IPCC Working Group I Fourth Assessment Report

Expert Review Comments on First-Order Draft

Chapter 6

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Please note that under IPCC procedures authors are required to take account of all substantive review comments in both review rounds. Thus responses to individual comments may be influenced by comments from other reviewers.

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6-1	A	0:0	0:0	E. Cook's paper, 'the segment length curse', is essential to the complete description of figure 6.8. The line should be inserted somewhere (unless I missed it), stating that tree ring time series are unable to resolve low frequency variance because of the way they are stadardized, with a link to this reference. [David M Anderson]	Noted, see comments to sect. 6.5
6-2	A	0:0	0:0	The FOD version of Chapter 6 is a major improvement with respect to the ZOD. The distribution of the sections is more appropriate, and the box topics usually well chosen. The full review has nevertheless allowed to spot some remaining inconsistencies or undesirable repetitions. Perhaps one of the arguable points of the present chapter is that it could have been more detailed with respect to climate mechanisms that the conjoint use of models and data have enabled to disantangle. For example, little is said about the details of the response of summer monsoon to insolation changes (the explanation is reduced to a response to land-sea contrast). Another example is the dependency of ocean carbon storage during the LGM on the paramaterisation of vertical diffusion in the ocean. A more in-depth discussion of these topics would add value to the chapter because they illustrate how, in practice, paleoclimate data may inform us on the physics of the climate system, and by consequent allow to identify crucial model development needs. Finally, it would be good to have a box dedicated to the astronomical forcing (see specific comments below). [Michel Crucifix]	Noted, will be considered
6-3	A	0:0	0:0	A table or something in the Glossary about the assumed dates of all the periods would bes useful.This would help focus the minds, of other chapters when they refer to the LIA and MWP as Ch 4 does. [Philip Jones]	Accepted Will be done, will decide at LA3 which terms to define
6-4	A	0:0	0:0	GENERAL COMMENTS: a) I find some significant mis-citation within the sections with which I am most familiar - particularly with respect to Heinrich events and Pa/Th isotopes. In this respect if Sidney Hemming has not already been asked to review chapter 6 I suggest the lead authors contact her directly for her input to the sections concerning H-events. b) As a general suggestion I would find a clearer distinction between model-based and paleo-proxy based interpretations very useful (see P14, line3 for an example where it is not clear whether a data or model value is cited), perhaps even a flag within citation boxes (D for data and M for model) would be useful, e.g. Blunier et al. 1998D; Stocker and Wright 1998M. c) The ordering of sections is sometimes hard to follow and I felt some sections arrived and departed very abruptly leaving the text without flow and hard to follow. The 'boxes' may help with this when they are introduced to the text. [Mark Siddall]	Noted, See comments to spcific section. Heming´s comments to this chapter are positive

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6-5	A	0:0	0:0	I hope the authors will consider making a table that defines the various paleoclimatic periods and events referred to in the chapter, such as last glacial maximum, Holocene, mid-Holocene, Medieval warm period, Heinrich events, altithermal, hypsithermal, etc. [Susan Solomon]	Accepted
6-6	A	0:0	0:0	I can imagine that you may be reluctant to call the glacial maxima "ice ages" but it would help the general reader if you could touch on this vernacular. Perhaps a sentence along the lines of "Popular literature sometimes refers to the glacial maxima as 'ice ages', and the last of these occurred about 20000 years ago" could go somewhere - maybe in association with the table of paleoclimatic periods? [Susan Solomon]	Accepted
6-7	A	0:0	0:0	I would like to compliment the authors on an excellent draft that will help to strengthen the AR4 as a whole. It answers many questions that the non-expert has in a very skillful way. The organization works extremely well by going from longer to more recent time scales. I hope that my comments may help the authors improve the readability of what is already a very fine chapter. [Susan Solomon]	Noted
6-8	A	0:0		The text uses the words "warmer" and "colder" often. These are relative terms, and most of the time it's not clear what it's relative to. Warmer than 2005, 1950, preindustrial Holocene? The chapter needs to be explicit on this point. [Becky Alexander]	Accepted, will define
6-9	A	0:0		I would like to commend the Chapter authors for a really interesting, thorough, well-written and logical Chapter. It certainly has drawn my attention to some highly relevant work that, up to now, has escaped my attention. So I guess on that basis alone, the chapter has therefore served its informative out-reach purpose well. Having said that and as a southern mid-latitude researcher - one can't escape noticing the huge (certainly disproportionate) body of data (particularly models) that exists for the NH realm compared to the SH. In the context of where a majority of paleoclimate research is based & funded - this is understandable. Hopefully some of my suggested amendments (below) relating to SH records (in particular - NZ) will be included. Certainly, New Zealand contains exceptional terrestrial and marine climate repositories that are strategically placed to critically evaluate differences in the detailed characteristics of climate events in the Southern Hemisphere as well as relate changes to variations in circum-Antarctic circulation, tropical influences, or perhaps climate events in the Northern Hemisphere. [Brent Alloway]	Accepted, See other SH comments
6-10	A	0:0		I am glad to see an entire chapter on paleoclimate. Understanding the past is key to predicting the future. Excellent chapter. [Richard Anthes]	Noted

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6-11	A	0:0		It has been suggested that the many paleogeological and -climatological terms, used in Ch 6, be added to the Glossary. However many, if not most, of them are used in Ch 6 only. I suggest to add a table to Ch 6 with definitions of these terms. If the CLAs prefer to have them in the Glossary, please provide me with a list of definitions. [Fons Baede]	Accepted
6-12	A	0:0		See comments on Chapter 1 above [Peter Barrett]	Noted
6-13	A	0:0		TSU NOTE: Please see supplementary review material [Hugo Beltrami]	Noted
6-14	A	0:0		While they are defined a various points in the text, a box or table at the beginning of the chapter defining the various geological ages would be very useful for readers who are not familar with these terms. [Lenny Bernstein]	Accepted, will appear in new glossary
6-15	A	0:0		The magnitude of climate sensitivity is one of the major unanwered questions in climate science. Past IPCC assessment have indicated that the paleoclimatic record is one source of information on this topic. However, this chapter does not address the question. If the issue is discussed elsewhere in this report, a cross-reference should be provided in the Introduction. If not, a section discussing the issue should be added. [Lenny Bernstein]	Accepted, detailed in Ch 9, but reference to this will be referred to in Ch6.
6-16	A	0:0		The overall tone of this chapter concerns me. The chapter reads as if it was specifically written to counter the claims of those who are skeptical of a human influence on climate. There is too much certitude and too few caveats. This tone is particularly inappropriate when the subject is paleoclimate, where uncertainties are rampant. [Anthony Broccoli]	Taken into account
6-17	A	0:0		Milankovich theory is important for the chapter understanding. It is referred in different parts of the chapter (e.g.question 6.1), and orbital forcing is mentioned several times in the executive summary. However, I have not found a clear description of the theory in the text or explanation how orbital forcing works. I understand that IPCC Report is not a textbook, but anyway a short description or illustration from Berger and Loutre's papers will help enormously to a reader not familar with paleoclimate theories. I suggest to include a box with a short explanation of orbital forcing concept and a color plot, e.g. of boreal summer insolation maximum (time, latitude), for the last 500,000 years. [Victor Brovkin]	Taken into account – new box
6-18	A	0:0		Our experience and therefore contribution relates mainly to the Venice situation and must be seen together with comments by Jane da Mosto (I). Several of the comments here could be equally relevant to sections of Chapter 12 (EUROPE) [Pierpaolo Campostrini]	Rejected – not relevant for Ch 6– for WG 2

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6-19	A	0:0		A paper was recently published (one day before this reviewer's comments were supposed to be sent to the TSU) in Nature (Church et al. 2005 Nature 438, 3 November 2005, doi:10.1038/nature04237). This paper shows a significant decadal-scale impact of volcanic eruptions (e.g., Pinatubo 1991) on sea level and ocean heat content. Thus, observed increases in sea level may be partly caused by recovering processes after cooling induced by eruptions. This new findings must be commented in this chapter (see also Robock 2005 GRL 32, L06702, doi:10.1029/2004GL022116,2005). [Paolo Cherubini]	Rejected, not relevant for Ch 6
6-20	A	0:0		I believe that the whole chapter is lacking a clear explanation of what is the value of tree-ring studies for the understanding of past climatic conditions, what are the limits, and why, although recently strongly criticized, they still are the best terrestrial proxies we have. Also, it should be made clearer that dendrochronological methods currently used are well established, having been used for several decades by hundrends of scientists and published in dozens of scholarly journals. This is not clear to every policy maker who will read the IPCC report. Sadly, often the study of tree rings is still believed to be an obscure discipline (I say that honestly although I am the Editor in Chief of the journal Dendrochronologia). [Paolo Cherubini]	Acccepted, will be dealt with in separate text at beginning
6-21	A	0:0		I like very much this chapter [Tiziano Colombo]	Accepted
6-22	A	0:0		the chapter presents an up-to-date version of the paleoclimate science. My only concerns are about the "non-linearity" of the text. Some parts are redundant, or not well structured. I imagine this is a very difficult task taking into account the big number of contributors, but I think this will symplify the reading if some attention can be given to this structuration question. [Elsa CORTIJO]	Noted, will be considered
6-23	A	0:0		why the figures do not present any marine records? [Elsa CORTIJO]	Accepted will add marine series to fig
6-24	A	0:0		Inconsistent capitalization of Northern/Southern Hemisphere throughout chapter. [James Crampton]	Accepted
6-25	A	0:0		Throughout, events in the past are referred to in the present tense (e.g., "…the climate of the LIG … is inferred to be warmer than today's (p. 15, line 27-28)) AND in the past tense (e.g., "the global annual radiation change for LIG from the present day insolation was small (p. 15, l. 26-27). The past tense should really be used consistently, although I realise that this is a big ask at this point! [James Crampton]	Accepted, see also comment to 6.8
6-26	A	0:0		Overall, I think this chapter is very well written and very informative - I enjoyed reading	Accepted

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				it! [James Crampton]	
6-27	A	0:0		Sections on recent glacier behaviour would be better placed in Chapter 4 [Rowan Fealy]	Noted, discus recent change only when there is a paleo perspeective
6-28	A	0:0		This chapter seems to have somewhat of a bias towards orbital mechanics as the only explanation of glacial cycles - it does not seem to present other points of view [Melanie Fitzpatrick]	Rejected, not relevant
6-29	A	0:0		This chapter does a good job of summarizing the evidence and presenting its relevance and application to improved understanding of climate change. In particular, the treatment of climate variability over the past millennium is measured and comprehensive. [Donald Forbes]	Accepted
6-30	A	0:0		A very impressive and, in general, successful attempt to present major progress in understanding of paleocliamates on 40 pages. My impression is, however, that partition of references between data- and modelling papers is biased towards data-papers. A number of important statements concerning the progress in paleoclimate modelling are not supported by appropriate references. [Andrey Ganopolski]	Taken into account, will be considered
6-31	A	0:0		Abbreviations should be used consistently. For example, on the page 6-15 the following abbreviations are used for ”kiloyear before present”: “ka”, “ky”, ”kyr”, “ka ago”! [Andrey Ganopolski]	Taken into account. Use kyr ago and ky Will discuss age scale in beginning
6-32	A	0:0		- [Savitri GARIVAIT]	
6-33	A	0:0		There is a lack of appreciation throughout this Chapter that paleoclimate data are usually only representative of limited regions, often only on land. Several sections are too eager to generalise properties of the entire earth from a few or even a single proxy observation. Models are no substitute for data. [Vincent Gray]	Noted
6-34	A	0:0		PLEASE standardise your references to past years. Replace “ka” with kyrBP and “ma” with” myrBP” and explain right at the beginning what you have done. I find “ka” and “ma” confusing, What do they stand for? What are “ma” and “ka”? Million and thousand what? [Vincent Gray]	Accepted, see 6-31
6-35	A	0:0		There is no need for paragraph headings in the form of questions They are particularly unnecessary in the Executive Summary. [Vincent Gray]	Rejected, we believe they are helpful
6-36	A	0:0		It is useful to have the longer perspective afforded by a palaeoclimate chapter and there is	Taken into account

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				a lot of interesting and relevant material here. I think that there should be more subdivisions, as some of the subsections are very long, and I had to search back a few pages to discover which one I was in. Also, please beware of coming too much up to date, since this is a palaeoclimate chapter. References to chapters 3, 4 and 5 can be made for modern variations. [Jonathan Gregory]	
6-37	A	0:0		Generally speaking, that chapter is more exhaustive in citations of modelling works than data results. It is also disequilibrated in favor to last 2000 years, for which the methodological considerations are more detailled than for the other parts. It spends too much energy in replying to papers tending to preclude a 20th century warming. [Joel GUIOT]	Noted, will be checked
6-38	A	0:0		In the field of paleoclimate the most fantastic data have been released since the TAR. The information from the North Grip on Greenland and the EPICA drillings on Antarctica are really important additions to our knowledge. But in this chapter there is almost nothing about these recent findings which is an unacceptable situation. At least some of the authors are part of the EPICA community and keep information recently published and publications to be published in the near future. Adding much more information from these drilling campaigns is a must. [Per Holmlund]	Accepted, have been waiting for the publication, will be portrayed in SOD
6-39	A	0:0		very interesting chapter, fully relevant to IPCC [Sylvie JOUSSAUME]	Accepted
6-40	A	0:0		Dear editors, [Dick Kroon]	
6-41	A	0:0		With pleasure I have read the Chapter 6 of the Fourth Assessment Report. The chapter concerns Paleoclimate. The chapter overall reads well and gives a good summary of what is for sale in Paleoclimate. I think the Table of Contents is well chosen with relevant subjects. However, I feel the document is missing punchy statements on relevant subjects for climate change today. I will give one example that I think should be worked out in much more depth. The subject concerns sea level change. Paleoclimate can be very strong with respect to this subject. [Dick Kroon]	Noted
6-42	A	0:0		I think that the current debate of future sea level change is the big issue that everybody is concerned about. The paleoclimatic record is actually full of hints about how fast sea level changes has changed in the past and thus indirectly tells us about dynamics of ice sheets. The modelers show potential sea level rise in the future mostly in the order of tens of centimeters up to one meter per century. Some modeling projects even show no sea level rise at all. This concerns me deeply because paleoclimate studies are very clear about this	Noted

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				and show in general a rate of sea level change in the order of one meter per century throughout the record, in particularly the last 150.000 years. Thus I had hoped a much more detailed and substantial discussion about this to tell the modelers that they have to rethink their models. I thought this is what IPCC reports are about. Thus the Paleoclimate record should become much more useful in this respect rather than just a good summary of what is being done in paleoclimate research. [Dick Kroon]	
6-43	A	0:0		I would like to suggest that the sea level discussion should be given more space than in the current document. It should be much more focused on rates of sea level change through time. This is exactly what geology can provide and these rates can be compared with modeling output. The authors could show how rates have changed through time both during glacial and interglacial periods. The authors could use excellent papers by several authors for instance Rohling et al, 2004, Nature, 430, 1016-1021. I am surprised this paper was not mentioned in the first place. The current figure on sea level change (Figure 6.4) could be much improved by using this paper. The Rohling et al 2004 record would be most useful. One of the findings by Rohling et al is that rates of change are in the order of one meter per century during the glacial period going from stadial to interstadials. This is important because it tells about the dynamics of ice sheets and melting. [Dick Kroon]	Taken into account, will be mentioned under glacial sea level
6-44	A	0:0		Of course the most interesting period of sea level change is during the Eemian because it could serve as an analogue for today although insolation patterns were different. The authors have only a very little space for this issue which is, however, highly relevant. The authors mention that sealevel was higher than today, but forget that sealevel wasn't stable during the Eemian. There is plenty of literature that shows this. There were at least two sea level highstands and potentially even three (Plaziat et al., 1998, Bull.Soc.Geol.Fr., 169, 115-125; Thompson and Goldstein, Science, 308, 401-404, 2005; Chen et al., 1991, Geol.Soc.Am. Bull, 103, 82-97; Bruggemann et al., 2004, Pal.Pal.Pal., 203, 179-2006). This is most important because it shows that the situation was much more dynamic than the current text suggest. This means that rates of sea level change were really fast during this warm period in the order of at least one meter per century and potentially even faster. Now this to my mind needs to be properly discussed with existing references and not some paper that appears to be in press. At least I would refer to the paper by Thompson and Goldstein in Science (308, 401-404, 2005) a highly relevant paper discussing this issue. Looking at their figures one can see how fast these sea level changes occurred (indeed in the order of one meter per century). [Dick Kroon]	Taken into account, will be considered in revision of sea level chapter Dick-Eystein
6-45	A	0:0		This issue of rates of sea level change becomes a major issue for future changes. The	Rejected, this is in Chapter 4

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				modelers refer to tens of centimeters in the future whilst the paleoclimatologists document at least one meter per century during the Eemian and then why not in the future. To my mind the Eemian situation shows that the modelers potentially could underestimate the present situation (e.g. Zwally et al., Science, 2002). This needs to be worked out in IPCC4. It is in this respect interesting that new observations are being made at the edge of ice sheets and that the models should potentially be adapted to these new observations (Alley et al., 2005,Science, 456-460). Thus melting rate could be higher than previously estimated. Thus the model results get closer to observations during the Eemian. [Dick Kroon]	
6-46	A	0:0		In summary I feel the document gives good descriptions of paleoclimatic work but it has missed the opportunity to be forceful about changes in rates of sea level. Particularly the discussion around the Eemian is weak and should be enforced because it is incredibly important to tell the modelers that they underestimate future rates of sea level change. I can't stress this enough that this gets properly discussed somewhere in the IPCC4 report. Thus the whole discussion needs to be expanded and explained. [Dick Kroon]	Taken into account, will be mentioned under glacial sea level
6-47	A	0:0		Regards, Dick Kroon [Dick Kroon]	
6-48	A	0:0		Past IPCC assessments have pointed to the paleoclimatic record as a source of information on climate sensitivity, but this issue is not discussed in this chapter. It should be, or if the author's assessment is that climate sensitivity cannot be determined from the paleoclimatic record, that conclusion should be stated. [Jeffrey Kueter]	Taken into account, will cumarine Ch9 findings at end of Ch6.
6-49	A	0:0		A statement is needed whether ages and durations stated in the chapter are in radiocarbon years or calendar years. [C.F. Michael Lewis]	Accepted, will use calendar ages, cite possible errors mentioned will be difined in introduction/glossaria
6-50	A	0:0		Overall, this is a really excellent chapter and a very important and useful addition to the set of chapters in the IPCC WG I reports. The authors should be highly commended for their initial effort--it provides almost a whole course in paleoclimate, and a wonderful update since I was really familiar with it all in the late 1960s. [Michael MacCracken]	Accepted
6-51	A	0:0		There seems to me to be some confusion in the chapter about what is forcing and what are feedbacks--and in the forcing area what might be considered internal and external. For example, I would think that the traditional IPCC definition of forcing applied to the LGM would treat the orbital effects on solar and the atmospheric composition changes as external forcing and changes in snow cover and glacial ice as a feedback, but the chapter	Taken into account. To be defined early in chapter, consistent with Ch9 and other chapters.

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				treats the change in ice sheet amount as an external forcing. In addition, the vegetation changes are treated as an external forcing. However, with carbon cycle and vegetation models, one would actually think that the change in composition will become a feedback (and so only human-injected carbon is a forcing) and the changes in vegetation should also be feedbacks (they are becoming so in GCM runs for the current climate). On the other hand, though I do not have a good idea how they might be included as an external forcing, one might want to include the isostatic changes as external, and maybe even the height of the ice sheet. In any case, I think there needs to be an effort to make things consistent across the IPCC WG I report--and to have some definitions and discussion of what is being done and why. In my view this is especially important because the way it is done here covers up a key possible paradox, namely that the IPCC traditional approach to forcing is that the spatial distribution of the forcing across the Earth does not matter (i.e., that the sensitivities to CO2 and sulfate aerosols are the same even though their geographic distributions are very different--okay, slightly different due to vertical distribution; but the glacial cycling tells us that spatial distribution is of critical importance as the orbital changes keep the annual total irradiance the same, just redistributing it by latitude and season. Making the glacial effects external--and their effects, like aerosols, have a strong latitudinal and seasonal variation--rather confuses things with respect to albedo feedback and what should be internal or external, forcing or feedback. I don't have a solution to all of this, but do believe it deserves attention--also see next comment relating to this. [Michael MacCracken]	
6-52	A	0:0		The one area where some additional discussion is needed concerns an evaluation of the assumption made in the IPCC treatment of radiative forcing that latitudinal and seasonal distribution do not significantly affect the response of the climate--so that there is a global summing that can be done. If indeed orbital element changes drive the glacial/interglacial cycling, as we interpret the evidence to indicate, then there would seem to be an inconsistency with the IPCC formulation of the forcing-response argument, as there is essentially no significant net annual, global change in radiative forcing but instead of no response, we get glacial maxima and interglacials--so simply a redistribution of the forcing is causing a huge change in climate. For the present situation, why then should not the spatially and seasonally varying sulfate forcing be causing a large climatic response? This chapter gets around this by, as noted in another comment, calling processes like changes in albedo due to snow, ice and vegetation forcings rather than feedbacks (presumably because they are so persistent), but this is really strange, for it would seem to imply that it is solely what might be called feedbacks that are driving the climate as given the IPCC forcing presumption, the orbital element variations should be having no effect. It really seems to me as if this question/paradox needs to be addressed more fully.	See comment 6-51 Regional character to be discussed in new orbital forcing box

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				[Michael MacCracken]	
6-53	A	0:0		There seems to me to be an inconsistency in the chapter, and it even carries over to other chapters, with regard to the references to the Little Ice Age, Medieval Optimum, etc. On the one hand, the chapter appears to indicate that such features of the climate are generally regional rather than global so that since 8200 BP no significant global variation is found and so the terms really should not be used--yet this chapter then has boxes describing these periods using the terms (and other chapters also seem to use these terms). At a minimum, I would suggest putting these terms in quote in at least the titles of boxes, etc., to indicate that this is a name that has been given but which should be interpreted carefully; better yet might be to not even use these terms if they are not valid, and simply, as is done in some cases, refer to the time intervals being discussed. It just is not clear from the text here whether these terms should or should not be used--the chapter seems to say in some places that these periods are not real (in the sense they are traditionally thought), yet it then goes on to use the terms as if these periods were as they are popularly thought. [Michael MacCracken]	Taken into account, more emphasis on terminology in introduction, need to be consistent with Ch 4 and 9, include other terms such as ice age, LGM deglaciations.
6-54	A	0:0		While I gather such problems will be caught by editors at some point, the chapter text is very inconsistent in its capitalization of terms like Northern Hemisphere (and should it generally be abbreviated NH or not), North Atlantic, western Europe, earth/Earth, etc. The inconsistencies are so prevalent it gives the appearance of the chapter having been pasted together with no author having read through the chapter to ensure overall consistency of the text--and so distracts from what is, really terrific content. [Michael MacCracken]	Taken into account, will be fixed
6-55	A	0:0		Opening Comment: In the Chapters that I am reviewing, I choose to not provide an anonomous review. This choice allows the various Chapter authors to contact me directly on matters of errors, concepts, or questions of disagreement. I have already performed thorough reviews of chapters 1-5. Due to the looming November 4th deadline for reviews, I am choosing to review Chapters 6-11 in a drastically shortened way . Rather than going through all of them as I did before, I am choosing to review only the Executive Summaries of chapters 6-11. There are some clear advantages for this strategy, independent of the obvious one of speeding up the very tedious reading and reviewing process. In the previous chapters I have reviewed, I have seen some significant disconnects between two obviously differering reporting strategies. First, it seems obvious to me that the fundamental purpose of these IPCC FAR reviews is to establish the case, or lack therof, for many of the diverse aspects of the human-caused global warming problem. Second, it is noteworthy that this draft WG1 report is roughly twice as long as the WG1 IPCC TAR report. Third, it seems very obvious that the key IPCC assessment-	Noted, will try and keep it short

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				relevant punchlines are hardly double those of IPCC TAR. It seems clear to me that the global-warming research-advancement doubling time scale is a lot closer to twenty years than it is to five years. The obvious conclusion for me is that we don't really need or desire to double the length of the WG1 chapter assessment every five years! For these nearly obvious reasons, and to help me and the other reviewers refocus on the fundamentally important conclusions that are centrally relevant to the IPCC's human-caused climate assessment's goals, I am thus choosing to reduce drastically my own submitted WG1 reviews. And, most importantly, this gives me a good shot at reviewing meaningfully all of remaining chapters 6-11 by the daunting November 4th reviewers' deadline. [Jerry Mahlman]	
6-56	A	0:0		GENERAL COMMENTS FOR CHAPTER 6: PALEOCLIMATE I was quite pleased to see that this paleoclimate chapter's main conclusions in its Executive Summary were very appropriately focussed on the important and relevant "paleo-punchlines" that provide valuable perspective and grounding for the overall IPCC AR4 Report. For this reason, I choose to focus on the Executive Summaries only for my reviews/overviews of chapters 6-11. Overall, I found the Executive Summaries for Chapters 2-5 to be quite clear and digestible (Chapter 1 is in a different category that lies properly outside the responsibilities of this AR4 endeavor.) [Jerry Mahlman]	Noted
6-57	A	0:0		The authors of the chapter, and in particular those of section 6.5, are to be commended for their efforts in providing a generally thorough, balanced, accurate and up-to-date assessment of where the science stands, particularly in the way that both real and specious controversies in the field are dealt with. There are a few very important recent updates that should be taken into account, as outlined in specific comments. In general, however, the conclusions drawn appear robust and rigorously defensible, and I hope that the authors of the chapter will resist any dubious outside efforts to force them to dilute these conclusions. [Michael Mann]	Taken into account, most recent papers will be taken into account
6-58	A	0:0		Throughout the chapter and particularly in Question 6.1, it is stated that the Milankovitch cycle is now well established. Should there be some discussion on this, as there have been studies like the Devil's Hole oxygen isotope record that suggested that timing is not right? [Katsumi Matsumoto]	Taken into account, will slightly reformulate Q6.1, and in new box on orbital forcing.
6-59	A	0:0		the explanation of glacial-interglacial CO2 variations remains a difficult attribution problem.- carry forward to Summary	Accepted, will put in summary

