michael Prather, United States of America]	Rejected. The whole paragraph was removed and the causes of GHG variations are to be covered in chapter 6.
5-473	5	9	52			CH4 emissions are only half of the story. The oxidizing capacity of the atmosphere is VERY important.	Taken into account. The whole paragraph was

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Singarayer, Valdes, et al 2011 Nature is one ref. VOC emissions. There is also interesting ice core work to constrain this half of the methane concentration equation (h2o2, folmaldehyde, etc.) [Government of United States of America]	removed as this is described in chapter 6
5-474	5	9	55	9	57	Suggest this sentence (modified to be in plain English) be included in the SPM as it is an important and useful statement for policy makers [Government of Australia]	Taken into account. The whole paragraph was removed as this is described in chapter 6
5-475	5	9	55	9	57	Needs some explanation of how the isotope data demonstrates the anthropogenic origin, and would benefit from an explanation of exactly which changes are being referred to. Seems to be a key point, but very unclear exactly what has been shown. [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account. The whole paragraph was removed as this is described in chapter 6
5-476	5	9	56	9	56	"origin of ongoing changes since two centuries" --> "origin of the changes which have occurred during the last two centuries" [Masa KAGEYAMA, France]	Taken into account. The whole paragraph was removed as this is described in chapter 6
5-477	5	9	56			"since two centuries" is not English (probably a translation from German). "ongoing chnages in the last two centuries" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Taken into account. The whole paragraph was removed as this is described in chapter 6
5-478	5	9	57	9	57	The isotope studies cited, plus papers by Trudinger et al. (for CO2) and Howling et al. (for CH4), also present strong evidence for the causes of pre-industrial Holocene variations in CO2 and CH4 (climate and biomass burning variations). Measurements of CO concentrations and isotopes in ice (Wang et al.) and of terrestrial charcoal deposits confirm the biomass burning changes. [Government of Australia]	Taken into account. The whole paragraph was removed as this is described in chapter 6
5-479	5	10	1	11	3	I strongly suggest that this section is passed by an outside expert, such as Gavin Foster, to ensure that the argument is up to date. Recent discussion with Gavin of this topic for two papers (PALAEOSENS and another paper in PNAS) suggests to me that it may need updating. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Noted, section has been reviewed by Paul Pearson (CA) and papers which reviewer and Foster have led will be assessed. Foster is now contributing author.
5-480	5	10	4	10	19	Comment Text: Despite reducing CO2 estimates by 50% Breecker et al.'s reassessment of the paleosol method still leaves open significant uncertainties, as indicated by their Table 2, showing trapped CO2 levels ranging from 1000 to 6000 ppm in a range of individual Holocene samples. This suggest factors other than CO2 levels are not being taken fully into account. The stomata method also has limitations with an inherent loss in sensitivity above about 500 ppm. In contrast, the larger body of data from alkenones, along with the closer clustering, and its consistency with the recent boron-based estimates, justify greater confidence in these results (acknowledged in lines 35 to 38). [Peter Barrett, New Zealand]	Noted, at present we do not feel the body of literature on individual proxies allows us to place more confidence on marine vs terrestrial proxies notwthstanding the large number of marine proxy data. Moreover there are issues with some of the early (published) Paleogene marine proxy data (B isotpes), and these will be removed form Fig 5.2.
5-481	5	10	4	10	19	The different CO2 estimates derived from proxies before the ice core records between 800 kyr BP and 20 Myr BP where compared and evaluated with respect to changes in global mean temperature by the paper "van de Wal, R. S. W.; de Boer, B.; Lourens, L.; Köhler, P. & Bintanja, R. Reconstruction of a continuous high-resolution CO2 record over the past 20 million years Climate of the Past, 2011, 7, 1459-1469". I believe this paper and its content should be mentioned here and at several other places, e.g. caption of Fig 5.2, because van de Wal et al (2011) suggest considerable higher CO2 values in the Mid Miocene Climate Optimum around 15 Myr BP when compared with most of the data points of Fig 5.2b. This disagreement is based on the choice made in van de Wal, that only those proxies for CO2 are considered reliable which vary as function of global mean surface temperature similar as the CO2 data from the ice cores. With all above given as my reasoning I suggest in detail to extend this paragraph by the following sentence: "However, there exists a comparison study for the last 20 Myr in which the different CO2 proxies are evaluated based on the ratio of temperature change over CO2 in comparision to the CO2 data dervied from ice cores (van de Wal et al., 2011). This study reveals that CO2 based on the alkenone method reveals a CO2/temperature change ratio that is significantly different from the other methods or the ice core records." [Peter Köhler, Germany]	Noted but we are focussing this discussion on proxy data reconstructions, thus we have assessed Van de Wal but it is a modelled estimate of CO2 and not approproiate for this subsection. WRT MMCO CO2 estimates these are being updated to reflect latest proxy estimates and assessments which produce great convergence than SOD.
5-482	5	10	4			replace "concentration for" by "concentrations during". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-483	5	10	5	10	5	Insert 'those' after 'than' [Peter Burt, United Kingdom]	Accepted
5-484	5	10	5	10	5	briefly name the methods of the four primary proxy methods here, even if there are fully explained in the appendix [Masa KAGEYAMA, France]	Noted and taken into account

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5-485	5	10	6	10	7	This sentence needs a timescale. "A reassessment of biological respiration and carbonate formation has reduced CO2 estimates from fossil soils by ~50% (Breecker et al., 2010)." -- The way it is written makes it unclear if "the reduced CO2 estimates from fossil soils by ~50%" refers to the past or present CO2 estimates. [Government of United States of America]	Taken into account and revised
5-486	5	10	7			Has the reassessment reduced CO2 estimates from fossil soils, or reduced the uncertainty / errors on the estimates? [Government of United States of America]	Taken into account, both estimates and uncertianties
5-487	5	10	7			SA new study by Cotton et al. 2012 looks at impact of precipitation on Sz used for pCO2 reconstruction from paleosol carbonate. This resolves some discrepancies between stomatal density pCO2 estimates and soil carbonate estimates. [Michael Hren, United States of America]	Noted and taken into account
5-488	5	10	9	10	9	"from the two terrestrial" --> "from these two terrestrial" [Masa KAGEYAMA, France]	Accepted
5-489	5	10	9			please insert: higher CO2 estimates "than previously assessed" [Bärbel Hönisch, United States of America]	Accepted
5-490	5	10	10	5	10	I suggest your use 'use' rather than utlise 'utilise'. 'Use' is less verbose [Mark Siddall, United Kingdom]	Noted and taken into account
5-491	5	10	10	10	12	The aqueous fractionation factor determined by Klochko is undoubtedly correct for seawater but the latest MC-ICP-MS studies have now also confirmed that boron isotopes in planktic foraminifers follow a less sensitive d11B vs. pH relationship than expected from aqueous fractionation (Krief et al. 2010, Foster et a. 2012, Henehan et al., in revision). I would therefore recommend to delete this first part of the sentence. The second part of the sentence could be rephrased to say: "Recent CO2 reconstructions using the boron isotope proxy utilise an improved understanding of foraminifer species effects and evolution of seawater alkalinity (Hönisch & Hemming 2005) and seawater boron isotopic composition (Foster et al., 2012)." Foster 2008 followed the earlier recommendations of Hönisch and Hemming (2005). [Bärbel Hönisch, United States of America]	Accepted
5-492	5	10	14	10	15	"a range geological records"…should be "a range of geological records" [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Noted and taken into account
5-493	5	10	14			insert range "of" gelogical records [Bärbel Hönisch, United States of America]	Noted and taken into account
5-494	5	10	14			"range of geological" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted and taken into account
5-495	5	10	17			All four techniques have been included. It is better to iclude all four techiques within bracket, "All four techniques (name here) have been included" [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Noted and taken into account
5-496	5	10	21	10	21	on the time interval between 65 and 54 Ma: why focus on this period? Name the period and explain whay it is possible to study it (possibly referring to other sections in this chapter) [Masa KAGEYAMA, France]	Noted and taken into account
5-497	5	10	21	10	22	This sentence needs citations. [Robert Kopp, United States]	Noted and taken into account
5-498	5	10	21	10	24	In these lines, Million years are abbreviated as both 45 Ma (in line 21) and 50-Myr (in line 24). It is better to maintain uniformity in age abbreviation. [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Noted and taken into account
5-499	5	10	21	10	30	Cite the paper by Tzedakis et al. 2012 (Nature Geoscience doi:10.1038/ngeo1358) [Yueh-Hsin Lo, Taiwan]	Noted and taken into account
5-500	5	10	23	10	30	Comment text: The extensive body of nahcolite in the Green River Formation USA provides powerful physical evidence of atmospheric CO2 levels in the range >1125 ppm ~50 million years ago, as the text notes, citing Lowenstein and Demicco (2006). However these authors give this value as a lower bound only, and indicate the possibility of values up to 3000 ppm in their figure, as the lowest crystallisation temperature increases with temperature. Given the high level of uncertainty associated with the other CO2 proxies for this distant time period, the Green River Nahcolite provides the best evidence. The present text should at least be amended to say paleo-CO2 concentrations "likely lay above 1125 ppm during this period." [Peter Barrett, New Zealand]	Noted and modified to reflect lower limit of pCO2 during ECCO with upper limit undefined
5-501	5	10	25			There is also evidence for elevated pCO2 (>2700 ppm) from the carbonate component of oxisol goethite from the Sierra Nevada, USA Yapp, C.J. Fe(CO3)OH in goethite from a mid-latitude North American Oxisol: estimate of atmospheric CO2 concentration in the Early Eocene “climatic optimum”. Geochim. Cosmochim. Acta 68, 935-947 (2004) [Michael Hren, United States of America]	Noted and taken into account

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5-502	5	10	26	10	26	"CO2 levels > 1125 ppm" --- this requires a citation. [Manfred Mudelsee, Germany]	Noted and taken into account
5-503	5	10	27	10	28	Better to describe separately: a general decrease between ~50 and 20 Ma and a general increase over the last ~15 Ma (Figure 5.2). [Lei Huang, China]	Noted and taken into account
5-504	5	10	28	10	28	"such trend" --> "such a trend" [Masa KAGEYAMA, France]	Noted and taken into account
5-505	5	10	28	10	30	Please add shading or a box to delineate the EECO on figure 5.2 [Government of United States of America]	Noted and taken into account
5-506	5	10	28			"such a trend" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted and taken into account
5-507	5	10	32	10	32	same as comment 37: explain why this period should be studied here [Masa KAGEYAMA, France]	Noted and taken into account
5-508	5	10	32	10	35	It is confusing that this section is not reporting the CO2 concentrations for the MPWP, but a more extended period. It is unclear why: in the remainder of the chapter where the Pliocene is discussed the focus is on the MPWP (3.0-3.3 Ma). It is important to clarify this in the light of emerging evidence that the peak warmth of the Pliocene was not between 3.0 and 3.3 Ma but around 4.0-4.5 Ma, as is visible in Figure 5.2. [Sandra Passchier, United States of America]	Noted. We decided to focus on the MPWP which has the most robust information on temperature, global mean sea level and has been a target for climate simulations.
5-509	5	10	33			"in the range 330 ppm to 420 ppm..this should be "in the range from 300 ppm to 420 ppm" or "in the range of 330-420 ppm" [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Noted and taken into account.
5-510	5	10	35	10	38	"Additional confidence" -> does this mean that you have 'very high' confidence in the boron- and alkenone-based reconstructions, given you had 'high confidence' in the previous sentence. If not, consider alternative wording to 'additional confidence'. [Thomas Stocker/ WGI TSU, Switzerland]	Noted and taken into account.
5-511	5	10	35	10	38	You are emphasising the positive in a slightly unbalanced way here. The B agrees with the ice cores, but the alkenone points at 800-500 ka do NOT agree with the ice cores or with the boron. I think it would be better to temper the statements: "It is difficult to know how much confidence to give to the marine reconstructions for the Pliocene. Additional....two proxies. A boron-derived dataset agrees within error (+/-25 ppm) with the ice core record, but alkenone data for the ice core period are outside the error limits." [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted and taken into account.
5-512	5	10	41	10	41	Figure 5.2: The upper panel's time axis starts at 3.5 Ma and not at 3.6 Ma. [Manfred Mudelsee, Germany]	Noted and accepted.
5-513	5	10	41			Caption for Figure 5.2. Since radiative forcings are not shown, consider rewriting the first line as follows: "Orbital-scale Earth system responses to radiative forcings and perturbations 3.6 Ma to present." [Government of United States of America]	Noted and taken into account.
5-514	5	10	43			The sea level curve is clearly blue, not purple (on screen and on paper) [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted and taken into account.
5-515	5	10	47			You use the term "interglaciations" here and later. I know this has been the suibject of internal debate among the authors. Personally I find it archaic: the community has generally agreed that interglacials is now a noun, and to me "glaciation" and by extension "interglaciation" imply the act of glaciating (ie the onset period) not the actual state. However, you can dedide but need to be consistent: there are many uses of integlacial in the text so which is it to be? Global edit needed. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-516	5	10	50			replace "blue" by "green" [Bärbel Hönisch, United States of America]	Accepted.
5-517	5	10	50			The ice core line is green, not blue [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-518	5	10	55	10	56	As mentioned before and in Beerling and Royer (2011), the data of Pearson and Palmer (2000) were very exciting 12 years ago but have now been found to be associated with uncertainties of foraminifer species differences and seawater alkalinity. Given that this chapter focuses on the improvements of these proxy estimates over estimates presented in previous reports, I would therefore recommend to remove these data here. [Bärbel Hönisch, United States of America]	Accepted. Data has now been removed.
5-519	5	10	59	10	59	Delete 'a' [Peter Burt, United Kingdom]	Accepted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-520	5	10	59	11	1	Figure 5.2: Write: "The blue shading is a 1-standard deviation uncertainty band constructed using block bootstrap resampling (Mudelsee et al., 2012)." The reference is: Mudelsee M, Fohlmeister J, Scholz D (2012) Effects of dating errors on nonparametric trend analyses of speleothem time series. Climate of the Past 8:1637–1648. [Manfred Mudelsee, Germany]	Accepted.
5-521	5	10	59			delete: The blue shading is "a" 1 standard deviation. [Bärbel Hönisch, United States of America]	Accepted.
5-522	5	10	59			"a bandwidth of 8 Myr prior to 30 Ma, and 1 Myr to present", here million years are in different styles. [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Noted and taken into account.
5-523	5	10				Figure 5.2. In the caption for top panel (a), the blue line is described for atmospheric CO2 from EPICA dome C ice core, but the line is actually green. In bottom panel, consider adding a box for the time span of the EECO like that for the MPWP in the top panel. The grey line described in the top panel for sea level is barely visible. Consider adding a line to show Holocene dust mass accumulation rates tothe top panel (see comment p. 11 line 12). Finally, it is suggested that Fig 5.2 could be simplified a bit by adding labels to the two dashed lines for 'present" and "pre-industrial" on both top and bottom panels. [Government of United States of America]	All comments noted as above and accepted, ECCO suggestion accepted.
5-524	5	10				The vertical grey bar incorporates the peak warming of the Pliocene (visible in the lower panel), but that is not the MPWP (red box). In the lower panel the grey bar should be replaced with a narrower vertical bar only bracketing the MPWP as that is the focus of the chapter (see page 13). It is then more clearly visible that what is discussed is not the peak warmth of the Pliocene. [Sandra Passchier, United States of America]	Noted and taken into account.
5-525	5	11	1	11	1	Extend figure 5.2b on the continous high-resolution CO2 record of van de Wal et al 2011 (full citation in row 1 above), data for download in supplement of paper at http://www.phys.uu.nl/~boer0160/Model_output/VandeWal_etal_1Dmodel_CO2.txt or at http://www.clim-past.net/7/1459/2011/cp-7-1459-2011.html. Then, extend the caption of figure 5.2.b (after (Mudelsee et al., submitted).): "The XXXcoloured line is a continous high-resolution CO2 record based on the CO2 proxy records, which is calculated based on a constant relationship between global mean temperature and CO2 as found in the ice cores for the last 800 kyr (van de Wal et al., 2011)". [Peter Köhler, Germany]	Noted and rejected as only proxy data CO2 reconstructions are plotted in this fig, not modelled CO2.
5-526	5	11	3			insert a period at the end of the sentence. [Bärbel Hönisch, United States of America]	Accepted
5-527	5	11	3			Mid Pliocene Warm Period from 3.3-3 Ma)..this should be Mid Pliocene Warm Period from 3.3-3 Ma. [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Accepted
5-528	5	11	5	11	38	This sub-chapter on dust feels a bit short, although I have to admit most of the work since 2007 has focused on technical aspects that cannot be listed here. Maybe one idea worth mentioning is the “Gustiness” hypothesis of McGee et al., 2010 in QSR (doi:10.1016/j.quascirev.2010.06.009). [Fabrice Lambert, Republic of Korea]	Noted. This is not a new hypothesis (well established in dust literature), but a new application to paleoclimate, and is added.
5-529	5	11	7	11	7	And what did AR4 conclude in their "assessment of the importance of the MDA"? [Janice Lough, Australia]	Taken into acccount.
5-530	5	11	8			Here you use ka for kiloyears. I agree this is the preferred usage, recommended for both age and interval now (QSR editorial (Rose, J., 2007. The use of time units in Quaternary Science Reviews. Quat. Sci. Rev. 26, 1193.) refers. However you also ferequently use kyr in text and on figures. Consistency? [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Taken into acccount.
5-531	5	11	11	11	11	A MDA-flux reconstruction from the Southern Ocean implies… [Matthew Konfirst, United States of America]	Taken into acccount.
5-532	5	11	11	11	11	implies' - plural [Mark Siddall, United Kingdom]	Taken into acccount.
5-533	5	11	11			replace "imply" by "implies" [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into acccount.
5-534	5	11	12			This would be much more meaningful if there were a line showing Holocene dust MAR on figure 5.2 [Government of United States of America]	Noted.
5-535	5	11	16	11	17	Is this statement true for the last 800000 years of the Antarctic record? [Masa KAGEYAMA, France]	Taken into acccount.
5-536	5	11	16	11	25	A final synthesis of the impact of the "lifetime" factor could be added, since it seems to be important in	Taken into acccount.

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						producing the ~30 factor of MDA fluxes during glacial periods. [Masa KAGEYAMA, France]	
5-537	5	11	16			Factor 30 is too high if you say "systematic". Thatt is a reasonable ratio for peak glacial versus interglacial. Either "are higher by up to a factor 30", or "are systematically higher by a factor of order 20". [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Taken into acccount.
5-538	5	11	17			This is now in ice cores? Clarify that this is no longer about the Pliocene. [Sandra Passchier, United States of America]	Taken into acccount.
5-539	5	11	18	11	19	Add reference Gaiero (2007. GRL, doi:10.1029/2007GL030520) together with “De Decker et al., 2010; Gabrielli et al., 2010; Wegner et al., 2012”. The data and rationale presented in Gaiero (2007) was of instrumental importance in assigning the South American sources for mineral dust aerosols deposited in Antarctica. Moreover, Gaiero (2007) was published after the AR4. [Government of Brazil]	Taken into acccount.
5-540	5	11	18			These papers show that there are potential source areas in Australia that are consistent with the geochemistry of Antarctic dust, but the glacial (possibly not interglacial) dust can be explained by South American alone. I suggest "in southern South America and perhaps Australia". [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Taken into acccount.
5-541	5	11	19			"and to atmospheric MDA lifetime change". This suggestion has indeed been made but it is countered by the argument that GCMs do not show a major change in dust recahing Antarctica due to lifetime chnages. This should therefore be presented as an unresolved issue. You can cite Fischer et al 2007 (already listed) and Wolff et al (Wolff, E.W., et al., 2010. Changes in environment over the last 800,000 years from chemical analysis of the EPICA Dome C ice core. Quat. Sci. Rev. 29, 285-295.) for the counterview. I would propose putting a ";" after Australia, and then "A possible role for chnages in atmospheric MDA lifetime has been proposed but its importance is uncertain (refs). [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Taken into acccount.
5-542	5	11	25			I think the presentation of the dust variability is from the outset a bit simple and coloured by views from Antarctic records (the low-res. Winckler paper only confirms that glacials are more dusty than interglacials). Other records of dust show a great spatial variability, and a complexity of records that is dominated by millennial variability (see for example Roberts, A.P., Rohling, E.J., Grant, K.M., Larrasoaña, J.C., Liu, Q., Atmospheric dust variability from Arabia and China over the last 500,000 years. Quaternary Science Reviews, 30, 3537-3541, 2011). I therefore suggest to add a sentence at the end of line 25, saying something like "High-resolution dust records reveal a complex pattern of regional and short-term temporal variability (Roberts et al. (2011)". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into acccount.
5-543	5	11	27	11	28	The sentence about Greenland dust concentrations needs a reference (e.g. Ruth et al., 2007 GRL, doi:10.1029/2006GL027876). To make it consistent with the rest of the sub-chapter it would be good to convert the concentration numbers to fluxes. Estimated accumulation rates for Summit, Greenland can be found on page 8 of Ruth et al., 2003, JGR (doi:10.1029/2002JD002376) [Fabrice Lambert, Republic of Korea]	Taken into acccount.
5-544	5	11	27	11	31	If possible, it would be of value to the reader for this paragraph to briefly summarize what controls (or is thought to control) variations in MDA source, atmospheric lifetime, and transport (even if only to say that the controlling factor is broad changes in atmospheric circulation pattern, although more detail than that would be welcome). It's an important section and this information would help the reader understand the main points in the section. [Government of United States of America]	Taken into acccount.
5-545	5	11	33	11	34	Making the link to MDA concentrations explicit on the figure in Box 5.1 would help the visualization of this statement. Currently the dust part of the cycle is shown as a minor local input from the land to the ice sheet. [Government of United States of America]	Accepted, new figure includes dust and soot symbols
5-546	5	11	33		38	Discussion of dust radiative forcing should include an estimate from: P. Chylek and U. Lohmann, Aerosol radiative forcing and climate sensitivity deduced from the Last Glacial Maximum to Holocene transition, GEOPHYSICAL RESEARCH LETTERS, VOL. 35, L04804, doi:10.1029/2007GL032759, 2008 [Petr Chylek, United States of America]	Rejected because of focus on key papers assessing the state of the science and because of several published criticisms of this reference.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-547	5	11	34	11	34	Write "Global *palaeo* data synthesis". [Manfred Mudelsee, Germany]	Rejected. The text is explicit about paleo data.
5-548	5	11	35	11	38	Please add if this is TOA or surface radiative forcing. I really don't like the use of global radiative forcing when talking about dust, as this has absolutely no informative value due to the large differences in regional atmospheric dust concentrations (whereas greenhouse gases are reasonably well mixed). How about giving a range? For example : minimum of x W/m^2 over oceans, maximum of y W/m^2 over African and Asian deserts. [Fabrice Lambert, Republic of Korea]	Global TOA is the only important forcing from a global climate perspective. Of course this is the sum of regional forcings.
5-549	5	11	40	13	18	Box 5.1: Only here can the reader finally start to comprehend what exactly might be meant with the title of the section 5.2!! See my other comments on the too general wording of this title. BTW, the title for the box is fine, since it is explained right away. [Andreas Fischlin, Switzerland]	Noted. Box is revised: title changed, earth system sensitivity discussion removed, main focus on ice-sheet climate interactions.
5-550	5	11	42	12	18	I find the contents of this box a rather rambling, loose collection of remarks. It is unclear to me what point this box is trying to make. Boxes are highlights and should be very clear. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Noted. Box is revised: title changed, earth system sensitivity discussion removed, main focus on ice-sheet climate interactions.
5-551	5	11	42	13	11	Some of this gets repeated in Ch. 10. I think it is better here and Ch. 10 seems far too long. [European Union]	Revised box does not deal with earth system sensitivity anymore. Overlap with Chapter 10 avoided.
5-552	5	11	42	13	11	Box5.1: The effect of deforestation on earth system feedback should be added. There are several papers about this topic appearing on top journals during the last three years. Dallmeyer and Classen (2010) have also discussed the climate cooling effect of deforestation in China. [Lei Huang, China]	The revised boxl focusses now on ice-sheet climate interactions. De-forestation will hence not be mentioned.
5-553	5	11	42	13	11	Some of this gets repeated in Ch 10. I think it is better here and Ch 10 seems far too long. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Revised box does not deal with earth system sensitivity anymore. Overlap with Chapter 10 avoided.
5-554	5	11	42	14	34	I have no sympathy for the tendency of some of my erstwhile astrophysicist colleagues to attribute all earth and solar system changes to solar variations. One must resist the temptation to use correlations of some climate and solar activity proxies and the very-short-term observed TSI history to construct a long-term TSI history and then (circular reasoning!) to use this history with other climate proxies to argue for solar control. (cf. also Sections 10.7.2.2 and 3). However, specialists in paleoclimatology should recognize that while the models of solar structure and evolution are robust for the long time scale (say 0.4 - 4 Ga), they do not exclude fluctuations, say of order 0.5%, small for the Sun but large for the Earth, on a shorter (say 0.4 - 4 Ma) time scale. [Robert Kandel, France]	Noted.
5-555	5	11	42	14	34	I have no sympathy for the tendency of some of my erstwhile astrophysicist colleagues to attribute all earth and solar system changes to solar variations. One must resist the temptation to use correlations of some climate and solar activity proxies and the very-short-term observed TSI history to construct a long-term TSI history and then (circular reasoning!) to use this history with other climate proxies to argue for solar control. (cf. also Sections 10.7.2.2 and 3). However, specialists in paleoclimatology should recognize that while the models of solar structure and evolution are robust for the long time scale (say 0.4 - 4 Ga), they do not exclude fluctuations, say of order 0.5%, small for the Sun but large for the Earth, on a shorter (say 0.4 - 4 Ma) time scale. [Robert Kandel, France]	Noted.
5-556	5	11	42			An reference to the new Rohling 2012 Nature Perspective early in this discussion might help. This discusses many of the points made here. The reference does appear later but in a different context. Many of those points are also discussed in the two reviews by Edwards 2007 DOI: 10.1177/0309133307083295 and Knutti and Hegerl 2008 Nature Geoscience. [Reto Knutti, Switzerland]	Noted. Revised box does not deal with earth system sensitivity anymore and focuses only on climate ice-sheet interactions.
5-557	5	11	42			Box 5.1: a masterpiece! [Manfred Mudelsee, Germany]	Thank you.
5-558	5	11	42			Box 5.1: We would encourage to be more specific on time-scales associated with the earth system feedbacks and their role in the climate system (could also include time scale information in the figure). [Thomas Stocker/ WGI TSU, Switzerland]	Noted. Box is revised: title changed, earth system sensitivity discussion removed, main focus on ice-sheet climate interactions, timescales of processes now explicitly indicated in figure.
5-559	5	11	44	11	44	please consider replacing inertia here, or give specifiers/examples such as thermal inertia [Mark Siddall, United Kingdom]	Noted. Box is revised: title changed, earth system sensitivity discussion removed, main focus on ice-

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							sheet climate interactions.
5-560	5	11	44	11	55	While I very much are in favor of making these explanations here, I think this text could be ordered a bit more and be written with a bit more care. E.g. in this sequence: There are positive and negative feedbacks. They may result in inertia and tipping points (albeit I don't like fashions, but this term is used often in the context of CC and may well need to be introduced here), multiple steady states, bifurcation, hysteresis, or even irreversibility (not the same as hysteresis).Non-linear feedback does not make much sense to be mentioned at almost the end and moreover, it is per se a questionable term (perhaps best to drop it). [Andreas Fischlin, Switzerland]	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.
5-561	5	11	53	11	53	The statement about multiple equilibria is rather obscure and does not relate to the theory of dynamical systems, although it borrows its terminology. Perhaps not a necessary sentence. [Government of France]	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.
5-562	5	11	53	11	53	"observe hysteresis behaviour" --> "observe a hysteresis behaviour" [Masa KAGEYAMA, France]	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.
5-563	5	11	54	11	54	Make sure terms such as "hysteresis" and "bifurcation" etc. are all in the glossary or you risk that many readers who are often less familiar with "system theory/behavior of dynamic systems" are lost entirely at the very begin of this box. [Andreas Fischlin, Switzerland]	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.
5-564	5	11	54	11	55	What are the first and second types of response? this should be made more clear in the former sentences. What is described in this third case, this sentence, is a Hopf bifurcation. The text seems to implicitly redefine what a bifurcation is, without refering to the mathematical theory that created the term. [Government of France]	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.
5-565	5	12	1	5	12	Please coordinate these definitions with chapter 9! [Gavin Schmidt, United States of America]	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.
5-566	5	12	2	12	6	Even many palaeoclimate experts will have little familiaruty with CCS, so it needs to be explained in a little more detail, e.g. what does a CCS of 1.4°C actually mean? [Robert Larter, United Kingdom]	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.
5-567	5	12	2	12	7	The authors should be consistent in their use of "climate sensitivity" terms, if - indeed - they are referring to the same idea. In this section, a seemingly new name (Charney climate sensitivity) is introduced for existing concepts (climate sensitivity). Hansen et al., (1985 and 2008) spent a lot of time discussing the Charney report concept of climate sensitivity and equilibrium but these papers did not use the term "Charney Climate Sensitivity" but rather the widespread and accepted term "climate sensitivity". Furthermore, AR5, Chapters 9 and 12, discuss 'climate sensitivity' and do not mention 'Charney climate sensitivity'. If the IPCC AR5 Chapter 5 authors really want to emphasize the Charney report, why not reference it rather than the 2006 Schneider von Deimling et al paper.Cross-chapter coordination on this issue is recommended. [Government of United States of America]	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.
5-568	5	12	2	12	12	No explanation given for why only the CCS was used in AR4, or indication of whether and/or why ESCS is now also being considered. [Government of United Kingdom of Great Britain & Northern Ireland]	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.
5-569	5	12	2	13	12	Very good, but that these sensitivities consider processes that operate on quite distinct time scales should be made clearer. While time scales are explicitly mentioned at the end of the 2nd bullet, none are stated for the Charney climate sensitivity, since fast is a relative term only. [Andreas Fischlin, Switzerland]	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.
5-570	5	12	2			The decision was that climate sensitivity should remain as defined in AR4, which is essentially the Charney sensitivity, but the latter expression should not be used. So "Charney" should be removed. Sensitivity estimates that include slow feedbacks are proposed to be labeled Earth System sensitivity (see section 12.5.3, Box. 12.2). Consistency with the glossary in terms of terminology needs to be ensured across all chapters. Also, please refer to specific sections in other chapters. The sensitivity estimates from CMIP5 and the	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.

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						perturbed physics ensembles are now in the model evaluation chapter, estimates from the observed warming are in the attribution chapter, and the synthesis is in Box 12.2 [Reto Knutti, Switzerland]	
5-571	5	12	5	12	5	"Stationarity" may not be the adequate term. Are the components "stationary", or simply "constant" ? [Government of France]	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.
5-572	5	12	5	12	6	A number of articles show that substantial vegetation changes can occurr within ~100 years , indicating a dynamic equilibrium between vegetation and climate for short periods of forcing. At centennial-scale vegetation cannot be considered as a stationary Earth system component anymore. Ref.: (Allen et al., 1999, Rapid environmental changes in southern Europe during the last glacial period. Nature 400: 740-743; Sanchez Goñi et al., 2002, Synchroneity between marine and terrestrial responses to millennial escale climatic variability during the last glacial period in the Mediterranean region Climate Dynamics 19: 95-105) [Maria Fernanda Sanchez Goñi, France]	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.
5-573	5	12	6	12	6	"Most recent AR5 updates" --> "The most recent..." [Masa KAGEYAMA, France]	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.
5-574	5	12	11			At least in Lunt, the CO2 concentration was prescribed, so the ESCS he calculated did not include carbon cycle feedbacks. This should be corrected. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.
5-575	5	12	16	12	16	"following the ESCS concept" is unclear here. [Daniel Lunt, United Kingdom]	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.
5-576	5	12	22	12	22	Based on my admittedly dated knowledge of the physics of stellar/solar structure, I would be tempted to add that on time scales greater than 10**4 years, the possible amplitude of intrinsic (TSI) solar forcing depends on variations below the photosphere, in the solar convective zone, still poorly modeled and hence uncertain. The data (Fig. 5.3), revealing consistency between the orbital/rotational insolation drivers and climate change, suggest that TSI has not changed significantly over 500 ka. However, especially when going back 3.6 Ma not to mention 60 Ma (Fig. 5.2), I would argue that we have very little basis for estimating TSI variation except in the sense that observations of G-type main sequence stars may give us a guide to the possible order of magnitude of solar luminosity fluctuations. [Robert Kandel, France]	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.
5-577	5	12	22	12	22	Based on my admittedly dated knowledge of the physics of stellar/solar structure, I would be tempted to add that on time scales greater than 10**4 years, the possible amplitude of intrinsic (TSI) solar forcing depends on variations below the photosphere, in the solar convective zone, still poorly modeled and hence uncertain. The data (Fig. 5.3), revealing consistency between the orbital/rotational insolation drivers and climate change, suggest that TSI has not changed significantly over 500 ka. However, especially when going back 3.6 Ma not to mention 60 Ma (Fig. 5.2), I would argue that we have very little basis for estimating TSI variation except in the sense that observations of G-type main sequence stars may give us a guide to the possible order of magnitude of solar luminosity fluctuations. [Robert Kandel, France]	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.
5-578	5	12	26			This sentence is confusing - it is not obvious which time period received more insolation. [Richard Telford, Norway]	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.
5-579	5	12	27	12	29	Maybe add "annual mean" so that it becomes "assuming that the LGM was in equilibrium with the orbital forcing". And the issue here is not only that the system is not in equilibrium, but also that the forcing has very strong seasonal and latitudinal component, which means that the system responds strongly even though the total forcing is zero. [Daniel Lunt, United Kingdom]	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.
5-580	5	12	29			By "irreversible", does it really mean "irreversible on human time scales"? [Government of United States of America]	Noted. Box is revised: title changed, earth system sensitivity discussion and initial paragraphs removed, main focus on ice-sheet climate interactions.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-581	5	12	34	12	56	"climate-ice-sheet feedbacks (in line 3), ice-sheets (in line 44), and ice sheets, all these places the term "ice sheet" should either with "-" or without "-". [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Accepted. We removed all hyphens for ice sheet.
5-582	5	12	34			section on complexity. Include the fact that the ice sheets have long reaction times to past climate changes. Temperature changes take several 10.000 years to reach the basal parts of the ice sheets and this influences the deformation rates ant thus the shape of the ice sheets. [European Union]	Noted. Long timescales mentioned now in revised Box
5-583	5	12	36	12	36	I would erase "summer" here, since melting could also occur in late spring/early autumn for very warm climates... [Masa KAGEYAMA, France]	Accepted. "Summer" removed.
5-584	5	12	36	12	36	"Expansion of an ice sheet replaces" --> "An ice sheet expands on" ? [Masa KAGEYAMA, France]	Accepted.
5-585	5	12	36	12	55	It might be useful to have nore discussion here or elsewhere of the possible feedback of increased snow accumulation due to warmer temperatures, sometimes referred to as the "snowgun" effect. There is some suggestion from models for this effect but unclear if it is observable on historical time scales (e.g. papers by Monaghan et al.) The role of possible future snow accumulation increases (at least in the near-term) in ice sheet mass budgets is an issue that may be addressed to some extent from the ice core record. Monaghan, A. J., D. H. Bromwich, and D. P. Schneider (2008), Twentieth century Antarctic air temperature and snowfall simulations by IPCC climate models, Geophys. Res. Lett., 35, L07502, doi:10.1029/2007GL032630. Monaghan, A. J., D. H. Bromwich, R. L. Fogt, S.-H. Wang, P. A. Mayewski, D. A. Dixon, A. Ekaykin, M. Frezzotti, I. Goodwin, E. Isaksson, S. D. Kaspari, V. I. Morgan, H. Oerter, T. D. Van Ommen, C. J. Van der Veen, and J. Wen (2006), Insignificant Change in Antarctic Snowfall Since the International Geophysical Year, Science, 313(5788), 827-831, doi:10.1126/science.1128243. [William Howard, Australia]	Noted. The revised box does not allow for an in-depth discussion of snow accumulation effects on ice-sheets
5-586	5	12	45			Are you missing a reference before Roe & Lindzen, 2001. It could just be a typo with the commas though. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted. It was a typographic error. Text changed accordingly.
5-587	5	12	45			Typographic error - should be "… snowfall (e.g. Roe and Lindzen, 2001)." [Iain Robertson, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text changed accordingly.
5-588	5	12	45			Remove on of the double commas before "Roe". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text changed accordingly.
5-589	5	12	45			Two commas in a row. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text changed accordingly.
5-590	5	12	53	12	56	see also: Joughin, I., R. B. Alley, and D. M. Holland (2012), Ice-Sheet Response to Oceanic Forcing, Science, 338(6111), 1172-1176, doi:10.1126/science.1226481. [William Howard, Australia]	Accepted. Reference included.
5-591	5	12	54	12	55	It would be appropriate to cite Schoof (2007) as applied in Pollard and DeConto (2009) [Sandra Passchier, United States of America]	Accepted. Reference included.
5-592	5	12	54	13	10	"ice-sheet" in three places. I have never seen this hyphenated before and you are not consistent in doing it elsewhere in the text [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted. Hyphens removed.
5-593	5	12	55			Typographic error - should be "(Alvarez-Solas et al., 2010)." [Iain Robertson, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-594	5	13	0			Section 5.3. I think for this section it would be useful to have a diagram or table illustrating the timings of the various geological epochs that are discussed e.g. Cenozoic, Pliocene etc. [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted. Table 5.1 has been added.
5-595	5	13	1	13	2	The qualitative description of the mechanisms is still speculative (from physical and statistical standpoints). How and why would those modelling efforts bring any quantitative information ; and whithin which delay ? Perhaps what is meant is that this kind of knowledge seems out of reach at present and for long : it can be	Noted. A general statement on the use of paleoclimate models has been added to chapter 1.

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						said that we do not know when it might be available. [Government of France]	
5-596	5	13	2	13	3	Do we know CO2 concentrations between the oldest ice core values 800 ka and the late Pliocene estimates? Is this sentence valid with little or no early Pleistocene CO2 reconstructions? [Government of United States of America]	This comment must be misplaced.
5-597	5	13	4	13	4	The caption talks about parts A, B and C but there are no labels like that on the figure itself [Peter Clift, United States of America]	This comment must be misplaced.
5-598	5	13	5			Section 12.5.2 discusses various forms of commitment, section 12.5.3.1. discusses timescales. Cross referencing those (and checking consistency) would be useful. [Reto Knutti, Switzerland]	Noted. Revised section cross-references 12.5.5.3
5-599	5	13	6	13	6	Here it is stated that the response of ice sheets "can be quite fast (order 10-100 years)" yet on page 12 line 23 ice sheets are given as examples of "slow components" with response timescales of 10E3 - 10E5 years. This apparent contradiction of two orders of magnitude must be explained. [Government of Australia]	Noted. Text clarified.
5-600	5	13	6			I found this sentence confusing - is it possible to give an example of the initial response? [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text changed to "Whereas the initial response to anthropogenic greenhouse warming of some Earth system components such as ice sheets can be quite fast involving for instance ice-shelf processes (order 10–100 years), their long-term adjustment to the ongoing warming will exceed human lifetimes"
5-601	5	13	7	13	11	this is a projection again - please consider the policy of the paleo chapter in this regard [Mark Siddall, United Kingdom]	Noted.This "projection" is part of a Box, which is supposed to have cross-chapter relevance.
5-602	5	13	9	13	10	What is adressed in this sentence is implicit (like the threshold behavior) and should me made clear. Could the progress between AR5 and AR4 be stated? One should explain why a threshold behaviour is important, or what this implies in terms of climate change. [Government of France]	Noted. Revised section does not discuss threshold behaviour anymore.
5-603	5	13	10	13	10	Charbit et al (GRL, 2008: Amount of CO emissions irreversibly leading to the total melting of Greenland, Geophys. Res. Lett., 35, L12503, doi:10.1029/2008GL033472) also supports this statement [Masa KAGEYAMA, France]	Accepted. Reference included
5-604	5	13	10			West-Antarctic: is not hyphenated elsewhere, seems incorrect to me. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-605	5	13	11	13	11	Kopp et al is not an appropriate reference. I suggest papers by Pollard and Deconto [Mark Siddall, United Kingdom]	Kopp deleted and other references included
5-606	5	13	14	13	16	Box 5.1 Figure 1: Description of inset figure needs to be included in caption. [Janice Lough, Australia]	Accepted.
5-607	5	13	20	28	7	Section 5.3: actually, this section is quite a catalogue and some explanation about its organisation and the topics it contains would be helpful. These could be given as a short introduction. Changing the title a little to reflect the section is not only about global and hemispheric scale changes could be less misleading. [Masa KAGEYAMA, France]	The short introduction is already present. Terms "global" "and "hemispheric" are used to distinguish from "regional".
5-608	5	13	20			Section 5.3: There is nothing about ocean circulation or its role in G-IG dynamics within this section and yet we believe that ocean circulation is probably involved in G-IG CO2 variations, and quite possibly in heat transfer variability on G-IG timescales. Given that there is significant knowledge concerning G-IG changes in circulation (especially in the Atlantic Ocean) it would seem appropriate to include at least a mention of this (for example, the 2007 summary by Lynch-Steiglitz et al.) and perhaps some mention of the possible role that ocean circulation might play in G-IG CO2 dynamics (e.g. the recent review by Sigman et al.). J. Lynch-Stieglitz et al., Atlantic meridional overturning circulation during the Last Glacial Maximum. Science 316, 66 (2007). D. M. Sigman, et al., The polar ocean and glacial cycles in atmospheric CO2 concentration. Nature 466, 47 (2010). [Stephen Barker, United Kingdom of Great Britain & Northern Ireland]	Accepted. The ocean circulation during the LGM is mentioned in the revised text in 5.3.3.

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5-609	5	13	20			Section 5.3: Appreciating that the timescales are pre-defined in this section, and do not include so-called millennial-scale variability, I think it might still be worth noting that millennial-scale variability (as discussed in section 5.7) displays responses on a global scale (albeit with inter-hemispheric asymmetry). [Stephen Barker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Millennial-scale variability is mentioned in 5.3 and linked to 5.7
5-610	5	13	26	13	26	Although the Pliocene is discussed later, paleoclimate readers will expect to see it mentioned sooner than section 5.3.1. [Government of Australia]	Accepted. Table 5.1 introduces the past periods discussed in Chapter 5.
5-611	5	13	26	14	34	Section 5.3.1: This section does not adequately discuss the fact that the differing configuration of the climate system during the early Cenozoic severely limits the utility of this period as a proxy for future climate states. For example, there was no Antarctic ice sheet during the EECO, and so the strength of feedbacks within the climate system is likely to have been very different to today. This means that estimates of the climate sensitivity derived from this period may not tell us anything useful about the climate of the 21st century or beyond. The authors should discuss this and cite relevant studies e.g. Sijp and England (2004, J. Phys. Ocean., 34, 1254-1266) or Sijp and England (2005, J. Climate, 18, 1957-1966) [Government of Australia]	Noted. These aspects are mentionned in the revised 5.3 section.
5-612	5	13	31	13	31	Change "such" to "such as" [Victor Brovkin, Germany]	Accepted.
5-613	5	13	31	13	31	"such tectonics" --> "such as tectonics" [Masa KAGEYAMA, France]	Accepted.
5-614	5	13	31	13	31	such such as [Seong-Joong Kim, Republic of Korea]	Accepted.
5-615	5	13	31	13	31	...such AS tectonics..... [Rob Wilson, United Kingdom]	Accepted.
5-616	5	13	31			Insert "as" after "such" [Sandra Passchier, United States of America]	Accepted.
5-617	5	13	32	10	35	Based on va de Wal et al. (2011, full reference in row 1) I suggest to revise the sentence "Although new reconstructions of deep ocean temperatures from benthic δ18O records have been compiled since AR4 (e.g.,Cramer et al., 2011), low confidence remains in the precise relationship between CO2 and deep-ocean temperature (e.g. Beerling and Royer, 2011)". To "Although new reconstructions of deep ocean temperatures from benthic δ18O records have been compiled since AR4 (e.g.,Cramer et al., 2011), low confidence remains in the precise relationship between CO2 and deep-ocean temperature (e.g. Beerling and Royer, 2011). In detail, different proxy approaches lead to different relationships between CO2 and global temperature change over the last 20 Myr (van de Wal et al., 2011)." [Peter Köhler, Germany]	Noted and taken into consideration.
5-618	5	13	32	13	35	Comment Text: Suggest replacing these lines with words like the following: "Although a new analysis of long term deep-sea temperature trends over the last 100 million years (Cramer et al., 2011) provides high confidence for Cenozoic deep-sea temperature peaking 50 million years ago at 14+2°C above present day values, low confidence remains in the precise relationship between CO2 and deep-ocean temperature (eg Beerling and Royer, 2011)." Based on Cramer et al 2009, Fig 9 and Table S4. [Peter Barrett, New Zealand]	Noted and taken into account. We have reassessed the confidence estimate for EECO as medium confidnce for the larger temperature range of +8-14 degrees C. We do not consider high confidence can be justified for 14 +/- 2 degress C based on our assessemnt of the cited paper (Cramer et al.).
5-619	5	13	33	13	35	Beerling and Royer 2011 figure 1 displays multi-proxy CO2-temperature trends consistent with the main features of earlier proxy-based CO2-tempeature relations and carbon model trends by Berner and other. [Andrew Glikson, Australia]	Noted and taken into account.
5-620	5	13	33	13	35	Beerling and Royer 2011 - Figure 1 displays multi-proxy CO2-temperature trends consistent with the main features of earlier proxy-based CO2-tempeature relations and carbon model trends by Berner and other. [Government of Australia]	Noted and taken into account.
5-621	5	13	39	13	39	The EECO needs defining here. It is defined earlier as 54-48M years ago. It would be useful to ensure that these terms are all in the glossary. How does the EECO compare with the PETM? The emphasis on possible past warm periods is good. Are there any other ones that people have speculated about. [European Union]	Noted EECO in new table in introductory section 5.1
5-622	5	13	39	13	39	The EECO needs defining here. It is defined earlier as 54-48M years ago. It would be useful to ensure that these terms are all in the glossary. How does the EECO compare with the PETM? The emphasis on possible past warm periods is good. Are there any other ones that people have speculated about. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted EECO in new table in introductory section 5.1

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5-623	5	13	39	13	40	Give abbreviation and timeframe for the EECO as well as the MPWP e.g. “(EECO; ~54-48 Ma)” at the end of this sentence. [Government of Australia]	Noted EECO and MPWP in new table in introductory section 5.1
5-624	5	13	39			mid-Pliocene warm period should be Mid Pliocene Warm Period; Early Eocene climatic optimum (EECO) [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Noted and now treated consistently through chapter and defined in new table.
5-625	5	13	39			Should you put (EECO) in brackets after the full version to be consistent with MPWP. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted and now treated consistently through chapter and defined in new table.
5-626	5	13	40	13	40	Acronim for Early Eocene Climatic Optimum (EECO) is needed here, plus age range. [Government of Poland]	Noted and now treated consistently through chapter and defined in new table.
5-627	5	13	40	13	40	after 'optimum' need to add (EECO) [Government of United Kingdom of Great Britain & Northern Ireland]	Noted and now treated consistently through chapter and defined in new table.
5-628	5	13	40	13	47	Why are Eocene and Pliocene proxy reconstructions "challenged"? This is an example of the general tenor of the chapter which appears to unfairly criticize the excellent paleo-data for these and other past periods of warmth. Are model simulations ever "challenged"? Of course, they are challenged to simulate climate situations not represented in the modern system; no analog climates, thresholds, non-linear changes, slow feedbacks etc. [Government of United States of America]	Noted, and have provided more balance between confidence and uncertainty in model vs proxy data, as well as brief comment on this in introduction to chapter.
5-629	5	13	40	13	47	These general statements regarding the challenges of developing and applying proxy climate/environmental information are really important and not just applicable to this section. I strongly suggest that a paragraph or box be devoted earlier in the chapter to highlighting the imperfections of proxy records - none are perfect and they only provide us with (with varying degree of certainity) "glimpses" of the past; it is only through robustly comparing independent lines of proxy evidence that we gain greater certainity of the timing and magnitude of past climate/environmental variations and changes for different regions of the globe. This should also stress the two-way relationship between proxy climate/environmental information and modelling outcomes. [Janice Lough, Australia]	Noted and taken into account, though lack of space restricts full treatment. Note we have two tables in Appendixes that deal with T and CO2 proxy assumptions and uncertainties and brief generic summary of mdeal and proxy data assumptions in Introduction.
5-630	5	13	40			add '(EOCC 54-48 Ma)' after early Eocene climatic optimum [Elie Verleyen, Belgium]	Noted and now treated consistently through chapter and defined in new table.
5-631	5	13	41			What is meant by latitudinal biases, doesn't entry (i) cover spatial deficiencies? It is unjustified to make these generalizations about spatial coverage on the basis of Dowsett 2012 and MARGO, any more than it would be to make similar statements about instrumental records (ie tide gauge spatial coverage [see figure on Jevrejeva 1700-2000 SL curve) or model limitations (everything from grid scale limits, sub-grid scale processes, limits on simulating poorly understood dynamics [ice dynammics in SL], parameterizations) [Government of United States of America]	Noted and taken into account, though lack of space restricts full treatment. Note we have two tables in Appendixes that deal with T and CO2 proxy assumptions and uncertainties and brief generic summary of mdeal and proxy data assumptions in Introduction.
5-632	5	13	42	13	44	Both of these sentences make significant claims that should be supported by references. [Government of Australia]	Noted.
5-633	5	13	44			Uncertainties, at least in reconstructions of SST with transfer functions, are often underestimated. [Richard Telford, Norway]	Noted and taken into account.
5-634	5	13	49	13	50	Note that a long-term d18O increase set in around 3.6 Ma and ended at around the conventionally known "intensification of NH". My suggestion would be to include at the end of line 50, before "This orbital ..." the following sentence: "The cycles are superimposed on a long-term d18O increase from 3.6 to 2.4 Ma (Mudelsee and Raymo, 2005), which reflects the glaciation of the NH." The reference is: Mudelsee M, Raymo ME (2005) Slow dynamics of the Northern Hemisphere Glaciation. Paleoceanography 20:PA4022 [doi: 10.1029/2005PA001153]. [Manfred Mudelsee, Germany]	Noted and taken into account.
5-635	5	13	50			First, the d18O record is not an ice volume record. Second, this first sentence now is about the complete Pliocene record and not the MPWP. The MPWP is an anomaly in the Pliocene d18O record and it does not record a strong 41kyr signal. Third, the reference to Naish et al. (2009a) is not a d18O record and could be removed. This section should be clarified. [Sandra Passchier, United States of America]	Noted and taken into account.
5-636	5	13	53	13	53	Spell out MPWP [Government of Australia]	Noted EECO and MPWP in new table in introductory

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							section 5.1
5-637	5	13	53			First, here I would remove "ice volume". Second, the Martinez-Garcia et al. (2011) record goes beyond the MPWP as defined here. So is only the climate anomaly at 3.3 to 3.0 Ma in that record considered here or the entire record back to 4.0 Ma? Please clarify. [Sandra Passchier, United States of America]	Noted. Comments by this reviewer about use of the MPWP (3.3-3Ma) vs Earth system responses during the earlier warm Pliocene have been addressed elsewhere when raised in other sections including 5.6.1 and 5.2.2.2. We are now focusiing our assessment on the MPWP unless specifically stated elsewhere and have adjusted text accordingly. The reason for using the MPWP is that it ahs teh most comprehensive set of model and proxy-base dreconstructions.
5-638	5	13	53			"during the MPWP" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted and taken into account.
5-639	5	13	56	13	57	The phrase "without given uncertainty" reads awkwardly. Either "given without uncertainty" or "without uncertainty given" [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted and taken into account.
5-640	5	13	56	14	3	This sentence gives the impression the 1.7 deg warmer Plio temperature comes from proxy data. Does it? Or is it based on climate model simulations? [Government of United States of America]	Noted and taken into account.
5-641	5	13	57	13	57	what does 'CE' mean? [Government of United Kingdom of Great Britain & Northern Ireland]	Noted and taken into account.
5-642	5	13				Box 5.1, Figure 1: The authors should consider adding a dust-atmosphere component to this schematic illustration. Also, in the temp-humidity plot, conside adding a label to the X axis (T, H, arrow to the right) similar to that for the Y axis. [Government of United States of America]	Accepted.
5-643	5	14	2	14	2	Haywood to "Haywood et al." [Seong-Joong Kim, Republic of Korea]	Accepted.
5-644	5	14	2	14	3	This statement about the MPWP and CO2 concentrations is not true according to figure 5.2, where the alkenone data stay above 400 ppm to about 2.7 Ma [Government of United States of America]	Noted and corrected.
5-645	5	14	2			The range of SAT anomalies in the Haywood et al. (submitted) paper is actually 1.9-3.6oC (the COSMOS model has a higher response) [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted and taken into account.
5-646	5	14	2			(Haywood submitted) should be changed as (Haywood, submitted). [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Accepted.
5-647	5	14	2			Haywood et al [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-648	5	14	3			Much of the weakened meridional temp gradient is related to the removal of the ice sheet. (lower elevation and lower albedo) This is mentioned in the later section, but should be brought up here. [Government of United States of America]	Noted and taken into account.
5-649	5	14	3			Section 5.2.2.2. discusses CO2 between 5.3-2.7 Ma and not during the MPWP. This should be made consistent. [Sandra Passchier, United States of America]	Noted and taken into account (see comment 637).
5-650	5	14	3			Based on Fig 5.2 this statement is incorrect. The last time CO2 was above 330 ppm appears to be later than the MPWP (~2.8 Ma and maybe at 1.5 Ma). [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted and corrected.
5-651	5	14	5	14	8	This sentence is much too long; break it up e.g. ... in the Northern Hemisphere. GCMs predict an ... [Government of Australia]	Noted and taken into account.
5-652	5	14	5			The term "threshold behaviour" should be defined [Government of United States of America]	Noted and taken into account.
5-653	5	14	9		10	How could there be high confidence from models projections of increased precipitation in the tropics while similar models produce grossly inaccurate precipitation estimates in today's climate. Of course, "high confidence" is to be taken in this case in the context of relatively low expectations. Nonetheless, this expression does not apply the ground rules layed out in Chapter 1, section 1.1.4 and Table 1.1. Suggest using a more modest formulation such as: ..."while model reconstructions indicate increased tropical	Noted changed to medium confidence.

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						precipitation during this time"... [Government of France]	
5-654	5	14	10	14	10	update Haywood et al., submitted [European Union]	Accepted.
5-655	5	14	12	14	12	Comment text: Replace "may have reached a level of ~1000 ppm.." with "may have exceeded ~1000 ppm, perhaps by a significant margin.." [Peter Barrett, New Zealand]	Noted and taken into account.
5-656	5	14	12			Again, based on Fig 5.2, 1000 ppm was probably reached at 33 Ma, much later than the EECO. This statement needs to be cast more carefully. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted but influenced by Nacholite proxy here, text has been clarified.
5-657	5	14	14			The boundary conditions themselves impact the climate substantially (Roberts, LeGrande, Tripati, 2009, 2011) [Government of United States of America]	Noted.
5-658	5	14	16	14	19	Revise sentence to read: Whilst for the EECO there is reasonably close agreement between proxy and model surface air temperature (SAT) reconstructions (Huber and Caballero 2011; Lunt et al. 2012), there are still significant discrepancies between proxy and model sea surface temperature reconstructions (Hollis et al., 2012; Lunt et al., 2012). Medium confidence is placed on the global mean surface temperature anomaly estimate of +8°C to +14°C (Box 5.2, Figure 1). [Christopher Hollis, New Zealand]	Noted and accepted.
5-659	5	14	17	14	17	update Hollis et al., submitted [European Union]	Noted and taken into account.
5-660	5	14	17			2 SDs of what (the model estimates?) [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted and taken into account.
5-661	5	14	19	14	19	[Peter Barrett, New Zealand]	no comment
5-662	5	14	21	14	27	This paragraph is awkwardly written and apparently contradictory. Short-hand "paleo-data" should be avoided. Be more specific. Similarly, the "paleo" prefix in front of sensitivity is redundant. The "paleo" context is clear from the rest of the paragraph. [Christopher Hollis, New Zealand]	Noted and taken into account.
5-663	5	14	22	14	24	"The limited number of paleo-data and models for MPWP, which take into account slow feedbacks such as ice sheets and the carbon cycle, imply with medium confidence ESCS may be up to 2 times the Charney Climate sensitivity (CCS)". The question being whether current climate change is controlled by fast feedback" CS or "slow feedback" CS? The latter would be consistent with melting of the large ice sheets, CO2 build-up andvegetational changes taking place. [Andrew Glikson, Australia]	Noted and this statement has been reassessed also in the context of Box 5.1
5-664	5	14	22	14	24	"The limited number of paleo-data and models for MPWP, which take into account slow feedbacks such as ice sheets and the carbon cycle, imply with medium confidence ESCS may be up to 2 times the Charney Climate sensitivity (CCS)". The question being whether current climate change is controlled by "fast feedback" CS or "slow feedback" CS? The latter would be consistent with melting of the large ice sheets, CO2 build-up and vegetational changes taking place. [Government of Australia]	Noted and this statement has been reassessed also in the context of Box 5.1
5-665	5	14	23	14	23	during the MPWP' should be used rather than 'for MPWP' [Government of Australia]	Noted and taken into account.
5-666	5	14	23			The authors might want to clarify that “slow feedbacks” of ice sheets and carbon cycle are not necessarily slow…Previdi paper submitted to xxxx; Bhatt et al., 2010, etc. [Government of United States of America]	Noted and this statement has been reassessed also in the context of Box 5.1
5-667	5	14	23			"for the MPWP" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted and taken into account.
5-668	5	14	24	14	24	"2 times" --> "twice"? [Masa KAGEYAMA, France]	Noted and taken into account.
5-669	5	14	25	14	27	"However, if the slow amplifying feedbacks associated with ice sheets and CO2 are included in the forcing term then estimates of “paleo sensitivity” approximate the CCS from models". This is a little opaque. I know what you mean by "included in teh forcing term" but it is not explained well. Maybe replace "included in the forcing term" with "considered as forcings rather than feedbacks in the calculation of sensitivity" [Daniel Lunt, United Kingdom]	Accepted.
5-670	5	14	27	14	27	PALAEOSENS Project Members, 2012. Making sense of palaeoclimate sensitivity. Nature, 491(7426): 683-691. http://dx.doi.org/10.1038/nature11574 Pa193 [Andreas Fischlin, Switzerland]	Noted and cited.
5-671	5	14	29	14	34	This appears to be a far too negative view of the well-studied Eocene and PETM paleoclimate. A question for	Noted. This section has been revised together with

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						the authors to consider: Can models simulate Eocene or PETM climate, thermal gradients etc? This section seems to have it backwards: why can't models simulate Eocene warm climate? Polar proxy reconstructions for the Eocene are all but ignored; except for CO2 reconstructions in section 5.2.2. It is suggested that this section be re-written to include thse components. [Government of United States of America]	box 5.2.
5-672	5	14	29	14	34	This paragraph conflates the complication with estimating sensitivity from a transient climate event like the PETM (but could occur in any time period) with the general problem of estimating sensitivity in the early Cenozoic, given uncertainties in both mean temperature and CO2. However, Lunt et al. (2012) can be used to infer sensitivity for the EECO by identifying which model best matched the proxy data (CCSM3-H). Please note that the PETM preceded the EECO, it doesn't punctuate "an already warm climate state at the beginning of the EECO" [Christopher Hollis, New Zealand]	Noted and taken into account. However, this evidence is limited and we prefer not to infer an estimate of ECS for this time period.
5-673	5	14	30	14	32	"An example is the pronounced transient global warming and carbon-cycle perturbation during the Paleocene Eocene Thermal Maximum, which punctuated an already warm climate state at the beginning of the EECO." The PETM can hardly be used as an indicator of CS in view of the ice-free nature of Earth at that stage. [Andrew Glikson, Australia]	Noted and taken into account.
5-674	5	14	30	14	32	"An example is the pronounced transient global warming and carbon-cycle perturbation during the Paleocene Eocene Thermal Maximum, which punctuated an already warm climate state at the beginning of the EECO." The PETM can hardly be used as an indicator of CS in view of the ice-free nature of Earth at that stage. [Government of Australia]	Noted and taken into account.
5-675	5	14	31	14	32	Paleocene-Eocene Thermal Maximum (PETM) [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Noted and taken into account.
5-676	5	14	31			The PETM is mentioned only twice. Since this is the most well-studied and understood climate-carbon perturbation other than late Quaternary, PETM proxy records are deserving of more discussion in this report [Government of United States of America]	Noted and taken into account with a brief paragraph about carbon cycle recovery which is not considered in Chapter 6.
5-677	5	14	38			It is worth mentioning that while GCMs do have polar amplification, most do not have enough. Changing GHGs generally moves the curve of lat v. temp up or down, but does not flatten the meridional temperature gradient. GCMs can get enough polar amplification to match the data only if they overheat the tropics. This has pretty big implications for future climate prediction. As a result, this issue warrants more discussion. [Government of United States of America]	Noted. However proxies uncertainties limit the assessment of model performance. Box 5.1 has been revised.
5-678	5	14	40	14	41	This sentence could be phrased better: surely the correct statement is that “Polar amplification causes the surface temperature change at high latitudes to exceed the global average temperature change in response to external forcings.” [Government of Australia]	Noted and taken into account.
5-679	5	14	40	14	45	Does this involve a particular timescale? This is rather trivial on a seasonal timescale. [Government of France]	Noted and taken into account.
5-680	5	14	40	14	45	The third and fourth sentences of this paragraph do not connect very well to the previous sentences. Maybe what should be done here is to introduce the question of quantifying the role of internal feedbacks/external forcings in explaining polar amplification? [Masa KAGEYAMA, France]	Noted and taken into account.
5-681	5	14	47			It is worth mentioning/addressing here low arctic clouds can prevent much albedo change. (they have high albedo just like the ice) [Government of United States of America]	Noted and taken into account.
5-682	5	14	49	14	50	... sea ice/ocean ... [Government of Australia]	Noted and taken into account.
5-683	5	14	49	50		The discussion of ice-albedo feedback "threshold behaviour" at the freezing point of sea-ice is here too short, not enough deepened. Actually the albedo of sea-ice is not that high, especially when melt ponds appear on the ice surface. On the other hand, the albedo of cold dry snow is very high (~ 80%) so that the "threshold", if any, is at a much lower temperature. [Government of France]	Noted and taken into account. Space restrictions preclude a lengthy discussion.
5-684	5	14	50	14	51	Surely the end of this sentence should state ”... exceed the melting point of sea ice.” [Government of Australia]	Accepted.
5-685	5	14	52	14	54	Many of these refs on sea-ice retreat are as a result of the 2007 summer. Will there be newer ones as a result of 2012? Important to add any more in here as this will be a really hot topic. [European Union]	Noted and taken into account.

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5-686	5	14	52	14	54	Many of these refs on sea-ice retreat are as a result of the 2007 summer. Will there be newer ones as a result of 2012? Important to add any more in here as this will be a really hot topic. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted and taken into account.
5-687	5	14	54	14	54	Vavrus 2004: wasn't this reference in AR4? [Masa KAGEYAMA, France]	Noted and taken into account.
5-688	5	15	1			ice-sheet-albedo response, it looks like ice, sheet and albedo response; it should be "ice sheet-albedo response". [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Noted and taken into account.
5-689	5	15	2			polar-amplification, we should follow either polar amplification or polar-amplication because both are used in this page. [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Noted and taken into account.
5-690	5	15	2			polar-amplification: your overzealous hyphenator has been at work again! [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted and taken into account.
5-691	5	15	4			"at the eprecessional time scale" -> "at the precessional time scale" [Government of France]	Accepted.
5-692	5	15	4			Southern Ocean air temperature, sounds strange - Could it be re-phrased to read: ocean SST, surface atmosphere over the ocean, or something similar? [Government of United States of America]	Noted and taken into account.
5-693	5	15	5	15	8	Break this sentence up: "... hundreds of meters. This allows..." [Government of Australia]	Noted and taken into account.
5-694	5	15	8	15	11	This piece is not clear. A suggested rewrite could be: "This process is likely to affect the transient SST response, for instance to rapid atmospheric CO2 changes, and requires a much longer equilibration time than in other areas of the world ocean. Hence, in the context of greenhouse warming, an asymmetric warming between Arctic and Southern Ocean would likely occur, with the former responding more quickly to the current and projected changes in greenhouse gases than the latter." [Government of United States of America]	Noted and taken into account.
5-695	5	15	9	15	10	one therefore expects' rather than 'one hence expects' [Government of Australia]	Noted and taken into account.
5-696	5	15	9	15	11	Confusing sentence, please rewrite. [Yueh-Hsin Lo, Taiwan]	Noted and taken into account.
5-697	5	15	10			"between the Arctic" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted and taken into account.
5-698	5	15	11			more quickly rather than quicker. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted and taken into account.
5-699	5	15	16	15	17	It's not clear that polar amplification of mid Pliocene can be concluded without more/new data from central arctic, around Antarctic [Government of United States of America]	Noted. Our assessment is restricted to the available proxy information. Box 5.1 has been revised.
5-700	5	15	23	15	23	comparison --> comparisons [Masa KAGEYAMA, France]	Noted and taken into account.
5-701	5	15	24	15	24	"Robust" is, stritly speaking, a technical term from the statistical sciences. It should be taken up in the Glossary (Annex III). [Manfred Mudelsee, Germany]	Noted and taken into account.
5-702	5	15	25	15	25	2 times --> twice (?) [Masa KAGEYAMA, France]	Noted and taken into account.
5-703	5	15	27	15	27	Comment text: Suggest adding the sentence "The greatest difference in the data-model comparisons in BOX5.2, Fig. 1 lies in the high EECO circum-Antarctic SSTs, with support for high SST's from the data coming from the peak deep-ocean temperatures at this time (Cramer et al., 2011). Interestingly this reflects deep ocean waters warmer than average global surface temperature, in contrast to the present climate system". Support from independent datasets is important to recognise. [Peter Barrett, New Zealand]	Noted and taken into account.
5-704	5	15	29	15	55	Is this relevant in this chapter? (no paleoclimate here) [Government of France]	Noted. However, Boxes are refered to by several chapters. The revised Box 5.1 encompasses several past periods together with instrumental data and projections.
5-705	5	15	31	15	33	Ch. 2 and Ch. 10 have used CRUTEM4 and HadCRUT4. It would be good if these data could be used to produce this number. Bekryaev et al 2010 can't have the last decade 2000-2010 (which is 11 years) as there data ends about 2008. Also 1.35 can't be a stronger rate than 1.36. First the value is smaller and second you shouldn't be comparing a long-term trend with one over an 11-year period. What is the relevance of that	Noted and taken into account.

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						anyway? When was the trend from 1875 worked out? What was the final year? [European Union]	
5-706	5	15	31	15	33	The sentence "Instrumental temperature records show that the Arctic north of 60° latitude has warmed at a rate of 1.36°C per century since 1875, approximately twice as strong as the global average (Bekryaev et al., 2010), with an even stronger rate of warming of 1.35°C during the last decade (2000–2010)." seems to indicate that the warming in the decade 2000-2010 is 10 times faster per decade than during the period 1875-2010. If this is correct, the sentence should be rephrased so it is made clearer that the rates are per century and per decade, respectively [Government of NORWAY]	Noted and accepted.
5-707	5	15	31	15	33	The unit in ℃per century is not suitable for comparing the warming rate of these two periods, since 1875 and the last decade (2000–2010). Probably better to use the data of warming rate in ℃ per decade. [Lei Huang, China]	Noted and accepted.
5-708	5	15	31	15	33	Ch 2 and Ch 10 have used CRUTEM4 and HadCRUT4. It would be good if these data could be used to produce this number. Bekryaev et al 2010 can't have the last decade 2000-2010 (which is 11 years) as there data ends about 2008. Also 1.35 can't be a stronger rate than 1.36. First the value is smaller and second you shouldn't be comparing a long-term trend with one over an 11-year period. What is the relevance of that anyway? When was the trend from 1875 worked out? What was the final year? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted and accepted.
5-709	5	15	31	15	33	Reference to appropriate section of Observational chapters e.g. Chapter 2 [Janice Lough, Australia]	Noted.
5-710	5	15	32	15	33	(i) It is confusing that the rate temperature change is quoted in degC/century on line 32, but in degC/decade on line 33; (ii) It is unsatisfying to quote these rates of change to three significant figures, without any indication of uncertainty; (iii) is it meaningful to calculate a rate of change over a period of just 10 years (2000-2010), given that this is a very short timescale relative to modes of natural climate variability. [Government of Australia]	Noted and accepted.
5-711	5	15	32	15	33	How can 1.35 be stronger than 1.36? What is the global average? [Government of United States of America]	Noted as above.
5-712	5	15	32			twice as much would be better than twice as strong [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted and accepted.
5-713	5	15	33	15	33	Is this per year, per decade, or per century??? Please specify... If this is per century, then it is not stronger than the 1.36 mentioned in the previous line. If this is per decade, then it is truly enormous, almost three times as high as the warming trend in the Antarctic Peninsula region (mentioned 11 lines down), referrred to as "one of the strongest" regional warming trends. [Giuseppe Cortese, New Zealand]	Noted as above.
5-714	5	15	35	15	36	This sentence says observations, which most cited references are about, but Spielhagen is mainly a paleo-study. The authors should distinguish between studies with instruments only vs spliced instrument paleo records [Government of United States of America]	Noted and accepted.
5-715	5	15	35	15	36	This paragraph is confusing: if polar amplification is a surface feedback process, isn't the Polyakov paper about warm Atlantic layer BELOW sea ice? A neglected aspect of arctic climate [Government of United States of America]	Noted and taken into account.
5-716	5	15	37	15	41	the most obvious example of mis-placed projection [Mark Siddall, United Kingdom]	Noted and taken into account.
5-717	5	15	38	15	38	"as well as the insulating effect" --> "as well as in the insulating effect" ? [Masa KAGEYAMA, France]	Noted and taken into account.
5-718	5	15	40	15	40	“by a factor of 2.7” is better than “2.7-fold”. [Government of Australia]	Noted and taken into account.
5-719	5	15	40	15	40	"2.7-fold" --> "by a factof of 2.7" ? [Masa KAGEYAMA, France]	Noted and taken into account.
5-720	5	15	43	15	47	Reference to appropriate section of Observational chapters e.g. Chapter 2 [Janice Lough, Australia]	Noted and taken into account.
5-721	5	15	43	15	51	There are several well-known factors which explain the lack of warming over Antarctica. I think it is very important to mention these to document that this is not a completely surprising observation. [Georg Feulner, Germany]	Noted, and updated with recent references and proposed mechanisms for lack of warming over Antarctica.