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				[Stephen McIntyre]	
6-60	A	0:0		the climate of the LIG in both the SH and NH is inferred to be warmer than today's (Kukla and al., 2002), - carry forward to Summary [Stephen McIntyre]	Rejected, global evidence not strong enough for summary
6-61	A	0:0		Current coupled models may underestimate the rate of melting and warming because of missing feedbacks and incomplete ice sheet physics. - carry forward to summary [Stephen McIntyre]	Accepted,
6-62	A	0:0		List uncertainties of paleoclimate clearly: alternative explanations of tree ring widths and density (precipitation), dO18 (precipitation) [Stephen McIntyre]	Taken into account, expanded explanation of uncertainties.
6-63	A	0:0		The exact cause and nature of these ocean circulation changes, however, is not universally agreed. -carry forward from 18 line 11,12 [Stephen McIntyre]	Rejected, not appropriate for summary
6-64	A	0:0		Modeling the ice sheet instabilities that are the likely cause of Heinrich events is a difficult problem where the physics is not sufficiently understood, - carry to summary from 6-18 [Stephen McIntyre]	Rejected, not appropriate for summary
6-65	A	0:0		carry forward summary of glacier changes from page 22, line 43ff [Stephen McIntyre]	Noted existing bullet made more specific.
6-66	A	0:0		The processes behind these observed abrupt shifts are not well understood, - carry forward from 25 line 48 [Stephen McIntyre]	Taken into acccount in existing bullet on abrupt change
6-67	A	0:0		Proxies have not been calibrated for post-1985 warm period. Proxies may have non-linear (upside-down U) response to increased temperature and may not record early warm periods. [Stephen McIntyre]	Taken into account, in strengthened section on uncertainties.
6-68	A	0:0		Proxy limitations from page 30 line 25 should be carried forward. [Stephen McIntyre]	Rejected, uncertainties discussed in text alreedy referred to in summary
6-69	A	0:0		Section on proxy uncertainties - problems distinguishing between salinity and temperature, precipitation and temperature, CO2 fertilization and temperature. [Stephen McIntyre]	Rejected, uncertainties discussed in text alreedy referred to in summary
6-70	A	0:0		Solanki et al claim of unprecednted solar activity from page 33 line 48 should be carreid forward. [Stephen McIntyre]	Rejected, subsequent discussion in text shows claim to be less certain. Main text revised.
6-71	A	0:0		failure to simualte hydorology should be carried forward to uncertainty claim - page 37 line 38	Taken into account to be discussed with Ch 10

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				[Stephen McIntyre]	
6-72	A	0:0		Bear in mind that a great many observers, especially the most motivated critics of the AR4, will start their reading by turning to the paleoclimate chapter and seeing how the IPCC deals with the hockey stick. I will present my comments on this chapter as helpfully and objectively as I can. But I begin with some exasperation at this first draft. You may not want any advice from me, but for what it's worth, do consider. Chapter 6 is obstinate in its rejection of criticisms of the hockey stick, yet is surprisingly weak on the technical issues at stake. If you truly want to proceed with the chapter in its current form then you will not only be handing the IPCC's traditional critics a large club to beat the AR4 with, but you will alienate those many scientists who have hitherto given the IPCC the benefit of the doubt, but who have followed these issues and are looking for a serious treatment of them, not a brittle, dogmatic dismissal. [Ross McKitrick]	Taken into account, section rewritten
6-73	A	0:0		In light of the above, the material in the chapter introduction needs to be revised, which I leave to you. [Ross McKitrick]	Taken into account, see above
6-74	A	0:0		references Bard, E., Raisbeck, G.M., Yiou, F., Jouzel, J., 2000. Solar irradiance during the last 1200 years based on cosmogenic nuclides. Tellus 52B, 985–992. Muscheler, R., Beer, J., Kubik, P.W., Synal, H.-A., 2005. Geomagnetic field intensity during the last 60,000 years based on 10Be & 36Cl from the Summit ice cores and 14C. Quat. Sci. Rev., 10.1016/j.quascirev.2005.01.012. Muscheler, R., Joos, F., Beer, J., Mueller, S.A., Vonmoos, M., Snowball, I., submitted. Changes in solar activity during the last 1000 years based on radionuclide records. Earth Science Reviews. Snowball, I., Muscheler, R., submitted. How high temporal resolution paleomagnetic field records can improve the reconstruction of solar activity. Geology. Usoskin, I.G., Solanki, S.K., Schüssler, M., Mursula, K., Alanko, K., 2003. A Millennium Scale Sunspot Number Reconstruction: Evidence For an Unusually Active Sun Since the 1940's. Physical Review Letters 91, 211101-1-4. Vonmoos, M., Beer, J., Muscheler, R., submitted. Large variations in Holocene solar activity - constraints from 10Be in the GRIP ice core. Solar Physics. [Raimund Muscheler]	Noted, total number of references limited, will be considered
6-75	A	0:0		If the report is in Enlish, write palaeoclimate (with a) and avoid abreviations like TAR, LIG etc. [Atle Nesje]	Rejected, scoping said Paleoclimate
6-76	A	0:0		Write "Little Ice Age" throughout the chapter	Accepted

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				[Atle Nesje]	
6-77	A	0:0		I miss a specific chapter on the 'Little Ice Age' (LIA) that discusses the concept and the data. There must at least be a cross-reference to Chapter 4 and the two chapters must be consistent regarding the LIA (~last 500 years. [Atle Nesje]	Taken into account
6-78	A	0:0		It would be helpful to have a simple table early in the chapter listing the terminology for the various time-periods used in the chapter. I find it a bit difficult keeping all of these in order, so I suspect some other readers will also. Terms like LIG, Stage 7, etc can be confusing and a simple table listing the acronym, spelling it out, and saying when (and what) it was, would be helpful. [Neville Nicholls]	Accepted
6-79	A	0:0		This chapter discusses the LIG sea level stand (versus today) many times. Unfortunately, many inconsistent sets of numbers are used. The same is true for LIG temperature versus today. Please comb through the chapter carefully to assure that all such values and discussions are consistent. [Michael Oppenheimer]	Accepted
6-80	A	0:0		This chapter includes new information compared to the TAR and is highly welcome. [Klaus Radunsky]	Noted
6-81	A	0:0		The work done by the authors could be appreciated. It is not easy to initiate such a work. More space, however, has been left for revision as compared to the other chapters of the group one report draft. A more balanced assessment could be made, and many publications, especially those by independent paleo-scientists and those in languages other than English, should be cited. It is also good to invite more paleo-scientists to review the draft in the expert reviewing process. IPCC should invent a better procedure for the reviewing work, which would be very important for substantial improvement of the report draft. I hope to see a better Chapter 6. The chapter in its current state would be severely criticized. [Guoyu REN]	Noted
6-82	A	0:0		In general the first draft of Chapter 6 is impressive and well-organized. However there are areas, not surprisingly, in need of change. Considerable attention over the last years has been directed at the TAR contention that current northern-hemisphere temperatures now exceed anything observed over the last millennium. The scientific basis of serious scientific critiques of this contention have been two-fold. Firstly multi-centennial (low-frequency) slow systematic calibration drifts are poorly determined in most proxy temperature determinations and as these are degenerate with real physical change there is an intrinsic problem to comparing temperature	Rejected, not supported by the litterature.

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				measurements separated by centuries. This problem should be largely eliminated in comparisons of rates of change of temperatures. Had the TAR put its primary emphasis on the conclusion that the warming change over the last fifty years had significantly exceeded that for any previous fifty year period over the last millennium it could have avoided a lot of controversy As a guide to policy this is in fact the central issue. [David Ritson]	
6-83	A	0:0		Secondly proxy responses to changes induced directly by temperature are closely degenerate to changes induced directly by atmospheric CO2 (tree fertilization) or by ocean acidity (coral growth). [David Ritson]	Taken into account, will be discussed in rewrite of uncertainty text.
6-84	A	0:0		I had hoped that the FAR would finally set these questions to rest, and it was therefore a disappointment to see from Figure 6.8b the new, apparently equal or better(?), data reconstructions of Cook (2004), and D'Arrigo (in preparation). These appear to show `fifty-year' changes as high as .7 deg C, substantially larger than those for the simulated data results shown in Figure 6.10b, and comparable with the run-up over the last fifty years. These are largely dismissed in the statement in your 6.30 lines 55-56 containing "all reconstructions are effectively encompassed within the uncertainty previously indicated in the TAR". For such an argument to be meaningful the text should make clear whether the error-bands are bounds to systematic errors over century time-scales. or are envelopes of two-sigma annual variances, or whatever. It of course stretches the bounds of credulity to believe that the TAR correctly provided error-bounds encompassing all later results. [David Ritson]	Noted, will be considered
6-85	A	0:0		Everybody has their own questions as to correlations, residual differences, scaling and systematic trends. The only way they can be answered is to make available the input plot data used to construct Figures 6.8b and 6.10b. Prior to the next draft I would appreciate it if you could provide these to me. [David Ritson]	Noted,
6-86	A	0:0		Relative to the relationship of proxy responses to temperature Chapter 6 overviews an impressive number of millenial proxy studies. Based on the collective set I would expect you to provide better evaluations of the fidelity, limitations and weightings to be assigned to proxy classes than are contained in the individual papers. The current draft is a good summary of extant results but weak on deriving conclusions. [David Ritson]	Noted,
6-87	A	0:0		With regard to referencing, I would suggest that, whenever possible, out-dated papers be discarded or, alternatively, flagged with a "see Doe et al. (20NN) for latest revised results". If a paper contains now known errors or inaccuracies these should be flagged in	Noted

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				the reference. As an example Zorita and von Storch have submitted a paper to Memorie della Societa Astronomica Italiana, July 31 2005 "Methodical Aspects of reconstructing non-local historical temperatures". This paper admits that their highly publicized critique of the Mann et al analysis, von Storch et al.(2004), while purporting to follow the Mann et al procedures, in fact used calibration scale factors qualitatively different from those used by Mann et al. This invalidates their conclusions specific to the Mann et al analysis procedures. If such papers are used they should be referenced with an attached warning of the type "see later work of (Zorita and Storch ...) for modifications to conclusions to this paper." [David Ritson]	
6-88	A	0:0		At 6-29 line 7 McIntyre and McKitrick (2003) are referenced and discussed. Since that time they have updated and modified their results in GRL (2005) and the Energy and the Environment (2005), This later work is omitted in Chapter 6. If you discuss their work it should be in a 2005 context. With limited space (their GRL paper has been followed by two published critical comments and two that are under consideration by GRL plus M&M replies) you might decide to omit discussion of their work. The current approach (only reference and discuss M&M03) is neither fish nor fowl and will not fly. [David Ritson]	Taken into account.
6-89	A	0:0		At the next draft level I may, or may not, have more detailed suggestions? [David Ritson]	Noted
6-90	A	0:0		Overall I would like to congratulate the authors for producing such a succinct overview from the smorgasborad of paleoclimatic data gathered during the past decades. [Michael Schulz]	Noted
6-91	A	0:0		The chapter is well written but could be a little shorter to be punchier. As a Southern Hemisphere correspondent it has the normal heavy bias to the Northern high latitudes to the detriment of the tropics and Southern hemisphere. This reflects the reality of paleoclimate work so is in a sense inevitable. Other than this bias the chapter is sensibly laid out and generally cautious and accurate in the views expressed. Overall, a good job. Thank you to all the authors who have put in so much effort. [James Shulmeister]	Noted
6-92	A	0:0		Most of the subsection headings are questions. This is not consistent with the other Chapters, and perhaps conveys a style more in keeping with a ‘popular’ magazine. Perhaps change this. [Ian Simmonds]	Rejected, authors believe style is appropriate
6-93	A	0:0		General remark: Recently another simulation covering the period 1500-2000 has been published (Stendel, M., I.A. Mogensen and J.H. Christensen, 2005a: Influence of various forcings on global climate in historical times using a coupled AOGCM. Clim. Dyn. 25,	Accepted

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				10.1007/s00382-005-0041-4). In this study, the coupled model ECHAM4-OPYC has been used. ECHAM4 was run with a higher resolution than the ECHO-G runs cited (T42 instead of T30). Contrasting other coupled GCM studies, latitudinally-dependent volcanic forcing and temporally-variable vegetation have been used as forcing data. Data can be obtained from the first author of this paper (mas@dmi.dk). [Martin Stendel]	
6-94	A	0:0		overall, an excellent job, a very useful and important chapter! [Thomas Stocker]	Noted
6-95	A	0:0		Authors deserve high praise for synthesizing large amounts of information from a very broad and rapidly growing field. In my view, they could take a more confident tone and spend less of the text focusing on statements of how much the field has advanced, how mature it is, etc. [Robert Thompson]	Noted
6-96	A	0:0		Much of the discussion involves placing 20th century warming in the context of past warm intervals. This is an important objective, but I would place more emphasis on studies of the past as providing the basis for understanding the complex climate-ocean-land system and for testing models of this system. [Robert Thompson]	Taken into account, will try to improve within space limitations
6-97	A	0:0		Maybe it is just me, but I found the organization of the chapter confusing. Initially say that they are following time, but then go into Pliocene, then back to Paleogene-Eocene. Much of the chapter is devoted to individual questions, but overlying organization often not clear to me. [Robert Thompson]	Rejected, P-E event is an event, not something that deals with the time evolution of climate
6-98	A	0:0		This chapter has a strong focus on the rôle of the North Atlantic Ocean in the climate system, probably due both to its true importance and to the interests of the authors. Is this too strong a focus on one region? [Robert Thompson]	Rejected, authors believe it is appppropriate
6-99	A	0:0		The organization of this chapter is very clear and convincing [Heinz Wanner]	Noted
6-100	A	0:0		This chapter mainly focuses on past temperatures. Should it not also include variations in past precipitation as e.g. in Pauling et al. 2005 for Europe? (Pauling, A., Luterbacher, J., Casty, C., and Wanner, H., 2005: 500 years of gridded high-resolution precipitation reconstructions over Europe and the connection to large-scale circulation, Clim. Dyn., accepted.). [Heinz Wanner]	Rejected, due to space limitations
6-101	A	0:0		Highlight more the uncertainties and differences among the various reconstructions and provide some reasonings for the discrepancies.	Accepted

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				[Heinz Wanner]	
6-102	A	0:0		Was it the clear intention of the authors not to discuss the processes which led to the Younger Dryas event, and why? [Heinz Wanner]	Rejected, level of detail is appropriate
6-103	A	0:0		I see the IPCC chapter on Paleoclimatology as a brief by the Paleoclimate Research Community to inform policy makers on the state of the science through an explanation of what we know, how well we know it, and what we cannot know. Our understanding of modern climate dynamics has evolved into a probablistic approach where careful attention is paid to signal to noise. For this reason I believe that the Chapter needs to revisit how well we know some of the information presented and treat our understanding and modeling of paleoclimate as probablistic rather than deterministic and a consistent analysis of signal to noise in the interpreations. For example on page 28 lines 51-54, there is a fair assessment of what we do not know all that well in the Southern Hemisphere whereas in Table 6.1 on page 67 a PMIP-2 concensus of 0-3 C for LGM tropical ocean cooling is presented which seems like a significant range to be considered a concensus. [Robert Webb]	Accepted
6-104	A	0:0		If the IPCC chapter on Paleoclimatology is to be of value to policy makers and other non-paleoclimate experts, then a table is needed that provides the range of dates associated with the names for various geologic times such as Bolling/Allerod, Holocene, Preboreal, LIA, LIG, LGM, first millenniun of the Christian era, etc. My sense is that the even various lead authors may have different definitions within the chapter. [Robert Webb]	Accepted
6-105	A	0:0		Consistent treatment of what we mean by regional versus continental, hemispheric, or global climate signals within the Chapter. For instance the Chapter presents a convincing argument that Medieval Warm Period is a transient regional event on page 28 lines 26-34; however, this level of rigor in terms assessing the regional signals and synchroneity/lead-lag of these signals is not applied consistently in discussion of climate conditions at other times in the past. [Robert Webb]	Accepted
6-106	A	0:0		Excellent review, but a bit short on "classical" quaternary; Nothing or little on: mid-Pleisto climate shift, mid-Brunher amplitude shift, possible reasons for multitude increase [Gerold Wefer]	Rejected, due to space limitations
6-107	A	0:0		Throughout the chapter you are inconsistent in units for years: yr or a [Eric Wolff]	Accepted
6-108	A	0:0		Throughout the chapter there are numerous uses of myr or ma for megayears. Should be Myr or Ma. [Eric Wolff]	Accepted

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6-109	A	0:0		For gas concentrations you use ppm, rather than the more common and precise ppmv [Eric Wolff]	Accepted
6-110	A	0:0		Augustin et al should read "EPICA Community Members 2004" throughout [Eric Wolff]	Accepted
6-111	A	1:0		A good chapter bringing together much material widely scattered over the literature. [Bryant McAvaney]	Noted
6-112	A	1:1	1:1	This chapter seems to have been written with an agenda: i.e., do everything possible to back-up the statement that human activity is driving the current warming. To fulfill this goal, the authors refuse to acknowledge that basic questions in paleoclimate are still quite uncertain, or state clearly that models have problems reproducing some basic aspects of paleodata - since that might cast aspersions on our projections for the future. Another element at play is an egostistical reluctance to admit, perhaps even to themselves, that many questions are not yet answered with any degree of certainty. The chapter should be rewritten to more properly indicate what we know and don't know, and what we can and cannot model, in each of these areas. That would actually give more credence to the chapter and to the report as a whole, and better serve the purpose of using paleoclimate studies to provide perspective on our ability to understand future climate projections. [Andrew Lacis]	Rejected, authors believe chapter is balanced
6-113	A	1:14	1:14	Fluckiger has to be Flückiger, Erik should be Eric [Thomas Blunier]	Accepted
6-114	A	1:45	1:45	By titling this box as it is done, it gives the (mis)impression that there really is such a Medieval Warm Period, when in fact the text indicates that the warming was not all at the same time around the world and this period was not really warmer than the mid 20th century. I would urge changing the name of the box by either putting "Medieval Warm Period" in quotes, or better still, giving the time period that this box is covering and do not re-enforce the image that there really is such a period. [Michael MacCracken]	Rejected, text makes this clear, but will be improved and more presise
6-115	A	1:54	1:55	What is the role of such "Questions"? Why are they out of the frame of the whole chapter? [Paolo Cherubini]	Noted
6-116	A	2:0	5:	Overall, I felt a bit lost, reading the executive summary. In some way, I missed a structured order in the summary. Somehow, the huge information given in such isolated sentences and paragraphs must be ordered, either following a sequential logical flow of the infos achieved, or going from the most important to the less important. [Paolo Cherubini]	Taken into account when SOD is written
6-117	A	2:0	5:	the executive summary should probably in the final version be shortened but at this stage is usefull to clearly emphasize the conclusions	As above

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				[Sylvie JOUSSAUME]	
6-118	A	2:0	5:	I wish to commend the writing team for the excellent Executive Summary, which does a splendid job in bringing out the lessons learned from paleoclimate research for the assessment of our current understanding of anthropogenic climate change. [Jochem Marotzke]	Noted
6-119	A	2:0	5:0	I like the large-scale structure of the summary, going from very long time scels to more recent time scales. But within each of the sections there could eb a clearer structure - start with the oldest evidence or longest time scales, and finish with the modelling results. At the moment there is insufficient coherence or structure within each of teh four sections of the summary. [Neville Nicholls]	Noted, will be considered
6-120	A	2:0		Executive Summary: I found the Executuve Summary quite difficult to follow. While some other chapters use the bullet-point style, they still lead the reader through the summary in an easy to follow way. Other chapters use a mixture of prose and bullet points. Chapter 8 is a good example of an easy to follow summary. A re-statement of the results is not sufficient; contrasts and connections between the different results also need to be described. [Julia Hargreaves]	Noted, will be considered
6-121	A	2:0		The style of the executive summary is good but more care in use of likelihood statements and more subjective statements eg "appear to have been" etc. [Bryant McAvaney]	Noted, will be considered
6-122	A	2:1	5:10	The points made in the Executive Summary are indeed very important and it will be very helpful to interpreting the results of the overall report to have this very use summary of key points. [Michael MacCracken]	Noted
6-123	A	2:1		Section "Executive Summary" Since this is the part that most people will read I would suggest to make it more "digestable" by shortening (see below) and focunsing on the most important points (there are too many bullets and one is not able to see the wood for the trees) [Michael Schulz]	Noted, will be considered
6-124	A	2:1		Should Larsen C be included in this statement? [Ian Simmonds]	Noted, will be considered
6-125	A	2:1		Section: Executive summary. This summary is probably a bit long, can it be made shorter ? I would suggest the deletion of the following lines: p. 3-6. L21-26; p.3-6 L. 38-40.; p. 3-4 L. 1-5. [Philippe Tulkens]	Noted, will be considered

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6-126	A	2:1		Section: Executive summary.. I would suggest stressing in the executive summary that studying past climates is relevant and essential to the understanding of the climate system but that such analyses also reveal that there is no real analog over the past of the current climatic situation and trends in the recorded history. Therefore, the actual state is unique and may reveal unknown patterns of climate evolution. [Philippe Tulkens]	Noted, will be considered
6-127	A	2:3	2:9	Lines 6 to 9 do not give an answer to the question in lines 3 to 4. [Paolo Cherubini]	Accepted, will be changed
6-128	A	2:3	2:3	It will likely be necessary to have a box or some sort of way of defining a few terms--like Quaternary, etc. The following sentences do define what is meant by, for example, mid-Pliocene, but Quaternary is not defined. [Michael MacCracken]	Accepted
6-129	A	2:3		Delete this Heading. It is unnecessary. [Vincent Gray]	Noted, will be considered
6-130	A	2:6	2:9	The underlying chapter (Pg. 7, line 44 - Pg 8, line 28) describes a more complex situation for the mid-Pliocene, significant warming at high latitudes, but no change in tropical SST. The section also indicates that climate models do not simulate these conditions. Both the complexity of the climate response and the inablity of climate models to simulate that response need to be preserved in the Executive Summary. As noted in my comments on that section they raise significant questions about the ability of climate models to project climate under high CO2 conditions and the nature of the climate changes that might occur under those conditions. [Lenny Bernstein]	Noted, will be considered
6-131	A	2:6	2:9	Pg. 7, line 44 - Pg 8, line 28, indicates that that during the mid-Pliocene, there was significant warming at high latitudes, but no change in tropical SST, and that climate models do not simulate these conditions. The Executive Summary needs to include the complexity of the climate pattern and to indicate the inablity of climate models to simulate the pattern observed in the proxy data. These points raise questions about the ability of climate models to project climate under high CO2 conditions and the nature of the climate changes that might occur under those conditions. [Jeffrey Kueter]	Noted, will be considered
6-132	A	2:6	2:6	The opening phrase needs to indicate that the CO2 levels are higher than present, or preindustrial, or what is expected during the 21st century, or what. [Michael MacCracken]	Accepted
6-133	A	2:6	2:9	This is a very useful insight and research result. [Jerry Mahlman]	Noted
6-134	A	2:6	2:9	I find these sentences confusing since it is not clear what CO2 in the past was higher	Noted, will be considered

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				than… I suggest: 'Many pre-Quaternary climates featured levels of CO2 which were higher than modern levels. All of the pre-Quaternary climates which featured such high levels of CO2 were also associated with significantly warmer temperatures than today. This is the case both for climate states stable over ....' [Mark Siddall]	
6-135	A	2:6		"higher" than what? [Vincent Gray]	Accepted text will be clarified
6-136	A	2:6		"warmer" than what? [Vincent Gray]	Accepted, text will be clarified
6-137	A	2:6		…higher levels of CO2…' than when? Start sentence with "Relative to present, many pre-…" [Tas van Ommen]	Accepted, text will be clarified
6-138	A	2:7	2:8	The formulation "climate states stable over millions of years", followed by "the mid-Pliocene (3.5 ma)" is misleading since the mid-Pliocene has ended now (so millions of years means less than 3.5). When did the "stable" period start and when did it ended ? Or is the Pliocene the best example of a long and stable period ? [Didier PAILLARD]	Noted, will be considered
6-139	A	2:8	2:9	…3.5 ma), for warm events lasting a few hundred thousand years (eg. the Paleocene-Eocene Thermal Maximum), and for warm events lasting a few tens of thousands of years (eg. interglacial times between the recurring ice ages of the last 3 million years). [Steven Clemens]	Accepted
6-140	A	2:8	2:9	Here and elsewhere use SI convention 'Ma' for million years [ago]. [Donald Forbes]	Accepted
6-141	A	2:8	2:9	the notation "ma" for "millions of years ago" is not obvious for an executive summary. [Didier PAILLARD]	Accepted
6-142	A	2:11		Delete Heading. It is unnecessary [Vincent Gray]	Rejected, appropriate
6-143	A	2:13	2:16	The connection of CO2 levels in our 0-800,000 year "near past" to the very much higher levels of today put a very valuable perspective on what has happened to CO2 levels over pre-industrial times. The measurements of radiocarbon 14 in CO2 tell us quite precisely how much of the CO2 increase in the past century is due to burning of fossil fuels. [Jerry Mahlman]	Noted
6-144	A	2:13	2:16	The juxtaposition of this statement with the previous statement makes for an apparent contradiction - it is clear that increased CO2 in pre-Quaternary climates was not due to anthropogenic causes. Something explaining that anthropogenic CO2 breaks the natural, quasi-cyclical variation over the last 800 ka would be useful, e.g.:'Post-industrial levels of	Noted, will be considered

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				atmospheric CO2 and CH4 have risen above the maximum levels found in ice cores for the last four glacial cycles...' [Mark Siddall]	
6-145	A	2:14	2:14	Suggest moving the parenthetical phrase to after "record" [Michael MacCracken]	Accepted
6-146	A	2:14	2:14	"up to 800,000 years": So far the the entire CO2 / CH4 record is not published --> statement should be consistent with the published lit. [Michael Schulz]	Accepted, will refer to new EPICA time period
6-147	A	2:14		replace 800,000 yr by 650,000 yr. The longer refers to only the climate reconstruction, the gas records extend to 650,000 years (Siegenthaler et al, Spahni et al, 2005, see below for correct ref.) [Thomas Stocker]	Accepted
6-148	A	2:14		Ice core GHG records only go to 650,000 years so far [Eric Wolff]	Accepted
6-149	A	2:15		these data dont inform about mechanisms. Suggest: ".... trace gases is beyond natural variabilty." [Thomas Stocker]	Accepted
6-150	A	2:15		…multi-millennial…' (missing 'l') [Tas van Ommen]	Accepted
6-151	A	2:16	2:16	should read "…temperature and CO2 co-vary." [William Howard]	Accepted
6-152	A	2:16		Add “concentration” after “CO2”. Replace “co-vary with each other” with “tend to rise and fall over the same time periods” [Vincent Gray]	Acccepted
6-153	A	2:18	2:21	This bullet is a good example of the tone that motivates my overall criticism of this chapter. I do not believe we understand orbital forcing well enough to make such an unqualified statement. We might say something like "modeling suggests that the earth would not...," but the statement, as written, is too strong. [Anthony Broccoli]	Noted, will be considered
6-154	A	2:18	2:21	The current warmong trend will not be mitigated by a natural cooling trend towards glacial conditions. Understanding of orbital forcing indicates that the earth will not naturally enter another ice age for at least 30,000 years. [Steven Clemens]	Noted, will be considered
6-155	A	2:18	2:21	Is the ability of increased CO2 to prevent another ice age explored in this chapter? It shouldn't be, it should go in a future projections chapter. And if it is not, it shouldn't be stated here.	Accepted

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				[Andrew Lacis]	
6-156	A	2:18	2:22	This is almost a "no-brainer", but not obviously so to those looking for an excuse to ignore the human-caused global warming problem. [Jerry Mahlman]	Noted
6-157	A	2:18	2:21	"Understanding of orbital forcing indicates that the earth would not naturally enter another ice age for at least 30,000 years. " It might be pro-mature to say so because we still don't have sufficient and robust evidence presently. [Guoyu REN]	Rejected, statement based on publsished litterature and knowledge about orbital forcing and response
6-158	A	2:20	2:21	The statement that rising co2 will delay or prevent an ice age is false. Ocean carbonate compensation will bring co2 levels back to close to pre-anthropogenic levels within 5,000 to 8,000 years, maybe sooner, and the progress towards the ice age will be un-affected. See Archer et al. 1997 for mechanisms, the interpretation with respect to glaciation is mine (unpublished) [David M Anderson]	Noted, will be considered
6-159	A	2:20	2:20	Reword to say "The rising atmospheric CO2 concentration is likely to delay or prevent the Earth …" Given IPCC's efforts to develop a lexicon, its words should be used and the word "may" should be expunged from the report. Also, "Earth" should be consistently capitalized. [Michael MacCracken]	Noted, will be considered
6-160	A	2:20		There is no rationale for arguing that the current climate regieme re: glaciation is to persist another 30,000 years - perhaps 3000 years, but as pointed out later in the chapter, the interglacials range from 10,000 years to 30,0000 years, and we are already 10,000 years into this one. We also need to be aware of the 1100 year solar cylce here, that predicataed the Medieval Cl;imate Optimum and the Roman Climate Optimum, we are approaching that cycle zenith within the next two to three hundred years. [Lee C. Gerhard]	Rejected, statement does not reflect published knowledge
6-161	A	2:23	2:23	Add comas and end sentence with 'cover', as in "…altered land, ice, and vegetation cover." [C.F. Michael Lewis]	Accepted
6-162	A	2:23	2:23	LGM modelling should not be taken as the source of our estimates of global temperature changes at that time. Both the delta T and estimated forcings should be from obs (to the extent possible). The modelling is principally a validation for the models, and a consistency check on the data, but a small amount of cooling in a model that used small forcings does not tell us anything about the actual LGM. [Gavin Schmidt]	Accepted
6-163	A	2:23	2:29	Paragr. is too technical; mix of different forcings is confusing. Should be removed. [Michael Schulz]	Accepted, will be changed

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6-164	A	2:23		Add: Compared to the pre-industrial period, the last glacial maximum…. [Eric Wolff]	Accepted
6-165	A	2:24		increased land ice and altered vegetation [Eric Wolff]	Noted
6-166	A	2:25	2:27	I believe this radiative forcing change should be negative for both values, i.e. -4 W m-2 to -7 W m-2. [Donald Forbes]	Noted, will be considered
6-167	A	2:25		simulate" is surely the wrong word. Do you mean ":predict"? Use the past tense; "predicted [Vincent Gray]	Noted
6-168	A	2:26	2:29	Line 26 is referring to warming from LGM to present, whereas line 29 talks about additional cooling of 2 degrees. Make consistent. [James Crampton]	Noted
6-169	A	2:27	2:27	Change "7" to "-7" [Anthony Broccoli]	Accepted
6-170	A	2:27	2:27	Suggest changing the phrase to "changes in continental ice" and indicating that the forcing goes from -4 to -7 W m-2 so as not to confuse the reader. [Michael MacCracken]	Accepted
6-171	A	2:27	2:27	radiative forcing change of -4 to -7 (not 7) ? [Didier PAILLARD]	Accepted
6-172	A	2:27		Section #. Executive summary: Should read "-4 to -7 W m-2", not "-4 to 7 W m-2". [Becky Alexander]	Accepted
6-173	A	2:27		change to "7" to "-7" [Melanie Fitzpatrick]	Accepted
6-174	A	2:27		…change of -4 to 7 Wm-2…' presumably should be -7. Page 15 line 10 for consistency should also give signs -4 to -7. There is potential confusion for the reader with values on page 13 line 52…. The document would hang together better if the different ranges were acknowledged. [Tas van Ommen]	Accepted
6-175	A	2:29	2:29	be a bit more explicit. E.g… initial results suggest that, together, they could cause additional cooling of ~2 oC [Michel Crucifix]	Noted, will be considered
6-176	A	2:31	2:31	Global warming since the… [Steven Clemens]	Noted, will be considered
6-177	A	2:31	2:31	Sentence should read: "Global warming after the Last Glacial Maximum is comparable in magnitude with the projected …" ?	Noted, will be considered

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				[James Crampton]	
6-178	A	2:31	2:32	How can a “cooling” be “comparable” to a “warming”? [Vincent Gray]	Noted
6-179	A	2:31	2:33	This is an important punchline. Even informed laypersons" tend to be quite ignorant of this key point. [Jerry Mahlman]	Noted
6-180	A	2:31	2:33	Would this be clearer and stronger as "Global cooling and warming associated with past glacial maxima and minima are comparable in magnitude but not in rate to the projected global mean warming over the 21st century. The warming of the last glacial maximum happened more than ten times slower than the projected 21st century human-induced warming." Also, please clarify if this statement is or is not scenario dependent. [Susan Solomon]	Accepted
6-181	A	2:31		global cooling" seems an unfortunate notion. Too close to "global warming". Also the time from LGM to Holocene goes towards warming. Suggest: "The temperature difference from the Last Glacial Maximum" to today is comparable ... [Thomas Stocker]	Accepted
6-182	A	2:32	2:33	the warming after the LGM was abrupt in some locations (Greenland). May be use "global warming" instead? [Didier PAILLARD]	Noted, will be considered
6-183	A	2:32		This makes no sense. There is a wide range of “projected global mean warming over the 21st century” Does the comment apply to ALL of the “projections”? [Vincent Gray]	Noted, will be considered
6-184	A	2:33	2:33	The warming will certainly go for more than 100 years, so change "100 years expected for future warming" to "few century period of rising temperatures"--and perhaps even rephrase to indicate that the period of rise will be a few centuries, but it will persist to a large extent for millennia or more. [Michael MacCracken]	Noted, will be considered
6-185	A	2:35	2:38	This comment is very misleading. Later on in the chapter it would be appropriate to show that the model responses do not do a good job in matching both the ocean and land observations in the tropics. To imply that there is no problem here is to misrepresent the case, misleading readers about the ability of models to reproduce observations and giving a false sense of security about our understanding of tropical sensitivity. [Andrew Lacis]	Noted, will be considered
6-186	A	2:35	2:35	“are able” is not a very well constrained statement [Gavin Schmidt]	Accepted, will be reworded
6-187	A	2:35	2:38	Although it is definiterly good that models do a good job in modeling LGM climate I	Noted, will be considered

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				don't see how this paragr. leads to an immediate implication with respect to the header of this sub-section. I would suggest to remove this bullet. Also the implicit message that one can faithfully model SST and SAT changes although there are uncertainties in AMOC is disturbing. This statement is in conflict with the importance of the AMOC stated in lines 47-52 on the same page. [Michael Schulz]	
6-188	A	2:37		Add after “Atlantic” “by suitable adjustment of climate parameters” [Vincent Gray]	Rejected, not relevant
6-189	A	2:38	2:38	… exists on the details of Atlantic thermohaline circulation response … OR … on the details of the haline and thermal deep structures of the Atlantic. [Michel Crucifix]	Accepted
6-190	A	2:40	2:41	The differentiation between warm Dansagaard-Oeschger events and cold Heinrich events is very confusing. It reads like there is a glacial level and from there temperature goes up during DO-events and down during H-events. This is not the case. I suggest deleting "and several cold Heinrich events". [Thomas Blunier]	Noted, will be considered
6-191	A	2:40	2:45	Disagree with associating Heinrich events with temperature changes directly. Heinrich events were a result of collapse of the Laurentide ice sheet sending armadas of sediment-laden icebergs into the North Atlantic. These events may have had a global signal but the proxy record is often complicated to interpret in this context. With only four reliable Heinrich events to compare to, I think this connection is overplayed and should not be as broadcast as it has been in the literature. The language here of "cold Heinrich events" needs to be changed to reflect this. [Michelle Koutnik]	Rejected, text may be revised, but statement reflect published litterature on this
6-192	A	2:40	2:41	D/O events are the shorter term oscillations (a few hundred years) the longer term peak cooling (thousands of years) symbolize the Heinrich events. They make up the so-called "Bond" cycle. The description in this paragraph differentiating them by 'warming' versus 'cooling' is inaccurate. [Andrew Lacis]	Noted, will be considered
6-193	A	2:40	2:40	Add after '120,000 years,' "prior to 10,000 years ago,". The youngest Heinrich event is 14 14C ka BP [C.F. Michael Lewis]	Noted, will be considered
6-194	A	2:40	2:45	This is an invaluable point that even talented scientists overlook when they over-extrapolate these regional changes to assume a net planetary warming. [Jerry Mahlman]	Noted
6-195	A	2:40	2:41	As far as my understanding goes it is incorrect to label the Heinrich events as 'cold events,' this makes it seem that the two things are synonymous. Heinrich events as	Noted, text is appropriate in the sense that it reflects the published view.

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				described by Hemming 2004 are 'concretised layers' with high levels of iceBERG rafted debris, originating from Hudson Bay. The misuse of the term Heinrich event to label 'cold events' or all ICE Rafted Debris (including sea-ice-rafted debris!) is unhelpful, although widespread. In fact Hemming 2004 makes it clear that the H-events likely trigger N.Atlantic warming and NOT coolling. I suggest replacing ', and several cold Heinrich events.' with 'interspersed with cold events.' [Mark Siddall]	Text will be revised differentiate between IRD eventswhich form H-layers and the cold temperatures occurring at same time. See comments by Heming
6-196	A	2:40		If abrupt climate events are common why do you consider the temperature rise of the last 100 years “unprecedented”? [Vincent Gray]	Noted
6-197	A	2:40		replace "more than 20" by "at least 25" (ref. NorthGRIP members, Nature 2004) [Thomas Stocker]	Acccepted
6-198	A	2:41		Remove the "cold" before "Heinrich events" - sounds like there are hot and cold H events [Katsumi Matsumoto]	Rejected, see comment 195
6-199	A	2:41		Heinrich events are events of increased IRD, not cold events. Although a case is made that they may be associated in some records with increased cooling, it's sloppy terminology to refer to Heinrich events in this way [Eric Wolff]	Rejected, see comment 195
6-200	A	2:42	2:42	Temperature change of 16 degrees - does this refer to all the D-O events, or just some, or just one? [James Crampton]	Accepted, will clarify
6-201	A	2:42	2:42	“around the North Atlantic” is too broad a claim. 'Greenland' is correct, but no other records demonstrate temperature changes that large (as far as I know). [Gavin Schmidt]	Accepted
6-202	A	2:42	2:42	North Atlantic temperature records other than that for Greenland (at several thousand metres altitude on an ice sheet) are available. For example Edouard Bard's alkenone records from the N.Atlantic. (Bard, 2002, Physics Today, December, 31-38). This record shows ranges more of the order of 6 degs C. [Mark Siddall]	Noted, will be considered
6-203	A	2:42	2:42	The text on page 17 line 7 is more balanced. Suggested rewrite of sentence "During one of these events, the temperature over Greenland are reconstructed to have changed by 8 to 16 C within a decade or so." [Robert Webb]	Accepted
6-204	A	2:43	2:43	For non-scientific readers, there may be a contradiction between "abrupt events" and the fact that such events persisted for centuries. Might need to clarify that the onset or termination of these events were abrupt, but that the conditions once established persisted	Accepted

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				for centuries. [James Crampton]	
6-205	A	2:44	2:45	Define probably not [Thomas Karl]	Noted, will be considered
6-206	A	2:47	2:47	Define strong evidence [Thomas Karl]	Noted, will be considered
6-207	A	2:47	2:52	Paleo-proxy evidence doesn't support such a claim. Nearly all of the rapid change around North Atlantic Ocean occurred in glacial periods or stages of glacial-to-interglacial transition, and they obviously demanded the condition with ice-sheets in North America and Europe. The posssiblity for the rapid change to occur in current interglacial period would be extremely low. [Guoyu REN]	Rejected, text does not make the implied statement
6-208	A	2:49	2:50	I would suggest making the phrase "critical threshold" more informative by saying "critical temperature-salinity threshold" or something similar. [Michael MacCracken]	Noted, will be considered
6-209	A	2:50	2:52	Here and in the main text, a statement such as this should be tempered with consideration of the (low) probability of sufficient meltwater volume being available. [Donald Forbes]	Noted, will be considered
6-210	A	2:50	2:52	While not intended to do so, this sentence suggests in some way that it is likely that major circulation changes will occur. I propose to change the sentence to emphasize that our lack of understanding is important here. E.g. "It is unclear at present what and where these tresholds are and how they differ between glacial and modern climate. Therefore it cannot be ruled out that future warming and meltwater inflow could again trigger major ocean circulation changes." [Hendrik M. van Aken]	Noted, will be considered
6-211	A	2:51	2:51	Other sources of freshwater e.g. precipitation are more likely than meltwater. [Jonathan Gregory]	Noted
6-212	A	2:51	2:52	Should read "...glacial and modern climate, future warming and meltwater inflow cannot be ruled out as triggers for future major ocean circulation changes." [William Howard]	Accepted
6-213	A	2:51	:52	"cannot .... changes". This refers to future changes, not the topic of this chapter and hence should not be mentioned in this exec. summary. [Thomas Stocker]	Accepted
6-214	A	2:54	2:55	This point doesn't belong in chapter 6, since it's an aspect of the synthesis of information on 20th-century sea level change which is being done by chapter 5. The long-term ice-sheet contribution is covered there, drawing on chapters 4 and 6.	Accepted

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				[Jonathan Gregory]	
6-215	A	2:54	2:54	This rather definitive limiting of the contributions of Greenland and Antarctic Ice Sheets to sea level rise seems rather hard to accept given the very limited measurements of their extent through most of the 20th century. While isostatic approaches are helpful, should not this estimate be more qualified, saying something like "Current evidence makes it likely that no more than 5% ..."--but saying this without qualification seems overstated given how little change over East Antarctica (or the other ice sheets) it would take to violate this limitation. [Michael MacCracken]	Noted, will be considered
6-216	A	2:54	2:55	It is important to point out here that this is the third chapter that has addressed sea-level rise. Some bunching and shortening might prove to be helpful. [Jerry Mahlman]	Noted, will be considered
6-217	A	2:54	2:54	Is the figure of 5% fully consistent with those given in chapter 4 (section 4.8.2) and 5 (page 5-9 L.9) on the contribution of ice sheets to sea level rise over the 20th century ? I did not check, if it's consistent, please ignore this comment. [Philippe Tulkens]	Noted, will be considered
6-218	A	2:54	:55	Section #. Executive summary: If only 5% of global sea level rise can be attributed to the disappearance of glacial ice sheets, what is the other 95% attributed to? It's not clear why this sentence focuses on this 5%. [Becky Alexander]	Accepted, text will be made clearer
6-219	A	2:54		The sentence starting "Regionally…" needs re-wording. I think the point here is that sea level rise is difficult to discern in places where crustal rebound is occurring - and that crustal rebound can be greater in magnitude. However the wording does not make that obvious. [Melanie Fitzpatrick]	Noted, will be considered
6-220	A	3:0		Given that the IPCC has agreed on a lexicon of likelihood to be used, these terms should be used consistently through the chapters, and words like "may" and "could" should be expunged from the text as violating the lexicon. [Michael MacCracken]	Noted, will be considered
6-221	A	3:0		It is a bit more than an editorial question, but while it is fine to talk about "climate change" rather than "climatic change" I do not think one should talk about "climate changes" but rather should say "changes in climate or "climatic changes"--in any case, there is a need for consistency across the chapters. [Michael MacCracken]	Noted, will be considered
6-222	A	3:1	3:2	Add the following sentence: However, the rest of Antarctica is not warming. While the current statement is correct, it is misleading. The Antarctic Peninsular is warming, but it is a small part of Antarctica and the rest of the continent is not warming. Chapter 5 (Pg. 3,	Rejected, not relevant for this chapter

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				lines 8-9) concludes that the net contribution of the Greenland and Antarctic ice sheets to sea level rise over the past decade is 0.0 +/- 0.2 mm/year. [Lenny Bernstein]	
6-223	A	3:1	3:2	This summary statement needs to include the fact that the rest of Antarctica has not been warming Focusing just on the Antarctic Peninsular is misleading. [Jeffrey Kueter]	Rejected, not relevant for this chapter, statement has specifically to do with ice streams on AA Peninsula
6-224	A	3:1	3:3	Would it be more accurate to refer to the 'enhanced' warming rather than 'prolonged' warming? The warming of the Peninsula region is unlikely to have been more prolonged than other regions, but it is enhanced compared to others. [Susan Solomon]	Accepted
6-225	A	3:1	3:2	Although Larsen B has not collapsed before, you should not ignore the evidence that another ice shelf on the Antarctic Peninsula (George VI, on the west side) is stable now, but apparently did collapse in the early Holocene (Bentley, M.J., D.A. Hodgson, D.E. Sugden, S.J. Roberts, J.A. Smith, M.J. Leng, and C. Bryant, Early Holocene retreat of George VI Ice Shelf, Antarctic Peninsula, Geology, 33 (3), 173-176, 2005.). It may be that a different oceanic regime affected George VI, but also it may be that Larsen B was stable in the early Holocene simply because it was grounded then. While current AP warming probably is important, it is not yet clear that it is unprecedented in the Holocene. [Eric Wolff]	Noted, will be considered
6-226	A	3:2	3:2	Does this chapter explore that the Larsen Ice Shelf B collapse was the result of recent warming? If not, then this comment is inappropriate. [Andrew Lacis]	Noted
6-227	A	3:4	3:7	This, of course, is somewhat speculative, but it does make the point very clear, even if the 13K time scale of the sea-level rise is beyond our current comprehension. It does raise a point, however, that many have ignored: responses of the world's ocean and ice systems are far slower than most humans can comprehend. [Jerry Mahlman]	Noted
6-228	A	3:4	3:5	You are ignoring the assertion of NorthGRIP Project Members (2004) that Greenland was 5 degrees warmer but was not much reduced in size in the last interglacial. This contradicts the idea that the ice sheet contributed 3-4 metres of sea level. Their line of reasoning, which involves the small Renland ice sheet, is logical, but they have in the end to rely on some disturbed ice at renland. I don't therefore see their contribution as decisive, but the balance of evidence is not represented here. [Eric Wolff]	Rejected, new publications support statement
6-229	A	3:5	3:7	The potential links between Greenland Ice sheet melting and deglaciation of West Antarctic Ice Sheet is not well known. If I have followed well the text of Chapter 6, this sentence is mainly based on a paper that is submitted (Overpeck et al. 2005). As a	Rejected, can bring statements forward into summary also from recent publications. Papers on this are

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				consequence, the community has not yet had the time to react to this hypothesis. I would thus not include it in the summary. [Hugues Goosse]	accepted for publication and available to reviewers
6-230	A	3:5	3:5	This phrase needs to have a term from the IPCC lexicon added, so might replace "contributed" by "likely contributed" [Michael MacCracken]	Noted
6-231	A	3:5	3:7	In this sentence, change the first word ("Warming" to "Temperatures" as these are likely what is comparable rather than the change in temperature; in line 6 change "may have" to "likely" and in line 7, for clarity for the general reader, change the parenthetical expression to "(equivalent to > 1 m/century of sea level rise)" [Michael MacCracken]	Noted, will be considered
6-232	A	3:6	3:7	warming in the Arctic during the previous interglacial is comparable to warming expected at the end of this century" : should add "although arising from a different forcing [Sylvie JOUSSAUME]	Accepted
6-233	A	3:6		“warming expected at the end of this century” Does this mean you expect a sudden rise in the year 2099? [Vincent Gray]	Rejected, comment not on topic
6-234	A	3:7	3:7	Not clear what the "(>1 m/century)" refers to - presumably this is sea level rise resulting from WAIS deglaciation? [James Crampton]	Noted, will be considered
6-235	A	3:7	3:7	Does (>1 m/century) refer to sea level rise? If so greater clarity in wording is proposed e.g. (sea level rise >1 m/century) [Klaus Radunsky]	Noted, will be considered
6-236	A	3:7		does this number « 1m/century » refer to the associated sea-level rise? [Robert Thompson]	Noted, will be considered
6-237	A	3:9		Delete Heading. It is unnecessary [Vincent Gray]	Rejected, heading approppriate
6-238	A	3:11	3:11	Change "warmer" to "warmer and cooler" as the changes were of both sign. [Michael MacCracken]	Accepted
6-239	A	3:15	3:19	Among commonly cited warm period, the Medieval Warm Period is mentioned but,at the end of the paragraph, it is said that “this is consistent with our understanding of orbital forcing”. Besides, the Medieval Warm Period was probably not related to orbital forcing, as mentioned later in the text. I would thus modify the wording “understanding of orbital forcing” by “understanding of past forcings” [Hugues Goosse]	Noted, will be considered
6-240	A	3:15	3:29	See below (comment 4)	Noted

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				[Julia Hargreaves]	
6-241	A	3:15	3:19	This is a very important point. Clearly, climate change can be intrinically regional or global, depending on the nature of the climate forcing mechanisms, and the time scales of the forcings. This a point that some of the other chapters, particularly, have not yet addressed meaningfully. [Jerry Mahlman]	Noted
6-242	A	3:15	3:19	How do you know that when there is no global or northern hemispheric average temperature curve up to now? Most paleo-climatologists would not agree to this conclusion, because a lot of paleo-records showed that there was ever a warmer period sometime during 9000-3000 yr. BP in northern hemisphere. [Guoyu REN]	Rejected, main text and published evidence show that the warming was most pronounced at high latitudes and that parts of the tropics were colder.
6-243	A	3:17	3:19	The statement "There are no known periods of synchronous global warmth comparable to the late [Henry Diaz]	Noted
6-244	A	3:17	3:19	Rewrite last sentence "Consistent with our understanding of orbital forcing, there are no known periods of synchronous global warmth comparable to the late 20th century during the Holocene." [Robert Webb]	Accepted
6-245	A	3:17		This statement is incorrest. There is insufficient coverage of data to be sure how pervasive warm or cold periods were over the Holocene. See Soon and Baliunas 2003 [Vincent Gray]	Rejected, the comment does not reflect published litterature. Cited references makes the point supportable
6-246	A	3:18	3:18	I would suggest moving the phrase "during the Holocene" to the start of the sentence [Michael MacCracken]	Noted, will be considered
6-247	A	3:21	3:25	Paragr. is too technical; Should be removed. [Michael Schulz]	Noted, will be considered
6-248	A	3:21	3:25	This item of the executive summary does not seem directly connected to the question given on line 9. Could it be moved elsewhere in the executive summary section or deleted ? [Philippe Tulkens]	Noted, will be considered
6-249	A	3:22	3:22	I would suggest changing "observed climate change" to "observed climate conditions" as that is really what is observed--plus "change" is used later in the sentence. [Michael MacCracken]	Noted, will be considered
6-250	A	3:24		... climate models for the mid-Holocene perform generally better than atmosphere-only models. ... [Jerry Mahlman]	Noted, will be considered
6-251	A	3:25		"." is missing	Noted, will be considered

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				[Paolo Cherubini]	
6-252	A	3:27	3:27	The beginning of this statement should read "The present near global retreat…" to be consistent with the underlying chapter (Pg. 22, line 23). [Lenny Bernstein]	Noted, will be considered
6-253	A	3:27	3:27	The present global retreat of glaciers is unparalleled…" Perhaps should read The present global retreat of alpine glaciers is unparalleled… [William Howard]	Noted, will be considered
6-254	A	3:27	3:27	I would suggest changing "glaciers" to "mountain glaciers" to be sure to be distinct from the ice sheet related glaciers--which should be mentioned separately. [Michael MacCracken]	Accepted
6-255	A	3:27	3:30	This is an insight that escapes even climate scientists at times. Lines 36-38. This is obviously a no-brainer, but it needs to be here to counter this silly assertion of the contrarians. northern latitudes' warming. [Jerry Mahlman]	Noted
6-256	A	3:27	3:27	The 20th/early 21st century global retreat of glaciers is apparently unparalleled since the mid Holocene (after ~7000 cal. yr BP), and disagrees with..... [Atle Nesje]	Noted, will be considered
6-257	A	3:27		Replace “is unparalleled” with “has not been found from at other periods in the Holocene from the incomplete data available”. Theere is insufficient evidence of glacier retrteat during the Holocene for this statement to tbe true [Vincent Gray]	Rejected, reflects assesment in glacier box
6-258	A	3:28	3:29	this bullet is not very clear. [Sylvie JOUSSAUME]	Noted, will be considered
6-259	A	3:32	3:32	I would suggest replacing "account for" by "explain" [Michael MacCracken]	Accepted
6-260	A	3:32		Insert “all of” after “for” [Vincent Gray]	Noted, will be considered
6-261	A	3:35	3:36	There is probably a global temperature variation during D/O events (the temporal structure of the events are quite different in the North and in the South). This is a consequence of net heat storage in the deep ocean. Cf., for example, Stocker T, and Johnsen S.J. Paleoceanography 20(1) PA 1002 (2005). This is a recurrent issue throughout the manuscript (e.g. p. 6-65) [Michel Crucifix]	Accepted, will be reworded
6-262	A	3:35	3:36	The following sentence, "There is no evidence for centennial to millennial modes of natural climate variability generating global warming and cooling in the past, or that could explain global warming of the last 150 years." sounds strange. If this sentence	Rejected, text will be made clearer, but the text refers to modes of variability.