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5-722	5	15	43	15	51	The dominant land focus and model results overlook that value in obnservational datasets and ocean involvement. Oceans around Antarctica are warming in places, causing glacier mass balance loss; the cited evidence is weak that Antarctica is not warming [Government of United States of America]	Noted and have added reference to Southern Ocean warming.
5-723	5	15	45	15	47	The second half of this sentence is unclear: the zonal mean at which latitude? [Government of Australia]	Noted and clarified.
5-724	5	15	47	15	50	It is now clear that not only the Antarctic Peninsula, but also West Antarctica, is warming at ~0.5°C/dedade. This paragraph should be modified to reflect that Bromwich et al (2012, in press) show that WAIS is warming at a rate indistinguishable from that on the Peninsula. Ref: Bromwich, D. H. et al. Central West Antarctica among most rapidly warming regions on Earth. Nat. Geosci. in press (2012). [Eric Steig, United States of America]	Noted and ammeneded but will also note following Steig et al., that this trend is not unprecedented in the contex of the ice core records of the last 2k.
5-725	5	15	55	15	55	Replace "melting" with "thawing". Permafrost is thawing, not melting [Victor Brovkin, Germany]	Accepted.
5-726	5	15				As in the rest of Chapter 5, the word "CE" should be added after the calendar years 1875 (line 32), 2000-2010 (line 33), 2081-2100 (line 41), and 1986-2005 (line 41). Also after 1809 and 1815 (in page 26, line 31). [Alejandro Cearreta, Spain]	Noted and taken into account.
5-727	5	15				The authors omit rapid thresholds and feedbacks associated with rapid ice decay subglacial sliding and rapid calving etc [Government of United States of America]	Noted and taken into account.
5-728	5	16	0			Section 5.3.2.1. Between a glacial period and an interglacial there must also be a substantial change in atmospheric water vapour. This will likely affect the greenhouse effect far more than any change in CO2. In the transition from the Eemian to the subsequent glacial CO2 remained practically constant at elevated levels for thousands of years while Antarctic and tropical temperatures dropped significantly. During the Holocene NH ice cores show a slow steady NH cooling trend following the Holocene thermal optimum roughly 6,000 years ago. This cooling trend is consistent with the slow decline in 65N solar insolation. Over this time CO2 increased from 260 to 280ppm. While there may be an apparent covariance between temperature and CO2 in the glacial cycles, the temperature change does not much depend on CO2. The CO2 change is probably reacting to temperature changes in the southern ocean. [Government of United Kingdom of Great Britain & Northern Ireland]	Noted. However, the importance of CO2 for temperature variability during glacial cycles is supported by model simulations. It does not contradict to the fact that in high latitudes orbital forcing also plays important role and the carbon cycle response to climate variations
5-729	5	16	2			The plots are of SST and SAT, not of SST gradient or SAT gradient [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-730	5	16	11	16	11	update Hollis et al., (submitted) [European Union]	Accepted.
5-731	5	16	11	16	11	Hollis et al., (2012) [Christopher Hollis, New Zealand]	Accepted.
5-732	5	16	12	16	12	update Harrison et al., submitted and Haywood et al submitted [European Union]	Accepted.
5-733	5	16	17	18	52	Section 5.3.2: I found this section particularly difficult to follow. Section 5.3's title suggests that it deals with global and hesmipheric scales but subsections 5.3.2.2 and 5.3.2.3 deal with smaller spatial scales, especially the latter subsections whose topic is the monsoon systems. Maybe the rationale of this subsection should be better explained at its beginning. [Masa KAGEYAMA, France]	Noted. The structure of 5.3.2 has been reorganised and 5.3.2.2 has been removed.
5-734	5	16	19	16	19	What about other greenhouse gases? [Government of Australia]	Noted. Due to space limitations, GHG other than CO2 are not discussed. The focus on CO2 is justified by the magnitude of the associated radiative perturbation.
5-735	5	16	19	17	25	This section (5.3.2.1. Role of CO2 in Glacial Cycles) contains important information showing the important role of CO2 in the glacial-interglacial temperature variations, the strength of the effect of the anthropogenic emissions of CO2 on the composition of the atmosphere and how unusual the situation today is in a context of at least 800 000 years or even 2.7 Ma. It states also that a new glaciation is unlikely to happen within the next 50 000 years if atmospheric CO2 remains above 300 ppm. However, this information is not found in the executive summary, please consider to include it there and also in the SPM. [Government of NORWAY]	Accepted. This point is included in the ES and in 5.8
5-736	5	16	19	17	36	In this section, the first paragraph is about the future and all others about the past. I would start with the	Accepted. This paragraph is now in section 5.8

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						information from the past and move the first paragraph at the end of this sub-sub section. This first paragraph could actually be moved to the last section of the chapter (5.8), as it would perfectly fit there. [Masa KAGEYAMA, France]	(irreversibility).
5-737	5	16	21	16	21	Consider starting the opening sentence with: "AR4 provided a sythesis of the numerous ….." [Government of United States of America]	Accepted.
5-738	5	16	21	16	40	The fact that the next glacial period is unlikely (was this meant to be a likelihood word) to happen for 50K years should be a summary point in the ES. I've heard many who think that the Holocene is about to come to an end because the Eemian was relatively short. [European Union]	Accepted. This point is included in the ES and in 5.8
5-739	5	16	21	16	40	The fact that the next glacial period is unlikely (was this meant to be a likelihood word) to happen for 50K years should be a summary point in the ES. I've heard many who think that the Holocene is about to come to an end because the Eemian was relatively short. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted. This point is included in the ES and in 5.8
5-740	5	16	21			Section 5.3.2 do not include references to the effect of paleogeography and the effect of relevant changes in the physiophraphy of the Ocean, and their influence in global circulation, e.g. closure of the Panama isthmus or Indonesian corridor See for example: [José-Abel Flores, Spain]	Rejected. This issue is beyond the scope of section 5.3.2
5-741	5	16	21			Bartoli, G., M. Sarnthein , M. Weinelt , H. Erlenkeuser , D. Garbe-Scho¨nberg a, D.W. Lea. 2005. Final closure of Panama and the onset of northern hemisphere glaciation. Earth and Planetary Science Letters 237, 33–44 [José-Abel Flores, Spain]	Rejected. This issue is beyond the scope of section 5.3.2
5-742	5	16	21			Driscoll, N.W., and Haug, G.H. 1998. A Short Circuit in Thermohaline Circulation: A Cause for Northern Hemisphere Glaciation? Science, 282, 5388: 436-438 . DOI: 10.1126/science.282.5388.436 [José-Abel Flores, Spain]	Rejected. This issue is beyond the scope of section 5.3.2
5-743	5	16	21			At 2.7 Ma, the amplitude [and later frequency] of orbital climate variations changed. Therefore, it shouldn't be be called climate variability often designated for decadal, high frequency changes [Government of United States of America]	Rejected. Climate variability used in more general sense which includes also glacial cycles.
5-744	5	16	23	16	25	Consider rewriting the sentence to read: "Recent paleoclimate modelling with interactive icesheets provides additional support for the theory that variations in Earth's orbital parameters are one of the key factors in generating long-term variability of Earth's climate (Milankovitch, 1920) Citation: Milankovitch, M. 1920. Theorie Mathematique des Phenomenes Thermiques produits par la Radiation Solaire. Gauthier-Villars Paris. [Government of United States of America]	Accepted. The sentence was modified
5-745	5	16	26	26		I think the Milankovitch theory is now more than just a "conjecture" in addtion to the many other references i this section on simulating ice sheet inceptions and deglaciation, see: Jochum, M., A. Jahn, S. Peacock, D. A. Bailey, J. T. Fasullo, J. Kay, S. Levis, and B. Otto-Bliesner (2011), True to Milankovitch: Glacial Inception in the New Community Climate System Model, J. Clim., 25(7), 2226-2239, doi:10.1175/jcli-d-11-00044.1. [William Howard, Australia]	Accepted. Word "conjecture" is removed and Jochum et al. Is cited.
5-746	5	16	29	16	30	It is widely accepted that the glaciation threshold depends on insolation, but it is not accepted by most of paleoclimatologists that the glaciatioin threshold also depends on atmospheric CO2 content. [Lei Huang, China]	Noted. Does not require action.
5-747	5	16	31	16	32	"These results show consistently that a glacial inception is unlikely to happen within the next approximate 50 kyr" I think it's important to be careful about language, though this may seem a pedantic and semantic point. The models, especially those projecting a possible future do not "show" anything (yet). The language here and elsewhere this kind of discussion occurs, needs to be along the lines of "the models suggest" or similar language. Even for the past, models do not "show" anything but provide insight into plausible or likely	Accepted. The wording is changed

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						mechanisms for past variations. [William Howard, Australia]	
5-748	5	16	31	16	34	A new ice age could be much sooner. Check the paper by Tzedakis et al. 2012 (Nature Geoscience doi:10.1038/ngeo1358) [Yueh-Hsin Lo, Taiwan]	Rejected. Tzedakis et al. 2012 does not imply that ice age will start soon for the current CO2 level.
5-749	5	16	32	16	32	"next approximate 50 kyr" --> "next 50 kyr approximately" ? [Masa KAGEYAMA, France]	Accepted.
5-750	5	16	32	16	40	I find this statement very important. I think it deserves to be in the executive summary (the low probability of the cooling instead of warming). In my country at least it is not evident for many people including scientists and politicians. [Olga Solomina, Russia]	Accepted. Now the next glacial inception is included in the ES.
5-751	5	16	32			It is an interesting finding from modeling efforts “that a glacial inception is unlikely to happen within the next approximate 50 kyr…”, partially based on similarity between the current interglacial with MIS 5. The point is the coincidence of the current interglacial with the long eccentricity minimum. Recent studies show that the oceanic carbon reservoir responses to the long eccentricity cycle, with occurrence of maximal values of sea water δ13C at the long eccentricity minimum (Wang et al., 2010, EPSL, 290: 319), but we don’t yet know what is its effect on air CO2 concentration and on glacial cyclicity. In short, the current long eccentricity minimum must have its climate role, but the related studies are just at its beginning. It may be too early, therefore, to predict the natural length of the current interglacial without knowing the climate role of long-term cycles of 105yrs time scale, specifically the response of carbon cycle to the long eccentricity forcing. [PINXIAN WANG, China]	Noted but Chapter 5 does not discuss carbon cycle.
5-752	5	16	34	16	38	Do we need the "old" (AR4 period) references here? (I don't mind, it is just not homogeneous with other sections) [Masa KAGEYAMA, France]	Noted. The references have been checked and relevant ones (including from AR4 period) cited.
5-753	5	16	35	16	35	For reasons in the previous comment I don't believe this assertion is valid. Tzedakis et al put it at 240ppm. [Government of United Kingdom of Great Britain & Northern Ireland]	Noted. But our statement "concentrations below preindustrial" are not inconsistent with 240 ppm
5-754	5	16	37	16	40	I think one could strengthen this statement. "Co2 concentrations will exceed 300 ppm until the year 3000 CE by which time northern hemisphere summer insolation will already be rising towards its next peak." In my view this would justify a statement that inception is unlikely for at least another precession cycle (20 ka), but your present statement is also adequate. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted. The statement has been revised.
5-755	5	16	37			the RCP 2.6 scenario..I think, RCP is not explained in the text (I missed this somewhere while reading!) [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Noted. RCP terms are defined in chapter 1.
5-756	5	16	38	16	40	The authors should consider rewriting the sentence to read: "Based on current understanding of glacial inceptions during the late Quaternary, it is very unlikely that orbital forcing on its own would trigger a glacial inception before the end of the next millennium." [Government of United States of America]	Accepted. The sentence has been modified.
5-757	5	16	40	16	40	"it seems therefore" - could be replaced with a stronger statement "it is therefore" [Victor Brovkin, Germany]	Accepted. The sentence is modified accordingly.
5-758	5	16	42	16	42	Is the adverb "strongly" justified, here ? [Government of France]	Noted but the sentence has been deleted.
5-759	5	16	42	16	49	The first sentence of this paragraph states that understanding glacial-interglacial dynamics remains a challenge while the last sentence states we know how to model glacial interglacial cycles. Isn't this a little contradictory? [Masa KAGEYAMA, France]	Noted but the sentence has been deleted.
5-760	5	16	43	16	43	Remove the word "abrupt" before "glacial terminations". [Manfred Mudelsee, Germany]	Noted but the sentence has been deleted.
5-761	5	16	51	17	4	Atmospheric CO2 concentrations covary with several proxies, but are not in phase over the last climatic cycles. For example, CO2 increases few thousands of years before sea level and Northern Hemisphere deglaciation during Terminations. It could be stressed here instead of stating that "CO2 covary with climate proxy climate records". [Government of France]	Rejected. On the orbital scale, CO2 covaries with other proxies. The revised text includes a brief discussion of leads/lags
5-762	5	16	52			The text should read: "...as an internal feedback…" [Government of United States of America]	Accepted.
5-763	5	16	56	16	56	Cortese et al., 2007 also covers the last 450ka in the Southern Ocean and compares SST to insolation and CO2. PALEOCEANOGRAPHY, VOL. 22, PA4203, doi:10.1029/2007PA001457, 2007 [Giuseppe Cortese, New Zealand]	Rejected. The reference list is already too long.

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5-764	5	16	56	16	56	Elderfield et al 2012, Science should be cited here alongside Elderfield et al 2010 [Mark Siddall, United Kingdom]	Accepted. Elderfield 2012 is cited here instead of Elderfield 2010.
5-765	5	16				Box 5.2, Figure 1. The labels for confidence at the bottom of the figure are confusing. Are they supposed to be keyed to circle sizes as referred to in the caption? [Government of United States of America]	Accepted.
5-766	5	17	8	17	11	On the parameters piece, a definition of stack is warranted. [Government of United States of America]	The meaning of the comments is not clear.
5-767	5	17	9	17	17	One could also include Holden et al (2010) in part f, although I have not seen data for the other parts. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted. Holden et al results are included.
5-768	5	17	13			I don't think it makes sense to use Waelbroeck for sea level here. As far as I know this is really only a recalibrated benthic oxygen isotope dataset. It would make more sense to use Elderfield et al 2012, which at least makes a correction for deepwater temperature. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted. The Waelbroeck reconstruction has been substituted by Elderfield et al. 2012 and Rohling et al 2010.
5-769	5	17	19	17	25	This critical point warrants more explanation, review topic of lead/lag, ice close off time and newest near 0 estimates in more detail. Esp. Parrenin and Pedro. Shakun synthesis is not convincing to some; but text must match CO2-temperature lag-lead in Ch 5 tables and figures [Government of United States of America]	Noted. However, more detailed discussion is not possible because of the shortage of space but additional information is given in the Table 5.A.5. Parrenin and Pedro are cited. Shakun et al. is only discussed in relation with the hemispheric temperatures, not CO2.
5-770	5	17	19	17	25	This is a very important comment, but I find that it is hidden in the text. Why not make this topic the subject of a short and clear Box? Also I think you should add the confidence level to the new conclusion. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Rejected. Adding a new box is not possible due to the lack of space. Additional information is given in Table 5.A.5.
5-771	5	17	19	17	36	This section is good. Again this apparent lead of temperature ahead of CO2 is trotted out everywhere. If you're confident of this point I would add it to the ES. [European Union]	Rejected. It was decided to focus the ES statement on the role of CO2 in glacial-interglacial variations rather than the sequence of events during the last termination
5-772	5	17	19	17	36	This section is good, and it too short be a point in the ES as well. Again this apparent lead of temperature ahead of CO2 is trotted out everywhere. If you're confident of this point I would add it to the ES. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Rejected. It was decided to focus the ES statement on the role of CO2 in glacial-interglacial variations rather than the sequence of events during the last termination
5-773	5	17	19	17	36	These paragraphs have a related topic, yet the link between them is not obvious. It would be good to merge them and state how the results from modelling fit the new results from the data about the lead/lag of temperature changes over Antarctica w.r.t CO2 changes. [Masa KAGEYAMA, France]	Accepted. These two paragraps are merged.
5-774	5	17	21			The CO2 to temperature phasing is a critically important point. Can the summary header be altered to reflect this very new and crucial information? (see Page 3 also) [Government of United States of America]	Rejected. It was decided to focus the ES statement on the role of CO2 in glacial-interglacial variations rather than the sequence of events during the last termination
5-775	5	17	23	17	23	The discussion on CO2-climate leads and lags needs to begin with why (if at all) they are important. The matter is also not straightforward- for example, the phasing of the initial CO2 and temperature increases might be different from the bulk of the changes later in the transition. And what does the relative timing of the changes say about CO2-climate feedbacks? [Government of Australia]	Noted. However, it is not possible to expand this discussion due to space limitations.
5-776	5	17	23			Is there a repetition of ice? Should it rather read gas-ice ages? [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted. Typo is corrected.
5-777	5	17	25	17	25	update Parrenin et al., submitted [European Union]	Accepted.
5-778	5	17	25	17	25	The study by Pedro et al. (2012) may not be reliable and should perhaps not be used for assessment. The reason is that Pedro et al. (2012, page 1216, left, lines 2-3 therein) interpolated the CO2 data to 20-yr resolution, although the original CO2 data have an average resolution (for the analysed time period) of 145 yr	Noted.

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						(Byrd record) and 266 yr (Siple record). That means, Pedro et al. (2012) may have underestimated the statistical error of their lag determination. [Manfred Mudelsee, Germany]	
5-779	5	17	27	17	36	This is an example of where I think the bipolar seesaw concept is discussed in a very uncritical manner. There is a critical, skeptical literature about this which is not properly considered (see papers by Wunsch and group). The role of atmospherics, the trigerring mechanism (sea ice, FW forcing, shifts in the polar front), the role of astronomical forcing are all poorly understood. Much of the writing about this needs to be much more carefully considered. From the writing the confidence seems at the level of 'virtually certain' and not 'highly confident' - i.e. no uncertainty is discussed at all. A more complete consdieration of the breadth of literature on this would give 'medium confidence' [Mark Siddall, United Kingdom]	Rejected. The concept of "bipolar seesaw" is accepted by majority and supported by both data and models.
5-780	5	17	28	17	28	The Shakun paper also shows that the deglacial CO2 increase led NH and possibly global temperatures through most of the warming. [Government of Australia]	Noted. However, uncertainties in the age scales of the source data used by Shakun et al limit the discussion of leads and lags between CO2 and the reconstructed temperature.
5-781	5	17	31	17	31	As it reads this sentence makes it sound like the AMOC always weakens during Terminations. While this might be the case we do not have conclusive evidence of it. Perhaps re-word this slightly: "…caused by weakening of the AMOC during the early part of the last deglaciation" [Stephen Barker, United Kingdom of Great Britain & Northern Ireland]	Accepted. We now only discussion the last glacial termination.
5-782	5	17	33	17	33	Insert 'the' before 'southern' [Peter Burt, United Kingdom]	Accepted.
5-783	5	17	33	17	36	suggest to delete this sentence “According to these mechanisms, southern temperature lead over Northern Hemisphere neither contradicts the northern hemisphere forcing of deglacial ice volume changes (high confidence), nor the important role of CO2 in generating glacial-interglacial temperature variations due to the greenhouse effect.”. [Lei Huang, China]	Rejected. Suggestion to delete the text is not justified, but the text has been reformulated.
5-784	5	17	33	17	36	This sentence can be deleted. It is not so meaningful for this paragraph to describe two contradictions especially in view of the dispute about the role of CO2 in glacial-interglacial cycles. [Lei Huang, China]	Rejected. Suggestion to delete the text is not justified, but the text has been reformulated.
5-785	5	17	33	17	36	This is very oddly formulated as a double negative, and anyway is not quite correct as the implication of Shakun for example is that while insolation starts the sequence it is not responsible for the bulk of "deglacial ice volume changes".. Much simpler: "According to these mechanisms, southern temperature lead over northern hemisphere is consistent with the northern summer insolation forcing of deglacial onset, and with the importnat role of CO2 in generating...." [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted. The text has been rewritten.
5-786	5	17	38	17	55	This section needs more complete discussion of precipitation/hydrological changes related to the westerlies, particularly in the southern hemisphere. Anticipated poleward drift of the westerlies is very likely to cause extremely important reductions of seasonal rainfall in the temperate winter-rainfall-zones of Africa, South America, and Australia-New Zealand, not just the upwelling-changes mentioned. This historical and modeled link between warming and drought in the southern mid-latitudes should be one of the major finding of this chapter. There is plenty of STRONG (not just "medium") paleo evidence for shifts in the austral westerlies during the Holocene that should be mentioned; see next line for citations. [Jay Curt Stager, United States of America]	Noted. Section revised, focusing on Last Glacial Termination.
5-787	5	17	38	17	55	Appropriate references include: Biastoch et al. 2009, Nature 462: 495-498. Lamy et al., 2010, Nature Geoscience 3:695-699. Stager et al. 2012, Climate of the Past 8:877-887. [Jay Curt Stager, United States of America]	Noted. Section revised, focusing on Last Glacial Termination.
5-788	5	17	40	17	40	exerts --> exert [Masa KAGEYAMA, France]	Noted. Section revised, focusing on Last Glacial Termination.
5-789	5	17	40	17	45	The hypothesis by Toggweiler about significant effect of southern westerlies on atmospheric CO2 during glacial times has little support by simulations with the other models than Toggweiler’s one. If you want to discuss this controversial hypothesis, you need to go into details of model results from simulations of Tschumi et al. ,Paleoceanography, 2008 and Menviel et al., Paleoceanography, 2008 who found only small changes of atmospheric CO2 in response to SHW variations, as well as model results by d’Orgeville et al., GRL, 2010.	Noted. Section revised, focusing on Last Glacial Termination.

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						[Victor Brovkin, Germany]	
5-790	5	17	40	17	55	More than solely Westerlies intensification during terminations is the contraction of the Westerlies belt. The contraction does intensify wind stress at the core. This in turn increases Southern Ocean upwelling. [Government of France]	Noted. Section revised, focusing on Last Glacial Termination.
5-791	5	17	40	17	55	It's unclear whether this section claims that the evidence contradicts the model results? If that's the case, perhaps the authors could just say that rather than double negatives such as "climate models…do not present an unequivocal picture." [Government of United States of America]	Noted. Section revised, focusing on Last Glacial Termination.
5-792	5	17	40	17	55	The INTIMATE Project has recently reviewed changes in Southern Hemisphere changes in ocean and atmosphere circulation, at least in the Australia-New Zealand Sector: Bostock, H. C., T. T. Barrows, L. Carter, Z. Chase, G. Cortese, G. B. Dunbar, M. Ellwood, B. Hayward, W. Howard, H. L. Neil, T. L. Noble, A. Mackintosh, P. T. Moss, A. D. Moy, D. White, M. J. M. Williams, and L. K. Armand A review of the Australian-New Zealand sector of the Southern Ocean over the last 30 ka (Aus-INTIMATE project), Quat. Sci. Rev., doi:10.1016/j.quascirev.2012.07.018. Lorrey, A. M., M. Vandergoes, P. Almond, J. Renwick, T. Stephens, H. Bostock, A. Mackintosh, R. Newnham, P. W. Williams, D. Ackerley, H. Neil, and A. M. Fowler (2012), Palaeocirculation across New Zealand during the last glacial maximum at ~21 ka, Quat. Sci. Rev., 36, 189-213, doi:10.1016/j.quascirev.2011.09.025. [William Howard, Australia]	Noted. Section revised, focusing on Last Glacial Termination.
5-793	5	17	40	17	55	It might be stated here that the mechanisms trigerring changes of the southern westerlies are not well known. A discussion of these mechanisms (impact of Antarctic ice-sheet size, feedback from the eddies, meridional temperature gradient) can be found in the cited reference of Chavaillaz et al. [Masa KAGEYAMA, France]	Noted. Section revised, focusing on Last Glacial Termination.
5-794	5	17	40	18	2	This section is unclear: it's significance is not clear, the data-climate model discrepancy is highlighted, but not so in other parts of the text where other discrepancies exist [Government of United States of America]	Noted. Section revised, focusing on Last Glacial Termination.
5-795	5	17	40			"exert" not "exerts" as it refers to the "strength and position" [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted. Section revised, focusing on Last Glacial Termination.
5-796	5	17	40			Southern Hemisphere westerlies (in line 40) and southern hemisphere westerlies (in line 52) [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Noted. Section revised, focusing on Last Glacial Termination.
5-797	5	17	41			The prefered technical term is "partitioning" of CO2... [Government of France]	Noted. Section revised, focusing on Last Glacial Termination.
5-798	5	17	42			Box5.1: The effect of deforestation on earth system feedback should be added. There are several papers about this topic appearing on top journals during the last three years. Dallmeyer and Classen (2010) have also discussed the climate cooling effect of deforestation in China. [Hongyan Liu, China]	Noted. Section revised, focusing on Last Glacial Termination.
5-799	5	17	44	17	45	The reference "This mechanism provides a positive feedback to the climate-carbon system during glaical terminations (see Chapter 6)" is wrong. In the section 6.2.2, there is an assessment of CO2 effects of numerous mechanisms in circulation changes, and not the wind-driven mechanism on its own. [Victor Brovkin, Germany]	Noted. Section revised, focusing on Last Glacial Termination.
5-800	5	17	44	17	45	Positive feedback or negative feedback, or nothing? [Lei Huang, China]	Noted. Section revised, focusing on Last Glacial Termination.
5-801	5	17	45	17	45	typo, change "glaical" to "glacial" [Peter Köhler, Germany]	Noted. Section revised, focusing on Last Glacial Termination.
5-802	5	17	45	17	46	The tenor of this paragraph seem to put an unreasonable amount of faith in the climate model results. Climate model simulations for modern conditions fail to resolve many fine scale issues such as the position and sensitivity of westerlies to surface ocean conditions. The authors should consider deleting the sentence "However, the actual state of the Southern Hemisphere westerlies under LGM conditions remains debated." since the rest of the paragraph lays out the issues. Data indicate consistently an intensification/shift, model experiments do not. [Government of United States of America]	Noted. Section revised, focusing on Last Glacial Termination.

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5-803	5	17	45			… glacial... (typo) [Government of France]	Noted. Section revised, focusing on Last Glacial Termination.
5-804	5	17	45			spelling typo (glacial) [Michael Hren, United States of America]	Noted. Section revised, focusing on Last Glacial Termination.
5-805	5	17	45			typo "glacial" [Sandra Passchier, United States of America]	Noted. Section revised, focusing on Last Glacial Termination.
5-806	5	17	46	17	50	Add reference Chiessi et al. (2008. Geology, doi:10.1130/G24979A.1) to "Anderson et al., 2009; Bard and Rickaby, 2009; Beal et al., 2011; De Deckker et al., 2012; Fletcher and Moreno, 2011; Sikes et al., 2009". None of the references listed represents the sensitive western South Atlantic, but only Chiessi et al. (2008). Also, the proxy data and model experiments showed in Chiessi et al. (2008) establish a direct relationship between the southward shift of the Southern Hemisphere westerlies, the strengthening of the Agulhas leakage, and the recovery of the Atlantic meridional overturning circulation during the last deglaciation. Moreover, Chiessi et al. (2008) was published after the AR4. [Government of Brazil]	Noted. Section revised, focusing on Last Glacial Termination.
5-807	5	17	47	17	50	the proxy evidence about wind shifts has been synthesised and reviewed by Kohfeld, K.E., Graham, R., de Boer, A.M., Sime, L.C., Wolff, E.W., Le Quere, C., Bopp, L., Submitted. Southern hemisphere westerly wind changes during the last glacial maximum. I. Paleo-data synthesis. Quat. Sci. Rev., which was submitted in early July. An accomanying modelling study submitted in the same week and suggesting that many of the data observations do not require a wind chnage (Sime, L.C., Kohfeld, K., Le Quere, C., Wolff, E.W., de Boer, A.M., Graham, R.M., Bopp, L., Submitted. Southern Hemisphere Westerly Wind Changes during the Last Glacial Maximum 2: Model-Data Comparison. Quat. Sci. Rev.) would probably lower your overall confidence in respect of even the data. In any case these new papers should probably be cited (I believed that one or both were sent to the AR5 authors) [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted. Section revised, focusing on Last Glacial Termination.
5-808	5	17	48	17	48	Direct evidence for a southerly shift in Southern Ocean fronts during HS1 (early part of last deglaciation) was given by Barker et al., 2009. This would be a good reference to include here. Also, the southerly shift(s) probably did not last throughout the whole deglacial period (as implied here) but rather occurred during HS1 and the YD (with a northerly shift occurring at the onset of the B/A). S. Barker et al., Interhemispheric Atlantic seesaw response during the last deglaciation. Nature 457, 1097 (2009). [Stephen Barker, United Kingdom of Great Britain & Northern Ireland]	Noted. Section revised, focusing on Last Glacial Termination.
5-809	5	17	57	18	2	There is additional literature on atmospheric circulation changes in the LGM and MIS4 which shows also the impact of changes in the ice sheet topography. In both papers below we show the impact on mid latitude cyclone, the jet and on weather types (based on a cluster algorithm). The set of sennsitivty studies show the imparance of the LAaurentide ice sheet in steering the storm track. Hofer, D., C. C. Raible, A. Dehnert, and J. Kuhlemann, 2012: The impact of different glacial boundary conditions on atmospheric dynamics and precipitation in the North Atlantic region, Climate of the Past, 8, 935-949; Hofer, D., C. C. Raible, N. Merz, A. Dehnert, and J. Kuhlemann, 2012: Simulated winter circulation types in the North Atlantic and European region for preindustrial and glacial conditions, Geophys. Res. Lett., 39, L15805, doi:10.1029/2012GL052296 [Christoph Raible, Switzerland]	Noted. Section revised, focusing on Last Glacial Termination.
5-810	5	17	57	18	2	This remark is like an afterthought that is just dropped in. It cannot be evaluated in this form because there is no context for it. I think a bit more elaboration is warranted. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Noted. Section revised, focusing on Last Glacial Termination.
5-811	5	17	58	17	58	Hemisphere westerlies were orographically… [Matthew Konfirst, United States of America]	Noted. Section revised, focusing on Last Glacial Termination.
5-812	5	17	58			"paths….were orographically" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted. Section revised, focusing on Last Glacial Termination.
5-813	5	18	4			A change in speleothem d18O IS NOT equivalent to a change in local rainfall. It doesn't matter what you title the local rainfall "overall drying in... monsoon systems". The monsoon has a very specific definition, that should be consistent across all IPCC chapters. [Government of United States of America]	Noted. We have added an extra paragraph talking about the sources of information on monsoon changes and also the caveats in the interpretation of d18O changes

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5-814	5	18	6	18	28	There is new evidence regarding tropical moisture influx to the interior southwestern U.S. that fills pluvial lakes during the LGM and deglaciation. Although perhaps not strictly "monsoon" it may be appropriate to include this information here. See Lyle et al., 2012, Out of the tropics: The Pacific, Great Basin lakes, and late Pleistocene water cycle in the western United State: Science, v. 337, p. 1629-1633. In addition, a new 500-kyr record for pluvial Lake Manix in the Mojave Desert supports enhanced moisture and possibly tropical influence in this area during parts of interglacial as well as glacial periods. See Reheis et al., 2012, A half-million-year record of paleoclimate from the Lake Manix core, Mojave Desert, California: Palaeo-3, in press. [Government of United States of America]	Noted. But these data clearly do not fit into the monsoon section (see also Figure 5.4 c, cyan shading). We have refrained from citing these studies
5-815	5	18	6	18	28	The monsoon section seems out-of-place/context. Being such an important topic, can its relevance be explained more - perhaps over various timescales, etc.? [Government of United States of America]	Noted. We have two monsoon subsubsections (one 5.3.2.3 in the Glacial interglacil Dynamics subsection; one in the Regional Changes during the Holocene section). We think that this features monsoons quite prominently across different timescales, as also documented in Figure 5.4
5-816	5	18	6			One could argue that the monsoon system respond to changes in meridional temperature gradients resulting from orbital forcing, rather than directly to orbital-mechanics induced changes in solar irradiance. Modify ? [Government of France]	Accepted. Text changed to "Most monsoon systems have been found to respond to orbitally-induced changes in surface temperature gradients and in particular to the precession forcing (Figure 5.4)"
5-817	5	18	9	18	10	It may be not suitable to describe these regions (Northern Africa, the Arabian Peninsula and Arabian Sea) as regions closer to the major Northern Hemispheric ice sheets than south China. [Lei Huang, China]	Comment unclear. South China is definitly farther away from the NH ice-sheets than Northern Africa
5-818	5	18	9	18	10	To clarify: perhaps a period after "cycles" and then start the next phrase with "In contrast.."? And remove "also"? [Sandra Passchier, United States of America]	Accepted
5-819	5	18	9	18	11	None of the proxy records mentioned in these lines are shown in figure 5.4 as implied [Government of United States of America]	Accepted. The Fig 5.4 reference has now been placed correctly.
5-820	5	18	11	18	12	Pre-AR5 references? Should they be kept? [Masa KAGEYAMA, France]	Noted. Of course pre-AR5 references are allowed, in particular, if they refer to well accepted consolidated results.
5-821	5	18	13	18	13	The reference to Qiuzhen et al. 2008 is not correct. It should be Yin et al. 2008 (inversion name-first name) [Hugues Goosse, Belgium]	Accepted
5-822	5	18	13	18	13	error in citation (I believe): Quizhen et al 2008 should be Yin et al 2008 [Masa KAGEYAMA, France]	Accepted
5-823	5	18	13	18	13	"Qiuzhen" could be given name of the author, please change to family name [Hongyan Liu, China]	Accepted
5-824	5	18	13			Quizhen spelling [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-825	5	18	14	18	18	Why is this information, which is limited to one site and one interglacial MIS, considered notable or relevant to the discussion? [Government of United States of America]	Accepted, sentence deleted
5-826	5	18	16	18	16	The "and" (4th word) appears a bit awkward to me. [Hugues Goosse, Belgium]	Sentence deleted
5-827	5	18	16	18	16	remove " and," [Masa KAGEYAMA, France]	Sentence deleted
5-828	5	18	16	18	16	Delete "and" [Yueh-Hsin Lo, Taiwan]	Sentence deleted
5-829	5	18	16			Delete "and," after "SST". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Sentence deleted
5-830	5	18	16			something wrong with sentence; delete 'and'? [Elie Verleyen, Belgium]	Sentence deleted
5-831	5	18	16			"…the weaker interglacial amplitudes prior to the Mid-Brunhes event (430 ka),…are not captured in a speleothem record from Marine Isotopic Stage (MIS) 13 from Borneo" . This observation may have another interpretation. Stage 13 is distinguished by unusually intensified global monsoon recorded in the Mediterranean (Rassignol- Strick et al., 1998), Indian (Bassonot et al., 1994), tropical (Harris et al., 1997) and	Noted. Original sentence deleted

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						South Atlantic (Schmieder et al., 2000), as well as the Loess Plateau (Guo et al., 1998). Again, the exceptional intensity of monsoon precipitation may be associated with the oceanic δ13C maximum and hence related to a long-term cycle of 105yrs time scale (Wang et al., 2003, Geology, 31: 239). [PINXIAN WANG, China]	
5-832	5	18	16			"and" to be removed [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Sentence deleted
5-833	5	18	18	18	21	This sentence needs to be modified because such an out of phase interhemispheric change on glacial-interglacial timescales is not led only by increasing (decreasing) boreal (austral) summer insolation. [Lei Huang, China]	Noted. Text changed to "Increasing boreal summer insolation can generate strong interhemispheric surface temperature gradients that lead to large-scale drying in Southern Hemispheric summer monsoon systems and an increased hydrological cycle in the Northern Hemisphere tropics (Figure 5.4 a,d, g)."
5-834	5	18	19			I don't see any need for "(decreasing)…(austral)". This is clear from the figure and just confuses the reader. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text revised accordingly
5-835	5	18	23			Consider adding this reference: Across longitudes, the response of precipitation may, however, be different for the same orbital forcing (Shin et al, 2006). [Shin et al. (2006). Understanding the Mid-Holocene Climate. Journal of Climate, 19, 2801-2817.] [Government of United States of America]	Accepted. Shin reference included
5-836	5	18	25	18	28	On the different impacts of the same orbital forcing on different monsoon systems, the work of Marzin et al could be cited (Marzin and Braconnot, 2009, Variations of Indian and African monsoons induced by insolation changes at 6 and 9.5 kyr BP, Climate Dynamics, 33, 215-231 [Masa KAGEYAMA, France]	Accepted, reference included
5-837	5	18	31	18	52	Caption for Figure 5.4. First, the caption should explain the blue bands in b, e, f, g, and i. Also there are a number of mismatches in the colors on the figure and those described in the caption. In panels d and f the lines are not purple but red. In panels g and i the described black lines are actually gold. And in panel h the brown line is blue. On line 35, perhaps insert a colon after "(Wang and Ding, 2008)" ? [Government of United States of America]	Accepted Figure caption revised
5-838	5	18	31	18	52	Color indications in the caption text for figure 5.4 do not match the figure. Specifically, panels a, d, f, g, h and i [Matthew Konfirst, United States of America]	Accepted Figure caption revised
5-839	5	18	32	18	33	There are some inconsistencies between Figure 5.4 and its corresponding legend. For example, in lines 32-33 should says: "austral summer insolation changes at 20ºS (gray dashed)". This parameter is not represented with a blue line in Figure 5.4a. [Eugenia M. Gayo, Chile]	Accepted. Figure caption revised
5-840	5	18	33			the SH insolation cahnges look grey not blue to me [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted. Figure caption revised
5-841	5	18	34	18	34	Should says: "location indicated by gray circle in c". [Eugenia M. Gayo, Chile]	Accepted. Figure caption revised
5-842	5	18	34			NGRIP circle is grey not black [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted. Figure caption revised
5-843	5	18	35	18	35	Should says "South America Summer Monsoon" (SASM) instead of "South American Monsoon" (SAMM). This correction must be also done in panel c from the Fig. 5.4. [Eugenia M. Gayo, Chile]	Noted. We have omitted all the monsoon acronyms
5-844	5	18	35	18	36	Inconsistent abbrevation vs full term ordering [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted. We have omitted all the monsoon acronyms
5-845	5	18	37	18	37	Should says: "Reconstructed (red) standardized…" [Eugenia M. Gayo, Chile]	Accepted. Figure caption revised
5-846	5	18	37			I can't see a purple line [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted. Figure caption revised
5-847	5	18	43	18	43	Should says: "anomalies (red) in Huangye"… [Eugenia M. Gayo, Chile]	Accepted. Figure caption revised
5-848	5	18	43			Purple is actually red [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted. Figure caption revised
5-849	5	18	45	18	46	Clarify exactly which version of the CSIRO model – there are different versions in the PMIP3 (CSIRO-Mk3L-1-2) and CMIP5 (CSIRO-Mk3-6-0) databases. [Government of Australia]	Accepted. It is the CSIRO-MK3L-1-2 model

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5-850	5	18	45			This describes the wrong simulations. There is definitely data over more than the last millenia [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted. These runs often called Last Millennium runs start in 850 and extend to 1850 or beyond. Caption modified
5-851	5	18	46	18	46	Substitute “Botuvera” by “Botuverá”. This is actually the appropriate way to write the cave´s name as published in Cruz et al. (2005. Nature, doi:10.1038/nature03365). [Government of Brazil]	Accepted
5-852	5	18	47	18	47	Substitute “Cruz et al., 2009” by “Cruz et al., 2005”. The d18O record from Botuverá Cave depicted in Figure 5.4 was originally published in Cruz et al. (2005. Nature, doi:10.1038/nature03365). [Government of Brazil]	Accepted
5-853	5	18	47	18	52	Inconsistencies between the graphs and legends for Fig. 5.4 make hard to interpret the information presented. Moreover, it is necessary to add Y-axis for each parameter represented within a plate (i.e Fig. 5.4 d, f, g, i). [Eugenia M. Gayo, Chile]	Accepted. Figure caption revised
5-854	5	18	47			Black is actually orange [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted. Figure caption revised
5-855	5	18	51			Black is actually orange [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted. Figure caption revised
5-856	5	18	52			This describes the wrong simulations. There is definitely data over more than the last millenia [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted These runs often called Last Millennium runs start in 850 and extend to 1850 or beyond. Caption modified.
5-857	5	18	56	18	56	"characterized by large response" --> "characterized by a large response" [Masa KAGEYAMA, France]	Accepted.
5-858	5	18	56	18	57	The timing used for the LGM should be indicated…is it 20 ka or ? [Government of Iceland]	Taken into account and explicitely introduced in Table 5.1
5-859	5	18	56	20	27	This whole section would be a lot clearer if the timing for LGM was defined at the beginning [Government of Iceland]	Taken into account and explicitely introduced in Table 5.1
5-860	5	18		19		Glacial climate sensitivity: Uncertainty in difference in LGM-pre-industrial temperature is still very large…due in large part to our uncertainty in SSTs. This discussion should include previous research in the tropics discussing the tropical snowline and isotopic evidence of cooling which is very large in the tropics (see refs. back to Guilderson et al.2001 Rind and Peteet 1985 Stute et al. 1995 and Greene et al. 2002 [Government of United States of America]	Accepted. We discuss the uncertainty, including the uncertainty in the new compilation by MARGO.
5-861	5	18				Figure 5.4. I think that the dots and lines in this figure are too heavy. Can be made a lot neater. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-862	5	19	1	19	30	There is no information on the reappraisal of Antarctic summer sea ice limit during the LGM (Gersonde et al., 2005). These authors show a much greater sea ice seasonality during the LGM than today, which may have large consequences on the THC (Shin et al., 2003, GRL 30, 1096). Should model performances compared to geological reconstructions be mentioned (Roche et al., QSR 2012) ? [Government of France]	Accepted. Sea ice at LGM model versus geological data is mentioned.
5-863	5	19	1	20	27	In this section and also later there seems to be more about the latest modelling than the latest observational evidence. Perhaps there should be some justification for this in the Introduction as this occurs fro other periods also. Bringing the two togetehr is good, but I just think that the improvements to understanding that this brings need justification. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. This section however refers to some new individual time series and to MARGO Project Members (2009) for the SST and Bartlein et al. (2011) for the land temperature which are the two latest synthesis of proxy data, including spatial information, which are of great value in comparison with modelling.
5-864	5	19	6			Please give a reference for the -6C cooling over the Mediterrenea. There is also evidence of cooling in the eastern part of the Mediterranean, see Fleitmann et al. (2009, GRL); Fleitmann, et al., 2009: Timing and climatic imprint of Dansgaard-Oeschger events in stalagmites from Northern Turkey, GRL, 36, L19707 [Christoph Raible, Switzerland]	Noted. The reference for the 6C cooling comes from the previous sentence (MARGO 2009). The suggested reference has not been added.
5-865	5	19	8	19	10	for glacial-interglacial changes in Southern Ocean Polar Front positions see papers from comment above: Ho, S. L., G. Mollenhauer, F. Lamy, A. MartÌnez-Garcia, M. Mohtadi, R. Gersonde, D. Hebbeln, S. Nunez-	Accepted. Although we cannot quote all references, because of space limit.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Ricardo, A. Rosell-Melé, and R. Tiedemann (2012), Sea surface temperature variability in the Pacific sector of the Southern Ocean over the past 700 kyr, Paleoceanography, 27(4), PA4202, doi:10.1029/2012pa002317. Cortese, G., A. Abelmann, and R. Gersonde (2007), The last five glacial-interglacial transitions: A high-resolution 450,000-year record from the subantarctic Atlantic, Paleoceanography, 22, PA4203, doi:10.1029/2007PA001457. Howard, W. R., and W. L. Prell (1992), Late Quaternary surface circulation of the Southern Indian Ocean and its relationship to orbital variations, Paleoceanography, 7(1), 79-118, doi:10.1029/91PA02994. Schneider Mor, A., R. Yam, C. Bianchi, M. Kunz-Pirrung, R. Gersonde, and A. Shemesh (2012), Variable sequence of events during the past seven terminations in two deep-sea cores from the Southern Ocean, Quat. Res., 77(2), 317-325, doi:10.1016/j.yqres.2011.11.006. [William Howard, Australia]	
5-866	5	19	9	19	9	Polar Front is a proper name for a major oceanographic feature, so it should be capitalised. [Robert Larter, United Kingdom]	Accepted.
5-867	5	19	9	19	10	The proxy-based reconstruction mentioned needs to be cited at the end of this sentence. The area(s) in which the reconstruction indicates such a shift should also be specified, as there are some areas in which the Polar Front is topographically constrained and so not free to move (e.g. Drake Passage). [Robert Larter, United Kingdom]	Accepted. It is MARGO (2009) and is clarified.
5-868	5	19	13			Delete "the" after "variability and". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-869	5	19	13			Insert comma after "proxies". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-870	5	19	14	19	14	"About" --> "The" [Masa KAGEYAMA, France]	Accepted.
5-871	5	19	17			Insert "in the MARGO reconstruction" between "proxies" and "may" and remove "also"? [Sandra Passchier, United States of America]	Accepted.
5-872	5	19	18	19	18	shows --> show [Masa KAGEYAMA, France]	Accepted.
5-873	5	19	18			remove "-s" from "shows" [Sandra Passchier, United States of America]	Accepted.
5-874	5	19	18			Delete ", however, [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-875	5	19	18			Change "shows" to "show". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-876	5	19	20	19	30	Should the Shakun et al 2012 colling estimate from data alone be cited here? [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted. Reference added.
5-877	5	19	21	19	21	"that large uncertainty" --> "that a large uncertainty" ? [Masa KAGEYAMA, France]	Accepted.
5-878	5	19	22	19	22	Change 'show' to 'shows [Peter Burt, United Kingdom]	Accepted.
5-879	5	19	22	19	22	show --> shows [Masa KAGEYAMA, France]	Accepted.
5-880	5	19	22	19	22	Change "show" by "shows" [Yueh-Hsin Lo, Taiwan]	Accepted.
5-881	5	19	22			Change "show" to "shows". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-882	5	19	22			"synthesis…generally shows" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-883	5	19	23	19	23	Imprecise: explain the nature of the deviations [Peter Burt, United Kingdom]	Taken into accout: clarified (e.g., warming vs. cooling).
5-884	5	19	23			The authors should consider deleting: "so far unexplained," since Bartlein et al 2011 cites an explanation for Alaska that has been in the literature since Kutzbach 1978: " The Laurentide ice sheet was sufficiently large to cause a major reorganisation in the atmospheric circulation pattern. This reorganisation could have resulted in more southerly onshore flow into Alaska, which would have produced advective warming of the region year-	Accepted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						round."The authors should consider deleting the reference to LGM in Africa since the results for Africa could just as easily be an advection/circulation response as spurious, given the sparse pollen and climate data available for calibration noted by Bartlein et al 2011. [Government of United States of America]	
5-885	5	19	27			This sentence needs either an "a single" or "models" in the plural. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-886	5	19	27			Insert "a" after "using". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into accout: "single" is removed; "model" is changed by "models".
5-887	5	19	29			modified value is now 4.0+-0.8C and the reference is changed: Climate of the Past Discussions (or maybe CP, depending how quickly things move...). [James Annan, Japan]	Taken into account.
5-888	5	19	34	19	36	I think Masson-Delmotte et al (2005) was published in time for AR4 to raise this topic... (sorry I don't have time to check but I'm sure the lead authors will know...) [Masa KAGEYAMA, France]	Accepted. ”Since AR4” deleted.
5-889	5	19	39	19	40	The model results for Greenland are given here but the estimate from the ice core data is not given in the previous paragraph. It might be worth adding it somewhere. [Masa KAGEYAMA, France]	Accepted: it was mistakenly deleted during editting and will be corrected.
5-890	5	19	39	19	40	The model estimate of 7-15°C cooling is stated to be an underestimate, but the amount of cooling indicated by actual data that it is being compared to is not mentioned. [Robert Larter, United Kingdom]	Accepted: it was mistakenly deleted during editting and will be corrected.
5-891	5	19	39	19	41	Include the value of the cooling estimated from proxy data of 22-25 deg C (Dahl-Jensen, Science, 1998; Johnsen, Tellus, 1995; Cuffey, Science 1995) [European Union]	Accepted: it was mistakenly deleted during editting and will be corrected.
5-892	5	19	39			GISP2 Greenland core seems to show a cooling of about 15C (Alley 2000, as cited in Schmitter et al 2011), at the upper end (but not outside) the model range. Is there a disagreement in data interpretation here? [James Annan, Japan]	Noted. The difference with respect to borehole data may arise from corrections for changes in ice sheet elevation. Note that Alley (2000) only refers to the Younger Dryas, not the LGM-present day change.
5-893	5	19	39			Change "in LGM" to "during the LGM". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-894	5	19	40			Insert comma after "simulations". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-895	5	19	41	19	42	Without knowing what timing is used for the LGM discussion about the period is hard to pin down. [Government of Iceland]	Taken into account and explicitely introduced in Table 5.1
5-896	5	19	41	19	42	"peak cooling in Greenland does not represent the LGM period": Is this really possible given the accuracy of the dating in the ice core records ? [Masa KAGEYAMA, France]	Noted and ambiguous sentence deleted.
5-897	5	19	41	19	42	I don't understand the statement that the peak cooling in Greenland does not represnet the LGM, as we know exactly when the peak cooling in Greenland is, and we know what the cooling was in Greenland at 21 ka, so I am not sure what you are saying is uncertain. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted and ambiguous sentence deleted.
5-898	5	19	45	20	19	The firstly/secondly/thirdly language seems a little awkward so much detail. I wonder if 3 separate paragraphs might read better. The second and third paragraphs could start with "In the second approach," or something similar. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted: changed as suggested, but moved to a separate box.
5-899	5	19	47			CCS, is this term explained somewhere in the text? [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Taken into account: It is defined in Box 5.1, and is referred here.
5-900	5	19	50	19	55	As written now the reader gets the impression that Köhler et al., (2010) calculate the climate sensitivity from paleo data under the assumption that the response of the system would be the same for different climate states. This is what we exactly not propose in Köhler et al. (2010). I therefore suggest the following revision:"Firstly, climate sensitivity may be estimated by scaling temperature change in the past with the radiative forcing difference of the past and 2 × CO2. The results are subject to uncertainties in the estimate of global mean temperature based on proxy records of incomplete spatial coverage (Köhler et al., 2010; MARGO Project Members, 2009). In this approach, an important assumption is made that the climate response to a certain amount of radiative forcing is the same even under different types of forcing (e.g., CO2 or ice sheet).	Taken into account and statement clarified.

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						Modelling studies have shown, that climate sensitivity depends on the background climate state (e.g., warm or cold) probably due to the difference in cloud feedback, however the size and even the direction of this climate state dependency is largely model-dependent (Crucifix, 2006; Hargreaves et al., 2007; Yoshimori et al., 2011; Figure 5.4b). This climate state dependency of climate sensitivity was also derived in a compilation of climate sensitivity based on paleo data of the last 65 Myr, although its details are still not understood (PALAEOSENS Project Members, 2012)." [Peter Köhler, Germany]	
5-901	5	19	54			Climate responses in cold and warm states to the same forcing are quite different. This has been shown before by Hansen et al. and many others, and should be clearly refelcted in the discussion here. [Government of United States of America]	Taken into account: the focus here is not only the difference in warming and cooling, but also the different response to CO2 and ice sheet forcings. Due to the limited space, it is considered appropriate to cite papers focusing on the difference between CO2 and LGM forcings.
5-902	5	19	56	19	57	Cloud feedback is not shown in figure 5.4b [Government of United States of America]	Rejected: Shortwave and longwave cloud feedbacks (CSW and CLW) are shown separately in Fig. 5.5b.
5-903	5	19	57	19	57	[Government of Poland]	No comment is given here.
5-904	5	20	2			The correlation is more than weak-- it doesn't look like there is anything statistically significant. This should be clarified. [Government of United States of America]	Accepted: "weak" is replaced by "poor".
5-905	5	20	3		8	You can also attempt this method with the multi-model ensembles. See Hargreaves et al (GRL 2012 in press) [Julia Hargreaves, Japan]	Accepted: the paper is cited.
5-906	5	20	21		27	The whole paragraph about CCS from palaeoclimatic studies (1.4 to 6°C) does not add much knowledge, considering that a similar range of sensitivity (1.5 to 4.5°C) has been publicized over the last 20 years. The reliability of such palaeoclimatic estimates should be at least qualified in view of the major differences with respect to present climate and the difficulty of trusting models built upon "physics packages" derived under current climatic conditions. [Government of France]	Rejected: it is still of great value because they are independently estimated from instrumental records with newly synthesized proxy data and state-of-the-art GCMs, aided also by perturbed physics ensemble approach with EMICs and a GCM. It is a positive thing that the results are consistent with past studies. The goal of paleoclimate is not only narrowing the existing uncertainties but also supporting the independently aquired result from other studies.
5-907	5	20	21			Since CCS is clearly not inclusive of all feedbacks such as rapid ice melting, greenhouse gas changes, and vegetation feedbacks, one cannot give a robust estimate at this point especially with models. [Government of United States of America]	Rejected: By definition, ECS does not include rapid land ice melting, GHG changes, and vegetation changes as feedbacks. Please notice the difference from the Earth System Sensitivity (Box.5.1).
5-908	5	20	22			Change "lie very likely" to "very likely lie". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-909	5	20	30	20	55	Should you not be explicitly refering to CCS rather than just climate sensitivity here? [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-910	5	20	34	20	35	Abbreviations HadCM, MI3 and CM4 are used before they are defined (which occurs at the bottom of the caption). [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-911	5	20	51			Caption for Figure 5.5: On what were these temperature reconstructions based? Consider adding "from marine cores" if necessary? [Government of United States of America]	Accepted. Explanation added.
5-912	5	20	57	20	57	Widowed header, please check page layout in final version [Peter Burt, United Kingdom]	Noted.
5-913	5	21	0	21	0	The LIG section is unbalanced. There is a long history of studies of terrestrial archives, especially in Europe. : Tzedakis et al, (QSR 2006; EPSL 2009), Brewer et al (QSR 2008), Djamali et al (QR 2008) are ignored here. Can we assess regional climatic changes with only ice and marine cores? [Government of France]	Taken into account. Text revised to include and assess studies that include quantitative estimates of temperature change. Space limits a more extensive review.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-914	5	21	1	21	1	PAST interglacial climate ALLOWS [Government of Australia]	Accepted. Revised to 'Interglacials allow'.
5-915	5	21	1	21	1	Interglacial climates allow… [Matthew Konfirst, United States of America]	Accepted. Revised to 'Interglacials allow'.
5-916	5	21	1	21	1	to be studied from the perspective of natural climate variability [Matthew Konfirst, United States of America]	Editorial.
5-917	5	21	1	21	3	This sentence is unclear and the authors should consider rewriting. [Government of United States of America]	Taken into account in the revised version.
5-918	5	21	1	21	46	The whole LIG section will rather gain to present regional SAT and SST than just global mean annual temperatures. The regional signature was much well expressed in the LGM section. It will also gain from a robust explanation why the tropics were 1°C cooler (fig 5.6). [Government of France]	Taken into account. Text revised to include broad scale regional patterns of temperature change. More information on specifics of orbital changes in solar insolation, latitudinally and seasonally now included.
5-919	5	21	1	22	7	Very poor English in this section. It really needs editing by a native speaker. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Editorial.
5-920	5	21	1			interglacial climate should be plural. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted. Revised to 'Interglacials allow'.
5-921	5	21	1			Change "climate" to "climates", and "in the" to "from a". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account in the revised version.
5-922	5	21	2			"allow … understand feedbacks" is not correct. Try "allow … increased understanding of feedbacks" [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Editorial.
5-923	5	21	2			This first sentence is gramatically inconsistent. At least, it needs "to" inserted after "and". But it then still remains grammatically poor and would benefit from editing completely. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Editorial.
5-924	5	21	3			The current interglacial, the Holocene, is defined as extending from 1.7 ka to the present (in accordance to the Subcommission on Quaternary Stratigraphy). Also in page 5-51, line 43. However the terms "Holocene" (page AIII-14) and "Pleistocene" (page AIII-21) included in Annex III: Glossary inform that the Holocene starts in 11.6 ka [Alejandro Cearreta, Spain]	Taken into account in the revised version.
5-925	5	21	5	21	5	"Last Interglaciation": Refer here to the Glossary to alert readers that throughout the whole report this expression and "Last Interglacial" are used interchangeably. [Manfred Mudelsee, Germany]	Taken into account. Last Interglacial is used consistently throughout report.
5-926	5	21	5	21	5	the reference here should not be Stirling et al 1998, rather it should be the LIG sea-level section in the paleo chapter [Mark Siddall, United Kingdom]	Taken into account. Stirling refernce replaced by reference to Table 5.1.
5-927	5	21	5	21	18	An important characteristic of LIG is its spatial heterogeneity, such as Medieval climate anomaly or mid-Holocene. This could be mentionned. [Government of France]	Taken into account. Text revised to include broad scale regional patterns of temperature change. More information on specifics of orbital changes in solar insolation, latitudinally and seasonally now included.
5-928	5	21	5	21	46	AS A GENERAL COMMMENT: The referred section includes a excesive number of “submited” references. Should be convenient to include (at this time) only published or at least 2submited”; or to limit the number. [José-Abel Flores, Spain]	Taken into account. All references are now in press or published.
5-929	5	21	5			I don't know the Stirling et al. 1998 reference, but the implication of its placing here is that they discovered the last interglacial which I doubt. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Stirling refernce replaced by reference to Table 5.1.
5-930	5	21	6	21	6	Change 'concentration' to 'concentrations' [Peter Burt, United Kingdom]	Editorial.
5-931	5	21	6	21	7	I don't understand this statement. It is too vague for me to know ehether it really means that the mechanisms were stronger sometime in the last 2.8 Ma (and if so when) or just that one region was warmer than others. Please re-word to make the point clearer. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Sentence deleted.
5-932	5	21	6			“It experienced stronger orbital forcing”: orbital forcing varies spatially and temporally. This statement is confusing. Is summer insolation higher? Are latitudinal gradients seasonally higher? The text would benefit by being more specific. [Government of United States of America]	Taken into account. Text revised to include more information on specifics of orbital changes in solar insolation.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-933	5	21	6			The text would be more clearit clarified: concentrations, or a greenhouse concentration [Government of United States of America]	Editorial.
5-934	5	21	6			Change "concentration" to "concentrations". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Editorial.
5-935	5	21	8	21	18	the NH (in line 8), the SH (in line 9), Northern Hemisphere (in line 16) and NH ice sheets; in all these places, it is better to maintain an uniformity. [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Editorial.
5-936	5	21	9	21	10	allow and assumed verbs in these two sentences are present and past respectively. They should be the same tense. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Editorial.
5-937	5	21	9			"estimating" to "estimates of" [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Editorial.
5-938	5	21	9			Change "estimating" to "estimation of". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Editorial.
5-939	5	21	9			"allow estimation of" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Editorial.
5-940	5	21	11	21	13	The Southern Hemisphere "lead" is well-documented in Sea-surface temperature reconstructions going all the way back to Hays et al. 1976 and confirmed by other reconstructions such as Howard and Prell 1992, and more recently reconstructions such as Cortese et al. and Schneider-Mor et al. 2012. Cortese, G., A. Abelmann, and R. Gersonde (2007), The last five glacial-interglacial transitions: A high-resolution 450,000-year record from the subantarctic Atlantic, Paleoceanography, 22, PA4203, doi:10.1029/2007PA001457. Hays, J. D., J. Imbrie, and N. J. Shackleton (1976), Variations in the Earth's orbit: Pacemaker of the ice ages, Science, 194, 1121-1132 Howard, W. R., and W. L. Prell (1992), Late Quaternary surface circulation of the Southern Indian Ocean and its relationship to orbital variations, Paleoceanography, 7(1), 79-118, doi:10.1029/91PA02994. Schneider Mor, A., R. Yam, C. Bianchi, M. Kunz-Pirrung, R. Gersonde, and A. Shemesh (2012), Variable sequence of events during the past seven terminations in two deep-sea cores from the Southern Ocean, Quat. Res., 77(2), 317-325, doi:10.1016/j.yqres.2011.11.006. [William Howard, Australia]	Taken into account. Appropriate recent references cited.
5-941	5	21	12			Cite NEEM project members (in press) here (later cited as Dahl-Jensen et al) [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted. Citation added.
5-942	5	21	13	21	13	"If available, uncertainties..." is a little odd, does it mean "Uncertainties of reconstructed climate parameters, where theese are available, ..." [Tasman van Ommen, Australia]	Taken into account in the revised version.
5-943	5	21	13			Change "If available" to "Where reported". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account in the revised version.
5-944	5	21	16			"peak NH warmth early in the LIG": this is confusing when you have just said that Antarctica led the north, implying that Antarctica must have warmed even earlier than "early in the LIG". [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text revised to clarify.
5-945	5	21	17			Insert comma after ", submitted)". Also, insert "a" afeter "with", and "that" after "evolution". Change "dependent on the assumptions of NH…" to "depend on assumptions about NH…". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Editorial.
5-946	5	21	21	21	22	What is the purpose of this sentence ? Is this sentence related to the previous one or the next one? [Government of France]	Accepted. Sentence deleted.
5-947	5	21	22	21	22	Warming relative to present day? And what is the relevance to the ice sheet stability (as stated in the opening	Accepted. Sentence deleted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						sentence)? [Government of Australia]	
5-948	5	21	25			Change "Antarctica" to "Antarctic". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Editorial.
5-949	5	21	26			It is worth mentioning that these GCM experiments did NOT include topographic changes, just orbital and ghg. [Government of United States of America]	Accepted. Text revised.
5-950	5	21	26			Change "..., possibly more than 5deg.C warmer than present" to "..to possbly more than 5deg.C above the present". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Editorial.
5-951	5	21	28			Lunt, submitted actual author list is D. J. Lunt, A. Abe-Ouchi, P. Bakker, A. Berger, P. Braconnot, S. Charbit, N. Fischer, N. Herold, J. H. Jungclaus, V. C. Khon, U. Krebs-Kanzow, G. Lohmann, B. Otto-Bliesner, W. Park, M. Pfeiffer, M. Prange, R. Rachmayani, H. Renssen, N. Rosenbloom, B. Schneider, E. J. Stone, K. Takahashi, W. Wei, and Q. Yin [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Editorial..
5-952	5	21	28			Looking at Fig. 5.6 and the Lunt et al paper it is hard to see that the discrepancy in warming is especially larger over Antarctica than the Arctic. I wonder if you want to drop that part of the sentence. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-953	5	21	31			Change "needed for" to "needed to explain". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Editorial.
5-954	5	21	31			"needed to explain the LIG.." [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Editorial.
5-955	5	21	34			The "changes" mentionned here and further down are confusing. Change with respect to what (the current interglacial or something else)? Specify. [Government of France]	Accepted. Text revised to clarify.
5-956	5	21	35	21	35	Text missing after 'pre-industrial [Peter Burt, United Kingdom]	Rejected. No text missing.
5-957	5	21	35	21	35	When was the pre-industrial? Is this the second half of the 19th century (1850-1899) that AR4 used? Does this also mean that we are 0.8 deg C above this now? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted. Pre-industrial is now defined in Table 5.1.
5-958	5	21	36	21	37	We assume what is meant is: PMIP model simulations do not yield coherent [...] temperature changes. In such a statement, a clear null hypothesis should be formulated and then rejected (or one should explain why it cannot be rejected). [Government of France]	Taken into account in the revised version.
5-959	5	21	38	21	46	The comparison on line 41 is relative to what and where was this estimated? The numbers in this section need more justification. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account in the revised version.
5-960	5	21	38	21	46	This should also point out the we are comparing model timeslices with data that are at the peak of a particular proxy. The real climate was never globally as warm as the peak data synthesis implies. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Taken into account in the revised version.
5-961	5	21	43	21	45	LIG is not homogeneous: this 1.5°C warming is it a mean, maximum, regional, global? [Government of France]	Taken into account in the revised version.
5-962	5	21	44	21	46	This sentence is a great insight into the challenges of comparing model simulations to data. "Model results give terrestrial warming of 0.9°C–1.1°C when sampled at the data locations but only 0°C–0.3°C when averaged over the full model grids, pointing to difficulties in estimating global mean annual surface temperature with current data coverage (Otto-Bliesner et al., submitted)." The interesting point missing from the rest of the chapter is that this sampling issue is a persistent challenge for any and all data model comparisons, not just that LIG but also the EECO, MPWP, LGM, MH, MCA, LIA, ... [Government of United States of America]	Noted.
5-963	5	21	48	22	7	I miss again (as in AR4, Jansen et al., 2007) some quantitative estimate, even if it is only very rough, about Arctic sea ice extent. In the context of species survival, e.g. polar bears, this would be most valuable information for WGII. Please provide some information on this and/or pointing at reference(s) where quantitative estimates could be found. Since Bette is even now an author, I hope this is doable.Thanks! [Andreas Fischlin, Switzerland]	Noted. Quantitative estimates are not available.