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				explains the conclusion of TAR, I can understand. But, we already know several papers reporting rather large fluctuations in the temperature of the northern hemisphere ([1] Esper et al., 2002; [2] Moberg et al., 2005; [3] Luckman, B.H. and Wilson, R.J.S. 2005. Summer temperatures in the Canadian Rockies during the last millennium: a revised record. Climate Dynamics 24: 131-144.). These results suggest that the temperature changes during 1850-1950 seem essentially the same as those in the past. If the temperature change in the second half of the 20th century cannot be explained considering only volcanoes and solar luminosity, neither the temperature changes in the past. Even if the mechanism of the large temperature fluctuations found by the above reports is not known or inexplicable, the observation should be approved as it is. (I am not talking about the latter half of the 20th century.) [Kiminori Itoh]	
6-263	A	3:35	3:36	Strongly agree with this bullet point. [Michelle Koutnik]	Noted
6-264	A	3:35	3:36	In fact there is evidence that the cycles in the Northern and tropical Atlantic are not reproduced, either in terms of frequency or phase, in the S.H. - a stronger statement is thus possible. [Andrew Lacis]	Noted
6-265	A	3:35	3:36	This is a very important statement, and I think is said very well. Because of this, I would suggest that the terms "Little Ice Age" and "Medieval Climate Optimum" and the like not be used, and that instead mentioned should be made of cooler than average or warmer than average conditions during certain periods in certain regions, etc.--so, do not continue to give credibility to supposed worldwide natural variations that are truly not global, etc. [Michael MacCracken]	Rejected, will be used but with qualifier
6-266	A	3:35	3:36	There were some periods of warming and cooling, even if these were relatively small - s they must have had natural causes. I think this statement is too extreme. [Neville Nicholls]	Rejected, authors think it is appropriate here
6-267	A	3:35	3:36	the conclusion would not be supported by most proxy-data analyses. It is still too early to say that there is no evidence for centennial to millennial modes of natural climate variability generating global warming and cooling in the past… [Guoyu REN]	Rejected, statment reflects the present basis in litterature
6-268	A	3:35		Insert “all of” after “for” [Vincent Gray]	Noted, will be considered
6-269	A	3:38	3:40	Shifts, as reported in the lit, are often a result of changes in land-use management (e.g., changes in grazing pressure). This fact should be emphasized. [Paolo Cherubini]	Rejected, we do not have enough evidence to support this
6-270	A	3:38	3:39	…northward shifts of the treeline…	Accepted

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				[Steven Clemens]	
6-271	A	3:38	3:40	It might also be noted here that the climate and vegetation model simulations indicate the importance of land-surface feedbacks in simulating the past degree of change. The 6 ka data-model comparisions also strongly indicate that changes in land-surface conditions have very important effects on the simulations of past climates. [Robert Thompson]	Noted, will be considered
6-272	A	3:40	3:40	"NH" - elsewhere northern hemisphere is spelt out in full. [James Crampton]	Accepted
6-273	A	3:40	3:40	Spell out 'NH' in executive summary - Northern Hemisphere as used on following page? [C.F. Michael Lewis]	Accepted
6-274	A	3:42	3:42	Spell out ENSO in full at first useage. [James Crampton]	Accepted,
6-275	A	3:42	3:44	Variation in ENSO extremes says nothing about the impacts of ENSO outside the Pacific - poor logic here. [Andrew Lacis]	Accepted, will modify statement
6-276	A	3:43	3:44	indicating that the impacts of ENSO are not stable as background climate and forcings change. [Steven Clemens]	Accepted, will modify statement
6-277	A	3:43	3:43	… impacts of ENSO are not stable … : these impacts (or imprint) change through time. It can't be said much about their "stability" (unstable = large variation if small perturbation). [Michel Crucifix]	Accepted, will modify statement
6-278	A	3:43	3:46	This is pretty shaky, but potentially interesting. [Jerry Mahlman]	Noted
6-279	A	3:46	3:49	What is the basis for stating that "slow changes in orbital forcing appear to have triggered abrupt changes" in hurricanes, floods, droughts, and tropical precipitation? In my opinion, the justification would require either (1) controlled experiments with models that show that such abrupt changes occur in response to orbital forcing, or (2) empirical evidence that such abrupt changes have occurred at similar phases of orbital variations multiple times in the past. Are either of these criteria met? [Anthony Broccoli]	Accepted, will modify statement
6-280	A	3:46	3:47	I ran out of time trying to find this in the main text, but I have misgivings about changes in orbital forcing triggering abrupt changes in hurricane frequency. [Donald Forbes]	Accepted, will modify statement
6-281	A	3:46	3:49	This is a rather weak statement, and should be clarified or removed. First says "changes in orbital forcing appear to have triggered abrupt changes in the frequency of hurricanes and floods, the frequency, extent and duration of droughts…" How well is this really known?	Accepted, will modify statement

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				In any case it then is contradicted by "The degree of forcing required to trigger such events remains uncertain, as causal mechanisms are not well understood." This whole section should stick to the best-established observational patterns. [William Howard]	
6-282	A	3:46	3:46	Define "appear to" [Thomas Karl]	Accepted, will modify statement
6-283	A	3:46	3:49	Since the last sentence says the causal mechanisms are not well understood, how can one say that slow changes in orbital forcing caused them (first sentence)? [Andrew Lacis]	
6-284	A	3:46	3:49	It seems to me that there should possibly be mention of other types of forcings, such as human-induced land cover change, as possibly contributing to the slow changes or regional departures, etc. While there are indeed considerable uncertainties, it does not seem to me that the assessment should be omitting the possibility that some of the changes could be due to human activities. [Michael MacCracken]	Accepted, will modify statement
6-285	A	3:46	3:49	Contradictory sentence - says that slow change in orbital forcing is the likely cause of abrupt climate change, yet also says that mechanism is not understood [Katsumi Matsumoto]	Accepted, will modify text
6-286	A	3:46	3:49	Rewrite entire bullet point to: The degree of forcing required to trigger hurricanes and floods, the frequecy, extent and duration of droughts, and the spatial and temporal character of tropical precipitation remains uncertain, as causal mechanisms are not well understood. [Atle Nesje]	Noted, will be considered
6-287	A	3:46	3:49	this statement should be revised. There were some changes in the frequency of climate extremes, but they might have nothing to do with the change in orbital forcing of Holocence. [Guoyu REN]	Rejected, boundary conditions may be important as stated in the litterature
6-288	A	3:46	3:46	Why just orbital forcing? [Gavin Schmidt]	Accepted, text modified
6-289	A	3:46	3:49	This also might be stated something like this: Gradual climatic and environmental changes during the Holocene have led to abrupt changes in climate, apparently because important thresholds were crossed. Studies of such past changes help identify these thresholds and to quantify the changes required to cross them. [Robert Thompson]	Noted, will be considered
6-290	A	3:46	:49	Section #. Executive summary: This statement is not addressed within the Chapter, and is not adequately supported. It should be removed.	Noted, will be clarified

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				[Becky Alexander]	
6-291	A	3:46	:49	Where is the evidence for these statement? [Melanie Fitzpatrick]	Noted
6-292	A	3:53	3:53	Spell out Third Assessment Report in full at first useage. [James Crampton]	Accepted
6-293	A	3:53	3:53	Possibly define first use of TAR in summary and chapter [C.F. Michael Lewis]	Accepted
6-294	A	3:53	3:53	Does TAR need to be defined here? [Mark Siddall]	Accepted
6-295	A	3:53		What is "TAR"? Please give the definition the first time it is mentioned. [Paolo Cherubini]	Accepted
6-296	A	4:1	4:5	This discussion strikes me as being vague and unquantitative. Rethinking this could help clarify the intended punchlines. [Jerry Mahlman]	Noted
6-297	A	4:1	4:5	It is unfair to say so. There are some researchers who showed one or two warmer conditions than were shown in the TAR, during 10-13 th century, including a few reconstructing regional average temperature in northern hemisphere. [Guoyu REN]	Rejected, the statement deals with the hemispheric mean T
6-298	A	4:4	4:5	The one study shows significantly warmer conditions during the 11th century, so much so that it is of some concern to scientists studying the effects of greenhouse warming. This sentence inappropriately plays down that result. [Andrew Lacis]	Rejected, the statement is based on a balance of evidence
6-299	A	4:4	4:16	This is not particularly convincing. [Jerry Mahlman]	Noted
6-300	A	4:4	4:5	While the 11th century temperatures for the reconstruction in question may be higher than the central estimate shown in the TAR, it is nonethelesss within the uncertainties in the TAR. This is a key point that should be made. [Michael Mann]	Accepted
6-301	A	4:4	4:4	Only one" This statement is related to Fig. 6.8. Here the reconstruction CED2004 also shows a pronounced max. around yr 1000 -- although less long than the warm phase in MSDDK2005. I would suggest to phrase this a bit more careful. E.g: "Only one ... suggests PROLONGED slightly ... [Michael Schulz]	Accepted, etxt will be revised
6-302	A	4:4	4:5	It would be very helpful if something could be said regarding the one reconstruction that suggests other findings. Without more explanation of whether this particular reconstruction is less reliable, subject to different uncertainties, etc., the rest of the	Accepted, text will be revised

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				paragraph and the next paragraph are weakened. [Susan Solomon]	
6-303	A	4:7	4:12	See below (comment 4) [Julia Hargreaves]	Noted
6-304	A	4:7	4:12	This is a very nice, and well considered, analysis. [Jerry Mahlman]	Noted
6-305	A	4:7	4:12	The sentence "Subsequent evidence reinforces this conclusion" is incorrect, and the rest of the paragraph is not consistent. I suggest you replace the text after the first sentence with: "Subsequent evidence has called into doubt the particular evidence relied on in the TAR. Some other studies have reached qualitatively similar conclusions regarding exceptional warmth, but a lack of independence of the data used across such studies, and the overall diversity of findings in the literature, indicates that while it is very likely the average Northern Hemisphere temperature in the late 20th century is higher than that of the past 500 years, a definitive comparison of the present-day global climate over the past 1000 or 2000 years remains more elusive than was thought to be the case at the time of the TAR." [Ross McKitrick]	Rejected, evidence cited in main text supports this conclusion
6-306	A	4:7	4:12	There are some researchers who showed one or two similar warm conditions during 10-13 th century. A few studies from China also indicate a similar warm period during that period with 20th century. If we exclude the urbanization effect on surface air temperature records of the past 50-100 years, it is in deed very difficulty to say that 20 th century was the warmest period in the past 1000 years and unusually warm compared with the last 2000 years. We still have a lot to do in this regard. [Guoyu REN]	Rejected, authors believe statement is supported by litterature. Uncertainties are mentioned in the main chapter
6-307	A	4:7	4:12	The report will be strongest if a bit more detail is provided here. It could be helpful to indicate that "Subsequent evidence has provided more information on how the latter half of the 20th century differed from the first half. It is likely that solar forcing contributed to the warmth of the first half of the 20th century, and some other 50-year periods may have been about as warm in the past, perhaps because of solar influences. However, it is very likely that....." [Susan Solomon]	Accepted
6-308	A	4:7	6:12	Underlying the debate about the hockey stick, there has been a real scientific controversy about estimation of uncertainty of the 1000 year record. It would be appropriate here to acknowledge the issues of estimating uncertainty before making any likelihood judgments in this paragraph. [Haroon Kheshgi]	Noted, is done in the main text
6-309	A	4:8		Replace “reinforces” by “casts doubt upon”. See McIntyre and McKitrick 2003, 2005 [Vincent Gray]	Rejected, text is adequate

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6-310	A	4:8		Replace “likely” with “ unlikely” [Vincent Gray]	Rejected, text is adequate
6-311	A	4:10		Replace “likely” with “ unlikely” [Vincent Gray]	Rejected, text is adequate
6-312	A	4:11	4:11	The phrase "unusually warm compared with the last 2000 years." needs to be defined or replaced. It refers to the last 50 years, not just the last decade. However, Figure 6.8 shows that the best estimate of temperature for 950-1100 was nearly as warm, and when uncertainty is taken into account, may have been as warm as the last 50 years. [Lenny Bernstein]	Noted, text will be clarified
6-313	A	4:11	4:11	The other parts of this finding are quantitative, but the phrase "unusually warm compared with the last 2000 years." is ambiguous, and probably pushes beyond a reasonable interpretation of the data. Figure 6.8 shows that the best estimate for the 950-1100 period was nearly as warm, and when uncertainty is taken into account, that period may have been warmer than the last 50 years. The conclusion should be limited to the last 1000 years. [Jeffrey Kueter]	Noted, text will be clarified
6-314	A	4:14	4:16	The data from the SH are too sparse to make such a conclusion. Suggest a statement following underlying text, page 6-32 line 16: “There are markedly fewer well-dated proxy records for the SH compared to the NH, and consequently little evidence of how large-scale average surface temperatures have changed in the past centuries." [Haroon Kheshgi]	Rejected, is adequately based on rest of chapter
6-315	A	4:14	4:16	It would be helpful if this statement for the SH were more specific. Please define what is meant by 'unusual in a 350 to 1000 year context'. [Susan Solomon]	Accepted
6-316	A	4:18	4:22	This is maybe the best example of what is missing in this executive summary: structure. These sentences are probably the most important of the whole chapter, but are somewhere at the line 18 and 21 of page 4! It should be put at the beginning. [Paolo Cherubini]	Rejected, authors believe this is appropriate as is
6-317	A	4:18	4:18	Remove "reconstructions and" since they are useful to highlight past T variations, but not to "point to the increasing importance of greenhouse gases as the cause of unprecedented recent warming". [Jan Esper]	Accepted
6-318	A	4:18	4:19	Rephrase to "Paleoclimate reconstruction and simulations of climate of the last 1000 year point to the increasing importance of greenhouse gases as the cause of unprecented rates of recent warming [Thomas Karl]	Noted, will be considered

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6-319	A	4:18	4:19	These sentences refer to detection and attribution and go beyond the legitimate scope of this chapter. They should be removed. [Ross McKitrick]	Noted, text will be modified
6-320	A	4:19	4:19	Delete "unprecedented." The previous bullet point states that it is likely that the past 50 years were the warmest period in the past 1000 years. Likely indicates a 66-90% chance of being correct, but does not justify "unprecedent." [Lenny Bernstein]	Noted, will be considered
6-321	A	4:19	4:19	The adjective "unprecedented" is hyperbole that is inappropriate for an IPCC asessment and also not justified in this case. The previous conclusion was that it is likely that the past 50 years were the warmest period in the past 1000 years. Likely indicates a 66-90% chance of being correct, but does not justify "unprecedent." [Jeffrey Kueter]	Noted, will be considered
6-322	A	4:19		Delete “unprecedented” [Vincent Gray]	Rejected, statement based on published litterature
6-323	A	4:19		... of greenhouse gas radiative forcing as the cause of unprecedented... [Jerry Mahlman]	Noted
6-324	A	4:20	4:21	Please rephrase and coordinate with chapter 2's discussions on this point to ensure consistency. [Susan Solomon]	Accepted
6-325	A	4:21	4:22	With respect to solar energy and cosmic ray flux, it is important to review the evidence to confront published arguments that variability of 20th century warming in some cases is more consistent with these forcings than with CO2. [Donald Forbes]	Rejected, belongs in Ch.9
6-326	A	4:21	4:22	Is there an expected punchline here? What cause and effect is anticipated here? If any at a quantitatively significant level? If so, please explain. [Jerry Mahlman]	Noted, will be considered
6-327	A	4:21	4:21	Solar magnetic fields do not 'affect' irradiance – they may 'imply' something about irradiance. [Gavin Schmidt]	Taken into account, text will be revised
6-328	A	4:24	4:28	Well said--should perhaps be said more prominently. [Michael MacCracken]	Noted
6-329	A	4:24	4:28	the sun's role should not be under-estimated. A more balanced assessment should be made with regard of solar influence. [Guoyu REN]	Rejected, statement reflects current basis in litterature
6-330	A	4:24	4:28	You don't make the important point here that the increased CO2 is clearly anthropogenic, based on its level cf natural, and on isotopic arguments.	Accepted

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				[Eric Wolff]	
6-331	A	4:25	4:26	What is the meaning of: "The hemispheric temperature reconstructions are broadly consistent with the ice core CO2 record over the past millennium..."? The CO2 record is pretty much a flat line with a dramatic increase over the past 150 years or so. The T reconstructions, however, show long term cooling and warming over the past millennium accompanied with rich decadal scale variability. These records have entirely different spectra. [Jan Esper]	Accepted, will be clarified
6-332	A	4:27	4:28	reads "The CO2 record is not compatible with the existence of alarge solar forcing effect on climate over the last millennium." Should read " The temperature record is not compatible with the existence of alarge solar forcing effect on climate over the last millennium." ? Or is the comment here meant to convey the temperature feedback in the carbon cycle, i.e. temperature driving CO2? [William Howard]	Accepted, will be clarified
6-333	A	4:27	4:28	I believe this conclusion follows in large part from Gerber et al (2003). While I agree with the spirit of what is what is being said, I believe it is not accurately stated. The Gerber et al (2003) result places a constraint (albeit one with its own caveats) based on carbon cycle information, on the amplitude of natural hemispheric mean temperature changes in past centuries--that's all! It doesn't place any specific constraints on e.g. the response to any one forcing (e.g. solar forcing). The findings appear to be inconsistent with large past changes in natural radiative forcing, which includes any large solar radiative forcing (e.g. as used by Von Storch et al, 2004), given moderate sensitivities. But an equally valid interpretation is that the results rule out large sensitivies (e.g. >4.5 C/2xCo2) to radiative forcing. Neither interpretation can be ruled out based on the constraints by this study alone. [Michael Mann]	Noted, will be considered
6-334	A	4:27	4:27	CO2 record is not compatible with large climate changes (however caused) – conceivably solar forcing could have been large, but climate sensitivity is small. [Gavin Schmidt]	Noted, will be considered
6-335	A	4:27	4:28	It would be helpful if the authors would define what is meant by 'large'. 0.5C in a century? [Susan Solomon]	Accepted
6-336	A	4:27	4:27	The chapter on projections is reffered to, the number of the chapter should be given. (chapter 10) [Philippe Tulkens]	Accepted
6-337	A	4:30	4:31	Is this a global statement or one about Europe specifically?	Acccepted, will clarify

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				[Jonathan Gregory]	
6-338	A	4:30	4:31	An example of the lack of clarity in the Executuve Summary. Whereas the latter two paragraphs emphasise regional warming and seem to imply that this means the current period is unusual, the earlier paragraph emphasises that regional warming is fairly commonplace. [Julia Hargreaves]	Noted, will be considered
6-339	A	4:30	4:31	This does not belong in Paleo Chapter --- if important should be in Chapter 3 [Thomas Karl]	Noted, will be considered
6-340	A	4:30	4:31	I think that you should leave this punchline, if any, to the first sections of Chapter 3 to deal with. It appears quite out of place here. [Jerry Mahlman]	Noted, will be considered
6-341	A	4:30	4:31	These lines add a narrow and gratuitous point, and convey an impression that the authors of the chapter are casting about for results indicative of warmth. The test of balance is simple: for those regions in which the available instrumental data shows, say, higher mean temperatures in the 1930s compared to the present, how many are listed in the bulleted summary with the statement that instrumental records show that the last 25 years was not the warmest in the past 280 years? No such regions are listed, though they exist, indicating that the chapter authors are especially alert for evidence that confirms a position they had adopted on prior grounds. This conveys to the reader that the evidence is organized in support of the prior conclusions, rather than the conclusions being drawn based on a comprehensive reading of the evidence. The chapter is thus hampered by confirmatory bias. [Ross McKitrick]	Rejected, statement in text accurate in terms of providing relevant evidence for this point. The scope of chapter is not on regional details
6-342	A	4:30		Replace" very likely" with "possibly" [Vincent Gray]	Rejected, statement in text is supported by the litterature
6-343	A	4:30	:31	If primarily restricted to Europe then regional signal "Early instrumental data, mostly from Europe, show that 1980–2004 was regionally very likely the warmest 25-year period during the last 280 years." [Robert Webb]	Accepted
6-344	A	4:33	4:34	It would be useful to add the period covered by these coral records. [Jan Esper]	Accepted
6-345	A	4:33	4:34	It would be helpful if this statement for the tropical Indo-Pacific were more specific. Please define what is meant by 'unusual ' and over what time interval. [Susan Solomon]	Accepted
6-346	A	4:40	4:41	This seems a bit odd. On interannual, decadal, century scales... What? [Jerry Mahlman]	Noted, will be considered

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6-347	A	4:40	4:41	The term "Asian monsoon" gives misunderstanding for asian climate. Mechanisms and variabilities of summer monsoon and winter monsoon are completely different. In this page (also in page 37), the term "Asian summer monsoon" would be appropriate. [Takehiko Mikami]	Accepted
6-348	A	4:40	4:41	I don't think that the evidence is strong and believable. [Guoyu REN]	Noted
6-349	A	4:40		Delete Heading. It is unnecessary [Vincent Gray]	Rejected, see same comment above
6-350	A	4:43	4:46	The statement seems a bit misleading. Cook et al (2004b) argue that the most recent drought in the western U.S. (measured by its total extent, from tree-ring data) is now approaching, or perhaps exceeding, that measure of drought over at least the past 1000 years. This is just one piece of evidence, and its tenative. But it is suggestive that we may be close to the edge of the envelope of past variability. The statement should be rephrased. [Michael Mann]	Noted, will be considered
6-351	A	4:45	4:45	The line starting “Current understanding....” adds absolutely nothing. Delete. [Gavin Schmidt]	Accepted
6-352	A	4:45	:46	Rewrite last sentence to reflect that the chance not only greater than zero but equally likely as in the past. " Current understanding suggests that the occurrence of decadal and longer drought remains at least as likely in the future. [Robert Webb]	Accepted
6-353	A	4:46		...occurrence of decadal and considerably longer drought ... [Jerry Mahlman]	Noted
6-354	A	4:48	5:10	Section: Executive summary. Some results given in question 6.2 would be worth mentioning in the executive summary. For instance, the ~80 ppm rise of CO2 concentration (p. 6-65 L.29-30) that took over 5000 years could be compared to the actual increase rate of CO2 concentration.. [Philippe Tulkens]	Noted, will be considered
6-355	A	4:48		In Chapter 7, a term "biogeophysical feedback" is used for biophysical processes important on planetary scale. Is there any difference with term "biophysical feedback" as it used in the Chapter 6? Would be good to have consistent terminology throughout the Report. [Victor Brovkin]	Noted, will be considered
6-356	A	4:50	4:51	This staement dosn't fit with the sub-section heading and should be removed (a similar statement is already at the start of execitive summary) [Michael Schulz]	Accepted

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6-357	A	4:50	4:51	The converse is also true (as seen during the LGM), and so this might be better stated to indicate the more general point – That is, there is a strong relation between global temperature and the concentration of carbon dioxide and other trace gases in the atmosphere. [Robert Thompson]	Accepted
6-358	A	4:50		Insert “often” before “consistent” [Vincent Gray]	Rejected, text is more accurate
6-359	A	4:53	4:56	"Paleoclimatic data suggest that biogeochemical and biophysical feedbacks have amplified changes in incoming solar energy caused by changes in the earth’s orbit around the sun." The statement might be read as biological feedbacks affect incoming solar energy which is not true for most feedbacks. I suggest to rephase "... feedbacks have amplified climatic changes caused by changes in the earth’s orbit around the sun" or "... feedbacks have amplified climatic changes caused by changes in the orbital forcing." Term "orbital forcing" is already used several times before, e.g. at page 3, line 18-19, and it presumably means changes in incoming solar energy caused by changes in the earth’s orbit around the sun. [Victor Brovkin]	Noted, will be considered
6-360	A	4:53	4:53	Define suggest in terms of this statement [Thomas Karl]	Noted, will be considered
6-361	A	4:53	4:56	The probability terms seem confounded in this section. It would seem that "suggest" is much weaker than likely, yet the suggestion of paleo-feedbacks is resulting in the likelihood of future amplification. Suggest removing the term likely, and replacing with the observation that modeled feedbacks are also positive, and refer to chapter 7. [Haroon Kheshgi]	Noted, will be considered
6-362	A	4:53	4:56	This "suggestion" needs expansion and clarification. [Jerry Mahlman]	Noted, will be considered
6-363	A	4:53	5:10	These paragraphs suggest that feedbacks have been very important, and that models capture them well in the key periods of *both* the last glacial maximum and the Holocene, periods characterized by markedly different forcings and changes in climate. This strengthens confidence in the model formulation, suggesting that dramatically different feedbacks are very unlikely to occur in the next century, and it would be helpful to state that. [Susan Solomon]	Accepted, will make statement to this extent
6-364	A	4:54	2:56	This statement implies that all feedbacks amplify. But we know this is not true. The greatest feedback on co2, buffering by the oceans, is negative (increasing co2 causes chemical reactions that reduce the increase). Eventually almost all the co2 emitted will be neutralized in the oceans, although the timescale is thousands of years. Specifically	Accepted, will clarify text

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				identify the feedbacks that are positive, or qualify the statement to say that many positive feedbacks exist that will amplify the effect, or include the statement that negative feedbacks also exist. [David M Anderson]	
6-365	A	4:54	4:54	Just a note that for consistency, it should be "Earth's" and "Sun" as these are the proper names we use for these celestial objects. [Michael MacCracken]	Accepted
6-366	A	5:0		FINAL COMMENT: It was a pleasure to review this chapter. Important new insights have been brought forward in ways that demonstrates the value of "looking backward" in a forward way. [Jerry Mahlman]	Noted
6-367	A	5:1	5:1	Replace "aeolian" with "wind-borne," more readers will understand what you mean. [Lenny Bernstein]	Accepted
6-368	A	5:1	5:3	The role of aeolian iron deposition into the oceans in regulating past atmospheric CO2 are evidenced by some researches, and it might be the best explaination for the lower level of atmospheric CO2 concentration in glacial period. [Guoyu REN]	Noted, is covered as a hypothesis in chapter
6-369	A	5:2	5:3	This is almost too vague to be of much value in this assessment process. [Jerry Mahlman]	Noted, will be considered
6-370	A	5:3	5:3	… strength of North Atlantic Deep Water… : remplace by … structure of the ocean circulation… [Michel Crucifix]	Rejected, this changes the meaning
6-371	A	5:5	5:6	And thus in localized ecosystems as well? I would expect so. [Jerry Mahlman]	Noted
6-372	A	5:8	5:9	Delete the first sentence of this conclusion. That judgement should be made by Chapter 8 on model evaluation. [Lenny Bernstein]	Noted, will be considered Noted, will be considered
6-373	A	5:8	5:10	Over what area is realism improved --- regional, local, global or all? [Thomas Karl]	Will clarify
6-374	A	5:8	5:10	Glad to hear this. [Jerry Mahlman]	Noted
6-375	A	5:8	5:10	Paragr. is too technical; Should be removed. [Michael Schulz]	Noted, will be considered
6-376	A	5:10	5:10	For a long time I couldn't find anything in the main text about vegetation-atmospheric interactions (coupling) at the LGM. This is because I was looking at 6-14, where it says that vegetation and aerosol changes have not yet been considered, let alone coupled into	Accepted

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				the models for the LGM. I should have been looking at 6-39, where vegetation-atmosphere modelling results are described. These two sections should refer to each other. [Julia Hargreaves]	
6-377	A	5:16	5:16	Delete “imminent” - this is way too limited in describing research during that period. [Gavin Schmidt]	Accepted
6-378	A	5:18		Should ‘fifteen’ be ‘seventeen’ (for publication in 2007)? [Ian Simmonds]	Noted, will be considered
6-379	A	6:1		The executive summary is very clear and excellent. However I would find it even stronger if it included a section on what is not known, or what is known but not yet understood. The unknowns may have consequences for climate projections, and it is better to identify these yourself rather than let someone from outside IPCC do the job. [Corinne Le Quere]	Accepted, will have a section in chapter on this
6-380	A	6:3	6:3	…data and knowledge of how the climate… [Steven Clemens]	Accepted
6-381	A	6:3	6:13	This paragraph strikes me as unnecessarily self-conscious. :-) All chapters have page constraints and other chapters are new as well. [Jonathan Gregory]	Noted, will be considered
6-382	A	6:3	6:3	I would suggest replacing "inform" by "provide information and context about" [Michael MacCracken]	Noted, will be considered
6-383	A	6:3	6:13	It is indeed important to provide a Paleoclimate chapter in IPCC. Nevertheless, in these first lines, the time scale has to be clearly defined with numbers. [GILLES RAMSTEIN]	Accepted
6-384	A	6:5	6:5	I would suggest replacing "for the future" with "with respect to the credibility of projections of future climate conditions" as more clearly expressing what is meant. [Michael MacCracken]	Noted, will be considered
6-385	A	6:6	6:6	"external" & "internal" are used without explanation, & the innocent reader has no chance of guessing that the effects of volcanoes are "external" but those of forests internal (only, the absence of a forest is external if humans are to blame...) Even if the discussion at 31-35 is adequate, it needs to be sooner. [William Ingram]	Noted, will ensure clarity
6-386	A	6:7	6:8	This statement does not belong in the Chapter -- delete [Thomas Karl]	Noted, will be considered
6-387	A	6:7	6:8	The sentence "Even so (…) page limits" could be deleted. All chapters face the same constraint and manage to give a relevant synthesis of the issues delat with. [Philippe Tulkens]	Noted, will be considered
6-388	A	6:8	6:11	You don’t need to explain how the structure of the chapter was determined.	Noted, will be considered

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				[Neville Nicholls]	
6-389	A	6:8	6:13	not relevant information, not specific to this chapter. Delete [Thomas Stocker]	Accepted
6-390	A	6:9	6:9	“Geologic” is misleading. To investigate even the last million years does not generally involve tectonics. Only Milankovitch forcing, glaciation and eustatic sea level rise are needed to understand the Quaternary. [GILLES RAMSTEIN]	Accepted
6-391	A	6:10	6:13	Simply indicate that paleo data provides a broader perspective on low-frequency changes and variations. [Thomas Karl]	Noted, will be considered
6-392	A	6:12	6:25	Very good, BUT a simple comment is important concerning the low frequencies: We need to know in which context the anthropogenic forcing is occurring. It takes place in an “Ice Age” [see Imbrie], when ice caps are existing on the Earth (which is not a frequent situation on the geologic time scale!). And this is drastically important because these ice sheets may melt, and the climate may come back to a more stable situation without any ice cap on the Earth. [GILLES RAMSTEIN]	Noted
6-393	A	6:13	6:13	"integrating" unless the meaning is already known (time integration, giving a "memory" & longer-timescale variability) [William Ingram]	Noted
6-394	A	6:13	6:13	Satellite data are able to give a superb view of climate variability but only for the last 30 years. To document events occurring that period or longer, we need records that are several centuries long. [GILLES RAMSTEIN]	Noted, will be considered
6-395	A	6:15	6:20	Yes, but the 1990 Assessment Report was not up to the state of the art in 1990 (unfortunately). One could consider citing COHMAP Project Members (1988), in Science, which was the first article that I know of to adopt the systematic joint use of palaeoclimate observations and modelling which has been widely adopted since then and which, to a large extent, informs tha current understanding of Quaternary palaeoclimates as summarized in this chapter. [Iain Colin Prentice]	Noted, will be considered
6-396	A	6:19	6:19	I think we are moving towards integrated observations and modelling, but we are not there yet for many of the periods outlined in the chapter. I would say "more integrated with respect to observations and modelling". [Julia Hargreaves]	Accepted

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6-397	A	6:19	6:20	this is also irrelevant information that can be deleted. Why should this non-specific sentence appear in chapter 6 ? [Thomas Stocker]	Accepted
6-398	A	6:29	6:29	I would suggest replacing "uncertainties become smaller" by "the range of our estimates becomes narrower" as this is really what occurs. [Michael MacCracken]	Accepted
6-399	A	6:31	6:32	This report is not being written for the paleo community, so why mention. [Thomas Karl]	Noted, will be considered
6-400	A	6:36	6:36	"modelling" - elsewhere spelt "modeling". [James Crampton]	Accepted
6-401	A	6:37	6:37	I find the sentence 'We attempt to balance...' confusing I suggest: 'We consider the contemporary understanding of paleo-climates on both large (e.g. hemispheric) and regional scales.' [Mark Siddall]	Noted, will be considered
6-402	A	6:38	6:41	This definition of rapid climate change - a transition short relative to the duration of the regime - is inconsistent with the Rahmstorf reference definition, as well as the NAS definition of abrupt climate changing as being a change rapid relative to the forcing. With the definition stated here, global warming would qualify as 'abrupt climate change'. [Andrew Lacis]	Noted, will be considered
6-403	A	6:39	6:39	involveS [James Crampton]	Accepted
6-404	A	6:40	6:41	a more accessible ref. is Alley et al, 2003, Science [Thomas Stocker]	Noted
6-405	A	6:43	26:40	There have been extensive works on Paleoclimate associated with Asian Monsoon. For example, using palaeobotanical and lithological data, Sun and Wang have provided evidence for the establishment of the East Asian monsoon around the Oligocene/Miocene boundary (Palaeogeography, Palaeoclimatology, Palaeoecology, 222, 2005). Another reference is the review of Asian Monsoon system by a working group jointly sponsored by SCOR and IMAGES (Wang el al., Quaternary Science Reviews, 24, 2005). The latter reference also covers extensive works by Tungsheng Liu (2002 Tyler Prize Laureate for Environmental Achievement for his contribution in developing ways to measure global climate patterns by studying loess) and his associates. These works need to be incorporated into contents from Section 6.2 (Paleoclimatic Methods) to Section 6.5 (The Last 2000 Years). [Jilan Su]	Accepted
6-406	A	6:43		While having such an extensive tutorial is a good idea the length may still be excessive -	Noted

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				decision needed on materail for the tarhet audience versus detail. [Bryant McAvaney]	
6-407	A	6:51	6:54	More recommendations! [Thomas Karl]	Noted
6-408	A	6:52	6:52	delete "more". [James Crampton]	Accepted
6-409	A	6:52	6:52	Delete "more" as redundant. [Michael MacCracken]	Accepted
6-410	A	6:52	6:53	I would suggest to add "G. Fischer and G. Wefer, Use of proxies in paleoceanography, 735 pp., Springer Verlag, Berlin, 1999" to the cited refs. [Michael Schulz]	Accepted
6-411	A	6:52		cite recent new and revised edition of Bradley's book. [Thomas Stocker]	Accepted
6-412	A	6:53	6:53	Thompson instead of Thomson [Eva Calvo Costa]	Accepted
6-413	A	6:53	6:53	Kucera et al., 2005 appears as in press in the reference list. This reference should be updated, see below [Eva Calvo Costa]	Noted
6-414	A	6:55	7:10	CO2 is not seriously introduced. What is extraordinary in ice cores, concerning the last 800 000 years, is to have temperature, CO2 and CH4 evolutions and to be able to see the correlation through time of these physical variables. This is, to my opinion, what has to be pointed out first. [GILLES RAMSTEIN]	Accepted
6-415	A	6:55		The reader expects that all natural forcing factors are already mentioned here (not only CO2). At least a short indication of other sections or chapters discussing other natural forcing factors should be included. [Heinz Wanner]	Noted, will be considered
6-416	A	6:57	6:57	Not sure what a time series of hypothesis is all about -- rephrase [Thomas Karl]	Accepted
6-417	A	7:0	8:	I am frustrated in reading about warming of 55 Ma to not have any explanation about causes for such warming [Joel GUIOT]	Noted, will be considered
6-418	A	7:0		insert after the 'uncertainties' "and our lack of ability to distiguish between spatially synchronous and transient events" [Robert Webb]	Noted, will be considered

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6-419	A	7:1	7:10	The authors point out that the use of multiple proxies in cross-validation of the estimates. Multiple proxies also provide information on different climatic variables, frequently on differing temporal and spatial scales. This potentially provides a richer and more encompassing view of climatic change that would be available from a single proxy. [Robert Thompson]	Accepted
6-420	A	7:3	7:3	I am concerned by the frequent use of the word "verified", such as here, which implies much higher degree of certainty than is appropriate. Especially the "cross-verification" is misplaced. [Jochem Marotzke]	Accepted
6-421	A	7:6		800,000 -> 650,000 [Thomas Stocker]	Accepted
6-422	A	7:7	7:7	I would suggest changing "measured" to "determined" [Michael MacCracken]	Noted, will be considered
6-423	A	7:9	7:9	e.g.' or 'i.e.'? [Mark Siddall]	Noted
6-424	A	7:12	10:14	A box is usually a compact text/figure unit not longer than a page. Box 6.1 takes several pages of text including two figures. Is it really a box? [Victor Brovkin]	ACCEPTED; no longer a box
6-425	A	7:12		Box 6.1. Is the length of this box compatible with the editing guidelines ? Its content is informative, I only wonder about its length. [Philippe Tulkens]	ACCEPTED; no longer a box
6-426	A	7:14	7:7	The title does not correspond to the content of Box 6.1. In this box only CO2 is mentionned. Title of Box 6.1 must be corrected in "The Pre-Quaternary - CO2 Forcing and Response" [André BERGER]	ACEEPTED: change to “Pre-Quaternary Climates”
6-427	A	7:14		Box 6.1 is poorly written in general and contains many inappropriate phrases. The whole section should be extensively revised. I list some specifics below. There are more. [Katsumi Matsumoto]	NOTED
6-428	A	7:15	8:50	It is assumed that BOX 6.1 stops at 6-8/47, not 6-10/14. Simply call it : "Before the Quaternary". Forcing and response are too ambiguous (CO2 is a response or a forcing?). [Michel Crucifix]	ACEEPTED: change to “Pre-Quaternary Climates”
6-429	A	7:16	7:16	I would think the symbol would be "Myr" rather than "myr" which to me means thousands of years ago. [Michael MacCracken]	ACCEPTED
6-430	A	7:16	7:25	Ok for the fact that 3 last million years represent a paleogeography and environmental context that is appropriate to derive, some lessons for future climate from the	TAKEN INTO ACCOUNT (will check the reference – might revise slightly)

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				understanding of these past climates; BUT it has to be said that for CO2 there is no direct measurement except for the last 800 000 years. Only model results or very indirect measurements are available. Nevertheless, if you want to say something concerning climate and the relationship between climate and CO2 for the last 500 Million years, it is necessary to be very cautious because this link is far from being straightforward! [See Wiezer et al., Nature for instance]. Veizer J., Godderis Y., Francois L.M.- (2000) Evidence for decoupling of atmospheric CO2 and global climate during the Phanerozoic eon Nature, 17, 698-701. [GILLES RAMSTEIN]	
6-431	A	7:17	7:17	Provide quantification of higher CO2 levels in deep past [Stephen McIntyre]	REJECT – is done later on
6-432	A	7:21	7:21	The phrase "I.e., beyond a million years in the past" should be in parentheses, and might even use the symbol Myr [Michael MacCracken]	NOTED
6-433	A	7:21		insert before the 'and' at the end of the line ", age control needed to identify leads and lags in the system," [Robert Webb]	REJECT (sentence becomes too long)
6-434	A	7:27	7:27	No answer is provided to question [Stephen McIntyre]	TAKEN INTO ACCOUNT – see edited text (question will now in effect be answered)
6-435	A	7:33	7:33	Paleoclimatology is not a "mature" field. There are many unanswered questions. [Stephen McIntyre]	REJECTED –comment out of place
6-436	A	7:34	7:35	Provide citation for claim [Stephen McIntyre]	ACCEPTED – see edited text (reference will be provided)
6-437	A	7:36	7:36	For clarity, change "Two periods" to "Periods" or one might think there were a total of 3 or even 4 periods. [Michael MacCracken]	ACCEPTED (remove ‘two’)
6-438	A	7:38	7:40	Provide citation for claim [Stephen McIntyre]	REJECTED (figure and associated references provide citation)
6-439	A	7:38	7:40	Consider adding the following ref.: R.M. DeConto and D. Pollard, Rapid Cenozoic glaciation of Antarctica Induced by declining atmospheric CO2, Nature 421, 245-249, 2003. [Michael Schulz]	ACCEPTED (reference will be added)
6-440	A	7:39	7:39	Either use "Myr" here or define "Ma" on line 16	ACCEPTED (changing to Myr)

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				[Michael MacCracken]	
6-441	A	7:39	7:40	ground temperature variations are not a "direct" measurement [Stephen McIntyre]	REJECTED (out of place)
6-442	A	7:40	7:40	The term Tertiary should no longer be used --> replace by Cenozoic [Michael Schulz]	NOTED (will check)
6-443	A	7:43	8:46	the paragraphs have been placed wrongly. They should be placed elsewhere. [Guoyu REN]	NOTED (probably editorial mistake – or decision concerning relative placement of Pliocene and PETM – see response to 6-444)
6-444	A	7:44	7:44	You said you were going from the oldest to the youngest. The mid-Pliocene should be afte the PETM then. [Philip Jones]	REJECTED (as the Pliocene is an equilibrium climate, and the PETM was an 'abrupt' change, the Pliocene is put first despite it being younger – a sentence noting thihas been added)
6-445	A	7:44	8:28	For me, it would be more logical to put the PETM before the Pliocene, so we keep going forward in time, as in the rest of the chapter [Eric Wolff]	REJECTED (as the Pliocene is an equilibrium climate, and the PETM was an 'abrupt' change, the Pliocene is put first despite it being younger – a sentence noting thihas been added)
6-446	A	7:44		Suggest reword title to – "What does the Mid-Pliocene record tell us?" [Brent Alloway]	NOTED
6-447	A	7:44		Delete "What does the record of" and "tell us?" [Vincent Gray]	REJECT (decision has been made to go with the questions)
6-448	A	7:44		Section. Provide summary clearly stating the discrepancy between models and paleo data. [Stephen McIntyre]	REJECTED (out of place)
6-449	A	7:45	7:47	It is unclear from Fig. 1 (BOX 6.1) and associated references whether there is really a concensus about CO2 being significantly higher than pre-industrial (you cite 360-400 ppmv) during the mid-Pliocene. This has therefore to be substantiated with appropriate references. This is important because the CO2 level conditions the interpretation to provide to SST reconstructions for that period. For example, de Garidel-Thoron et al. (Nature, 433 294-298, 2005) recently used the fact that SSTs were similar to today during that period to infer that CO2 must have been of the same order of magnitude as today (this holds for the past 1.75 Myears only). [Michel Crucifix]	ACCEPTED – see edited text (will comment on the higher CO2 levels)
6-450	A	7:45	7:45	"highest" and "smallest" compared to what?	ACCEPTED – see edited text

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				[Michael MacCracken]	
6-451	A	7:47	7:47	Citation [Stephen McIntyre]	ACCEPTED (reference will be added)
6-452	A	7:47	7:47	Temperatures are derived from GCM experiments, you should explain CO2 measurements are obtained. [GILLES RAMSTEIN]	REJECTED (CO2 reconstruction techniques are discussed above)
6-453	A	7:47	7:47	I guess "temperatures" refers to global mean --> should be clarified [Michael Schulz]	ACCEPTED (global will be added)
6-454	A	7:47		Insert "than today" after "higher" and "warmer" [Vincent Gray]	ACCEPTED (then pre-industrial will be brought forward)
6-455	A	7:47		rewrite end of sentence '… and global temperatures have been estimated to be 2 to 3 C above preindustrial." [Robert Webb]	ACCEPTED (then pre-industrial will be brought forward)
6-456	A	7:48	7:48	I would suggest changing "future" to "mid-21st century" to be a bit more definitive. [Michael MacCracken]	ACCEPTED – see edited text
6-457	A	7:48	7:48	Citation [Stephen McIntyre]	ACCEPED (will add reference)
6-458	A	7:48		Replace "is" with "might be" [Vincent Gray]	ACCEPTED– see edited text
6-459	A	7:50		Suggest reword – "The Pliocene is also recent enough for the [then] location and configuration of continents and ocean basins to be comparable with that of the present. Hence this time period is currently studied intensively both through the collection of proxy data as well as model simulations". [Brent Alloway]	ACCEPTED– see edited text
6-460	A	7:51	7:53	It's not clear what this means: "The middle Pliocene presents us with the mature state of a warmer world, essentially the resulting climate impact of a prior and continuing global warming." Do we have the Mid-Pliocene resolved well enough to know the PRISM slice was "mature" (whatever that means)? It seems to imply it was an equilibrium state? [William Howard]	REJECTED – evidence indicates it lasted sufficiently long to be an equilibrium climate
6-461	A	7:53	7:54	Back in the Pliocene, was there more than one ice sheet? I thought there was only the Antarctic Ice Sheet. [Michael MacCracken]	TAKEN INTO ACCOUNT (will check)
6-462	A	7:53	7:53	suggest 'Global sea level was...' [Mark Siddall]	ACCEPTED– see edited text
6-463	A	7:54	7:54	"where" is typo	ACCEPTED– see edited text

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				[Stephen McIntyre]	
6-464	A	7:54	7:54	A little short! “Ice cape smaller”…: Greenland grew around 3Myr ago and Antarctica about 40Myr…Before there is a long period, since the Permocarboniferous (300Myr), without ice sheets. Therefore the last million years corresponds indeed to a cold period with permanent ice sheets and “periodic” ice sheets which were indeed smaller before 800 000 years (sea level changes). [GILLES RAMSTEIN]	TAKEN INTO ACCOUNT (will check)
6-465	A	7:54		were", not "where [Eric Wolff]	ACCEPTED– see edited text
6-466	A	7:56	8:16	It seems odd to discuss stable Pliocene tropical temperatures in one paragraph and then introduce the possibility that tropical temperature may have been warmer in the next. It would be better to say that there is a consensus regarding high-latitude warming and uncertainty regarding tropical temperatures. [Anthony Broccoli]	TAKEN INTO ACCOUNT (warming will be changed to warmer, which does not imply temporal instability)
6-467	A	7:56	8:16	On line 56 define time period (3.5 ± ?). With the age control available the reconstrcutions based on paleoclimate data are more art than science. At best the mid Pliocene data being used to generate the temperature estimates are from a broad period representing ±250,000 years and not all that meaningful to compare with climate model experiments forced with a single set of boundary conditions. If we attempted to make a similar analysis for the last 500Kyr of the Pleistocene and then compare it to a climate model simulation with forcings representing the last 500Kyr, it too would agree in some places, disagree in others, and need to invoke remarkable circulation changes . How well do we really know the mid Pliocene climate. I suspect we already have more data in the Southern Hemisphere for the time of the Medieval Warm Period (page 28 lines 51-54) than globally for the Pliocene. [Robert Webb]	TAKEN INTO ACCOUNT (comment about dating range will be added)
6-468	A	8:1	8:2	Given how temperature changes in the vertical, I would suggest changing "latitudinal temperature gradient" to "near-surface latitudinal temperature gradient." What happens aloft might be quite different. [Michael MacCracken]	ACCEPTED– see edited text
6-469	A	8:4	8:4	warming relative to today of 10-20 C … [André BERGER]	TAKEN INTO ACCOUNT – (presumably the chapter will have a general statement about what all the temperature changes are being compared to...)
6-470	A	8:5	8:5	Line 5 is confusing: Chandler is to my knowledge, a modeller and not a proxy – data person!	REJECTED (reference also provides data compendium, for model

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				[GILLES RAMSTEIN]	comparisons)
6-471	A	8:10	8:11	"The lack of tropical warming results…" : the style must be more linear. E.g. : "Microfaunal evidence suggest that mid-Pliocene SSTs were similar to today. Cf. also my earlier comment on the interpretation to give the SST / CO2 relationship (is CO2 at that period so well known that one can speak about weak sensitivity ?). Finally, Greenland was not an ice sheet, which probably explains part of the larger sensitivity of high latitudes. No need to resort to big variations in ocean heat transport. [Michel Crucifix]	REJECTED (edited text will note that the high latitude warmth was widespread, discrepancy occurs in models without Greenland Ice, and occurred in the SH as well)
6-472	A	8:10	8:11	Interpretation of the lack of tropical warming results ... [Donald Forbes]	ACCEPTED– see edited text
6-473	A	8:10	8:12	Seriously misleading and confusing. The tropical LGM SST problem has no real link with Pleistocene SST. As it is written, it is very confusing. The LGM SST problem in the tropics has been intensively discussed from the data point of view [from CLIMAP 76 to CLIMAP 81 and most recently MARGO [Kucera, 2005] through different proxies and by a modelling exercise within PMIP [see S.Pinot et al. 1999 for PMIP1 review modelling and Farrera (1999) for continental Data]. The situation is very different for the Pleistocene, where tropical SSTs were not the focus of such a debate. The recent paper from Haywood, which is the first AOGCM result showing that higher CO2 should lead to 1.5 C warmer tropical SST in contrast with data is very new and raises the issue of measuring higher tropical SST than PD in a context of no analogue. Kucera M., Rosell-Melé A., Schneider R., Waelbroeck C., Weinelt M., 2005. Multiporxy approach for the reconstruction of the glacial ocean surace (MARGO). Quat. Sci. Rev. 24, 813-819] Pinot S., Ramstein G., Harrison S.P., Prentice I.C., Guiot J., Stute M., Joussaume S., 1999. Tropical paleoclimates at the Last Glacial Maximum: comparison of Paleoclimate Modeling Intercomparison Project (PMIP) simulations and paleodata. Clim. Dyn., 15, 857-874. Farrera I. et al,1999. Tropical climates at the Last Glacial Maximum: a new synthesis of terrestrial palaeoclimate data. I. Vegetation, lake-levels and geochemistry. Clim. Dyn. 15, 823-856 [GILLES RAMSTEIN]	REJECTED (modern analogue technique is similar in the two time periods, as are the isotope reconstructions)
6-474	A	8:13	8:14	Box 6.1: "GCM reconstructions" is inappropriate; models do not really reconstruct but simulate. Also, need to be more specific about what a GCM is. Coupled? [Katsumi Matsumoto]	ACCEPTED– see edited text
6-475	A	8:14	8:14	Which previous example?	ACCEPTED– see edited text (as in

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				[James Crampton]	Haywood, op cit)
6-476	A	8:14	8:16	text "as in the previous example, the climate models … Rind and Chandler, 91)" is not necessary and redondant with the beginning of the following paragraph [Sylvie JOUSSAUME]	REJECTED (different point is being made, relating tropics to high latitudes)
6-477	A	8:14	8:16	It seems to me that mention here should also be made of the proposal by Kerry Emanuel (as I recall in a 1999 paper) about how increased ocean transport could possibly be stimulated by an increase in the number of tropical cyclones, which would do this by more vigorously mixing subtropical surface waters with the colder water below, and so allowing a strengthening of the meridional overturning circulation. The net consequences, if this is mechanism is what limits the increase in tropical temperature, is that human-induced warming could greatly enhance tropical cyclone activity (so bad for those in low latitudes) while pumping more heat to higher latitudes (increasing the warming and glacial melting there)--so a really dire outcome. Right now, unproven, but interesting hypothesis that deserves mention to balance the mention of those saying climate change might be small or even beneficial. [Michael MacCracken]	NOTED
6-478	A	8:14	8:14	"GCM reconstructions" seems odd to me. May be this could be used when a data assimilation procedure is used (which is not the case here). "GCM simulations" seems more correct. [Didier PAILLARD]	ACCEPTED– see edited text
6-479	A	8:24	8:26	It is misleading to make the false comparison of equilibrium climate for the Pliocene with transient climate for the 21st century. In fact there is not a conflict because models exist for which the deep water formation recovers to a higher level after the initial decrease in the 21st century (eg Stouffer, R. J., and S. Manabe, Equilibrium Response of Thermohaine Circulation to large Changes in Atmospheric CO2 concentration. Climate Dynamics, (20): 759-773, 2003.; J. C. Hargreaves and J. D. Annan. Using ensemble prediction methods to examine regional climate variation under global warming scenarios. Ocean Modelling Vol 11 Nos 1-2 p174-192). [Julia Hargreaves]	ACCEPTED– see edited text (words ‘at least’ to be added before the ‘transient climate)
6-480	A	8:24	8:24	Change "thermohaline" to "thermohaline circulation" [Michael MacCracken]	ACCEPTED– see edited text
6-481	A	8:27	8:28	The statement in the current draft is correct, but should be expanded to indicate the issues involved. There are at least three: (1) The inability of climate models to simulate the conditions of the mid-Pliocene calls into question their ability to accurately project future high CO2 climate conditions. (2) One of the more common projections of future climate is more persistant El Nino conditions. However, if high CO2 leads to warmer conditions at high latitudes, but no change in tropical SST, there should be no change in ENSO. (3)	TAKEN INTO ACCOUNT (consider adding a final paragraph being more explicit about this implications)

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				A reduction in the latitudinal temperature gradient would have significant impacts on the climate system; what are they? While the authors may not have answers to the questions that the paleoclimatic information raises, they should be willing to raise the questions for future consideration. [Lenny Bernstein]	
6-482	A	8:27	8:27	"this time period" - specify mid-Pliocene. [James Crampton]	REJECTED (redundant)
6-483	A	8:27	8:28	The conclusion raises at least three questions: (1) How accurate will projections of climate be in future high CO2 conditions if climate models cannot reproduce the climate conditions of the high CO2 mid-Pliocene? (2) How accurate are more persistant El Nino conditions if there was no warming of the tropical Pacific in the high CO2 mid-Pliocene? (3) What are the climate impacts of a reduced latitudinal temperature gradient? If the authors do not have answers for these questions they should still raise them for future consideration. [Jeffrey Kueter]	TAKEN INTO ACCOUNT (consider adding a final paragraph being more explicit about this implications)
6-484	A	8:30	8:46	It would be useful to include one of the estimates of the magnitude of carbon release during the PETM (Dickens et al., 1997) in gigatons C, for comparison to the magnitude of the anthropogenic carbon input. [William Howard]	ACCEPTED– see edited text (good suggestion)
6-485	A	8:30	8:46	It would be more logical to re-order the treatment of deep time periods. First would be general information about the Palaeocene and early Eocene (as necessary background to the PETM), then the PETM, then the Pliocene. [Iain Colin Prentice]	REJECTEDsince the section is of limited length, general information about the Paleocene is not included to any extent – therefore the first equilibrium climate discussed is the Pliocene. The PETM then represents a more 'transient response', and as in the other sections, the equilibrium climates are put before the transient response).
6-486	A	8:30	8:30	PETM should be introduced very differently. For instance, as the famous paper of Zachos (2001) did, which demonstrated that since 65 My, the stable isotopes O18 and C13 are paced by Milankovitch (high frequency) and plate tectonics (low frequency), but there are also rapid events with drastic changes in O18 and especially C13 records. This very peculiar event because of the large amplitude of the ?C13 (-2.5‰) whose duration is similar to the rapid anthropogenic perturbation we are currently producing, may be explained by a release of methane hydrate is that are characterised by a -60 to -80‰ ?C13 and that may produces a large warming. Such a scenario has been successfully simulated by G. Schmid (2003).	NOTED (might consider additional references, but there are already a lot, including Zachos refs.)