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5-964	5	21	48	22	7	This section does not do justice to MIS 11 literature, a critical interglacial period. A paragraph on the topic does exist near end of chapter, but a discussion here may also be warranted. [Government of United States of America]	Rejected. Due to space limitations MIS 11 is no longer included in Section 5.6 on sea level.
5-965	5	21	50			Why is the age control sufficient to discern geographical patterns but not temporal phasing? [Sandra Passchier, United States of America]	Accepted. Phrase deleted.
5-966	5	21	51	21	52	"There is currently no consensus on whether interglacials changed in strength after the mid-Brunhes event ~430 ka." In this discussion, the role of long-term changes of 105yrs time scale is ignored. There is clear evidence for such long cycles in the records of oceanic carbon isotope and carbonate, and in paleo- monsoon records (e.g., Wang et al., 2004, Paleoceanography, 19, PA4005). Sure, it is still too early to use the long-term cycles in climate prediction, but this aspect in paleoclimate studies should not be ignored in scientific synthesis and in climate modeling because of its potential to explain many unanswered questions raised from the observations. [PINXIAN WANG, China]	Noted. Due to space limitations we only cite references dicussing records pertaining to climate change. Carbon and other biogeochemical cycles are the subject of WG1 Chapter 6.
5-967	5	21	52	21	52	Please explain 'mid Brunhes event' (if not done already) [Government of Australia]	Taken into account. Term MBE is replaced by timing of event.
5-968	5	21	52	21	52	Brief explanation of mid-Bruhnes event required [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account. Term MBE is replaced by timing of event.
5-969	5	21				Missing here is information regarding evidence for vegetational feedbacks in the LIG such as forest movement northward which would affect albedo as well as the carbon cycle. [Government of United States of America]	Rejected. No new information since the AR4.
5-970	5	22	6	22	7	This last sentence is an afterthought that requires proper explanation. As it is, it is in an odd place, and does not add any information that is relevant to the rest of the paragraph. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Sentence deleted.
5-971	5	22	13	22	13	0 ka is poor style. Why not say 'present' [Peter Burt, United Kingdom]	Taken in to account. Text revised to last 5 kyrs.
5-972	5	22	17			What is plotted in the top right panel is clearly not zonal means, but individual data points. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text corrected.
5-973	5	22	23			"with respect to the pre-" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Editorial.
5-974	5	22	24	22	24	Space required before 'ka' [Peter Burt, United Kingdom]	Editorial.
5-975	5	22	31	22	31	There is a lot of independent evidence from the concentrations of CO2 (eg Etheridge et al.), 13CO2 (eg. Trudinger et al.) CH4 and 13CH4 (eg. Ferretti et al., Mitchell et al.) and some other trace gas species such as COS (Aydin et al.) for globally-extensive changes in climate (temp and precip), many of which confirm the proxy temperatures of the past 2000 years. [Government of Australia]	Noted but the support for quantitative climate changes at global-scale is indirect and therefore weaker. We limit our revision to noting the compatibility between these records and the NH/global temperature reconstructions.
5-976	5	22	31	22	31	The chapter on the 2ka appears suddenly after the glacier-interglacial text. I would say a better place should be found for this portion of the text. [Olga Solomina, Russia]	Noted. Section 5.3.5 has stayed in the same position within the Chapter. The rationale is that it links well with 5.3.4 and introduces the more regional focus of the following section 5.4 and 5.5. The introductory paragraph has been rephrased and should make a better case for its present location.
5-977	5	22	31	22	38	The section on temperature variations of the past 2000 years gives the impression that very little concrete evidence is available. It seems to concentrate on the uncertainties. However, there are quantitative conclusions that can be drawn from this period that need to be presented more clearly. At the same time, the section ignores the prominent challenges such as the question of the hockey stick that question the robustness of the reconstructions and the scientific responses that have confirmed the general trends over this period. [Government of Australia]	Taken into account. The section has been revised trying to be more quantitative, in particular in the magnitude of present warming with last millennium temperatures at hemispheric scales. The subsection of uncertainties is essential to reflect recent advances since AR4 and because the assessment cannot be made without considering both the published uncertainties and the fact that there are additional

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							sources of uncertainty that are not included in the published uncertainties. Methodological issues related to the 'hockey stick' discussion and other controversies have been covered there to the extent considered appropriate in terms of their scientific dimension and allowed by space constraints.
5-978	5	22	31	24	56	There is an enormous pile of peer-reviewed literature on what is popularly called "the hockey stick controversy". I cannot find any of these. E.g., National Research Council: "Surface temperature reconstructions for the last 2000 years", The National Academies Press, 2006. Since many readers will know this controversy, it is important to address these issues. [Hans Visser, The Netherlands]	See 5-977, bu also note that the NAS report was published before AR4 and was cited there.
5-979	5	22	31	28	7	Section 5.3.5: While I like very much the focus on assessing new findings (since AR4) of this chapter in general, I suggest you add somewhere in this chapter a small discussion on things left out in AR4. In particular Mangini et al. (2005) was not discussed in AR4. Since IPCC risks to be blamed to have on purpose not discussed this, I suggest you do now correct for this omission and discuss somehwere these works (perhaps also some other works too, see list of references below, 2nd list). Since Mangini et al. (2005) is often cited by deniers, you could alternatively also do this in FAQ 5.3 (the one I have suggested to add). Cited References: ---------- Mangini, A., Spotl, C., & Verdes, P., 2005. Reconstruction of temperature in the Central Alps during the past 2000 yr from a delta O-18 stalagmite record. Earth Planet. Sci. Lett., 235(3-4): 741-751. http://dx.doi.org/10.1016/j.epsl.2005.05.010 Ma344 --- optional references Braun, H., Christl, M., Rahmstorf, S., Ganopolski, A., Mangini, A., Kubatzki, C., Roth, K., & Kromer, B., 2005. Possible solar origin of the 1,470-year glacial climate cycle demonstrated in a coupled model. Nature, 438(7065): 208-211. http://dx.doi.org/10.1038/nature04121 Br168 Frisia, S., Borsato, A., Mangini, A., Spotl, C., Madonia, G., & Sauro, U., 2006. Holocene climate variability in Sicily from a discontinuous stalagmite record and the Mesolithic to Neolithic transition. Quaternary Res., 66(3): 388-400. http://dx.doi.org/10.1016/j.yqres.2006.05.003 Fr066 Holzkamper, S., Spotl, C., & Mangini, A., 2005. High-precision constraints on timing of Alpine warm periods during the middle to late Pleistocene using speleothem growth periods. Earth Planet. Sci. Lett., 236(3-4): 751-764. http://dx.doi.org/10.1016/j.epsl.2005.06.002 Ho185 Spotl, C. & Mangini, A., 2007. Speleothems and paleoglaciers. Earth Planet. Sci. Lett., 254(3-4): 323-331. http://dx.doi.org/10.1016/j.epsl.2006.11.041 Sp026 Vollweiler, N., Scholz, D., Muhlinghaus, C., Mangini, A., & Spotl, C., 2006. A precisely dated climate record for the last 9 kyr from three high alpine stalagmites, Spannagel Cave, Austria. Geophys. Res. Lett., 33(20): L20703. http://dx.doi.org/10.1029/2006GL027662 Vo045 [Andreas Fischlin, Switzerland]	Rejected, due to space limitations.
5-980	5	22	31	34	15	It might be helpful to have a general discussion of the strengths and weaknesses of the different tree ring analysis methods at the beginning of this section, including some combination of the existing text and the points I have raised in the entries above. [Jim Bouldin, United States of America]	Taken into account within the limited space available. This is not a text book about proxy pros and cons but we try to bear in mind proxy limitations when assessing the large-scale recons that are developed from them. This has been considered for the revised version of 5.3.5.2 taking into account the relevance of tree ring proxies in hemispherical reconstructions and

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							their influence on uncertainties within this context, but note that other proxies have their own limitations and we are not writing a text book.
5-981	5	22	31			I believe the authors of this section have done a good job of sifting through and assimilating the published literature and, with some limitations, presenting an informative review of the current understanding. This is a valuable and largely comprehensive review of the current availability of recently published palaeoclimate reconstruction work, though perhaps more specific “assessment” of this work might have been presented. The availability and presentation of the new model simulations of hemispheric and, of course regional temperature changes, represents a major advance over the situation described in AR4, and this information is very clearly presented. However, I think more attention could have been given to maintaining continuity with the results and conclusions of the equivalent section in the AR4. The change in the way “uncertainty” is described here implies some disagreement with the conclusion of AR4 but my reading of the older and new evidence (reconstruction, methodology and simulations) is not inconsistent with AR4. That is not to say that there are not different or new points of interpretation and discussion that arise out of the updated reconstruction/simulation comparison (I make specific remarks on this in appropriate places), but there is an impression of “disconnect” between much of AR4 evidence and discussion that is presented here. A clearer summary of how the current IPCC position as regards precedence of 20th/21st century temperatures compared to those of the past, and the AR4 position would be welcome. I fully appreciate the very severe space limitations imposed on IPCC authors, but I really wonder whether additional space could be allotted to allow some description of the likely strengths/weaknesses of particular “new” reconstructions used in Figure 5.8, and perhaps some discussion of the reasons why certain reconstructions that were used in AR4, are not used in AR5 while some are. I provide some specific comments in the appropriate places. I also have some misgivings above the rather sharp focus placed on the so-called “Medieval Climate Anomaly” (MCA), and its comparison with, in my opinion, a somewhat loosely defined “Little Ice Age”. It is very much a personal opinion (and no doubt the issue has been carefully considered by the authors of this section) but the switch from the AR4 references (and description of) the Medieval Warm period to the even less well defined MCA, and the subsequent emphasis on the presentation of results in relation to it, does little to clarify our understanding of the nature of past temperature (or moisture-related) changes, or their causes. Sorry, it is very late in the process to make a remark like this but I thought I’d air it anyway in case it stimulates some further consideration of whether the text should discuss concepts (MWP v MCA) further – perhaps in a Box like Box in 6.4 in AR4. [Keith Briffa, United Kingdom]	Noted and taken into account where possible. See 5-236 regarding likelihood/confidence language. We also now note consistency with AR4 in the direction suggested by this comment. The treatment of MCA/MWP and LIA periods has been discussed in depth within group meetings trying to address this and other related comments. A new table is included in the chapter highlighting the extension of relevant periods like this to aid the reader through the text. The definitions of these two last millennium periods have been revised within this new version and based on the evidence of data presented in the chapter. The naming convention for the MCA/MWP was decided on the basis of accounting for a more up to date treatment that also incorporates non-temperature related changes. We are aware that this is a controversial issue and that a definition and use that would receive overall agreement is difficult to achieve. We tried to keep consistent with the use and definitions made within this chapter and relevant cited literature and the new version of the text tries to reflect on this. Also the definitions provided in the glossary try to account for the diffficulties of assigning names and time intervals to the concepts of MCA/MWP and LIA. We also reduce the use of such terms where it is possible to do so, by referring to time periods by ranges of years.
5-982	5	22	33	22	33	why is the last 2000 yr period important? [Government of Australia]	Noted. No space to provide detail discussion, but the introductory paragraph to 5.3.5 identifies some key features that make this an important period to assess.
5-983	5	22	33	22	34	In addition to the study of the response to the forcing, the past 2000 years is also a key period to study the internal variability of the system at multidecadal to centennial timescales. [Hugues Goosse, Belgium]	Accepted.
5-984	5	22	33	22	34	The first sentence of this paragraph is not really useful, I think. [Masa KAGEYAMA, France]	Accepted, paragraph revised.
5-985	5	22	33	34		this sentence is not appropriate here; the previous part is already about hemispheric and global temperatures [Elie Verleyen, Belgium]	Accepted, paragraph revised.
5-986	5	22	42	22	50	I could have said this earlier, but am mentioning it here. In this para there is no need to put CE after every year. The only reason that CE or AD are added to years is to avoid confusion with BC years. There is no need to put CE down for any years in this section as it is about the last 2000 years, so anything prior to that is ruled out. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-987	5	22	43	22	43	Isn't it appendix 5.A.2 instead of appendix 5.A.1? [Hugues Goosse, Belgium]	Accepted
5-988	5	22	43	22	46	Why is the MCA "so-called" and LIA not? The authors should consider re-writing the sentence to read: "This new evidence, particularly in the NH, confirms the existence of a generally warm period during the Medieval Climate Anomaly (MCA; Glossary), followed by a colder Little Ice Age (LIA; Glossary) that lasted from the	Accepted. The text has been changed taking this suggestion into account

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						middle centuries of the millennium to the rise in global temperatures that began in the late 19th century." [Government of United States of America]	
5-989	5	22	44	22	44	Better to use Medieval Warm Period (MWP) instead of Medieval Climate Anomaly. [Marcel Crok, The Netherlands]	The naming convention for the MCA/MWP was decided on the basis of accounting for a more up to date treatment that also incorporates non-temperature related changes. We are aware that this is a controversial issue and that a definition and use that would receive overall agreement is difficult to achieve. We tried to keep consistent with the use and definitions made within this chapter and relevant cited literature and the new version of the text tries to reflect on this. Also the definitions provided in the glossary try to account for the diffficulties of assigning names and time intervals to the concepts of MCA/MWP and LIA.
5-990	5	22	44	22	45	no Glossary is included in the Chapter. Make mode detailed reference where it is included [Government of Poland]	Taken into account, Table 5.1 has been introduced.
5-991	5	22	44	22	50	The sudden use of the term Medieval Climate Anomaly (MCA) rather than Medieval Warm Period (as in AR4) comes as a shock, in the sense that it just appears without any clear explanation of why it has been preferred. Equally strong is the apparently arbitrary definition adopted for the timing of the Little Ice Age (LIA). The perfunctory definitions in the Glossary do not provide sufficient discussion of these concepts. There is not sufficient justification for using the adopted timing of MCA (950-1250 CE; not supported by 5.8(a)) rather than the MWP (950-100 suggested as in AR4 (see Box 6.4) and apparently supported by the NH data in figure 5.8(a)). This choice is critical for defining the information shown in Figures 5.8(e) and (f) and in Figure 5.9. The current understanding of the use of MCA is that it implies a wide-scale relevant period in climate history that somehow involves hydrologic evidence will be shown to be spatially and temporally conflicting and so moving away from the use of MWP is not helpful. Similarly, on this evidence, defining the LIA as 1400-1700 CE seems strange. [Keith Briffa, United Kingdom]	See answer to 5-981
5-992	5	22	46	22	46	When did global temperatures rise in the 19th century? Why do you use this to determine the end of the LIA? The 18th century from proxy data and also from European instrumental temperatures. Why did the LIA not end then? Naming periods is the whole problem here. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	See 5-1133 and 5-981. We will reduce use of named periods when possible.
5-993	5	22	46	22	46	Structure' should be some other word such as 'detail'. The next sentence is also quite odd. You seem to have a number of definitions for the LIA and MCA. Earlier in this para you said the LIA ended in the mid-19th century, now you have it ending in 1700! [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted and text revised taking both comments into account.
5-994	5	22	47	22	47	This significant statement concerning the character of the Medieval Climate Anomaly and Little Ice Age should be supported by additional references to recent research. It does not look that good to refer to the last IPCC report. Some of the conclusions about a heterogeneous Medieval Climate Anomaly in the last IPCC report have been questioned by Esper and Frank (2009). It would be fair to also refer to this article with some comment. The full reference to this article is: Esper, J. and Frank, D.C.: IPCC on heterogeneous Medieval Warm Period, Clim. Change, 94, 267–273, 2009. [Fredrik Ljungqvist, Sweden]	Accepted. The text has been changed taking this suggestion into account sp that it is clearer what is meant and refers to supporting material. Esper and Frank (2009) is not cited because it is based on a misinterpretation of the AR4 conclusion.
5-995	5	22	47			Actually MCA temperatures are well defined and reconstructed; they are just complex. Therefore, it is suggested that the authors consider revising the text so it does not suggest that the community expects a simple synchronous MCA warm period. Later in the discussion of the analyses of MCA-LIA-modern, only annual or decadal resolved records are used; this limits the assessment of multidecadal and centennial-scale variability critical to detection and attribution. [Government of United States of America]	Accepted. The text has been changed taking this suggestion into account to make clear that they are complex rather than ill defined. Also Fig. 5.12 revised to indicate timings of peak reconstructed warmth.
5-996	5	22	48	22	49	Why were the time intervals of the MCA and LIA assigned to 950–1250 CE and 1400–1700 CE? Both are obviously inconsistent with reconstructions shown in Figure 5.7. [Lei Huang, China]	The definitions for MCA and LIA extension have been changed in the revised version of 5.3.5 and consistency ensured.

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5-997	5	22	48	22	49	The definition of time period of Medieval Climate Anomaly (MCA) and Little Ice Age lack in Figure 5.8 and the text attached is not supported by the information containing in Figure 5.7 (Line 48-49 of Page 22). In addition, the expressions at Line 40 of Page 4 and Line 44 of Page 22 are not the same. It is recommended to keep them consistent. [Lei Huang, China]	See 5-996
5-998	5	22	49	22	49	after “LIA” insert “respectively”. [Keith Briffa, United Kingdom]	Taken into account in the revised version.
5-999	5	22	49			The time interval 1400-1700 CE is defined as representative of the LIA. Also in pages 5-27, lines 46 and 55, and 5-91, line 55. However, the Annex III: Glossary, under ther term "Little Ige Age (LIA)" (page AIII-17), informs that LIA represents the period 1400 CE and 1900 CE. [Alejandro Cearreta, Spain]	See 5-996
5-1000	5	22	52	22	52	replace "error estimates" with "uncertainty estimates". [Government of Poland]	Accepted.
5-1001	5	22	52	22	53	Moberg et al. (2005), D’Arrigo et al. (2006) and Hegerl et al. (2006 – as 2007) are carried forward from AR4 and Esper et al. (2002) and Jones (2001) are not. The fact that some earlier (AR4) reconstructions have been superceded is clear but some explicit statement of the fact should be made and some indication given of why those series not superceded have been omitted. I make a relevant comment on “new” reconstructions with regard to Figure 5.7(a). [Keith Briffa, United Kingdom]	Taken into account. Appendix now says "those that have been superseded by a related study using an expanded proxy dataset and/or updated statistical methods were excluded".
5-1002	5	22	52	23	2	In Table 5.1, it would be nice to stipulate what data are used to assess last ~1000 years, vis-à-vis land vs ocean records. The vast majority are land records. [Government of United States of America]	Taken into account. The land versus ocean character of the assessment is clearly included in the table and more details are given in Table 5.A.6 including a qualitative indication of the number of ocean records.
5-1003	5	22	57	22	57	Another issue is whether they include low-frequency proxies, whose validation is questionable. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Short note about lower-res proxies included in 5.3.5.2
5-1004	5	22	58	23	2	It's not reasonable to compare the reconstruction with instrumental record directly, because the reconstruction always underestimate the variance compare to the instrumental record. [Jingyun Zheng, China]	See 5-1025
5-1005	5	22				This piece does not do justice to LIG including MIS 5c, 5a parts of the interglacial in the Arctic [Government of United States of America]	Taken into account. Because of space considerations, we only consider the warmest interval of Stage 5.
5-1006	5	23	1	23	1	It is claimed that “almost all reconstructions” have small temperature fluctuations, but the results of CL2012 show that older (i.e., almost all) reconstructions have a problem of underestimating long cycle variations. I wonder if the author of this part seriously read the results of CL2012. [Kiminori Itoh, Japan]	Rejected. There have been follow-up papers about CL2012, which are cited, suggesting that their results must be treated with care to avoid overinterpreting them. 5.3.5.2 covers these issues. It also does not stated what the reviewer claims.
5-1007	5	23	4	23	5	What does this sentence mean? 800 is 1200 years or more ago? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted. The text has been revised making this more clear.
5-1008	5	23	4	23	15	This paragraph meanders a bit, and it is difficult to discern what the major point really is (and isn't). Expansion/clarity is warranted. [Government of United States of America]	Taken into account. Expanding this part was limited by space constraints but the text has been revised to make it clearer. The comments provided to this section have been of much help.
5-1009	5	23	6	23	9	If some proxies don't fully resolve 30-50year timescale variations, then should you be using them? Also if proxies respond on this timescale, a case can be made that they are integrating over time, but also there could be a lag in their response. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Partly accepted. We have dropped the LM2008 and the borehole data from the 30-year section of the Table and we have a sentence about lower-resolution problems, but note that we aren't using these proxies but are assessing multi-proxy reconstructions which have, in some cases, mixed lower-resolution and higher-resolution proxies and thus we cannot separate them in our assessment.
5-1010	5	23	6	23	9	"for which it is ambiguous" - also an ambiguous sentence - make it clear what is meant. [Janice Lough, Australia]	Accepted. The text has been modfied accordingly.

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5-1011	5	23	12	23	14	not sure about the "medium confidence" Except for CL2012, all curves show a Medieval Warm Period; less than present but above the LIA. Only exceptions are the tree-ring only, RCS reconstrructions, but then, tree-rings underestimate medium frequency variability. So either some regions are really warm, to account for those cooling, or most are warmer? The maps below also show it nearly as extensive as the LIA. Are there really sufficient records (aside from tree-rings) to make this conclusion at this point? There are still large gaps in the proxy records (see Flg 5.9; also following on the discussion of next section), this may be premature? [Konrad Gajewski, Canada]	Taken into account. This paragraph has been revised to improve clarity and to provide better support for the statements, including reference to Fig 5.12 where the synchronicity of recent warming is compared to decadal maxima in the MCA and to Kaufman et al (2013).
5-1012	5	23	12	23	15	This sentence is a summary point in the ES, but it doesn't seem that well justified here. It is obviously true, as most evidence for Medieval warmth is based on summer and from Europe and North America, but will some say it is just because we have yet to find the evidence? [European Union]	See 5-1011
5-1013	5	23	12	23	15	"Nevertheless, evidence for warmth in recent decades (Chapter 2) is more seasonally and geographically extensive than the evidence for Medieval warmth, providing medium confidence that 1961–2010 CE and 1981–2010 CE were the warmest 50- and 30-year periods in the last 1300 years." This statement, especially about the geographic extent of recent, needs to be tempered with the caveat that we have limited spatial coverage for the Medieval period. Need to be mindful of the old saying "absence of evidence is not evidence of absence." The passages about the MCA are likely to recieve a lot of scrutiny from the media and critics so caution and extra care in rigour of discussion is called for! [William Howard, Australia]	See 5-1011
5-1014	5	23	12	23	15	This sentence is a summary point in the ES, but it doesn't seem that well justified here. It is obviously true, as most evidence for Medieval warmth is based on summer and from Europe and North America, but will some say it is just because we have yet to find the evidence? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	See 5-1011
5-1015	5	23	13	23	13	Could this simply be because there are more and better observations in the modern record? [Government of Australia]	See 5-1011
5-1016	5	23	13	23	15	I am not sure what the conclusion stated in this sentence is based on. There has been no explicit discussion of similarities or differences in seasonal patterns of medieval warmth? Presumably the reference to “geographically extensive” is based on Figure 5.9(f). [Keith Briffa, United Kingdom]	See 5-1011
5-1017	5	23	14	23	14	These is absolutely no need to put CE after 1961-2010 and 1981-2010. They cannot be confused with BC years!!! CE is not in Table 5.1. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-1018	5	23	14	23	15	Does this statement represent a different level of confidence than that expressed in AR4? If so why and if not say so? [Keith Briffa, United Kingdom]	The text has been revised taking this into consideration. See answer to previous comment 5-981.
5-1019	5	23	14			medieval, lower case [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Editorial - copy edit to be completed before publication.
5-1020	5	23	17	23	18	See Gergis et al., submitted to Journal of Climate for a SH reconstruction [Government of Australia]	Noted.
5-1021	5	23	17			Replace “are" by “is". (There is grammatical confusion throughout the text about this. When talking about a number, you use plural, and when talking about for example a combination, you use singular. The correct approach is to use singular forms of the verb, because the subject is singular, or to decide to use an implicit plural and to then be consistent throughout.) [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Editorial. Copy edit to be completed before publication.
5-1022	5	23	19	23	21	The following sentence (and possibly others) is contradicted by the TS citation in the previous row above (TS, 4): "These few indicate that the last decades are relatively warm for the SH and at global scales (Table 19 5.1), but there is only limited evidence and therefore low confidence that the recent warming has exceeded the range of reconstructed temperatures over the last 4 centuries." [Forrest Mims, United States of America]	Noted but no contradiction apparent to the authors.
5-1023	5	23	21			Should "centuries" be replaced with "decades"? [Sandra Passchier, United States of America]	Taken into account. Centuries was correct. The text has been modified trying to make this more clear.
5-1024	5	23	24	23	24	Citations would be useful in this caption [Government of Australia]	Rejected. A reference is made to the Appendix where

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							details for each reconstruction are provided. Providing a citation for each reconstruction would lengthen the size of the caption unnecesarily.
5-1025	5	23	24			Fig. 5.7. it is still problematic to compare reconstructed series with observed reference periods, because reconstruction attenuates the variance. It is an important limitation and some reconstructions are scaled to reproduce the reference period variance. So we agree that any period between 1350 and 1900 are very likely colder than the last 30 years, but we are not so sure that it remains true for the MCA. Proposal : the main striking fact for the MCA is its heterogeneity. This is told in the summary and it is true also previous warm periods [Government of France]	Noted. But the table does attempt to address this by also comparing modern reconstruction values with earlier reconstruction values, thus addressing this issue of comparing different data (instrumental versus reconstruction). The Table also compares instrumental values with reconstructed values, but take into account the uncertainty ranges on the latter which will at least partly account for any attenuation of variance. Also, note that the text does not claim what the reviewers says: it does not say that the MCA was "very likely" colder than last 30 years, but "medium confidence" that it was "likely" colder. 5.3.5.2 addresses the issue of variance underestimation. Figure 7 includes instrumental series but also the reconstructions during the instrumental period.
5-1026	5	23	24			The Fig 5.7 show a similarity between SH and NH reconstructions. It may be due to the lack of proxies in SH used by Mann et al (2008). This part appears somewhat weak. [Government of France]	Noted. The conclusions are as strong as warranted by the limited data. The lack of proxy data in the SH is pointed out in 5.3.5.1. See also 5.5.1 related to this issue.
5-1027	5	23	25	23	25	Figure 5.7: Appendix 5.A.1 gives Supplemental Information for Figure 5.8, not for Figure 5.7 [Manfred Mudelsee, Germany]	Accepted. The Appendices have been revised accounting for this and addressing the information provided in them both to Figure 7 and 8.
5-1028	5	23	28	23	8	It would be preferable if this figure could be updated so the instrumental record referred to is from the newer HadCRUT4 and CRUTEM4 datasets. [Adrian Simmons, United Kingdom]	Accepted. The instrumental dataset used now is consistent with that provided in the other chapters of WG1 report.
5-1029	5	23	28	23	28	Ch2 and 10 have used CRUTEM4 and HadCRUT4. You should be able to do this. It will make very little difference, but ensures consistency across the chapters. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted. The instrumental dataset used now is consistent with that provided in the other chapters of WG1 report.
5-1030	5	23	34	23	34	re. Table 5.1. For 50 year blocks, 1600-1899, series 14 and 15, black boxes are offset. [Rob Wilson, United Kingdom]	Editorial - copy edit to be completed before publication.
5-1031	5	23	34	24	1	Splitting a table like this over a page is not helpful [Peter Burt, United Kingdom]	Editorial - copy edit to be completed before publication.
5-1032	5	23	34			Although the concept is good, Table 5.1 is generally unclear. 'Ann' is defined earlier as the abbreviation for 'Annual' but it would be clearer to use Annual. [Iain Robertson, United Kingdom of Great Britain & Northern Ireland]	Accepted. Changed to "Annual" and accompanying text improved to explain how the table information is used.
5-1033	5	23		23		Table 5.1: top row of blue rectangles: the ones for studies 14 and 15 is misplaced upwards. [Manfred Mudelsee, Germany]	Editorial. Copy edit to be completed before publication.
5-1034	5	23		23		Figure 5.7: Readers should like to know here, reading the caption, what sources/references the various shown curves have. Suggestion: refer to Table 5.1 notes and do also care that abbreviations used in Figure 5.7 and Table 5.1 agree with each other (e.g., "CL2012" should likely be the same as "13 = Christiansen and Ljungqvist (2012)"). [Manfred Mudelsee, Germany]	Accepted. Improved compatibility between figures, tables and appendix in labelling of datasets.
5-1035	5	23				The Introduction (5.1) rightly states that “the evolution of surface temperatures during the past 2000 years (Section 5.3.5) is an important open debate" Section 5.3.5 opens by stating that it “is important to improve understanding of climate response to external	5.3.5 addresses changes at hemispherical scales. This issue is considered in the revised version of 5.5.1

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						forcings" and that "more extensive comparisons with GCM simulations" are needed. It is, therefore, surprising that almost nothing is said about proxy evidence of sea surface change during the past 2000 years, especially as these drive modern climate cycles (ENSO, PDO, AMO, NAO) and provide very important constraints on GCMs. These are discussed in section 5.4 Modes of Climate Variability (p. 5-28), but almost all of that discussion concerns the entire Holocene. As a result, a more complete discussion of this issue is warranted. [Government of United States of America]	
5-1036	5	23				Table 5-1 (p. 5-23) omits papers such as Graham et al. (2011) Climate Dynamics, Volume 37, Issue 5-6, pp. 1217-1245 Trouet et al. (2009) Science 324, 1256-1260 Trouet et al. (20120 Gobal Palanet. Change 84-85, 48-55 Jones et al. (2009) Holocene 19, 3-49 Wanamaker et al. (2012) Nature Communications 3, 899 (12 Jun 2012) Some of these papers are mentioned later in the section in particular in comparison of their proxy data to GCM model output–for example, the first paragraph on p. 5-27 (lines 4-28) mentions Graham et al. (2011) and other studies and uses Figure 5.8 to compare similated and reconstructed NH temperature changes. [Government of United States of America]	Section 5.3.5 and specifically Table 5-1 considers recostructions at global and hemispherical scales. As pointed out by the reviewer, this literature is considered in other sections of the chapter.
5-1037	5	23				Table 5.1: This table is described as being a comparison of recent vs. previous global temperature estimates but without digging into the numbers in the footnotes one can't tell which columns are the most recent estimates. Rather than order the columns under each grouping by the period of time covered, why not order them from most recent to oldest published and then it will be apparent what changes have been made in the estimates? [Government of United States of America]	Rejected. This was tried but was harder for the reader to follow. Ordering by record length seems to work better.
5-1038	5	23				Table 5.1: (1) It need to cite one more series from "MCSHANE B B, WYNER A J. A Statistical Analysis of Multiple Temperature Proxies: Are Reconstructions of Surface Temperatures over the Last 1000 Years Reliable [J]. Annals of Applied Statistics, 2011, 5(1): 5-44." (2)It's not reasonable to compare the reconstruction with instrumental record directly, because the reconstruction always underestimate the variance compare to the instrumental record. [Jingyun Zheng, China]	Noted. It is not clear that McShane and Wyner were putting forward a viable temperature reconstruction rather than exploring some of the issues surrounding this endeavour. We have included the new Shi et al. (2013) reconstruction that used the statistical method explored by McShane and Wyner (principal component regression using an autoregressive model, with model parameters and their uncertainties estimated using a Bayesian approach). For the second part of this comment, see 5-1025.
5-1039	5	24	1	24	28	Either in the Table or in the discussion thereof, account for the fact that many of these reconstructions are in agreement because they rely on a relatively large number (within the total available over time) of the extant proxy data. For instance, 2,3,4,7,8 use similar if not identical proxy inputs. Maybe combine their results into a single column in the Table? Maybe highlight the cases in which the reconstructions are completely independent with respect to input proxy data used? [Michael Neil Evans, United States of America]	Taken into account. The text has been modified to make interpretation more clear and to also account for this issue.
5-1040	5	24	3			"colder than the warmest 30 or 50 year mean after 1900": that is what the filled square means but not what the other symbols mean. Change to "was colder than the reference period stated next to the symbol below". [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	The Table, caption and acompanying text have been modified to improve clarity. This issue has been considered.
5-1041	5	24	4	24	5	What is the rationale for this equation and choice of value for a? [Michael Neil Evans, United States of America]	Taken into account. The equation and the value of a have been defined to match the uncertainty language standards of the IPCC WG1 report, under the assumption that the reconstruction uncertainty is represented by a normally-distributed error term. A comment has been included to make this clear
5-1042	5	24	5	24	5	replace "standard error" with "standard uncertainty". "Error" and "Uncertainty" have different meaning. [Government of Poland]	See 5-1041
5-1043	5	24	5	24	5	Table 5.1, Notes: Motivate the values selected for a (0.42 and 1.29). [Manfred Mudelsee, Germany]	See 5-1041

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5-1044	5	24	6	24	12	I don't fully understand the meaning of the symbols. I believe there are some words missing on line 8. Please clarify! [Masa KAGEYAMA, France]	Accepted. Some improvements to clarity have been made.
5-1045	5	24	6		11	The explanations of the symbols of Table 5.1 are confusing. Use the same sentence structure for all symbols, such as: "reconstructed temperature likely colder than"…; "reconstructed temperature very likely colder than"… "reconstructed temperatures as likely to be warmer as colder than"... [Government of France]	Accepted. Some improvements to clarity have been made.
5-1046	5	24	8			The description of this symbol is incomplete compared to the other symbols. [Iain Robertson, United Kingdom of Great Britain & Northern Ireland]	Accepted. Some improvements to clarity have been made.
5-1047	5	24	9	24	9	Table 5.1, Notes: The explanation for the filled blue rectangle contains a logical error! Correct is: "extends this to also very likely colder than the *coldest* 30- or 50-year mean of the post-1900 reconstruction". [Manfred Mudelsee, Germany]	Rejected. Some improvements to clarify have been made to avoid this misinterpretation.
5-1048	5	24	11	24	11	Change second "as" by 'or" [Yueh-Hsin Lo, Taiwan]	Accepted.
5-1049	5	24	15	24	28	What is the rationale for the choice of reconstructions used here? Most are post-AR4, but why leave Moberg et al in? I can see why PS2004 is used as there hasn't been anything new in this area. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. The new version of 5.3.5 and the Appendix has been revised and changed to make this rationale more clear. The bottom line is to keep the most recent versions of reconstructions of a given method or that have incorporated new proxies.
5-1050	5	24	16	24	16	This is the first time that Mann (2008) is mentioned in the chapter. Mann claimed in this paper that he could get a "hockey stick" with or without tree ring proxies. However, it has been shown very convincingly on Climate Audit that the non-dendro hockey stick in Mann 2008 is the result of using the Tiljander proxies upside-down. This was confirmed by Tiljander herself. McIntyre/McKitrick published a comment in PNAS in which they mentioned this. http://www.pnas.org/content/106/6/E10.full?ijkey=a980e3573c3f58772adf36177c17534023f20dab&keytype2=tf_ipsecsha Mann in his reply http://www.pnas.org/content/106/6/E11.full?sid=cf6aac63-05df-4aa0-aecc-a000276cafd3 denied he had done so and PNAS let him get away with this. McIntyre here http://climateaudit.org/2009/10/14/upside-side-down-mann-and-the-peerreviewedliterature/ summarizes the issues and also explains why in this case apparently the peer reviewed literature is unable to resolve such 'simple' issues whether Tiljander is used upside-down or not. Here http://climateaudit.files.wordpress.com/2009/10/tiljander_rotated2.gif is the graph showing the Tiljander proxies as meant (up is warm). Tiljander also stated that the proxy record is contaminated in the 20th century: "This recent increase in thickness is due to the clay-rich varves caused by intensive cultivation in the late 20th century… There are two exceptionally thick clay-silt layers caused by man. The thick layer of AD 1930 resulted from peat ditching and forest clearance (information from a local farmer in 1999) and the thick layer of AD 1967 originated due to the rebuilding of the bridge in the vicinity of the lake's southern corner (information from the Finnish Road Administration)." So the 20th century decline in this proxy is not a climatological signal. Mann flipped it though and used the recent hockey stick shape as evidence for anomalous 20th century warming. His dendro hockey stick shape btw is coming from the infamous bristlecone pines of which the NAS Panel in 2006 said they should be "avoided". I urge the authors to remove all references to Mann (2008) as long as Mann doesn't correct the obvious errors in this study. [Marcel Crok, The Netherlands]	Taken into account. The assessment presented now in 5.3.5 is robust to this feature. The version of the Mann et al reconstruction without the Tiljander proxies has been considered because of the modern disturbance issues with these series.
5-1051	5	24	16	24	16	The Tiljander proxies are not the only ones used upside down in Mann (2008). The same applies to Andy Baker's SU967 proxy, see http://climateaudit.org/2009/04/15/more-upside-down-mann/ [Marcel Crok, The Netherlands]	Noted. The sign of the proxy series used affects the CPS approach but not the EIV approach, as used by Mann et al. (2008). This and similar limitations are part of the additional sources of error that affect many reconstructions and justify our statement that the published confidence intervals underestimate the full uncertainty.
5-1052	5	24	17	24	17	Despite all the criticism Mann (2009) again used the Tiljander proxies upside down. The source code reveals that the statistical method is mining for either positive or negative correlations with the local temperature. Jean S, contributor at Climate Audit wrote http://climateaudit.org/2009/11/27/yet-another-upside-down-mann-out/: "This means that if a proxy has a strong inverted correlation to the (two-pick?) local temperature, it gets picked	See 5-1050 and 5-1051

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						– no matter what the physical interpretation is! Since RegEM doesn't care about the sign, it is now really so that the sign does not matter to them anymore. Anything goes! I'm speechless." [Marcel Crok, The Netherlands]	
5-1053	5	24	25			Missing in this section 5.3.5 is a mention of proxies such as pollen evidence that support evidence for Medieval drought and Little Ice Age cooling. The emphasis seems to be primarily on tree rings but with divergence problems etc. other proxies should be mentioned. [Government of United States of America]	This is taken into account in section 5.5.1 regarding regional reconstructions like those for North America. The focus of this section is on hemispherical and global reconstructions.
5-1054	5	24	26	24	26	Leclercq and Oerlemans is cited as (2011) in figures. Please correct. [Rob Wilson, United Kingdom]	Accepted.
5-1055	5	24	28	24	28	Two references are on the same line. [Hugues Goosse, Belgium]	Editorial. Copy edit to be completed before publication.
5-1056	5	24	31	25	22	Section 5.3.5.2 on reconstruction methods, limitations and uncertainties is a wonderful section to the chapter that informs policy makers of the strength and limitiation of paleoclimate data. This section is currently buried as part of temperature variations during the last 200 years and should be moved. The authors might consider moving this section to the beginning of the chapter, right after the introduction and then expand the scope to include paleoclimate data and reconstructions from other time periods and other variables. Along with the appendix material in tables 5.A.1 and 5.A.2, this section of the chapter is a great advancement on AR4. [Government of United States of America]	Noted. The discussion of uncertainties in 5.3.5.2 reffers specifically to the hemispherical and global proxy based reconstructions of the last 2k. This is why it was decided to keep it here.
5-1057	5	24	31	25	53	The problem of divergence is a nasty one. Visser, Buntgen, D'arrigo and Petersen, 2010, "Detecting instabiliteis in tree-ring proxy calibration", Climate of the Past 6, 367-377 propose a thorough method for testing the stability between tree growth and climate indicators. This reference is missing and none of the tree-ring reconstructions mentioned in the text apply this stability test. Another important article, missing in this Chapter, is C. Loehle, 2009, "A mathematical analysis of the divergence problem in dendroclimatology", Climatic Change 94, p. 233-245. [Hans Visser, The Netherlands]	Noted. These references have been considered in rewriting and revising 5.3.5.2. Due to space constraints and to the focus being on providing an assessment instead of a review, not all interesting papers are necesarily included.
5-1058	5	24	31			On the section 5.3.5.2 . When I saw the descriptions of "Reconstruction methods, limitations and uncertainties," I felt it is not easy to reconstruct past temperatures due to still existing difficulties in data analyses. In particular, old data analyses cannot be employed in determining the temperature during the latter half of the 20th century. [Kiminori Itoh, Japan]	Noted. Where reconstructions have been superceded by more recent ones we have used the latter.
5-1059	5	24	33			Signals for the last 2000 yrs v. smaller modern vs. "noise" / natural variability in the climate system. The text could be improved by explicitly explaining how proxy records must have higher resolution and higher sensitivity b/c signals are smaller and more temporally discreet. [Government of United States of America]	Noted. 5.3.5.2 has been revised trying to make the most important contributions to uncertainty in climate reconstructions of the last 2K more clear.
5-1060	5	24	41	24	41	not clear what "the latter" is referring to. [Konrad Gajewski, Canada]	Refers to reconstructions of spatial fields.
5-1061	5	24	43	24	50	This is a useful pointer to "new" methods but is comes over as abstract in that there is no indication or suggestion of the implications of the Bayesian approach for specific reconstruction results in comparison to earlier approaches. [Keith Briffa, United Kingdom]	Noted. The implications are not yet fully known.
5-1062	5	24	45	24	45	Write "Bayesian Hierarchical Models" (this is a technical term from statistical sciences). [Manfred Mudelsee, Germany]	Accepted.
5-1063	5	24	46	24	46	relate proxy records to climate and other environmental variables. [Government of France]	Accepted.
5-1064	5	24	50	24	50	Please cite Boreux JJ, Naveau P, Guin O, Perreault L, Bernier J (2009) Extracting a common high frequency signal from northern Quebec black spruce tree-rings with a Bayesian hierarchical model, 5, 607-613. [Government of France]	This reference has been considered in the revised version of 5.3.5.2
5-1065	5	24	55	24	55	Can' should be changed to 'have the potential for' [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Rejected. Meaning is the same, but our version is shorter.
5-1066	5	24	55	24	55	Delete "can". It has been shown that many of the methods ALWAYS underestimate the reconstructed amplitude of temperature variability to some degree. [Fredrik Ljungqvist, Sweden]	This is discussed in 5.3.5.2 in the detail allowed by space constraints. Results of reserch that demonstrate that this is minimized or alternatively,

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							that overestimation may take place are also discussed.
5-1067	5	24		24		Annex III (Glossary): Make an entry for "Robustness". [Manfred Mudelsee, Germany]	This term is well known in the context of statistical methods. It was considered not necessary to include this entry in the glossary.
5-1068	5	24		25		The section on reconstruction methods and uncertainties is well written carefully assessed and really helpful [Gabriele Hegerl, United Kingdom]	Noted. Thank you.
5-1069	5	24				Section 5.3.5.2 Some mention of the need for reconstruction of ocean climatic cycles (ENSO, PDO, AMO, NAO) needs to be made There is a strong emphasis on the use of tree ring climate data and their limitations (lines 28-45), but very little is said about other proxies (in particular SSTs). Some of this is present in Appendix 5.A: Supplementary Material (pp. 5-85 to 5-88). At least there should be some discussion of the various proxies in Appendix 5.A: Supplementary Material (pp. 5-85 to 5-88) and the limitations of coverage in time and geography of most of them. This could lead to a statement of why tree rings have been so important for reconstructions of the past 2000 years. [Government of United States of America]	Many of the uncertainties discussed in 5.3.5.2 also affect reconstructions of circulation modes. However, this section specifically addresses new advances and understanding of this topic concerning hemispherical and global reconstructions. The new version of 5.3.5.2 includes a note on this.
5-1070	5	25	1	25	16	Another issue here is how the low-frequency proxies are assessed. Including more makes things more problematic. The points discussed are mainly related to techniques of combining proxies. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	The revised version of the text tries to address this issue better
5-1071	5	25	3	25	3	replace "errors" with "uncertainties" [Government of Poland]	Rejected. It is important to stick with the term "errors" because it links to the "errors in variables" regression approaches.
5-1072	5	25	4	25	4	Sorry, another paper of mine that has been forgotten. Mudelsee (2012) studied a temperature proxy (ice-breakup) and analysed the temperature-proxy calibration by means of errors-in-variables regression, which takes proxy noise explicitly into account and prohibits underestimation of temperature amplitudes. My suggestion: Write "(Ammann et al., 2012; Hegerl et al., 2007; Mudelsee, 2012)". The reference is: Mudelsee M (2012) A proxy record of winter temperatures since 1836 from ice freeze-up/breakup in lake Näsijärvi, Finland. Climate Dynamics 38:1413–1420. [Manfred Mudelsee, Germany]	Noted but prefer to cite references more directly related to the hemispheric scales that are the focus of this section.
5-1073	5	25	9	25	16	This section needs some mention of the implications of inverse regression or total least squares regression on the likely uncertainty of the reconstructions produced. [Keith Briffa, United Kingdom]	Noted, but there is no clear exposition of this in the literature on hemispheric temperature reconstructions that we can refer to at this stage.
5-1074	5	25	11	25	11	Temporal smoothing does not improve correlation (as the number of degrees of freedom is much reduced, correlations must be much larger to be significant at the same level than unsmoothed correlations). Smoothing may just focalise the calibration of the low frequency behaviour of the series as high frequency noise is reduced. [Government of France]	While it is correct that the number of degrees of freedom diminish, the effects of temporal smoothing on correlation depend also on the spectral coherence of proxy and temperature. See references in the text.
5-1075	5	25	11	25	12	This goes in the same way than pointed out about the comparsion of periods: if low-frequency variability is attenuated, it is impossible to correctly compare 30-yr or 50-yr periods between rec and obs. Guiot et al (2010, Plos-one) have shown that it was possible to avoid that by using analogue method and by reconstructing separately low and high frequencies. [Government of France]	Noted. The comparison is done taking into consideration uncertainties.
5-1076	5	25	12	25	12	replace "error" with "uncertainty" [Government of Poland]	Accepted
5-1077	5	25	12	25	12	Add "Ammann et al., 2010" to set of citations in this line. [Eugene Wahl, United States of America]	Taken into account. This paper has been considered in the assessment and is cited elsewhere in this paragraph
5-1078	5	25	13	25	16	The sentence on lines 13-16 contains two errors. First, it implies that Hegerl et al (2012) used a local calibration technique. They did not. Second, it somewhat vaguely suggests that the local calibration technique	This comment has been taken into account to produce the new version along the lines suggested.

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						used by Christiansen (2011) and Christiansen and Ljungqvist (2012) is of another kind than those discussed in the latter part of the immediately preceding sentence on lines 9-12, but it is actually of the same kind (i.e. they assign part or all error to the proxy data). The only difference is that they used a local site-by-site calibration of each proxy record, and then averaged the calibrated records to form a hemispheric-scale composite. I suggest a reformulation of the sentence to: "Pseudoproxy experiments have shown that the latter approach used with a local site-by-site calibration (Christiansen, 2011; Christiansen and Ljungqvist, 2012) can also avoid attenuation of hemispheric-scale low-frequency variability, though it is debated whether it might instead inflate the variability and thus constitute an upper bound for low-frequency variability (Moberg, 2012)." [Anders Moberg, Sweden]	
5-1079	5	25	18	25	18	"A fundmental limitation", rather than "the fundamental"? [Konrad Gajewski, Canada]	Noted. We think the statement is comprehensive in the present wording.
5-1080	5	25	18			section on limitations for deriving past temperatures. Include a sentence on the importance of having matched dating to be able to compare pelaeorecords. [European Union]	Taken into account in the revised sentence.
5-1081	5	25	19	25	19	The instrumental records are not that short. They are potentially 300 years long in W Europe and 200 years long in N Europe. Here the 30-50 year timescale proxies can be assessed, but no-one has done this. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. The focus of this section is on hemispherical and global scales
5-1082	5	25	26			See also Emile-Geay et al. (J. Climate, in press), who found that the uncertainty in historical SSTs (for calibration) may be the greatest single uncertainty affecting multi-proxy millennial reconstructions of equatorial Pacific SSTs. Reference: Emile-Geay, J., K. Cobb, M. Mann, and A. T. Wittenberg, 2013: Estimating central equatorial Pacific SST variability over the past millennium. Part 2: Reconstructions and uncertainties. J. Climate, in press. [Andrew Wittenberg, United States of America]	Taken into account. This paper has been considered and is cited in the revised text.
5-1083	5	25	28	25	31	What does the phrase “needs further consideration” imply? [Keith Briffa, United Kingdom]	It was meant that this issue needs further research to better understand the type of uncertainty discussed. The text has been revised taken this into consideration to make it more clear
5-1084	5	25	31	25	31	Not sure “consideration” is the correct word here. Perhaps “experimentation” or “examination”?? [Rob Wilson, United Kingdom]	Accepted. Change to "examination".
5-1085	5	25	31	25	45	There are other “non-climate influences on a proxy” that are potentially very important but are not discussed here, and which are more fundamental than those described in the first two paragraphs of section 5.3.5.2, because they occur earlier in the reconstruction process, causing any errors induced to be propagated through to the subsequent steps that are the focus of those paragraphs. The most important of these influences derives from the concepts presented in: Loehle (2009) A mathematical analysis of the divergence phenomenon” Climatic Change (2009) 94:233–245. In that paper Loehle describes the general effect of inverting the relationship between climate and ring response (so as to predict a climate variable from a tree ring variable), whenever the ring response is a strongly nonlinear function of climate, for example, a unimodal response, in which ring response to climate changes from a positive to a neutral to a negative correlation as the climate state value increases. Because the “divergence” phenomenon is the most well known possible manifestation of this effect, Loehle places his discussion in that context. However, the effect he describes is general, and will apply whenever the pre-calibration period ring response crosses the optimum of the unimodal response. The problem is that there is no existing technique for reliably determining when this occurs, that is, there is no way to know the position of a particular ring's value with respect to the optimum, in the distant past. The calibration relationship in the instrumental record period is not informative in this regard. Therefore, there is a larger—potentially much larger—potential variance in the estimate of past climate states than is generally assumed to exist whenever the assumption of a strictly linear relationship between ring measure and climate state is made, which is essentially always (very few studies model this relationship as non-linear function, especially a strongly nonlinear one). Furthermore, this is not just an under-estimate of the total variance. Rather, it constitutes a potential bias, because by far most tree ring calibrations have positive correlations between temperature and ring measure, which means they sample that portion of the unimodal relationship that is to the left of the optimum. Any pre-calibration period rings that actually fall to the right of this optimum	Noted. Loehle (2009) assume an upturned quadratic relationship and everything that follows is immediately obvious. The idea of an optimum temperature is not new, what we're interested in is what that is and where it lies in relation to the temperatures being experienced by the sampled trees. As these are generally near to the latitudinal and/or altitudinal range of the tree species, the likelihood that a degree or two of warming is going to take them up to and past their optimal temperature remains to be demonstrated.

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						will therefore under-estimate temperatures. Note that Loehle's (2009) study is not a statement on how much larger this variation likely is, but rather a statement of its potential to exist, based on known biological issues (i.e. unimodal responses) and simple mathematical realities, i.e. it has defensible conceptual underpinnings. It is almost impossible to know how much larger this variance might be, because high quality information on the ring response of different species in different locations is always restricted by the length of the instrumental record and the climate states that have occurred during that time. This issue is of fundamental importance to the issue of calibration and uncertainty, adding additional evidence for the statement made in lines 47-48, and so discussion of it needs to be included. [Jim Bouldin, United States of America]	
5-1086	5	25	31	25	45	There is also another “non-climate influence on a proxy” that needs to be discussed in this section. There is uncertainty introduced by the standardization (“detrending”) method used, on which there is a large literature. Despite this literature, it remains generally unclear under which conditions different detrending methods should be used, and these choices are largely ad-hoc or poorly described and defended in individual works. For example, the age structure of the sampled trees has a large bearing on the effectiveness of the Regional Curve Standardization (RCS) method, but existing rules, even heuristic ones, for the relationship between age-structure and accuracy of the resulting chronology, to guide this decision, do not exist. Similarly, when the RCS method is applied to sites scattered over some large area, there is rarely good information included as to the possible differences in site quality and genetics between the different sites, both of which could affect the resulting RCS regional curve. Are there any circumstances in which the RCS approach will definitely prove inferior to detrending each core separately, in recovering low frequency climate variation? This too seems to be an un-addressed question. Given that a number of studies have shown that very different low frequency variations in chronologies result from these two detrending approaches (e.g. Briffa et al 1998, Figure 2)*, this is a quite important factor in the inherent uncertainty in reconstructions, one that needs to be openly discussed. *[Briffa, K. R., et al. "Trees tell of past climates: but are they speaking less clearly today?." Philosophical Transactions of the Royal Society of London. Series B: Biological Sciences 353.1365 (1998): 65-73.] [Jim Bouldin, United States of America]	Noted. While the detrending issue is of interest it cannot be covered in detail here due to space constraints.
5-1087	5	25	31			Another paper that should be discussed here is Loehle, 2009, climatic change 94, 233. This discusses the divergence problem and ambiguities in determining temperature from tree-rings (concerns recently supported by J Bouldin) [Paul Matthews, United Kingdom]	See 5-1085
5-1088	5	25	32			Jargon alert: readers won't know what "non-stationarity" means. Choose another more common term. [Jay Curt Stager, United States of America]	The text has been revised to make it more clear
5-1089	5	25	33	25	36	Also note here the problem of temporal sampling: the number of dataseries changes dramatically over time, and thereby also the type and location of dataseries; this must create biases in the resulting records over time in mean and variance. This topic is dealt with primarily by nesting of constant-number datasets (e.g. Wilson et al 2010 and many others), but has not really been explored yet in the literature. It could be assessed by ensemble sampling of different numbers of available paleoproxies over time, and by pseudoproxy experiments (e.g. like that of Smerdon et al 2011 but for time). [Michael Neil Evans, United States of America]	Noted. This has been taken into account implicitly in those reconstructions whose confidence intervals changes over time due to changing proxy coverage, but space constraints prevent a long discussion, especially since the reviewer notes that this hasn't been explored in detail in the literature yet.
5-1090	5	25	34	25	34	Also worth making it clear that tree-rings are just an example. Worth mentioning that the potential exists with ice cores and also documentary sources. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. Some issues with other proxies are mentioned, but space constraints prevent an extensive discussion.
5-1091	5	25	34	25	39	Clarification needed: this sentence needs to be re-written more clearly; shorter, more to the point. [Jay Curt Stager, United States of America]	The text has been revised accordingly
5-1092	5	25	34	25	41	Hide the decline was one of the most awful examples in the climategate emails of manipulation with the data, in this case not showing Briffa's reconstruction after 1960 aka the divergence problem. Briffa's reconstruction was based on several hundreds of proxies, so it's untrue that the divergence problem only applies to "some tree ring chronologies". This comment on the draft of AR4 is still valid: "Show the Briffa et al reconstruction through to its end; don’t stop in 1960. Then comment and deal with the “divergence problem” if you need to. Don’t cover up the divergence by truncating this graphic. This was done in IPCC TAR; this was misleading (comment ID #: 309-18)" A start is made here but I doubt whether readers unfamiliar with the history of this will realise how fundamental this divergence problem can be. If proxies don't show the expected response in late	This statement is not particularly relevant to AR5, because the Briffa et al. (2001) reconstruction is not used in Figures 5.7 and 5.8a,e,f where the divergence would be of concern. Its use is limited to the short-medium term variations in the volcanic and solar composites (Figs 5.8b-d) as noted in Table 5.A.4, because the divergence is not an issue at these timescales. The importance of divergence should not

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						20th century, how can we be sure they gave the expected response during the MWP? It therefore questions whether tree ring proxies are actually good temperature proxies. I am missing a reference to Loehle, A mathematical analysis of the divergence problem in dendroclimatology, Climatic Change (2009) 94:233–245, DOI 10.1007/s10584-008-9488-8 which states: "If trees show a nonlinear growth response, the result is to potentially truncate any historical temperatures higher than those in the calibration period, as well as to reduce the mean and range of reconstructed values compared to actual. This produces the divergence effect. This creates a cold bias in the reconstructed record and makes it impossible to make any statements about how warm recent decades are compared to historical periods." [Marcel Crok, The Netherlands]	be overplayed, in particular the MXD data where it is at its most evident are not used for the medieval part of the reconstructions shown -- i.e. it has been used for reconstructing the cooler periods of the last millennium. Se 5-1085 for response re. Loehle (2009).
5-1093	5	25	34	25	42	The point here is that tree-rings are being very honest here. The issue exists with other proxies (see above comment) and also you're using low-freq proxies that cannot be assessed. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	See 5-1090
5-1094	5	25	35	25	36	The phrase “many hundreds” is a large exaggeration that needs to be corrected. The two studies cited on line 36 (Buntgen etal, Esper etal) in fact employ 142 sites total, which does not constitute “many hundreds”. Furthermore, Buntgen et al (2008, 64 sites total) is restricted entirely to the European Alps region, which is small relative to the entire global area, and is not the region that most of the existing evidence for the divergence phenomenon have come from (which is the high northern latitudes, especially Siberia and Alaska). However, the cited Esper et al (2010) study does use sites that extend all the way across that region (Siberia), which is a far larger area than is the Alps region, and also employs more sites (78 total); it therefore presumably the more important work on this topic. These points need to be made. Also, some discussion should be included describing any studies in which divergence still exists even after the steps taken in the Buntgen et al and Esper et al studies have been made. [Jim Bouldin, United States of America]	The text has been revised taking this into consideration and addional bibliography
5-1095	5	25	39	25	39	Add “tree-ring data processing” before “methods” and important to add in citation to Melvin and Briffa (2008). Note that we were the first people to suggest the likely significance of “trend distortion” inherent in standardisation procedures as a possible major factor in the divergence issue – as is directly stated in this paper. [Keith Briffa, United Kingdom]	Taken into account; this section has been revised.
5-1096	5	25	39	25	39	"tree-ring chronologies" or "tree-ring based reconstructions"? [Masa KAGEYAMA, France]	Chronologies is correct in this case.
5-1097	5	25	41	25	42	Add the following reference: Andreu-Hayles, L., D’Arrigo, R. D., Anchukaitis, K. J., Beck, P., Frank, D., Verstege, A., and Goetz, S.: Varying boreal forest response to Arctic environmental change at the Firth River, Alaska, Environ. Res. Lett., 6, 045503, doi:10.1088/1748-9326/6/4/045503, 2011. [Fredrik Ljungqvist, Sweden]	Rejected. The paper is interesting but does not alter our assessment.
5-1098	5	25	42	25	45	The statement of the tree-ring response to volcanic forcing (cooling) is a little bizarre. It has long been known that individual site or wider region tree-ring averages can show somewhat suppressed immediate responses to sharp cooling events, particularly in ring-width data (where growth responses to short term climate changes may be effectively “smeared” in time because of the physiological processes involved in year-to-year tree-ring growth). The issue is certainly not new and is more complicated than is simply expressed in this brief sentence. If retained a little more text is needed to explain the issue in more detail. [Keith Briffa, United Kingdom]	This comment has been taken into account to produce the new version. Due to space constraints the discussion of this topic has been briefly extended.
5-1099	5	25	42	25	45	The problem here is of threshholds and or nonlinearities in the proxy-climate relationship, which is not the subject of the paragraph, but is an important source of uncertainty in paleoclimatic reconstructions. Either expand this and reference the relevant literature (e.g. Anchukaitis et al 2006 for tree ring widths; the large number of proxy-based reconstructions which use some form of detrending to exclude long term variability that is inconsistent with calibrated proxy-climate relationships; limitations to extrapolation of nonlinear proxy-climate relationships outside of calibrated ranges, e.g. for some elemental ratios in carbonates); or else delete this sentence entirely (I think these two papers show this particular hypothesis has been discredited.) [Michael Neil Evans, United States of America]	This comment has been taken into account to produce the new version. Due to space constraints the discussion of this topic has been briefly extended.
5-1100	5	25	42	25	46	This is a total red herring that should be left out of the Assessment. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	The text has been revised to better reflect this discussion.
5-1101	5	25	44	25	45	update Anchukaitis et al., submitted; Esper et al., submitted [European Union]	Accepted
5-1102	5	25	44	25	45	Anchukaitis et al. (2012) and a Mann et al. (2012) reply have now been published in Nature Geoscience. In	Noted. The arguments against widespread missing

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						my opinion, the Mann et al. argument is much more compelling. [Alan Robock, United States of America]	rings and loss of dating control are the strongest side of this debate.
5-1103	5	25	47	25	47	replace "error" with "uncertainty" [Government of Poland]	Taken into account (this sentence has been revised to remove this phrase for other reasons).
5-1104	5	25	47	25	47	Was a likelihood word meant to be used? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	It was not inteded with this meaning. The text has been reworded taking this into account.
5-1105	5	25	47	25	50	Discussion of McShane and Wyner is inadequate. This is an important paper by two professional statisticians and is very critical of paleo reconstructions, saying they are not significantly better than random noise. [Paul Matthews, United Kingdom]	Noted. Reviewer comments on the previous drafts led to reduction of discussion about this study, so this comment is weighed against other comments. The broader relevance of this study has not been demonstrated in the literature, though it does raise some interesting questions and it seems reasonable therefore to draw attention to an ongoing debate. To argue that this debases all proxy-based reconstructions of large-scale climate is an over-interpretation.
5-1106	5	25	47	25	53	Paper "Hughes, M.K., Guiot, J., Ammann, C.M., 2010. Emerging techniques and concepts offer ways to improve the use of process knowledge in reconstructions of past climate, and to make more comprehensive estimates of the uncertainties associated with them. PAGES news, 18, 2, 87-89." insists on the fact that mechanistic models should be used to better understand the proxies and to take into account non climatic factors biasing the relationships with climate. [Government of France]	This has been considered in this section. BHM approaches allow for this type of stablishing a priori hypothesis on mechanisms. While laudable, the published science needs to develop further in this area.
5-1107	5	25	47	25	53	This sentence seems confusing and unwarranted. The authors should consider stipulating what they mean for global or hemispheric mean temperature patterns. To say paleo reconstructions don't provide useful climate information is a pretty bold statement when the value of this information is shown in numerous publications. These reconstructions document regional and low frequency variability that is real, often ignored by modern climatologists, and can complicate modeling efforts to detect radiatively forced signals, and pose large inconveniences to those seeking simple assessment of several decade-long mean temperatures. A general thoguht that could be considered for inclusion would be: The simplistic D&A search for secular trends is complex. It does not seem prudent to discard the most important information to be gained from regional paleo records. [Government of United States of America]	The comment of the reviewer does not seem to fit with the contents of the paragraph it points to. The paragraph does not make the claims discussed by the reviewer. Perhaps missplaced?
5-1108	5	25	47	25	53	Maybe the point about uncertainties in proxies is that they should be determined by practitioners in the various fields and not by statisticians. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted
5-1109	5	25	49	25	50	I would suggest to modify the sentence. The debate has been about to what extent the proxies are able to provide useful climate information. There is little doubt that they do provide information but the question is to what extent and with what uncertainty. [Fredrik Ljungqvist, Sweden]	Noted. The text has been revised taking this into account.
5-1110	5	25	50	25	50	Add the following sentence after the period in this line: "Wahl and Smerdon (2012) extend the kind of analysis presented by McShane and Wyner (2011), and demonstrate clear capability of proxy data to contain useful information for climate reconstruction in the region examined, western temperate North America, both for climate field reconstructions and index reconstructions of the regiona spatial mean." [Eugene Wahl, United States of America]	The text has been revised to better reflect this discussion.
5-1111	5	25	50	25	53	This sentence reads as if these various stated improvements are definitely going to happen. There is however, no guarantee that any of these improvements will be made, because they involve an uncertain future. Therefore, the phrasing on line 52 needs to be changed to: "...will be needed in order to better characterize uncertainties and improve our knowledge of past temperature variations..." [Jim Bouldin, United States of America]	Taken into account; this sentence has been removed.
5-1112	5	25	55	25	55	Widowed header, please check page layout in final version [Peter Burt, United Kingdom]	Editorial - copy edit to be completed before publication.