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				Zachos, J. C. , Pagani, M., Sloan, L., Thomas, E., and Billups, K., 2001, Trends, rhythms, and aberrations in global climate 65 Ma to Present. Science, v. 292, p.686 Gavin A. Schmidt and Drew T. Shindell, Atmospheric composition, radiative forcing, and climate change as a consequence of a massive methane release from gas hydrates, Paleoocanography, vol. 18, NO. 1, 1004, 2003. [GILLES RAMSTEIN]	
6-487	A	8:31		Box 6.1: "spectacular global warming" is inappropriate. [Katsumi Matsumoto]	TAKEN INTO ACCOUNT ((better word should probably be used)
6-488	A	8:33	8:33	Add ")" after "PETM". [Martin Stendel]	ACCEPTED– see edited text
6-489	A	8:33		close bracket after PETM [Brent Alloway]	ACCEPTED– see edited text
6-490	A	8:35	8:35	The part in () is a bit confusing. I assume that 10 kyr is the time over which warming occurred, 100 kyr the cooling, but it doesn't seem quite clear. [Jonathan Gregory]	ACCEPTED– see edited text
6-491	A	8:39	8:40	Box 6.1: "The mass of carbon was sufficiently large" doesn't sound quite right… [Katsumi Matsumoto]	NOTED (possibly will be altered)
6-492	A	8:40	8:46	Conspicuously absent here is a reference to Schmidt and Shindell (2003) [Schmidt, G.A., and D.T. Shindell 2003. Atmospheric composition, radiative forcing, and climate change as a consequence of a massive methane release from gas hydrates. Paleoceanography 18, no. 1, 1004, doi:10.1029/2002PA000757.] who provide evidence that methane may have played a significant role here. [Michael Mann]	NOTED
6-493	A	8:43	8:43	Remove extra brackets around references. [James Crampton]	ACCEPTED– see edited text
6-494	A	8:45	8:46	I believe it would be worth emphasizing here other possible interests in the PETM than simply the "climate sensitivity" which is here uncertain. For instance the long time-scale of the perturbation and the interactions with the carbonate systems (with a cross-ref. to parag. 7.3.2.2.3 - D. Archer) or the effects on ecosystems: this is not simply a climatic event but a geological transition. [Didier PAILLARD]	TAKEN INTO ACCOUNT (will investigate)
6-495	A	8:49	8:49	A title is needed here: this is no more PETM. [Didier PAILLARD]	Accepted, editing error
6-496	A	8:49	10:14	is this section really part of box 6.1? There is no relation between the two paragraphs on	Accepted, editing error

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				page 8 around line 48. I wonder if box 6.1 ends on line 49, p8? If this is the case, hen a title is missing. If it is not the case, the whole section is not in the right box because it is not related o pre-quaternary climate [Elsa CORTIJO]	
6-497	A	8:49		Surely this should be the end of box 1, which you have placed 2 pages later [Eric Wolff]	Accepted, editing error
6-498	A	8:50	8:57	This paragraph seems out of place here. [Anthony Broccoli]	Accepted, editing error
6-499	A	8:50	8:57	This paragraph seems to be out-of-place, or to require its own sub-heading? [James Crampton]	Accepted, editing error
6-500	A	8:50	8:51	"astronomically driven insolation changes". Given that this is the first time in the chapter that this notion appears, it should be more expanded. I.e. : there are changes in the orbit defined by changes in eccentricity, obliquity and climatic precession; these induce variations in the seasonal and latitudinal distributions of insolation at the top of the atmosphere; these variations are very well known for the last and future 3 Myr (Berger et al., JAS, 1978; Berger and Loutre, Q.S.R., (10) 297-317 (1991) and even further back in time if one is primarily interested in the frequency domain (J. Laskar, Phil. Trans. R. Soc. Lond. A (357) 1735-1759, 1999) [Michel Crucifix]	Taken into account, new box on orbital forcing will be inserted
6-501	A	8:50	8:57	Box 6.1: "forcing series" also doesn't sound right [Katsumi Matsumoto]	Accepted
6-502	A	8:54		Suggest insert – “Records of volcanic eruptions during the late Quaternary in Indonesia, the Mediterranean, Japan and more recently in New Zealand, have suggested the close relationship between orbital climatic periodicities and eruption recurrence intervals (Rampino and Self 1992; Carter et al. 2004).” Rampino, M.R. and Self, S. 1992. Volcanic winter and accelerated glaciation following the Toba supereruption. Nature 359, 50-52. Carter, L.C., Alloway, B.V., Shane, P.A., and Westgate, J.A. 2004. Late Cenozoic major rhyolitic eruptions and dispersal – Deep ocean records from off New Zealand. New Zealand Journal of Geology & Geophysics 47, 481-500. [Brent Alloway]	Rejected, not appropriate for chapter
6-503	A	8:56	8:57	delete lines 56 and 57 or clairfy…the reference to ice cores suggests the interval of time being discussed the last few hundred thousand years -- for this interval solar forcing is the best constrained forcing. If you're talking about the PETM, it's not so well know... [Steven Clemens]	Noted
6-504	A	8:56	8:57	Statement that solar variability 'does' play a role is inconsistent with the rest of this	Accepted

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				chapter as well as the discussion in chapter 2. It would be more appropriate to say 'might' play a role. [Andrew Lacis]	
6-505	A	9:0	9:	A key question is what do we need chronology for? For instance, the relative chronology of sea level, temperature and CO2 changes is different for glacial inception and glacial melting. It is very important to show that we get this knowledge from ice and marine cores, and this is why paleoclimate is very useful. For absolute chronology the paper is OK. [GILLES RAMSTEIN]	Accepted
6-506	A	9:0	10:	Page 6.9 et beginning of page 6.10 do not appear to belong to Box 6.1 devoted to Pre-Quaternary, but rather to 6.2.2. As presented, this box is difficult to understand [Joel GUIOT]	Accepted, editing error
6-507	A	9:2	9:30	Suggestion to reformulate title (if it necessarily has to be formulated as a question) : "What is the uncertainty on the dating of palaeoclimatic records". Make clear that age models may be constructed on absolute indicators (isotopes, but specific difficulties like reservoir age), varves (high precision but may be uncertainties on the "zero" or absolute time), by reference to a physical model (e.g. ice cores, but uncertainties on the physics) or by stratigraphic arguments (correlation with other records — wiggle matching — or astronomical forcing, but danger of circular arguments). Beyond 20,000 years, few really independent datation methods. Many rely indirectly either on a correlation with GRIP or GISP (e. g, Voelker et al., Radiocarbon (40) 517-534 (1998)) or on the U/Th calibration (Bard et al., Nature 1990). Therefore, important to improve varved chronologies of Greenland ice cores (J. Southon, Radiocarbon 46 (3) 1239-1259 (2004). This is a key to understanding the temporal structure of climatic change in the past, thus climate mechanisms. [Michel Crucifix]	Noted, will be considerered
6-508	A	9:2	9:2	Change time control to dating methods [Thomas Karl]	Accepted
6-509	A	9:5	9:8	Probably, some of the most important sentences of the whole chapter. And they are somewhere difficult to be found! [Paolo Cherubini]	Noted
6-510	A	9:5		I agree that tree-ring records are best, but for which time period is that true (e.g. last centuries, millennia, etc.)? In addition, it has to be mentioned that tree-rings offer the estimation of excellent reconstructions mainly for summer temperatures but not in any case for winter, or for other important state variables like air pressure. If the important rule of the documentary data for climate reconstruction during the last centuries is not	Accepted, will modify text

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				mentioned in this context, the two subsections on the pages 6-9 are implausible and must be criticized (see e.g.: Brázdil, R., Pfister, C., Wanner, H., von Storch, H., and Luterbacher, J., 2005: Historical climatology in Europe – The State of the Art, Climatic Change, 70, 363 - 430. DOI: 10.1007/s10584-005-5924-1). Note that many observers (monks, pastures, professors) in the preinstrumental period observed up to 25 parameters like wind direction and speed, form and amount of precipitation, cloud distribution, phenological phenomena, etc., and that with daily or subdaily resolution! [Heinz Wanner]	
6-511	A	9:8		suggest add line to the effect that "marine and terrestrial paleoclimate records are disparate and poorly correlated". [Brent Alloway]	Taken into account, will rewrite text
6-512	A	9:8	:10	Suggest insert after paragraph 1 - "The routine detection and identification of tephra, both visible and invisible (cryptotephra) forms, has considerable potential to enhance more precise correlations between marine, ice-core and terrestrial records. The occurrence of inter-regional to globally distributed tephra holds the key to testing the reliability of high precision correlations between sequences and current theories about the degree of synchroneity of climate change at regional to global scales." [Brent Alloway]	Noted, will be considered, but there are space limitations
6-513	A	9:12	9:13	This sentence reads like this chapter is written for college students not policy makers [Thomas Karl]	Noted
6-514	A	9:12		not sure the books are still cited above [Robert Webb]	Noted
6-515	A	9:24	9:30	This paragraph is not very useful. The information is not specific enough. Either typical numbers for different methods are to be given or the paragraph can be deleted. [Thomas Blunier]	Accepted, will rewrite
6-516	A	9:28	9:30	Box 6.1: "larger" and "more accurate" than what? [Katsumi Matsumoto]	Accepted
6-517	A	9:28	9:29	You have just said that other methods have errors of a few percent, and here you say that radiometric age errors are "somewhat larger". This sewems incorrect to me, people would normally quote errors of only a few percent at 40 kyr for example. [Eric Wolff]	Accepted
6-518	A	9:30	9:30	"With proper care, current methodologies allow more accurate age models." … more accurate than what? [James Crampton]	Accepted, will rewrite
6-519	A	9:30		The sentence « With proper care, current methodologies allow more accurate age models	Accepted, will rewrite

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				» is not particularly informative. [Robert Thompson]	
6-520	A	9:31		Insert 2 paragraphs summarizing the age control and temporal resolution for the LIG and the preQuaternary [Robert Webb]	Noted will be considered in view of space constraints
6-521	A	9:32	9:32	There is no mention of documentary data in this section. This is paleo data to my mind. [Philip Jones]	Accepted
6-522	A	9:32	9:32	title should be "..past climates" and not "past climate dynamics" [Michael Mann]	Accepted
6-523	A	9:32	10:12	Comment in general on section 'How good are the methods used to reconstruct past climate dynamics?' Periglacial and permafrost remnants may be very helpful for climate reconstructins. Suggestion to add near the end of line 51: Sedimentary deformations caused by frost or thaw processes enable quantitative reconstructing the mean annual temperatures of cold environments (e.g. ice-wedge casts, cryoturbations)--ref. Huijzer & Vandenberghe (op. cit.), Vandenberghe, J. & Pissart, A. 1993 Permafrost changes in Europe during the last glacial. Permafrost and Periglacial Processes 4, 121-135. [Jef Vandenberghe]	Noted, to be considered within space limitations
6-524	A	9:33	9:33	Why be so defensive? [Thomas Karl]	Accepted, will modify text
6-525	A	9:33	9:43	These statements about the quality of paleoclimate reconstructions, besides being debatable, appear defensive and inappropriate. Better to let the rest of the large paragraph (lines 43 on) speak for themselves. [Andrew Lacis]	Accepted, will modify text
6-526	A	9:33	9:37	Box 6.1: "mature field" is inappropriate; and this paragraph repeats earlier statements. [Katsumi Matsumoto]	Rejected, authors believe statement is correct
6-527	A	9:39	9:43	There also is a written document such as a diary. A famous example is a record of "Omiwatari" (the divinity's pathway) in Lake Suwa in central Japan, which is an indicator of winter temperature and is recorded since 1443. Ishiguro, N., 2001: Homogeneity of the Omiwatari records of Lake Suwa as the database for winter temperature estimation. Chirigaku Hyoron (Geographical Review of Japan), 74, 415-423 (English sum.). [Akio Kitoh]	Accepted, see above
6-528	A	9:39	9:40	I think GHG measured in ice cores could be added to the list od "direct measurements" [Michael Schulz]	Accepted
6-529	A	9:39	9:41	Trace gases in air bubbles could be included as one of the direct measurements as opposed to proxy measurements.	Accepted

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				[Eric Wolff]	
6-530	A	9:39	10:6	Claims in that paragraph tend to be overstated ("highly quantitative manner", "well-recorded", "well-calibrated", "well understood" if front of which it is difficult to justify that (p. 10 l. 2) "there are remaining issues concerning the degree to which the methods have spatial and seasonal biases". May be better to shorten the text of the paragraph and recap methods in a table. [Michel Crucifix]	Noted, will be considered in revision
6-531	A	9:39		an important example of direct measurements are the greenhouse gases from polar ice cores. Needs to be mentioned here, since this is extensively used in AR4 [Thomas Stocker]	Accepted
6-532	A	9:40		An important direct measurement is past air composition from ice core bubbles [Tas van Ommen]	Accepted
6-533	A	9:42	9:42	the term "highly quantitative and well-understood" is promotional and not justified [Stephen McIntyre]	Noted, text revision will be made
6-534	A	9:43	9:44	Corals and some plankton are also animals. I would either remove "animals" as an example of biological organisms or be more specific. [Eva Calvo Costa]	Noted
6-535	A	9:43	10:6	Nevertheless, the rest of this paragraph does not give an accurate impression of the uncertainties associated with paleoreconstructions. We don't know that tree rings really can help us understand long-term climate records, the relationship of pollen and plankton from sediment cores to actual climate records using transfer functions is in many cases highly contentious (as noted in this chapter), establishing actual temperature records from Mg/Ca and Sr/Ca ratios or alkenones has numerous debatable assumptions, resulting, not surprisingly, in often conflicting results, etc. The 'bone' thrown to these uncertainties at the end of this paragraph is insufficient - it is better to say that while there are significant uncertainties in each of these approaches, using multiple approaches to produce a paleo-record has the advantage of increasing confidence when they agree, and highlighting the uncertainties when they don't.. Convergence of evidence is the key, as suggested on lines 8-12, pg. 10. [Andrew Lacis]	Taken into account, text will be revised
6-536	A	9:46		Suggest amend “Trees,” to “Tree-rings”. Also mention recent advances in the development of robust temperature-transfer-functions based from beetle and chironomid analysis. [Brent Alloway]	Noted
6-537	A	9:48	9:49	"comprehensive calibration with temporally overlapping instrumental data" - this is promotional. Many tree ring networks are not so calibrated. Statement of limitations and uncertainties needs to be made.	Noted, some new text on this will be made in text on last 200 years

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				[Stephen McIntyre]	
6-538	A	9:49	9:57	Limitations and uncertainties need to be stated, Promotional as it stands. [Stephen McIntyre]	Noted, text will be rewritten
6-539	A	9:49	9:49	....and plankton from lake sediment cores can... [Atle Nesje]	Noted
6-540	A	9:49	9:50	I would prefer not to see pollen cited for paleothermometry. It is a widely used but generally poor tool for this purpose. Why not use 'micro-fossils' instead. [James Shulmeister]	Rejected, pollen may work well if study deals with long term changes
6-541	A	9:49	9:51	The concentration of atmospheric CO2 can have a strong influence on the moisture requirements of plants, particularly in arid and semiarid regions. For studies involving periods of substantially lower (e.g. LGM) or higher (e.g. Pliocene) atmospheric CO2, modern calibration studies may not provide an adequate basis for paleoclimatic reconstructions. [Robert Thompson]	Noted
6-542	A	9:53	9:56	Box 6.1: "O-isotope" should be spelled out as "oxygen isotope"; "infer past temperature and salinity" should be rephrased, because it sounds like d18O can infer both of them individually. [Katsumi Matsumoto]	Accepted
6-543	A	9:56		mention also N and Ar isotopes in the gases of polar ice cores. [Thomas Stocker]	Acccepted
6-544	A	10:0	10:	Two major ideas are missing at the beginning 1- Models have a large diversity from coupled GCMs to conceptual models This chapter is more focused on sophisticated GCMs coupling ocean atmosphere and biosphere because we are interested in the variability on time scales of one year to millennia, and regional pattern changes. Therefore these models are appropriate. 2- As for the data, intercomparaison of different models (GCM) is completely necessary to assess, their results with some confidence, (see PMIP for LGM and Mid Holocene, or Jost et al. (2005) for the use of different high resolution GCMs) These two points have to be clarified at the very beginning. Jost A., Lunt D., Kageyama M., Abe-Ouchi A., Peyron O., Valdes P.J., Ramstein G., 2005. High-resolution simulations of the last glacial maximum climate over Europe: a solution to discrepancies with continental palaeoclimatic reconstructions? Clim. Dyn. DOI 10.1007/500382-005-0009-4 [GILLES RAMSTEIN]	Taken into account, text will be revised
6-545	A	10:0	10:	Bottom of the page10 The coupling with ice sheets is not correctly introduced.	Noted

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				For time scale of 100 years AOV GCMs are useful tools. If we want to lengthen this time scale, it is absolutely necessary to include an ice sheet model in modelling the Earth system. This has to be clarified. It is drastically important because glacial variability is due to ancient ice sheets variability (Fennoscandian and Laurentide). Therefore future ice sheet instability, may lead, at this time scale, to large climate instabilities. [GILLES RAMSTEIN]	
6-546	A	10:1	10:12	This text seems very defensive and unnecessary. [Robert Thompson]	Noted
6-547	A	10:4	10:4	"have the potential to provide" - merely using multiproxy is no guarantee of a more rigorous estimate. [Stephen McIntyre]	Accepted
6-548	A	10:14		Box 1 should have ended much earlier [Eric Wolff]	Accepted
6-549	A	10:16		PMIP and PMIP2 can be introduced in this sub-section. [Akio Kitoh]	Noted. To save space and duplication, PMIP will be introduced later where its results are discussed.
6-550	A	10:18	10:28	The strategy of citation is unclear : just one is given , why ? Example line 26 could cite results from Berger et al on the glacial cycles. [Sylvie JOUSSAUME]	Taken into account. The strategy is to cite a selection of key papers, as space does not allow us to be comprehensive.
6-551	A	10:19	10:19	"time evolution" … doesn't make sense. [James Crampton]	Accepted.
6-552	A	10:20	10:22	Remove the word "physical" before "hypothesis. It is unnecessary and one can quibble with it. The "Khodri et al., 2001" reference is not as appropriate as, say, Milankovitch's original paper or Hays et al. 1976. Replace. [Katsumi Matsumoto]	Noted, but we don't see the problem with the word physical. Accepted, ref. changed.
6-553	A	10:20	10:21	The definition of forcing and feedback here makes the ice sheets a feedback, not a "forcing". [Stephen McIntyre]	Forcing/feedback now properly discussed in revised text.
6-554	A	10:21	10:21	The spelling of Milutin Milankovitch name in Latin letters is “Milankovitch” (not “Milankovich”). Under this name he published his major works. [Andrey Ganopolski]	Accepted.
6-555	A	10:22	10:22	"Models allow us to link cause and effect…" I think this is too bold! Models allow us to test and rank competing hypotheses of relationships. [James Crampton]	Taken into account - see 557

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6-556	A	10:22	10:22	Quote "Milankovitch" and not "Khodri" as a reference for the Milankovitch theory. Khodri et al. is appropriately referred p. 63 l. 27. [Michel Crucifix]	Accepted
6-557	A	10:22	10:22	I would suggest changing the text to read "Models allow us to investigate cause-effect linkages in past …"--the present wording seems a bit too definitive. [Michael MacCracken]	Accepted
6-558	A	10:22	10:25	citations [Stephen McIntyre]	Noted. Number of citations limited, hence not many are given in this general introduction.
6-559	A	10:24	10:26	Poor example, since with or without models, we do not understand why CO2 and CH4 have varied from glacial to interglacial times! [Andrew Lacis]	Accepted. Replaced "understood" with "explored"
6-560	A	10:26	10:28	I would suggest starting this sentence with "Developing a quantitative understanding …" [Michael MacCracken]	Accepted
6-561	A	10:27	10:28	makes no sense to me whatsoever - delete entirely [Steven Clemens]	Rejected. It makes sense to us and to the other reviewers.
6-562	A	10:31		change "the response" to 'the spacial and temporal signature of the response" [Robert Webb]	Rejected. Unneccessary jargon.
6-563	A	10:32	10:34	In this sentence, on line 32, I would change it to "forcings and responses cover a much larger range, but …" and on line 34 I would change "signal" to "response"--and I would generally try not to keep using different words for the same thing--so stick with "response" and don't use "signal" [Michael MacCracken]	Accepted
6-564	A	10:35	10:36	I would suggest changing this to read "For example, good performance in simulating the present climate is not a conclusive test that the climate sensitivity is being realistically represented; as one step in testing this, simulation of a climate with a very different CO2 level can be helpful." [Michael MacCracken]	Taken into account - text partly changed
6-565	A	10:36	10:36	Change 'must' to 'can' – climate sensitivity can theoretically be assessed independently of GHG forcings. [Gavin Schmidt]	Accepted
6-566	A	10:39	10:39	…test-bed against which models can be tested. [Steven Clemens]	Accepted
6-567	A	10:39	10:39	The independent test-bed of different climate states only increases confidence if the simulation turns out well. It can also have the opposite effect. Perhaps you should say, "... independent test-bed that can increase the confidence in the models." I think that these	Taken into account, see 566

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				other climate states have actually been more useful for increasing understanding of model deficiencies than increasing confidence. [Julia Hargreaves]	
6-568	A	10:39	10:39	That 'can' increase confidence in the models - it doesn't necessarily have to. [Andrew Lacis]	Taken into account, see 566
6-569	A	10:42	10:42	"forcing" is an ambiguous term. On the one hand, it should also include ice sheets and sea-level. On the other hand, these "forcings", as well as GHGs, may be viewed as part of the response. It may therefore be more appropriate to use the phrase "elements of the climate configuration imposed to the model (total solar irradiance, orbital parameters, greenhouse gases concentrations, ice sheets extent and topography, land-surface properties)". [Michel Crucifix]	Forcing/feedback now properly discussed in revised text.
6-570	A	10:42	10:42	I would reword to say "The only differences between the two model simulations are the external forcing and, for the deep past (tens of millions of years ago), …"--while I think this would be clearer wording, I also wonder if different initial conditions are not imposed for the land and/or ocean conditions (like ocean temperatures) in order to avoid start-up problems. [Michael MacCracken]	Taken into account. Text partly changed. Concerning initial conditions, most of these experiments run climate into equilibrium
6-571	A	10:44	10:46	Claussen et al's reference could be remove if the other two types of models (simple conceptual models and coupled general circulation models) are not backed with a reference either. [Eva Calvo Costa]	Taken into account. The cited ref discusses the idea of a model spectrum and was shifted to a more appropriate place in the sentence
6-572	A	10:44	10:44	In order not to introduce another new term, change "spectrum" to "hierarchy"--and change "is" to "has been" [Michael MacCracken]	Noted. Cross-chapter meeting decided to use "spectrum", not "hierarchy", throughout.
6-573	A	10:44	10:44	Another aspect (apart from difference in forcing) - and it is a negative - is that it is uncommon for exactly the same model to be used for palaeoclimate modelling as for projections. (There still is no 'seamless web' of models from NWP to climate and palaeo) [Bryant McAvaney]	Noted. But almost all models used in paleoclimate have also been used for futurer climate - this applies to EMICS as well as GCM's, where often the older, now cheaper versions are used in paleoclimate studies to allow for the longer time scales.
6-574	A	10:45	10:46	I am confused as to why there is only one reference here given there are three different types of models mentioned--either do a fuller list, or have none of types referenced. [Michael MacCracken]	Taken into account. The cited ref discusses the idea of a model spectrum and was shifted to a more appropriate place in the sentence
6-575	A	10:47	10:47	An additional phrase is needed, so it says "a limiting factor in models where processes are	Noted. We feel this is clear without the

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				relatively realistically represented rather than heavily parameterized," [Michael MacCracken]	additional words, and space is at a premium
6-576	A	10:48	10:49	I think we need to be careful that we are talking about model simulations changing, and not having models tailored for particular conditions. So, change wording to be "standard in models that are used to simulate the present climate,"… Also, the word "or" should, it would seem be eliminated in two spots later in this sentence. [Michael MacCracken]	Accepted
6-577	A	10:51	10:52	Better wording would be "Representations of vegetation and ecosystems are increasingly being included" as we really do not want to confuse the reader by talking about multiple models of this type as well of various complexity. [Michael MacCracken]	Accepted
6-578	A	10:51	10:51	Add Roche et al. which is more recent important result concerning O18 direct simulation. Roche D., Paillard D., Cortijo E., 2004. Constraints on the duration and freshwater release of Heinrich event 4 through isotope modelling. Nature 432, 379-382. [GILLES RAMSTEIN]	Accepted
6-579	A	10:51	10:52	Consider: "Vegetation as well as TERRESTRIAL AND MARINE ecosystem models…" [Michael Schulz]	Accepted
6-580	A	10:53	10:57	I think it would read better if the order of the main thoughts were reversed--"Because a rich body of … nutrients, the representation of … model simulations." [Michael MacCracken]	Noted, but a matter of style where we prefer the original
6-581	A	11:0	11:	Why to limit fig.1 to 450 Ka while in the text, it is said that ice cores cover 800 ka ? [Joel GUIOT]	Accepted
6-582	A	11:0		The temperature history of Antarctica and the North Atlantic region i.E. Greenland are different. The report refers to this as "asynchronous" or "out of phase". These descriptions have caused a fair amount of confusion in the past and should therefore be omitted. I suggest describing the records instead. [Thomas Blunier]	Taken into account
6-583	A	11:0		figure 6.1: this figure is not really recent, why the authors did not choose to put the EPICA-DC record? Moreover, the tet refer to deep-sea, continental sediments and ice cores and the figure shows only ice core. I am sure a deep-sea sediment record can be added to the figure. [Elsa CORTIJO]	Acccepted
6-584	A	11:1	20:22	The section on Glacial-Interglacial Variability and Dynamics seem disproportionately long relative the rest of the chapter and the lower relevance of glacial-interglacial variability and dynamics in the overall IPCC assessment that is overall focusing on what	Rejected, the weight is about right.