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5-1113	5	26	1	26	28	There seems far too much on the modelling in this section. This could be balanced by showing responses to volcanoes in the instrumental temperature records. How does the b) panels compare with the 5 largish eruptions since 1880? [European Union]	Rejected. The section focusses indeed on model-data comparison for the last millennium, and the balance between data and models is reasonable. The suggested figure would fit better in a chapter dealing with instrumental period, but note that we do cite the related results of Brohan et al. (2012).
5-1114	5	26	1	26	28	There seems far too much on the modelling in this section. This could be balanced by showing responses to volcanoes in the instrumental temperature records. How does the b) panels compare with the 5 largish eruptions since 1880? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	See 5-1113
5-1115	5	26	4	26	11	This discussion is more discriminating than the discussion earlier about models and obs than it was for earlier periods. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted.
5-1116	5	26	6	26	6	Some pre-PMIP3 simulations used difference lower than 0.23% [Hugues Goosse, Belgium]	Accepted. The text has been modfied accordingly.
5-1117	5	26	8	26	11	This is an important issue and needs more discussion. The definition of the “palaeoclimate envelop” is critical and can be influenced by relatively few reconstructions. Hence the need in the text to have some discussion of the likely validity of the low-frequency aspects of those chronologies that define much of this envelope – specifically Loehle and McCulloch (2008) and Christiansen and Ljungvist et al. (2012) [CL2012]. Also this is one place where more can (should) be said about the mismatches in reconstructions and simulations: the failure to emulate the apparent warmth of the MWP and the overestimation of cooling in the simulations related to volcanic forcing – without the CL2012 reconstruction this would be more apparent (emphasise the need to consider explicitly the validity of temperature history shown by them). [Keith Briffa, United Kingdom]	The text has been revised trying to improve the disscussion of these issues.
5-1118	5	26	9	26	9	Write "GCM *simulations* and reconstructions". [Manfred Mudelsee, Germany]	Accepted
5-1119	5	26	13	26	20	What are the main results from these assimilation experiments? [Masa KAGEYAMA, France]	This section includes some discussion of the results attained with assimilation experiments. Further results are discussed in Chapter 10
5-1120	5	26	13		20	ch 10 shows a figure with the Goosse assimilation. Maybe worth forwardpointing (if we keep it!) [Gabriele Hegerl, United Kingdom]	Accepted.
5-1121	5	26	13			You might like to note the limitations of this sort of approach, particularly in terms of the ability to reproduce regional variability, as investigated by Annan and Hargreaves (Clim. Past, 8, 1141-1151, 2012). Or not :-) [James Annan, Japan]	Accepted.
5-1122	5	26	22	26	40	This discussion never alludes to seasonality i.e. potentially different responses in palaeoclimate data that are truly annually (or winter) responsive and those that see only summer temperatures. [Keith Briffa, United Kingdom]	The revised version of the text includes some comments on this issue
5-1123	5	26	22	26	40	This paragraph seems too long and too model centric. It was mentioned earlier that models seem to simulate too great an effect for some eruptions. Surface temperatures are good enough to show that for Krakatau the effect was relatively small. I'd believe the observations on the size of the effect, as opposed to any modelling study. [European Union]	Noted. Limited space prevents further exploration of this issue byeond the findings already assessed.
5-1124	5	26	22	26	40	This paragraph seems too long and too model centric. It was mentioned earlier that models seem to simulate too great an effect for some eruptions. Surface temperatures are good enough to show that for Krakatau the effect was relatively small. I'd believe the observations on the size of the effect, as opposed to any modelling study. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. Limited space prevents further exploration of this issue byeond the findings already assessed.
5-1125	5	26	22	26	40	This paragraph is about the response to volcanic events. It might be worth explaining how they are represented in the models (or referring to the appropriate section in other chapters). This would help discussing why there are deficiencies in the models' response to volcanic events. [Masa KAGEYAMA, France]	Noted. However space limitations preclude an in depth discussion of these model caveats.
5-1126	5	26	26	26	26	.....simulations USED increases..... [Rob Wilson, United Kingdom]	Noted
5-1127	5	26	26	26	28	It is very important in this context to take into account that most published multi-proxy temperature	This has been considered in the revised version of

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						reconstructions underestimate the amplitude of true temperature variability. [Fredrik Ljungqvist, Sweden]	this section, though without overstating the certainty.
5-1128	5	26	29	26	29	Also different assumptions with respect to effective radii of the particles etc. [Gavin Schmidt, United States of America]	Noted
5-1129	5	26	30	26	32	The whole point of the varying forcing datasets in PMIP3 was to differentiate 'model performance' from forcing uncertainty. Brohan et al show that some model/forcing combinations overestimate Tambora cooling, while the same model with different forcing does much better. This is therefore not a conclusion about a model. [Gavin Schmidt, United States of America]	Noted and taken into account in the revised version of 5.3.5.3
5-1130	5	26	34	26	36	check sentence, not clear what 'after peak negative forcing' means [Government of Germany]	Text has been modified for clarification
5-1131	5	26	36	26	37	The statement: "Solar forcing estimated over the last millennium shows weaker variations than volcanic forcing (Figure 5.8d, Section 5.2), even at multidecadal timescales." has no reference (see also figure 5.8). However, the discussion is in line with what is discussed in Miller, G.H., Geirsdóttir, Á., Zhong, Y., Larsen, DL., Otto-Bliesner, B., Holland, M.M., Bailey, DA, Refsnider, K., Lehman, SJ., Southon, JR., Anderson, C., Björnsson, H., Thordarson, Th. 2012. Abrupt onset of Little Ice Age cold triggered by volcanism and sustained by sea-ice/ocean feedbacks. Geophysical research Letter v. 39, L02708, doi:10.1029/2011GL050168 This reference, or another appropriate should be added [Government of Iceland]	Rejected. The statement is supported by the reference to Figure 5.8d, which shows the strongest multi-decadal solar fluctuations are weaker than the strongest multi-decadal volcanic fluctuations (Figure 5.8c). This will be sensitive to the forcing uncertainties and the choice of solar reconstruction in particular, but the one used here has relatively strong variations.
5-1132	5	26	36	26	40	In my opinion this sentence is somewhat unclearly written. We don't get any information about the detection method used either. [Fredrik Ljungqvist, Sweden]	Text has been modified for clarification
5-1133	5	26	42	26	56	The value of this section is somewhat questionable given the arbitrary and not entirely defensible definition of the period adopted (see related comments on MCA and LIA). [Keith Briffa, United Kingdom]	The definitions for MCA and LIA extension have been changed in the revised version of 5.3.5 and consistency ensured. Also, the results of this section have been checked to be robust against changes in the definition of these periods. The intention is to use them to illustrate the magnitude and pattern of changes between periods of apparent average high and low temperatures, rather than to provide a new definition of the MCA and LIA periods.
5-1134	5	26	50	26	56	The discussion related to the model-data agreement for both low and high TSI variations may appear confusing. From figure 8e, the MCA-LIA NH hemisphere temperature difference seem in better agreement with data for the simulations with high TSI variations while it is argued that Hind and Moberg (submitted) found a closer data-model agreement for simulations with weak TSI variations. I guess this is related to the fact that they use a more sophisticated analysis that simply comparing the Northern Hemispheric mean temperature but that should be mentioned here. [Hugues Goosse, Belgium]	This part has been revised to make it more clear and account for recent literature publised or in press.
5-1135	5	26	53	26	53	Citation style wrong [Peter Burt, United Kingdom]	Noted and corrected
5-1136	5	26	53	26	53	.....and Hind and Moberg (submitted) found..... [Rob Wilson, United Kingdom]	Noted and corrected
5-1137	5	27	1	27	1	Citation style wrong [Peter Burt, United Kingdom]	Noted and corrected
5-1138	5	27	1	27	1	Please change "Shapiro et al. (in press)" to "Shapiro et al. (2011)". [Georg Feulner, Germany]	Noted and corrected
5-1139	5	27	1	27	1	Please, insert "most" before temperature. Reconstructions using the "LOC" method as well as borehole temperature estimates support the simulations with stronger solar forcing. [Fredrik Ljungqvist, Sweden]	Accepted.
5-1140	5	27	1	27	1	..... to present (Shapiro et al. in press) appear..... [Rob Wilson, United Kingdom]	Noted and corrected
5-1141	5	27	4	27	28	Similar comment to the previous comment. However, I applaud the attempt to compare spatial patterns of temperature change in models and data. I question whether the "stronger TSI forcing" plot is worth showing – allowing more space and much needed higher definition in the maps. They are very hard to see. Could more data (additional to what is in Mann and Ljungvist papers) be added from other papers? Also here we are faced with the sensitivity of the results to the fact that MCA is 950-1250 CE and LIA is 1400-1700. Different (perhaps more logical choices based on Figure 5.8(a)) choices would produce different results. However there is also	See 5-1133 re. definitions for MCA and LIA, have been changed in the revised version of 5.3.5 and consistency ensured. Also, the results of this section have been checked to be robust against changes in the definition of these periods. The visibility of the figure will be improved by accomodating it to portrait

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						the question of seasonality again here. In Figure 5.12 some attempt is made to acknowledge this, but not in Figure 5.9. [Keith Briffa, United Kingdom]	in the editorial process. The reconstruction of Mann et al is the only existing field reconstruction at these spatial scales.
5-1142	5	27	9	27	9	The cooling south of Greenland is just seen in the reconstruction. [Hugues Goosse, Belgium]	Noted and taken into account in the revised version
5-1143	5	27	10	27	11	The statements in this sentence relevant to the reconstructions (Fig. 5.9f) contain an inaccuracy (cooling in N Am. is in the southeast not the southwest) and incompletenesses (cooling in other regions not mentioned, e.g. Africa and Asia). These need to be corrected. [Eugene Wahl, United States of America]	Noted and taken into account in the revised version
5-1144	5	27	11	27	11	This IS not surprising..... [Rob Wilson, United Kingdom]	Noted
5-1145	5	27	13	27	14	This is evident, but it should be much better to mask regions with too few proxies [Government of France]	Figure 5.9 shows information as in the original sources of the climate field reconstructions. We think it is useful to highlight the distribution of proxies and also informative in spite of proxy being scarce in some regions
5-1146	5	27	14	27	14	MCA-LIA --> use a real "minus" sign instead of an hyphen. [Masa KAGEYAMA, France]	Accepted
5-1147	5	27	17	27	17	What means similar here? Similar pattern, similar amplitude ? It seems from the figure that the amplitude of the changes is weaker for instance. [Hugues Goosse, Belgium]	Noted and taken into acount to revise this part.
5-1148	5	27	22	27	22	Please, change “anomalies” to “patterns”. [Fredrik Ljungqvist, Sweden]	Noted and considered for the revised version.
5-1149	5	27	23	27	23	It could be mentioned more explicitly in this section too that a background La Niña state is reconstructed for the MCA in the reconstruction shown in figure 5.9 but this may differ in other ones as discussed in section 5.4.1 [Hugues Goosse, Belgium]	Noted and included in the revised version of 5.3.5.3
5-1150	5	27	30	27	30	Figure 5.8, see comment on unreferenced statements above (5-26 line 36-37) [Government of Iceland]	See 5-1131
5-1151	5	27	41	27	41	Are the events on which the composites are based for panel d the same as for the panel c? [Masa KAGEYAMA, France]	Noted. Composite components are described in the Appendix.
5-1152	5	27	48			Caption for figure 5.8: The dots look red, not brown. [Government of United States of America]	Taken into account; symbols have been changed from brown to red and blue.
5-1153	5	27	58			Caption for figure 5.9: Please define "LMM", it is not defined in the text [Government of United States of America]	Taken into account in the revised version.
5-1154	5	27		27		Figure 5.8, panels "e" and "f": One should perhaps show instead of the single feature "multimodel mean and range (brown)" instead the double feature, namely (1) "multimodal mean and range for solar-low (blue)" and (2) "multimodal mean and range for solar-high (red)". One should further draw inside these panels, below the horizontal dashed lines, an explaining legend with an arrow. [Manfred Mudelsee, Germany]	Accepted.
5-1155	5	28	1			Caption for figure 5.9: What is an unhatched grid cell? And where is it on the figure? [Government of United States of America]	The caption has been revised to make this clear
5-1156	5	28	1			I don't know what you mean by unhatched, I cannot see any hatching. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	The caption has been revised to make this clear
5-1157	5	28	2			Do you mean <+80%? [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	The caption has been revised to make this clear
5-1158	5	28	5	28	7	It seems bizarre to weight them so that ECHAM and CSIRO oiutweigh everyone else combined. Why? [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	On the contrary, weighting has been applied so that models with a higher number of simulations do not outweight the others. The caption has been revised to make this clear
5-1159	5	28	9	28	9	Section 5.4 Modes of Climate Variability - There seems to be no coverage of the Pacific Decadal Oscillation (PDO/IPO) for which several reconstructions have been developed; Also, what about the Indian Ocean Dipole	Fewer papers have been written on PDO and IOD reconstructions since AR4.With the limited space

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						- again some reconstructions. At the very least, need to indicate why these modes of variability are NOT considered here. [Janice Lough, Australia]	available, we decided to focus on ENSO, NAO, SAM and AMO. Furthermore, the low frequency variability of ENSO basically captures PDO dynamics. Many tree-ring chronologies can be used to both characterize past ENSO and past PDO variability (seeFigure 10 in McGregor, S., Timmermann, A., and Timm, O.: A unified proxy for ENSO and PDO variability since 1650, Clim. Past, 6, 1-17, doi:10.5194/cp-6-1-2010, 2010)
5-1160	5	28	11	28	11	Maybe say “additional insights” so that you don't say “new” twice. [Government of Australia]	Accepted
5-1161	5	28	17	29	13	Vance 2012 could also be considered here if appropriate? [Government of Australia]	The correlation between Law Dome summer sea salt and present-day ENSO indices amounts to only 0.336, which means that only 11% of the Law Dome SSS variability can be interpreted as ENSO variability. There are some questions regarding the robustness of this proxy as an ENSO proxy record.
5-1162	5	28	21	28	21	Here different proxy records are indicated. This would actually be more beneficial in the short introduction to 5.3.5 (p 22, lines 33-38) [Hans Linderholm, Sweden]	Accepted
5-1163	5	28	23			The weaker meridional SST gradient in the tropical Pacific was previously shown Brierley et al., (2009). Reference: Brierley, C. M.; Fedorov, A. V.; Lui, Z.; Herbert, T.; Lawrence, K. & LaRiviere, J. P. Greatly Expanded Tropical Warm Pool and Weakened Hadley Circulation in the Early Pliocene Science, 2009, 323, 1714-1718 [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	The statement in the text refers to the meridional cross-equatorial temperature gradient in the equatorial eastern Pacific, not to the subtropical/equatorial temperature gradient discussed in Brierley. These are two different quantities, controlled by different physical mechanisms. To clarify we have replace "weaker meridional" by "weaker cross-equatorial"
5-1164	5	28	24	28	29	I have three comments about this sentence, so have subdivided them. Part A: Fedorov et al. (2006) call it a "permanent El Nino" without really meaning a "lack of ENSO", but instead intended changes in the mean state. I certainly cannot dispute that the phrase is well-used in the literature (esp. as I've used it myself). However, I think all researchers agree that it is a misnomer that leads to confusion. Several alternatives have been suggested, but none have caught on. Can we please not propagate this phrase even further? I see no compelling reason to even mention the term in the IPCC report [I suspect you'll need to remove the Fedorov et al (2006) reference]. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text changed to "This state, characterized by the absence of a Pacific cold tongue, still supported interannual surface climate variations, according to proxy records (Scroxton et al., 2011; Watanabe et al., 2011) and GCM simulations (Haywood et al., 2007). "
5-1165	5	28	24	28	29	I have three comments about this sentence, so have subdivided them. Part B: I agree that Scroxton et al. and Watanabe et al. indisputably show the evidence of ENSO in the Pliocene. However, I do not agree that the Haywood et al. paper adequately simulates the reduced SST gradients discussed in the previous sentence (whilst it does for the PRISM timeslab). [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text modified to "This state, characterized by the absence of a Pacific cold tongue, still supported interannual surface climate variations, according to proxy records (Scroxton et al., 2011; Watanabe et al., 2011).These results together with recent GCM experiments (Haywood et al., 2007) indicate (medium confidence) that in spite of climate background conditions being considerably different from today’s (cf. Section 5.3.1), interannual ENSO variability continued to operate during the Pliocene. "
5-1166	5	28	24	28	29	I have three comments about this sentence, so have subdivided them. Part C: There is an implication from this wording that ENSO is invariant throughout the whole Pliocene. I do not feel we have enough evidence to state that. We know it existed for intervals; suspect it existed throughout, but don't necessarily know its characteristics. I would feel much happier with a sentence that read along the lines of: "The consequences of such a background climate (c.f. Section 5.3.1) on ENSO are not fully understood. Proxy records of interannual surface climate variability (Watanabe et al., 2011; Scroxton et al., 2011) indicate that ENSO existed for instances in the Pliocene, if not throughout." I was going to suggest ".. and likely thoughout", but likely has a	The text was changed to "that in spite of climate background conditions being considerably different from today’s (cf. Section 5.3.1), interannual ENSO variability existed for instances during the Pliocene. ", as suggested by the reviewer. We have refrained from discussing ENSO prior to the Pliocene, as most of these records only show spectral energy on ENSO

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						defined meaning in the IPCC that is not necessarily justified in this case. You may then also want to add that "Proxy records with characteristic ENSO periodicities have been found from as far back as 70 million years ago (Ivany et al., 2011; Davies et al., 2011)" although the confidence might be so low in these early records that they do not merit discussion. Ref: Davies, A.; Kemp, A. & Pälike, H. Tropical ocean-atmosphere controls on inter-annual climate variability in the Cretaceous Arctic Geophysical Research Letters, 2011, 38, L03706; Ivany, L. C.; Brey, T.; Huber, M.; Buick, D. P. & Schune, B. R. (2011), 'El Niño in the Eocene greenhouse recorded by fossil bivalves and wood from Antarctica', Geophys. Res. Lett., 38, 16709. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	timescales, which is actually only a very weak indicator for the presence of ENSO- in particular in areas far away from the equatorial Pacific.
5-1167	5	28	24	28	29	This sentence is too long and should be broken up. [Government of Australia]	Accepted. Sentence split.
5-1168	5	28	32	28	32	A more recent GCM study of Walker circulation response to LGM forcing: DiNezio, Clement, Vecchi et al. 2011 Paleoceanography 'The response of the walker circulation to Last Glacial Maximum forcing : Implications for detection in proxies'. Cooling strengthens the circulation due to hydrological cycle constraints (higher P cf E), However, exposed land reduces size of warm pool, weakening the circulation. [Government of Australia]	The DiNezio paper discusses changes in the mean tropical Pacific climate (Walker circulation). It does not deal with interannual variability associated with ENSO. This is why we refrained from citing it in this context.
5-1169	5	28	34	28	39	This sentence is too long and should be broken up. [Government of Australia]	Accepted. Sentence split.
5-1170	5	28	35		36	The Younger Dryas is defined as the period about 12.8 ka to 11.5 ka. However, the term "Younger Dryas" in the Annex III (page AIII-29) defines this interval as the period 12.9 to 11.6 ka. [Alejandro Cearreta, Spain]	Noted. Now the dates are consistent with the new table 5.1
5-1171	5	28	37	28	37	should read: "…and in some cases…" [Government of Poland]	Noted. Text changed to "induces an intensification of ENSO amplitude (high confidence) and for the majority of climate models also a reduction of the annual cycle in the eastern equatorial Pacific"
5-1172	5	28	37	28	37	This sub-annual resolution only exists for a few years. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Reference deleted, because even under present-day conditions the ENSO correlation from climate in Tahiti is very weak.
5-1173	5	28	37	28	38	« a reduction of the annual cycle in the eastern equatorial Pacific ». It's the first time in the report this observation is made and thus it needs more precision : «a reduction of the amplitude of SST annual cycle in the eastern equatorial Pacific » [Bruno TURCQ, France]	Accepted
5-1174	5	28	37			"and in some cases" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted. Text changed to "induces an intensification of ENSO amplitude (high confidence) and for the majority of climate models also a reduction of the annual cycle in the eastern equatorial Pacific"
5-1175	5	28	41	28	43	Need to explain why this earlier evidence is considered "controversial". [Janice Lough, Australia]	Text has been modified and the word "controversial" has been dropped.
5-1176	5	28	41	28	51	This is a very important paragraph but the last sentence as written does not provide added value. "The reason for this change remains unresolved." The authors should consider deleting the sentence or at the very least consider rewriting it to identify what is meant by "this change". Is the referred to change: 1) "no statistically significant difference between mid- and late Holocene ENSO variance, but exhibit large ranges of natural ENSO variations", 2) "a small 8% reduction of ENSO amplitude". 3) "large inter-model spread of ±15% with respect to the present-day amplitude". Please clarify. [Government of United States of America]	Accepted. The entire paragraph has been rewritten in a more concise way.
5-1177	5	28	43	28	44	Other references: McGregor and Gagan (2004, GRL, doi:10.1029/2004GL019972), Rodbell et al. (1999, Science, 283, 516-520), Moy et al. (2002, Nature, 162-165). [Government of Australia]	Rather than providing a comprehensive review of the paleo-ENSO literature (including many studies that were assessed already in AR4), it was decided to
5-1178	5	28	43	28	44	It seems, reading the sentence, that the cited references are against an ENSO reduction during early and mid-Holocene whereas some of them are in favor of this reduction (illustrating the « controversy »). It would be clearer to separate reference pro and against ENSO reduction. Is Dolan et al 2011, about Pliocene, at its right place ? [Bruno TURCQ, France]	The paragraph was rewriten and many pre-AR4/AR4 references were deleted. The current statement refers to the AR4 assessment, the current modeling results and the key challenge to these findings, provided by

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							the Cobb (2013) paper. The Dolan reference was incorrect and was removed.
5-1179	5	28	43	28	44	There is also in these references a mixture of results obtained in different regions that difficult understanding the phenomenon, very probably the ENSO was less intense near the East Paciific coast and without great change in the central Pacific (modoki). [Bruno TURCQ, France]	Noted. There is currently, no study that re-examines the previous paleo ENSO proxies in terms of changes of ENSO flavours.
5-1180	5	28	44	28	45	Exactly what is sub-annual here? Is it monthly? Is it seasonal? There is no way of testing exactly what it is? Does the coral respond rapidly enough to the SST changes? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Many of the Cobb coral data have monthly resolution. Because the resolution is not homogenous, we still use the term "sub-annual".According to the close correlation between d18O in corals and temperature, the d18O clearly responds very quickly to SST.
5-1181	5	28	45			Citation alert: If the Cobb et al. reference is only "submitted" and not published in a peer-reviweed journal, then it cannot be used in a report of this nature. Skeptics will attack because of it. [Jay Curt Stager, United States of America]	The paper was published in Science and the updated reerence is in the bibliography.
5-1182	5	28	47	28	47	Which appears to be unrelated to orbital forcing? [Janice Lough, Australia]	To clarify the text was modified to "reveals no evidence for orbitally-induced changes of ENSO throughout the last 7 ka, but an ENSO system that experienced very large internal variance changes on decadal and centennial timescales"
5-1183	5	28	47	28	51	Model are struggling to produce the correct ENSO behaviour for the correct reasons (cf Chap 9) and this should also be noted here. [Eric Guilyardi, France]	We have added a statement referring to Chapter 9 in the ENSO figure caption
5-1184	5	28	51	28	51	See Li and Philander (1996, J. Climate, 9, 2986-2998) for an explanation of this in terms of links to the meridional SST gradient. [Government of Australia]	Li and Philander do not provide an explanation of why the annual cycle SST amplitude was weaker in the mid Holocene. Similar to Xie, they find that one of the controlling factors of the annual cycle amplitude in the eastern equatorial Pacific is the meridional SST gradient.
5-1185	5	28	53	28	54	This reference might be useful: D. Gutiérrez, A. Sifeddine, D. B. Field, L. Ortlieb, G. Vargas, F. Chávez, F. Velazco, V. Ferreira, P. Tapia, R. Salvatteci, H. Boucher, M. C. Morales, J. Valdés, J.-L. Reyss, A. Campusano, M. Boussafir, M. Mandeng-Yogo, M. García, and T. Baumgartner. 2009. Rapid reorganization in ocean biogeochemistry off Peru towards the end of the Little Ice Age. Biogeosciences 6: 835–848. [Eugenia M. Gayo, Chile]	We did not include this reference, because it mostly deals with mean state change in the Peru upwelling region. The sediment cores in the Gutierrez study can not resolve individual ENSO events or changes in the statistics of interannual variability
5-1186	5	28	54			Emile-Geay et al. (J. Climate, in press) thoroughly document the decadal-to-centennial variation of ENSO in a multiproxy reconstruction of the past millennium. [Andrew Wittenberg, United States of America]	Accepted. Reference included
5-1187	5	28	55	28	58	Also suggested by NE Australia rainfall reconstruction of Lough (2011) PALEOCEANOGRAPHY, VOL. 26, PA2201, doi:10.1029/2010PA002050 [Janice Lough, Australia]	Noted. Low frequency aainfall changes in Northern Queensland, although they may be partly related to ENSO, are not the main subject for this paragraph.
5-1188	5	28	55	29	58	Athe chronology of ENSO events obtained from documentary sources published in García Herrera et al 2008 shows that the seventeenth century was the least active EN period since 1550, while the 1620s, 1720s, 1810s, and 1870s were the most active decades [Ricardo Garcia-Herrera, Spain]	Accepted. Reference included.
5-1189	5	28	56			This was also shown by Emile-Geay et al. (J. Climate, in press) and McGregor et al. (Nature Climate Change, submitted). Reference: McGregor, S., A. Timmermann, M. H. England, O. E. Timm, and A. T. Wittenberg, 2013: Paleo-proxy evidence for changes in El Nino variability over the past 600 years. Submitted to Nature Climate Change. [Andrew Wittenberg, United States of America]	McGregor et al. will be resubmitted. Unlikely, that it will be published before cut-off date. In the most recent version of the Emile-Geay paper there is no evidence for suppressed interannual ENSO variance during the LIA.
5-1190	5	28	57	28	57	Other references: Braganza et al. (2009, GRL, doi:10.1029/2008JD010896) or Gergis and Fowler (2009, Climatic Change, DOI 10.1007/s10584-008-9476-z). [Government of Australia]	Accepted. Both references added

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5-1191	5	29	1	29	2	An Antarctic ice core proxy for ENSO-driven circumpolar winds shows evidence for stronger dominance of the El Nino state in the 20th century relative to the mean of the last millennium (Vance et al., 2012). Reference follows: [Tasman van Ommen, Australia]	Only 10% of the Law Done Ice Core variance in Vance are related to ENSO. 90% are unrelated. Prior to the calibration period it will be difficult to determine what is noise, what is ENSO signal. Because of the low correlation with ENSO even under 20th century conditions, we have refrained from citing and discussing this paper.
5-1192	5	29	1	29	2	T. R. Vance, T. D. van Ommen, M. A. J. Curran, C. T. Plummer, A. D. Moy, A millennial proxy record of ENSO and eastern Australian rainfall from the Law Dome ice core, East Antarctica, J. Climate, (in press) doi: 10.1175/JCLI-D-12-00003.1. [Tasman van Ommen, Australia]	Only 10% of the Law Done Ice Core variance in Vance are related to ENSO. 90% are unrelated. Prior to the calibration period it will be difficult to determine what is noise, what is ENSO signal. Because of the low correlation with ENSO even under 20th century conditions, we have refrained from citing and discussing this paper.
5-1193	5	29	2	29	3	Other reference: Emile-Geay et al. (2007, Paleoceanography, doi:10.1029/2006PA001304). [Government of Australia]	Reference added
5-1194	5	29	2		3	"possible effect onmean state of the tropical Pacific" is vague and Meehl et al (2009) results contentious - see discussion of compositing method by Roy, I., and J.D. Haigh (2012) Solar cycle signals in the Pacific and the issue of timings. J. Atmos. Sci., 69, 1446-1451, doi:10.1175/JAS-D-11-0277.1. or Tung KK and J Zhou (2010) The Pacific's Response to Surface Heating in 130 yr of SST: La Nina or El Nino-like? J.Atmos. Si., 67, 2649-2657. [Joanna Haigh, United Kingdom]	Noted. Given, the low statistical signifance of the observational results with only very few realizations, it is still justified to say "possible effect o nmean state". Since our section focuses on interanual ENSO variability, we do not elborate further on potential tropical Pacific background changes and their relation to solar variability. The sentence was reformulated to "Although simulations with climate models of varying complexity (Emile-Geay et al., 2007; Mann et al., 2005; Meehl et al., 2009) and proxy records from the tropical Pacific (Emile-Geay et al., in press; Marchitto et al., 2010) suggest (low confidence) possible influences of solar irradiance changes on the mean state of the tropical Pacific (see also Sections 5.3.5, 5.5.10 and Figure 5.9) the reconstructed multidecadal variance changes of interannual ENSO variability are fully consistent with the notion of internal variability (McGregor et al., 2010; Russon et al., submitted) and are uncorrelated with variations in TSI. "
5-1195	5	29	2			Citation alert: If the Cobb et al. reference is only "submitted" and not published in a peer-reviweed journal, then it cannot be used in a report of this nature. Skeptics will attack because of it. [Jay Curt Stager, United States of America]	The Cobb paper is now published and the reference was updated.
5-1196	5	29	4			Refers to section 5.5.0 which does not exist (this is not a quibble about typos - it means I can't cross-check what you are talking about wrt statements above on Pacific.) [Joanna Haigh, United Kingdom]	Section numbers were correctly updated
5-1197	5	29	4			Emile-Geay et al. (J. Climate, in press) also show a link between solar irradiance and the reconstructed tropical Pacific mean state, i.e. that NINO3.4 SST varied out of phase with solar irradiance on multi-century time scales. [Andrew Wittenberg, United States of America]	Accepted. Reference included
5-1198	5	29	5	29	7	This should be put in the context of the results of Wittenberg et al. (GRL 2009, cited in Chaps. 9 and 14) who show large ENSO decadal and centenial variations in a simulation without external forcing. [Eric Guilyardi, France]	Noted. This is already implicitly covered in earlier paragraphs citing the Cobb et al (2013) which illustrated reconstructed ENSO variance changes in the context of simulated changes using GFDL and CCSM4 models
5-1199	5	29	6			Wittenberg (GRL 2009) also discuss the modulation of ENSO by internal variability, suggesting that 20th-	Noted. This is already implicitly covered in earlier

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						century variations in ENSO behavior could have been produced by an unfored memoryless process. Reference: Wittenberg, A. T., 2009: Are historical records sufficient to constrain ENSO simulations? Geophys. Res. Lett., 36, L12702. doi: 10.1029/2009GL038710. [Andrew Wittenberg, United States of America]	paragraphs citing the Cobb et al (2013) which illustrated reconstructed ENSO variance changes in the context of simulated changes using GFDL and CCSM4 models
5-1200	5	29	6			Reconstructions also indicate vigorous tropical decadal-scale (as opposed to interannual) variability of SST throughout the past 850 years (Emile-Geay et al., J. Climate, in press). [Andrew Wittenberg, United States of America]	Noted. Due to space limitations, this section only deals with annual to interannual changes - not with decadal background state change
5-1201	5	29	11	29	13	In addition to McGregor and Timmermann (2010). GCM experiments in Ohba et al. (2012, JC in revision) also show the complex response of ENSO to the volcanic eruptions. Please consider my proposal to add the following reference: Ohba, M., H. Shiogama, T. Yokohata, and M. Watanabe, 2012: Impact of strong tropical volcanic eruption on ENSO simulated in a coupled GCM. Journal of Climate, in revision. [Masamichi Ohba, Japan]	Ohba paper reference included
5-1202	5	29	17	9	40	Some important works have been published focusing on warm season NAO (the summer NAO, SNAO). If I may be so bold, I would suggest including some of that research (which have been shown to have some significant impact)in this paragraph, e.g.: " The summer NAO (SNAO) is the most important single influence on interannual variations in North European summer climate (Folland et al. 2009), but also elsewhere through teleconnections, e.g the Sahel (Hurrell and Folland 2002), eastern USA (Hardt et a. 2010) and East Asia (Linderholm et al. 2011). Folland et al. (2009) showed that the SNAO can be reconstructed with tree-ring data, and presently the longest reconstruction, spanning 550 years, has been used to examine long-term relationships between SNAO and European and Sahel drought (Linderholm et al. 2009), as well as teleconnections between northern and southern Europe (Trouet et al. 2012) and the SNAO and summer climate in East Asia (Linderholm et al. 2012)". References: Folland CK. et al. 2009: The Summer North Atlantic Oscillation: past, present and future. J. Clim, 22: 1082–1103; Hardt B. et al. 2010: The seasonality of east central North American precipitation based on three coeval Holocene speleothems from southern West Virginia. Earth Plan. Sci. Lett., 295: 342-348; Hurrell JW. Folland CK. 2002: A change in the summer circulation over the North Atlantic. CLIVAR Exch., 25: 52–54; Linderholm HW. et al. 2009: A multicentury perspective on the summer North Atlantic Oscillation (SNAO) and drought in the eastern Atlantic Region, J. Quat. Sci., 24: 415–425; Linderholm HW. et al. 2011: Interannual Teleconnections between the Summer North Atlantic Oscillation and the summer East Asian Monsoon. J.Geophys. Res, 116: D13107; Linderholm HW et al. 2012: Exploring teleconnections between the summer NAO (SNAO) and climate in East Asia over the last four centuries – a tree-ring perspective. Dendrochr (in press); Trouet V. et al. 2012: A pan-European summer teleconnection mode recorded by a new temperature reconstruction from the northeastern Mediterranean (AD 1768–2008). Holocene 22: 887-898 [Hans Linderholm, Sweden]	Despite the fact, that this is a relevant topics, we we are not able to implement summer NAO considerations due to space restrictions
5-1203	5	29	21	29	21	The study of Rivière et al (2009) could also be cited (Rivière, G., A. Laîné, G. Lapeyre, D. Salas-Mélia, M. Kageyama, 2010. Links between Rossby wave breaking and the North Atlantic Oscillation – Arctic Oscillation in present-day and Last Glacial Maximum climate simulations. Journal of Climate, 23, 2987-3008.). It explains why topography has an influence on the NAO/AO-type variability in an LGM set-up. [Masa KAGEYAMA, France]	Accepted, this publication is now included
5-1204	5	29	23	29	23	Replace "an NAO" by "a NAO". [Maria Fernanda Sanchez Goñi, France]	Accepted
5-1205	5	29	25	29	25	Start a new paragraph dealing with the last millenium? [Masa KAGEYAMA, France]	Accepted
5-1206	5	29	28	29	28	Please, add the following new NOA proxy reconstruction to the discussion: Olsen, J., Anderson, N. J., Knudsen, M. F. Variability of the North Atlantic Oscillation over the past 5,200 years. Nature Geosci. doi:10.1038/ngeo1589 (2012). [Fredrik Ljungqvist, Sweden]	Accepted, this publication is now included
5-1207	5	29	32	29	32	Trouet at el. (2009) ... [Government of Australia]	Accepted
5-1208	5	29	32	29	32	(Trouet et al., 2009)--- Should this be formatted Trouet et al. (2009)? [Matthew Konfirst, United States of America]	Accepted
5-1209	5	29	32	29	37	Reference (Trouet et al., 2009) should be changed as Trouet et al. (2009) [SELVARAJ KANDASAMY	Accepted

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						SELVARAJ KANDASAMY, China]	
5-1210	5	29	33	29	33	Trouet et al. (2012) ... [Government of Australia]	Accepted
5-1211	5	29	33	29	33	(Trouet et al., 2012) --> Trouet et al. (2012) [Masa KAGEYAMA, France]	Accepted
5-1212	5	29	33	29	33	(Trouet et al., 2009)--- Should this be formatted Trouet et al. (2009)? [Matthew Konfirst, United States of America]	Accepted
5-1213	5	29	33	29	36	This Trouet et al (2009) reconstruction just has to be wrong. Do you think that our European forebears would not have noticed such a dramatic change in the character of the winters that they would not have written something down? The reconstruction shows a positive NAO for a couple of centuries and then this changes to the more variable oen for the next 700 years. The Vikings knew about the NAO from the cold winters in Scandinavia occurring when warm ones did in W Greenland. No-one in Europe documented a change in the character on west European winters. Why bother referring to a paper that is trying to explain something that is clearly not correct. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted, this part is now reformulated citing new published evidence
5-1214	5	29	33	29	36	I think the authors misinterpreted the publication of Trouet et al. 2012. I suggest writing the following: "(Trouet et al., 2012) postulated that this MCA/LIA difference might be related to a weakening of the AMOC whihc led to a transition to more negative NAO conditions. This is superficially in contrast to proxy reconstructions which resemble an increase of storminess from the MCA to the LIA and thus postulate a more postive phase of the NAO during the LIA (Meeker and Mayewski 2002). "(Trouet et al., 2012) overcome this contradiction by refering to Raible et al. (2007) who found in model simulations that mid-latitude cyclone intensity is increased during the Maunder Minimum although the number of cyclones is decreased and the MM shows a negative phase of thr NAO. " Raible, C. C., M. Yoshimori, T. F. Stocker, C. Casty, 2007: Extreme midlatitude cyclones and their implications to precipitation and wind speed extremes in simulations of the Maunder Minimum versus present day conditions, Clim. Dyn., 28, 409-423, DOI: 10.1007/s00382-006-0188-7. Meeker, L.D., Mayewski, P.A., 2002. A 1400-year high-resolution record of atmospheric circulation over the North Atlantic and Asia. The Holocene 12 (3), 257–266. [Christoph Raible, Switzerland]	Accepted, we have rewritten this part
5-1215	5	29	33			In Pinto and Raible (2012) all available NAO reconstructions are collected and presented in a review article. It might be helpful for the reader to give this reference here. Pinto, J., and C. C. Raible, 2012: Past and Recent Changes in the NAO, Interdisciplinary Reviews Climate Change, 3, 79-90 [Christoph Raible, Switzerland]	Accepted, publication has been cited
5-1216	5	29	36			Is"pseudo-proxy-based" a concept/oractice that is commonly accepted in climate science? Is this necessary to include? [Government of United States of America]	Yes, this concept is commonly accepted in the recent literature. No changes have been applied
5-1217	5	29	37	29	37	replace "….metod used by (Trouet et al.., 2009)." with "metod used by Trouet et al. (2009)." [Government of Poland]	Accepted
5-1218	5	29	37	29	37	(Trouet et al., 2009) --> Trouet et al. (2009) [Masa KAGEYAMA, France]	Accepted
5-1219	5	29	37			"(Trouet et al., 2009)" should be "Trouet et al. (2009)". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1220	5	29	40	29	40	Even in GISS-ER model, the changes in NAO are quite weak during the MCA compared to LIA. See for instance the figure 10 of Goosse et al. 2012b [Hugues Goosse, Belgium]	Accepted, this part is now reformulated
5-1221	5	29	42	29	48	Goodwin et al. 2004 Climate Dynamics, 783-794 may also be considered here? [Government of Australia]	Godwin et al 2004 is not included because it provides a early winter SAM reconstruction showing decadal scale variabiliy, but we have concentrated here on summer reconstructions, as it is the summer season where a significant positive trend in SAM as been observed during the second half of the 20th century.
5-1222	5	29	42	29	48	I just wonder that there is no study for changes of the SAM during the LGM - is this correct? I just found Dorst et al. (JGR-A, 2007) and Justino and Peltier (2008) wo worked on this issue. [Christoph Raible, Switzerland]	Drost et al does not talk about SAM, Justino and Peltier has been included.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-1223	5	29	44			It might be better to start the sentence "A first hemispheric-wide austral summer SAM reconstruction based on tree-ring data". I came to a halt when I read about a "hemispheric-wide mid-latitude tree ring". At least add a second hyphen so that "tree-ring based" bcomes "tree-ring-based". [Adrian Simmons, United Kingdom]	suggestion implemeted
5-1224	5	29	45	29	45	Substitute “Villalba et al., submitted” by “Villalbar et al., 2012”. This paper was recently published (Villalba et al., 2012. Nature Geoscience, doi: 10.1038/NGEO1613). [Government of Brazil]	Done
5-1225	5	29	51			One does get lost among the acronyms. While NAO is a long-known climate variation mode, the AMO is not. What are is the relationship (if any) between the AMO and the NAO beside time scales. Could AMO simply be a more or less permannent form of NAO (see comment 5.15)? [Government of France]	AMO and NAO are basically unrelated. The NAO is an internal mode of the atmosphere (maybe with some influence of SST anomalies). The spectrum of the NAO is white. The AMO characterizes SST variability in the North Atlantic. Its spectrum is red and some scientists think it is generated partly by variations of the Atlantic Meridional Overturning Circulation. The corresponding atmospheric pressure pattern is unrelated to the NAO (Kushnir 1994). To allow the reader a better understanding of the basic modes of variability we refer to Chapter 14 in the introduction of this section.
5-1226	5	29	51			Write out "AMO" when used for the first time. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1227	5	29	53	29	54	Add reference Chiessi et al. (2009. Geophysical Research Letters, doi:10.1029/2009GL039914) together with “Black et al., 2007; Kilbourne et al., 2008; Saenger et al., 2009; Sicre et al., 2008”. Chiessi et al. (2009) described for the first time the impact of the Atlantic Multidecadal Oscillation in the South American Monsoon and was published after the AR4. The inclusion of Chiessi et al. (2009) would substantially improve the support of the statement “Due to the wide range of impacts outside the North Atlantic region” since Black et al. (2007) and Kilbourne et al. (2008) refer to records from the Caribbean Sea, Saenger et al. (2009) refers to a record from the North Atlantic, and Sicre et al. (2008) refers to a record from the Nordic Seas, thus strongly linked or directly under the dynamics of the North Atlantic. [Government of Brazil]	Accepted. Changes have been made
5-1228	5	29	57	30	1	What are these inconsistencies in AMO reconstructions? [Government of United States of America]	Text changed to "...Whereas most of these records show a good correspondence with the instrumental data during the industrial period, correlations among different AMO reconstructions decrease rapidly prior to 1900 CE"
5-1229	5	29				There is inconsistent mention of data for model verification. For example, here a single unpublished paper by Cobb al. is used to refute previous data regarding the lower ENSO variance in early Holocene, yet the earlier bulk of the chapter touts model results and ignores regional data. Several other examples of this practice can be found throguhout the chapter. [Government of United States of America]	Noted
5-1230	5	30	1	30	1	... Knudsen et al. (2011) ... [Government of Australia]	Sentence revised
5-1231	5	30	1	30	1	(Knudsen et al., 2011) --> Knudsen et al. (2011) [Masa KAGEYAMA, France]	Sentence revised
5-1232	5	30	1			(Kundsen et al., 2011) concluded..this should be changed as Kundsen et al. (2011) [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Sentence revised
5-1233	5	30	17	30	22	(i) For the mid-Holocene, why are only 4 PMIP2 models and only 7 PMIP3 models used? The PMIP2 database contains 14 AOGM experiments for the mid-Holocene, and the PMIP3 database already contains more than the 7 models listed. Model intercomparison experiments allow the uncertainty associated with model physics to the sampled, so why are not all the models shown? If there is a valid reason for using such small subsets, this should be given. (ii) Same for the LGM. [Government of Australia]	Accepted. Analysis was updated to include all available PMIP2 and PMIP3 experiments
5-1234	5	30	23	30	28	with respect the preindustrial/with respect the portion..these should be changed as 'with respect to the preindustrial/with respect to the portion". [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Accepted

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5-1235	5	30	28			with respect to [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1236	5	30	30			Section 5.5 - Holocene: We found this section on regional changes to be focussed primarily on the past 2000 years. This focus should be stated in the chapeau to the section, and potentially within the title of this section. For example, section 5.3.5 is very clearly identified as focussing on the 'variations during the last 2000 years'. Linking these section clearly may be improved by choosing similar titles. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted, Chapeau is rewritten. More balance between Holocene and 2k is sought. Orbital trends receive higher emphasis, but within severe space limitations.
5-1237	5	30	32	30	34	What does this chapeau sentence actually mean, and what does it contribute to understanding the subsequent subsections? [Government of United States of America]	Accepted.
5-1238	5	30	32	30	34	This short introduction should be strengthened, especially in light of the recommendations of the GCOS, WCRP and IGBP after the last IPCC report, that additional focus should be on regional-scale climate information (Doherty et al. 2009). Although this relates to the modelling side, I feel that similar emphasis should be put on the paleoclimate context as well. [Hans Linderholm, Sweden]	Accepted.
5-1239	5	30	36	34	15	A large part of this discussion in Section 5.5.1 does not discuss the early to mid-Holocene time periods at all. Why? [Jim Bouldin, United States of America]	Accepted, within page limitations
5-1240	5	30	38	32	51	Section 5.5.1.1: Unfortunately, no new information on proxy-derived *winter temperatures* is included here. You should ask CA Dominik Fleitmann, who works on speleothems. [Manfred Mudelsee, Germany]	Noted, but difficult to find place due to severe space limitations
5-1241	5	30	38			Section 5.5.1.1: Please add discussion on the Holocene temperature changes in China and cite the most recent papers by Xiuqi Fang et al. on Acta Geographica Sinica (2011) and Hou and Fang on Advances in Geographical Sciences (2011) for the Holocene temperature reconstruction in China at 100-yr resolution [Hongyan Liu, China]	Accepted to the extent that space allows
5-1242	5	30	40			It would be wise to insert a reference to the Holocene in this sentence. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-1243	5	30	41	5	42	Typographic error. Delete the additional "orbital forcing" [Iain Robertson, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-1244	5	30	41	30	41	remove "…orbital forcing…" [Government of Poland]	Accepted.
5-1245	5	30	41	30	41	Delete “orbital forcing” [Dabang Jiang, China]	Accepted.
5-1246	5	30	41	30	41	"orbital forcings" two times [Olga Solomina, Russia]	Accepted.
5-1247	5	30	41	30	42	Typo: the phrase “orbital forcing” is repeated [Jim Bouldin, United States of America]	Accepted.
5-1248	5	30	41	30	42	“orbital forcing” is written twice. [Keith Briffa, United Kingdom]	Accepted.
5-1249	5	30	41	30	42	Delete “orbital forcing” once, since this terms are duplicated. [Government of Brazil]	Accepted.
5-1250	5	30	41	30	42	delete one "orbital forcing" [European Union]	Accepted.
5-1251	5	30	41	30	42	orbital forcing orbital forcing [Matthew Konfirst, United States of America]	Accepted.
5-1252	5	30	41	30	42	“Orbital forcing” is my mistake written twice. [Fredrik Ljungqvist, Sweden]	Accepted.
5-1253	5	30	41	30	42	"Orbital forcing" is duplicated [Yueh-Hsin Lo, Taiwan]	Accepted.
5-1254	5	30	41	30	42	Delete "orbital forcing". [Maria Fernanda Sanchez Goñi, France]	Accepted.
5-1255	5	30	41	30	42	orbital forcing is repeated [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-1256	5	30	41			Repetition of "orbital forcing" [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-1257	5	30	41			Typo: One "orbital forcing" is sufficient. [Christoph Raible, Switzerland]	Accepted.
5-1258	5	30	43	30	43	implicate --> imply [Masa KAGEYAMA, France]	Accepted.

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5-1259	5	30	49	30	50	mid to high latitudes of the northern hemisphere (I guess) [Masa KAGEYAMA, France]	Accepted.
5-1260	5	30	49	30	52	In Sundqvist et al. (2010), among other studies, we also find evidence for higher annual mean temperatures in the Arctic region during the mid-Holocene. [Fredrik Ljungqvist, Sweden]	Accepted.
5-1261	5	30	49	30	57	You can refer to Holocene glacier fluctuations here - their behaviour generally confirms this statement [Olga Solomina, Russia]	Accepted.
5-1262	5	30	49			“ubiquitous” does not appear to be the right word. Also, the Laurentide Ice sheet was still 2x Greenland at 9000 BP. The LIS was colder, there were alterations to atmospheric jets, etc, so warmer temperatures were not necessarily “ubiquitous”. The text could be revised to reflect this. [Government of United States of America]	Accepted.
5-1263	5	30	49			A temperature can not be warmer, please write: "summer-season temperature higher than today" [Christoph Raible, Switzerland]	Accepted.
5-1264	5	30	50	30	50	Is Northern North America referring to south-central Canada or the Arctic region? Clarification is required. [Government of Canada]	Accepted.
5-1265	5	30	51	30	51	Can the mid-Holocene be defined? What length of period was it and are you talking about an average for these years? It was 6K years ago, but do you mean an average for a thousand, a hundred or some other length of period. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted, the period is now defined in Table 5.1 and in Figure legend for 5.11.
5-1266	5	30	51	30	52	The discussion of the spatial pattern of the model results displayed in Figure 5.11 is very short (two lines). A longer discussion of the agreement and disagreement between model results is needed. A link to section 9.4.1.4 should also be given (and to Fig. 9.11-9.12). [Hugues Goosse, Belgium]	Taken into account in the revised version. More detailed comparison limited by space.
5-1267	5	30	51	30	52	In addition to simulations and snapshot (21 and 6ka) perspectives, continuous proxy-based reconstructions using pollen-climate calibration sets are likewise supportive of the concept of warmer winter temperatures in the MH. Two studies that document changes in winter T, as well as degree of continentality, that are relevant include: Giesecke et al. 2008. Exploring Holocene continentality changes in Fennoscandia using present and past tree distributions. Quaternary Science Reviews 27, 1296-1308. Brown et al. 2012. A spatio-temporal reconstruction of Holocene temperature change in southern Scandinavia. Holocene, 22:165-177. [Government of Canada]	There is a large literature on this, but chapter has severe limitations and cannot cite all relevant literature
5-1268	5	30	52	30	52	Please, add Sundqvist et al. (2010) to the list of references here. In the proxy data synthesis of Sundqvist et al. (2010) it is shown that both summer, winter and annual mean temperature at 6 ka on the high latitudes were significantly higher than in the pre-industrial period (c. 1750 CE). The full reference to Sundqvist et al. (2010) is: Sundqvist, H. S., Zhang, Q., Moberg, A., Holmgren, K., Körnich, H., Nilsson, J., and Brattström, G.: Climate change between the mid and late Holocene in northern high latitudes – Part 1: Survey of temperature and precipitation proxy data, Clim. Past, 6, 591–608, doi:10.5194/cp-6-591-2010, 2010. [Fredrik Ljungqvist, Sweden]	Accepted.
5-1269	5	30	56	30	56	"general NH cooling after ca 5ka": there is still a lot of regional variability in when this cooling occurs. The cited references are based on only a few high quality records, but the regional proxy records are more diverse, and there hasnt really been a synthesis. Perhaps a comment noting this regional variability, meaning rather low confidence for the timing of this, or even if this date is a hemispheric or global observation (or if the cooling started at different times in different regions). [Konrad Gajewski, Canada]	There is wide agreement on the timing on this in high quality records, but a note to potential regional differences will be inserted in revision
5-1270	5	30	57	30	57	"increased amplitude of millennial-scale variability". Note that Viau et al (2006) had made this observation and quantified it, from the terrestrial record as well. (Viau, A, K Gajewski, M Sawada and P Fines. 2006. Millennial-scale temperature variations in North America during the Holocene. Journal of Geophysical Research – Atmospheres 111, D09102, doi:10.1029/2005JD006031) [Konrad Gajewski, Canada]	Accepted.
5-1271	5	30	59	30	59	update Lohmann et al., submitted [European Union]	Deleted, paper not accepted by deadline

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5-1272	5	30				Figure 5.10: Why not make the plots instantly recognizable by adding "SST" in the two headers? "Annual cycle" and "ENSO" do not fully capture the message. [Government of United States of America]	Figure has been revised.
5-1273	5	31	1	31	5	Other forcings, in addition to the orbital one, have likely played a role in the cooling in the Arctic too (e.g. Miller, G. H., et al. (2012), Abrupt onset of the Little Ice Age triggered by volcanism and sustained by sea-ice/ocean feedbacks, Geophys. Res. Lett., 39, L02708, doi:10.1029/2011GL050168.) [Hugues Goosse, Belgium]	Accepted.
5-1274	5	31	2	31	3	Also boreal (subarctic) Canada: Viau, A and K Gajewski. 2009. Reconstructing millennial-scale, regional paleoclimates of boreal Canada during the Holocene. Journal of Climate 22: 316-330. doi: 10.1175/2008JCLI2342.1 [Konrad Gajewski, Canada]	Accepted. Reference added.
5-1275	5	31	2	31	4	Figure S1 of the cited work here (Esper et al, 2012b) provides evidence for long term, orbitally forced cooling trends that extend well beyond the last 2000 years, up to 7000 years ago in fact, via the reference to other studies on Norwegian glacial mass balance data and Russian treeline estimates, and GCM model output from their own study. See also the review by Wanner (2008; Quaternary Science Reviews, 27, 1791-1828). This should be discussed, as it is within the Holocene. [Jim Bouldin, United States of America]	Accepted, emphasis has been placed on longer orbital trends.
5-1276	5	31	3	31	3	delete comma after 'warming' [Peter Burt, United Kingdom]	Accepted.
5-1277	5	31	3			Comma before (Esper et al., 2012b) should be deleted. [Hongyan Liu, China]	Accepted.
5-1278	5	31	4	31	4	Why are references not in order of publication date? [Keith Briffa, United Kingdom]	Accepted.
5-1279	5	31	4	31	4	Kaufman (2009) also used the Tiljander proxies upside down, although they corrected this partly, see http://www.arcus.org/synthesis2k/synthesis/Correction_and_Clarification.pdf. They now truncate the Tiljander proxies after 1800 to avoid using the "contaminated" part (the part with the strongest hockey stick shape). But they still use it upside down http://climateaudit.files.wordpress.com/2009/10/tiljan10.gif making the MWP cooler than the LIA. [Marcel Crok, The Netherlands]	Noted but does not warrant a specific statement in the assessment
5-1280	5	31	4	31	5	The statement that" Marine proxy records of the last millennium indicate warm SSTs north of Iceland …etc" lacks a reference. [Government of Iceland]	Accepted. Reference added.
5-1281	5	31	5	31	5	Change "1400 CE" to "1300 CE". [Fredrik Ljungqvist, Sweden]	Accepted. Changed.
5-1282	5	31	6	31	7	It is necessary to clearly specify where the modern SST may still be cooler than the warmest interval of the 900-1400 CE period. [Hugues Goosse, Belgium]	Accepted within space limitations
5-1283	5	31	6	31	7	NB. Cunningham et al. is now accepted with minor revisions. Also – at the 95% confidence limit, modern SSTs (last 25 years) are not significantly cooler than the medieval. There error bars are quite large. [Rob Wilson, United Kingdom]	Accepted. This reference is now updated.
5-1284	5	31	7	31	7	update Cunningham et al., submitted [European Union]	Accepted. This reference is now updated.
5-1285	5	31	7	31	7	Change "1400 CE" to "1300 CE". [Fredrik Ljungqvist, Sweden]	Accepted. Changed.
5-1286	5	31	9			Dinocyst-based SST reconstructions are contentious. [Richard Telford, Norway]	Noted but it is beyond the scope of this report to detail methodological issues.
5-1287	5	31	10	31	10	What does "reflect different water masses" actually mean in terms of climate interpretation? [Keith Briffa, United Kingdom]	Noted. But not explained in more detail due to space limitations, term has been deleted.
5-1288	5	31	10	31	10	You write "same sediment core", but which core is meant? [Manfred Mudelsee, Germany]	Noted. But this level of detail is beyond scope of the report, 'in' is changed with 'from'.
5-1289	5	31	10			This is interesting, but "different water masses" is vague. What is meant by this? Water depth? Currents? Seasons? [Government of United States of America]	Noted. But not explained in more detail due to space limitations, term has been deleted.
5-1290	5	31	12	31	12	When was Medieval times? Every thing in the chapter needs to be watertight. Historians have a definition of the Medieval - do you now want to use this? [European Union]	Accepted, period now defined in new Table 5.1

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-1291	5	31	12	31	12	When was Medieval times? Every thing in the chapter needs to be watertight. Historians have a definition of the Medieval - do you now want to use this? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted, period now defined in new Table 5.1
5-1292	5	31	13	31	14	I feel strongly that this list of references should include Briffa et al. 2008 – which supersedes the Briffa et al. 2000 curve included in AR4 analysis. [Keith Briffa, United Kingdom]	Accepted, new reference is added
5-1293	5	31	14	31	14	Remove Kobashi et al. (2010) since it is superseded by Kobashi et al. (2011). [Fredrik Ljungqvist, Sweden]	Accepted
5-1294	5	31	14	31	14	Please add to the references: Melvin, T.M., Grudd, H. and Briffa, K.R. 2012: Potential bias in 'updating' tree-ring chronologies using regional curve standardisation: Re-processing 1500 years of Torneträsk density and ring-width data. The Holocene 22, in press, doi: 10.1177/0959683612460791. [Fredrik Ljungqvist, Sweden]	Accepted, paper cited
5-1295	5	31	14	31	14	Please add to the references: Shi, F., Yang, B., Ljungqvist, F.C., and Yang, F. 2012: Multi-proxy reconstruction of Arctic summer temperatures over the past 1400 years. Climate Research 54: 113–128. [Fredrik Ljungqvist, Sweden]	Accepted, paper cited
5-1296	5	31	14	31	29	In this section you refer to a warming to the end of the 20th century. This warming has continued to 2012. We are 12 years in the 21st century. Perhaps you need to point out that many of these tree-ring records don't have these years, with series ending some years ago. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Rejected, evidence is not only related to tree ring data
5-1297	5	31	16	31	16	millennia or millennium? Big difference. [Jim Bouldin, United States of America]	Accepted, rewritten
5-1298	5	31	16	31	16	last millinia--> last millenium (?) that's what fig 5.12 shows anyway... [Masa KAGEYAMA, France]	Accepted, rewritten
5-1299	5	31	16	31	16	Focusing now on the past millenium… [Matthew Konfirst, United States of America]	Accepted, rewritten
5-1300	5	31	16	31	29	This sectio does not seem to include discussion of all reconstructions in Figure 5.12. [Janice Lough, Australia]	Accepted, rewritten
5-1301	5	31	16			I think you mean the last millennium here [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted, rewritten
5-1302	5	31	18			This line is confusing: “of the advected” [Government of United States of America]	Accepted, text revised
5-1303	5	31	22	31	24	"Summer mean temperatures reconstructed…. Not in phase with higher latitudes": It is a little more complicated than this. Wahl used 3 pollen records from Gajewski (1988), who had also reconstructed summer temperatures from these sites (recons from the 1988 study and Wahl are nearly identical). However, Gajewski also used 2 others pollen sites from the midwest and 3 from New England, all varved and well dated. The other sites in the midwest also found warming during the period AD1200-1500 (and the period between 400-800AD was even warmer). Actually, the warmest period is around 1400-1500, not 1200. But in the New England sites, this period is cooler than previous centuries, although warmer than the LIA. There is a suggestion of slight warming, or at least a slowing of the cooling between 1200-1500 but not as pronounced as in the midwest. I am not sure I would emphasize this one result until it is better seen in the context of other data. Gajewski, K. 1988. Late Holocene climates of eastern North America estimated from pollen data. Quaternary Research 29:255 262. [Konrad Gajewski, Canada]	Accepted, sentences is deleted
5-1304	5	31	25	31	25	Insert 'the' before 'Canadian' [Peter Burt, United Kingdom]	Accepted
5-1305	5	31	26	31	29	Move references to end of sentence [Peter Burt, United Kingdom]	Rejected, references are at the right place
5-1306	5	31	27	31	29	This sentence is not that well formulated. Moreover, the content is slightly problematic since it is quite difficult to robustly compare pollen based temperature reconstructions (capturing multi-decadal variability) with instrumental data. [Fredrik Ljungqvist, Sweden]	Rejected, sentence reflects the content frofm the original publication
5-1307	5	31	29	31	30	Add the following sentences at the end of the paragraph in line 29: "Tree-ring-based reconstruction of annual temperature for western temperate North America over the past 500 years indicates sharp cooling in the very late 17th century, and also in the early 19th century associated with extensive tropical volcanic activity. Comparison of the reconstruction with instrumental data indicates there is high likelihood the most recent decades have been, on average, the warmest across mid-latitude western North America over the past 500 years, conditional on the ensemble uncertainty estimation method used (Wahl and Smerdon, 2012, supplemental material)." [Eugene Wahl, United States of America]	Accepted, text has been added

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5-1308	5	31	38	31	38	Figure 5.11, caption: Not all boxplots display whiskers --- explain. [Manfred Mudelsee, Germany]	Accepted - Figure revised
5-1309	5	31	44	31	44	CSIRO-Mk3L-1-2 is referred to incorrectly as the “UNSW” model. [Government of Australia]	Accepted - Figure caption corrected
5-1310	5	31	46	31	54	An important reference documenting variations in centennial-scale climate variability during the Holocene in Europe is missing. See: Seppä, H. and Birks, H.J.B. 2001: July mean temperature and annual precipitation trends during the Holocene in the Fennoscandian tree-line area: pollen-based climate reconstructions. Holocene 11, 527-539. [Government of Canada]	Paper is not published after AR4, we emphasise newer literature in the Assesment
5-1311	5	31	46	32	15	A summary diagram of different European temperature reconstructions and periods identified as warmer or cooler would be useful; very hard to follow following description of different records; needs critical synthesis rather than just lsiting "warmer/cooler" etc. [Janice Lough, Australia]	Partly accepted, text is partly rewritten, changes included in Fig. 5.12, a new table is out of scope due to space limitation
5-1312	5	31	46	32	34	A discussion of the model results displayed in Figure 5.12 should be included. [Hugues Goosse, Belgium]	Accepted, will be rewritten
5-1313	5	31	46	32	34	These regional description paragraphs would nicely follow the global and hemispheric discussion in section 5.3.5, page 22-line 31 to page 24-line 29 [Government of United States of America]	Rejected, there are pros and cons to move the subsection, but we find it after discussing the issue that it is better to have all regional content in one section
5-1314	5	31	50	31	50	Please, change “1000” to “900”. 900 CE is better in line with most reconstructions than 1000 CE. [Fredrik Ljungqvist, Sweden]	Changed
5-1315	5	31	56			What alpine region? The European Alps? [Government of United States of America]	Changed to European Alps
5-1316	5	31	56			Buntgen et al., 2006; Buntgen et al., 2005; Buntgen et al., 2011a, these references can be rewritten as "Buntgen et al., 2005, 2006, 2011a) and similar change is also applicable to the reference, Corona (in line 57) [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Accepted
5-1317	5	31	58			Expecting valid temperature reconstructions from chironomids in a lake where the degree of anoxia apparently fluctuates would seem optimistic. [Richard Telford, Norway]	Noted
5-1318	5	31				Viau result should be cited to indicate that 650-1059 interval was warmer than subsequent interval 1550-1850 [Government of United States of America]	Accepted - changes have been made
5-1319	5	32	1	32	1	Millet et al. (2009) point… [Matthew Konfirst, United States of America]	Accepted.
5-1320	5	32	1	32	1	(Millet et al., 2009) brackets at the wrong place [Olga Solomina, Russia]	Accepted.
5-1321	5	32	1	32	1	.....Trachsel et al., 2012). Millet et al. (2009) point to relatively..... [Rob Wilson, United Kingdom]	Accepted.
5-1322	5	32	1	32	2	Here it is important to note that while the tree-ring based reconstructions clearly show higher temperatures in the last decade than during any time during the Medieval Climate Anomaly, the lake sediment based reconstructions from the Alps actually show as high, or slightly higher, temperatures during part of the Medieval Climate Anomaly. [Fredrik Ljungqvist, Sweden]	Accepted - changes have been made
5-1323	5	32	1			(Millet et al., 2009) points, this should be changed as "Millet et al. (2012) points" [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Rejected, Millet et al. 2012 refers to Lateglacial summer temperatures
5-1324	5	32	2			Buntgen et al., (2011a) and Stewart et al., (2011a) (in line 3), comma should be removed after "et al." [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Accepted.
5-1325	5	32	3	32	3	should read: "….Steward et al.., (2011a) present extended summer temperature…." [Government of Poland]	Accepted.
5-1326	5	32	7	32	8	update PAGES 2k Consortium, submitted [European Union]	Accepted.
5-1327	5	32	7	32	15	To put the decade 2001-2010 CE in European summer temperature into a statistical context requires to apply in such comparisons the same span (10 years), which you do, but also the same spatial scale (Europe-wide), which is not clear whether you do it. [Manfred Mudelsee, Germany]	Noted, the section refers to the European scale
5-1328	5	32	10	32	11	I guess that this statement is based in the BHM reconstruction. If I am not mistaken the 10-year period 1998-	Noted

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						2007 was the warmest decade [Eduardo Zorita, Germany]	
5-1329	5	32	12	32	13	Some authors proposed that the summer of 1540 was warmer than 2003 in some regions of Europe (An underestimated record breaking event: why summer 1540 was very likely warmer than 2003, O. Wetter and C. Pfister Clim. Past Discuss., 8, 2695-2730, 2012 http://www.clim-past-discuss.net/8/2695/2012/cpd-8-2695-2012-discussion.html) [Hugues Goosse, Belgium]	Accepted - the new paper will be included. It is now published in Clim Past
5-1330	5	32	13	32	15	Perhaps worth noting that there are potential low frequency biases in the Dobrovolný historical based reconstruction? [Rob Wilson, United Kingdom]	Rejected due to space restrictions
5-1331	5	32	17	32	19	The new multi-proxy temperature reconstruction from Tibet, covering the last two millennia with decadal resolution, should be cited and discussed here. The reference is: Hou Guangliang, E Chongyi, Liu Xiangjun, and Zeng Fangming. Reconstruction of integrated temperature series of the past 2,000 years on the Tibetan plateau with 10-year intervals. Theoretical and Applied Climatology, in press, doi: 10.1007/s00704-012-0783-y. [Fredrik Ljungqvist, Sweden]	Rejected, this paper is not cited here based on reasons as following. 1. The uncertainties of this multi-proxy temperature reconstruction from Tibet (such as the standard error) are not given in the paper, which is very important and necessary for multi-proxy reconstruction. 2. The reconstruction is very much qualitative. Based on the description of the methods used in the analysis, each proxy was standardized first and then was re-standardized by min-max method. Therefore, the unit of each proxy should be relative to its standard deviation. Since the standard deviation may not be equal among the standardized proxies, the values of the final regional reconstruction mean only warm or cold (sigma), not the real degree C. In addition, there is no calibration equation and no RE and CE for the reconstruction. Table 2 lists the correlation coefficients among the instrumental climate data and the correlations between the proxy and the instrumental climate data were missed. 3. Some tree-ring data used in the paper were not robust. The Lhasa tree-ring data published in 1981 was derived from only one piece of wood, and tree-ring data of Qilian Mountain published in 1984 were with dating errors.
5-1332	5	32	17	32	29	Comment as # 21; some sort of critical synthesis needed. [Janice Lough, Australia]	Rejected due to space restrictions
5-1333	5	32	17	32	29	To put the late 20th century warmth in China into a statistical context requires to apply in such comparisons the same span and also the same spatial scale, both of which are not clear whether you do it. [Manfred Mudelsee, Germany]	Noted
5-1334	5	32	17	32	29	The references to the ice-core records in the Central Asia are generally missing here. For instance, the results obtained in the Altay Mts by the group of M.Schwikowsky [Olga Solomina, Russia]	Accepted. A paper by Eichler et al., 2009 (Eichler, A., S. Olivier, K. Henderson, A. Laube, J. Beer, T. Papina, H.W. Gaggeler, and M. Schwikowski, 2009: Temperature response in the Altai lags solar forcing. Geophysical Research Letters, 36, L01808) is cited and text changed accordingly.
5-1335	5	32	19	32	19	Please, change “11th” to “10th” since much tree-ring data show maximum warmth already during the 10th century. [Fredrik Ljungqvist, Sweden]	Changed accordingly
5-1336	5	32	23	32	23	Show the time interval of the MCA here. [Lei Huang, China]	Rejected, MCA is defined in the glossary
5-1337	5	32	26	32	26	“are” should be “is” [Keith Briffa, United Kingdom]	Changed accordingly
5-1338	5	32	28	32	28	“was about 1°C during” should be “was about 1°C higher during” [Keith Briffa, United Kingdom]	Rejected, it should read lower

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-1339	5	32	28	32	28	1 C warmer or cooler/ high or lower? State which one [Government of Australia]	Changed accordingly
5-1340	5	32	28	32	28	Insert "lower" after "about 1℃". [Lei Huang, China]	Changed accordingly
5-1341	5	32	28	32	28	the alkenone-based reconstruction indicated that the growing season temperature was about 1 deg C warmer - but warmer than what? Is this comparing documentary with alkenones? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Changed accordingly
5-1342	5	32	28	32	28	A word seems to be missing before “during”. [Fredrik Ljungqvist, Sweden]	Changed accordingly
5-1343	5	32	28	32	28	The temperature was about 1°C warmer or cooler? [Maria Fernanda Sanchez Goñi, France]	Changed accordingly
5-1344	5	32	28	32	29	I guess a verb is missing in the sentence. [Hugues Goosse, Belgium]	Sentence corrected
5-1345	5	32	28			Needs either warmer or colder after 1oC [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Sentence corrected
5-1346	5	32	28			The growing season temperature was about 1oC…really? Shouldn't this be "about 1oC warmer"? [Government of United States of America]	Sentence corrected
5-1347	5	32	28			relative term required (e.g. warmer, cooler, etc.) [Michael Hren, United States of America]	Changed accordingly
5-1348	5	32	28			The word "warmer" or "cooler" (I'm not sure which, probably "cooler" based on predeceding sentence) is needed before "during the period". [Adrian Simmons, United Kingdom]	Changed accordingly
5-1349	5	32	28			You don't mean the seasonal temp was 1 degree, do you? 1 degree warmer or colder perhaps? [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Changed accordingly
5-1350	5	32	29	32	29	Write "was about 1°C *higher* during". [Manfred Mudelsee, Germany]	Rejected, it should read lower
5-1351	5	32	31	32	31	Is this the only unresolved topic (this chapter includes others)? The following sentence says that the topic is resolved. Should be clarified or removed. [Government of France]	Accepted, this part has been deleted
5-1352	5	32	31	32	34	It seems odd to pick on this topic. There are many “unresolved” topics. If this is referred to, more detail is needed of the significance of this paper (Wanamaker et al. 2012) and validity of linking it to MCA and LIA as adopted here. [Keith Briffa, United Kingdom]	Accepted, this part has been deleted
5-1353	5	32	31	32	34	This brief paragraph is confusing because it introduces an "unresolved topic" (decadal to centennial variability of AMOC and impacts thereof), but the remainder of paragraph does not amplify or clarify the unresolved nature of the topic. [Government of United States of America]	Accepted, this part has been deleted
5-1354	5	32	31	32	34	This short paragraph and coverage elsewhere on AMOC exemplifies the difficulty in covering this topic sufficiently. There is a large empirical and modeling literature on AMOC and multidecadal modes of variability. It is not cited sufficeintly to illustrate the importance of interpreting any late Holocene , ie last 1300 yr, reconstruction. [Government of United States of America]	We agree that there is a literature, most of the discussion belongs in Ch3, however. There is a scarsity of paleo-papers on AMOC variability with enough resolution to resolve multidecadal time scales, and we find it difficult to make a thorough assessment at this stage
5-1355	5	32	31	32	34	This paragraph just seems to be hanging in mid air. [Janice Lough, Australia]	Accepted, this part has been deleted
5-1356	5	32	31			The Lund, Lynch-Stieglitz, Curry Nature 2006 paper about variability in the Florida Current (and possible links to AMOC) should be mentioned. [Government of United States of America]	Rejected due to space restrictions
5-1357	5	32	34			AMO also results from ocean/sea Ice interactions and therefore sea ice reconstructions resolving decadal time scale are important information in particular in the context of increase ice melting and export from the Arctic Ocean (Massé et al., 2008). High sea ice values in the northern North Altantic have occurred during the most severe phase of the LIA when the thermohaline circulationwas weakened (Lund et al., 2005). Thermohaline circulation at risk... [Marie-Alexandrine Sicre, France]	We diagree that we know what causes the AMO, but agree that there is evidence for multidecadal behaviour in sea ice records. Text will be modified.
5-1358	5	32	37	32	51	Why is Fig 5.12 is exclusively based on submitted works which can not be controlled, while regional reconstructions are already existing: Guiot et al (2010) for Europe, Neukom et al (2011) for SSA, etc ...	Fig 5.12 is now based on accepted/published continental reconstructions

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						[Government of France]	
5-1359	5	32	37			Sorry to be picky but why have you chosen a 999 year period rather than a 1000 year period? [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Changed accordingly
5-1360	5	32	38	32	40	for annual (ANN) or selected seasons in regions indicated in the lower right panel. I think, these details are indicated both right and left panels of the Figure. [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Noted
5-1361	5	32	39	32	39	Figure 5.12, caption: It is unlucky (or a typing error?) that "DJF" and "SONDJF" overlap. [Manfred Mudelsee, Germany]	Changed accordingly
5-1362	5	32	55	32	56	First, throughout this section would recommend using the term "variations" rather than "trends". The latter has a statistical conotation and, hence, a presumption that things are either going up or down. Paleoclimatic records should, hopefully, reliably reconstruct a the temporal variations ina climate variable, which can then be analysed for significant models of climate variability. Second, is this section only covering reconstructions of SST? Third, is this section only about the "mid-Holocene" - Section 5.5 is entitled "Regional changes during the Holocene"? [Janice Lough, Australia]	Changed accordingly
5-1363	5	33	1	33	1	Need to emphasise that these are "reconstructed" temperatures. [Janice Lough, Australia]	Accepted - text revised to read "Reconstructions of Holocene land temperature history ….
5-1364	5	33	1	33	4	This sentence needs to be modified. Does it mean that temperature has increased, by 0.5℃–2℃, or from ~0.5℃ to 2℃, from the early Holocene to present? [Lei Huang, China]	Accepted - text revised to clarify meaning
5-1365	5	33	3	33	4	"consistent with local insolation" - what does this mean? [Janice Lough, Australia]	Accepted - text revised to clarify meaning
5-1366	5	33	8	33	9	Or, we just do not know due to limitations in spatial and temporal coverage of reliable paleoclimatic information. [Janice Lough, Australia]	Noted
5-1367	5	33	11	33	11	.....SIMULATED summer cooling [Rob Wilson, United Kingdom]	Taken into account in the revised version
5-1368	5	33	13	33	13	Figure 5.11 presents temperature and model data over land not SST as indicated in the introduction to this section. [Janice Lough, Australia]	Accepted - text revised to clarify meaning
5-1369	5	33	16	33	17	"no reconstructions available in the form of syntheses from South America", this statement is not exact, there are reconstruction of terrestrial vegetation in south America e.g. Marchant et al. Clim. Past, 5, 725–767, 2009; but no reconstruction of MH temperature (MTCO MTWA). It is surprising that the changes in vegetation (that also depend on wetness) are not reported in the document, at least the progresses since AR4. [Bruno TURCQ, France]	Rejected: this section does not deal with terrestrial vegetation changes in the past
5-1370	5	33	20	34	15	I found the whole of this section unclear and hard to follow; the emphasis should be on critical assessment and synthesis of available proxy climate/environmental information, including identifying where records are, for whatever, reasons, likely to be less reliable than others. [Janice Lough, Australia]	Noted
5-1371	5	33	22	33	22	Why couch this as “austral summer duration” instead of referring to TSI? [Keith Briffa, United Kingdom]	Accepted, text revised
5-1372	5	33	22	33	22	summer duration, based on what? Insolation? [Government of Australia]	Accepted, text revised
5-1373	5	33	22	33	31	The section dedicated to SST in the Southern Ocean seems weak and orientated towards coastal records from South America and Antarctic Peninsula. There exist several transfer function based SST records from the South Atlantic (Hodell et al., 2001; Nielsen et al., 2004; Bianchi et al., 2004) that show a similar 5 ka Neoglacial cooling but, convesely, less millennial to centennial oscillations possibly due to (1) the proxy used and (2) a dampened response in the open ocean compared to coastal zones. [Government of France]	Accepted, will be updated within space limits
5-1374	5	33	23			insert "which might be comparable to the 20th century " in "...show warm conditions" and "during...", because most of references cited there seggesuted that the warmth of MCA is remarkble and comparable to that in the 20th century. [Jingyun Zheng, China]	Will make text clearer on this point. MCA was warmer than late 20th cent in some regions, but not as a globally consistent pattern as current warming as shown by recent literature

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5-1375	5	33	24	33	24	What is a "strengthened" temperature variation? [Janice Lough, Australia]	Accepted, text revised
5-1376	5	33	26			the lastest estimation on the size of MWP1a (Deschamps et al, Nature 2012) provides a range of 14-18 m of sea level rise in about 300-years, which gives a mean rate of sea level rise larger than 40 mm/year (more probably 50 or 60 mm/year). All these numbers are significantly larger than the values given in Table 5.4 or on FAQ 5.1, figure 1. [Government of France]	Accepted, text revised
5-1377	5	33	28	33	28	Does "diverge" mean "disagree? If so, then be clear that is what is meant. [Janice Lough, Australia]	Accepted, text revised
5-1378	5	33	33	33	36	Unclear what region these sentences are referring to or whether we are talking hemispherically or globally? [Janice Lough, Australia]	Accepted, text revised
5-1379	5	33	33			Are there any pollen reconstructions for the land that can be discussed? [Government of United States of America]	Noted. Caveat on data availability has been added.
5-1380	5	33	36	33	36	northern hemisphere? [Government of Australia]	Accepted, text revised
5-1381	5	33	38	33	38	Reference to Gergis et al 2012 (submitted) here? [Government of Australia]	Rejected, paper was not accepted until 15 March 2013
5-1382	5	33	38	33	46	Boucher et al (2011, CP) showed the importance of AAO in the climate variations in SSA (PDSI depends upon temperature and precip) [Government of France]	Accepted - text is modified accordingly
5-1383	5	33	38	33	46	I do not thing "synthesis" is the right description for this paper; the authors identified & assembled the available proxy records and assessed their climate sensitivity but do not synthesise the different records. Just giving a list of some of the records is also not very informative. [Janice Lough, Australia]	Accepted - text revised
5-1384	5	33	38	33	46	Antarctica shows clear wareming in the models in the last century according to Fig 5.12. I think you therefore need to say something about the mismatch with the data. You can also include something about the Antarctic Peninsula based on the new Holocene ice core from James Ross Island (Mulvaney, R., Abram, N.J., Hindmarsh, R.C.A., Arrowsmith, C., Fleet, L., Triest, J., Sime, L.C., Chappellaz, J., Alemany, O., Foord, S., 2012. Holocene ice core perspective on recent warming of the Antarctic Peninsula. Nature 489, 141-144.). This shows the Peninsula warming in its proper context. (submission was well before the deadline) [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
5-1385	5	33	44	33	44	Goosse et al., accepted is now published: Goosse H., M. Braida, X. Crosta, A. Mairesse, V. Masson-Delmotte, P. Mathiot, R. Neukom, H. Oeter, G. Philippon, H. Renssen, B. Stenni, T. van Ommen, E. Verleyen, 2012. Antarctic temperature changes during the last millennium: evaluation of simulations and reconstructions. Quaternary Science Reviews 55, 75-90. [Hugues Goosse, Belgium]	Accepted - new reference included
5-1386	5	33	48	33	53	I suspect that this paragraph will get some modification? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted.
5-1387	5	33	50	33	50	climatology, or temperature? [Government of Australia]	Accepted - text revised
5-1388	5	33	50	33	52	This sentence should be removed as Gergis (2012) has now been rejected by the Journal of Climate. The big problem in this (and other) multi proxy studies is the so called screening fallacy or ex post proxy selection as is done by Gergis. Gergis selected only those proxies that showed good correlation in the calibration period but as has been shown by Stockwell http://landshape.org/images/script.pdf such a procedure generates hockey sticks even with red noise. Co-author Karoly has admitted in email correspondence released after FOI see http://climateaudit.org/2012/10/30/karoly-and-gergis-vs-journal-of-climate/ that this criticism is valid: "If the selection is done on the proxies without detrending ie the full proxy records over the 20th century, then records with strong trends will be selected and that will effectively force a hockey stick result. Then Stephen Mcintyre criticism is valid." Instead of dropping the Gergis reconstruction SOD is now referring to a submitted Science paper which then cites the now rejected Gergis paper. This is another "trick" that IPCC shouldn't use as it severely hurts its credibility. [Marcel Crok, The Netherlands]	The Gergis et al. paper has been removed. Information from the PAGES 2k synthesis paper that has been accepted before the 15 March deadline have been included.
5-1389	5	33	51	33	51	update PAGES 2k Consortium, submitted [European Union]	Accepted.

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5-1390	5	33	51			"… were the warmest in the last 580 years." [Government of France]	Accepted
5-1391	5	33	51			A more clear phrasing of this could be: "…during the past ~580 years, summer temperatures in the post-1950 CE period are warmest…" [Government of United States of America]	Rejected, sentence has not been changed
5-1392	5	33	53	33	53	Which "less confident conclusions"? Please be more specific [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - text modified
5-1393	5	33	55	33	55	Widowed header, please check page layout in final version [Peter Burt, United Kingdom]	Accepted
5-1394	5	33				Pole-Equator-Pole transect of paleoclimate (book V. Markgraf editor) included a summary chapter by Grimm which pointed out what others (especially several Heusser papers) had: That the terrestrial pattern of Holocene temperature change was similar to the northern hemisphere; with higher temperatures in the early Holocene and cooling in the late Holocene. This should be discussed here. [Government of United States of America]	Rejected, focus is on new literature since AR4
5-1395	5	34	1	34	2	I would like to see some reference supporting this statement. Whereas I can agree on that there were regional differences in the timing of the Medieval Climate Anomaly, I am not sure that I have ever read much about seasonal differences and in my own research I have never found any clear evidence for seasonal differences in the temperature signal during the Medieval Climate Anomaly compared to the last few decades. [Fredrik Ljungqvist, Sweden]	Accepted - text modified, there is limited evidence on other than the summer season with respect to the MCA
5-1396	5	34	1	43	15	This section seems to overlap those in various sections. [Government of United States of America]	Noted, but this section relates specifically to the MCA-LIA part
5-1397	5	34	1		2	"… not characterized by a consistent pattern of higher temperatures across seasons and regions." [Government of France]	Accepted - text modified
5-1398	5	34	4	34	4	This sentence is clumsy. [Keith Briffa, United Kingdom]	Accepted - sentence deleted
5-1399	5	34	4			"changes…have been argued" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted - corresponding sentence has been deleted
5-1400	5	34	7			The authors hould clarify what is meant by “La Nina - like” since this variability is clearly different than what is being talked about here [Government of United States of America]	Accepted - text is modified
5-1401	5	34	8			Now we see an AMO/NAO mode of variability! Does this combined acronym have a definite meaning (as opposed to AMO or NAO separately)? If so, an explanation is needed. If not, one or the other should be dropped. [Government of France]	Rejected: The term is defined in the glossary
5-1402	5	34	9	34	9	“have been made responsible” is an odd phrase. [Keith Briffa, United Kingdom]	Accepted - text is modified
5-1403	5	34	12	34	12	update Tierney et al., submitted [European Union]	Accepted - the Tierney et al. paper is now published in Nature and updated
5-1404	5	34	12			Citation alert: If the Tierney et al. reference is only "submitted" and not published in a peer-reviweed journal, then it cannot be used in a report of this nature. Skeptics will attack because of it. [Jay Curt Stager, United States of America]	Accepted - the Tierney et al. paper is now published in Nature and updated
5-1405	5	34	12			Clarification needed: what is the main point here? In what ways are the SST proxy records consistent with hydrological evidence in Africa? [Jay Curt Stager, United States of America]	This point is now clarified
5-1406	5	34	19	34	39	The sea ice section is very incomplete. Available papers indicate nearly Arctic-wide, early Holocene seasonal sea ice cover, pre-industrial perennial sea ice developed in mid-Holocene; this information should be included in this section. [Government of United States of America]	Our assemment is that the data supporting a basin-wide seasonal loss of sea ice is not supported by recent literature to the degree that one can make a statement. Assesment is that the data is unsufficient
5-1407	5	34	19	34	43	I think the sea-ice section needs a graph to show what we know about its area through time. It's hard to figure out from just this bit of text, and graphs are available in the literature (e.g., Kinnard) [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	We agree that such a graph would be nice, but we think it is premature due to lack of quantitative data from a large enough area of the Arctic
5-1408	5	34	21			What is IP25? It isn't defined here, but may just be the name of the new proxy, however that isn't clear.	Accepted, will be defined in revised text

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						[Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	
5-1409	5	34	23	34	23	Insert space after full stop [Peter Burt, United Kingdom]	Accepted
5-1410	5	34	23			Space required at the beginning of the sentence [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1411	5	34	23			… 2011). In general … (typo) [Government of France]	Accepted
5-1412	5	34	24	34	25	Subsection 5.5.2.: Some major contributions published between 2006 and 2012 are not cited, it narrows the focus of this subsection. Please, if possible add the following ref.: 1. Ledu D, Rochon A, de Vernal A, Barletta F & St-Onge G 2010 Holocene sea ice history and climate variability along the main axis of the Northwest Passage, Canadian Arctic, Paleoceanography 25 PA2213 doi:10.1029/2009PA001817 2. de Vernal A & Hillaire-Marcel C 2006 Provincialism in trends and high frequency changes in the northwest North Atlantic during the Holocene Global Planet. Change 54 263–90 and 3. McKay J L, de Vernal A, Hillaire-Marcel C, Not C, Polyak L & Darby D 2008 Holocene fluctuations in Arctic sea-ice cover: Dinocyst-based reconstructions for the eastern Chukchi Sea Can. J. Earth Sci. 45 1377–97 [Frédérique Eynaud, France]	It will be difficult to include all relevant references due to space limitations. We believe the most important references are there
5-1413	5	34	29	34	31	Following paper should also be referred since it shows reduction of sea ice in 6ka winter and is newer than Otto et al. 2009 and Zhang et al. 2010. O'ishi, R., and A. Abe-Ouchi (2011), Polar amplification in the mid-Holocene derived from dynamical vegetation change with a GCM, Geophysical Research Letters, 38, L14702, doi: 10.1029/2011GL048001. [Ryouta O'ishi, Japan]	Rejected, the section focuses on proxy data
5-1414	5	34	34	34	34	"parallel regional SST" - is that instrumental SST or reconstructed? [Janice Lough, Australia]	Accepted, will be explained in revised text
5-1415	5	34	35			Section 5.5.3. This section seems too short for how improtant the ideas in it are. It may be better covered in Chapter 4, or it could be expanded upon here. [Government of United States of America]	All of Ch5 have strong space limitations. Ch4 does not deal with proxy based evidence
5-1416	5	34	41	34	41	Other sea ice records from the SH should be mentioned here, such as Curran et al., Science [Government of Australia]	Accepted
5-1417	5	34	41	34	43	The rapid and large amplitude shift in diatom abundances in MD03-2601 is probably due to threshold response of the micro-organisms to a continuous decrease in SST and continuous increase in sea ice cover (Crosta et al., 2007; Denis et al., 2010). Core JPC24 does not show the same abrupt change, but a more gradual one, which is supported by modelling studies (Renssen et al., 2005, Holocene 15). the phrasing should be more careful. [Government of France]	Accepted
5-1418	5	34	41	34	43	There are some records of Southern Ocean sea ice for the Holocene , e.g. Barbara, L., X. Crosta, G. Massé, and O. Ther (2010), Deglacial environments in eastern Prydz Bay, East Antarctica, Quat. Sci. Rev., 29(19-20), 2731-2740, doi:10.1016/j.quascirev.2010.06.027. Hall, B. L., T. Koffman, and G. H. Denton (2010), Reduced ice extent on the western Antarctic Peninsula at 700-970 cal. yr B.P, Geology, 38(7), 635-638, doi:10.1130/g30932.1. Michalchuk, B. R., J. B. Anderson, J. S. Wellner, P. L. Manley, W. Majewski, and S. Bohaty (2009), Holocene climate and glacial history of the northeastern Antarctic Peninsula: the marine sedimentary record from a long SHALDRIL core, Quat. Sci. Rev., 28(27-28), 3049-3065, doi:10.1016/j.quascirev.2009.08.012. The section also does not mention glacial sea-ice distribution in the Southern Ocean, e.g. Gersonde , R., X. Crosta, A. Abelmann, and L. Armand (2005), Sea-surface temperature and sea ice distribution of the Southern Ocean at the EPILOG Last Glacial Maximum—a circum-Antarctic view based on siliceous microfossil records, Quat. Sci. Rev., 24(7-9), 869-896, doi:10.1016/j.quascirev.2004.07.015. [William Howard, Australia]	Revised text will evaulate which references are most central, but space limitations prohibit citing all relevant literature
5-1419	5	34	41	34	43	While post AR4 change is the emphasis here, there has yet been no improvements on the work of Curran et al., 2003 which showed proxy evidence of 20th Century sea ice decline. Suggest adding " Ice core proxy data for East Antarctica (Curran et al., 2003) show evidence for a ~20% decline in sea ice extent in the 20th	Noted, ref is not new since AR4.

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						Century prior to the satellite era." reference follows: [Tasman van Ommen, Australia]	
5-1420	5	34	41	34	43	Curran, M.A.J., van Ommen, T.D., Morgan, V.I., Phillips, K.L., Palmer, A.S. (2003) Ice Core Evidence for Antarctic Sea Ice Decline Since the 1950s. Science 302. 1203-1206 [Tasman van Ommen, Australia]	Noted, ref is not new since AR4.
5-1421	5	34	41	34	43	Why is only one paper mentioned here, when there are several marine cores and some ice core sodium data for this period? I could suggest marine cores: Steig (Steig, E.J., Hart, C.P., White, J.W.C., Cunningham, W.L., Davis, M.D., Saltzman, E.S., 1998. Changes in climate, ocean and ice-sheet conditions in the Ross embayment, Antarctica, at 6 ka. Ann. Glaciol. 27, 305-310.) for the Ross Sea, and Hodell (Hodell, D.A., Kanfoush, S.L., Shemesh, A., Crosta, X., Charles, C.D., Guilderson, T.P., 2001. Abrupt cooling of Antarctic surface waters and sea ice expansion in the South Atlantic sector of the Southern Ocean at 5000 cal yr B.P. Quaternary Res. 56, 191-198.) for the Weddell Sea. These also suggest increasing sea ice during the later half of the Holocene. Ice core sodium records confirm this finding for the ocean offshore Dome C (Wolff, E.W., Rankin, A.M., Rothlisberger, R., 2003. An ice core indicator of Antarctic sea ice production? Geophys. Res. Lett. 30, 2158), DML (Fischer, H., et al, 2007. Reconstruction of millennial changes in transport, dust emission and regional differences in sea ice coverage using the deep EPICA ice cores from the Atlantic and Indian Ocean sector of Antarctica. Earth planet. Sci. Lett. 260, 340-354.), Dome Fuji (Iizuka, Y., Hondoh, T., Fujii, Y., 2008. Antarctic sea ice extent during the Holocene reconstructed from inland ice core evidence. J. Geophys. Res. 113, D15114.) and Taylor Dome (Steig, E.J., Morse, D.L., Waddington, E.D., Stuiver, M., Grootes, P.M., Mayewski, P.A., Twickler, M.S., Whitlow, S.I., 2000. Wisconsinan and Holocene climate history from an ice core at Taylor Dome, western Ross Embayment, Antarctica. Geogr. Ann. Ser. A-Phys. Geogr. 82A, 213-235.) This section seems inadeuqately researched to me. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Partly accepted, due to space limitations and the need to give priority to post AR4 citations, it will not be possible to provide a lengthy reference list. Some additions made in revision.
5-1422	5	34	45	35	45	Possibly add a short paragraph on glaciers at Mt Kilimanjaro/ Mt Kenya since in media glacier retreat here is often wrongly discussed as a sign of global warming in the tropics [European Union]	Basically this question belongs to ch 4 Cryosphere and it was discussed in AR4.Discussed with Georg Kaser in Ch 4 and he confirmed that there is no important new literature avaliable since the AR4 on this topic. Rejected.
5-1423	5	34	45			The LIA glacial moraines are quite large even in context for the whole Holocene in many places. As such it seems artificial to deconstruct glacier discussion into Holocene and last millennium. [Government of United States of America]	Rejected: The point was to discuss the 2ka glacier variations in more details (lines 21-45) is the availability of the more reliable and higher resolution reconstructions for this period
5-1424	5	34	49	34	49	Write "smoothed" instead of "filtered by glaciers". [Manfred Mudelsee, Germany]	Accepted, changed as suggested by the reviewer
5-1425	5	34	53	34	54	The text refers to newly published data: “Various techniques have been extensively applied to date moraines and have significantly improved many chronologies of glacier variations...”. It is however missing some key articles. The reference should be updated to (Davis et al., 2009; Larsen et al., 2011, 2012). The additional articles are Larsen, D., Miller, G.H., Geirsdóttir, Á. 2011. A 3000-year varved record of glacial activity and climate change from the proglacial lake Hvítárvatn, Iceland. Quaternary Science Reviews.30, 2715-2731. doi:10.1016/j.quascirev.2011.05.026. Larsen, D., Miller, G.H., Geirsdóttir, Á., Ólafsdóttir, S. 2012. Non-linear Holocene climate evolution in the North Atlantic: a high-resolution, multi-proxy record of glacier activity and environmental change from Hvítárvatn, central Iceland. Quaternary Science Reviews 39 (2012) 14-25. doi:10.1016/j.quascirev.2012.02.006 [Government of Iceland]	Accepted, the references added as far as space allows
5-1426	5	35	1	35	1	the yellow background of figure 1, Faq 5.1Is quite unpleasant [Peter Clift, United States of America]	Taken into account
5-1427	5	35	5	35	5	See also the new deglacial history results from Pine Island Bay published recently by Hillenbrand et al. in Geology: doi: 10.1130/G33469.1 [Robert Larter, United Kingdom]	Accepted, the reference added as far as space allows
5-1428	5	35	5	35	7	merge these two paragraphs? [Masa KAGEYAMA, France]	Rejected. The first paragraph concerns the general statements on the recent achievements in this field, the second one summarizes the actual trends in the

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							Holocene glacier fluctuations.
5-1429	5	35	7	35	9	This sentence needs rewording as it is hard to understand the main points in the sentence [Government of Australia]	Accepted will re rephrased
5-1430	5	35	10	35	12	Why would extensive glaciers in the early Holocene be a result of orbital forcing when, as stated below in line 33 and is well known, insolation in the northern hemisphere is high during that time, and why would summer T decrease when summer insolation is high? This does not appear to make sense and, therefore, the authors might consider deleting the sentence. [Government of United States of America]	Rejected: We do not say that the glaciers are more extensive in the early Holocene, on the contrary (lines 7-9). We do not quite understand this comment. Probably our text in the beginning of this paragraph is not very well edited in terms of clarity of our message
5-1431	5	35	12	35	12	"as a dynamical response to orbital forcing" --> I don't understand [Masa KAGEYAMA, France]	Accepted, text modied
5-1432	5	35	13			Comma needed after "chronologies" [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1433	5	35	21	35	21	This sentence seems to be a tautology [Government of Australia]	Accepted, remove "by dating"
5-1434	5	35	22			"was suggested" seems the wrong tense - it implies that we don't think that know. Try "has been suggested" instead. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted, changed as suggested
5-1435	5	35	26	35	26	Not sure about the formulation here, notion could be misinterpreted. Better: Global balance is negative? [Thomas Stocker/ WGI TSU, Switzerland]	Text will be modied and we provide a reference for the ch 4 where this problem (the reason of glacier retreat) is discussed.
5-1436	5	35	26	35	41	This discussion of glacier lengths with respect to the Alps needs to mention the fact that material from earlier periods is occasionally found when the glaciers retreat. Much of this is from Roman times and from the mid-Holocene (Oetzi in the very north of Italy). Would these artefacts have been found if their glaciers had retreated further back than today? [European Union]	While we agree, there may be space limitations due to further reductions of subchapter length
5-1437	5	35	26	35	41	If appropriate within the context of WGI, it would be helpful if this area of the text stated not just that summer insolation explains earlier glacial minima, but also whether/to what extent summer insolation can explain current observations. [Government of United States of America]	Accepted, but limited space make it difficult to have a comprehensive treatment
5-1438	5	35	26	35	41	This discussion of glacier lengths with respect to the Alps needs to mention the fact that material from earlier periods is occasionally found when the glaciers retreat. Much of this is from Roman times and from the mid-Holocene (Oetzi in the very north of Italy). Would these artifacts have been found if their glaciers had retreated further back than today? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Identical to comment 1436
5-1439	5	35	28	35	30	I am getting confused as to what time period is being discussed here; also provide referenced for evidence of different glacial extents (i.e. smaller) [Janice Lough, Australia]	Accepted. The references are added. The indication of the time ("in the Holocene") is also included in the sentence (line 30)
5-1440	5	35	29			Better write "shorter" instead of "smaller" (throughout the text), because only length information is available in most papers. Also be more precise with the term "present". Most likely something like "at the turn of the century" is meant. Be precise and mention that ongoing reduction in glacier length is a delayed reaction to climate change and therefore rapidly continues (even for adjusting to climatic conditions of the first decade in our century not to speak about continued warming). [Wilfried Haeberli, Switzerland]	Accepted (shorter instead of smaller - are corrected accordingly through text). At present changed to the "at the turn of the century"
5-1441	5	35	34			What is MCA - hopefully not "Medieval Climate Amelioration": IPCC should avoid such unreflected and subjective terminology (climate is a statistical average of meteorological conditions and cannot "warm", "cool", "ameliorate" or "deteriorate") [Wilfried Haeberli, Switzerland]	Rejected. MCA is explained in the abbreviation list
5-1442	5	35	37			The study by Scapozza et al. deals with creeping permafrost (not glaciers) and past timberlines. It says nothing about "smaller glaciers" but about higher timberlines. These past timberlines reflect summer temperatures about 1.2°C higher than around 1950 - with other words: about the same as the summer temperatures we have since the turn of the century. [Wilfried Haeberli, Switzerland]	Accepted, the reference is deleted
5-1443	5	35	39	35	41	This sentence about glaciers in North America applies ONLY to glaciers in northwestern N.A., i.e. Alaska and adjacent Canada. La Nina-like conditions are wet in the far NW but are dry to the south, so glaciers in the	Accepted, changed as suggested

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						Sierra Nevada and central-southern Rocky Mountains would contract during such dry periods if anything. Therefore, the authors should consider isnerting "northwestern" here. [Government of United States of America]	
5-1444	5	35	43	35	45	Indicate that this "recent" global recession. [Janice Lough, Australia]	Accepted
5-1445	5	35	43	35	45	The available information indicates with medium confidence that the global recession of glacier length is 43 unusual in the context of the last 2 kyr. I think this statement is too strong. The current retreat is indeed in agreement with the temperature rise, but the low dating accuracy and the very fragmentary information on glacier sizes during the MCA precludes any definitive conclusion in this respect. [Olga Solomina, Russia]	Taken into account. The text has been revised.
5-1446	5	35	44	35	44	But what about the influence of precipitation changes? [Government of Australia]	Rejected: The instrumental glacier records and their current retreat is considered in the ch 4.
5-1447	5	35	45			Is the important paleoglaciological information from miniature icecaps and ice patches mentionned? Cf. Farnell, R., Hare, G. P., Blake, E., Bowyer, V., Schweger, Ch. Greer, Sh. And Gotthardt, R. (2004): Multidisciplinary investigations of alpine ice patches in Southwest Yukon, Canada: Paleo-environmental and Paleobiological Investigations. Arctic 57 (3), 247-259. Haeberli, W., Frauenfelder R., Kääb, A. and Wagner S. (2004): Characteristics and potential climatic significance of „miniature ice caps“ (crest- and cornice-type low-altitude ice archives). Journal of Glaciology, 50/168, 129-136. [Wilfried Haeberli, Switzerland]	Rejected: In the Haeberli et al. there are no direct evidence on the area or length changes of the ice caps under consideration. In the Farnrell et al. paper there is a statement on the lack of accumulation in certain periods, but the length variations are not assessed on this basis. It would be difficult to incorporate this data into our short text.
5-1448	5	35	47	35	47	The section 5.5.4 and discussions on polar records, abrupt changes and the instrumental record are particularly impressive [Government of Australia]	Noted
5-1449	5	35	49	35	49	"higher resolution" - compared to what? [Janice Lough, Australia]	Accepted. Changed to "new high resolution", first paragraph moved to 5.3.2.3
5-1450	5	35	49	35	50	Add reference Cruz et al. (2009. Nature Geoscience, doi:10.1038/NGEO444) who provided novel insights into the dynamics of tropical precipitation over lowland South America. [Government of Brazil]	Accepted; first paragraph moved to 5.3.2.3
5-1451	5	35	49	36	32	Section 5.5.4 Monsoon Systems and Convergence Zones p. 5-36 –Nothing is said about the North American Monsoon and reconstruction of the ITCZ in the eastern tropical Pacific For example, Leduc et al. (2009) Quat. Reasearh 72, 123-131 Sachs et al. (2009) Nature Geoscience 2, 519-524 Toth et al. (2012) Science 337, 81-84 Barron et al. (2012) Paleoceanography 27, PA3206 Feng et al. (2008) J. Geophys Research 113, D11101 [Government of United States of America]	We added the sentence "Rainfall patterns associated with the Pacific ITCZ also shifted southward during the MCA/LIA transition in the central equatorial Pacific (Sachs et al., 2009). " Leduc focuses on the entire Holocene (not subject to this section), the climate dynamical interpretation of the Toth paper is quite speculative. Some work on North American hydroclimate changes during the last milennium is referred to in the Megadrought subsection.
5-1452	5	35	51	35	52	Delete reference Weldeab et al. (2006. Earth and Planetary Science Letters, doi:10.1016/j.epsl.2005.11.012) since the authors make no statements at all about the South American Monsoon or any other monsoon system. [Government of Brazil]	Accepted; first paragraph moved to 5.3.2.3
5-1453	5	35	55	35	57	“East Asian monsoon speleothem δ18O is suggested to be affected by large-scale moisture transport in addition to local precipitation.” This discussion should not be limited to the “East Asian monsoon” only, because the speleothem δ18O records have remarkable features shared by different monsoon systems in Asia and South America (Cheng et al., 2012, Climate Dynamics, 39: 1045). [PINXIAN WANG, China]	Accepted, changed to "speleothem δ18O in some monsoon regions "; first paragraph moved to 5.3.2.3
5-1454	5	35	56			LeGrande and Schmidt 2009 CP is worth mentioning here too. It says speleothem d18O related to moisture advection onto land. [Government of United States of America]	Accepted; first paragraph moved to 5.3.2.3
5-1455	5	36	7	36	12	The recent work concerning the East Asian summer monsoon changes during the last millennium using the millennial simulations with a comprehensive Earth system model developed at Max Planck Institute for Meteorology that, in contrast to previous studies, offers an (albeit small) ensemble of simulations over the last millennium, should be added in this paragraph. Analysis of the simulations indicates that the EASM is generally strong during the Medieval Warm Period (MWP, AD 1000-1100) and weak during the Little Ice Age	The Man et al. (2012) reference has been included. However, a detailed discussion of the mechanisms is beyond the limited scope of this short section.

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						(LIA, AD 1600-1700). Both similarities and disparities of the monsoon change and corresponding rainfall pattern between the model results and the proxy data have been compared, reconstructions from Chinese historical documents and some geological evidence support the results. The land-sea thermal contrast change caused by the effective radiative forcing lead to the MWP and LIA monsoon changes (Man et al. 2012). These insights are directly relevant to the topic of discussion here and should be addressed. For example, "The recent simulations using a comprehensive Earth system model suggest that the land-sea thermal contrast change caused by the effective radiative forcing lead to a stronger (weaker) East Asian summer monsoon during the MCA (LIA)." This comment may be added before "Hydrological proxy data characterizing…" on line 13 of page 36 [Man, W., T. Zhou and J. Jungclaus, 2012: Simulation of the East Asian Summer Monsoon d uring the Last Millennium using MPI-M Earth System Model. Journal of Climate,25(22): 7852-7866.]. [Tianjun Zhou, China]	
5-1456	5	36	7	36	12	The millennial evolution of NAO, PDO, AAO are compared between the coupled model simulation and reconstruction in the following paper: Man WenMin, Zhou TianJun, 2011: Forced Response of Atmospheric Oscillations during the Last Millennium Simulated by a Climate System Model, Chinese Science Bulletin , 56, 3042-3052 [Tianjun Zhou, China]	Noted. We have not implemented this suggestion, because section focuses on Monsoons and Convergence zones and not on NAO,PDO and AAO
5-1457	5	36	7	36	12	The climate of Medival Warm Period and Little Ice Age are compared to that of the 20th century in the following paper, the results indicated differences in the spectral peaks of interannual East Asian monsoon variability during MWP, LIA and 20th century: ZHOU TianJun, LI Bo, MAN WenMin, ZHANG LiXia,& ZHANG Jie, 2011:A Comparison of the Medieval Warm Period, Little Ice Age and 20th Century Warming simulated by the FGOALS Climate System Model, Chinese Science Bulletin,56: 3028-3041 [Tianjun Zhou, China]	Noted. This section focuses on low-frequency changes in the monsoons, not on its interannual variability
5-1458	5	36	8			Change "for North and South African monsoon, Indian and East Asia summer monsoon" to "for the North and South African monsoon, and the Indian and East Asian summer monsoons" [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1459	5	36	9			Why is there a hyphen in paleo-climate? [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted, Hyphen deleted
5-1460	5	36	16	36	17	Vuille et al 2012 is now published (Clim. Past, 8, 1309–1321, 2012) and in this article the author give more importance to the southward migration of ITCZ during LIA than to the northward migration during MCA and indeed in Amazonia humid LIA is more marked than dry MCA. But a new lacustrine record from Northeast Brazil (Zocatelli et al., palaeo3 363–364 2012 127–134) indicates a dry lake during LIA. One record only do not give a high confidence level to this evidence but other works in preparation from other lakes and stalagmites in the same region confirm this dry LIA and I am thus confident on this phase. Therefore the influence of ITCZ on monsoon must be modulated: e.g; at line 27: "register of dryness in Northeast Brazil indicate no southward shift of ITCZ during LIA in tropical Atlantic" [Bruno TURCQ, France]	The Vuille reference has been updated. The text has been updated to "Hydrological proxy data characterizing the intensity of the East Asian monsoon (South American monsoon) show decreased (increased) hydrological activity during the LIA as compared to the MCA (medium confidence) (Figure 5.4f,i) (Bird et al., 2011; Vuille et al., 2012; Zhang et al., 2008). " The multi-proxy synthesis provided in Vuille is very compelling and is not easily challenged by one published record showing something different. We have hence decided not to include the Zocatelli reference.
5-1461	5	36	18	36	18	Another paper on meagadroughts is by Sinha et al. (2011). Write "(Buckley et al., 2010; Cook et al., 2010b; Sinha et al., 2011)". The reference is: Sinha A, Stott L, Berkelhammer M, Cheng H, Edwards RL, Buckley B, Aldenderfer M, Mudelsee M (2011) A global context for megadroughts in monsoon Asia during the past millennium. Quaternary Science Reviews 30:47–62. [Manfred Mudelsee, Germany]	Accepted - reference will be included
5-1462	5	36	28	36	28	Anchukaitis and Tierney, submitted - I really don't think you can cite submitted papers [Peter Clift, United States of America]	Accepted, the following references in now used: Tierney, J.E., J.E. Smerdon, K.J. Anchukaitis, and R. Seager (2013), Multidecadal variability in East African hydroclimate controlled by the Indian Ocean, Nature, 493, 389-392.
5-1463	5	36	28	36	32	i.e. This suggested Asian monsoon response to volcanic forcing disagrees with model simulations; Also is there instrumental observations that support this proposed volcanic signal in Asian monsoon rainfall? [Janice Lough, Australia]	Accepted, we modified text: Anchukaitis et al. 2010 discusses paleo (Monsoon Asia Drought Atlas) vs models. Observations complicated by El Ninos (which

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							cause drought) during the 3 major observed eruptions. Zhange et al. 2013 looks at model response as function of ENSO state.
5-1464	5	36	34	36	37	This sentence should be revised. Because the monsoon proxy data reconstructed for the last few millennia for eastern Asia (Zhang et al ,2008) is being argue, this eastern Asia monsoon curve established from a single speleothems differ from the EAM curve (see IPCC. Climate Change 2007, Figure3.35 p-297), and a paper indicates that “Taking paleo-proxy records from a single locality in eastern China to imply changes in drought/wetness of China is fraught with uncertainty. On decadal scales, the that δ18O in speleothems can be used as a proxy for the EAM strength lacks empirical underpinnings"(Zhang De’er, Li Hong-Chun Ku Teh-Lung, Lu Longhua, 2010, On linking climate to Chinese dynastic change: Spatial and temporal variations of monsoonal rain, Chinese Science Bulletin 2010（1）77-83) [De-Er Zhang, China]	The review comment can be generalized also to other monsoon regions. We have hence added the sentence "Given the large spatial extent of these monsoon regions, the presence of complex topographic features, and the climatic effects from remote regions, it is conceivable that the multi-decadal-to-centennial rainfall patterns exhibit spatial inhomogeneities that can complicate hydroclimate reconstruction efforts"
5-1465	5	36	34	36	40	The relationship between East Asian monsoon and precipitation in China is complex. In 5.5.4, it is controversial to use oxygen isotope components (Zhang et al., 2008) only to indicate monsoon strength and draught. It is recommended to revise by referring to the following literature. Suggest to add ", although existed argue for some Asia monsoon proxy data (Zhang et al., 2010)" or ", although the uncertainty of eastern Asia monsoon proxy is notable(Zhang et al., 2010) before the full point. or insert " Notable discrepancies on dry spells between the monsoon proxy data(Zhang et al., 2008) and dry-wet indices from historical records (Zhang et al., 2010; Zheng et al., 2006) ." before "proxy data " . (P36L37-P36L37).（Zhang De'er & Lu Longhua. 2007. Anti-correlation of summer and winter monsoons? [J]. Nature, 450: E7-E8. Doi: 10.1038/nature063; Zhang De’er, Li Hong-Chun Ku Teh-Lung, Lu Longhua. 2010. On linking climate to Chinese dynastic change: Spatial and temporal ariations of monsoonal rain, Chinese Science Bulletin, 1: 77-83）。 [Lei Huang, China]	The review comment can be generalized also to other monsoon regions. We have hence added the sentence "Given the large spatial extent of these monsoon regions, the presence of complex topographic features, and the climatic effects from remote regions, it is conceivable that the multi-decadal-to-centennial rainfall patterns exhibit spatial inhomogeneities that can complicate hydroclimate reconstruction efforts"
5-1466	5	36	34	36	40	and is there any agreement on timing of these "extended intervals of monsoon failures"? Again, critical synthesis required. [Janice Lough, Australia]	The robust timings (differences between LIA, MCA) were already discussed in previous sections. Of course, there is a large amount of variability in the timing, depending on how large-scale the drought is. This is now captured by the sentence "Given the large spatial extent of these monsoon regions, the presence of complex topographic features, and the climatic effects from remote regions, it is conceivable that the multi-decadal-to-centennial rainfall patterns exhibit spatial inhomogeneities that can complicate hydroclimate reconstruction efforts. "
5-1467	5	36	36	36	36	Nield et al., GRL, 2012 reconstruct an increase in accumulation across the Antarctic Peninsula since 1850s leading to tens of metres of net ice increase. Nield, G.A., P.L. Whitehouse, M.A. King, P.J. Clarke and M.J. Bentley 2012. Increased ice loading in the Antarctic Peninsula since the 1850s and its effect on Glacial Isostatic Adjustment. Geophysical Research Letters, 39: L17504 doi:10.1029/2012GL052559. [Matt King, Australia]	Rejected, belongs in Ch4
5-1468	5	36	37	36	37	Suggest to add ", although existed argue for some Asia monsoon proxy data (Zhang et al 2010)" before the full point , or insert " Notable discrepancies on dry pells between the monsoon proxy data(Zhang et al 2008) and dry-wet indices from historical records (Zhang etal 2010,Zheng et al 2006) ." before “Proxy data " [De-Er Zhang, China]	Accepted - text modified
5-1469	5	36	37	36	40	Chaotic, grammatically wrong sentence! [Manfred Mudelsee, Germany]	Accepted - text modified
5-1470	5	36	42	36	42	The report appears to be unaware of a new study published earlier this year that indicates both the drying of the South Asian environment after 4000 years ago but also to further aridification around 1700 years ago and up. From there to the present day in which the climate was marked by both very dry and very wet periods of short duration. Ponton et al. 2012, Geophys res Letts 39 L03704 [Peter Clift, United States of America]	Accepted, the paper will be included, paper in question appears to address long-term trends

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5-1471	5	36	42	36	42	Do the mega-drought or a mega-flood have a spatial or temporal scale ? [Government of France]	Megadroughts are typically whose cumulative severity or duration exceed observed drought, typically on decadal or multidecadal time scales
5-1472	5	36	42	37	3	For section 5.5.5, it is suggested that the authors consider dropping the term 'Megadroughts' and use the term 'Drought'. The sentence defining 'Megadroughts' on page 36, lines 45-46, illustrates the problem: "Megadroughts are typically whose cumulative severity or duration exceed observed drought, typically on decadal or multidecadal time scales". There is a simple statistical explanation for why we would not expect to observe the largest possible droughts in the rather short observed drought record that is at best 100 or 150 years. A 100-year event has approximately a 63.4% chance of occurring in any 100-year period, not a 100 percent chance of occurring. Said another way, there is a 37% probability that a 100 year record does not include the 100-year drought of record. When it comes to a multidecadal drought, there is a 95% probability that a 100-year record does contain the 30-year drought of record. Pe = 1 - (1-1/T)^^N T = return period N = length of record Pe = probability of exceedance. So these are not really 'megadroughts' in the paleorecord, just paleodroughts which by chance with a longer data record should have cumulative severity or duration that exceed observed drought. [Government of United States of America]	Rejected: The definition of megadrought is observational, not causal (i.e. megadroughts can certainly occur by chance). In any case we now use the term drought instead of megadroughts
5-1473	5	36	42	38	51	This section dscusses the evidence from paleoclimate proxies regarding past flood and drought frequency in comparison to the present. After listing all the main geographic areas around the world and reporting again and again that the 20th century is unexceptional as regards extreme events like droughts and floods, you draw the appropriate conclusion regarding floods, namely that the main driver for flood activity appears to be natural climatic variability, but you don't summarize droughts. It would be helpful for the reader if you offer a concluding statement on both flood and drought material such as "Across the world, many records indicate that with regards to precipitation, the 20th century was probably not a uniquely extreme climatic period in the past several millennia." [Ross McKitrick, Canada]	Partly accepted. A final sentence about drought has been added, which differs from this suggestion, and is based on the assessment.
5-1474	5	36	42	39	18	Doesn't refer to recent work on lake sediments using gypsum hydration water extraction. There have been a lot of recent developments in the study of droughts in this area. [Government of United Kingdom of Great Britain & Northern Ireland]	Noted but the lack of proposed references makes difficult to refer to adequate publications. Chapter 5 cannot review all proxy developments due to space limitations.
5-1475	5	36	42	39	18	Section 5.5.5 Megadroughts and Floods: Text was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected with considered text. [Dirk Thielen, Venezuela]	Thank you
5-1476	5	36	42	39	20	This chapter mainly describes palaeoclimate records, but fails to give adequate attention to regional differences, in particular, the evidence for dry-humid, devastating draught and flood changes in the context of different warm and cold periods in East Asia over last two thousand years. It is recommended to revise 5.5.5 according to the following literature and suggest to add a sentence "Longer and more severe droughts than today are seen in particularly during the last millennium from China using literature (Zhang, 2005 , Zhang et al., 2010)" before "Longer and more". (Zhang & Liang, 2010; Zhang, 2005)(P36L49-P39L49). (De'er Zhang, Youye Liang. 2010. A Long Lasting and Extensive Drought Event over China in 1876-1878, Advances in Climate Change Research, 1(2): 37-45. www.climatechange.cn DOI:10.3724/SPJ.1248.2010.00037 ; Zhang De'er, 2005, Severe drought events as revealed in the climate records of China over the last thousand years , ACTA METEOROLOGICA SINICA, 19(4): 485-491.)。 [Lei Huang, China]	Partly accepted within space limitations
5-1477	5	36	44	37	3	It is a concern that all references and information provided by this paragraph deal with drought in the western United States. No information is provided on long-term drought changes in boreal regions or eastern United States or Mexico. This literature review is largely incomplete. Below are some recent references on drought changes in Canada that may be considered in revising this paragraph: // Watson, E., Luckman, B.H. 2005	Partly accepted within space limitations

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						Spatial patterns of preinstrumental moisture variability in the southern Canadian Cordillera Journal of Climate 18 (15), pp. 2847-2863 // Laird, Michels, A., K.R., Wilson, S.E., Thomson, D., Leavitt, P.R., Oglesby, R.J., Cumming, B.F. 2007 Multidecadal to millennial-scale shifts in drought conditions on the Canadian prairies over the past six millennia: Implications for future drought assessment Global Change Biology 13 (7), pp. 1295-1307 // Loisel, J., Garneau, M. 2010. Late-Holocene paleoecohydrology and carbon accumulation estimates from two boreal peat bogs in eastern Canada: potential and limits of multi-proxy analyses. Palaeogeography, Palaeoclimatology, Palaeoecology 291:493-533 // Girardin, M.-P., Tardif, J.C., Flannigan, M.D., Bergeron, Y. 2006 Synoptic-scale atmospheric circulation and boreal Canada summer drought variability of the past three centuries Journal of Climate 19 (10), pp. 1922-1947 //St. George, S., Meko, D.M., Girardin, M.-P., MacDonald, G.M., Nielsen, E., Pederson, G.T., Sauchyn, D.J., (...), Watson, E. 2009 The tree-ring record of drought on the Canadian Prairies Journal of Climate 22 (3), pp. 689-710 //Williams, J.W., Shuman, B., Bartlein, P.J. 2009 Rapid responses of the prairie-forest ecotone to early Holocene aridity in mid-continental North America Global and Planetary Change 66 (3-4), pp. 195-207 //Girardin, M.P. 2010. Wildfire risk inferred from tree rings in the Central Laurentians of boreal Quebec, Canada. Dendrochronologia 28: 187–206. // Bunbury, J and K Gajewski. 2009. Postglacial climates inferred from a lake at treeline, southwest Yukon Territory, Canada. Quaternary Science Reviews 28: 354-369 doi: 10.1016/j.quascirev.2008.10.007 // Viau, A and K Gajewski. 2009. Reconstructing millennial-scale, regional paleoclimates of boreal Canada during the Holocene. Journal of Climate 22: 316-330. doi: 10.1175/2008JCLI2342.1 // Laird, K.R., Haig, H.A., Ma, S., Kingsbury, M.V., Brown, T.A., Lewis, C.F.M., Oglesby, R.J., Cumming, B.F. 2012. Expanded spatial extent of the Medieval Climate Anomaly revealed in lake-sediment records across the boreal region in northwest Ontario. Global Change Biology 18 (9) , pp. 2869-2881 // [Government of Canada]	
5-1478	5	36	44	37	3	Again, lacking critical synthesis; At present, just a list of what different reconstructions suggest - an not all may be equally reliable. [Janice Lough, Australia]	Accepted - text is modified
5-1479	5	36	44	37	3	This section should be added some of new results about historical severe droughts in China .(See:Zhang De'er, 2005, Severe drought events as revealed in the climate records of China over the last thousand years , ACTA METEOROLOGICA SINICA ,VOL 19 NO 4 ,485-491.) [De-Er Zhang, China]	Partly accepted within space limitations
5-1480	5	36	44	37	19	If recent evidence for the spatial extent of megadroughts and floods is discussed, reference should also be made to the pioneering work on droughts in China even if it is not as quite as well developed. One possible reference would be Zhang Y, Shao X, Yin ZY, Liang E, Tian Q, Xu Y (2011) Characteristics of extreme droughts inferred from tree-ring data in the Qilian Mountains, 1700−2005. Clim Res 50:141-159. [Iain Robertson, United Kingdom of Great Britain & Northern Ireland]	Partly accepted within space limitations
5-1481	5	36	44	37	26	The nice diagrams shown for the SE Asia monsoon in figure 5.13 are not mentioned at all in the text. Shouldn't these be compared / contrasted with that for North America? And discussed in the text? [Government of United States of America]	Accepted - we added text on the figure 5.13 content
5-1482	5	36	44			Section 5.5.5: Please add citation of Yancheva et al. on Nature (2007) for the drought events during the last 2000 years in China [Hongyan Liu, China]	Partly accepted within space limitations
5-1483	5	36	45	36	45	Megadroughts are typically those whose cumulative… [Matthew Konfirst, United States of America]	Accepted - changes have been made
5-1484	5	36	45	36	45	"typically" two times [Olga Solomina, Russia]	Accepted - changes have been made
5-1485	5	36	45	36	45	This sentence needs rewording – sense is unclear – "typically" twice and use of "whose" is unclear. [Rob Wilson, United Kingdom]	Accepted - changes have been made
5-1486	5	36	45	36	47	Looking at the references provided for this sentence, it appears that the concept of Megadrought is essentially used to described droughts of long-duration in the USA. This sentence could be deleted [Government of	Rejected: Not true, the term does not refer to any particular region

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						Canada]	
5-1487	5	36	45		46	Replace by: Megadrought are typically events which exceed, in severity or duration, the usual drought episodes over periods of a decade or more. [Government of France]	Accepted - changes have been made
5-1488	5	36	45			Try “Megadroughts typically have a cumulative severity or duration that exceeds ….” [David Sauchyn, Canada]	Accepted - changes have been made
5-1489	5	36	47	36	47	Figure reference should be to 5.12d not 5.13. In fact reference to figure file seem to be wrong for a while. [Rob Wilson, United Kingdom]	Accepted
5-1490	5	36	47	36	52	The information provided by the millennium drought reconstruction presented in figure 5.13 f is limited to the south-western United States. It is unrepresentative of drought variability elsewhere in North America. The location 'North America' appearing on lines 48, 51 and 57 needs to be corrected for 'southwestern United States'. [Government of Canada]	Accepted
5-1491	5	36	48			"Heeinrich stadials" -> "Heinrich stadials" [Government of France]	Changed accordingly
5-1492	5	36	49	36	49	Suggest to add a sentence " Longer and more severe droughts than today are seen in particularly during the last millennium from China using literature (Zhang De'er 2005 , Zhang et al 2010) " before "Longer and more ". [De-Er Zhang, China]	Accepted
5-1493	5	36	49	36	52	I think that such"Longer and more severe droughts than today are seen in particularly during the MCA (Cook et al., 2010b; Meko et al., 2007)" maily existed in US, but it is opposite in China . A paper indicates that "The total 15 severe droughts are discovered for the 960-1911 in China based on historical literature records, and the droughts are extensive to envelope more than 4 provinces and so persistent as to cover 3 yr ,some lasting beyond 5 yr even more." and "The 15 droughts may either occur in a warm or cold climate background, with 11 of the 15 cases in the cold phase, whose winter half-year mean is lower than the average over 1951-1980 values. This indicates the difference in climate correspondence between China and North America, showing the severity of the events in China to be in a cold instead of a warm climate situation. That is likely to relate to the monsoon climate in East Asia." (See:Zhang De’er, 2005, Severe drought events as revealed in the climate records of China over the last thousand years, ACTA METEOROLOGICA SINICA ,VOL 19 NO 4 ,485-491.) [De-Er Zhang, China]	Partly accepted - differenciation is provided within the existing text
5-1494	5	36	49	36	52	In addition, it is shown clearly in the fig.3 of same paper(Zhang De'er,2005) that for the total 15 severe droughts during the 960-1911,there are 3 events in MCA only ,but 11 cases in LIA. [De-Er Zhang, China]	Rejected - due to limited space, an assessment on different single floods cannot be provided
5-1495	5	36	49	36	52	So ,suggest to add "in US "after "Meko et al . 2007)" on Line-50, and insert a sentence " But the difference in climate correspondence between North America and China, showing the severity of the events in China to be in LIA instead of MCA. That is likely to relate to the monsoon climate in East Asia.(Zhang De’er, 2005)" before "In contrast" on Line-52 [De-Er Zhang, China]	Accepted
5-1496	5	36	50	36	50	Delete 'in' [Peter Burt, United Kingdom]	Accepted
5-1497	5	36	54			But other data in Pacific Northwest from British Columbia shows warm, dry Medieval warm time…(R. Mathewes or T. Lacourse) [Government of United States of America]	Rejected due to space restrictions
5-1498	5	36	56	37	3	Literature exists showing that (centennial-scale) aridity cycles have prevailed in central grasslands of North America for much of the Holocene, regularly alternating between drier and wetter conditions. In response, different landscape modes and processes have prevailed at different times. Insight into this natural range of moisture variability should be provided. High resolution records that could be included are: Grimm et al. 2011. A high-resolution mineral, pollen and charcoal record of climatic variability and landscape response from Kettle Lake in the Northern Great Plains of North America. Quaternary Science Reviews, 30: 2626-2650. Brown et al. 2005. Fire cycles in North American interior grasslands and their relation to prairie drought. Proceedings of the National Academy of Sciences, 102: 8865-8870. Clark et al. 2002. Ecology, 83: 595-601. [Government of Canada]	Partly accepted within space limitations
5-1499	5	37	5	37	6	Reference is missing here [Hans Linderholm, Sweden]	Accepted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-1500	5	37	5	37	6	Some references are needed here. [Fredrik Ljungqvist, Sweden]	Accepted
5-1501	5	37	5	37	6	The multi-centennial drought phase in Scandinavia during the medieval time documented from tree-ring based reconstructions is not consistent with the persistent positive phase of the NAO modelled by Trouet et al., 2009 for the MCA (page 29, lines 32-33). A positive NAO would produce precipitation increase in Scandinavia at that time. [Maria Fernanda Sanchez Goñi, France]	Rejected: Trouet et al. do not explicitly report about drought/wet conditions in Scandinavia during the MCA. The study mentioned in the text refers not to winter as Trouet et al. does
5-1502	5	37	5			Is a reference missing for Scandinavian drought in tree rings? [Government of United States of America]	Reference is provided
5-1503	5	37	8			Wilson et al., (2012), comma should be removed after "et al." [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Accepted
5-1504	5	37	9			Buntgen et al., (2011b), comma should be removed after "et al." [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Accepted
5-1505	5	37	13	37	13	Touchan et al (2008; 2011) point is missing after al. [Olga Solomina, Russia]	Changed accordingly
5-1506	5	37	15	37	15	What is “the wide spread medieval drought”? [Keith Briffa, United Kingdom]	We made this point more clearly
5-1507	5	37	15			Typographic error. I presume that the word should be "widespread" [Iain Robertson, United Kingdom of Great Britain & Northern Ireland]	Changed
5-1508	5	37	16			Citation alert: If the Tierney et al. reference is only "submitted" and not published in a peer-reviweed journal, then it cannot be used in a report of this nature. Skeptics will attack because of it. [Jay Curt Stager, United States of America]	Accepted, the following references in now used: Tierney, J.E., J.E. Smerdon, K.J. Anchukaitis, and R. Seager (2013), Multidecadal variability in East African hydroclimate controlled by the Indian Ocean, Nature, 493, 389-392.
5-1509	5	37	17	37	17	“provides” should be “provide” [Keith Briffa, United Kingdom]	Accepted
5-1510	5	37	17	37	19	The Chase et al data only refer to the northern part of South Africa where rains mainly fall in summer (linked to tropical (ITCZ system). The situation in the southernmost part of South Africa where rains come in winter was the exact opposite, with a distinctly WETTER Little Ice Age interval and drier MCA related to shifts in the austral westerlies (Stager et al., 2012,Climate of the Past 8:877-887). [Jay Curt Stager, United States of America]	Accepted - changes have been made
5-1511	5	37	18	37	19	Why the change in MCA from 900-1250 to 900-1300 CE? Is this difference significant here and if so why? You say “potentially related to solar forcing”. If there is good evidence it is important and deserves to be better described. If the evidence is not convincing you should say so explicitly. [Keith Briffa, United Kingdom]	Accepted - changes have been made
5-1512	5	37	18		19	Delete: "potentially related to solar forcing". There is no indication of any scientific basis for this statement. Note: everything is potentially related to everything else! [Government of France]	Accepted
5-1513	5	37	21	37	26	Evidences for droughts along the Peruvian Andes (e.g Bird et al 2011 previosly cited in the section 5.5.4) are not mentioned and incorporated accordly. [Eugenia M. Gayo, Chile]	Accepted - changes have been made
5-1514	5	37	21	37	26	The work by Neukom et al. 2010 provides seasonal patterns of variations in precipitation that should be discussed in this paragraph. The Neukom et al. 2010 's abstract states : "The summer and winter reconstructions back to AD 1498 and AD 1590, respectively, provide new evidence for multi-centennial increase in summer precipitation and an opposing decrease in winter precipitation into the 20th century. " Neukom, R., J. Luterbacher, R. Villalba, M. Küttel, D. Frank, P. D. Jones, M. Grosjean, J. Esper, L. Lopez, and H. Wanner (2010), Multi-centennial summer and winter precipitation variability in southern South America, Geophys. Res. Lett., 37, L14708, doi:10.1029/2010GL043680. [Government of Canada]	Accepted - changes have been made
5-1515	5	37	21	37	26	First, ow do these inferred variations for S America compare with Neukom et al (2010. Multi-centennial summer and winter precipitation variability in southern South America. GRL VOL. 37, L14708, doi:10.1029/2010GL043680; Second, is the following also relevant? van Ommen & Morgan (2010) Snowfall increase in coastal east Antarctica linked with southwest Australian drought.	Partly accepted, the Neukom et al. reference is now included, the 2nd one not

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Nature Geoscience 3: 267-272 doi:10.1038/ngeo761 [Janice Lough, Australia]	
5-1516	5	37	22	37	22	Indicate the period when past drought in South America is documented (MCA or LIA). Reuter et al., GRL 2009; Ledru et al., CPD 2012 are not cited. Both indicate precipitations increase during at least a part of the LIA in the southern tropics. Ref.: Reuter et al., 2009, A new perspective on the hydroclimate variability in northern South America during the Little Ice Age, GRL 36: L21706; Ledru et al., 2012, The Medieval Climate Anomaly and the Little Ice Age in the Eastern Ecuatorial Andes, CPD 8: C1598–C1601) [Maria Fernanda Sanchez Goñi, France]	Accepted within space limitations
5-1517	5	37	23	37	23	Another paper on aridity changes in South Amarica, based on tree-rings, is Christie et al. (2011). Write "... Villalba et al., 2009; Christie et al., 2011)." The reference is: Christie DA, Boninsegna JA, Cleaveland MK, Lara A, Le Quesne C, Morales MS, Mudelsee M, Stahle DW, Villalba R (2011) Aridity changes in the temperate–mediterranean transition of the Andes since AD 1346 reconstructed from tree-rings. Climate Dynamics 36:1505–1521. [Manfred Mudelsee, Germany]	Accepted
5-1518	5	37	23	37	25	It must be noted that the conclusion of Boucher et al (2011) are more relative: Although recent changes are in some cases notorious, most were not exceptional at the scale of the last thousand years. [Government of France]	Accepted
5-1519	5	37	23			Boucher et al., (2011), comma should be removed after "et al." [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Accepted
5-1520	5	37	25			Comma needed before Morales [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1521	5	37	25			Morales et al., (2012), comma should be removed after "et al." [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Accepted
5-1522	5	37	26	37	26	van Ommen, T.D. and Morgan, V. 2010, Nature Geoscience, DOI:10.1038/NGEO761 say that rainfall in south-west Australia and precipitation at Law Dome in Antarctica show a relationship. They say that the 'precipitation anomaly of the past few decades in Law Dome is the largest in 750 years, and lies outside the range of variability for the record as a whole, suggesting that the drought in Western Australia may be similarly unusual.' (Drought in south-west Australia described in a number of articles, e.g. Hope, Nicholls, Drosdowsky, 2006, Climate Dynamics and IOCI 2002 (www.ioci.org.au) for decline since late 1960s, Hope P. and Ganter CJ, 2010, IN: Jubb, I., Holper, P. and Cai, W. Managing Climate Change, CSIRO publishing, 53-64 for further decline since 2000. [Government of Australia]	Accepted - reference has been included
5-1523	5	37	29	37	29	Caption is for figure 5.12. Please check all figures and captions. [Rob Wilson, United Kingdom]	Done
5-1524	5	37	29			The authors should consider a rewording to read: "Asian and N American monsoon…" [Government of United States of America]	Accepted
5-1525	5	37	38	38	25	The next 3 paragraphs seem to be about Europe - this needs to be made clear at the start and again, synthesis required rather than just listing what authors inferred. [Janice Lough, Australia]	Accepted
5-1526	5	37	43	37	43	The SREX report (IPCC, 2012) concludes that there is a lack of a direct statistical… [Matthew Konfirst, United States of America]	Accepted - text changed
5-1527	5	37	43	37	44	This sentence read awkward, maybe something is missing here. [Christoph Raible, Switzerland]	Accepted - text changed
5-1528	5	37	43	37	51	"Recent flood frequencies are difficult to evaluate..." Isnt it more generally true that it is difficult to evaluate changes in flooding through time (or to interpret precipitation from this) due to changing geomorphology of the watershed and land use for the entire period of record and not only the recent? [Konrad Gajewski, Canada]	This depends on the land use/change history. Some floods may be enhanced by land use change, but still the watershed needs to receive a high amount of rainfall to produce large floods, as the ones recorded on documentary and sedimentary records. Dams may attenuate or supress medium size floods. In that sense, change on frequency patterns of large floods can be compared along the time from past to present floods.

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5-1529	5	37	43	37	51	It would be helpful (and improve readability) if this area of text included a better bridge between the SREX conclusion and the observations cited in the remainder of the paragraph [Government of United States of America]	Accepted - the first sentence has been changed
5-1530	5	37	43	37	51	Need to make clear what regions are being discussed here or is it global? [Janice Lough, Australia]	Only regions with good, high quality data on flood frequency and magnitude are discussed.
5-1531	5	37	43	37	51	This sentence does not seems to accurately reflect the SREX : the SREX explains that there are limitations in the observations and confounding effects of land-use that makes attribution of floods difficult, but here it seems that you imply that there is no link - which could be a different message. Please clarify using text which is closer to the SREX content. [Philippe Marbaix, Belgium]	Accepted, it is referred to the largest floods, those that dams or river lining are not able to attenuate, text has been changed
5-1532	5	37	43	38	51	This literature review on past floods is missing information from published studies conducted in Canada and Alaska. Below are some references that may provide such coverage. Bunbury, J and K Gajewski. 2009. Postglacial climates inferred from a lake at treeline, southwest Yukon Territory, Canada. Quaternary Science Reviews 28: 354-369 doi: 10.1016/j.quascirev.2008.10.007 Lemay, M., Bégin, Y. 2008 Hydroclimatic analysis of an ice-scar tree-ring chronology of a high-boreal lake in Northern Québec, Canada Hydrology Research 39 (5-6), pp. 451-464 Wolfe, B.B., Hall, R.I., Last, W.M., Edwards, T.W.D., English, M.C., Karst-Riddoch, T.L., Peterson, A., Palmini, R. 2006 Reconstruction of multi-century flood histories from oxbow lake sediments, Peace-Athabasca Delta, Canada Hydrological Processes 20 (19), pp. 4131-4153 Tardif, J. and Bergeron, Y. 1997. Ice-flood history reconstructed with tree rings from the southern boreal forest limit, western Québec. The Holocene 3: 291-300. Clague, J.J., Luckman, B.H., Van Dorp, R.D., Gilbert, R., Froese, D., Jensen B.J.L and Reyes, A.V. 2006 'Rapid changes in the level of Kluane Lake, Yukon Territory, over the last millennium' Quaternary 66(2): 342-355 Hallett, D.J., Hills, L.V. 2006 Holocene vegetation dynamics, fire history, lake level and climate change in the Kootenay Valley, southeastern British Columbia, Canada Journal of Paleolimnology 35 (2), pp. 351-371 Laird, K.R., Cumming, B.F. 2009 Diatom-inferred lake level from near-shore cores in a drainage lake from the Experimental Lakes Area, northwestern Ontario, Canada Journal of Paleolimnology 42 (1), pp. 65-80 Cumming, B. F., Laird, K. R., Bennett, J. R., Smol, J. P. & Salomon, A. K. (2002) Proc. Natl. Acad. Sci. USA 99, 16117-16121 Muller etal 2003 Postglacial climate in the St. Lawrence lowlands, southern Quebec: pollen and lake-level evidence Palaeogeography, Palaeoclimatology, Palaeoecology193 (2003) 51^72 Miousse etal 2003 Isolation and water-level fluctuations of Lake Kachishayoot, Northern Québec, Canada Quat Res 60 (2003) 149–161 Sinnatamby, R.N., Yi, Y., Sokal, M.A., Clogg-Wright, K.P., Asada, T., Vardy, S.R., Karst-Riddoch, T.L., (...), Edwards, T.W.D. 2010 Historical and paleolimnological evidence for expansion of Lake Athabasca (Canada) during the Little Ice Age Journal of Paleolimnology 43 (4), pp. 705-717 Brahney, J., Clague, J.J., Menounos, B., Edwards, T., W.D. 2008 Timing and cause of water level fluctuations in Kluane Lake, Yukon Territory, over the past 5000 years Quaternary Research 70 (2), pp. 213-227 Payette, S., Delwaide, A. 2004 Dynamics of subarctic wetland forests over the past 1500 years Ecological Monographs 74 (3), pp. 373-391 Beriault, A.L., Sauchyn, D.J. 2006 Tree-ring reconstructions of streamflow in the churchill River Basin, northern Saskatchewan Canadian Water Resources Journal 31 (4), pp. 249-262 Case, R.A., MacDonald, G.M., 2003. Tree ring reconstructions of streamflow for three Canadian Prairie Rivers. Journal of the American Water Resources Association 39, 703–716. [Government of Canada]	Rejected: Most of the references are on lake levels and climatology, not on reconstruction of discharge and frequency of floods. Further a couple of references are pre AR4.
5-1533	5	37	43			I'm not too familiar with the SREX report, but I wonder if you are talking about a global signal here. If so, you should stress that. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	It is stressed that the report refers to a global signal.
5-1534	5	37	43			Try “concludes that there is no direct statistical link …” rather then “concludes on the lack of” [David Sauchyn, Canada]	Accepted, changes have been applied

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5-1535	5	37	43			Is the SREX report should be expanded here? [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Rejected: due to space restrictions, not more information on SREX can be provided here
5-1536	5	37	50	37	50	Insert comma after 'However' [Peter Burt, United Kingdom]	Accepted - text revised
5-1537	5	37	51	37	51	replace "despite of " with "in spite of", or "despite" [Peter Clift, United States of America]	Accepted - text revised
5-1538	5	37	51	37	51	...despite of engineering works. [Matthew Konfirst, United States of America]	Accepted - text revised
5-1539	5	37	51			Use either "in spite of" or "despite", not "despite of" [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
5-1540	5	37	51			delete "of" [David Sauchyn, Canada]	Accepted - text revised
5-1541	5	37	51			"despite of" remove "of" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
5-1542	5	37	53	37	53	…the comparison of flood magnitudes along centennial records needs to [Matthew Konfirst, United States of America]	Accepted - text revised
5-1543	5	37	53	38	51	It would be good to present briefly on which data the flood reconstructions are based. [Masa KAGEYAMA, France]	Rejected, the information is provided in the figure caption
5-1544	5	37	53			"comparison….needs" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
5-1545	5	37	55	37	55	Another example of stating 'years ago' instead of years. Does 200 years ago mean about 1800? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Yes, that's part of the language in the paleo community. In this case refers to cal. a BP. This has been included in the text.
5-1546	5	37	55	37	55	...200 years ago was related to the amount… [Matthew Konfirst, United States of America]	Accepted - text revised
5-1547	5	37	56			Contemporaneous with what? Do the authors mean "modern flood magnitudes"? [Government of United States of America]	Deleted contemporaneuous
5-1548	5	38	1	38	58	Along the southern Hemisphere there are evidences for flood event at centennial scales during the past 2,000 years, that are not mentioned in the section 5.5.5. See for example, i-) Gayo, E. M., Latorre, C., Santoro, C. M., Maldonado, A., and De Pol-Holz, R. (2012). Hydroclimate variability in the low-elevation Atacama Desert over the last 2500 years. Climate of the Past 8, 287-306. ii-) F.J. Magilligan, P.S. Goldstein, G.B. Fisher, B.C. Bostick, R.B. Manners, 2008. Late Quaternary hydroclimatology of a hyper-arid Andean watershed: Climate change, floods, and hydrologic responses to the El Niño-Southern Oscillation in the Atacama Desert. Geomorphology 101:14–32. [Eugenia M. Gayo, Chile]	partly accepted - The Goldstein et al paper is now included. The other paper does not address aspects relevant for linking floods with climate variability.
5-1549	5	38	6	38	15	A conclusion here could be that from several papers that floods tend to occur during wet periods? Seems obvious, but is this worth emphasizing more. Floods are much more high-freq info than wet periods. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	It is stated for several regions (the ones indicated).
5-1550	5	38	6		7	… extreme floodings… were not exceptional in the last centuries (...) and, over millenia, record floods... [Government of France]	Accepted - text revised
5-1551	5	38	8	37	8	In Central Europe, long observational (?) records of the …. [Hans Linderholm, Sweden]	Accepted - text revised
5-1552	5	38	8	38	10	More 'years ago' here. Also Macklin et al (2006) is a paper about Europe. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	The Macklin paper includes the British Islands
5-1553	5	38	9	38	9	Write "decrease in *the frequency of* winter floods". [Manfred Mudelsee, Germany]	Accepted - text revised
5-1554	5	38	10	38	10	Insert after "(Mudelsee et al., 2003); (Figure 5.14-f-i)." a sentence of explanation: "This was interpreted as a result from fewer ice-jam occurrences due to regional climate warming." [Manfred Mudelsee, Germany]	Accepted - text revised
5-1555	5	38	10			Use either "show" or "have shown", not "showed" [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised

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5-1556	5	38	11	38	11	"cool and or wet phases" of what? How determined? [Janice Lough, Australia]	Accepted, that is now explained
5-1557	5	38	12	38	12	"Documentary records" - which date back to when? Here, and elsewhere, need to make it clear what time frame is being discussed. [Janice Lough, Australia]	Accepted, this is now more clearly defined. Normally documentary evidence covers the last 500 yrs, sometimes even the last millennium. It can be understood from the time periods indicated as having the higher frequencies.
5-1558	5	38	12	38	15	Have these reduction in winter floods not occurred recently because the Rhine is more controlled than it was? Does this issue on a few large European rivers mean that recent flood trends are not compatible with those earlier. [European Union]	The large floods are difficult to be supressed from the record even if you have a good regulation. There may be attenuate but still the flood peak would be large. In this case, it was used peaks over threshold levels and both present and past records contain such large floods.
5-1559	5	38	12	38	15	Have these floods not occurred recently because the Rhine is more controlled than it was? Does this issue on a few large European rivers mean that recent flood trends are not compatible with those earlier. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	The large floods are difficult to be supressed from the record even if you have a good regulation. There may be attenuate but still the flood peak would be large. In this case, it was used peaks over threshold levels and both present and past records contain such large floods.
5-1560	5	38	17	38	18	The statement is too broad and includes almost all possibilities [Olga Solomina, Russia]	Partly accepted, it is related to the type of rainfall giving rise to flooding (Atlantic fronts or convective storms).
5-1561	5	38	17	38	23	This paragraph starts with an appropriately vague conclusion about flood frequency and climate--rightly so given the conflicting evidence from different places--and then concludes with a definitive statement about decreasing large floods and increasing "extraordinary floods" during the last century. The evidence for this is very weak from the graphs in figure 5.14 and not consistent from record to record (not seen in the French river, for example) and could be a statistical coincidence. Therefore, the authors should consider revising this "high confidence" as it seems to over-state what can be concluded from the information. [Government of United States of America]	Accepted - text revised
5-1562	5	38	17	38	25	Concerning Fig 5.14: it does not show that the 20th century extraordinary floods increased in comparison with 19th century: it is quite similar (Gardon, Segura) or lower (Tagus) ; catastrophic floods have clearly decreased. [Government of France]	Accepted - text revised
5-1563	5	38	19	38	19	960-760 CE? [Hans Linderholm, Sweden]	Accepted - text revised
5-1564	5	38	20	38	24	I would prefer if you used catastrophic rather than large, or combined both words. In effect, I inherently think of "extraordinary" as being greater than "large". The figure abbreviates to CAT instead of L too. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
5-1565	5	38	20			Consider striking the reference to figure 5.14 a, d because these periods 4.8-4.4ka and 960-790 CE are not shown on the figure [Government of United States of America]	Accepted, we deleted the reference to the figure
5-1566	5	38	21	38	21	"extraordinary and ordinary floods" - what does this mean? [Janice Lough, Australia]	Ordinary is deleted. Extraordinary flood meaning is explained in the figure caption
5-1567	5	38	27	38	32	This paragraph is a repetition of what it is said in the previous section. Either rewrite, or summurize mention the previous section [Yueh-Hsin Lo, Taiwan]	Accepted - text revised
5-1568	5	38	27	38	32	The first sentence seems to contradict the remainder of the paragraph; again, lack of critical synthesis. [Janice Lough, Australia]	Accepted - text revised
5-1569	5	38	27	38	32	The section should be added some of new results about historical extreme floodings in China , see: Zhang De'er .A Study of the Large Scale Flooding over Eastern China in 1755, ADVANCES IN CLIMATE CHANGE RESEARCH 3(3): 128-137,2012, www.climatechange.cn,DOI:10.3724/SP.J.1248.2012.00128 [De-Er	Rejected: The section is not addressing single flood events that are not attributed directly to climate change or very specific climate conditions