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				we know as information to help guide policy making to address future climate change. Fascinating material but needs a critical editorial assessment of why this information should be in the IPCC FAR. I would suggest trimming the section to focus on the subset of topics that are the basis for and thus an expansion of the information presented to answer questions 6.1 and 6.2. [Robert Webb]	
6-585	A	11:5	11:6	Figure 6.1 represents the last 450,000 years as recorded in Vostok and not the last 800,000 years as stated in this first sentence. If there is any reason not to plot the EPICA records (Nature 429, 623-628) in Figure 6.1 then, it would be more appropriate to call Figure 6.1 in the next sentence when referring only to the last 500,00 years. [Eva Calvo Costa]	Accepted
6-586	A	11:5	11:11	On the continents the last 500 ky are also well documented (Cheddadi et al 2005). Analysis of the climate and vegetation changes during the interglacial periods of Velay sequence (France) reveals comparable features and identical major vegetation successions, even if some IG (MIS 11.3) are less similar to Holocene than the following ones. Amplitude between an IG and a GM is usually about 12 C (annual). [Joel GUIOT]	Taken into account, will insert into rrewritten section. Dominique
6-587	A	11:5	11:11	Cheddadi, R., et al., 2005. Similarity of vegetation dynamics during interglacial periods. PNAS 2005 102: 13939-13943. [Joel GUIOT]	See above
6-588	A	11:5	11:11	It might be good to note that extreme glacial conditions (such as the LGM) and interglacial conditions (such as the Holocene and Stage 5e) both represent a relatively small portion of the Quaternary. For most of this time, global environmental conditions have been in the range between these two extremes. [Robert Thompson]	Accepted.
6-589	A	11:5		Suggest reword to “Paleoclimate records from ice cores, and marine and terrestrial sediments document a sequence …” Note: Not all key records are from deep sea sediments .. some notable paleoclimate records are also retrieved from Plio-Pleistocene basin margins occurring around the world. For instance, 45 superimposed cyclothems deposited since 2.5 Ma have been recognised in Wanganui Basin, New Zealand. This succession represents the most complete, on-land, shallow marine record of late Neogene climatic and sea-level change yet described. [Brent Alloway]	Rejected, too complicated to be inserted
6-590	A	11:6	11:6	the text is refering to 800 000 years record and the associated figure shows only 450 kyr. Why?	Accepted

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				[Elsa CORTIJO]	
6-591	A	11:6		Fig 6.1 does not cover 800 kyr. Commented also on figure. [Eric Wolff]	Accepted
6-592	A	11:9	11:9	Replace this Hayse et al 76 ref with the following two refs. Imbrie, J., Berger, A., Boyle, E. A., Clemens, S. C., Duffy, A., Howard, W. R., Kukla, G., Kutzbach, J., Martinson, D. G., McIntyre, A., Mix, A. C., Molfino, B., Morley, J. J., Peterson, L. C., Pisias, N. G., Prell, W. L., Raymo, M. E., Shackleton, N. J., Toggweiler, J.R., 1993. On the structure and origin of major glaciation cycles. 2. The 100,000-year cycle. Paleoceanography, 8, 699-735. Imbrie, J., Berger, A., Boyle, E. A., Clemens, S. C., Duffy, A., Howard, W. R., Kukla, G., Kutzbach, J., Martinson, D. G., McIntyre, A., Mix, A. C., Molfino, B., Morley, J. J., Peterson, L. C., Pisias, N. G., Prell, W. L., Raymo, M. E., Shackleton, N. J., Toggweiler, J.R., 1992, On the structure and origin of major glaciation cycles, 1. Linear responses to Milankovitch forcing. Paleoceanography 7, 701-738. [Steven Clemens]	Taken into account. Valerie orbital box
6-593	A	11:11	11:11	Strictly speaking, the Holocene is not a period - it is an epoch. [James Crampton]	Accepted
6-594	A	11:11	11:11	Citation [Stephen McIntyre]	Rejected, no citation necessary
6-595	A	11:15	11:25	One need a better survey/referrencing of actual modelisations of the glacial interglacial cycles (Gallée et al 91; Tarasov and Peltier (1997); Yoshimori and Weaver 2001; Charbit et al., Quat. Sci. Rev. 21 243-265 for the deglaciation) + different means by which this is achieved (on-line vs off-line coupling). It also needs to be said what are the identified elements of non-linearity that allow the 100 kyr cycle to dominate in the response (CO2 concentration [Berger et al. Clim. Dyn. (14) 615-629, 1998; Paillard et al., EPSL 227 (3-4) 263-271 (2004), and isostasy. [Crucifix et al., Earth and Plantetary Science Letters 184 (3-4) 2001). What is meant by "primary variations" ? Note that the dominant cycle of eccentricity is 400 kyr, not 100 kyr. Cf. my suggestion of gathering all the info on orbital forcing in one box, which is easily referred to in the rest of the text. [Michel Crucifix]	Noted, will be considered in orbital forcing box.
6-596	A	11:16	11:17	I wonder how Antarctic climate records (Watanabe et al., 2003; Augustin et al., 2004) can “reconfirm Milankovitch theory”, since this theory tells us nothing about the Southern Hemisphere. [Andrey Ganopolski]	Accepted, will replace with box on orbital forcing
6-597	A	11:16	11:22	This paragraph is contradictory. How can one say that recent studies validate the astronomical hypothesis of Milankovitch on driving ice ages when the large signal is the	Rejected, issue will be dealt with in orbital box.

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				100K one, and the eccentricity forcing can't explain it (as noted in this paragraph). The comment about nonlinearity implies that we know the eccentricity was doing the forcing, so the repsonse 'must have been' non-linear. This is circular reasoning - we don't know what forced it, or how it happened, so the Milankovitch hypothesis, outside of the smaller cycles, has not yet been validated. [Andrew Lacis]	
6-598	A	11:16	11:25	It seems to me that the phrase "confirm the validity" is too strong to be used given the state of our knowledge--what we really have is mainly a correlation that has been confirmed, and we are still working to fully explain how all the various linkages work--and with the last sentence only indicating understanding for a 10 ka period rather than for all of the last 500 ka, the opening phrase seems too strong to me. [Michael MacCracken]	Rejected, issue will be dealt with in orbital box.
6-599	A	11:16		replace Augustin et al, 2004 by EPICA Community Members, 2004 (same paper) [Thomas Stocker]	Accepted
6-600	A	11:16	:22	The use of the word "implies" in the sentence 'The strong response to the 100 kyr cycle, which is associated with only weak insolation forcing, implies that the climate system reacts in a highly nonlinear manner with large positive feedbacks." underscores that we can with increasing confidence postulate the role of orbital forcing but cannot simulate the response using a fully coupled dynamical earth system model. Change on line 17, "the major role" to 'the major but not completely understood role' [Robert Webb]	Accepted, issue will be dealt with in orbital box.
6-601	A	11:20	11:21	“The strong response to the 100 kyr cycle, which is associated with only weak insolation forcing”. It is unclear which 100 kyr cycle is meant here: is it amplitude modulation of precessional signal by eccentricity (this signal is strong, not weak), or a pure impact of eccentricity on global insolation, which is, indeed, very small. [Andrey Ganopolski]	Accepted, issue will be dealt with in orbital box.
6-602	A	11:20	:22	This is a contentious statement - there is argument over whether the response is in fact to a 100k year forcing. It should be pointed out that this is a major current research question. [Melanie Fitzpatrick]	Accepted, issue will be dealt with in orbital box.
6-603	A	11:22	11:22	… Climate models (see for instance Gallée et al., 1992) … [André BERGER]	Rejected, cited elswhere
6-604	A	11:22	11:25	Which climate models “indicates that the changes from glacial to interglacial conditions … can be consistently explained…”? Some references would be very helpful. [Andrey Ganopolski]	Accepted

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6-605	A	11:22	11:25	references? [Mark Siddall]	Accepted
6-606	A	11:23	11:23	what is a deglaciation? Strictly speaking, it should be the decrease of global ice volume, nd in this case the last deglacial did not occured between 20 and 10 ka ago. If the authors are considering temperature change, the timing is more or less OK. I think the word "deglaciation" has to be defined clearly. This is a particularly crucial when considering phases leads and lags between temperature, CO2, ice volume... [Elsa CORTIJO]	Accepted
6-607	A	11:23	11:23	There is always a confusion of words when talking about glacials, interglacials or deglaciations. I would prefer these words to characterize the ice volume on Earth only, and not the temperature at some location. The deglaciation, defined by the sea-level rise, occurs mostly between 15 and 6 ka ago, not between 20 and 10 ka. This sentence is also in contradiction with other sentences later in the text (eg. page 19, line 34: deglaciation ends at 4 ka). "Global warming associated with deglaciation" could be better... [Didier PAILLARD]	Accepted, will define deglaciation.
6-608	A	11:25	11:25	Citation [Stephen McIntyre]	Accepted
6-609	A	11:25	11:25	It is seriously misleading to make people believe that orbital forcing explains the onset and melting of the ice caps. For melting, as stated here, the state of the art is much more puzzling that for the onset, just because ice sheets records have demonstrated that CO2 increases before any ?O18 benthic foraminera changes. The help of paleoclimate is there: pointing and building consistent scenario to explain CO2, sea level and temperature relation ships and it is not at all the same for inception and deglaciation! [GILLES RAMSTEIN]	Rejected, language in text is not meant to imply that deglaciation is solely explained by orbital forcing acting alone
6-610	A	11:27	11:38	see my comment below about the definition of a deglaciation: the lack of definition for "deglaciation" is a problem. [Elsa CORTIJO]	Taken into account
6-611	A	11:29	11:29	typo: deglaciation [Didier PAILLARD]	Accepted
6-612	A	11:29	11:30	the statement "starts to rise …several hundred years before CO2" may need some further explanation (reinforcement!) --> otherwise it may play into the hands of climate critics [Michael Schulz]	Taken into account, will add sentence.
6-613	A	11:29	11:29	degaciation" should read "deglaciation [Philippe Tulkens]	Accepted

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6-614	A	11:30	11:34	This sentence sounds like Bolling/Allerod-like events occurred during each of the last four deglaciations, which is not the case. [Andrey Ganopolski]	Accepted, will rewrite text. Dominique
6-615	A	11:30		change "degaciation" to 'deglaciation (~21Kyr to present) [Robert Webb]	Accepted
6-616	A	11:33		This is Petit et al., 1999. (other places as well) [Thomas Stocker]	Accepted
6-617	A	11:34	11:38	The sentence beginning « Current data . . . » ends with the line « which are out of phase between the hemispheres ». The next sentence then says « These are much more pronounced in the Northern Hemisphere ». I lost the thread on this one – what are more pronounced? [Robert Thompson]	Accepted, will clarify in rewrite.
6-618	A	11:34	11:38	This section begs for a few more lines on the "out of phase" "reversals in the warming trend". Important here is the timing established by high-res gas ties in Law Dome core which really constrain the Antarctic reversal to precede the Bolling (Morgan et al., Science, 297, p1862, 2002; Stocker, Science, 297, 1814, 2002). There is some discussion about where (north or south) the drivers of this millennial change arise (e.g. Knorr & Lohmann, Nature, 424, p532. 2003). This could alternatively be dealt with as part of abrupt changes at pages 17-18, or page 63, lines 41-50, but it needs discussing. [Tas van Ommen]	Rejected, will become too detailed to incorporate in the text.
6-619	A	11:36	11:28	"Strong reversals" needs further explanation. I take it this refers to the Younger Dryas and the Antarctic Cold Reversal. [Thomas Blunier]	Accepted, language will be modified
6-620	A	11:36	11:38	Might say here which hemisphere leads which in the timing of the strong reversals. Furthermore, the whole concept of strong reversals has not yet been introduced, so it is out of place here. [Andrew Lacis]	Accepted, language will be modified
6-621	A	11:36	11:38	State clearly the order of the phases - which hemisphere leads [Stephen McIntyre]	Taken into account, text rewritten
6-622	A	11:37	11:37	One should probably move away from the phrase "out of phase between the two hemispheres", as the temporal structure of the temperature evolution is fundamentally different. This is a consequence of the capacity of the deep ocean to store heat over periods of several centuries . For a reflexion on this subject, cf Stocker T, and Johnsen S.J. Paleoceanography 20(1) PA 1002 (2005) doi:10.1029/2004PA001108. and Crucifix M, Berger A, Paleoceanography 17 (4): Art. No. 1054 (2002) doi:10.1029/2001PA000702 [Michel Crucifix]	Taken into account, text rewritten

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6-623	A	11:37		"These" at the beginning of sentence: not clear to what "these" refers [Michael Schulz]	Taken into account, text rewritten
6-624	A	11:37		change "These" to 'These temperature reversals' [Robert Webb]	Noted
6-625	A	11:42		Replace “What caused” with “Possible causes of”. Delete question mark at end [Vincent Gray]	Rejected, style is adequate
6-626	A	11:44	11:45	The information on CO2 variations is now streched longer back in time and it is somewhat strange that the text describes findings from EPICA and the figure shows the 450 kyr Vostok data. [Per Holmlund]	Accepted
6-627	A	11:44	11:44	I would suggest changing "within" to "in the range of" [Michael MacCracken]	Accepted
6-628	A	11:44	11:54	It seems to me that what is covered here would imply that apparent abrupt changes seen in the Greenland ice core record should not, therefore, be implied to mean that the global climate would change so abruptly. This paragraph seems to make clear that there can be rather long delays (e.g., "centuries to a millennium") and so we should not be over-interpreting what the Greenland record shows. [Michael MacCracken]	The reviewer’s interpretation is correct
6-629	A	11:45	11:45	The text here also refers the reader to Figure 6.1 for the CO2 record of the last 650 kyrs. Either plot the whole CO2 record from EPICA in Figure 6.1 (when available) or change 650 for 450 kyrs. [Eva Calvo Costa]	Accepted, plots have been updated
6-630	A	11:45		the correct reference is Siegenthaler, U., T.F. Stocker, E. Monnin, D. Lüthi, J. Schwander, B. Stauffer, D. Raynaud, J.-M. Barnola, H. Fischer, V. Masson-Delmotte, and J. Jouzel, Stable carbon cycle-climate relationship during the Late Pleistocene, Science, in press, 2005. [Thomas Stocker]	Accepted, ref. updated
6-631	A	11:46	11:47	Consider citing "M. Mudelsee, The phase relations among atmospheric CO2 content, temperature and global ice volume over the past 420 ka, Quaternary Science Reviews 20, 583-589, 2001" for a quantification of the time lag. [Michael Schulz]	Accepted.
6-632	A	11:47	11:48	see #4. Suggestion: The northern temperature history over the past 120 kyr differs from the Antarctic one and also from the CO2 concentration history. [Thomas Blunier]	Sentence modified.
6-633	A	11:47	11:47	Check consistency between "650 thousand years" cited here, and "500,000 years", line 7 of the same page.	Accepted. 650,000 yr is correct.

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				[Michel Crucifix]	
6-634	A	11:47	11:47	"Often asynchronous". I would add "on millennium time scale" because this statement is not true for orbital time scales. [Andrey Ganopolski]	Accepted
6-635	A	11:48		123,000 years rather than 120,000 years [Eric Wolff]	Accepted
6-636	A	11:50	11:52	I would suggest changing the wording to "For example, different phases in the CO2 increase … can be distinguished" [Michael MacCracken]	Accepted
6-637	A	11:52	11:52	Box 6.2, Figure 1 instead of Figure Box 6.2 [Eva Calvo Costa]	References to figures updated
6-638	A	11:53	11:53	What does 'only' refer to? Is it "only a few decades" or "by about only 10 ppm"? [C.F. Michael Lewis]	Accepted. Word 'only' deleted
6-639	A	11:53	11:54	I would suggest changing the wording to "Antarctica, decreased slightly during Antarctic cooling, and increased by about 10 ppm over a few decades at the onset of …" [Michael MacCracken]	Accepted
6-640	A	11:56	11:57	quantitative and mechanistic explanation of these CO2 variations remains one of the big unsolved questions in climate research - carry forward [Stephen McIntyre]	The problem is explained in the box
6-641	A	11:57	11:57	The sentence explaining that the reason it is poorly understood is that the problem is complex should be omitted. Since we don't know what the solution is, we don't know whether the solution itself is complex. As for theoretical complexity involving all the components mentioned, the same can be said about the climate system itself; and unless we want to say climate is equally unknown, we should not blame complexity or suggest why we don't know the answer. [Andrew Lacis]	Accepted. Sentence deleted
6-642	A	12:0	13:	Very good description on the state of the art of our understanding of low CO2 value during glacial times. [GILLES RAMSTEIN]	Accepted. Thank you
6-643	A	12:1	12:1	"dynamicS". [James Crampton]	Accepted
6-644	A	12:5	12:5	Add "with which to test hypotheses" after 'proxy data'. [C.F. Michael Lewis]	Accepted
6-645	A	12:6	12:6	What are "conflicting data ? Isnt'it rather their interpretations that are inconsistent ? [Michel Crucifix]	Accepted

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6-646	A	12:8	12:24	Paragr. with unneccessary details. Could be skipped [Michael Schulz]	Partly accepted. Paragraph to be condensed
6-647	A	12:16	12:17	On both lines, replace "deep" with "deep ocean" for clarity. [Michael MacCracken]	Accepted
6-648	A	12:17	12:18	"The formation of calcium carbonates causes a higher CO2…" This is technically correct but counter-intuitive and might need a one-sentence explanation. [James Crampton]	Accepted
6-649	A	12:17	12:19	For many readers, it would help to replace "atmospheric CO2" with "the atmospheric CO2 concentration" even though it is longer. [Michael MacCracken]	Accepted
6-650	A	12:22	12:22	Since North Atlantic Deep Water is obviously also a 'deep water', the sentence should read 'where the coldest and deepest water masses'… [Andrew Lacis]	Sentence changed following 6-651
6-651	A	12:22	12:23	I would suggest changing this to read "where most of the cold deep-water masses of the world ocean are currently being formed and where large amounts of …"--"today" is too limited given we are talking about a period of climate change. [Michael MacCracken]	Accepted
6-652	A	12:26	12:26	Citation [Stephen McIntyre]	Overview citation are given on p11. line 3-4. Not further references added due to space limiation
6-653	A	12:29	12:29	Replace "material" by "material to the deep ocean" [Michael MacCracken]	Accepted
6-654	A	12:30	12:30	Give a reference for the carbonate compensation mechanism. Archer et al., Rev. Geoph, 2000 is a very good one (already cited in the chapter). [Michel Crucifix]	Accepted
6-655	A	12:30		rewrite sentence " The available sediment data does not support the proposed carbonate compensation mechanism to explain the low glacial CO2 levels' [Robert Webb]	Accepted
6-656	A	12:31	12:31	"This is in conflict with the available sediment data." What does "this" refer to? If it is "change in export ratio" it's correct. It is still not clear to what extent sediment data agree or conflict with the coral reef hypothesis (depends on how much postglacial reef growth is assuumed). [William Howard]	See 6-655
6-657	A	12:31	12:31	Replace "This" with "This mechanism, however," [Michael MacCracken]	See 6-655
6-658	A	12:34	12:34	Citation	See 6-652

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				[Stephen McIntyre]	
6-659	A	12:36	12:36	Add a phrase to read "from the atmosphere after being lofted from colder, drier continental areas," [Michael MacCracken]	Accepted
6-660	A	12:41	:43	Section # 6.2.2: "enhanced biological production and increased dustiness (iron supply) are coincident with only 20 to 50 ppm changes". "Only"?? Isn't 50 ppm half the change in CO2 concentrations on the glacial/interglacial timescale. 50% seems significant to me. This needs a reference. [Becky Alexander]	Accepted. Word 'only' deleted
6-661	A	12:42	12:43	Box 6.2 Fig. 1 does not illustrate relationship of dustiness to productivity, as stated. [James Crampton]	Accepted. Fig. Reference removed
6-662	A	12:42	:43	This sentence needs amending. It is a little misleading, given that Box 6.2 Figure 1 does not show cycles of dryness with only 20-50 ppm changes. It only shows one decreasing change - in a single cycle. [Melanie Fitzpatrick]	Accepted. Figure removed
6-663	A	12:43	12:43	The illustration in Box 6.2, Fig. 1 may be there but it is not explained. At the least, the text should explain what one is supposed to see in the figure. [Andrew Lacis]	Figure removed for space limitations
6-664	A	12:47	12:47	Citation [Stephen McIntyre]	See 6-652
6-665	A	12:52	12:52	Citation [Stephen McIntyre]	See 6-652
6-666	A	12:56	12:56	Adkinson => Adkins [Michel Crucifix]	Accepted
6-667	A	12:56	12:56	It should be "(Adkins et al., 2002)" [Andrey Ganopolski]	Accepted
6-668	A	12:56	12:56	Adkinson et al becomes Adkins et al in the bibliography list [Joel GUIOT]	Accepted
6-669	A	12:56	12:56	Should read "Adkins et al., 2002" [William Howard]	Accepted
6-670	A	12:56	12:56	I guess the reference is Adkins et al., 2002 [Didier PAILLARD]	Accepted
6-671	A	13:0	14:	Major breakthroughs and limitations are missing here. 1-Better understanding of tropics at LGM The conflict between marine record (SST – CLIMAP 81) and large continental temperature decrease in the tropics is now better understood. Thanks to MARGO (Kucera et al.2005) for marine data, Pinot et al.(1999a)	Rejected, level of detail appropriate

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				for modelling (PMIP1) and Farrera et al.(1999) for continental data. All this studies as well as the most recent PMIP 2 simulations computing SSTs with OAGCMs, show that no model depicts tropical SST increase at LGM. 2- Limitations On the other hand, the major discrepancy between LGM PMIP1 simulations and data over Western Europe [Kageyama 2001, Perron 1998] is still existing and reinforced by new SST in North Atlantic [Weinelt 1996, Pinot 1999b] It has been shown, using high resolution GCM (Jost et al.2005) that this disagreement remains and is still not understood. The THC behaviour at LGM as described by different OAGCMs is still an open question. ?C13 shows a decrease of NADW and increase of intermediate water whereas till now, many different responses are depicted from the different OAGCMs, simulation at LGM.. This is a major issue, if we want to assess the future behaviour of NADW, we have to be able to reproduce the data for ocean dynamics at LGM. Kucera M., Rosell-Melé A., Schneider R., Waelbroeck C., Weinelt M., 2005. Multiporxy approach for the reconstruction of the glacial ocean surace (MARGO). Quat. Sci. Rev. 24, 813-819] Pinot S., Ramstein G., Harrison S.P., Prentice I.C., Guiot J., Stute M., Joussaume S., 1999a. Tropical paleoclimates at the Last Glacial Maximum: comparison of Paleoclimate Modeling Intercomparison Project (PMIP) simulations and paleodata. Clim. Dyn., 15, 857-874. Pinot S., Ramstein G., Marsiat I., De Vernal A., Peyron O., Duplessy J.C., Weinelt M., 1999b. Sensitivity of the European LGM climate to North Atlantic sea-surface temperature. Geophys. Res. Lett. 26, 1893-1896. Farrera I. et al,1999. Tropical climates at the Last Glacial Maximum: a new synthesis of terrestrial palaeoclimate data. I. Vegetation, lake-levels and geochemistry. Clim. Dyn. 15, 823-856 Kageyama M., Peyron O., Pinot S., Tarasov P., Guiot J., Joussaume S., Ramstein G., 2001. The Last Glacial Maximum climate over Europe and Western Siberia: a PMIP comparison between models and data. Clim. Dyn. 17, 23-43], Peyron O, Guiot J, Cheddadi R, Tarasov P, Reille M, de Beaulieu JL, Bottema S, Andrieu V (1998) Climatic reconstruction in Europe for 18,000 years BP from pollen data. Q Res 49:183–196 Weinelt M, Sarnthein M, Pflaumann U, Schulz H, Jung S, Erlenkeuser H, 1996, Ice-free nordic seas during the last glacial maximum? Potential sites of deepwater formation, Paleoclimates 1-4 :283-309 Jost A., Lunt D., Kageyama M., Abe-Ouchi A., Peyron O., Valdes P.J., Ramstein G., 2005. High-resolution simulations of the last glacial maximum climate over Europe: a	