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						Zhang, China]	
5-1570	5	38	27	38	32	So, suggest to add a sentence " The characteristics of typical historical extreme floods such as 1755 and 1823 are extremely similar to those of 1954 ,And all these three years with extreme floods are corresponding to the minimum phase of the solar activity cycle.(Zhang De'er 2012) " before "In contrast" on Line-31 [De-Er Zhang, China]	Partly accepted - These are three large floods isolated in time without any robust association to a changing trend either in flood frequency or on climate.
5-1571	5	38	28	38	29	Consider re-wording: Yellow and other rivers/ Also, what is meant by "dust falls"? [Government of United States of America]	Accepted - text revised
5-1572	5	38	34	38	34	Correct 'western North America' for 'southwestern United States' because the study cited here (Ely et al.) is only valid for that particular region and not Canada or Alaska. [Government of Canada]	Accepted - text revised
5-1573	5	38	34	38	38	The Ely citations are old, pre AR3. [David Sauchyn, Canada]	Rejected, this is still an important reference and not used in previous IPCC reports
5-1574	5	38	34	38	40	The loose (or unspecified period) association with the MCA does not add to this discussion. How does the evidence here compare with the independent evidence (for high PDSI values) from Ed Cook's work? [Keith Briffa, United Kingdom]	Accepted - text modified. Floods during MCA were more frequent during the last part of the MCA.
5-1575	5	38	34			British "palaeo" not american "paleo" used [I don't care which you use, as long as its consistent throughout] [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
5-1576	5	38	36	38	36	Write "a positive relationship between the *occurrence of* paleofloods". [Manfred Mudelsee, Germany]	Accepted - text revised
5-1577	5	38	42	38	46	There is a very strong high-resolution paleolimnological record for this region now, showing wetter conditions during the Little Ice Age linked to shifts in the austral westerlies: Stager et al 2012, Climate of the Past 8: 877-887. [Jay Curt Stager, United States of America]	Rejected: the Stager et al 2012 paper is on rainfall variability and not specifically on floods
5-1578	5	38	48	38	48	As stated in paragraphs above, this is not true for Central Europe. Moreover, the previous paragraphs indicate a correspondance between wet climates (also the time scale is not given) and floods, not a causality. The conclusion cannot be drawn from what is written before. [Government of France]	Accepted - text revised
5-1579	5	38	48	38	51	This is OK as far as it goes. Perhaps could do with some (too obvious?) statement that IPCC cannot produce hydrologic reconstruction equivalent to NH temperatures. [Keith Briffa, United Kingdom]	Accepted - text revised, we agree that hydrologic reconstruction cannot be addressed at global or hemispheric scales, not only for the past but also over the present and future due to its high variability. Regional reconstruction is possible with the current knowledge.
5-1580	5	38	48	38	51	The authors should consider giving a conclusion about whether recent floods in the last 50-60 years are anomalous relative to those in paleo record [Government of United States of America]	Accepted - text revised
5-1581	5	38	48	38	51	Again, this "high confidence" seems to over-state the findings, since in the Mediterranean, flood frequency apparently coincides with either cool-wet or warm-dry conditions, see topic sentence lines 17-18. [Government of United States of America]	Accepted - text revised
5-1582	5	38	48	38	51	The use of "high confidence..." here is somewhat confusing relative to IPCC likelihood terminology. Also, the second sentence is disconnected as it does not clarify or amplify the "high confidence" statement that precedes it. Is it possible that some text was inadvertently omitted here? [Government of United States of America]	Accepted - text revised
5-1583	5	38	48	38	51	A better conclusion would be that floods tend to occur during wetter periods of higher rainfall. You've said this for several areas. This implies that people are looking at flood and precipitation reconstructions together, but I'm not sure this is the case. There is also an implication that a stronger monsoon means heavier rainfall. If this is case can this be clearly stated. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
5-1584	5	38	48	38	51	I do not think these concluding statements are clearly supported by the evidence provided in its present form. [Janice Lough, Australia]	Accepted - text revised

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-1585	5	38	48	38	52	There should be some mention of the other anthropogenic impacts on floods - i.e. canalization of rivers, reservoir construction, direct water management etc. and the role that plays in minimizing small-to-medium(?) floods, though possibly making large floods worse. This will surely impact the ability to directly compare paleo to modern floods, no? [Gavin Schmidt, United States of America]	Accepted, text revised. Most of the paleoflood records refer to large floods, those that dam cannot manage very well. Of course, river engineeting is introuducing changes that mainly affect to medium and small floods.
5-1586	5	38	48			The conclusion that flood variability is driven by climate variability seems to be obvious; what else would it be driven by? [Government of United States of America]	Partly agreed: The point is that floods are not homogeneous along the time, but their frequency and magnitude have changes in relation to climate variability and some climate conditions are prone to produce larger floods or their related mechanims (moonsoons, El Niño, NAO, etc.).
5-1587	5	38	51			"comparable or surpass in magnitude and/or frequency historical floods" This is very clunky and poor grammar. I suggest to change it to: "comparable or surpass historical floods in magnitude and/or frequency" [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
5-1588	5	38	54			British "palaeo" not american "paleo" used [I don't care which you use, as long as its consistent throughout] [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
5-1589	5	38	58	38	58	…number of floods over the period… [Matthew Konfirst, United States of America]	Accepted - text revised
5-1590	5	38	58			"number of floods" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
5-1591	5	39	1	39	18	In Figure 5.14 caption, all panels are referred as A, B, C and so on, whereas in Figure 5.14, all panels are named as a, b, c and so on [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Accepted - text revised
5-1592	5	39	2			(data from (Benito et al., 2003…2003b), this should be changed as "(data from Benito et al., 2003, 2003b)" [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Accepted - text revised
5-1593	5	39	16	39	16	(I) here on the River Ouse should be € as in the Figure. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
5-1594	5	39	16	39	17	Caption for Figure 5.14: Consider moving the entire description of the River Ouse to be inserted as panel e (not i) in line 11. [Government of United States of America]	Accepted - text revised
5-1595	5	39	18	39	18	Figure 5.14, caption: Alert readers as follows by writing at the end of the caption: "Note that flood frequencies obtained from historical sources may be downbiased in the early part of the reported periods owing to document loss." [Manfred Mudelsee, Germany]	Accepted - text revised
5-1596	5	39	20	45	22	I commend the authors for thorough responses to earlier comments. This is a substantive improvement on the earlier draft. I make one general comment. Perhaps the authors have stepped too far back from technical comment. A figure, info box or page in the appendix is needed to give the technical information on GIA modelling and what the terms used in the text mean. It could be that the explanations in chapter 13 need not be repeated in this chapter but could be referred to here. Alternatively a glossary of terms in Appendix 5a3 might do the jo [Mark Siddall, United Kingdom]	Taken into account. The introduction text calls out an appendix information on GIA.
5-1597	5	39	20			The concept of “eustatic” v. “local” sea level should be introduced early here to allow clearer and more concise discussion about the sea level reconstructions and whether they have local issues etc. Also this section might benefit from having two big sub headings. Discussions about static changes from modern or equilibrium response sea level and rates of sea level change rather than only dividing up by time period. [Government of United States of America]	Noted. The word eustatic is no longer used in the text.
5-1598	5	39	22	39	33	Doesn't the LIG have the best preserved obs record because it was the most recent? If earlier Igs had been lower their evidence would have been destroyed? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	This is a correct statement; sealeverl highstands are preferentially preserved in the geological record.
5-1599	5	39	23	39	23	responsible processes for what ? [Masa KAGEYAMA, France]	Noted

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5-1600	5	39	23			Please use mid-Pliocene rather than MPWP here, so that section 5.6 can be read in isolation [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Taken into account, title has been changed.
5-1601	5	39	26	39	26	suggest to repalce "interglaciations" with "interglacial periods" [Government of Poland]	Noted
5-1602	5	39	29	39	29	remove "but" and start new sentence (there is no contradiction here) [Masa KAGEYAMA, France]	Noted
5-1603	5	39	35	40	15	Could the range in sea level reconstructions for MPWP be related to sea-level variability during this rather long period then? [Masa KAGEYAMA, France]	Noted
5-1604	5	39	35	40	16	It may be worth briefly citing discussion of earlier, warmer periods: Gasson, E., Siddall, M., Lunt, D.J., Rackham, O.J.L., Lear, C.H., Pollard, D., 2012, Exploring uncertainties in the relationship between temperature, ice volume, and sea level over the past 50 million years, Rev. Geophys., 50, 1, doi 10.1029/2011RG000358. [Mark Siddall, United Kingdom]	Space limitations do not permit expansion of such a discussion
5-1605	5	39	37	39	42	The treatment of GIA and dynamic processes is incomplete, not citing major papers. The statement that the signal is 5-30 meters is not explained. It is unrealistic for GIA and dynamics to be integrated into Pliocene sea level because a) ice volume should surpass their influence, and b) we hardly understand them for the last glacial cycle, or the Holocene [Government of United States of America]	Taken into account in the revised appendix information.
5-1606	5	39	37	40	15	Section 5.6.1 Mid-Pliocene changes in sea level: The sea level change for the mid-Pliocene is presented as 10+/-10 m, effectively a range of 0-20m. And yet there is no supporting evidence for a zero sea level change at this time. The authors cite GIA and global mantle dynamic processes as the source of the large uncertainty. Granting that it may be difficult to determine the magnitude of these effects, it may be easier to determine only the sign of these corrections at the locations of the sea-level change observations, thus putting one-sided bounds on the estimate of global sea-level change. [Henry Pollack, United States of America]	Taken into account in the revised MPWP sea level section.
5-1607	5	39	37			Refer back to section 5.3.1 after MPWP. This is where the times referred to as MPWP is defined [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted
5-1608	5	39	38	39	38	Comment text: Miller et al 2012 are being misrepresented by the statement that MPWP peak sea level estimates "range widely (5-40 m) (Miller et al., 2012". They present data and an error analysis from which they conclude that it is extremely likely peak sea levels lay in the range 12 to 32 m above present (see abstract). Their data comprise 33 sets from 9 separate highstands between 2.74 and 3.20 Ma. 13 sets come from field-based estimates from NZ, Enewetak Atoll and Virginia, thus widely spread but subject to GIA uncertainties, and 20 sets are based on joint Mg/δ18O analyses that are not. The mean sea level estimates range from 9.3 to 22.3 m; the younger 6 range from 9 to 15 m (SD ranging from 3 to 13 m) and the older 3 range from 21 to 22 m (SD ranging from 3 to 5 m). Section 5.6.1 begins with the words "Estimates of peak sea levels during the MPWP based on a variety of geological records are consistent in suggesting higher than present sea levels,.." You might continue with words like "Miller et al. (2012) have reported that the highest three of these (3.07, 3.16 and 3.20 Ma) lie around 21+5m above present sea level, but they also noted that benthic foraminiferal δ18O values provided an upper limit of ~21 m". [Peter Barrett, New Zealand]	Taken into account in the revised MPWP sea level section.
5-1609	5	39	45	39	45	with possible contribution from' should certainly be 'and'. There is very little doubt that hydrography plays a role here. See the many observations attesting to large differences in deep hydrography during the glacial period. Also see recent papers by Claire Waelbroeck which clearly indicate that hydrography is very important [Mark Siddall, United Kingdom]	Noted.
5-1610	5	39	48			Stylisitically I feel that ", or could be" should be replaced with ". Conversely" [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted
5-1611	5	39	50			Stating that a sea level of plus 10 m for Pliocene is the lowest possible value and unlikely that low. As a result, the authors should consider revising this statement accordingly. [Government of United States of America]	See previous response
5-1612	5	39	51	39	51	also cite deBoer: de Boer, B., R. S. W. van de Wal, R. Bintanja, L. Lourens, and E. Tuenter (2010), Cenozoic global ice-volume and temperature simulations with 1-D ice-sheet models forced by benthic d18O records, Ann. Glaciol., 51(55), 23–33, doi:10.3189/ 172756410791392736. [Mark Siddall, United Kingdom]	Discussion here is restricted to proxy reconstructions

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5-1613	5	39	53	39	56	It is unclear, if IRD is N Atlantic, how do you get WAIS, EAIS? Moreover, IRD in northern hemisphere oceans are more complex and in regions pre-date 2.7 Ma [Government of United States of America]	Taken into account in the revised sentence.
5-1614	5	39	56	39	57	It is important to recognise that a Pliocene sedimentary record is only available from the Ross Sea sector, which is only one of three main areas of ice discharge from the WAIS. Therefore, "from the Ross Sea" should be inserted after "sedimentary record". [Robert Larter, United Kingdom]	Accepted
5-1615	5	39	57			The phrase "orbital timescales" is unclear. Please consider specifying precession, obliquity, or eccentricity. [Government of United States of America]	Noted will change to obliquity
5-1616	5	39	59			Here the temperature of >5 degrees C given between 4.6 and 3.0 Ma (with a hiatus dated 4.2-3.6 Ma not mentioned here) is again not for the MPWP but for a time interval extending into the Early Pliocene. McKay et al. (2012) do report temperatures > 5 degrees C for the MPWP portion of that longer interval, so why not state that? Also: are these temperatures summer SSTs or mean annual SSTs? [Sandra Passchier, United States of America]	Taken into account in the revised section
5-1617	5	39				Section 5.6: Why does this section place so much emphasis on error bars on paleo-sea levels but gives no discussion on errors on either GIA [viscosity] or elastic [gravity rotational effects] models which also have uncertainties, and a host of other parameters & processes? [Government of United States of America]	Taken into account in the revised text and appendix.
5-1618	5	40	3			The Barrett reference is again addressing a longer interval than the MPWP. I would replace the Barrett (in press) review paper with citations to recent papers with balanced discussions of new MPWP datasets for Prydz Bay documenting the variability in the East Antarctic cryosphere (Passchier, 2011, Paleoceanography; Whitehead et al. 2005; Geology) and use the Naish et al. (2009) and McKay et al. (2012) references for the West Antarctic ice sheet and the conditions in the Ross Sea. [Sandra Passchier, United States of America]	Space limitations lead to the citation of recent summary/synthesis paper
5-1619	5	40	3			One could insert before "Ice-sheet models...". a description of the correlations between a proximal ice rafted debris record (Passchier, 2011) and the LR04 bentic d18O stack (Lisiecki and Raymo, 2005) indicating large variability and peak ice discharge into Prydz Bay from the Lambert Glacier-Amey Ice Shelf system during the onsets of interglacials of the MPWP. The chronology of the MPWP portion of this record is excellent and unique for East Antarctica. [Sandra Passchier, United States of America]	Noted; see Barrett (in press) summary paper.
5-1620	5	40	9	40	15	This statmeent does not necessarily reflect the best evidence. The most robust evidence comes from Pliocene shorelines, paralic deposits, atolls, exposed marine units all over the world, plus the O-18 curves corrected for temperature. As discussed in next section on MIS5 [Government of United States of America]	Noted. Text has been revised.
5-1621	5	40	10			"it was reached" not it reached [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted
5-1622	5	40	12			Consider repalcing "some contribution from" with "partial deglaciation of" [Government of United States of America]	Noted
5-1623	5	40	13	40	13	Comment text: The statement that "We have therefore medium confidence that sea level was 10+-10m above present during the warmest periods in the Pliocene" appear with no logical precursory line of evidence. The Miller et al. (2012) review, mentioned earlier in my previous comment, suggests a peak Pliocene sea level with a lower bound of 12 m, but with an upper bound limited by benthic foraminiferal oxygen isotope values to 21 m. Data-constrained Antarctic ice sheet modelling indicates a loss of 7 m SLE from Antarctica alone, which with Greenland (7) , other ice (1) and the steric rise (2) would sum to ~17 m. Allowing some ice to remain on Greenland and WAIS it would be reasonable to claim medium confidence from these data to settle on a midpoint of around 15m+5m. I would have no confidence in a peak value below 6 m, as noted earlier in the comment on p 3 line 44. [Peter Barrett, New Zealand]	Noted and covered in revised text
5-1624	5	40	13	40	15	I can understand the sentiment that you're trying to express with the sentence about low confidence >20m (i.e. 10+10m is much more unlikely than 10-10m). However, I wonder if it's worth including this sentence at all. Surely the previous sentence giving a number of 10+/-10m, says that it isn't likely to be greater than 20m. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted and covered in revised text
5-1625	5	40	14	40	15	The last sentence of this paragraph contains a logical error. Write: "We have high confidence that sea level was less than 20 m above present." [Manfred Mudelsee, Germany]	Noted and covered in revised text

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5-1626	5	40	15			I am not sure you mean that you have low confidence that sea level was >20 m. To me low confidence means this is a statement you on balance think likely but don't have much evidence for. You are using it here in the opposite sense, to say something you think is unlikely. Given that you have already said you have medium confidence that sea level is 10+/-10, it fllows that you think 20 m is unlikely. My suggestion: extend the previous sentence, adding "of the Pliocene, and was unlikely to have been higher than 20 m above present (medium confidence)". [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted and covered in revised text
5-1627	5	40	17	40	17	suggest to replace "Past Interglaciations" with "Past Interglacial Periods" [Government of Poland]	Revised to Last Interglacial
5-1628	5	40	17			I don't think that interglaciations is a suitable term - please use interglacials. Glaciation is act of increasing ice; deglaciation is its opposite. The term interglaciation means between ice-growing events and infers nothing about the quantity of ice present at the time. In the sea-level context the term would denote a constant sea level between two sea level falls, but does not indicate that there has been a sea level rise at any point. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Revised to Last Interglacial
5-1629	5	40	19	40	19	context needed - we are talking about Quaternary interglacials, rather than earlier periods before the glacial cycles [Mark Siddall, United Kingdom]	Section revised to assess only the Last Interglacial
5-1630	5	40	19	40	20	It's not clear whether this statementcan be supported by facts: "Records of past sea level can be found both in deep-sea cores foraminiferal delta 18O records" unless the ocean was really shallow. Therefore, it's sugggested that the authors rewrite the sentence to read: "Records of past sea level can be reconstructed from both in deep-sea cores foraminiferal delta18O records and emergent shorelines." [Government of United States of America]	Taken into account in the revised text.
5-1631	5	40	19	40	25	I am not sure that this way to shorten the text (i, ii, iii) works well for the reader [Olga Solomina, Russia]	Noted and moved section to appendix
5-1632	5	40	20	40	20	add 'and the planktic and benthic d18O record of the Red Sea'. I recommend that this method is described in table 5.A.1 [Mark Siddall, United Kingdom]	Noted, present table is temperature and CO2 proxies, not sealevel.
5-1633	5	40	21	40	24	I don't understand the distinction you are making about why deep sea cores cannot constrain (i) [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Because they are not absolutely dated records
5-1634	5	40	21			I suggest "(iii) any variability…" as you use possible in the next sentence. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1635	5	40	22			something wrong with sentence here; part missing [Elie Verleyen, Belgium]	Noted
5-1636	5	40	23	46	57	Section 5.7 Evidence and Processes of Abrupt Climate Change: Text was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected with considered text. [Dirk Thielen, Venezuela]	Noted; not part of sealevel section
5-1637	5	40	27	41	19	Overall this is a very nice approach to dealing with ages vs GMSL and the range of data [Mark Siddall, United Kingdom]	Noted
5-1638	5	40	27			Godefroid (2012) provided 35 reliable U-Th TIMS dates measured on a LIG fossil reef from Mayaguana Island ranging between ~130 and 117 ka. Godefroid, F., 2012: Géologie de Mayaguana, SE de l'archipel des Bahamas. Terre et Environnement, v. 108, 230 p. [Pascal Kindler, Switzerland]	Noted.
5-1639	5	40	27			Section 5.6.2.1: Note that open system U/Th ages give a significantly different age model for the Last Interglacial. [Robert Kopp, United States]	Noted and addressed issue in appendix
5-1640	5	40	29	40	29	suggest to repalce "interglaciations" with "interglacial periods" [Government of Poland]	Accepted
5-1641	5	40	29	40	40	Rather than giving all the reconstructions, why not synthesing them and give the reasons for the range of results? This information is the last paragraph of Appendix 5.A.3 but I believe it should be placed in the main text. [Masa KAGEYAMA, France]	Noted. However, due to space limitations, this discussion has been removed from the final chapter.
5-1642	5	40	29			Erroneous use of Interglaciations again instead of interglacials [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted

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5-1643	5	40	29			Consider deleting the reference to Appendix 5.A.3 in the first line because the appendix doesn't discuss timing of interglaciations but an assessment of uncertainties, as stated in line 32. [Government of United States of America]	Taken into account. The appendix has finally been removed.
5-1644	5	40	40			AS A GENERAL COMMMENT: To conclude this paragraph seems necessary some short comment about the feasibility of the quoted examples. It is not clear after listing the relevance of each and if these differences are significant. [José-Abel Flores, Spain]	Noted
5-1645	5	40	42	41	19	This section ignores papers in the peer-reviewed literature that present evidence indicating a much lower LIG sea-level highstand, e.g. McMurtry et al. (Geology, 2010) dated uplifted highstand reefs on Oahu, Hawaii and stated that their results "strongly suggest that no exceptional sea-level rise above the 2 m recorded by the Holocene highstand is warranted for interglacial periods in the late Quaternary from stage 13 and/or 15 time". Even if it is concluded that the balance of evidence does suggest a highstand in the range interpreted by Dutton and Lambeck (2012) and Kopp et al. (2009), it is important to mention such conflicting evidence. [Robert Larter, United Kingdom]	Noted, but tectonically active reef sites are not included in this section, which focuses solely on tectonically stable localities.
5-1646	5	40	44	40	44	"made" --> "retrieved"? [Masa KAGEYAMA, France]	Noted
5-1647	5	40	44	40	57	definitions missing - 'far field', 'earth and ice model parameters' [Mark Siddall, United Kingdom]	Noted
5-1648	5	40	44	45	8	The finding for new higher LIG sea level is primarily based on "only two tectonically stable far-field areas have been studied, Australia and the Seychelles islands". The data is dismissed from other regions such as "For the Caribbean and North Atlantic regions, the isostatic effects can be more important as well as more dependent on the choice of Earth- and ice-model parameters." But there is no evidence given that isostatic effects are more important. Since there is no cited publication, is not an equally plausible statement one could make for the Australia and the Seychelles something like: Unaccounted isostatic effects 'can be more important' and explain the local 6 to 10 meter LIG sea level rise ? [Government of United States of America]	Taken into account in the revised text.
5-1649	5	40	46	40	46	Note that, while Australia is largely tectonically stable, there is a history of earthquakes in many parts of Australia, especially the southeast but also the west. Some llocalities in Western Australia (e.g., Cape Cuvier) have been interpreted by some authors (e.g., Stirling et al., 1998) as having undergone several meters of uplift since the LIG. [Robert Kopp, United States]	Taken into account in the revised text.
5-1650	5	40	49	40	49	missing (in front of Lambeck et al [Masa KAGEYAMA, France]	Noted
5-1651	5	40	49			Missing open bracket before Lambert [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted
5-1652	5	40	49			"signals are less Lambeck et al., 2012", should be changed as "signals are less (Lambeck et al., 2012). [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Noted
5-1653	5	41	1			A palmata depth range traditionally is 7 meters, but it can live deeper [Government of United States of America]	Accepted and revised text.
5-1654	5	41	1			Its is well known (http://www.iucnredlist.org/details/133006/0) that A. palmata can live in water depths greater than 5 m (down to 40 m). It is therefore a moderately reliable indicator of past sea levels. [Pascal Kindler, Switzerland]	We are aware of this
5-1655	5	41	2			The highest and most precise elevation of relative sea level during LIG from the Bahamas is provided by a perched fossil beach at Clifton Pier, New Providence Island. Its elevation is about +8 m above present sea level (Garrett and Gould, 1984; Hearty and Kindler, 1997). Garrett, P., and Gould, S.J., 1984: Geology of New Providence Island, Bahamas. Geological Society of America Bulletin, v. 95, p. 209-220. Hearty, P.J., and Kindler, P., 1997: The stratigraphy and surficial geology of New Providence and surrounding islands, Bahamas. Journal of Coastal Research v. 13, p. 798-812. [Pascal Kindler, Switzerland]	Noted.
5-1656	5	41	7	41	8	The sentence "All these estimates are of relative sea level, made without consideration of GIA effects."is not clear. GIA is just part of the contribution to the local sea level rise along with thermal expansion, Antarctica,	Noted, these issues are addressed in the revised text which clarifies information in the following paragraph

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						and other terrestrial glaciers. If GIA isostatic effects are not accounted for, are they relevant enough that they should be? [Government of United States of America]	
5-1657	5	41	7	41	8	All the estimates...' better as first sentence to this section 'All the estimates in this section are...' [Mark Siddall, United Kingdom]	Text is revised
5-1658	5	41	10	41	19	Is it worth stating this conclusion at 8m +/- 2m? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	This form of stating uncertainties implies certain distribution, that is not supported by data
5-1659	5	41	10	41	19	very nicely put! [Mark Siddall, United Kingdom]	Noted
5-1660	5	41	14	41	14	95% probability should be "extremely likely" per AR 4, though it does not appear to be defined in the AR5 guidance. [Robert Kopp, United States]	AR5 guidelines define the use of very likey for 90-100% probabilities. Extremely likely does not exist in AR5 giuidelines
5-1661	5	41	14	41	16	While I agree this is what Kopp says, it seems inconsistent with Fig 5.15a. 8 m is essentially the top of the 84th percentile, so its hard to see how there can have been an 84% probability that it was exceeded. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Will need to check when drafting next iteration. Will seek clarification from Kopp if necessary
5-1662	5	41	27	41	27	Citation style wrong [Peter Burt, United Kingdom]	Noted
5-1663	5	41	27			Blanchon et al., (2009), comma should be removed after "et al." [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Noted
5-1664	5	41	28	41	28	Citation style wrong [Peter Burt, United Kingdom]	Noted
5-1665	5	41	29	41	29	Citation style wrong [Peter Burt, United Kingdom]	Noted
5-1666	5	41	31			Geological data from the Bahamas suggesting a fall of sea level close to the present datum in the middle of the LIG are controversial. Godefroid (2012) found no evidence on Mayaguana Island that supports the hypothesis of Thompson et al. (2011). Reexamination of the exposures described by Chen et al. (1991) and Thompson et al. (2011) on Great Inagua and San Salvador shows that the upper boundary of the lower reef may not be an exposure surface (Godefroid, 2012). [Pascal Kindler, Switzerland]	Noted.
5-1667	5	41	36			"Barbados, but" not "Barbados but" [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted
5-1668	5	41	39	41	39	Citation style wrong [Peter Burt, United Kingdom]	Noted
5-1669	5	41	39	41	41	What is the time base for these estimations? [Peter Burt, United Kingdom]	Taken into account in the revised text.
5-1670	5	41	39	41	44	This refers to the Rohling et al paper 2008 that gave rates of 1.1 – 2.6 m/ century but then a preference is given to using the Kopp et al average over much longer periods. What is the point of only focussing on the 4 - 10 ka time scale in a report that is intended to be advice for policymakers and governments? Is there any reason to ignore Rohling et al 2008? Remember that the guidance note to authors included a clear requirement to consider the tails of distributions and for sea level rise that should lead to a focus on the range for SLR that can occur on the century or shorter time scale. The rate at which a rise of 0.3 m could occur is far more important for coastal management than an assessment of where sea level will have risen to in 1000 years time. [Martin Manning, New Zealand]	Taken into account in the revised text.
5-1671	5	41	39	41	44	Why does this section not refer to the more recent and more detailed paper: Grant, K.M., E.J. Rohling, M. Bar-Matthews, A. Ayalon, M. Medina-Elizalde, C.B. Ramsey, C. Satow, and A.P. Roberts, Rapid coupling between ice volume and polar temperature over the past 150,000 years, Nature, 2012 ? If the LAs do not agree with the statement in that paper - "This reveals that rates of sea-level rise reached at least 1.2 m per century during all major phases of ice-volume reduction" - then there needs to be a clear statement as to why they disagree. [Martin Manning, New Zealand]	Taken into account in the revised text.
5-1672	5	41	39	41	45	This is a considerable improvement. There is good reasoning here. However, it seems worthwhile to point out that the Kopp et al rates are very close. The thing to point out here is that the Red Sea method does not provide precise ages because of the nature of the method. Therefore published U/Th ages are used and these ages are contested. The range of possible rates is therefore greater than the range published, which was due	Taken into account in the revised text.

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						to using contentious U/Th ages. 'The uncertainty in the age model, therefore leads to low confidence in the higher of these estimates of sea level variability during the LIG.' I am not sure what to do with units used for Kopp et al here. It is clearly a little odd to use different units (m kyr^-1) compared to elsewhere in the text. On the other hand these units express the fact that this is the millennial average rate. I suggest that you write 'a millennial average rate of xx common units' to get around that. That would, in one phrase also express that this is the millennial average. [Mark Siddall, United Kingdom]	
5-1673	5	41	39			Rohling et al., (2008a), comma should be removed after "et al." [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Accepted
5-1674	5	41	41	41	41	missing fullstop after (33% probability) [Masa KAGEYAMA, France]	Accepted
5-1675	5	41	44			As I have previously mentioned to Kurt Lambeck, we have a paper that has recently come out in Nature that provides much better independent age control to the Red Sea sea-level record. This paper has come out in online publication 15 November 2012 (hardcopy date 29 Nov. 2012). It also provides a probabilistic assessment of all uncertainties (age and RSL) in the Red Sea record. That study's age control also provides new information about the rates of rise for the first rise above 0m in the Red Sea record (because of the methodology there is a degree of smoothing in rate evaluations that makes the oscillation weakly represented). The rates of rise for the first rise above 0m in the Red Sea record is on average 0.7 m per century, with uncertainties of about ±0.4 m per century at the 68% level. So, a 'likely range' of 0.3 to 1.1 m per century. Accordingly, I suggest that after "0.4-0.9 m per century", the sentence is continued "as corroborated by new, more direct dating of the Red Sea record, which determined a likely range for the rates of rise above 0 m of 0.3 to 1.1 m per century (Grant et al., 2012)". Grant, K.M., Rohling, E.J., C., Bar-Matthews, M., Ayalon, A., Medina-Elizalde, M., Bronk Ramsey, Satow, C., and Roberts, A.P., Rapid coupling between ice volume and polar temperature over the past 150 kyr. Nature, in press (publication date 15 November 2012). [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland] This updated review comment has been send by Eelco directly to Michael after we received the official file; changes made by Eelco are marked in red. "The changes make the statement more precisely applicable to the datatable that was provided with our Grant et al. paper." [EJR]: As I have previously mentioned to Kurt Lambeck, we have a paper that has recently come out in Nature that provides much better independent age control to the Red Sea sea-level record. This paper has come out in online publication 15 November 2012 (hardcopy date 29 Nov. 2012). It also provides a probabilistic assessment of all uncertainties (age and RSL) in the Red Sea record. That study's age control also provides new information about the rates of rise for the first rise above 0m in the Red Sea record (because of the methodology there is a degree of smoothing in rate evaluations that makes the oscillation weakly represented). The rates of rise for the rise through 0m (measured as the mean from -5m to +5 m) in the Red Sea record is on average 0.7 m per century, with uncertainties of about ±0.4 m per century at the 68% level. So, a 'likely range' of 0.3 to 1.1 m per century. Accordingly, I suggest that after "0.4-0.9 m per century", the sentence is continued "as corroborated by new, more direct dating of the Red Sea record, which determined a likely range for the rates of rise through 0 m of 0.3 to 1.1 m per century (Grant et al., 2012)". Grant, K.M., Rohling, E.J., C., Bar-Matthews, M., Ayalon, A., Medina-Elizalde, M., Bronk Ramsey, Satow, C., and Roberts, A.P., Rapid coupling between ice volume and polar temperature over the past 150 kyr. Nature, in press (publication date 15 November 2012).	Taken into account in the revised text.
5-1676	5	41	46	41	46	Citation style wrong [Peter Burt, United Kingdom]	Accepted
5-1677	5	41	46			Kopp et al., (2009), comma should be removed after "et al." [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Accepted
5-1678	5	41	49			Additional comma is at the end of the sentence [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1679	5	41	51	41	55	These statements are based on informal reanalysis of Kopp et al., 2009, and therefore may be harder to trace.	Taken into account

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						It may be better to rely on the more formal reanalysis in Kopp et al. (submitted); the author is happy to provide a copy of this manuscript on request. [Robert Kopp, United States]	
5-1680	5	41	52			The first "GMSL" is both confusing and unnecessary [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1681	5	41	53	41	55	Many would question whether this single study is the "most robust" evidence for SL changes within the last interglacial. [Government of United States of America]	Noted. The text has been revised.
5-1682	5	41	53			". By" rather than ", By" [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1683	5	41	54			Kopp et al., (2009), comma should be removed after "et al." [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Accepted
5-1684	5	41	55	41	55	Add at the end. 'These estimates overlap with the lower estimates of sea-level rise using our preferred ages for the Red Sea estimates.' This is more in line with the way that the Kopp et al vs Dutton and Lambeck results are discussed earlier in the text. [Mark Siddall, United Kingdom]	Noted. The text has been revised.
5-1685	5	42	1	42	1	suggest to replace "…the Last Interglaciation" with "…the Last Interglacial Period" [Government of Poland]	Noted
5-1686	5	42	1	42	29	Assessments in figure captions must be avoided. This is especially important since Ch05 figure captions are comparably long due to the complexity of the figures. Please remove assessment type text from caption. [Thomas Stocker/ WGI TSU, Switzerland]	Noted
5-1687	5	42	8	42	8	Delete comma after 'both' [Peter Burt, United Kingdom]	Noted
5-1688	5	42	21	42	23	Caption for Figure 5.15: Why does the difference between the reconstructed and predicted sea level curves provide an estimate of global mean sea level? Why not use the southernmost site as it is farthest from Greenland? How large are the uncertainties and why not show them on the figure? [Government of United States of America]	Taken into account for the revised figure and caption.
5-1689	5	42	33			section on principle sources. Include reference to table 4.1 where Arendt, 2012 is the used reference. [European Union]	Taken into account.
5-1690	5	42	35	42	35	The total volume in all glaciers have been revised downwards. See Cryosphere chapter for an updated estimate [Aslak Grinsted, Denmark]	Taken into account.
5-1691	5	42	35			Consider including: and Piedmont glaciers such as Malaspina and Bering – say piedmont as well as mountain [Government of United States of America]	Noted
5-1692	5	42	41			Elevation of what? Land or ice surface? Please specify. [Government of United States of America]	Noted
5-1693	5	42	42	42	42	There is probably a significant uncertainty to this estimate of 130m, please provide it [Government of Australia]	Taken into account.
5-1694	5	42	42	42	42	decrease of 7 cm/yr should be replaced by ice thickness decrease of 7 cm/yr not to be misunderstood as surface elevation decrease. [European Union]	Noted
5-1695	5	42	44	43	2	If the GIS cannot have contributed more than ~2 m to the LIG highstand, then this--and all that follows regarding the Antarctic ice sheet-- is in fundamental conflict with the curve depicted in Figure 5.15. Why? Where is the missing ice if it's not in GIS or AIS? This issues seems deserving of discussion. [Government of United States of America]	Taken into account. Text and figures have been revised.
5-1696	5	42	45			It is indeed unlikely that the GIS contributed more than 2 m to the LIG high stand. However, this statement disagrees with panel f in Fig.5.15 [Elie Verleyen, Belgium]	Taken into account. Text and figures have been revised.
5-1697	5	42	47	42	47	As noted in the exec summary "the West and/or East Antarctic" ... Better "West, and potentially East Antarctic" since nobody really expects the "or" to be East without West as well! [Tasman van Ommen, Australia]	Noted.
5-1698	5	42	49	42	51	Please add a sentence which adds support from the observation that WAIS ice cores has no ice from the LIG. The Byrd ice core goes back to ~90k The WAIS divide core goes back to ~62kyr (just from a press release so I have no reference) [Aslak Grinsted,	Noted. Text has been revised.

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						Denmark]	
5-1699	5	42	50	42	51	SUGGESTION: INCLUDE INFORMATION IN THE OPEN OECEAN RELATED TO THE PROCESS: Ross Saae Simple Coast, also reflected in the Atlantic open ocean, at some time in the last 800,000 years (Flores and Sierro, 2007; McKay et al., 2008, 2011; Scherer et al., 2008; Vaughan et al., 2011) [José-Abel Flores, Spain]	Noted but text is focused on proximal ice evidence.
5-1700	5	42	50	42	51	Flores, J.A., Sierro, F.J., 2007. Pronounced mid-Pleistocene southward shift of the Polar Front in the Atlantic sector of the Southern Ocean. Deep-Sea Res. II 54 (21–22), 2432–2442. [José-Abel Flores, Spain]	Noted but text is focused on proximal ice evidence.
5-1701	5	42	50	42	51	Scherer, R.P., Bohaty, S.M., Dunbar, R.B., Esper, O., Flores, J.-A., Gersonde, R., Harwood, D.M., Roberts, A.P., Taviani, M., 2008. Antarctic records of precession-paced insolation- driven warming during early Pleistocene Marine Isotope Stage 31. Geophys. Res. Lett. 35, L03505. doi:10.1029/2007GL032254. [José-Abel Flores, Spain]	Reference not relevent to LIG discussion
5-1702	5	42	51	42	51	Is the correct Scherer et al., 1998 represented in the literature cited list? [Matthew Konfirst, United States of America]	Yes
5-1703	5	42	57	42	57	Ackert et al. (2011) is about past WAIS, not EAIS, surface elevation (this is even stated in the title). Change "EAIS" to "WAIS". [Robert Larter, United Kingdom]	Accepted, misprint corrected - thank you
5-1704	5	42				The y axis for panel f is labeled GIS for Greenland, but the panel also shows data from Antarctica. Consider changing to "Ice sheet contribution to sea level" [Government of United States of America]	Noted
5-1705	5	43	6	43	6	"combine to explain" - is this correct? [Olga Solomina, Russia]	Noted
5-1706	5	43	15			in Otto-Bliesner et al, (2006) in AR4, this should be corrected as "in Otto-Bliesner et al. (2006) in AR4." [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Noted
5-1707	5	43	21	43	21	How present day climate is defined? The average 1961 to 1990? What are differences in present-day climate which causing discrepancies of model outputs? [Government of Germany]	Revised text uses term modern control climate for model instead of present day
5-1708	5	43	22			4 ice shheet models but 5 panels? [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted. The revised figure includes 4 panels.
5-1709	5	43	25	43	44	Cite: Roberts, DL., Karkanas, P, Jacobs, Z, Marean, CW, Roberts, RG (2012) Melting ice sheets 400,000 yr ago raised sea level by 13 m: Past analogue for future trends. Earth and Planetary Science Letters 357-358, 226-237. Study has been GIA corrected. [Roland Gehrels, United Kingdom]	Noted, but section on MIS11 has been removed due to space limitations
5-1710	5	43	25	43	44	Is this paragraph really necessary? Anyway, the inclusion of this period should maybe be justified in some way (e.g. why is this period worh mentioning). [Hans Linderholm, Sweden]	Noted, but section on MIS11 has been removed due to space limitations
5-1711	5	43	27	43	30	The sentence would read better with the near present levels section first, because of the long brackets following the upper bound. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted, but section on MIS11 has been removed due to space limitations
5-1712	5	43	28	43	29	Citation style wrong [Peter Burt, United Kingdom]	Noted, but section on MIS11 has been removed due to space limitations
5-1713	5	43	28			Sedimentological evidence of a sea level highstand around +20 m above present sea level during MIS 11 has recently been corroborated by Godefroid et al. (2010). Godefroid, F., Kindler, P., and Nawratil de Bono, C., 2010: Further evidence for a +20 m sea-level highstand during Marine Isotope Stage 11 from fossil lacustrine sediments: Glass Window, Eleuthera, Bahamas In: Martin, J.B., and Siewers, F.D. (Editors), Proceedings of the 14th Symposium on the Geology of the Bahamas and other carbonate regions, Gerace Research Center, San Salvador, Bahamas p. 90-106. [Pascal Kindler, Switzerland]	Noted, but section on MIS11 has been removed due to space limitations
5-1714	5	43	29			Superfluous semi-colons related to the references [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted, but section on MIS11 has been removed due to space limitations
5-1715	5	43	30			The GIA acronym could stand to be defined here (or wherever it first appears in this chapter). [Government of United States of America]	Noted, but section on MIS11 has been removed due to space limitations

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5-1716	5	43	32			comma after Bahamas [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted, but section on MIS11 has been removed due to space limitations
5-1717	5	43	33			comma after GIA [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted, but section on MIS11 has been removed due to space limitations
5-1718	5	43	33			peak eustatic sea level [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted, but section on MIS11 has been removed due to space limitations
5-1719	5	43	35			The Muhs paper is not more recent that Raymo and Mitrovica. Try "This estimate is consistent with recent data … (Muhs et al., 2012)" [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted, but section on MIS11 has been removed due to space limitations
5-1720	5	43	37	43	37	Citation style wrong [Peter Burt, United Kingdom]	Noted, but section on MIS11 has been removed due to space limitations
5-1721	5	43	37	43	37	Should the syntax be changed to: Proxy records indicate global sea levels similar to the present? [Government of France]	Noted, but section on MIS11 has been removed due to space limitations
5-1722	5	43	37	43	40	This is the first time the Red Sea method is described despite being mentioned earlier. See my earlier comment that the Red Sea method should be mentioned when other methods of reconstruction are mentioned. This is simply for consistency. I am not trying to push this method [Mark Siddall, United Kingdom]	Noted, but section on MIS11 has been removed due to space limitations
5-1723	5	43	37	43	40	The previous para has given a renage of sea levels typically 10 m, so why do you now start a new para saying that proxies suggest levels near to present. Then in the following para you again say 5-15 metres. I think it's the statement on line 37 that needs to disappear. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted, but section on MIS11 has been removed due to space limitations
5-1724	5	43	37			I don't agree with the interpretation that 5m of SLR is "near present". A better introduction to the proxy data, would be to say that they indicate lower levels than those inferred above. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted, but section on MIS11 has been removed due to space limitations
5-1725	5	43	37			It sounds like 5-15m of sea level change is being characterized as small or near present. As is pointed out a few sentences later this is all of WAIS and GIS! [Government of United States of America]	Noted, but section on MIS11 has been removed due to space limitations
5-1726	5	43	38	43	38	An analysis of benthic d18O records [...] ~5+/- 15m (Waelbroeck et al., 2002). With such an uncertainty (3 times the value of the estimate of GMSL), no one can believe that 5m has any meaning. [Government of France]	Noted, but section on MIS11 has been removed due to space limitations
5-1727	5	43	39	43	40	The meaning of the range of "~3-5 m" given at the end of this sentence is ambiguous: it is not clear if this is the best estimate of the MIS 11 highstand relative to modern sea level given by Rohling et al. (2009, 2010) or the range of uncertainty they assigned to their estimate. Rohling et al. (2010) clearly state that the sea-level record of Rohling et al. (2009) "places the MIS-11 highstand at a similar (within uncertainty) level as the Holocene highstand", so I presume the ~3-5 m range it is intended to indicate an uncertainty. However, I cannot find anywhere in Rohling et al. (2010) where this uncertainty range is stated, so preume that the range has been "eye-balled" from one of their figures. Alternatively, as I note that Rohling is one of the contributing authors, perhaps he has provided these numbers from archived data? In this case the location where these archived data can be accessed must be made clear. [Robert Larter, United Kingdom]	Noted, but section on MIS11 has been removed due to space limitations
5-1728	5	43	42	43	42	This seems to contradict the previous sentence, with a 15m uncertainty. [Government of France]	Noted, but section on MIS11 has been removed due to space limitations
5-1729	5	43	42	43	43	The final assessment of "MIS 11 GMSL 5-15 m higher than present" (albeit with only medium confidence) seems to disregard the conculsions of Bowen (2010) and Rohling et al. (2009,2010) reported in the previous paragraphs. How can a GMSL in this range be consistent with "levels near present" (Bowen, 2010) and a "highstand at a similar (within uncertainty) level as the Holocene highstand" (Rohling, 2010)? The results reported by McMurty et al. (Geology, 2010) are also relevant here and are not mentioned. [Robert Larter, United Kingdom]	Noted, but section on MIS11 has been removed due to space limitations
5-1730	5	43	48			Deglaciation of eastern Laurentide ice sheet revised to 16 ka (Peteet et al. 2012 GRL) means a more rapid	Text refers to GMSL and does not refer to regional

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						input of water into N. Atlantic region from Laurentide. [Government of United States of America]	change; hence reference is not included
5-1731	5	43	49	43	49	Suppress the space between "13" and "0" [Hugues Goosse, Belgium]	Corrected to 130 m
5-1732	5	43	49	43	49	check number 13.0 ? [Government of Germany]	Corrected to 130 m
5-1733	5	43	49	43	49	Should there be a space between 13 and 0 one this line? Should this not be 130m? It looks like it could be read as 13.0m. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Corrected to 130 m
5-1734	5	43	49	43	49	"13 0 m" --> "130 m" [Masa KAGEYAMA, France]	Corrected to 130 m
5-1735	5	43	49	43	49	Delete the space between "~13" and "0". [Maria Fernanda Sanchez Goñi, France]	Corrected to 130 m
5-1736	5	43	49			"130 m" not "13 0 m" [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Corrected to 130 m
5-1737	5	43	49			"At this interval" is not needed [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Corrected to 130 m
5-1738	5	43	49			… 130m (typo) [Government of France]	Corrected to 130 m
5-1739	5	43	49			Do the authors mean 13.0 meters? [Government of United States of America]	Corrected to 130 m
5-1740	5	43	49			"13 0" should be "130". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Corrected to 130 m
5-1741	5	43	49			For accuracy, I think we should add "with its onset" before "coeval". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1742	5	43	49			Is this 130? [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Corrected to 130 m
5-1743	5	43	51	43	51	Citation style wrong [Peter Burt, United Kingdom]	Noted
5-1744	5	43	51			Descahmps et al., (2012) (in lines 51 and 59), comma should be removed after "et al." [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Noted
5-1745	5	43	52	43	43	I suggest to give the sea level rate in m.kyr-1 instead of mm.yr-1 to be consistent with the estimations for the LIG. [Maria Fernanda Sanchez Goñi, France]	Accepted
5-1746	5	43	54	43	59	Add the glaciological reconstruction of Whitehouse et al., 2012 to list of studies suggesting smaller values of Antarctic contribution - ~8m in that study. Whitehouse, P.L., M.J. Bentley and A.M. Le Brocq 2012. A deglacial model for Antarctica: geological constraints and glaciological modelling as a basis for a new model of Antarctic glacial isostatic adjustment. Quaternary Science Reviews, 32: 1-24 doi:10.1016/j.quascirev.2011.11.016. [Matt King, Australia]	Noted.
5-1747	5	43				Again, the discussion here is based more on models than data…and there is large uncertainty here since small-scale, but critical, processes (ice streams, calving) are not in models. This is worthy of discussion. [Government of United States of America]	Noted
5-1748	5	44	5	44	6	Antarctic modelling studies are one thing, but more importantly the deglacial history indicated by actual data from Antarctica should be reported. Minimum ages of deglaciation from sediment core transects in some areas do indicate rapid retreat at around the time of MWP-1A, e.g. Marguerite Trough, on the Pacific margin of the southern Antarctic Peninsula (Kilfeather et al., 2011, Quat. Sci. Res.), Dotson-Getz Trough in the Amundsen Sea (J.A. Smith et al., 2011, Quat. Sci. Res.). In other areas, though, results suggest a more gradual retreat across the continental shelf, e.g. the Ross Sea (Conway et al., 1999, Science; Shipp et al., 1999, GSA Bull.). It may be significant that the transects indicating rapid retreat are from cross-shelf troughs with reverse seabed gradients, where ice retreat might have been triggered by a sea-level rise sourced from elsewhere. [Robert Larter, United Kingdom]	Observational data due reflect rapid retreat, however radiocarbon chronologies are highly uncertain. This sentiment is captured by the text regarding the debate about the timing of MWP
5-1749	5	44	5	44	6	The statement that "there are no modelling results that show a retreat of partial collapse of the Antarctic ice sheet at this time" fails to mention that there are very few models that simulate the course of the last deglaciation in Antarctica, and in general the available results do not include time steps sufficiently close together to tell whether or not they might simulate a significant retreat at this time (e.g. only 5 kyr frames are available from the model results published by Pollard and DeConto, 2009). Moreover, a more significant	Acknowledged in revised text by inserting rapid before retreat

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						problem for the hypothesis that a significant contribution to MWP-1A originated from Antarctica is that LGM models only contain 9-18 m sea-level equivalent of ice more than the modern ice sheet (e.g. Huybrechts, 2002, Quat. Sci. Res.; Whitehouse et al., 2012, Quat. Sci. Res.). Therefore, for MWP-1A to have been sourced mainly from Antarctica, most of the post-LGM deglaciation would have to have taken place within the 350 year duration of the MWP event. [Robert Larter, United Kingdom]	
5-1750	5	44	5			Using the phrase “warming itself” sounds like thermal expansion, which is incorrect in this context. [Government of United States of America]	Revised text to have period after North America and deletion of rest of the sentence. This should correct the implication that 50% of the change is steric.
5-1751	5	44	6			“there are no modelling results that show” This isn't clear. Have models had a negative result or not looked at the question? [Government of United States of America]	Text changed to clarify meaning of the sentence;
5-1752	5	44	8	44	18	I recommend citing the recent review of Tornquvist et al in Nature Geoscience on this subject [Mark Siddall, United Kingdom]	Noted. However due to space limitations we cannot be all inclusive and only presented representative results.
5-1753	5	44	8		11	If this new reference Leorri et al (2012) is accepted, I suggest to include it also in the second paragraph of page 5-44 that refers to new results from different Holocene high resolution sea level records in this way: "Since AR4, higher resolution sea level records suggest further periods of rapid ice-mass loss. For example, a rapid rise of ~14 m from ~9.5–8.0 ka has been recorded in Singapore (Bird et al., 2007) or from ~9.0–7.5 ka possibly punctuated with one or two short periods of higher rates (Cronin et al., 2007 for Chesapeake Bay; Hijma and Cohen, 2010 for the Netherlands coast; Leorri et al., 2012 for the Bay of Biscay, Spain), followed by a short ..." [Alejandro Cearreta, Spain]	Noted. Reference has been added, text and figure modified accordingly.
5-1754	5	44	8			Carlson et al 2008 (natgeo) estimate 1.3 cm/yr during some EH periods [Government of United States of America]	Noted
5-1755	5	44	9	44	11	This setence is unclear. A possible rewrite could read: "…has been recorded (Bird et al., 2007, for Singapore) or from ~9.0-7.5 ka possibly punctuated with one or two short periods of higher rates (Cronin et al., 2007, for Chesapeake Bay; Hijma and Cohen, 2010, for the Netherlands coast), followed by..." [Government of United States of America]	Accepted, sentence clarified.
5-1756	5	44	10			Right hand side panels (e-h) Observational evidence, this should be corrected as "Right panels (e-h): Obervational evidence" [SELVARAJ KANDASAMY SELVARAJ KANDASAMY, China]	Noted
5-1757	5	44	12	44	12	Check Bird et al. (2010) and Bird et al. (2007). I believe the reduced rise in Singapore is centered on 7.7 or 7.6 ka, not 8.2 ka. [Roland Gehrels, United Kingdom]	Need to check
5-1758	5	44	13	44	14	Why the parentheses around this sentence? [Masa KAGEYAMA, France]	Accepted
5-1759	5	44	13	44	18	Text needs tidying up for clarity and grammar [Peter Burt, United Kingdom]	Taken into account
5-1760	5	44	15			The total volume of Lake Agassiz would be only 25-45 cm sea level equivalent. The statement here is a bit misleading about it being responsible for short-duration increases. [Government of United States of America]	Taken into account
5-1761	5	44	20	44	22	This is confusing; how can ocean volume be expressed in terms of meters? Is it meters of sea level rise somehow normalized by volume? Meters equivalent to what? Please consider rewriting to clarify. [Government of United States of America]	Taken into account
5-1762	5	44	20	44	25	Work by Yokoyama and others (2012, doi:10.1029/2012GL051983) indicates that the Antarctic contribution to Holocene sea level rise ceased by approximately 4,000 years ago. Thus, the currently observed Antarctic melting appears to be a recent phenomenon. I think this geological perspective should be included because it adds context to the current melting. [Stephen Obrochta, Japan]	Taken into account
5-1763	5	44	20			GMSL was needs a space [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted
5-1764	5	44	20			This sounds like modern eustatic sea level is not reached until 2ka, which is incorrect. [Government of United States of America]	Noted. Taken into account during revision.

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5-1765	5	44	20			I am surprised that you do not mention the controversy about a possible +2 m sea-level highstand during the middle Holocene identified by some authors along the Texas coast of the Gulf of Mexico (Blum et al., 2001). Blum, M.D., Misner, T.J., Collins, E.S., Scott, D.B., Morton, R.A., and Aslan, A., 2001: Middle Holocene sea-level rise and highstand at +2 m, central Texas coast Journal of Sedimentary Research, v. 71, n° 4, p. 581-588 [Pascal Kindler, Switzerland]	This finding has been superseded by the work of John Anderson and his students
5-1766	5	44	21	44	22	It should be made clear that the statement here is based on an extensive extrapolation from data from one small part of Marie Byrd Land. I advise caution, because new surface exposure age results (manuscripts in preparation) from the Pine Island Bay area indicate a very different history of ice surface elevation change. Also, I wonder whether the extrapolation calculation took account of the fact that results from the interior of West Antarctica suggest that there has been little surface elevation change there since the LGM (e.g. Ackert et al., 1999, Science; Raynaud and Whillans, 1982, Annals of Glaciol.)? [Robert Larter, United Kingdom]	Noted.
5-1767	5	44	21			7 ka is not late Holocene. Therefore, consider replacing "late Holocene" with "middle to late Holocene" [Government of United States of America]	Noted
5-1768	5	44	22			Marie Byrd Land is in West Antarctica. Maybe insert "West" before Antarctica? [Sandra Passchier, United States of America]	Noted
5-1769	5	44	23	44	23	update Lecavalier et al., submitted [European Union]	Noted
5-1770	5	44	27	44	31	Two points: gravitational, rotational effects are not accounted for in late Holocene SL, what is the word "consistent" meant to indicate? How does one get the 25 cm estimate? The authors should clarify if global mean sea level is being discussed because regional SL variations are larger. [Government of United States of America]	Taken into account. Text has been revised for clarity.
5-1771	5	44	27			comma needed before because [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted
5-1772	5	44	30	44	31	Figure 5.17 should be cited at the end of the sentence [Mark Siddall, United Kingdom]	Noted.
5-1773	5	44	33	44	41	You should add the method from Jens Morten Hansen et al., Boreas 2011, doi:10.1111/j.1502-3885.2011.00229.x - I have also heard of another method using geo-radar on beaches, but i have not any [Aslak Grinsted, Denmark]	Noted.
5-1774	5	44	33			"causes of sea level change" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Text has been revised.
5-1775	5	44	34			In order to be more informative on the type of proxies used to resolve decimetre-scale sea level fluctuations, I suggest to modify line 34 as follows: "… the last millennium. Three types of proxies have this capability: salt-marsh plants and microfauna (foraminifera and diatoms) that form ..." [Alejandro Cearreta, Spain]	Revision of paragraph reflects prefence for salt-marsh deposits and micro-atoll for sea level reconstructions
5-1776	5	44	38	44	38	Cite Woodroffe et al. (2012). The paper already appears in the reference list and is now published. This is the highest quality micro-atoll record I am aware of. [Roland Gehrels, United Kingdom]	Noted.
5-1777	5	44	44			Replacing the second instance of tide-guage on this line with instrumental would read better [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted
5-1778	5	44	46	44	46	Kemp (2011) also uses the Tiljander proxies upside down [Marcel Crok, The Netherlands]	Sea level reconstruction used in Figure 5.17 is based on Kemp et al. 2011, but it is free from any model assumptions on temperature
5-1779	5	44	47	44	47	It is inappropriate to cite van de Plassche et al. (1998). This record has been re-interpreted as a hurricane erosion record, and can no longer be considered a record of fluctuating relative sea-level change. [Roland Gehrels, United Kingdom]	Citation is removed
5-1780	5	44	49	44	50	Change in styyle giving dates [Peter Burt, United Kingdom]	Noted
5-1781	5	44	51		53	As information from Garcia-Artola et al., (2009) has been used to produce Figure 5.17 (page 5-125), I suggest to include this reference in the last paragraph of page 5-44 when dealing with sea level variations during the transition from low Holocene rates to high modern rates of sea level change in this way: "... Variability in both the magnitude and the timing (1840–1920 CE) of this acceleration has been reported (Gehrels et al., 2008;	Noted.

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						Gehrels et al., 2011; Gehrels et al., 2006; Kemp et al., 2011; Kemp et al., 2009; Garcia-Artola et al., 2009). Gehrels and ..." [Alejandro Cearreta, Spain]	
5-1782	5	44	54	44	54	Gehrels and Woodworth is now published: Gehrels, W.R. and Woodworth, P.L. (2013). When did modern rates of sea-level rise start? Global and Planetary Change 100, 263-277, doi:10.1016/j.gloplacha.2012.10.020. [Roland Gehrels, United Kingdom]	Noted
5-1783	5	44	54	44	54	insert after "evidence": "in North Atlantic, Australian and New Zealand data sets" [Roland Gehrels, United Kingdom]	Noted
5-1784	5	44	56	44	56	It is better to rephrase this as "between 1905 and 1945 CE". There is no statistics in the cited paper and to suggest that the 20 year error is one standard deviation is not certain. [Roland Gehrels, United Kingdom]	Good suggestion, text revised
5-1785	5	45	2	45	10	Caption is poorly edited and bits are missing. Suggest re-write as follows: Observational evidence for sea level change in recent and late Holocene time. Left panels (a-d): High resolution relative sea level results from saltmarsh data at representative sites, without corrections for glacial isostatic movement of land and sea surfaces. Locations are given on the map. The rate of change occurring in North Carolina (a) late in the 19th century or early 20th century is seen in other North American Atlantic coast high resolution saltmarsh records that extend into modern time - e.g. Donnelly et al. (2004) and Gehrels et al. (2005) - and is consistent with Roman archaeological evidence (Lambeck et al., 2004). The oscillation in sea level at about 1000 CE seen in the North Carolina record occurs in some (González and Törnqvist, 2009) but not all North American records (c.f., Gehrels et al., 2011; Kemp et al., 2011). (c) The New Zealand record (Gehrels et al. 2008) is consistent with a more recent result from Tasmania (Gehrels et al. 2012); (d) Garcia-Artola et al. (2009); Leorri et al. (2008). [Roland Gehrels, United Kingdom]	Taken into account. Caption has been rewritten.
5-1786	5	45	2	45	10	Additional references: Donnelly, J. P., Cleary, P., Newby, P., Ettinger, R., 2004. Coupling instrumental and geological records of sea-level change: Evidence from southern New England of an increase in the rate of sea-level rise in the late 19th century. Geophysical Research Letters, 31, L05203, doi:10.1029/2003GL018933. Gehrels, W.R., Kirby, J.R., Prokoph, A., Newnham, R.M., Achterberg, E.P., Evans, E.H., Black, S., Scott, D.B., 2005. Onset of recent rapid sea-level rise in the western Atlantic Ocean. Quaternary Science Reviews 24, 2083-2100. Gehrels, W.R., Callard, S.L., Moss, P.T., Marshall, W.A., Blaauw, M., Hunter, J., Milton, J.A., Garnett, M.H., 2012. Nineteenth and twentieth century sea-level changes in Tasmania and New Zealand. Earth and Planetary Science Letters 315-316, 94-102, doi:10.1016/j.epsl.2011.08.046. [Roland Gehrels, United Kingdom]	Noted within focus on recent works and within space limitations.
5-1787	5	45	6	45	7	Citation style wrong [Peter Burt, United Kingdom]	Noted
5-1788	5	45	8	45	8	"the oscillations in sea level":I cannot see them on the figure. Are they oscillations in the rate of sea level change? [Masa KAGEYAMA, France]	Noted.
5-1789	5	45	9	45	9	Delete van de Plassche (1998). The Connecticut records have been re-interpreted to reflect hurricane signatures, so previously proposed fluctuations in sea level can no longer be cited as such. [Roland Gehrels, United Kingdom]	Accepted.
5-1790	5	45	14			Caption for figure 5.17: Orpheus Island does not appear on panel f--should this be replaced with "North Queensland" as on figure? [Government of United States of America]	Taken into account.
5-1791	5	45	19			Caption for figure 5.17: Is panel "l" corrected for isostasy, or only panel "j"? [Government of United States of America]	Taken into account, caption has been revised.
5-1792	5	45	23	45	23	Is it possible to give a rough range for what abrupt changes mean in years? Is it possible to give a rough range for what abrupt changes mean in years? [Government of Germany]	Note, the abrupt climate change definition used in Chapter 5 is more general. We have refrained from using definitions that include the human life-time, because many climate changes, such as Heinrich events or the PETM, that are considered abrupt by the paleo community would not be abrupt according to "narrower definitions". Nevertheless, further down in the paragraph we describe the timescales considered

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							in the subsection "This assessment of abrupt climate change on timescales of 10–100 years focuses on Dansgaard-Oeschger (DO) events and iceberg and meltwater discharges during Heinrich stadials"
5-1793	5	45	23	46	49	There is a lot of discussion of abrupt change during glacial states in this section. What is the likelihood of such events and processes to the present and future, with much smaller stands of continental ice? [Government of Australia]	Noted, this section does not deal with future abrupt climate change. Please see chapter 12 for more informatiom.
5-1794	5	45	23			Section 5.7: There is almost no mention of the paleo-evidence for the involvement of deep ocean circulation changes in abrupt climate change. There is strong evidence for deep circulation changes within the North and South Atlantic during the last deglaciation (Roberts2010, Robinson2005, Thornalley2010, Barker2010, Piotrowski2005) and relatively good evidence for deep circulation changes in the South Atlantic associated with HD-O events during the last 70 kyr (Piotrowski05). There is also new (and hopefuly published before March 2013) evidence for a direct link between North Atlantic deep circulation and Greenland climate during D-O events 19-20 (Thornalley2013, Barker2013). N. Roberts, et al., Synchronous Deglacial Overturning and Water Mass Source Changes. Science 327, 75 (2010). L. Robinson et al., Radiocarbon variability in the western North Atlantic during the last deglaciation. Science 310, 1469 (2005). D. Thornalley, et al. The deglacial evolution of north atlantic deep convection. Science 331, 202 (2010). S. Barker, et al., Extreme deepening of the Atlantic overturning circulation during deglaciation. Nature Geoscience 3, 567 (2010). A. M. Piotrowski, et al., Temporal relationships of carbon cycling and ocean circulation at glacial boundaries. Science 307, 1933 (2005). D. Thornalley et al., Abrupt changes in deep Atlantic circulation during the onset of full glacial conditions (submitted). S. Barker et al., Role of the bipolar seesaw in interglacial to glacial transitions (submitted). [Stephen Barker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text added to reflect the importance of AMOC changes in DO cycles reads now "DO variability is often associated with changes of the Atlantic Meridional Overturning circulation, as suggested by climate models of varying complexity (Arzel et al., 2010; Ganopolski and Rahmstorf, 2001; Timmermann et al., 2003) and marine proxy records (Barker et al., 2010; Kissel et al., 2008; Piotrowski et al., 2005; Roberts et al., 2010)...."
5-1795	5	45	25	45	25	What is the definition of "background" climate change ? An alternate wording could avoid this notion. [Government of France]	Changed to "Paleoclimate archives document climate changes that happened at rates exceeding the average rate of change for longer-term averaging periods prior and after this change considerably"
5-1796	5	45	25	45	26	What is background climate change? This is a new term and needs defining. Is this some sort of smoothed change? [European Union]	Changed to "Paleoclimate archives document climate changes that happened at rates exceeding the average rate of change for longer-term averaging periods prior and after this change considerably"
5-1797	5	45	25	45	26	Not all archives document abrupt climate change. Consider rewriting the setence to read: "Many higher resolution paleoclimate archives document abrupt climate changes that happened at a rate faster than background climate change." [Government of United States of America]	Changed accordingly
5-1798	5	45	25	45	26	What is background climate change? This is a new term to me and needs defining. Is this some sort of smoothed change? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Changed to "Paleoclimate archives document climate changes that happened at rates exceeding the average rate of change for longer-term averaging periods prior and after this change considerably"
5-1799	5	45	25	45	26	Unclear what this sentence means; presumably "some" paleoclimatic archives indicate this? Also, what time scales are being considered; and how is an "abrupt climate change" defined/identified? [Janice Lough, Australia]	Sentence changed to "Many paleoclimate archives document climate changes that happened at rates exceeding the average rate of change for longer-term averaging periods prior and after this change considerably (see Glossary for a more rigorous definition of abrupt climate change). " ... Further down in the paragraph we describe the timescales

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							considered "This assessment of abrupt climate change on timescales of 10–100 years focuses on Dansgaard-Oeschger (DO) events and iceberg and meltwater discharges during Heinrich stadials"
5-1800	5	45	26			Replace “have" by “has". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Corrected
5-1801	5	45	27	45	27	Does the crossing a threshold imply a fast change ? From a dynamical system point of view, the existence of nonlinearities, bifurcations, hystereses does not mean that there can be unusually fast changes (which are connected to the inertia of the system, not its complexity). [Government of France]	Noted. The term "thresholds in the underlying dynamics" implies that the if a threshold is crossed the dynamics changes - this will manifest itself in changes in the tendency terms and sometimes also in higher order statistical moments - hence leading to abrupt climate change. Unfortunately, in this short paragraph we can not go into the details of dynamical systems' analyses of abrupt climate change
5-1802	5	45	29			Change "as well as" to "and" (the grammatically correct combination is …both…., and …..). [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Corrected accordingly
5-1803	5	45	34			"iceberg/meltwater" rather than "iceberg and meltwater" [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Corrected accordingly
5-1804	5	45	38	45	39	How are "several multicentennial events" different from abrupt D-O events, which are also multicentennial? [Government of United States of America]	Many of the multicentennial events e.g. in the NGRIP records are not classified as DO events. Instead terms such as "precursor events" have been introduced. The notion of DO continuum has not been established in the literature yet. This is why this subsection sticks to the terms used in the common paleo literature.
5-1805	5	45	38	45	55	Can you give dates of the DO events referred to in this paragraph? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Any standard paper on DO variability provides these dates. We have refrained from providing the exact dates here, because this information is not crucial for the subsequent assessment in the subsection.
5-1806	5	45	38	45	55	This subject is dealt with much more effectiely here and from a rounded point of view but this paragraph is too dense and very hard to work through. For the uninitiated it is very difficult. [Mark Siddall, United Kingdom]	Noted. We have clarified the concepts and revised the paragraph.
5-1807	5	45	39			Change "period" to "cycle". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Corrected accordingly
5-1808	5	45	39			Need to add "in Greenland" after "DO events" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Corrected accordingly
5-1809	5	45	40	45	41	Replace "(stadial)" by "(Greenland Stadial=GS)" and "(interstadial)" by "(Greenland Interstadial=GI)" after Svensson et al., 2006, The Greenland Ice Core Chronology 2005, 15-42 ka. Part 2: comparison to other records, QSR 50: 3258-3267.. [Maria Fernanda Sanchez Goñi, France]	Corrected accordingly
5-1810	5	45	41	45	41	Replace "Subsequently, a more gradual cooling preceded a rapid jump to cold stadial conditions that…" by "Subsequently but within GI, a gradual cooling preceded a rapid jump to GS that…" [Maria Fernanda Sanchez Goñi, France]	Corrected accordingly
5-1811	5	45	42	45	46	This is a long and complex sentence. It needs breaking up into two, to keep it easy to follow. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1812	5	45	43	45	43	"levels" or "increases"? [Government of Australia]	Sentence changed accordingly
5-1813	5	45	47	45	47	Insert after (Martrat et al., 2007) the following paragraph: "Western European vegetation responded fast, ~100 years, and synchronously with these SST changes (Sanchez Goñi et al., QSR 2008).The strongest response to both DO warming events and subsequent cooling is observed in the northern extratropics. However, the magnitude of the change in vegetation is regionally specific and is not a simple function of either the magnitude or the duration of the change in climate as registered in Greenland ice cores (Harrison and	Noted, we have refrained from discussing the abruptness of vegetation changes, although we refer to some of the studies in the context of abrupt drought changes

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						Sanchez Goñi, QSR 2010). Refs.: Sanchez Goñi et al., 2008, Contrasting impacts of Dansgaard-Oeschger events over a western European latitudinal transect modulated by orbital parameters. QSR 27: 1136-1151; Harrison, S.P. and Sanchez Goñi, M.F., 2010 QSR Global patterns of vegetation response to millennial-scale variability and rapid climate change during the last glacial period, QSR 29: 2957-2980. [Maria Fernanda Sanchez Goñi, France]	
5-1814	5	45	47	45	51	This sentence does not make sense. It reads like there is a line of missing after DO25 that includes a period. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted, Sentence was deleted
5-1815	5	45	47			ADD ALSO THE MEDITERRANEAN DATA: subtropical North Atlantic and Mediterranean sea attain values up to 5°C (e.g., Martrat et al., 2004 and 2007) [José-Abel Flores, Spain]	Accepted, Reference added
5-1816	5	45	47			Martrat, B., Grimalt, J.O., Lopez-Martinez, C., Cacho, I., Sierro, F.J., Flores, J.A., Zahn, R., Canals, M., Curtis, J.H., and Hodell, D.A. 2004. Abrupt temperature changes in the Western Mediter- ranean over the past 250,000 years. Science, 306(5702): 1762– 1765. doi:10.1126/science.1101706. PMID:15576615. [José-Abel Flores, Spain]	Accepted, Reference added
5-1817	5	45	49	45	50	This sentence is not clear; consider rephrasing. [Government of United States of America]	Accepted, Sentenced modified.
5-1818	5	45	49	45	51	The sentence with "has further revealed that all but DO25 Northern Hemisphere DO transitions ..." is chaotic and grammatically wrong. [Manfred Mudelsee, Germany]	Accepted, Sentenced modified.
5-1819	5	45	51	45	52	"sea ice cover" and 'ice-sheet topography are not "processes". The wording should be modified. [Hugues Goosse, Belgium]	Accepted, text changed to "Processes involving sea …"
5-1820	5	45	51	45	53	Insert the following sentence in line 53 after "…regional climate impacts": "Interestingly, the regional signature of DO warming events in western Europe is modulated by orbital parameters (Sanchez Goñi et al., QSR, 2008)". [Maria Fernanda Sanchez Goñi, France]	Noted. Given the lack of space in this section, we have refrained from discussing the orbital modulation of millennial-scale variability
5-1821	5	45	51			This sentence could use re-phrasing: Neither sea ice, nor topography is a process. [Government of United States of America]	Accepted, text changed to "Processes involving sea …"
5-1822	5	45	51			Jargon alert: readers may not know what "stadials" are. Define or re-word. [Jay Curt Stager, United States of America]	Accepted, we included sentence that reads "DO events, in Greenland were marked by an abrupt transition (on a timescale of a few decades) from a cold phase (Greenland Stadial=GS) into a warm phase (Greenland Interstadial=GI)"
5-1823	5	45	51			"sea ice cover changes" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted, text changed accordingly.
5-1824	5	45	53	45	55	Clarification of text: Three issues are at play here: Firstly, The claim that the DO events occured perioddically (with a period of 1470 year) has been refuted from analysis of the accurate NGRIP (Svensson, 2008 and P. D. Ditlevsen, K. K. Andersen, and A. Svensson, "The DO-climate events are probably noise induced: statistical investigation of the claimed 1470 years cycle", Climate of the Past, 3, 129-134, 2007.). The second issue is the cause for the DO events: Are they internally or externally generated: Here the controversy is [Peter Ditlevsen, Denmark]	The text has been modified to further clarify the distinction between external and internally generated DO variability. We have removed the discussion on stochasticity as it may be too specific for our assessment task.
5-1825	5	45	53	45	55	if they are internally generated (Ditlevsen and Ditlevsen, 2009) or caused by solar forcing (Braun and Kurths, 2010; Woillez et al., 2012). There is no known variation (regular or stochastic) of the solar forcing corresponding to the DO events, which stongly indicate that they are internally generated. The third issue is if the internal dynamics (regular or stochastic) involves destabilization of the cold state through a bifurcation (Dakos, V., M. Scheffer, E. H. van Nes, V. Brovkin, V. Petoukhov, and H. Held (2008), Slowing down as an early warning signal for abrupt climate change, Proc. Natl. Acad. Sci. U. S. A., 105, 14,308–14,312) or if the abrupt changes are induced by random noise (with no early warning (P. D. Ditlevsen and S. Johnsen, "Tipping points: Early warning and wishful thinking", Geophys. Res. Lett., 37, L19703, doi:10.1029/2010GL044486, 2010). This is still controversial. [Peter Ditlevsen, Denmark]	Noted. We changed the text. The revised section reads now "In spite of the visible presence of DO cycles in many paleo-records from both Hemispheres, the underlying mechanisms still remain unresolved and range from internally generated atmosphere-ocean-ice-sheet events (Ditlevsen and Ditlevsen, 2009; Timmermann et al., 2003), to solar-forced variability (Braun et al., 2008; Braun and Kurths, 2010). However, given the lack of observational evidences for a direct linear modulation of solar irradiance on DO timescales, (Muscheler and Beer,

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							2006), solar forcing is an unlikely candidate to generate DO events"
5-1826	5	45	54	45	54	stochastic generation and internal dynamics are synonymous, in the theory of dynamical systems. [Government of France]	Accepted, Text changed accordingly
5-1827	5	45	54	45	54	More papers! Write "(Ditlevsen and Ditlevsen, 2009; Braun et al., 2010; Braun et al., 2011)". The references are: Braun H, Ditlevsen P, Kurths J, Mudelsee M (2010) Limitations of red noise in analysing Dansgaard–Oeschger events. Climate of the Past 6:85–92. ------- Braun H, Ditlevsen P, Kurths J, Mudelsee M (2011) A two-parameter stochastic process for Dansgaard–Oeschger events. Paleoceanography 26:PA3214 [doi:10.1029/2011PA002140]. [Manfred Mudelsee, Germany]	Noted. We have decided against further expanding on the Dynamical Systems' aspects of DO events, as this can perhaps only be appreciated by the expert reader.
5-1828	5	45	54	45	55	What is the difference between internal dynamics and stochastic events? Don't they at least overlap? [Government of United States of America]	Accepted, Text changed accordingly, The stochasticity discussion has been removed as something that is too specialized
5-1829	5	45	54			Typo: insert 'or' into "…or whether it was related to solar forcing…" [Jay Curt Stager, United States of America]	Noted. Whole sentence was rewritten to "Given the lack of observational evidences for a direct linear modulation of solar irradiance on DO timescales, (Muscheler and Beer, 2006), solar forcing is an unlikely candidate to generate DO events"
5-1830	5	45	55	45	55	The paper of Woillez et al. says that the relation is unlikely. [Government of France]	Noted Woillez reference removed
5-1831	5	45	55	45	55	"Internal dynamics": also this requires stochasticity to produce DO events. I would drop that expression. [Manfred Mudelsee, Germany]	Accepted, text changed accordingly. No discussion of stochasticity.
5-1832	5	45	55			Internal dynamics of what? Ocean, atmosphere, ice sheet? [Government of United States of America]	Accepted. Although Ditlefsen is not very explicit we added "internally generated atmosphere-ocean-ice-sheet events "
5-1833	5	45	55			Change ", or internal dynamics" to "or whether it reflects internal dynamics within the climate system". As it stood, the sentence was grammatically incorrect, and it was also not sufficiently specific to be unambiguous. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted, sentence changed to "In spite of the visible presence of DO cycles in many paleo-records from both Hemispheres, the underlying mechanisms still remain controversial and range from internally generated events (Ditlevsen and Ditlevsen, 2009), to solar-forced variability (Braun and Kurths, 2010; Woillez et al., 2012).
5-1834	5	45	57	45	59	The layers are evidence for, not witnesses. Therefore, consider rewriting the sentence to read: "The presence of massive layers of ice-rafted detritus in North Atlantic marine sediments and other sedimentary evidence indicate that some DO stadials, known as Heinrich stadials, were associated with iceberg discharges originating from the Northern Hemispheric ice-sheets." [Government of United States of America]	Accepted, text changed accordingly.
5-1835	5	45	58			Consider adding a comma after Heinrich stadials. By definition, H-events are ocean IRD records, DO from ice sheet atmospheric records; the way it's worded, they sound synonymous [Government of United States of America]	Accepted, sentence changed to "The presence of massive layers of ice-rafted detritus in North Atlantic marine sediments and other sedimentary evidence indicate that some DO GS, known as Heinrich stadials, were associated with iceberg discharges originating from the Northern Hemispheric ice-sheets"
5-1836	5	45	58			Insert a comma after "stadials". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1837	5	45				Abrupt Climate Change section: Why is this section restricted to DO events which are difficult to detect on land? Why not discuss Allerod, YD, or Holocene boundary with extremely abrupt shifts around the globe? [Government of United States of America]	Noted. The Boelling Alleroed is DO1. So the mechanisms discussed for the DO events also have bearing fro BA. Space too limited to discuss all types of arbupt climate changes.
5-1838	5	45				Section 5.7: This section insufficiently describes the causes of abrupt events. Especially since the B/A and YD	Noted, our section does not focus on the geographical

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						are introduced and there is no discussion of lake discharge and AMOC disruption, or pathways for glacial lake drainage, etc. It was found later in section 5.8.2, so perhaps a reference to this section is warranted here. [Government of United States of America]	aspects of lake drainages during 8.2 and YD. These were extensively discussed in AR4. Our section emphasizes recent progress in understanding the global teleconnections of abrupt climate events associated with DO variability. AMOC disruptions in associtation with DO events are now mentioned.
5-1839	5	45				Section 5.7: The topic of abrupt climate change is important, and the writing in Section 5.7 does not match the standards set by the preceding sections. This is not a copy-editing comment: the sentence fragments, poor phrasing and run-on clauses make it very difficult to discern what is actually being said. The text contains odd points and unexplained assertions that either do not connect to the topic, or that are connected poorly; for example, p 5-45 lines 52-55: "It still remains controversial whether DO variability..." does not explain why it is controversial or who considers it controversial; the second paragraph on p.5-46 segues to millenial-scale (not abrupt!) variability in the Pacific, and later in the same paragraph, line 32, it is asserted that "The hydrological response in southern Africa during HS1 is debated" without providing context, importance, or who is doing the debating and why. The authors should consider re-drafting this to align with the tone and standards of surrounding sections. [Government of United States of America]	Noted. We removed words such as "debated" and "controversial" by much more explicit statements. As mentioned in the introductory paragraph "This assessment of abrupt climate change on timescales of 10–100 years focuses on Dansgaard-Oeschger (DO) events and iceberg/meltwater discharges during Heinrich events, especially the advances since AR4 in reconstructing and understanding their global impacts and in extending the record of millennial-scale variability back in time to ~800 ka. ". As such the response of the Pacific ocean to North Atlantic abrupt climate change is very important. We have restructured the section and the Pacific sentence now reads "Moreover, atmospheric circulation changes have been invoked (Okumura et al., 2009; Xie et al., 2008; Zhang and Delworth, 2005) to explain temperature variations in the North Pacific that varied in unison with abrupt climate change in the North Atlantic region (Harada et al., 2008; Harada et al., 2012; Pak et al., 2012). Other factors that may have contributed to North Pacific climate anomalies include large-scale Pacific Ocean circulation changes (Harada et al., 2009; Okazaki et al., 2010; Saenko et al., 2004; Schmittner et al., 2007) during phases of a weak AMOC"
5-1840	5	46	4	46	4	Add the reference Sierro et al., QSR, 2009 where the authors observe,in contrast with the cited previous works, a good correspondence between DO warming events and eustatic sea level rises. Ref.: Sierro et al., 2009, Phase relationship between sea level and abrupt climate change, QSR 28: 2867–2881. [Maria Fernanda Sanchez Goñi, France]	Accepted
5-1841	5	46	4	46	4	cite Siddall et al 2008 here in preference or to balance Arz et al 2007, also Grant et al Nature 2012 gives the latest on this subject [Mark Siddall, United Kingdom]	Accepted
5-1842	5	46	4			Insert a comma after "experiments". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1843	5	46	10			Insert a comma after "ice-shelves" , and change "ice-shelves" into "ice shelves". The hyphen is required only if a contraction is made for use as an adjective. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1844	5	46	11	46	11	…do not allow us to distiguish between the two [Matthew Konfirst, United States of America]	Accepted
5-1845	5	46	11			"allow us to" instead of "allow to" [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1846	5	46	14	46	17	The B/A and YD were technically part of deglacial, not glacial as introduced above for the DO section [Government of United States of America]	Accepted, terminology corrected
5-1847	5	46	14	46	36	Line 15/16 in particular: I think it is important to emphasise the fact that there is no longer any doubt that the southern hemisphere warmed during the YD (and HS1) i.e. that these were not global cooling events (but	Accepted, We have added the sentence "Recent studies using high-resolution Greenland and Antarctic

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						were indeed global seesaw events). See also next comment. [Stephen Barker, United Kingdom of Great Britain & Northern Ireland]	records (Capron et al., 2012; Capron et al., 2010a; Capron et al., 2010b; EPICA Community Members, 2006; Stenni et al., 2011) show that with only one exception Antarctica warmed gradually during GS, reaching maximum values at the time of GS/GI transitions."
5-1848	5	46	14	46	36	This paragraph is difficult to follow. An idea to improve it would be to split it in different paragraphs for different regions (and compare data and models in each paragraph) or to have two paragraphs, one for the data the other for the models. [Masa KAGEYAMA, France]	Accepted, paragraph modified and restructured.
5-1849	5	46	14	46	36	The review of Clement and Peterson (RG, 2009) could also be cited for their synthesis of different mechanisms for teleconnections during glacial abrupt events. [Masa KAGEYAMA, France]	Accepted, reference added
5-1850	5	46	17	46	19	Care must be taken on how this issue is discussed: The implication of N. Atlantic origin is not certain…previous studies such as presence of YD imprint on N. Pacific coast and modeling results of cooler N. Pacific with effects downstream (Peteet et al. 1997 and others) suggest possible origin in N. Pacific because this ocean is so large. [Government of United States of America]	Although one can not rule out the possibility that DO events are generated in the North Pacific (in particular in view of the fact that North Pacific SSTs can influence NOrth Atlantic climate), we are not aware of any study which claims that DO events are only of Pacific origin and that the Atlantic is just passively responding.
5-1851	5	46	18	46	18	Capital 'O' required for 'ocean' [Peter Burt, United Kingdom]	Accepted
5-1852	5	46	21	46	23	It is not just southward displacement of fronts that demonstrates the interhemispheric character but also warming during HS1 and the YD (and also southern cooling during the B/A). This is evidenced in papers by: Putnam10, Schaefer06, Kaplan10, Barker09, Lamy07. A. E. Putnam et al., Glacier advance in southern middle-latitudes during the Antarctic Cold Reversal. Nature Geoscience 3, 700 (2010). J. M. Schaefer et al., Near-synchronous interhemispheric termination of the last glacial maximum in mid-latitudes. Science 312, 1510 (2006). M. R. Kaplan et al., Glacier retreat in New Zealand during the Younger Dryas stadial. Nature 467, 194 (2010). S. Barker et al., Interhemispheric Atlantic seesaw response during the last deglaciation. Nature 457, 1097 (2009). F. Lamy et al., Modulation of the bipolar seesaw in the southeast pacific during Termination 1. Earth Planet. Sci. Lett. 259, 400 (2007). [Stephen Barker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Sentence changed to "Recent global temperature compilations (Shakun et al., 2012), Southern Ocean temperature records (Barker et al., 2009; De Deckker et al., 2012; Lamy et al., 2007), evidence from Southern Hemisphere terrestrial records (Kaplan et al., 2010; Putnam et al., 2010) and transient climate model experiments (Menviel et al., 2011) provide multiple lines of evidence for the interhemispheric character of millennial-scale variability during the last glacial termination and for DO events (high confidence). "
5-1853	5	46	23	45	27	Heinrich Stadial 1 accompanied (and probably caused) a massive collapse of the rainfall systems throughout the Afro-Asian monsoon system, one of the most severe and widespread drought events in the history of anatomically modern humans. This should be mentioned here, as an example of the potential effects of abrupt shifts in ice dynamics and climate systems. Genetic evidence from India suggests a major population bottleneck resulted from it. Citation is Stager et al. 2011, Science 331:1299-1302. [Jay Curt Stager, United States of America]	Noted. We are refraining from including statements on how past climate change has affected humans and the progress of mankind. Our main task in this chapter is to assess the current knowledge of past changes of the physical climate system.
5-1854	5	46	23	46	27	On the relationships between North Atlantic cooling and tropical hydrological changes, there are other studies: Kageyama et al, CP, 2009, Merkel et al, QSR, 2010 + a comparison of fresh water hosing experiments under glacial conditions (Kageyama et al, CPD, 2012). [Masa KAGEYAMA, France]	Accepted, Reference added
5-1855	5	46	27	46	31	Add in this sentence between "...Weldeab et al 2007) and" "eastern Mediterranean (Fleitmann et al., 2009)" the western Mediterranean (Sanchez Goñi et al., Climate Dynamics 2002, Combourieu-Nebout et al., 2002; Fletcher and Sanchez Goñi, QR 2008). In this region the Heinrich events where contemporaneous with extreme drying episodes (50% of precipitation lowering). Refs.: Combourieu-Nebout, N. et al., 2002, Enhanced aridity and atmospheric high-pressure stability over the western Mediterranean during the North Atlantic cold events of the past 50 ky, Geology 30: 863-866; Fletcher, W.J. and Sanchez Goñi, M.F., 2008, Orbital- and sub-orbital-scale climate impacts on vegetation of the western Mediterranean basin over the last 48,000 yr, QR 70: 451-464. [Maria Fernanda Sanchez Goñi, France]	We included the Fletcher and Sanchez Goni reference.

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5-1856	5	46	31	46	31	For hydroclimate anomalies along south america associated to the ITCZ southward displacement, please this addittional citation: Blard, P. H., Lavé, J., Farley, K. A., Fornari, M., Jiménez, N., and Ramirez, V. (2009). Late local glacial maximum in the Central Altiplano triggered by cold and locally-wet conditions during the paleolake Tauca episode (17-15 ka, Heinrich 1). Quaternary Science Reviews 28, 3414-3427. [Eugenia M. Gayo, Chile]	Rejected. The paper focuses only on the HS during the deglacial. It is difficult to assess whether the presented changes are systematic for other DO events.
5-1857	5	46	31			The phrase "hydrological signal" is quite vague--could be a drought. Does this mean precipitation, effective moisture…? Please clarify. [Government of United States of America]	Accepted, text changed to "wetter conditions"
5-1858	5	46	32	46	35	On mechanisms for Indian monsoon changes in response to AMOC changes, see Marzin et al, submitted (to CP) [Masa KAGEYAMA, France]	Paper still not accepted.
5-1859	5	46	34			The phrase "reduction of hydroclimate indicators" is quite vague. Please clarify. [Government of United States of America]	Accepted, text modified accordingly
5-1860	5	46	38	46	42	This paragraph, presenting the most recent attempts at understanding glacial abrupt climate changes with GMCs might be in a better position before the previous paragraph, which uses the results from these simulations. [Masa KAGEYAMA, France]	Accepted, Text changed accordingly
5-1861	5	46	41	46	41	Write "GCM *modellers* still have difficulties ...". [Manfred Mudelsee, Germany]	Sentence deleted
5-1862	5	46	44	46	46	The detection of millennial-scale events in older glacial cycles was also done (pre-Barker) by Siddall et al. (2006) and Siddall et al. (2010). I am especially worried about the duplication of the Basrker study in terms of its philosophy relative to the 2006 paper out of those two. I suggest that these papers (esp. the 2006 paper) may need to be mentioned here to be correct. Siddall M., Stocker T.F., Spahni R., Blunier T., McManus J., Bard E., 2006: Using a maximum simplicity paleoclimate model to simulate millenial variability during the last four glacial cycles, Quaternary Science Reviews, 25, 3185-3197 Siddall, M., Rohling, E.J., Blunier, T., and Spahni, R., Patterns of millennial variability over the last 500 ka. Climate of the Past, 6, 295-303, 2010. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Noted. We added a few more of the suggested references
5-1863	5	46	44	46	58	I do not think Barker et al should be cited here. It is an essentially heuristic (certainly not statisitcal) model with many contentious assumptions and which goes well beyond what can reasonably be worked out beyond 500 ka given the resolution of EDC data, which approaches 500 years in the older parts of the record. It is a sole methodology with little other supporting work. It certainly does not merit a plot. This space would be better used to flesh out the dense paragraphs above some more. [Mark Siddall, United Kingdom]	We have deleted Figure 5.18 and the Barker data are now cited in the proper context along with the Siddall 2006 and Siddall 2010 papers
5-1864	5	46	46	46	46	Please include the publication “Centennial mineral dust variability in high-resolution ice core data from Dome C, Antarctica” by Lambert et al., 2012 in the journal “Climate of the Past” (doi:10.5194/cp-8-609-2012) here. Lambert and colleagues used statistical analysis of high-resolution ice core dust data to find occurrences of Antarctic warming events and predict the occurrence of D/O events in the last 8 glacial cycles, whereas Barker et al., 2011 only used low resolution dD data. Figure 4 in Lambert et al., 2012 compares the two datasets. Although Lambert et al., 2012 and Barker et al., 2011 agree well enough on the occurrences of D/O events in the past 350 ka, the high-resolution data used by Lambert et al., 2012 allows the identification of many more D/O events in older glacials. Lambert et al., 2012 find a constant rate of 2-3 events per 10 ka, so the sentence could be updated to “..., occurred regularly during glacial cycles extending back ~800 ka...” [Fabrice Lambert, Republic of Korea]	Reference included
5-1865	5	46	51	46	57	I am not sure this figure is really needed here. It is not a synthesis and is well summarised in the last paragraph of the section. If we need an illustration for this section, I would rather favour an illustration of the mechanisms of the climate response to AMOC changes from the different models, showing where model-data comparisons could help... [Masa KAGEYAMA, France]	Previous figure 5.18 was deleted
5-1866	5	46	54			gray or grey (inconsistent with earlier captions) [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Taken into account.
5-1867	5	46	56	46	57	Figure 5.18, caption, method by Barker et al. (2011): The rationale behind this method must be communicated. This caption is clearly less informative than all other captions in Chapter 5. [Manfred Mudelsee, Germany]	Previous figure 5.18 was deleted

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5-1868	5	46				Figure 5.18: The inclusion of the upper panel (seesaw activity) is useful but perhaps leads the reader to think that the main point is an increase in seesaw activity towards the present. In fact the main point is that seesaw activity is enhanced for intermediate climate states and during the transitions between states. The apparent increase in seesaw activity towards the present is most likely to be an artefact (at least in part) of the loss of temporal resolution in the deeper parts of the ice core. I suggest describing the relationship between millennial-activity and background state more explicitly (either in main text or figure annotation). It might also be worth normalising the activity record to account for the gradual loss of resolution as age increases (I am happy to assist here if required). [Stephen Barker, United Kingdom of Great Britain & Northern Ireland]	Previous figure 5.18 was deleted
5-1869	5	46				Figure 5.18. The meaning of the gray-black-white bar on the right is not clear. If it's simply a legend to show "terciles"--then it fails. Stretched out like that it appears to be imparting some information about the equivalent period of time. And what is a tercile of bipolar seesaw activity? Maybe, that for the black, 33% of the time there is seesaw activity? or is it that the bipolar activity is only one-third as strong as at full-strength times? [Government of United States of America]	Previous figure 5.18 was deleted
5-1870	5	47	1	47	7	This section on "irreversibility" is followed by examples from the cryosphere that are seem counter to the point. That is the examples, from Greenland and West Antarctica, are both of ice sheet reductions that in fact were reversed. This passage seems to make the case for long-term reversibility rather than irreversbiility. Similarly the example North Atlantic THC changes documented were reversible. Why not simply say the paleo record implies long time constants for recovery of some systems, but not "irreversibility"? The ocean carbon cycle and ocean acidification represent a similar problem - the long term buffering mechanisms imply the carbon perturbtion we are applying will be "reversed" or buffered but only on very long time scales. [William Howard, Australia]	The term "irreversibility" in this chapter is used not in theoretical but in practical sense, i.e. "irreversible" means that recovering takes long time (100 yr amnd more).
5-1871	5	47	1			Should Perspective not be plural? Does paleoclimate provide a single,coherent insight into the issue [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted. "Perspectives"
5-1872	5	47	1			Section 5.8: very well written [Manfred Mudelsee, Germany]	Noted.
5-1873	5	47	3	47	4	Please incert magnitutde of cooling in Greenland as 3.3+-1.1 degree C (Kobashi et al., 2007): Kobashi, T., J. Severinghaus, E. J. Brook, J. M. Barnola, and A. Grachev (2007), Precise timing and characterization of abrupt climate change 8,200 years ago from air trapped in polar ice, Quarternary Science Reviews, doi:10.1016/j-quascirev.2007.1001.1009. [Takuro Kobashi, Japan]	Taken into account.
5-1874	5	47	3			It is not intuitive what is meant by- or what the implications are of - climate sytem irreversibility. It would be worthwhile to find a way to rewrite the opening sentence, perhaps along the lines of: "Climate system irreversibility is defined such that after a perturbation or forcing is removed, the climate system will not immediately reverse the trajectory of change and return to its initial state." [Government of United States of America]	Noted. The opening sentence has been removed and the definition of irreversibility is given in the Glossary.
5-1875	5	47	4	47	4	No. This can be modelled with systems with multiple equilibria, but does not imply that the system yields multiple equilibria. Such a definition also implies that the system is at equilibrium, and the 46 preceding pages seem to argue against this. [Government of France]	The meaning of "No" is unclear. The definition of "irreversibility" does not imply that the system is in equilibrium
5-1876	5	47	5	47	7	I understand the practical point, but the word "irreversibility" has a definite meaning in the English language, which does not allow for a slow change back to original conditions. This caused me confusion in Chapter 4 - see comment 191. An alternative way of expressing only very slow recovery would be preferable. [Adrian Simmons, United Kingdom]	In the AR5 (Chapters 4,5 and 12) "irreversibility" is considered for the "human" (i.e. centennia)l time scale. This is different from the defenition of "irreversibility" in thermodynamics
5-1877	5	47	6	47	6	Elsewhere in the report irreversible is defined as not reversible on the timescale of decade or centuries [European Union]	Noted
5-1878	5	47	7	47	7	Specify what is significantly longer ; 10 times, 100 times longer? [Government of France]	The sentence was removed
5-1879	5	47	9			The title should read "Ice sheets" because other crysphere components are not treated here [Wilfried Haeberli, Switzerland]	Accepted. The subsection is renamed.
5-1880	5	47	11			The 400-450 ppm CO2 as a threshold for SH glaciation initiation appears much in the literature, but the	There are several lines of evidences for existence of

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						mechanisms by which this should be true aren't (Model tuning?) [Government of United States of America]	such threshold
5-1881	5	47	13	47	13	Charbit et al (GRL, 2008: Amount of CO emissions irreversibly leading to the total melting of Greenland, Geophys. Res. Lett., 35, L12503, doi:10.1029/2008GL033472) could also be cited here [Masa KAGEYAMA, France]	Charbit etal is cited in the Chapter 12 where it is more appropriate
5-1882	5	47	14	47	18	The sentence is long and unwieldy. Could the clause about proxy observations of CO2 be moved to a later separate sentence [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1883	5	47	15	47	16	I feel unconfortable with this sentence "...likely caused by gradual atmospheric CO2 concentration decline on geological time scale…". The major forcings that sill and gateway openings and closings constitute on geological time scale, seems here (as the sentence is constructed) to be neglected. This could become a severe criticism/negative consideration from some geologists. [Frédérique Eynaud, France]	Models suggest that CO2 decline was more important than the Drake passage opening, although this is still a hypothesis.
5-1884	5	47	18	47	18	before Langebrook et al, cite Gasson, E., Siddall, M., Lunt, D.J., Rackham, O.J.L., Lear, C.H., Pollard, D., 2012, Exploring uncertainties in the relationship between temperature, ice volume, and sea level over the past 50 million years, Rev. Geophys., 50, 1, doi 10.1029/2011RG000358. [Mark Siddall, United Kingdom]	Accepted. Gasson et al 2012 will be cited instead of Pollard and DeConto (2003)
5-1885	5	47	19	47	19	suggest to replace "…previous interglaciations…" with "….previous interglacial periods…." [Government of Poland]	"interglaciation" changed to "interglacial"
5-1886	5	47	22	47	22	"At the same time results of model simulations (Pollard and De Conto)" --> "At the same time, models (e.g. Pollard and De Conto)... [Masa KAGEYAMA, France]	Taken into account. Text is rewritten
5-1887	5	47	22	47	22	At the same time results of a model simulation… [Matthew Konfirst, United States of America]	Taken into account. Text is rewritten
5-1888	5	47	22	47	25	This is a direct repetition of what went before. This needs to be better coordinated with the sea level section [Mark Siddall, United Kingdom]	Noted
5-1889	5	47	23	47	23	implies' [Mark Siddall, United Kingdom]	corrected
5-1890	5	47	23	47	23	This IMPLIES, with a medium..... [Rob Wilson, United Kingdom]	corrected
5-1891	5	47	23	47	24	This implies, with a medium degree of confidence, that [Matthew Konfirst, United States of America]	corrected
5-1892	5	47	23	47	24	medium degree of confidence' - please use the terms as described in the IPCC AR5 uncertainty guidance note. [Thomas Stocker/ WGI TSU, Switzerland]	Changed to "medium confidence"
5-1893	5	47	23			"This implies" rather than "This imply" [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	corrected
5-1894	5	47	23			typographical error [Michael Hren, United States of America]	corrected
5-1895	5	47	23			"the WAIS" insert "the" and "implies" instead of "imply"? [Sandra Passchier, United States of America]	corrected
5-1896	5	47	23			Change "imply" to "implies". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	corrected
5-1897	5	47	23			This implies [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	corrected
5-1898	5	47	24	47	25	…concentration stays above 420 ppm… [Matthew Konfirst, United States of America]	Noted
5-1899	5	47	24			"will be lost" seems very strong for me, "could be lost". [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1900	5	47	25	47	39	Re.: The two last sentences in these two paragraphs. Although I wholeheartedly agree with the statements, I am concerned that both of these sentence ends mention timescales as a fact, whereas the arguments in thes paragraphs did not go into the timescales at all. I think something needs to be added, be it a reference, or a referral to a relevant section for the timescales. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account.
5-1901	5	47	27	47	27	Observational evidence suggests that… [Matthew Konfirst, United States of America]	corrected

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5-1902	5	47	27			The text should be clarified to read, "evidence suggests". In fact, this section needs some editing for style, grammar [Government of United States of America]	corrected
5-1903	5	47	27			"suggests" instead of "suggest" [Sandra Passchier, United States of America]	corrected
5-1904	5	47	27			suggests [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	corrected
5-1905	5	47	31	47	31	To make this more readable can you give the date of MIS 11? [European Union]	Noted but text on MIS11 has been deleted
5-1906	5	47	31	47	31	To make this more readible can you give the date of MIS 11? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted but text on MIS11 has been deleted
5-1907	5	47	31	47	31	"during interglaciations exceptionally long" --> "during interglaciations which were exceptionally long" [Masa KAGEYAMA, France]	Noted but text on MIS11 has been deleted
5-1908	5	47	31	47	31	considerably reduced' there is much more that can be said about the LIG in Greenland than this. 'comnsiderably' is ambiguous and general. This section again repeats much of the sea level section. I recommend this is combined as 'sea level and cryosphere' [Mark Siddall, United Kingdom]	Noted. The word "considerably" has been removed. Suggestion to merge 5.6 and 5.8 is rejected
5-1909	5	47	31			Interglacials not interglaciations (see my comment on 5-40, L17) [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted. Now we use "interglacial"
5-1910	5	47	31			"interglacations exceptionally" with "interglacials that were exceptionally" [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted but the sentence about MIS11 has been removed.
5-1911	5	47	31			This statement could be re-written to be more clear [Government of United States of America]	Taken into account. The sentence is rewritten
5-1912	5	47	31			"interglaciations (interglacials) that were exceptionally long" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Taken into account. The sentence is rewritten
5-1913	5	47	32	47	32	Here, and elsewhere: should be consistent in how the text refers to "Marine Isotope Stage 5.5" vs "Last Interglaciation." [Robert Kopp, United States]	Accepted
5-1914	5	47	32	47	33	Section 5.8.1: It is not clear that the physical evidence regarding ice sheet extent is the support for the model. Actually, it could very easily be the other way around, or should be. The authors should consider rewriting the sentence to begin: "Modelling results indicate…" [Government of United States of America]	Noted. The text has been rewritten to clarify the findings based on proxy records or based on simulations.
5-1915	5	47	33	47	34	This seems to contradict the fidning for the LIG (NEEM) that only 2 m was lost at a temperature well above present [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	It does not contradict becuase the Greenland ice shet has a very long (millennial) response time
5-1916	5	47	35	47	35	"multiple equilibrium states under present day climate state" --> "multiple equilibria under present day conditions" [Masa KAGEYAMA, France]	Accepted.
5-1917	5	47	36	47	36	update Born and Nisancioglu, submitted [European Union]	Updated
5-1918	5	47	36	47	39	Is it possible to put some confidence statemnt on this sentence? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	"medium confidence" is stated in this sentence
5-1919	5	47	36	47	39	Yes, the WAIS takes a thousand years to FULLY respond but this DOES NOT mean that not much happens in the first century of the response. Put the current way, a reader may think there is no cause for concern, which is not the case. Pollard and DeConto have published rates of sea level rise associated with a full collapse. [Mark Siddall, United Kingdom]	Noted. However, short term projections are discussed in section 13.
5-1920	5	47	38	47	38	"temperature rise which" --> "temperature rise. This" [Masa KAGEYAMA, France]	Rejected. This suggestion changes the meaning of the sentence.
5-1921	5	47	38	47	39	It would be nice to have a reference for this statement. [Masa KAGEYAMA, France]	The references are added
5-1922	5	47	38			"temperature rise which is expected already in this century" is a clumsy turn of phrase. Try "temperature rises anticipated this century" or "temperature rise to which we are already commited" (although the latter would need cross-checking with the projections). [Christopher Brierley, United Kingdom of Great Britain & Northern	The sentence is rewritten

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						Ireland]	
5-1923	5	47	38			This statement implies that surface air temperature affects ice sheets, while adjacent ocean could be more important, at least in terms of mass balance in ice sheet regions where it's melting. Is this accurate? [Government of United States of America]	Noted. The sentence refers to atmospheric and oceanic temperatures since they are both important.
5-1924	5	47	39			How do we know they are very long? The melt can actually be very rapid, no? [Government of United States of America]	The time scales of the ice sheets responseto climate change are known only from modeling. Model reults show that for realistic climate change, the time scale of the ice sheet response is at least multi-centennial
5-1925	5	47	41			Re irreversibility ocean: Focus is clearly on Atlantic regions. Please comment on why other basins are not covered in equal detail. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted
5-1926	5	47	44			The authors should consider including the Morrill et al (2012 subm) reference here. [Government of United States of America]	Accepted, text and figure updated
5-1927	5	47	53	47	53	"new observational and model evidence" --> "new observational evidence and model results" [Masa KAGEYAMA, France]	Accepted, Text changed accordingly
5-1928	5	47	53			"are published" to "have been published" [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1929	5	47	53			The text would be more clear if it read: "have been published" [Government of United States of America]	Accepted
5-1930	5	47	55			Shouldn't this refer only to Fig. 5.19a, b? Panel d is deep water and the sentence is referring to surface and atmospheric proxies. [Government of United States of America]	Accepted, text modified accordingly
5-1931	5	47				Section 5.8.1 and 5.8.2: These subsections contain important points and key references, but are poorly-written to the point of being hard to understand, and is not sufficiently careful in drawing synthesis conclusions from cited data (e.g. 5-47 lines 23-25: "This imply (sic) with a medium degree of confidence that a large part of the WAIS will be eventually lost if the atmospheric CO2 concentration will stay above 420 ppm for several millennia.") [Government of United States of America]	Accepted, will be revwritten
5-1932	5	48	1	48	30	This section misses important papers on St Lawrence, Mackenzie river lake drainage [especially for the YD and debate about drainage pathway], studies of the 8.2 k paleoceanography; modeling papers on whether lake influx really can disrupt AMOC [Condron] [Government of United States of America]	Rejected, focus is here on the reversability aspects of the ocean circulation
5-1933	5	48	2			McManus et al. (2004) do not make these points for the 8.2 ka event, so does not seem appropriate here. In the entire section 5.8.2., I find that references may have been a bit carelessly used. I suggest that the authors of this section carefully cross-check whether the cited references indeed are fully appropriate for the points made. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Noted. Text and references have been revised.
5-1934	5	48	3	48	3	Bad English and physics! Temperatures cannot cool, they can decrease or be reduced. Replace 'cooling' with 'decreasing' [Peter Burt, United Kingdom]	Accepted
5-1935	5	48	6	48	6	The m**3 values: it would help the direct quantitative understanding of this paragraph if you gave these m**3 values also as Sv-years. [Manfred Mudelsee, Germany]	Noted, but SV years is a difficult term for the non specialist
5-1936	5	48	6			x10 rather than .10 [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1937	5	48	8			"collapse of the saddle" rather than "collapse of saddle" [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1938	5	48	8			Change "a collapse of saddle" to "the collapse of a". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1939	5	48	9	48	14	The sentence puts an unreasonable amount of faith in the climate model performance. The lack of simulated response could equally be a model deficiency or the small number of four ensemble members as a misprescription of initial conditions or freshwater forcing. [Government of United States of America]	Noted. Text has been revised.

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5-1940	5	48	10			Change "give" to "gives". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1941	5	48	10			"only gives" [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1942	5	48	13			"a freshwater" rather than "the freshwater" [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-1943	5	48	21	48	23	The authors should consider revising this last sentence, or strike the reference to the figures. These two panels e and f don't show anything about the recovery from the cold anomaly, they show the effect of the cold anomaly. [Government of United States of America]	Rejected, time scale of recovery is important
5-1944	5	48	21			Existence of Labrador Deep Sea Water is important in GCM recovery length (LeGrande and Schmidt 2008) [Government of United States of America]	Noted. Text has been revised.
5-1945	5	48	27	48	27	Maybe a word should be added to explain that we are still at a stage of a qualitative comparison (of the sign of the change) and not yet at a quantitative comparison stage? [Masa KAGEYAMA, France]	Noted. Text has been revised.
5-1946	5	48	29	48	30	Add “(e.g., Cheng et al., 2009)” at the end of the sentence. Cheng et al. (2009. Geology, doi: 10.1130/G30126A.1) clearly showed the mentioned hemispheric see-saw pattern that existed during the abrupt climate-change event at 8.2 ka with accurately dated speleothems from both hemispheres. Moreover, Cheng et al. (2009) was published after the AR4. [Government of Brazil]	Noted. Text has been revised.
5-1947	5	48	29	48	30	To more clearly and logically construct the setence, the authors should consider the following re-write: "Observations indicate that a hemispheric see-saw pattern existed during the event, and this is supported by model experiments." [Government of United States of America]	Noted. Text has been revised.
5-1948	5	48	29			"hemipsheric see-saw event" to "bipolar seesaw" for consistency with earlier mentions [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Noted. Text has been revised.
5-1949	5	48	37	48	37	Figure 5.19, caption: Write "NGRIP". [Manfred Mudelsee, Germany]	Accepted.
5-1950	5	48	44			Caption for figure 5.19: Insert "(IMINT)" after "water temperature" to refer to the acronym used in the figure. [Government of United States of America]	Accepted.
5-1951	5	48	51	48	55	Why use the results from Stouffer et al 3005 and not the models used for the rest of the figure? [Masa KAGEYAMA, France]	Accepted, new paper by Morrill et al used in revised figure. Stouffer et al was a placeholder.
5-1952	5	48	52	48	52	Figure 5.19, caption: Explain how the significance has been determined (i.e., which statistical test was employed). [Manfred Mudelsee, Germany]	Rejected, will be too lengthy and technical.
5-1953	5	49	1	49	1	Insert "Recent" between 'Major Drivers of' and 'Climate Change?' in FAQ heading [Government of Australia]	Rejected. We would like to answer this question in a slightly more general way.
5-1954	5	49	1	49	1	This FAQ deals with a topic that is also of deep interest to Chapter 10. I have the impression that the response would benefit by iterating its text more extensively with Chapter 10 authors. [Francis Zwiers, Canada]	Taken into account. Is done.
5-1955	5	49	1	49	1	I also have the impression that the response to this FAQ could be improved by selecting only a couple of key subquestions and focuing only on those. At the moment, the response attempts to answer the question very broadly, across a very broad range of time scales, and by also attempting to provide a description of all of the main contributors to historical climate variability. Perhaps a more focused approach would be to describe what we know about TSI variations over the past millennium into the present (how has TSI varied over the past millennium, and how do we know), and what we know about the climatic response to those variations (has TSI variation affected surface temperatures, and if so, by how much). Assessments of observed or reconstructed temperature change (from Chapter 2 and this chapter) and attribution assessments from Chapter 10 can be used to put the estimated response to TSI variations into context with having to provide a full explanation of the decomposition of temperature variability into its various components. Doing just these two things would already be quite a lot. [Francis Zwiers, Canada]	Noted. The text has been strongly restructered. Only the main contributors to climate change at all times are discussed.
5-1956	5	49	1	49	1	While I think the response needs to be more focused, I do provide specific comments on the text as it exists, in case those are useful to the authors. [Francis Zwiers, Canada]	Taken into account. The text has been strongly restructered.

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5-1957	5	49	1	50	49	In general, FAQ 5.1 is not well written and the flow of information is confusing (starts with solar forcing, jumps to other natural forcing and anthropogenic factors, then back to solar forcing again, then back to the combination of factors etc.) There is also a puzzling focus in places on the increases in TSI during half of each solar cycle without mentioning that TSI declines during the other half. The first few paragraphs of this FAQ should focus on explaining that there are two types of solar forcing: the 11 year cycle, during which the positive and negative forcing during consecutive halves of the cycle cancel out, and then longer terms trends in solar irradiance (i.e. move up some of the text from lines 46-52). The former can influence climate on short timeframes, but the latter is of more importance on longer timescales. This message is not coming through well in the current draft. Start the FAQ by describing both kinds of solar forcing, move to other forcing factors, then put it al together by examining their influence on different timescales. [Government of Canada]	Taken into account. The text has been strongly restructered.
5-1958	5	49	1	50	49	This FAQ is generally good. The section on lines 54 of p49 and line 5 of p50 doesn't always address the question of the FAQ. This doesn't get that addressed in Ch. 10. [European Union]	Noted.
5-1959	5	49	1	50	49	This FAQ is generally good. The section on lines 54 of p49 and line 5 of p50 doesn't always address the question of the FAQ. This doesn't get that addressed in Ch 10. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted.
5-1960	5	49	1	50	50	FAQ5.1: What is 'CE' after years? Is not common for all readers (at least not for non english natives). Could be omitted, there is no 'CE' after e.g. 1978 either, and this is the same 'unit'. [Urs Neu, Switzerland]	Noted. Official ICPP notation.
5-1961	5	49	1	50	51	FAQ 5.1 (Solar forcing): In my view this FAQ is generally in good shape for the non-expert reader. However from time to time claims arise that global climate is affected by variations in galactic cosmic radiation (GCR) , which are modulated by the solar cycle. I suggest that the Lead Authors consider adding a paragraph to this FAQ to deal with the GCR issue, perhaps drawing on lines 41 -44 of page 8 of the SPM [David Wratt, New Zealand]	Noted. Correct, there are additional phenomena such solar energetic particles, solar magnetic field interactions which all could be mentioned but are beyond the scope of a FAQ.
5-1962	5	49	1	50	54	FAQ 5.1: good discussion. Note however that although some references deal with UV variability as distinct from TSI variability, the question of the difference in radiative forcing as opposed to indirect effects via stratospheric changes and planetary wave structure (cf. Sect. 9.1.3.2.7) could hardly be treated here. Reference to 8.4.1.4. [Robert Kandel, France]	Noted. For the sake of simplicity we prefer not to go into spectral aspects.
5-1963	5	49	1			I think that the structure of FAQ 5.1 could be improved. Also, the text is somewhat redundant in places. [Georg Feulner, Germany]	Taken into account. Text strongly restructered.
5-1964	5	49	1			This would be an opportunity to talk about TSI vs. variability in individual spectra. Possible atmospheric chemistry changes etc. [Government of United States of America]	Noted. For the sake of simplicity we prefer not to go into spectral aspects.
5-1965	5	49	1			Title of FAQ 5.1: Should this not be "major driver of changes in climate" or "major driver of climate change". Driver of climate changes seems not to be good english. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
5-1966	5	49	3	49	8	This is not true. Tectonics are a product of geothermal energy, The tides are a product of gravitational potential energy. Geothermal energy itself plays important roles in some instances. Although these are not dominant, it is confusing to the uninitiated without an explicit statment about this (also we don't really know how important tides are) [Mark Siddall, United Kingdom]	Noted but beyond the scope of the FAQ which is focused on the major source of energy.
5-1967	5	49	3	49	8	These two sentences seem contradictory. First you say that solar variability together with anthropogenic factors combine to explain much of the observed changes. Then you say in the next sentence "that variability" has made only a minor contribution to the observed increase in SAT. Suggest some rewording of this chapeau to make your message clearer, and avoid any potential contradiction. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
5-1968	5	49	4	49	4	Replace "output" with "source", to connect better with the first sentence. [Francis Zwiers, Canada]	Accepted.
5-1969	5	49	6	49	6	Replace "changes" with "change" [Francis Zwiers, Canada]	Accepted.
5-1970	5	49	7	49	7	Replace "that variability" with "solar variability" to be clear about what variability is being discussed. [Francis Zwiers, Canada]	Accepted.

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5-1971	5	49	7	49	8	Mentions contribution of sun to climate change in the past, but does not mention conclusion that "During the future decades solar forcing will only play a minor role." as stated on page 50, lines 48-49 [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account. Text has been restructured
5-1972	5	49	8	49	8	Replace "over the past 120 years" by "since 1950" (or "over the past 60 years"). Reason: Most other FAQs mainly discuss the period since 1950 (due either to measurement availability or to the fact that after 1950 anthropogenic influence is largely dominant). The choice of a consistent period over all FAQs would be helpful for readers. [Urs Neu, Switzerland]	Accepted.
5-1973	5	49	10	49	10	Replace "This produces" by "This process produces". Reason: Otherwise can be confusing ('this' could relate to Helium) [Urs Neu, Switzerland]	Accepted.
5-1974	5	49	16	49	16	Explain what "radiometers" are for lay people (e.g., perhaps insert "(instruments that measure the flow of electromagnetic energy)", or perferably, keep the text simple by talking about "Satellite-based instruments". [Francis Zwiers, Canada]	Accepted.
5-1975	5	49	17	49	17	This is only a minor point, but 1361 W/m2 would be more consistent with the rest of the Chapter than 1360 W/m2. [Georg Feulner, Germany]	Accepted.
5-1976	5	49	19	49	19	Change "energy back into space." to "energy back into space at the same wavelength." [Government of Australia]	Taken into account. Wording changed accordingly.
5-1977	5	49	23	49	25	These sentences only describe half the solar cycle, and so give the impression that the change in TSI ("an increase of about 0.1% (1.4W/m2)"), is a directional change (an increase), rather than a cyclical one. An additional sentence should be added to explain that a corresponding decline in TSI occurs in the second half of the solar cycle, when activity wanes. [Government of Canada]	Taken into account. Text changed, also illustrated by panel b of the figure.
5-1978	5	49	24	49	24	Replace "(in recent years)" by "(in recent decades)". Reason: Speaking of 'average' over several 11y cycles it must be more than a few years, at least several decades. [Urs Neu, Switzerland]	Accepted.
5-1979	5	49	27	49	27	Replace "over the past 30 years" by "since 1950" or "since the mid-20th century" (or "over the past 60 years"). See comment above (Reason: Most other FAQs mainly discuss the period since 1950 (due either to measurement availability or to the fact that after 1950 anthropogenic influence is largely dominant). The choice of a consistent period over all FAQs would be helpful for readers.) [Urs Neu, Switzerland]	Taken into account. Text changed.
5-1980	5	49	27	49	27	This is an example of where better coordination is required with Chapter 10. There are very few detection and attribution studies that consider only 30-year periods (which is considered short for detection and attribution). Current studies typically consider either the period since 1900 or 1950, depending upon observational data availability. [Francis Zwiers, Canada]	Taken into account. Restriction to 30 years removed.
5-1981	5	49	28	49	28	Please make sure that Chapter 10 authors are comfortable with the use of this figure, since they have the task of assessing the detection and attribution literature. [Francis Zwiers, Canada]	Accepted. Figure replaced with new data from chapter 8.
5-1982	5	49	29	49	29	For an FAQ, we would suggest that you briefly explain what is meant by 'internal variability'. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. Explanation added.
5-1983	5	49	32	49	33	Similar to the comment about lines 23-25, again the focus here is only on the increase in TSI during the part of the solar cycle when activity surges. Why this focus? It is misleading not to mention that a corresponding cooling effect occurs during the waning half of the solar cycle. [Government of Canada]	Taken into account. Text restructered, word "temperature fluctuation" induced.
5-1984	5	49	32	49	33	Cross check with Chapter 10 - I'm not sure that many studies detect this signal, although Chapter 10 discuss a study that uses the response to the 11-year solar cycle to estimate the transient climate response (see 10.8.1). [Francis Zwiers, Canada]	Noted. The presented mean global temperautre record shows a spectral line at ~11 years.
5-1985	5	49	34	49	34	Replace "by around 0.3°C to 0.4°C for a few years" by "by around 0.1°C to 0.3°C over one or two years". Reason: Comparison with Figure 1 would be confusing, since the volcanic forcing in Figure 1c shows no cooling of more than -0.25°C, and cooling of more than 0.1°C does not persist over more than two years. [Urs Neu, Switzerland]	Accepted.

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5-1986	5	49	34			"for a few years" may a bit long. "for one to two years" might be better as long as the reference is to cooling of 0.3K-0.4K at the Earth's surface. Impact declines in amplitude over time. This is discussed in Chapter 8, although there are apparently conflicting remarks there about large impacts for decades there that need clarification. [Adrian Simmons, United Kingdom]	Accepted.
5-1987	5	49	36	49	36	Delete "s" from "La Niñas". [Maria Fernanda Sanchez Goñi, France]	Noted. Text restructered.
5-1988	5	49	39	49	39	Replace "during the last 30 years" by "since 1950" (see comment on line 27 same page) [Urs Neu, Switzerland]	Taken into account. Text restructered.
5-1989	5	49	39	49	42	Circularity in FAQ 5.1 In the First Order Draft, Chapter 5, FAQ 5.2 had originally included the following (FOD page 5-44, lines 25-28): "[The sun can't be] a major driver of the climate changes over the past 40 years because instrumental TSI and SSI records contain no significant trend; whereas records of global mean temperature and GHG concentrations contain significant trends of increasing values. This lack of agreement in trends demonstrates that the Sun did not play a role during this period." This claim that temperature is driven by the trend in the forcing rather than the level of the forcing has been removed from the SOD. Excellent. But what replaces it is a model of circularity. Apparently the authors felt that SOME grounds for dismissing a solar explanation for late 20th century warming was required, and so the renumbered FAQ (now 5.1) contains the following monstrosity (SOD p. 5-49, lines 39-42): "During the last 30 years, anthropogenic factors, including rising concentrations of greenhouse gases, and other changes in atmospheric composition and land use, have produced warmed global surface temperatures by about 0.4°C. Hence, the Sun's role in climate change is modest: it is not a major driver of systematic climate warming over the past 30 years, especially in terms of global mean surface temperatures." The amount of warming that is here attributed to rising concentrations of greenhouse gases ("about 0.4°C"), is arrived at by ASSUMING that the change in solar forcing over the last 150 years was negligible. In the GCMs that AR5 employs, the change in solar forcing is ASSUMED to be limited to the very slight variation in TSI, which is fixed on the input side of the model at one 60th the forcing from anthropogenic effects (0.04 w/m^2 vs. 2.41 w/m^2, from SOD table 8.7 on page 8-39). This all happens on the INPUT side of the model. It is assumed from the outset that the ratio of greenhouse-warming to solar warming is 60 to 1. The output of this model (that 0.4°C of warming is due to human activity) cannot then be used to DEDUCE that this warming cannot be due to the sun ("HENCE the Sun's role in climate change is modest"). The 0.4°C figure is arrived at by ASSUMING that "the Sun's role in climate change is modest" (1/60th the role of GHGs). It cannot be used to deduce what was assumed. Beyond the matter of circularity, please note the SOD has rejected the idea that solar forcing is limited to variation in TSI (p. 7-43, lines 2-4): "The forcing from changes in total solar irradiance alone does not seem to account for these observations, implying the existence of an amplifying mechanism such as the hypothesized GCR-cloud link." Any analysis in which the only solar forcing is TSI is invalid and must be overhauled. [Alec Rawls, United States of America]	Some facts: since relatively reliable TSI data exist (since 1978, satellite based radiometers) TSI shows no significant trend, while greenhouse gas concentrtaions and temperatures show significant increasing trends. Cosmic rays do also not show a significant trend.
5-1990	5	49	40	49	40	Delete "produced" since this term is most probably misplaced and the sentence is meaningless with the term. [Government of Brazil]	Taken int account. Text changed.
5-1991	5	49	40	49	41	Again, cross check with Chapter 10 to be sure that the attribution of change to the response to anthropogenic forcing that is given here is consistent with current estimates from Chapter 10. [Francis Zwiers, Canada]	Noted.

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5-1992	5	49	41	49	41	Either replace "0.4°C" by "0.6°C" or give an explanation why this number is different of what is visible in Figure 1. Reason: the given amount of 0.4°C is confusing. Observed global temperature increase (linear trend) from 1975 to 2006 (as in Figure 1) as well as since 1950 is around 0.6°C (according to all availabel data sets, CRU, NASA/GISS, NOAA as well as Figure 1). Thus, if anthropogenic contribution is only 0.4°C, there must be a substantial contribution of 0.2°C by natural factors. This would be not 'modest'. However, most studies (e.g. Ammann et al. 2007) estimate the combined solar/volcanic influence since 1950 and especially since 1975 to be neutral or slightly negative. This is also visible in Figure 1 (anthropogenic forcing is about 0.55°C). [Urs Neu, Switzerland]	Taken int account. Text changed.
5-1993	5	49	41	49	42	Up to here in the FAQ, readers will get the impression that the solar forcing over the past 30 years has been positive because only the positive forcing from increases in activity during the solar cycle have been mentioned (with an impact on GW on the order of 0.1 degreesC) and here, on lines 41-42, all it says is that the sun's role in the observed GW of about 0.4degreesC has been modest. This is not consistent with conclusions about solar forcing as reported in the SPM and in particular in the RF chart that shows that solar forcing has been negative since 1978. [Government of Canada]	Taken into account. Text removed.
5-1994	5	49	41	49	42	"Hence, the Sun's role in climate change is modest: it is not a major driver of systematic climate warming over the past 30 years, especially in terms of global mean surface temperatures". What if the solar contribution and other effects cancel out each other? I am not sure if this conclusion "Hence, the Sun's role in climate change is modest" can be drawn from this 30 year period. [Raimund Muscheler, Sweden]	Taken into account. Text removed.
5-1995	5	49	42	49	42	Replace "during the last 30 years" by "since 1950" (see comment on line 27 same page) [Urs Neu, Switzerland]	Taken into account. Text removed.
5-1996	5	49	44	49	44	"TSI variations have nevertheless featured" --> "TSI variations nevertheless appear" ? [Masa KAGEYAMA, France]	Taken into account. Text removed.
5-1997	5	49	44	49	44	explain better what is displayed on this figure (the paragraph on page 50, lines 17ff could be moved here?) [Masa KAGEYAMA, France]	Taken into account. Text changed.
5-1998	5	49	44	49	45	Replace "TSI variations have nevertheless featured in the past 120 years of observed climate change" by "In the past 120 years of observed climate change TSI variations have nevertheless featured". Reason: In the original version, the next sentence is misleading (here would relate to TSI variations instead of 120 years, and anthropogenic factors are not involved in TSI variations). [Urs Neu, Switzerland]	Taken into account. Text changed.
5-1999	5	49	44	49	52	The first two sentences of this paragraph refer to climate change over the past 120 years but then the rest of the paragraph discusses solar forcing in the distant past. Suggest the information in lines 46-52 be brought up higher in the FAQ to be part of an introduction about solar forcing. Lines 44-46 could be merged with the paragraph beginning on line 54 which also addresses changes over the last century or so. [Government of Canada]	Taken into account. Text changed.
5-2000	5	49	44	49	52	This feels rushed to me and I think trys to say too much in a short space. The paragraph promises to discuss the most recent 120 years years (why that choice, and not the period since 1900, for example), but then almost immediately jumps onto long time scales. [Francis Zwiers, Canada]	Taken into account. Text and time interval changed.
5-2001	5	49	49	49	49	Replace "thought" by "estimated". Reason: It is more than 'thinking', there are at least different reconstructions showing this result. [Urs Neu, Switzerland]	Accepted.
5-2002	5	49	54	49	47	It would be good to have a quantification of this variability in the text (from this FAQ figure) [Masa KAGEYAMA, France]	taken into account. Range mentioned.
5-2003	5	49	54	49	54	I think readers will be confused by the implication that internal variability is a "signal". Detection and attribution studies consider internal variability as "noise" that masks the responses to external forcincs (that does not imply, however, that the "noise" does not have dynamical, well understood, origins). [Francis Zwiers, Canada]	Taken inot account. Text changed.
5-2004	5	49	55	49	55	Suggest inserting "episodic" before "cooling". [Francis Zwiers, Canada]	Accepted.
5-2005	5	49	56	49	57	Why is there only mention of the solar forcing trend over the first half of the 20th century and not the second	Taken into account. Text changed accordingly.

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						half? Please add this missing information to enable the reader to link the text to FAQ5.1 Figure 1. [Government of Canada]	
5-2006	5	49	57	49	57	Replace "underlying alternating warming and cooling with a strong 11-year cyclicity" by "underlying the warming and cooling of the 11-year cycle". Reason: Easier to understand. [Urs Neu, Switzerland]	Taken into account. Text changed.
5-2007	5	49		50		Section FAQ 5.1: This whole section refers only to global mean temperature. While I do not consider that the Sun is driving climate change I do feel you could put a brief nod in the direction of potential regional influences. [Joanna Haigh, United Kingdom]	Taken into account. Summary specified for global scale.
5-2008	5	49		50		We ran out of time coordinating the FAQ on solar forcing with chapter 10 - we need to crosscompare before finalizing, some statements seem a bit stronger than I'd recommend or not well enough connected to ch10 text. Overall a good FAQ though [Gabriele Hegerl, United Kingdom]	Noted. Some fine tuning to be done.
5-2009	5	49		50		The FAQ 5.1 is just great - very clear, interesting, directly related to the IPCC focus [Olga Solomina, Russia]	Noted.
5-2010	5	49				FAQ 5.1: This FAQ is well written but I wonder why it is in this chapter. It only covers the instrumental period, and is essentially a detection attribution argument that discusses solar vs. anthropogenic influence. I think this should go in the attribution chapter. [Reto Knutti, Switzerland]	Taken into account. Rejected by chapter 10.
5-2011	5	49				chapter "FAQ 5.1: Is the Sun a Major Driver of Climate Changes?" This section discusses only the mechanism connected to TSI changes. I think it would be very useful to also mention that solar UV changes might influence regional climate/circulation patterns (which might explain some of the paleoevidence for a sun-climate link) and that the cosmic ray cloud hypothesis stands on weak grounds (see chapter 7) [Raimund Muscheler, Sweden]	Noted. Correct, however FAQ's are supposed address specifically non-specialists.
5-2012	5	50	3	50	4	Replace "warming—by around 0.8°C—over the past 120 years, especially the prominent increase over the last 30 years" by "warming - by around 0.6°C - since 1950". See comment on page 49 line 27. [Urs Neu, Switzerland]	Accepted.
5-2013	5	50	4	50	5	This sentence is misleading and could be removed. [Masa KAGEYAMA, France]	Taken into account. Text changed.
5-2014	5	50	4	50	5	"However, solar forcing provides a diminishing contribution, relative to anthropogenic forcing, to the observed increase in global surface temperatures over the past 120 years.". This is a very strong conclusion. I am wondering if this is justified considering that there is still a low scientific understanding of the solar radiative forcing. In addition: What does "diminishing" mean? Less than 10%? Less than 1%? [Raimund Muscheler, Sweden]	Taken into account. Text changed.
5-2015	5	50	4	50	5	Replace "However, solar forcing provides a diminishing contribution, relative to anthropogenic forcing, to the observed increase in global surface temperatures over the past 120 years." by "Solar foring provides only a minor contribution, relative to anthropogenic forcing, to the observed increase in global surface temperatures since 1950". See comment on page 49 line 27. [Urs Neu, Switzerland]	Taken into account. Text changed.
5-2016	5	50	7	50	36	See my general comment on the way the response to the question has been scoped. I wonder whether this figure, and related discussion, are needed. This FAQ does not have to discuss in detail the full decomposition of historical, instrumental, temperature change. [Francis Zwiers, Canada]	Noted. Text restructered to make the main message more clear.
5-2017	5	50	7			FAQ 5.1, Figure 1: Top panel could be strengthened by adding additional observational datasets as assessed in Figure 2.15 of Chapter 2. Consider using consistent spacing for y-axes of all 5 panels to allow direct comparison of magnitudes for all factors. If such a change is not feasible, then the caption should prominently highlight the differences in the vertical axes to avoid misinterpretations. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. To keep figure simple, no more records added, spacing changed to steps of 0.2°C.
5-2018	5	50	8	50	9	The figure itself refers to CRU observations. Can you explicitly state what these CRU observations are? CRU has lots of observation series [European Union]	Taken into account. Reference added.
5-2019	5	50	8	50	9	The figure itself refers to CRU observations. Can you explicitly state what these CRU observations are? CRU has lots of observation series! [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Reference added.

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5-2020	5	50	8	50	15	caption of Figure 1 should be corrected. It shows temperature anomalies with respect to chosen reference level (how it is defined?) but not absolute temperatures. This has to be clarly stated. [Government of Poland]	Accepted.
5-2021	5	50	11	50	11	Replace "used as an indicator of" by "representing a major cause of". Reason: more adequate description [Urs Neu, Switzerland]	Accepted.
5-2022	5	50	17	50	28	This paragraph repeats some of the information from the previous paragraphs. Maybe they could be merged? [Masa KAGEYAMA, France]	Taken into account. Text changed.
5-2023	5	50	19	50	19	Note the different Y scales of the figure, or plot on the same scale for FAQ5.1 Figure 1. [Government of Australia]	Taken into account.
5-2024	5	50	19	50	19	should read "…the mean global monthly temperature anomalies…." [Government of Poland]	Accepted
5-2025	5	50	20	50	20	"with the four factors" --> "with the impact of the four factors" [Masa KAGEYAMA, France]	Taken into account. Wording changed.
5-2026	5	50	20	50	29	Clarify whether 1899 is a typo (for 1890) or is reference intended to be to 1899? The change made here should be reflected on FAQ 5.1, Figure 1. [Government of Canada]	Taken into account. New time window.
5-2027	5	50	21	50	21	I think that the reference should be "(Lean 2010)" rather than "(Lean et al., 2011)". [Georg Feulner, Germany]	Noted. The original reference is Lean & Rindt, 2008.
5-2028	5	50	21	50	22	How can we validate the estimation of anthropogenic forcing to temperature, in view of the uncertainties of estimation of the other three factors and the overlook of other potential factors? [Lei Huang, China]	Noted. This is not a validation, see reference Lean & Rindt, 2008.
5-2029	5	50	23	50	28	These lines repeat exactly the text in the FAQ5.1 Figure 1 caption and could be deleted. [Government of Canada]	Taken into account. Text changed.
5-2030	5	50	24	50	24	Replace "is used as an indicator of" by "represents a major cause of" [Urs Neu, Switzerland]	Accepted.
5-2031	5	50	26	50	26	Replace "on steadily" by "on the steadily" [Urs Neu, Switzerland]	Taken into account. Text changed.
5-2032	5	50	28	50	28	Replace "due to the greenhouse gases" by "due to greenhouse gases". Reason: equivalent to "due to aerosols". [Urs Neu, Switzerland]	Taken into account. Text changed.
5-2033	5	50	30	50	30	The text here is not consistent with FAQ5.1 Figure 1. The two time intervals are described here as being 1890-1965 and then 1965-2006, but in the Figure, the period 1975 - 2006 is marked as the second interval. Changes made here should be reflected on page 130, FAQ5.1, Figure 1. [Government of Canada]	Taken into account. Text changed.
5-2034	5	50	30	50	30	Replace "1965" by "1950". See comment on page 49 line 27. In FAQ5.1 Figure 1, the division is in the year 1975. This does not correspond to the existing text. I strongly recommend to use 1950. [Urs Neu, Switzerland]	Taken into account. Text changed.
5-2035	5	50	30	50	36	Why was the figure divided into two time intervals with 1965 CE being the boundary? Should we divide it based on changes in observed global surface temperature? [Lei Huang, China]	Taken into account. Text changed.
5-2036	5	50	30	50	36	FAQ 5.1: It would be interesting for the reader if some discussion of the contribution of different forcings to global temperatures over the past 10 years could be included here, when discussing Figure 1. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. The only clearly decreasing trend over the past 10 years in this simple approach is due to solar forcing.
5-2037	5	50	32	50	36	Replace "1965" by "1950". See comment above. [Urs Neu, Switzerland]	Taken into account. Text changed.
5-2038	5	50	34	50	34	Correct the spelling to "El Chichón" with an accent on the o. [Alan Robock, United States of America]	Accepted
5-2039	5	50	38	50	39	Please explain how scientists reconstruct solar activity for the past 10,000 yr. [Government of United States of America]	Noted. This is beyond the scope of a short simple answer to a FAQ. See e.g. Steinhilber et al., 2009.
5-2040	5	50	38	50	44	It is good to include a paragraph about future solar forcing and its strength relative to that of projected changes in GHG emissions, as this is a question often asked. However, as written, this paragraph is not very clear in answering this question. On line 41, it is not clear whether the projected decrease in solar activity in coming decades will be down to a grand minimum or something less extreme. Clarity is required here. It would also be helpful to add to lines 42-44 an estimate of the impact on global temperature of moving from a grand minimum to a grand maximum. On page 49 lines 51-52 it says this forcing would be up to twice that associated with	Noted. There is clear indication that the solar forcing will decrease in the next decades. However, it is very speculative by how much. There is large confidence that a new Maunder Minimum will occur sometimes in the future which will provide the missing information how much lower solar forcing really is during such a

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						min-max of the 11 yr cycle, so would a Grand minimum be up to about 0.2degreesC cooler then? Providing this information would be very useful to understand the relative contribution of solar forcing at its most extreme, compared to GHG forcing projections. [Government of Canada]	period.
5-2041	5	50	38	50	44	I think this discussion does add a useful additional facet to the response. Can you give a two word view on the consensus on the reliability of the solar forcing predictions for the next several decades. [Francis Zwiers, Canada]	Taken into account. New text added.
5-2042	5	50	43	50	43	Capital 'S' and 'M' required for solar maxima/minima [Peter Burt, United Kingdom]	Accepted.
5-2043	5	50	47	50	47	This is an incorrect statement. Greenhouse gas forcing had a similar magnitude of influence as solar and volcanic focings over the past 4000 years. See Kobashi et al. (2012).:Kobashi, T., Kawamura, K., Goto-Azuma, K., Box, J. E., Gao, C.-C., and Nakaegawa, T.: Causes of Greenland temperature variability over the past 4000 yr: implications for northern hemispheric temperature change, Clim. Past Discuss., 8, 4817-4883, doi:10.5194/cpd-8-4817-2012, 2012. [Takuro Kobashi, Japan]	Taken into account.
5-2044	5	50	47			"During recent decades" might be better than "During the past decades". [Adrian Simmons, United Kingdom]	Accepted.
5-2045	5	50	48	50	49	add ‘very likely’ or ‘high confidence’ or similar since we are not able to predict solar activity ahead of time, so it cannot be ruled that a prolonged Grand Minima might occur. [Gavin Schmidt, United States of America]	Accepted.
5-2046	5	50	49	50	49	One could cite Feulner & Rahmstorf, 2010, Geophys. Res. Lett., 37, L05707, doi:10.1029/2010GL042710 here. [Georg Feulner, Germany]	Accepted.
5-2047	5	50	55	51	3	Since there are no global SL curves for the last 2 millennia, certainly none at the tide gauge resolution, how can it be concluded that mean 20th century and post-1993 rates are unusual? Especially after devoting so much text to the patterns and causes of late Holocene climate variability? Also, why bring up rapid local SL rates due to tectonics, but not regional rates due to gravitation/rotational effects, LOCAL GIA, subsidence and other processes? The authors might consider cross-checking the relevant aspects of Chapter 13 (Sea Level Rise). [Government of United States of America]	In the FAQ we are only using century-scale estimates, which can be resolved, not tide guage resolution.
5-2048	5	50	55			FAQ 5.2: We would suggest a minor rewording of this title, which currently reads awkwardly to "Is the current rate of sea level rise unusual?" [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
5-2049	5	50	55			FAQ 5.2: A major problem with FAQ 5.2 is that it largely fails to provide any substantive basis supporting the chapeau text that the current rate of sea level change is unusual in the context of the past 2000 years. Most of the discussion is on longer time scales. The only evidence given is from one reconstruction from salt-marsh deposits on the Atlantic Coast of the US. Would expect some expansion upon that paragraph (lines 31 - 36) to provide some further evidence from other sites, which then gives you the basis for the "not unusual in the past 2000 years" statement, i.e., what reconstructions have gone in to producing the "last 2 millennia" bar in the FAQ 5.2 Figure 1? how well distributed are the reconstructions globally? In fact, FAQ Figure 1, is not currently even mentioned in the text, and requires much more discussion and embedding into the text. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. Text has been revised.
5-2050	5	50	57	50	58	These rates of sea level change reported here since 1993 are inconsistent with the rates given in Chapter 3 and Chapter 13 (and the SPM). Please check carefully with the latest drafts of these chapters and ensure consistency. This applies also to line 20. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. Text and rates have been revised to be consistent with chapter 3 and 13.
5-2051	5	50				FAQ 5.1 Figure 1: In the solar forcing panel (panel d), the gray line is at approximately y = -0.07. Was that intentional? If so, what sdoes it represent? [Matthew Konfirst, United States of America]	Taken into account. Figure replaced without this line.
5-2052	5	51	1	51	3	Include reference to other rapid periods resulting from special circumstances (as explained in lines 51-56) here. Otherwise could easily be taken out of context. [Government of United Kingdom of Great Britain & Northern Ireland]	Noted.
5-2053	5	51	2	51	2	Rates during previous interglacials may have been greater than the present rate of change (see Kopp et al and the sea level section on the LIG) [Mark Siddall, United Kingdom]	These are not included here because of the larger uncertainties see 5.6
5-2054	5	51	7	51	9	Should include salinity/density changes in this list of climatic factors affecting sea level. [Thomas Stocker/ WGI	Revise text to include changes in water density as a

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						TSU, Switzerland]	climate factor affecting sea level.
5-2055	5	51	7	51	9	Here is a suggestion to make this sentence a bit more lay reader friendly. Also note that I think the first "and" on line 8 should be replaced with "that cause". "Climate factors affecting sea level include changes in ocean temperatures that cause expansion or contraction of ocean water (thermal contraction/expansion), changes in the amout of ice that exists on land in glaciers and ice sheets, and changes in ocean currents." [Francis Zwiers, Canada]	Accepted.
5-2056	5	51	8	51	8	Replace "and thermal" by "and corresponding thermal". Reason: explain explicitly, because the connection might not by clear for everyone. [Urs Neu, Switzerland]	Revised to read "Climate factors affecting sea level include changes in ocean temperatures that cause expansion or contraction of ocean water (thermal contraction/expansion), changes in the amout of ice that exists on land in glaciers and ice sheets, and changes in ocean currents."
5-2057	5	51	8	51	9	Suggest revising "terrestrial glacier ice mass and current velocities" to "the contribution of water from terrestrial ice sources" for ease of understanding. [Government of Canada]	Revised to read "Climate factors affecting sea level include changes in ocean temperatures that cause expansion or contraction of ocean water (thermal contraction/expansion), changes in the amout of ice that exists on land in glaciers and ice sheets, and changes in ocean currents."
5-2058	5	51	9	51	9	Replace "and current velocities" by "and current velocities of ice flow in glaciers and ice sheets". Reason: is not clear for readers otherwise. [Urs Neu, Switzerland]	Revised to read "Climate factors affecting sea level include changes in ocean temperatures that cause expansion or contraction of ocean water (thermal contraction/expansion), changes in the amout of ice that exists on land in glaciers and ice sheets, and changes in ocean currents."
5-2059	5	51	10	51	10	explain "glacial isostatic adjustment", e.g. by adding "(land uplift after the melt of overlaying heavy ice masses that have pushed down the Earths' crust)" [Urs Neu, Switzerland]	Revise text to read "Geophysical factors affecting sea level include land subsidence or uplift and glacial isostatic adjustments.Where the latter refers to ….
5-2060	5	51	10	51	10	Remove "(GIA)" No need to define an acronym that is not used again. [Alan Robock, United States of America]	Accepted.
5-2061	5	51	10	51	12	Delete the sentence "Glacial isostatic adjustments (GIA) refer to the response of relative sea level to changes in the distribution of mass on the Earth, specifically water and ice." Reason: This sentence is only understood by experts and thus not suitable for FAQs. [Urs Neu, Switzerland]	Revise text to read "Geophysical factors affecting sea level include land subsidence or uplift and glacial isostatic adjustments.Where the latter refers to ….
5-2062	5	51	15	51	15	Replace "at rate" by "at a rate" [Urs Neu, Switzerland]	Accepted.
5-2063	5	51	15	51	15	Suggest replacing "since" with "that melted at the end of" [Francis Zwiers, Canada]	Revise text to read "continental ice that melted since the last glacial period".
5-2064	5	51	16	51	16	Ground subsidence caused by what? Ground water extraction? [Francis Zwiers, Canada]	Revise text to read "mainly in response to subsidence due to ground water extraction".
5-2065	5	51	18	51	18	Is this uncertainty or the spatial range of variations? [Government of France]	Spatial variation is causing the uncertainty.
5-2066	5	51	20	51	20	replace 'quite important' with 'significant' [Mark Siddall, United Kingdom]	Replace quite significant with important.
5-2067	5	51	21	51	21	Even integrated over 100 years, the rate of SLR seems small. Suggest further emphasizing that ~20 cm over a 100 years can still be significant by adding to the end of the last sentence the following: "where even small increases in sea level can inundate large land areas" (or something to that effect). [Government of Canada]	Will revise text to include "where even small increases in sea level can inundate large land areas" at end of sentence.
5-2068	5	51	23	51	24	These lines state that post-1700 is instrumental record but in same sentence says SL change is derived from proxy sediment/marsh records? Please clarify. [Government of United States of America]	Revise text will delete "CE (the instrumental period)".

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5-2069	5	51	23	51	24	Here a reference to Section 5.6 could be inserted [Masa KAGEYAMA, France]	Can not refer to sections in a FAQ.
5-2070	5	51	24			I suggest to replace the term "biological" by the term "paleontological", as the different archives mentioned to record indirect measures of sea level change are buried and represent records of the past (not the present). [Alejandro Cearreta, Spain]	Change to biological to fossil.
5-2071	5	51	29	51	30	Replace "to use proxy records to overlap with, but extend beyond the instrumental period" by "to produce records which overlap and even extend beyond the instrumental period" by "and even extend". Reason: Otherwise unclear or not logic (how could reconstructions extend beyond the instrumental period?). [Urs Neu, Switzerland]	Replace "but" with "and".
5-2072	5	51	29	51	34	This is a good point considering that the goct of NC has said that sea level rise can be ignored for design purposes. [European Union]	Noted.
5-2073	5	51	29	51	34	This is a good point considering that the goct of NC has said that sea level rise can be ignored for design purposes. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted
5-2074	5	51	36			This paragraph seems to revisit - and duplicate - the LIG & deglacial sea level section previously. Authors should consider parts to delete that are repetitive. [Government of United States of America]	FAQ text must stand alone, hence some repetition is unavoidable
5-2075	5	51	41	51	41	high latitude ice-sheets --> mid to high latitude ice-sheets (the North American ice-sheet was also in the mid-latitudes) [Masa KAGEYAMA, France]	Remove word high latitude in revised text
5-2076	5	51	46	51	47	Insert "the" before "formation". Also, it might help readers if you include a few additional words explain what a "basaltic plateau" is. A question that came to my mind is that evaculated lava would leave a void somewhere under the crust, which presumably would lead to crustal deformation that would offset at least some of the sea level rise caused by the evaculated lava. [Francis Zwiers, Canada]	Paragraph not expanded due to space limitations
5-2077	5	51	47	51	47	set a comma before "but" [Urs Neu, Switzerland]	Accepted
5-2078	5	51	47	51	48	Suggest putting a full stop at the end of "to raise sea level by 5-10 m." and then continuing with "However, it takes millions to tens-of-millions of years for a plateau to form and thus the maximum rate of …". [Francis Zwiers, Canada]	Accepted
5-2079	5	51	51	51	56	Missing from the FAQ is a reminder that while past high rates of SLR occurred during glacial-interglacial transitions, currently we are in an interglacial period. However, even now there is sufficient land ice left on Earth to raise sea level substantially (provide an estimate of the impact of GIS melt and WAIS melt at least). Suggest some text be added to make these two points, perhaps to the paragraph on lines 51-56. [Government of Canada]	This comment address potential future sea leve rise that is not the focus of this faq
5-2080	5	51	51	52	2	The term 'special circumstances' is unusual could be misleading. What is meant by the term, "tectonic upheavals"?, Why distinguish deltas, which were not really discussed, from any other type of coastal setting?, Why ignore tides, subsidence, karstification and a host of other processes? In the final 2 lines: as long as your baseline SL records are local [ie tidal marshes mainly], then you do not have a global SL curve for the last 2 millennia to compare to TGs and satellite records. Also, more discussion of acceleration is warranted. Disputes about acceleration during 20th century are critical because they expose complexities of all regional records, for ex. see Spada et al. [Government of United States of America]	We did not ignore subsidence, rather we chose to discuss deltas to highlight role of subsidence. See previous response about composite sl record
5-2081	5	51	55	51	56	Suggest adding something in parentheses to explain "ground-fluid" (e.g., add "(such as water, oil, gas, or brine)" [Francis Zwiers, Canada]	Noted.
5-2082	5	51	56	51	56	Add "occurs" at the end of the sentence. Reason: The sentence is incomplete. [Urs Neu, Switzerland]	Accepted. Text has been reformulated
5-2083	5	52	1	52	2	Why is there no discussion of Mediterranean archaeology records, which were so prominent in AR4? It's possible the reason is because they are so complex. Are the recent SL rise rates unusual, rapid or whatever? And if so, over what timescales? Can AR5 conclude that >3 mm/yr SL rise rates are fast, even compared to best estimates of last glacial and within the LIG? Does literature since AR4 on the rapid effect of melting ice [and we DO know glacial mass balance is decreasing] raise concern about global, but more importantly, regional rates of SL rise? [Government of United States of America]	This comment is beyond the scope of this faq. See discussion in 5.6

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-2084	5	52	1	52	2	As noted in our previous comment, the summary here is not sufficiently supported by the text of the FAQ, and more evidence/discussion is needed to support this conclusion. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account.
5-2085	5	52	2	52	2	Pre-industrial sea-level variations during the past two millennia were not discussed very extensively in the response - perhaps there should be a bit more on that topic to support what is said here in the summary sentence? [Francis Zwiers, Canada]	Taken into account. Text has been revised.
5-2086	5	52	4			FAQ 5.2, Figure 1: Please make figure and caption consistent; the caption currently is not very clear [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. Caption has been revised and expanded.
5-2087	5	52	5	52	5	Replace "select" by "selected" [Urs Neu, Switzerland]	Accepted, figure caption has been completely revised and updated
5-2088	5	52	5	52	6	The caption should call attention to the break in scale rate between 2.5 and 10 mm/yr. [Government of United States of America]	Accepted
5-2089	5	52				We suggest at least two additional FAQ: one concerns the climate surprises: is a climate surprise possible during the next century (with reference to Younger Dryas and 8.2kyr BP event). The second one concerns the comparison of the present and future warming to previous warm periods: what is different? rapidity of warming, and also spatial heterogeneity of the warming and not only the amplitude [Government of France]	Due to space limitations we cannot add FAQs.
5-2090	5	54	18	54	18	The Barker et al reference is missing but I don't think it should be there so may not need adding [Mark Siddall, United Kingdom]	Refs are in alphabetical order Barker et al appears in line 39 on page 54
5-2091	5	57	25	57	27	This Ref needs the book title and editors. Maybe other books will need this as well. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Added to EndNote Ref, matter of EndNote output style
5-2092	5	59	23	59	23	Citation "Dahl-Jensen, D., et al., submitted: Eemian interglacial reconstructed from Greenland folded NEEM ice core strata. Nature." should read "NEEM Community Members, accepted: Eemian interglacial reconstructed from a Greenland folded ice core. Nature" [Peter Köhler, Germany]	Accepted and changed accordingly
5-2093	5	60	57	60	58	The initial of this paper for the first two authors are wrong. Jan and Ulf are their first names! [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted and changed accordingly
5-2094	5	62	51			Reviews [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted and changed accordingly
5-2095	5	64	37	64	38	Updated reference (accepted 6 Sep 2012): Hind, A., and A. Moberg, 2012: Past millennial solar forcing magnitude: a statistical hemispheric-scale climate model vs. proxy data comparison. Climate Dynamics, doi:10.1007/s00382-012-1526-6, advance online publication [Anders Moberg, Sweden]	Accepted and changed accordingly
5-2096	5	64	51	64	52	Hollis, C.J., Taylor, K.W.T., Handley, L., Pancost, R.D., Huber, M., Creech, J., Hines, B., Crouch, E.M., Morgans, H.E.G., Crampton, J.S., Gibbs, S., Pearson, P., Zachos, J.C., 2012. Early Paleogene temperature history of the Southwest Pacific Ocean: reconciling proxies and models. Earth and Planetary Science Letters 349-350, 53-66. [Christopher Hollis, New Zealand]	Accepted and changed accordingly
5-2097	5	67	46	67	46	The publication “The role of mineral dust aerosols in polar amplification” has now been accepted for publication by Nature Climate Change [Fabrice Lambert, Republic of Korea]	Rejected. The paper has been published online by NatClimChange 06 January 2013 (http://www.nature.com/nclimate/journal/vaop/ncurrent/full/nclimate1785.html)
5-2098	5	70	3	70	4	This is the wrong reference. It should be "Lunt, D. J., Dunkley Jones, T., Heinemann, M., Huber, M., LeGrande, A., Winguth, A., Loptson, C., Marotzke, J., Roberts, C. D., Tindall, J., Valdes, P., and Winguth, C.: A model–data comparison for a multi-model ensemble of early Eocene atmosphere–ocean simulations: EoMIP, Clim. Past, 8, 1717-1736" [Daniel Lunt, United Kingdom]	Thank you! Corrected.
5-2099	5	72	29	72	30	References: Replace Mudelsee, Fohlmeister, Scholz Climate of the Past Discussions by: Mudelsee M, Fohlmeister J, Scholz D (2012) Effects of dating errors on nonparametric trend analyses of speleothem time series. Climate of the Past 8:1637–1648. [Manfred Mudelsee, Germany]	Accepted and changed accordingly

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-2100	5	72	31	72	31	Use the German umlaute and write the names correctly as: "Börngen" [Latex: B\"orngen] instead of "Borngen" and "Grünewald" [Latex: Gr\"unewald] instead of "Grunewald". [Manfred Mudelsee, Germany]	Accepted and changed accordingly
5-2101	5	74	10	74	10	Full citation available now: PALAEOSENS Project Members, 2012: Making sense of palaeoclimate sensitivity. Nature 491, 683-691. [Peter Köhler, Germany]	Accepted and changed accordingly
5-2102	5	75	5	75	28	The few experiments of iron ferilisation seem to show amuch much lower effect than the models. Need to discuss this. Assess what the best estimate could be. Why are only models estimates reported in the table? [Pierre Friedlingstein, United Kingdom]	This comment seams to be misplaced and referring to another chapter.
5-2103	5	75	10	75	10	Replace M'elia with Melia [DAVID SALAS Y MELIA, France]	Accepted and changed accordingly
5-2104	5	85	0	85	0	the third column of the table should be expanded a bit in order to avoid moving the units to a new line [Government of Poland]	Editorial
5-2105	5	85	0	85	0	in the last column, 5th line the sentence "Aqueous pCO2 is in equilibrium with atmospheric pCO2" is assigned medium confidence. However, in line 20 of the same column identical statement has "high" confidence. I would opt for "medium" in both cases [Government of Poland]	Accepted
5-2106	5	85	0	85	0	last column, line 3 from the bottom. Very questionable assumption. I would downgrade the confidence to "low". In fact, soil pCO2 can easily vary by more than order of magnitude, depending on the season, type of soil or amount of rainfall. [Government of Poland]	Accepted
5-2107	5	85	1	90	13	Within the chapter, the authors should consider elevating the stature and role of the wealth of material compiled as appendix material in tables 5.A.1 and 5.A.2 [Government of United States of America]	Accepted
5-2108	5	85	1	90	13	include the Red Sea sea level method in the table - I recommend Eelco Rohling write this [Mark Siddall, United Kingdom]	Rejected as it is not a CO2 proxy
5-2109	5	85	3			Under "Boron isotopes in Foraminifera": remove "The equilibrium constant for dissociation of boric acid...(high)." As mentioned above, planktic foraminifers follow a less sensitive d11B-vs pH relationship than expected from aqueous fractionation. This statement is therefore misleading. I would also recommend downgrading the confidence of estimating ocean alkalinity or DIC to "medium", since Cenozoic estimates from various proxies and models still range from 1000-2000 µmol/kg (e.g. Tyrrell & Zeebe, 2004) [Bärbel Hönisch, United States of America]	Accepted
5-2110	5	85		88		Section 5.3.5.2 Some mention of the need for reconstruction of ocean climatic cycles (ENSO, PDO, AMO, NAO) needs to be made There is a strong emphasis on the use of tree ring climate data and their limitations (lines 28-45), but very little is said about other proxies (in particular SSTs). Some of this is present in Appendix 5.A: Supplementary Material (pp. 5-85 to 5-88). At least there should be some discussion of the various proxies in Appendix 5.A: Supplementary Material (pp. 5-85 to 5-88) and the limitations of coverage in time and geography of most of them. This could lead to a statement of why tree rings have been so important for reconstructions of the past 2000 years. [Government of United States of America]	Table 5A1 and 5A2 only deal with "deep time" geological temperature and CO2 proxies
5-2111	5	85				tbl. 5.A.1 Under main assumptions for determining CO2 from alkenones: Is the confidence level really high for determining SST? Maybe that is appropriate for modern measurements but in a marine core, there is mixing and bioturbation and in any case 1 cm samples represent a certain number of years or decades depending on deposition rate. [Government of United States of America]	Noted and taken into account
5-2112	5	86	0	86	0	Line 10-11 from top. Questionable assumption. I would downgrade the confidence to "low". Atmospheric pCO2 close to the plant will be representative for the whole atmosphere only on rare occasions. [Government of Poland]	Noted and taken into account
5-2113	5	86	3	86	3	Table 5.A.2 - this table only covers a selection or proxy data sources that have been used to infer past SSTs - it either needs to be made more encompassing (e.g. What about modern and fossil corals?) or the reasons for its specificity made clear. [Janice Lough, Australia]	Table 5A1 and 5A2 only deal with "deep time" geological temperature and CO2 proxies

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-2114	5	86				In Table 5.A.2, Limitations column (second from the right), the term "forams" should be replaced by "foraminifera" as it is too informal. The same occurs twice in Table 5.A.2, Main Assumptions (relative confidence) column. [Alejandro Cearreta, Spain]	Accepted
5-2115	5	86				Under main assumptions for determing CO2 from paleosols: Is "high confidence" too strong a conclusion for the statements - "Burial (late) diagenetic effects are minimal or can be quantified". It is very difficult to determine the degree of replacement of soil carbonates by younger soil carbonates if they are in an environment where the depth of leaching changes from time to time. Perhaps it is possible, with qualifications, in a setting where the soils are rapidly buried, but not near the surface or where they have been slowly buried. [Government of United States of America]	Noted and taken into account
5-2116	5	86				tbl. 5.A.2 These confidence assessments for Archaea seem dubious, given that species are not identified. Has any work been done to determine whether different species fractionate, or form these rings in different proportions? [Government of United States of America]	Noted and taken into account
5-2117	5	88	0			"The ecology of modern assemblages is largely controlled by SST (high)" : The identity of the environmental variable that is most ecologically important is scale dependent. Variables that appear to be important at large spatial scales may not be important at small spatial scales or on temporal scales - that a 30°C equator-pole SST gradient is the dominant environmental control on species assemblages in the modern ocean, does not necessarily imply that SST will be important on the much shorter gradient found in time at any one core site. We should have no more than medium confidence in this assumption, especially in the tropics. [Richard Telford, Norway]	Noted and taken into account
5-2118	5	88	0			"That the extant species used to reconstruct SST mainly reside in the mixed layer (medium to high)." This is species group dependent. For groups that are photosynthetic, or have photosynthetic symbionts, we can have high confidence in this assumption, for other groups, there is evidence that many species live below the mixed layer. [Richard Telford, Norway]	Noted and taken into account
5-2119	5	88	0			"Depositional and post-depositional processes have not biased the assemblage (medium to high)" This is very dependent on the species group and the depositional setting. For light-weight organic microfossils in the open ocean, the confidence is rather low. [Richard Telford, Norway]	Noted and taken into account
5-2120	5	89		89		Table 5.A.3, page 89, upper entry (Mudelsee, 2001). (A) Investigated period is "0 to 420 ka"; write "ka" instead of "ky"; (B) source CO2 data is "Vostok* (GT4 gas age scale)"; (C) source temperature data is "Vostok dD (GT4 ice age scale)"; (D) lag quantification method is "Lagged Generalized Least Squares regression with parametric bootstrap resampling, entire record". [Manfred Mudelsee, Germany]	Noted and taken into account
5-2121	5	89		89		Table 5.A.3, page,89, lower entry (Shakun et al. 2012). (A) This study assumes that timescale errors (e.g., from reservoir ages) are independent from each other. This assumption is very likely violated, which could lead to higher-than-reported lag estimation uncertainties. [Manfred Mudelsee, Germany]	Taken into account. This point has been added.
5-2122	5	90	9	90	12	Appendix 5.A, Table 5.A.3: it should be added the Ahn et al. GRL 2012 paper on the time lag between Antarctic temperature and CO2 for D-O warming events 8 and 9. [Maria Fernanda Sanchez Goñi, France]	Taken into account. The reference has been added.
5-2123	5	90		90		Table 5.A.1: The study by Pedro et al. (2012) may not be reliable and should perhaps not be used for assessment. The reason is that Pedro et al. (2012, page 1216, left, lines 2-3 therein) interpolated the CO2 data to 20-yr resolution, although the original CO2 data have an average resolution (for the analysed time period) of 145 yr (Byrd record) and 266 yr (Siple record). That means, Pedro et al. (2012) may have underestimated the statistical error of their lag determination. [Manfred Mudelsee, Germany]	Noted. This aspect has been added.
5-2124	5	90		90		Table 5.A.2 - Pedro et al 2012 give a lag value that is not actually 200+/-200 years, but -56 to 381 years (likely range) (i.e. Does not exclude a lead) [Tasman van Ommen, Australia]	Taken into account albeit with rounded values (-60 to 380 years).
5-2125	5	91	1	92	37	Section 5.A.1 and 5.A.2: It seems that these two supplements are out of order. Since 5.A.2 refers to Section 5.3.5 including figures 5.7-5.9 and 5.A.1 describes figure 8 only, shouldn't the first paragraph of 5.A.2 come first, along with tables 5.A.4 and 5.A.5? The authors should then consider deleting all of lines 14-37 on p. 92 because they just repeat in less detail what is in 5.A.1. [Government of United States of America]	Taken into account. These appendix subsections reformulated to avoid repetition and to be a in a more logical order.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-2126	5	91	4	91	5	Tables 5.A1 and 5.A2 are not related to Figure 5.8 [Government of Poland]	Accepted. Cited table numbers were incorrect and are now fixed.
5-2127	5	91	12	91	12	The fact that this (i.e. 1500-1850) is slightly different to the period used in AR4 (1500 to 1899) should be stated. [Keith Briffa, United Kingdom]	Noted. Captions here and in AR4 state the periods, and the difference is not large. But we have included a brief statement about reference periods in the main text.
5-2128	5	91	15	91	16	Specify "1 standard deviation" of which time series (variable). [Hugues Goosse, Belgium]	Accepted. Changed to "1 standard error of the temperature reconstruction"
5-2129	5	91	30	91	30	State explicitly how many (high-frequency) reconstructions in (c-d) are used. [Keith Briffa, United Kingdom]	Accepted. Add this number to the text.
5-2130	5	92	14	92	37	the text largely repeats what was written on the previous page. [Government of Poland]	Taken into account. These appendix subsections reformulated to avoid repetition and to be a in a more logical order.
5-2131	5	92	17	92	18	Specify "1 standard deviation" of which time series (variable). [Hugues Goosse, Belgium]	Accepted. Will change to "1 standard error of the temperature reconstruction"
5-2132	5	92				In Table 5.A.4 - I fully realise why you might not wish to use Briffa et al. (2001) in Figure 5.7(a) and 5.8(a) why bother to include it in 5.8 (b) to (d)? Is this because it will leverage the high-frequency reconstruction response to volcanic events? [Keith Briffa, United Kingdom]	Noted. The Briffa et al. reconstruction, based on MXD data, provides some of the best evidence for the summer cooling response to volcanic eruptions, better than most of the other proxy data used in the other reconstructions and therefore valuable to include it.
5-2133	5	94	0	0	0	Table 5.A.5: For CSIRO-Mk3L-1-2, it should be “(3x) 1-2000 CE” and “(3x) 501-2000 CE” in the first and second lines, respectively. [Government of Australia]	Accepted.
5-2134	5	94	32			Section 5.A.3: This section header and the header for subsection 5.A.3.1 don't make sense hierarchically -- what is the LIG doing in a section on the Pliocene? [Robert Kopp, United States]	Noted. Taken into account during revision.
5-2135	5	94	37	94	39	Section 5.A.3:It would be helpful to add what proxies were used in the deep-water temperature anomaly reconstruction of Dowsett et al. since it must be quite different than d18O [Government of United States of America]	Noted. Taken into account during revision.
5-2136	5	95	10	95	10	The heading should have the same level as 5.A.3. Although related, this is a different issue. [Government of Poland]	Noted. Taken into account during revision.
5-2137	5	95	10			Section 5.A.3.1: Note that quoted uncertainties or coral U/Th ages may tend to underestimate true variability; see Scholz & Mangini (2007), Geochim. Cosmochim. Acta 71: 1935-1948. [Robert Kopp, United States]	Noted. Taken into account during revision.
5-2138	5	95	26	95	26	note importance of ongoing work on this subject [Mark Siddall, United Kingdom]	Noted. Taken into account during revision.
5-2139	5	96	1	96	33	Why is this page repeated here? [Giuseppe Cortese, New Zealand]	Noted. Taken into account during revision.
5-2140	5	97	7	97	9	Volcanic sulfate peaks: I cannot see them on the figure [Masa KAGEYAMA, France]	Taken into account. Text adjusted.
5-2141	5	97	9	97	9	brown squares missing in the figure. [Government of Poland]	Taken into account. Color changed.
5-2142	5	97	9	97	9	I could not find brown squares which indicate Antarctica. [Masakazu Yoshimori, Japan]	Taken into account. Color changed.
5-2143	5	97	9			Figure 5.1: couldn't find the brown squares denoting sulfate peaks in Greenland [Elie Verleyen, Belgium]	Taken into account. Color changed.
5-2144	5	97	13	97	13	Figure 5.1: Addition of 11-year cycle: no details on the amplitude (which is furthermore not time-constant) is given. [Manfred Mudelsee, Germany]	Taken into account: Explanation provided.
5-2145	5	97	13	97	13	prior to 1610. [Gavin Schmidt, United States of America]	Accepted.
5-2146	5	97	16	97	18	Figure 5.1, panel d. A colored scale (i.e., percent power) is missing but should accompany the wavelet. [Government of Canada]	Accepted. Colorbar added.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5-2147	5	97	17	97	17	meaning of colors in Fig. 5.1d ? [Government of Poland]	Accepted. Colorbar added.
5-2148	5	97				For part d, there is no colour scale [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted. Colorbar added.
5-2149	5	98	4	98	4	3.6 Ma to present --> from 3.6 Ma BP to present [Masa KAGEYAMA, France]	Accepted.
5-2150	5	98	4	98	4	Figure 5.2: The upper panel's time axis starts at 3.5 Ma and not at 3.6 Ma. [Manfred Mudelsee, Germany]	Accepted.
5-2151	5	98	6	98	6	should read "blue" [Government of Poland]	Accepted.
5-2152	5	98	6			caption Fig.5.2: purple should be blue? [Elie Verleyen, Belgium]	Accepted.
5-2153	5	98	9	98	9	The previous versions of some of these figures showed the Red Sea estimates of Rohling et al 2009 alongside Waelbroeck. This is the best approach. The problem is that the benthic isotopes are a signal which is mixed and include deep hydrographic changes (linked in turn to the carbon cycle) and temperature changes, which vary in phase. The Red Sea method is robust because the Red Sea strait cross section varies by three orders of magnitude over the glacial period. This is much larger than any other effect, whereas temperature and in some places hydrography play a large role in the benthic isotopes. This independence of the Red Sea method from other processes affecting GHGs and temperature is why it should be included here and in later figures. [Mark Siddall, United Kingdom]	Accepted (but hard to read when curves are overlain).
5-2154	5	98	11	98	11	no dashed line representing modern sea level [Government of Poland]	Accepted and corrected.
5-2155	5	98	13	98	13	should read "green" [Government of Poland]	Accepted and corrected.
5-2156	5	98	13	98	13	Is the line for the EPICA CO2 record green instead of blue? [Masa KAGEYAMA, France]	Accepted and corrected.
5-2157	5	98	13	98	13	EPICA Dome C ice core record looks green line (not blue line). [Masakazu Yoshimori, Japan]	Accepted and corrected.
5-2158	5	98	13			caption Fig.5.2: blue line should be green line? [Elie Verleyen, Belgium]	Accepted and corrected.
5-2159	5	98		98		Comment Fig 5.2. Noting the top part of Figure 2 compares Atmospheric CO2 over the last 3 million years with Tropical SST and Global Sea Level (+ dust accumulation) it would be instructive to show a similar comparison of the 65 million year trend in CO2 with Deep-sea temperature (proxy for high latitude winter sea surface temperature) and Global Sea Level (from Cramer et al., 2011). The Cramer paper also includes the first error analysis of such a compilation. [Peter Barrett, New Zealand]	Rejected due to space constraints.
5-2160	5	99	1	99	1	Figure 5.2: "Hönisch" [Manfred Mudelsee, Germany]	Accepted.
5-2161	5	99	4	99	4	remove "is" in front of "reconstructed" [Masa KAGEYAMA, France]	Accepted
5-2162	5	99	5	99	5	complied --> compiled [Masa KAGEYAMA, France]	Accepted
5-2163	5	99	9	99	10	Figure 5.2: Write: "The blue shading is a 1-standard deviation uncertainty band constructed using block bootstrap resampling (Mudelsee et al., 2012)." The reference is: Mudelsee M, Fohlmeister J, Scholz D (2012) Effects of dating errors on nonparametric trend analyses of speleothem time series. Climate of the Past 8:1637–1648. [Manfred Mudelsee, Germany]	Accepted as Mudlesee is a contributing author to this figure
5-2164	5	99	12	99	12	The sentence about the red box should be moved on line 4 before the information on the bottom panel [Masa KAGEYAMA, France]	Noted.
5-2165	5	100	0	100	0	inserted picture: I would use "water content" instead of "humidity" because the term "humidity" is intuitively linked to "relative humidity" [Government of Poland]	Noted, we use moisture
5-2166	5	100	5	100	6	cf. Comment No 5 [Government of Poland]	Unclear what comment No 5 is
5-2167	5	101	0	0	0	Box 5.2, Figure 1: Why is the model output for the land and ocean shown as uniform shading? This is extremely misleading. Either show the full spatial fields, or simply quote the figures for the mean model anomalies over land and the ocean. [Government of Australia]	Noted. Figure has been revised.
5-2168	5	101	1	102	1	Box 5.2 Figure 1: Need to put the actual dates in here so we know when the anomaly was calculated and what	Noted. Figure has been revised.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						the baseline is. These figures are a little hard to see. The last lines of the caption (page 5-102 lines 8-9) should be made more consistent, ie: for the Pliocene are from Haywood.... for the EECO are from Lunt. [Government of United States of America]	
5-2169	5	101	4	101	4	full name for SST required [Government of Poland]	Noted. Figure has been revised.
5-2170	5	101	5	101	5	remove "gradient" (twice) as the figures shows the SST anomalies, not the gradients stricto sensu [Masa KAGEYAMA, France]	Noted. Figure has been revised.
5-2171	5	101	8	101	8	Hollis et al., (2012) [Christopher Hollis, New Zealand]	Noted. Figure has been revised.
5-2172	5	101		101		It is unclear why the "EECO" panel (a, top) has an orange background, instead of white like the other panels. [Daniel Lunt, United Kingdom]	Noted. Figure has been revised.
5-2173	5	101		101		The EECO model data (panel b,c,d, top) needs to be updated with the final published version of the Lunt et al (2012) data. [Daniel Lunt, United Kingdom]	Noted. Figure has been revised.
5-2174	5	101				Box 5.2, Figure 1, the meaning of the shading (that is fairly homogenous in my pdf file) is not clear to me. [Hugues Goosse, Belgium]	Noted. Figure has been revised.
5-2175	5	101				I assume this plot is where the above comment on polar amplification comes from. I think what is shown in b and c are the observational data points with an error, and the latitude average mean and 2 std dev range of model ensembles. If so, then it is hard to conclude anything since the two properties are not comparable – you should do point-wise comparisons from the models. Secondly, the SAT polar amplification looks pretty good for all the 3 periods, and may even be slightly overestimated by the models at the LGM. The only dubious points seem to be a handful of points for SST in the EECO and MPWP. I really think this is insufficient evidence to make a general statement about models under predicting polar amplification. From the figure, I really wouldn't like to say whether in general the models over or under predict polar amplification and it seems likely that some models over-estimate polar amplification. Anyway – the statement in the executive summary needs to be weakened or removed unless some more rigorous analysis can be presented. [Julia Hargreaves, Japan]	Noted. Figure has been revised.
5-2176	5	101				Box 5.2, Figure: I don't find this figure very convincing for claiming that polar amplifcation is present during these three periods. For the EECO there are harly any data for temperate/tropical regions. For the MPWP, there is no polar amplification present in the data from the Southern ocean [Elie Verleyen, Belgium]	Noted. Figure has been revised.
5-2177	5	101				What are the dashed lines and numbers on each plot in b and c? I thought they might be the global average change, but at least for the LGM SAT anomaly, -8.5 degrees is way too high for any data or model I am aware of. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted. Figure has been revised.
5-2178	5	101				In a and d, the circles are invisible. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Noted. Figure has been revised.
5-2179	5	102	2	102	2	gradients --> temperatures [Masa KAGEYAMA, France]	Noted. Figure has been revised.
5-2180	5	102	3	102	3	standard deviation uncertainty --> what is it? Is it the spread of model results from the zonal mean? Or from different longitudes? [Masa KAGEYAMA, France]	Taken into account. Figure caption has been revised.
5-2181	5	102	9	102	9	LGM results: from PMIP2? PMIP3? Both? [Masa KAGEYAMA, France]	Noted. Figure has been revised.
5-2182	5	103	9	103	10	The previous versions of some of these figures showed the Red Sea estimates of Rohling et al 2009 alongside Waelbroeck. This is the best approach. The problem is that the benthic isotopes are a signal which is mixed and include deep hydrographic changes (linked in turn to the carbon cycle) and temperature changes, which vary in phase. The Red Sea method is robust because the Red Sea strait cross section varies by three orders of magnitude over the glacial period. This is much larger than any other effect, whereas temperature and in some places hydrography play a large role in the benthic isotopes. This independence of the Red Sea method from other processes affecting GHGs and temperature is why it should be included here and in earlier figures. [Mark Siddall, United Kingdom]	Accepted. Figure was modified and now includes Red Sea record
5-2183	5	103	12	103	14	state whether these models are climate-"only" or climate-ice sheet models? Some of the lines are difficult to	Figure was modified to improve readibility

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						distinguish. Are some model results given only for a given variable? For instance, the I can see the IcIES results only on the sea level plot, but I cannot see the Bern3D results on this plot... [Masa KAGEYAMA, France]	
5-2184	5	103				Fig 5.3. Not much is gained and much is lost by change of scale; for example frequency of large amplitude events in precession. Suggest maintain constant horizontal scale. If need be, widen the figure. [Stephen E Schwartz, United States of America]	Noted
5-2185	5	103				Fig 5.3 Show insolation at summer solstice 65 N commonly thought to be trigger of glaciation [Stephen E Schwartz, United States of America]	Rejected. The figure is already too busy
5-2186	5	103				Fig 5.3 Suggest different colors for models instead of line code. [Stephen E Schwartz, United States of America]	Accepted
5-2187	5	103				Fig 5.3. Show methane; show present CO2 and methane. [Stephen E Schwartz, United States of America]	Rejected. The figure is already too busy
5-2188	5	103				Part d is CO2 concentration, not pCO2 (a marine term!) [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Corrected
5-2189	5	103				The short dashed, long dashed and dotted lines cannot be seen on most panels. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Figure was modified to improve readibility
5-2190	5	104	0	104	0	top heading of Fig. 5.4a should be expanded to be consistent with the caption [Government of Poland]	Accepted - caption revised
5-2191	5	104	1	104	1	Ligh blue bars within panels b, e, f, h and I must be explained in the corresponding legend for Figure 5.4 [Eugenia M. Gayo, Chile]	Accepted - caption revised
5-2192	5	104	1	104	1	In fig. 5.4 f and i the ensemble of models is filtered and produces two nice curves whereas the measurements non filtered and higly dispersed are represented by big points. This give the wrong impression that data are dispersed while models are coherent and I suspect it is exactly the contrary. It would be better to represent by a cloud of points the different model results and by one or two curves the very coherent data from the records. The reconstructed precipitation change from the ensemble of models is clearly contrary to the stalagmite and lake 180 data: this should be written somewhere in the text (p.36) [Bruno TURCQ, France]	Noted, The reason why the simulation curve is coherent, is because it represents an ensemble mean that filters out automatically random internal variability and mostly shows the forced signal, whereas observations still contain the internal variability signal. Now the text mentions that for the SH the model signal does not match the proxy signal (referring to Figure 5.4 i)
5-2193	5	104	5	104	5	"since 850" does not seem to be correct at that place. [Hugues Goosse, Belgium]	Accepted. Caption revised
5-2194	5	104	5	104	5	since 850 CE? Really? [Masa KAGEYAMA, France]	Accepted. Caption revised
5-2195	5	104	5			delete 'since 850 CE'. Should be since 90 ka BP [Elie Verleyen, Belgium]	Accepted. Caption revised
5-2196	5	104	16	104	16	In fig. 5.4 i legend the color for the yellow curve (model) is wrong (black) [Bruno TURCQ, France]	Accepted. Caption revised
5-2197	5	104				Figure 5.4: In this figure, substitute “Hulu caved1, Sanbao caved2” by “Hulud1, Sanbaod2 Caves” for coherence with other panels. Also, substitute “Botuvera cave” by “Botuverá Cave” that is the appropriate way to write the cave´s name as published in Cruz et al. (2005. Nature, doi:10.1038/nature03365). [Government of Brazil]	Accepted. Caption revised
5-2198	5	104				Figure 5.4: no one panel shows climate changes since 850 CE. panels a, d and g cover from 90 to 10ka; panels b, e and h cover from 50 to 36ka; panels c, f and i cover from 900 to 2000 CE; panel a. austral summer insolation is a grey dashed line and not a blue line. [Maria Fernanda Sanchez Goñi, France]	Accepted. Caption revised
5-2199	5	104				Figure 5.4: labels c1 and c2 should be d1 and d2 in panel c 'proxy data locations'. Please check color codes: the caption for pannel a: blue should be grey for austral summer insolation. The caption for pannel d: purple should be red for D18O; same for caption for pannel f; the caption for panel g: black should be orange; the caption for pannel h: brown should be blue; caption for pannel i: black should be orange. please elaborate why some periods are shaded (e.g. in pannels b, e, h and i [Elie Verleyen, Belgium]	Accepted. Caption and figure revised accordingly.
5-2200	5	105	2	105	2	purple --> red? [Masa KAGEYAMA, France]	Accepted

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5-2201	5	105	8	105	8	purple --> red? [Masa KAGEYAMA, France]	Accepted
5-2202	5	105	10	105	11	Clarify exactly which version of the CSIRO model – there are different versions in the PMIP3 (CSIRO-Mk3L-1-2) and CMIP5 (CSIRO-Mk3-6-0) databases. [Government of Australia]	Accepted. Correct model version is provided
5-2203	5	105	10	105	17	Why are the annual means given for the model results and not the summer season averages? [Masa KAGEYAMA, France]	Noted. We use annual mean values, because the cave d18O reords are also likely an annually integrated signal rather than a seasonal signal. Water drips in caves not only during the summer monsoon season.
5-2204	5	105	12	5	12	Is the multi-model mean always in black? [Hugues Goosse, Belgium]	Accpted, correct color label is orange
5-2205	5	105				Figure 5.4: panel g. the simulated standardized multi-model average is in orange no in black; panel h. the standardized negative ð18O anomaly is in blue no in brown; panel i. the 30-year low-pass filtered rainfall anomalies is in orange no in black. [Maria Fernanda Sanchez Goñi, France]	Accepted. Caption revised
5-2206	5	106	4	106	4	are there any abrupt 4xCO2 results in panel a? [Masa KAGEYAMA, France]	Yes
5-2207	5	106	8	106	8	"it was taken" can be removed [Masa KAGEYAMA, France]	Accepted
5-2208	5	106	8			not where 'all' is plotted in pannel b [Elie Verleyen, Belgium]	cannot understand the comment but the plot and caption arerevised
5-2209	5	106				The values from Annan&Hargreaves (submitted – now in CPD) may be slightly different to what you used here. [Julia Hargreaves, Japan]	Accepted
5-2210	5	106				In part a, the bars don't work. I can understand why boxes were avoided, but the bars give no impression of the relationship between the two variables. [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted - caption revised
5-2211	5	107	4	107	4	Please say the reference period for the anomalies right up front in the caption. Are they with respect to 1961-1990? And is this a standard IPCC reference period for all figures in all chapters? If not, why did you choose this period? [Alan Robock, United States of America]	Accepted - caption revised
5-2212	5	107				figure 5.6: right bottom panel: are the data for the reconstructions missing near 60°S? [Elie Verleyen, Belgium]	Taken into account - Figure redrawn to align latitudes in Robinson plot with latitudes in zonal plots
5-2213	5	108	1	108	10	Please avoid dotted lines, - use thin/thick instead. [Aslak Grinsted, Denmark]	Noted. Tested but found not possible due to the number of lines that must be distinguished -- need to use both thickness and dotted variations
5-2214	5	108	5	108	5	Isn't it appendix 5.A.2 instead of appendix 5.A.1? [Hugues Goosse, Belgium]	Accepted.
5-2215	5	108	5	108	5	Figure 5.7: Appendix 5.A.1 gives Supplemental Information for Figure 5.8, not for Figure 5.7 [Manfred Mudelsee, Germany]	Accepted. Appendix order, structure and numbering improved.
5-2216	5	108	5	108	5	I can't find the references for the codes for each of the abbreviations used in this figure in the Appendix. Appendix 5.A.1 refers to two tables in the Appendix, but none of them have the codes or the references. Please provide an easily accessible list of the codes and what papers they refer to. It would also be nice to have a table that summarizes the differences in the analysis techniques and data sources. I finally found Table 5.A.4 which has this information, but nowhere does it say that. Add the reference to this table in the figure caption. [Alan Robock, United States of America]	Accepted. Link to appendix added, plus ensured consistency between figures, table, text and appendix.
5-2217	5	108		108		On Fig.5.7. The newest paper in the figure CL2012 shows that the temperature fluctuation is considerably larger than we thought before. This result will weaken the claim that the temperature increase during the latter half of the 20th century. Also I doubt if there is a suitable climate model which can explain this result. Of course, the newest paper does not necessarily mean the best. But, Cl2012 states ”The forward model is the physical sound choice, as we expect proxies to respond to local temperature and not the other way around.” This sounds very plausible, and thus, their method is clearly superior than those in older papers. There is also an important statement in CL2012; that is, ”Recently, there has been increasing evidence that	Noted. This is evolving science and not yet reached a point where the reviewer's assessment can be supported with confidence, including model-data comparisons that consider the localised calibration and/or forward modelling approaches.

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						many reconstructions are based on statistical methods that seriously underestimate low-frequency variability and trends." This definitely means that the older estimations for the temperature fluctuations are not suitable. Considering these new aspects of CL2012, I cannot help to conclude that the cause of the temperature fluctuation should not be explained on the basis of older papers. In fact, after looking Fig. 6 of CL2012, a friend of mine who is a very active economist of The University of Tokyo said that we also should study economical evaluations without an assumption of CO2-based climate changes [Kiminori Itoh, Japan]	
5-2218	5	108		108		Figure 5.7: Readers should like to know here, reading the caption, what sources/references the various shown curves have. Suggestion: refer to Table 5.1 notes and do also care that abbreviations used in Figure 5.7 and Table 5.1 agree with each other (e.g., "CL2012" should likely be the same as "13 = Christiansen and Ljungqvist (2012)"). [Manfred Mudelsee, Germany]	See 5-2216
5-2219	5	108				In Fig. 5-7, proxy data and measurement data are superposed. This way is understandable just because the same way was employed in AR4. However, the proxy data of recent papers are much better in quality than before; that is, they also cover the period from 1960 to 2000 unlike older papers. Thus, I think the proxy data should be plotted independently for the readers to see them clearly. [Kiminori Itoh, Japan]	Rejected. It is valuable to see the comparison of instrumental and reconstructed values - e.g. the mismatch for the CL2012 series -- and anyway some of the reconstructions contain instrumental data in the 20th century so for those it isn't possible to show only a proxy-based series anyway.
5-2220	5	109	4	110	12	The LIA definition here is different from that generally used. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	See 5-1133
5-2221	5	109		109		Figure 5.8, panels "e" and "f": One should perhaps show instead of the single feature "multimodel mean and range (brown)" instead the double feature, namely (1) "multimodal mean and range for solar-low (blue)" and (2) "multimodal mean and range for solar-high (red)". One should further draw inside these panels, below the horizontal dashed lines, an explaining legend with an arrow. [Manfred Mudelsee, Germany]	Accepted
5-2222	5	109				Figure 5.8 - This figure is a great concept and really interesting. Is the climate reconstruction envelope correct just after 1000CE? It just seems that when you renormalised all records to the 1500-1850 mean, the envelope "peak" should be more distinct at circa 1000. Is the basis for calculating this envelope directly equivalent to the method used in AR4 (Figure 6.10)? Also why is the (controversial) volcano at circa 1258 not included among the volcanic events used to produce Figures 5.8 (b) to (d)? [Keith Briffa, United Kingdom]	Response still needs to be decided, especially in relation to inclusion/exclusion of 1258 and need to check how far into the tails of the reconstruction spread is depicted by the grey shading.
5-2223	5	109				On Fig. 5-8. The reconstructed curves in Fig. 5-8 look out of date when compared with the curves in Fig. 5-7. Thus, the relevant description will not plausible. [Kiminori Itoh, Japan]	Noted. Same series are used for both figures, but a different reference period and need to check how far into the tails of the reconstruction spread is depicted by the grey shading.
5-2224	5	110	9			change the colour of the symbol denoting the multi-model mean and range - it is not not really brown as well [Elie Verleyen, Belgium]	Accepted.
5-2225	5	110	11	110	12	Give a direct reference to Table 5.A.4 for the codes for the curves. [Alan Robock, United States of America]	Accepted
5-2226	5	111	0			Fig 5.9 has some disconcerting horizontal lines on it (that probably come from the plotting routines solid fil rendering). The hatching is hard to make out, as is the Ljungqvist et al. data. [Christopher Brierley, United Kingdom of Great Britain & Northern Ireland]	Changes have been made to improve the visibility of this Figure
5-2227	5	111	1	111	17	Fig 5.9c: La Nina state in the MCA? I think this interpretation is limited for the reasons discussed under paleoproxy uncertainties (section 5.3.5.2): data going into the reconstruction are far too sparse and not well-positioned for reconstructing ENSO; pseudoproxy experiments have shown that the reconstructions have both mean and variance biases especially in regions far removed from proxy data locations (e.g. Smerdon et al 2011 but also uncertainty discussion in Mann et al 2008, 2009). This should be reflected in the uncertainties in the climate field reconstruction (which is consistent with the summary statement on pg 5-5, l 40-45) in the discussion and interpretation of the figure. [Michael Neil Evans, United States of America]	Taken into account. The new version notes the limitations in the reconstructed values in these regions.
5-2228	5	111	11	111	11	"scaled by a factor of 0.5 for display purposes" --> please explain. Couldn't those points be circled in black so that we can easily spot them without changing the scale??? [Masa KAGEYAMA, France]	Changes have been made to improve the visibility of this Figure. The 0.5 factor is arbitrary, but this value

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							happens to scale the uncalibrated Ljunqvist values so that they lie in the same colour scale range as the Mann et al dataset. The purpose is to show locations with relative warm and cold inferred anomalies, not a comparison of the magnitude of these values which are uncalibrated for Ljunqvist.
5-2229	5	111	12			shouldn't this be <80% of the simulations? [Elie Verleyen, Belgium]	Correct. This part has been revised to make it more clear
5-2230	5	111	15	111	16	What means exactly "weighted by the number of experiments"? Does it mean that each member of an ensemble has a weight equal to 1 divided by the total number of ensemble for this ensemble? [Hugues Goosse, Belgium]	Correct. This part has been revised to make it more clear
5-2231	5	111				Figure 5.9 and especially the right hand column is very difficult to see – should be larger. If these patterns are shown I believe you need equivalent maps of mean and standard deviations for MCA, LIA and “present”. [Keith Briffa, United Kingdom]	Accepted. The figure has been changed from portrait to landscape layout to increase the size and clarity.
5-2232	5	112	1	112	24	fig 5.10: the annual cycle across the NINO3 box (150-90W) changes dramatically in amplitude over this region, and somewhat in phasing; has this been considered in the statistical compilation? For both the annual cycle and the interannual variation, the simulations should be weighted by their skill in representing modern ENSO; e.g. see Collins et al (2005, 2010). There seems to be little agreement toward the central tendency shown in the MH results for ENSO variance change; maybe this situation would change were a skill weighting applied. [Michael Neil Evans, United States of America]	Noted. Nino3 is still an excellent region to characterize the annual cycle of SST. For ENSO we use the central Pacific Nino 3.4 region. We are refraining from applying ENSO model metrics as a weighting factor, because the choice of these metrics are also quite arbitrary and subjective. Furthermore, making choices on these metrics actually requires more research, which is not part of the LAs tasks.
5-2233	5	113	7	113	7	PMIP2 and PMIP3 model results: is it the average of model results which is shown? [Masa KAGEYAMA, France]	Taken into account - Figure caption clarified.
5-2234	5	113	10	113	10	Figure 5.11, caption: Not all boxplots display whiskers --- explain. [Manfred Mudelsee, Germany]	Accepted - Figure revised
5-2235	5	114	6	114	6	CSIRO-Mk3L-1-2 is referred to incorrectly as the “UNSW” model. [Government of Australia]	Accepted - Figure caption corrected
5-2236	5	115	0	115	0	See my earlier comment about Gergis (2012), (comment on Ch5, 33, 50-52); this reconstruction has been rejected by the Journal of Climate after basic problems had been detected at Climate Audit. So the Australasia reconstruction should be removed from figure 5.12 [Marcel Crok, The Netherlands]	Noted. While Gergis (2012) is still not accepted, an Australasian reconstruction has been described and published in PAGES2k (2013) and is used here.
5-2237	5	115	0	115	0	different shades of blue can be hardly resolved [Government of Poland]	Done.
5-2238	5	115	5	115	5	Why using a reference period of 1961-1990 in this figure, 1500-1850 on figure 5.8a, and 1881-1890 for figure 5.7 ? [Hugues Goosse, Belgium]	we have unified the reference periods now.
5-2239	5	115				Figure 5.12 is an excellent development over AR4 but some discussion of the basis for “season” definition would help (I am dubious about some) and certainly more discussion of the implication of these results (re specific models) would be very valuable. Briffa et al. (2008) results that show summer temperatures over N.W. Eurasian “Arctic” should be included among the records shown in the “Arctic” panel. [Keith Briffa, United Kingdom]	Noted. Will be taken into account during final revision.
5-2240	5	116	1	116	1	Figure 5.12, caption: It is unlucky (or a typing error?) that "DJF" and "SONDJF" overlap. [Manfred Mudelsee, Germany]	has been changed
5-2241	5	117		118		Figure 5.13: Figure was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected. [Dirk Thielen, Venezuela]	Noted
5-2242	5	118				Any chance you could aopt the same colour convention for figs 6.1, 6.2 and 6.4. Natural is blue in 6.1 and 6.4, but green in 6.2 [Pierre Friedlingstein, United Kingdom]	This comment is not addressed to chapter 5
5-2243	5	119		119		Figure 5.14, panel g (Oder River): write "Western Poland/Germany" instead of "Western Poland". [Manfred	Accepted. Figure revised

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						Mudelsee, Germany]	
5-2244	5	119		120		Figure 5.14: Figure was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected. [Dirk Thielen, Venezuela]	Noted
5-2245	5	120	4	120	4	Figure 5.14, caption: Alert readers as follows by writing at the end of the caption: "Note that flood frequencies obtained from historical sources may be downbiased in the early part of the reported periods owing to document loss." [Manfred Mudelsee, Germany]	Accepted. Figure caption changed
5-2246	5	121	4			Figure 5.15a: I suggest truncating the Kopp et al. (2009) curve before 130 ka, or otherwise adjusting the image so as to make clearer that the curve at this time is an upper confidence bound rather than a best estimate. It might also be helpful to make the y axis scale more consistent with the other subplots [Robert Kopp, United States]	Noted. Figure has been revised.
5-2247	5	121	4			Figure 5.15f,g: These figures are somewhat confusing. I suggest labeling f "Modeled GIS contribution", much as a-d have labels, and labeling "Modeled Greenland temperature." [Robert Kopp, United States]	Noted. Figure has been revised.
5-2248	5	121	4			Figure 5.15e: Suggested labeling "Western Australia - modeled GIA correction" and including a legend for the different curves. [Robert Kopp, United States]	Noted. Figure has been revised.
5-2249	5	121	11	121	11	remove comma after "both" [Masa KAGEYAMA, France]	Noted. Figure has been revised.
5-2250	5	121	20	121	21	I don't understand this "lower limit" "upper limit" estimates. Would it be possible to rephrase this sentence? [Masa KAGEYAMA, France]	Noted. Figure has been revised.
5-2251	5	121				Fig 5.15 pannel f: the label of the Y-axis should be ice sheet contribution to ... And not GIS because one of the estimates is from the Antarctic Ice Sheets [Elie Verleyen, Belgium]	Noted. Figure has been revised.
5-2252	5	123		123		The caption for this figure uses "best guess" which i don't think is a good phrase. "best estimate" might be a more appropriate phrase. [Daniel Lunt, United Kingdom]	Accepted
5-2253	5	125	1	125	5	Delete Bay of Biscay record from this diagram. It entirely overlaps with instrumental data (San Sebastian and Brest) and doesn't add anything extra. The instrumental data are much less noisy. This proxy record was published to demonstrate the method of reconstruction, rather than produce a new independent sea-level record. It is inappropriate to use it here outside that context. [Roland Gehrels, United Kingdom]	Noted. Figure has been revised.
5-2254	5	125	1	125	5	I don't understand why the Blekinge record is shown. It doesn't tell any story and is a very imprecise record (4 m error bars!). It doesn't even cover the past 3000 years. There are so many records to choose from - what's the point of this one? It would be good to start the figure caption with a sentence on the rationale for selecting the examples that are shown (e.g., geographical spread, variable trends, etc.). [Roland Gehrels, United Kingdom]	Noted. Figure has been revised.
5-2255	5	125	1	125	5	I would suggest to plot the Tasmania and New Zealand together in the same panel. Tasmania (Gehrels et al. 2012) is a very well-dated and high-quality record. [Roland Gehrels, United Kingdom]	Noted. Figure has been revised.
5-2256	5	125	1	125	5	Please add the high resolution record from Jens Morten Hansen et al., Boreas 2011, doi:10.1111/j.1502-3885.2011.00229.x . (It is pretty high resolution, uses a different technique, and comes from an author team which is orthogonal to those already in the figure, also validated with tide gauge records.) [Aslak Grinsted, Denmark]	Noted. Figure has been revised.
5-2257	5	125	1	125	5	Please add the Archeological records from the mediterranean. E.g. Toker et al. 2012, http://dx.doi.org/10.1016/j.epsl.2011.07.019 (It is pretty high resolution, uses a different technique, and comes from an author team which is orthogonal to those already in the figure) [Aslak Grinsted, Denmark]	Noted. Figure has been revised.
5-2258	5	125				Figure 5.17 shows observational evidence for sea level change in recent and late Holocene time. The left four panels include high resolution relative sea level results from saltmarsh data at representative sites for the last 2000 years. Information in the lowermost panel d) Bay of Biscay, Spain includes only data for the last 350 years whereas the other 3 panels include also late Holocene results. It could be easily completed with additional data for the last 2000 years recently published for the same geographical area by Leorri, E., A.	Noted. Figure has been revised.

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						Cearreta, and G. Milne, 2012: Field observations and modelling of Holocene sea-level changes in the southern Bay of Biscay: implication for understanding current rates of relative sea-level change and vertical land motion along the Atlantic coast of SW Europe. Quaternary Science Reviews, 42, 59-73. Sea-level curve is presented in Figure 2, and complete information of sea-level index points is available in Table 1 of that publication. [Alejandro Cearreta, Spain]	
5-2259	5	125				Figure 5.17. Showing the relative see level for panels a,b,c,d, while the global mean value is discussed in the text makes the section harder to understand to my point of view. The comparison of sea level rise between panels a) and i) can be misleading and conduct some readers to disagree with the explanation that sea level rises significantly accelerates after the mid 19th century (page 5-44 lines 43-56). [Hugues Goosse, Belgium]	Noted. Figure has been revised.
5-2260	5	125				panel f of Fig 5.17: The label "Orpheus island" should be displaced for clarity [Masa KAGEYAMA, France]	Noted. Figure has been revised.
5-2261	5	125				panel g of Fig 5.17: is it Port Gros or Port Cros (near Giens)? [Masa KAGEYAMA, France]	Noted. Figure has been revised.
5-2262	5	127	1	127	11	Figure 5.18: It could be nice to have an extra panel showing a close up for the last glacial period and duration for each D-O event. [Eugenia M. Gayo, Chile]	Noted, we have dropped Figure 5.18 from the Chapter
5-2263	5	127	1	127	11	see earlier comment. I do not think this has a place here [Mark Siddall, United Kingdom]	Noted, we have dropped Figure 5.18 from the Chapter
5-2264	5	127	5	127	9	The sequence of the descriptions in the caption of figure 5.18 (Benthic stack – terciles – Greenland d18O) does not match the sequence in the figure (terciles – Benthic stack – Greenland d18O). Please adjust the caption. [Fabrice Lambert, Republic of Korea]	Noted, we have dropped Figure 5.18 from the Chapter
5-2265	5	127	6	7		Unclear how the bipolar seesaw index was calculated; unclear what high and low frequency mean [Elie Verleyen, Belgium]	Noted, we have dropped Figure 5.18 from the Chapter
5-2266	5	127	6	127	7	The tercile plot in Figure 5.18 suggests a lower frequency of events in the older half of the record, which could be due to the low resolution of the dD data used by Barker et al., 2011. Lambert et al., 2012, Climate of the Past (doi:10.5194/cp-8-609-2012) have estimated the occurrence of D/O events using high resolution (1 cm, sub-annual at the top of the core to ~25 years at the bottom) dust data instead of the low resolution (55 cm, ~8 years at the top of the core to ~700 years at the bottom) dD data used by Barker et al., 2011, and found a constant rate of 2-3 events per 10 ka in all glacial periods. Although Lambert et al., 2012 and Barker et al., 2011 agree well enough on the occurrences of D/O events in the past 350 ka, the high-resolution data used by Lambert et al., 2012 allows the identification of many more D/O events in older glacials. I suggest to use the data from Lambert et al., 2012 for the tercile-plot. [Fabrice Lambert, Republic of Korea]	Noted, we have dropped Figure 5.18 from the Chapter
5-2267	5	127	8	127	9	Figure 5.18, caption, method by Barker et al. (2011): The rationale behind this method must be communicated. This caption is clearly less informative than all other captions in Chapter 5. [Manfred Mudelsee, Germany]	Noted, we have dropped Figure 5.18 from the Chapter
5-2268	5	127				Figure 5.18: Figure was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected. [Dirk Thielen, Venezuela]	Noted, we have dropped Figure 5.18 from the Chapter
5-2269	5	128	1			The data in the following article may reinforce the abrupt climate changes and its spatial extent: Kobashi, T., J. P. Severinghaus, E. Brook, J.-M. Barnola, and A. Grachev, Precise timing and characterization of abrupt climate change 8,200 years ago from air trapped in polar ice, Quaternary Science Reviews, 26, 1212-1222,2007. [Tosiyuki Nakaegawa, Japan]	Taken into account.
5-2270	5	128				Figure 5.19: Methane concentration data should be added in the figure, which shows significant reduction by cooling and drying in wide-spread areas (Kobashi et al., 2007). Data is available from NOAA Paleoclimate data center.: Kobashi, T., J. Severinghaus, E. J. Brook, J. M. Barnola, and A. Grachev (2007), Precise timing and characterization of abrupt climate change 8,200 years ago from air trapped in polar ice, Quarternary Science Reviews, doi:10.1016/j-quascirev.2007.1001.1009. [Takuro Kobashi, Japan]	Rejected, focus is on the AMOC, not the 8.2 event per se
5-2271	5	128				Figure 5.19, panel b: Replace "Gardner Drift" by "Gardar Drift". [Maria Fernanda Sanchez Goñi, France]	Accepted

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5-2272	5	128				In part b, Gardner should be Gardar I think [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-2273	5	128				Maps need latitude scale as well as longitude [Eric Wolff, United Kingdom of Great Britain & Northern Ireland]	Accepted
5-2274	5	129	2	129	2	Figure 5.19, caption: Write "NGRIP". [Manfred Mudelsee, Germany]	Accepted
5-2275	5	129	15	129	15	Legend of Figure 5.19: e) There are two "mean". Is that correct? [Maria Fernanda Sanchez Goñi, France]	Accepted - figure revised
5-2276	5	129	17	129	17	Figure 5.19, caption: Explain how the significance has been determined (i.e., which statistical test was employed). [Manfred Mudelsee, Germany]	Rejected, text will bee too long and technical
5-2277	5	129	18	129	18	Legend of Figure 5.19: f) There are two "mean". Is that correct? [Maria Fernanda Sanchez Goñi, France]	Accepted - figure revised
5-2278	5	130	0	130	0	cf. Comment No 25 [Government of Poland]	Noted. Comment not found.
5-2279	5	130	1	130	1	FAQ5.1 Figure 1: Extend dark shaded area back to 1950. See comments above. [Urs Neu, Switzerland]	Accepted.
5-2280	5	130	1	130	11	The figure and the legend mention and model, but do not specific which model. If this diagram is a redrawn version of a figure from Lean et al, perhaps it could be updated in collaboration with Chapter 10, using HadCRUT4, multiple models, etc. [Francis Zwiers, Canada]	Accepted. Figure is up-dated.
5-2281	5	130	2	130	2	The background colors should be more contrasted. [Government of France]	Accepted.
5-2282	5	130	4	130	5	Lean et al. (2010) is cited here for the Figure, but Lean et al (2011) is cited in the FAQ text (Page 50, line 21). [Government of Canada]	Taken into account. References fixed.
5-2283	5	130	6	130	6	the term "model" should be explained. In the text we read that this is just the sum of partial anomalies presented in figures 1b-3 [Government of Poland]	Accepted. Text modified.
5-2284	5	130	9	130	9	Reference should be made about the second half of the 20th century and to make sure the description of solar forcing for this time period matches what is reported in the SPM where a small negative forcing since 1978 is reported. [Government of Canada]	Taken into account. Text changed.
5-2285	5	130	10	130	11	Recommend adding to the end of the last sentence the following: "with a net warming effect that has become stronger since the middle of the 20th century." [Government of Canada]	Accepted.
5-2286	5	130		130		This figure is not labeled correctly and is confusing. You have panels c, d, and e labeled as forcing, but the units are in degrees. Are they response or forcing? Are these the actual equilibirum response to these forcings? Are they the transient response to the forcings? The solar response looks much too high in any case. Also, panel 2 only represents one source of internal variability. Shouldn't it be labeled "ENSO" or "El Niño contributions to internal variability?" [Alan Robock, United States of America]	Accepted.
5-2287	5	130				FAQ 5.1 Figure 1: Why does this end at 2006? I'm sure we have more data than that. [Government of United States of America]	Taken into account. Figure up-dated.
5-2288	5	130				FAQ-5.1 figure 1 caption Is there any justification for using ENSO index as a surrogate for internal variability [Government of United States of America]	Noted. The ENSO index seems not to be related to any external forcing.
5-2289	5	130				Fig 1, FAQ 5.1: explain method to split temperature change into the four factors [Masa KAGEYAMA, France]	Taken into account. Explanation added.
5-2290	5	130				Figure 1. comment: what are the faint horizontal lines representing in b,c,d and e. [Ting Wang, United States of America]	Noted. Lines have been removed.
5-2291	5	130				FAQ 5.1 Figure 1: The vertical scales of panes (a) and (e) of this figure are more compressed than the vertical scales of panes b. c and d. Since the point of this figure is to compare the contributions of the various factors in the individual panes it would be helpful if all had the same vertical axis scales. I suggest the LAs experiment with whether it is feasible to modify this figure so that all of the vertical scales are the same. [David Wratt, New Zealand]	Taken into account. This is a valid request. However, since greenhouse warming is so much larger equal scales will suppress any details of the other components. Compromise: all scales use the common step unit of 0.2°C.
5-2292	5	131	1	131	1	I suggest changing the label under the last bar to something like "Satellite altimetry record". "Latest" seems a bit grammatically challenged (I would say "late"), but "Late 20th century" would also not be correct, since the	Accepted - caption revised

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						period extends into the 21st century (and in fact, the 21st century component is now longer than the 20th century component). Another option would be to extend the end date to 2012, and change the label to "Last two decades". [Francis Zwiers, Canada]	
5-2293	5	131	4	131	5	Legend of FAQ 5.2 Figure 1: This legend is not clear. There are 6 selected intervals and not 4: last interglacial period, average of the last 5 glacial/interglacial transitions, meltwater pulse 1A, last 2 millennia, 20th century, satellite altimetry era (1993-2010). [Maria Fernanda Sanchez Goñi, France]	Accepted - caption revised
5-2294	5	131	4	131	5	Describe data sources in the caption, and give references if appropriate? [Francis Zwiers, Canada]	Taken into account - caption revised, but FAQ text may not include citations/references to the published literature.
5-2295	5	131	45	131	45	"high summer insolation" should be replaced by "increasing summer insolation" (according to the cited paper). [Kenji Kawamura, Japan]	incorrect page and line number reference
5-2296	5	131				A description of what the dark and light blue shading mean should be included. [Government of Canada]	Accepted - caption revised
5-2297	5	131				Fig 1, FAQ 5.2: precise that the dark blue boxes should be read on the upper scale and the light blue on the lower scale... [Masa KAGEYAMA, France]	Taken into account - Figure caption clarified.
5-2298	5	145				Fig6.25 caption says thick line is IAM, but thick line is multi model mean, while thich dashed line is IAM. On the bottom panel uu might want to show how much of these compatible emissions are in fact the prescribed dCA [Pierre Friedlingstein, United Kingdom]	Misplaced comment, probably refers to chapter 6.
5-2299	5					Fig. 5.1 Why omit carbon cycle here…vegetation [Government of United States of America]	Noted. The main focus of Fig. 5.1 is on solar forcing. Volcanic forcing is included because it often interfers with solar.
5-2300	5					Figure 5.1: These are anomalies, but it's unclear what the baseline is. [Government of United States of America]	Noted. For the model runs they were all used relative to the same mean value (1366.14 W/m2 for the period 1976-2006).
5-2301	5					Figure 5.6: Dates should be inserted here. From the last interglacial (date) to present (2011? 2012?)? [Government of United States of America]	Taken into account. Dates already clearly explained in Figure legend.
5-2302	5					Figure 5.13: Though it is not a long term record, it would be interesting to see the graph of the Drought Monitor record [Government of United States of America]	Rejected. The Drought Monitor is a spatial map of qualitative drought conditions for the United States, but it is instrumental only and to the best of our knowledge there are no historical time series data available.