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				solution to discrepancies with continental palaeoclimatic reconstructions? Clim. Dyn. DOI 10.1007/500382-005-0009-4 [GILLES RAMSTEIN]	
6-672	A	13:2	13:2	Needs to be clarified: Broecker et al. state in their abstract that there is not enough data in the Pacific to assert whether the deep Pacific ocean was older, as old as, or younger to today at the Last Glacial Maximum. To quote them: "The conclusion is that the scatter in these results is so large that the apparent 14C age of glacial deep Pacific water could lie anywhere between double and half today's."Therefore, it does not seem that Broecker et al. 2004 provide a good ground to reject Paillard's hypothesis (cite, Paillard and Parrenin, EPSL, 2004). [Michel Crucifix]	There seems to be a confusion between Broecker's Science paper and an earlier paper by Broecker on the same subject. Comment will be considered.
6-673	A	13:4	13:4	1-It should be emphasized that this value ~ 180/190ppm is reached for all glacial maxima since 800 000My and therefore it is reflecting some robust feature (not contingent to LGM). 2-Our understanding of this question has consequences for equilibrium of CO2 after anthropic perturbation at the time scale of millennia (see Archer) [GILLES RAMSTEIN]	Accepted.
6-674	A	13:4	:7	This describes the quandary very well. There are difficulties and that they must be addressed by further research. [Lee C. Gerhard]	Accepted. Thank you
6-675	A	13:5		Lagoons can also be considered "sentinels" of climate change processes. This was considered in detail in: Eisenreich, Stephen J. (2005) (Ed) Climate Change and the European Water Dimension EUR 21553 EN, European Commission, where a chapter on Venice is presented http://ies.jrc.cec.eu.int/fileadmin/Documentation/Reports/Inland_and_Marine_Waters/Climate_Change_and_the_European_Water_Dimension_2005.pdf [Pierpaolo Campostrini]	Comment seems misplaced.
6-676	A	13:13	13:13	Be more specific about 'impact, eg. "are likely to reduce future…" or "are likely to increase future…" [C.F. Michael Lewis]	Not accepted, as sign of future feedbacks not entirely clear.
6-677	A	13:18	13:27	Taken literally, this paragraph implies that an extremely weak forcing like the eccentricity variations produce glacial and interglacial cycles through the dominent influence of feedbacks in the system - implying that the climate system is extremely, extremely sensitive. Why then did only the very weak eccentricity variations make use of these large	Accepted

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				feedbacks? And what implication does that have about future climate changes, when we're providing a very large forcing? The certainty with which this chapter places eccentricity forcing at the center of the 100K cycles has very strong implications that should not be ignored - or alternatively, and more accurately, the chapter should be more humble about the cause of the 100K cycles. [Andrew Lacis]	
6-678	A	13:21	13:21	Among the greenhouse gases it is especially water vapor which contributes to amplify the astronomical forcing. See Berger A., Tricot C., Gallée H., Loutre M.F., 1993. Water vapour, CO2 and insolation over the last glacial-interglacial cycles. Phil. Tans. R. Soc. Lond. B, 341, 253-261. Correction: "water vapor (Berger et al., 1993) and CO2 and the shrinkage ..." [André BERGER]	Accepted
6-679	A	13:21	13:21	It is fundamentally wrong to state that the "initial forcing due to ecentricity is amplified". The changes in insolation induced by eccentricity alone are so weak that they probably have no significant influence on the climate system. By contrast, those related to precession (the amplitude of which is *modulated* by eccentricity) AND obliquity are non-linearly filtered by the climate dynamics to produce a 100-kyr cycle over the last 800 kyr. Non-linear agents include the build-up of ice sheets, isostasy and CO2. In terms of energy balance, note the dominant role played by the water-vapour feedback (Tricot et al., Phil. Trans. Roy. Soc. Lond. B, 341, 256-261, 1993). [Michel Crucifix]	Rejected, authors believe text is justified.
6-680	A	13:21	13:24	This does not follow from previous discussion [Thomas Karl]	Rejected, due to spcae limitations
6-681	A	13:21	13:21	Add "during deglaciation" after 'northern hemisphere ice sheets'. [C.F. Michael Lewis]	Accepted, need a short summary and x-ref to Ch. 9.
6-682	A	13:22	13:22	Which “initial forcing due to orbital eccentricity” is meant here. [Andrey Ganopolski]	Accepted
6-683	A	13:22	13:23	I don't think "prerequisite" is the right word--suggest saying "Greenhouse gas forcing was thus an important contributor to driving the strong …" [Michael MacCracken]	Rejected, authors believe text is clear.
6-684	A	13:24	13:25	In the Southern Hemisphere! Not to be confused with Greenland or North Atlantic. [Thomas Blunier]	Taken into account, will be in separate section.
6-685	A	13:24	13:27	High latitude temperature, indeed, led CO2, but only in Antarctica. Which orbital forcing in the Southern Hemisphere caused this initial warming which was then amplified by CO2? [Andrey Ganopolski]	Rejected, confusion between orbital forcing and solar activity

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6-686	A	13:24	13:24	I would suggest rewording to say "Observations indicate that the high latitude …" [Michael MacCracken]	Noted, to be checked
6-687	A	13:25	13:26	I would suggest changing this to read "but leads to changes in sea level. Changes in greenhouse gas concentrations were thus an important feedback mechanism critical …" [Michael MacCracken]	Accepted
6-688	A	13:29	13:30	Consider to de-emphasize the LGM in the entire Chapter. The notion of the LGM as counterpart for future climate change is misleading. The 80 ppm CO2 change from LGM to pre-industrial is about the same as from pre-industrial to today. The corresponding temp. changes are of course not comparable. Without a very careful discussion this could again play into the hands of climate critics. I miss this careful discussion. [Michael Schulz]	Noted, will rewrite to emphasise regional and seasonal changes in forcing. Refer to new orbital box.
6-689	A	13:29	15:13	Comment in general on sections'what does the last ice age tell us?' and 'How realistic are results from climate model simulations etc.?' There is too much focus on simulation models: there are good (geological) quantitative temperature reconstructions for the LGM and Younger Dryas, for instance for Europe by Huijzer, B. & Vandenberghe, J. 1998 (Climatic reconstruction of the Weichselian Pleniglacial in northwestern and central Europe, J. Quat. Sc. 13, 391-417); Velichko, A. 1982 (Paleogeography of Europe during the last one hundred thousand years. Nauka, Moscow, 156p.); Isarin, R. 1997 (Permafrost distribution and temperatures in Europe during the Younger Dryas, Permafrost and Periglacial Proc. 8, 313-333); Isarin, R. & Bohncke, S. 1999 (Mean July temperatures during the Younger Dryas in northwestern and central Europe as inferred from climate indicator plant species. Quat. Res. 51, 158-173); Vandenberghe, J., Lowe, J., Coope, R., Litt, T. & Zöller, L. 2004 (Climatic and environmental variability in the Mid-Latitude Europe sector during the last interglacial-glacial cycle. In: 'Past climate variability through Europe and Africa' eds. R. Battarbee, F. Gasse & C. Stickley, 393-416). [Jef Vandenberghe]	Accepted
6-690	A	13:30	13:40	Here, or somewhere similar you should refer to Chapter 9 (9.6.2). In various parts of this chapter the potential usefulness of the LGM as a "test-bed" for climate models is pointed out, but you seem unaware that it has already been used to constrain climate sensitivity. I suppose it reads this way because the scope of this chapter is only to describe the paleoclimates, but I think that it makes sense to refer to the other chapter describing the related work. [Julia Hargreaves]	Accepted
6-691	A	13:30	13:38	Select and use one spelling for 'modeling' as in this line, or as 'modelling' as in line 38. [C.F. Michael Lewis]	Taken into account, see 6-693
6-692	A	13:33	13:33	I would prefer "changes in climate" as "climate change" is what we are talking about referring to current event.	Noted

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				[Michael MacCracken]	
6-693	A	13:42	13:54	It seems to me confusing to be making surface ice an external forcing rather than a feedback. If we had the ultimate model, natural changes in the cryosphere and vegetation would be considered feedbacks and not forcings. The chapter does not really go into distinguishing and defining what are forcings and feedbacks, so I think this section will be confusing to readers. It is indeed true that these conditions are being imposed on the models, so could be viewed as a forcing, but I think it would be more self consistent to be talking about these simulations making runs where particular feedbacks have been taken to their observed limits to account for the shortened time of these simulations than to say that they are external forcing, for by the next IPCC assessment, they will likely be feedbacks. This also becomes important when talking about CO2, as it is currently a forcing--but during glacial times was a feedback. [Michael MacCracken]	Accepted
6-694	A	13:42	:54	There is no explanation why small changes in solar activity are so well correlated with measurable changes in climate. IPCC needs to address this, either by refereed literature or by suggetions for additional rsearch. [Lee C. Gerhard]	Rejected, authors believe text is clear
6-695	A	13:43	13:43	2.8 Wm-2: using the TAR rules for inferring GHG forcings, and PMIP2 guidelines for LGM GHG concentrations, I arrive at 3.25 Wm-2. [Michel Crucifix]	Taken into account, see 6-693
6-696	A	13:44	13:45	"Solar insolation" is repetitive. Replace either by "Insolation" or by "Solar irradiation". [Martin Stendel]	Accepted
6-697	A	13:45	13:45	provide clearer explanation of how orbital forcing of 0.014 w m-2 leads to ice sheets, lower CO2 and large climate changes [Stephen McIntyre]	Accepted, text will be modified within space limitations
6-698	A	13:46	13:46	Using results of PMIP2 experiments, the ice sheet forcing is: 2.83 W/m2 (MIROC3.2, but no sea-level change), 4.04 (HadCM3M2), 2.44 (CCSM) and 3.55 (FGOALS). [Michel Crucifix]	Accepted, text will be modified within space limitations
6-699	A	13:46	13:46	I would say “radiative forcing of the LGM ice sheets” [Andrey Ganopolski]	Rejected, too detailed, due to space limitations
6-700	A	13:46	13:47	The ice is shownhere as a forcing, but this does not explain the formation of the ice, which is a feedback under the forcing defintions elsewhere. [Stephen McIntyre]	Rejected, see Smith et al. 2005
6-701	A	13:48	13:48	the parameterisation of bare soil albedo also influences the amplitude of the "ice sheet forcing". [Michel Crucifix]	Accepted

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6-702	A	13:52	13:54	I fail to see the justification for asserting that radiative forcings cannot be combined additively. [Anthony Broccoli]	Accepted
6-703	A	13:52		Noted above in crossref to page 2 line 27 … -6 to -11 differs from the PMIP simulation mentioned on page 15 line 10 [Tas van Ommen]	Accepted, wording will be changed.
6-704	A	13:53	13:53	vegetation is a feedback [Stephen McIntyre]	Accepted, text will be modified within space limitations
6-705	A	13:56	13:56	The central values for LGM radiative forcings and their uncertainties need more quantitative justification. There also should be a subjective estimate of confidence, as was employed for modern radiative forcings in the TAR. [Anthony Broccoli]	Accepted
6-706	A	13:56	13:56	The right part of Fig. 6.2 (regional dT vs. global dT) is apparently nowhere discussed, which is a pitty. [Michael Schulz]	Noted, but there is a lack of space for this level of detail
6-707	A	14:1	14:28	More should also be made of the spatial heterogeneity of the LGM climate anomaly, especially as the literature is replete with papers which assume that the cooling was unifporkm right across the tropics! For example, many aspects of the land climate anomaly pattern shown by Farrera et al. also show up in the MARGO reconstructions of SSTs. [Iain Colin Prentice]	Accepted, text will be modified within space limitations
6-708	A	14:1	14:11	This comment probably reflects my particular interests, but I believe it should be noted that there were areas (such as the present deserts and steppes of the western USA) that experienced much wetter than current climates during the LGM. Also, it is worth mentioning that is mountain regions the limits of forest species were as much as 1000 m below their current levels. [Robert Thompson]	Accepted
6-709	A	14:1	15:13	Consistent with the underplay of uncertainties, this discussion, and Table 6.1, carefully avoids the big question concerning models and tropical sensitivity - can the models reproduce the apparently contradictory conclusions of large cooling over the tropical land and much smaller cooling of the tropical ocean. The answer is NO - yet we don't find that result stated in any clear fashion in this section. This has big implications for future climate, in which the tropical sensitivity is similarly uncertain. Model simulations of the future climate do not produce such a large distinction between tropical ocean and land warming for that time period either. If it really did happen, then we need to know that, and know that the model simulations for the future are somehow faul.ty. If it did not happen, then the observations are at fault. The general avoidance of stating this issue	Rejected, need for historical perspective

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				clearly in this section is inexcusable. [Andrew Lacis]	
6-710	A	14:6	14:7	Page 6.14 – line 6-7 : Peyron et al (2005) present just a reconstruction for small region and even not for LGM (older period covered is 16ka BP) ; it is better to cite Peyron et al (1998) which is a synthesis for the whole Europe (reconstructed vegetation + climate); Peyron O, Guiot J, Cheddadi R, Tarasov PE, Reille M, de Beaulieu J-L, Bottema S, Andrieu V (1998) Climatic reconstruction in Europe from pollen data, 18 000 years before present. Quat Res 49 : 183—196 ; [Joel GUIOT]	Rejected, due to space limitations
6-711	A	14:6	14:7	Mention also that in Northern Eurasia, tundra at north and steppes at south were more extended than today and forest were strongly reduced (Tarasov et al., 2000); Tarasov, P.E. et e. al., 2000. Last glacial maximum biomes reconstructed from pollen and plant macrofossil data from northern Eurasia. Journal of Biogeography, 27, 609-620. [Joel GUIOT]	Rejected, too detailed
6-712	A	14:10	14:11	Please state that this refers to tropical warming after LGM [Thomas Karl]	Taken into account
6-713	A	14:10	14:11	More is known than this about LGM land conditions. See, above all, the synthesis by Farrera et al. in Climate Dynamics, and the recent special issue of Quaternary International dervoted to LGM snowline changes. [Iain Colin Prentice]	Accepted
6-714	A	14:10	14:11	Some caution is needed about the statement of the scale tropical terrestrial temperature declines at the LGM. Recent work by Jacquie Smith has undermined the scale of LGM glaciation in the tropical Andes, reducing ELA depressions significantly and consequently 'warming up' the terrestrial tropical LGM for this region. [Smith, J. A., Seltzer, G. O., Farber, D. L., Rodbell, D. T., and Finkel, R. C., 2005, Early Local Last Glacial Maximum in the Tropical Andes: Science, v. 308, p. 678-681.] This is unlikely to be a singular case. In many areas the 'LGM' moraines have assumed rather than chronometrically dated ages. Temperature reconstructions from tropical floras are extraordinarily difficult to assess - In New Guinea major floral changes are recognised but the LGM ecotones no longer exist and other factors such as carbon dioxide changes may play a role in the floral reorganisation. At lower elevations in the tropics, precipitation effects typically swamp out temperature signals. This leaves only noble gas thermometry as a 'secure' indicator of tropical cooling. As noted later in this chapter, interpretation of this thermometer is not always straightforward. In short, the terrestrial tropics could still be interpreted as showing thermal declines of 0-3 deg C rather than the 5 C mentioned here.	Accepted

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				[James Shulmeister]	
6-715	A	14:11		Page 6.14, line 11 : in southern Hemisphere, LGM is more complex, as the migration of the ITCZ towards north could have locally induced more precipitation (Baker et al., 2001; Cruz et al., 2005; Garcin et al., subm) but other studies tend to show genral drought (Gasse et al., 2002; Filippi & Talbot, 2005) ; Baker, P.A. et al., 2001. Tropical climate changes at millennial and orbital timescales on the Bolivian Altiplano. Nature, 409(6821): 698-701. ; Cruz, F.W., Jr et al., 2005. Insolation-driven changes in atmospheric circulation over the past 116,000 years in subtropical Brazil. Nature, 434(7029): 63-66. ; Gasse, F., Barker, P. and Johnson, T., 2002. A 24,000 yr diatom record from the northern basin of Lake Malawi. In: E.O. Odada and D.O. Olago (Editors), The East African Great Lakes: Limnology, Palaeolimnology and Biodiversity. Kluwer Academic Publishers, Dordrecht, Netherlands, pp. 393-414. ; Filippi, M.L. and Talbot, M.R., 2005. The palaeolimnology of northern Lake Malawi over the last 25 ka based upon the elemental and stable isotopic composition of sedimentary organic matter. Quaternary Science Reviews, 24(10-11): 1303-1328 ; Garcin, Y. et al. Subm. Multi-decennial to multi-millennial changes in maar-lake deposition during the last 45,000 years in South Tropical Africa (Lake Masoko, Tanzania). Paleo3. [Joel GUIOT]	Accepted
6-716	A	14:13	14:13	The magnitude of ocean cooling at the LGM has been established through a number of studies. [Steven Clemens]	Accepted
6-717	A	14:13	14:28	May need to stress better modern paleodata synthesis efforts. E.g. GLAMAP for the North Atlantic. (Pflaumann et al. Paleoceanogr. 18 (3): 1065, 2003 doi:10.1029/2002PA000774 for SSTs and Sarnthein M., U. Pflaumann, M. Weinelt ,Past extent of sea ice in the northern North Atlantic inferred from foraminiferal paleotemperature estimates, Paleoceanography, 18 (2), 1047, doi:10.1029/2002PA000771, 2003 for sea-ice. [Michel Crucifix]	Accepted, text will be modified within space limitations
6-718	A	14:13	14:28	This section should discuss differences between the LGM and the modern, not differences between the CLIMAP reconstructions and later reconstructions, as many readers may not as intimately acquainted with the CLIMAP work as many in the paleoclimate community. [William Howard]	Accepted, phrasing will be rewritten.
6-719	A	14:13	14:28	In addition to Guilderson's coral records, other types of records such as Mg/Ca and alkenone thermometry should be mentioned. [Katsumi Matsumoto]	Accepted, will check references
6-720	A	14:13	14:28	There remain still problems in lnking the oceanic (Atlantic) and continental (European) temperature reconstructions, as shown by Rrenssen & Vandenberghe 2003 (cited also p.	Accepted

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				14, line 5). [Jef Vandenberghe]	
6-721	A	14:13		Should read: "ocean cooling, particularly in the tropics, has been hotly debated." [Eric Wolff]	Accepted, text will be modified within space limitations
6-722	A	14:14	14:18	"More recent reconstructions indicate more pronounced cooling". Insist on the fact that we are considering the Pacific here. [Michel Crucifix]	Accepted
6-723	A	14:15	14:16	Delete sentence starting with "More recent reconstructions indicate more…." [Steven Clemens]	Taken into account, summarised briefly here, referred to Ch 9.
6-724	A	14:16	14:17	syntheses confirm that tropical SST cooling… [Steven Clemens]	Taken into account
6-725	A	14:16	14:16	It needs to be indicated where it is 4-5 C cooler [Michael MacCracken]	Taken into account, new forcing and feedback text
6-726	A	14:17	14:17	There needs to be an explanation of what controls how much cooling occurs--where and why can it be zero, or 3.5 C or whatever--for example, is the change 0 or 3.5 in the western Pacific, [Michael MacCracken]	Rejected, authors feel text represents a balanced view
6-727	A	14:19	14:19	“More meridional surface circulation…” Where? Probably, the northern part of the North Atlantic is meant here. [Andrey Ganopolski]	Rejected, not appropriate for summary
6-728	A	14:20	14:25	The McManus 2004 record does NOT show that AABW was 'much shallower' during the LGM it DOES show that the MOC may have been reduced at the LGM, but this is not conclusive. Further Piotrowski et al. 2005, Science 307, 1933-1938 should be added to the references on line 24. I suggest removing the McManus citation from line 24 and citing McManus et al. 2004; Rutberg et al. 2000; Piotrowski et al. 2005 after '...vary among the proxy indicators.' on line 26 - perhaps also a citation relating to 13C work is needed here. [Mark Siddall]	Accepted, text will be modified within space limitations
6-729	A	14:21	14:21	This would make sense if "migration" were replaced by "variation" [Michael MacCracken]	Accepted
6-730	A	14:25	14:26	"Changes in…" statement is too vague. Add details [Michael Schulz]	Rejected, authors feel table is justified
6-731	A	14:26	14:26	"the strength of the overturning cell is more difficult to determine and vary among proxy indicators". To my knowledge, only Pa/Th is supposed to provide an estimate of the velocity of deep currents and, consequently, of the strength of the overturning cell. Other proxies rather provide information on the distribution and depth of water masses. [Michel Crucifix]	Rejected, due to space limitations

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6-732	A	14:28	14:28	An estimate should be made of the climate sensitivity using the estimated forcings and the estimated cooling at the LGM – my naïve calculation give between 0.5 and 1 deg/ (W/m2), with a rough mean of around 0.75. It should also be stated that the long tail of climate sensitivity numbers that are discussed in Chapter 9 (i.e > 6 deg/ doubling CO2) are almost certaintly ruled out by the LGM results. This calculation has been done by many authors (Hansen et al, 1985; Lorius et al , 1991; etc.). [Gavin Schmidt]	Rejected, due to space limitations
6-733	A	14:30	14:43	The discussion of data-model comparisons here is highly quantitative. I think that it is worth noting that, in general, the model simulations reproduce the patterns atmospheric circulation inferred from the paleodata. This may be important for the simulations of future climatic conditions (and especially in regard to the future distribution of moisture-related variables). [Robert Thompson]	Rejected, to be kept for perspective
6-734	A	14:30	15:13	There needs to discussion about how reliable the models are with "ice sheet forcing". Ice sheets of course should be a part of a more comprehensive "climate model". High latitude continental temperatures would be pegged with prescribed ice sheets, and so it would not be surprising that there would be consistency in that regard. [Katsumi Matsumoto]	Accepted, will be fixed
6-735	A	14:30	15:13	I question the implication "of solving the problem" in final sentence of this section becasue in Table 6.1 on page 67 a PMIP-2 concensus of 0-3 C for LGM tropical ocean cooling is presented which seems like a significant range to state current couple climate models are able to simulate the response to large scale climate forcing change. My understanding is that the PMIP-2 simulations are highly dependent on the method used to spin up the glacial ocean. If this is true then there are not only disparities among the models but we can expect different results from the same model depending on how it is initialized. It is misleading to the policy maker audience to suggest that have solved the problem [Robert Webb]	Accepted, will be fixed
6-736	A	14:31	14:35	The models discussed here involved prescribed ice sheets: are there models which generate the ice sheets from the Milkowitch forcing. If so, discuss. If not, state and carry forward to summary, [Stephen McIntyre]	Accepted, will be fixed
6-737	A	14:42	14:43	"radiative forcing decrease of 4 to 7 Wm2" : replace by "radiative forcing by reference to the pre-industrial of -4 to -7 W/m2". Several studies have already attemped to quantify the global impact of vegetation changes at the LGM (Kubatzki and Claussen, Clim. Dyn 14 (461-471) 1998; Wyputta and McAveney, Clim Dyn (17) 923-932 2001, Crowley and Baum, JGR 102 (D14) 16463-16480 (1997), Levis et al. JGR (104) 31191-31198 (1999),	Noted, will be considered

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				Crucifix and Hewitt, Clim. Dyn, 2005). In the latter, the global impact of vegetation change on surface temperature is —0.6 C. Also need to cite systematic biases identified in models. E.g. Kageyama et al. (submitted, but already referred in chapter) identify underestimated winter cooling in PMIP and PMIP2 simulations. Good also to emphasise possible difficulties related to data interpretation. Kageyama et al. give substance to the idea that some data reconstructions, especially in western Europe / Central Siberia, may be biased due to their sensitivity to extreme events. Wohlfart et al (in press, reference provided by Sandy Harrison), benchmarks coupled ocean-atmosphere simulations — focus on high latitudes — by comparing BIOME4 model outputs with pollen spectra, and identified systematic biases in central Asia. [Michel Crucifix]	
6-738	A	14:43	14:43	I cannot find the Schneider reference in the reference list. [Julia Hargreaves]	Accepted
6-739	A	14:45	14:45	I find Tab. 6.1 not too useful in this context. Could be deleted [Michael Schulz]	Accepted
6-740	A	14:47		please cite Tarasov et al., 1999; Tarasov PE, Peyron O, Guiot J, Brewer S, Volkova VS, Bezusko , LG, Dorofeyuk NI, Kvavadze EV, Osipova IM, Panova NK (1999) Last glacial maximum climate of the former Soviet Union and Mongolia reconstructed from pollen and macro-fossil data. Clim Dyn 15:227–240 [Joel GUIOT]	Accepted
6-741	A	14:49	14:50	Broccoli (2000) simulated a cooling of >20 K over Greenland. Although the statement made here may be intended to apply strictly to PMIP-2 models, it leaves the misleading impression that all models have been unable to simulate the large LGM cooling of Greenland. [Anthony Broccoli]	Accepted, text will be modified within space limitations
6-742	A	14:52	14:54	The statements about PMIP-1 simulations don't seem to add much (they were after all described in the TAR and the field has moved on since then). However, a reference to Pinot et al. (Climate Dynamics) might be in order. [Iain Colin Prentice]	Accepted, will be revised
6-743	A	14:54	14:54	“Colder than cooling obtained from observations”. This statement is in odd with the line 10 on the same page. Actually, reported models results are warmer than implied by data. [Andrey Ganopolski]	Accepted, will be revised
6-744	A	14:56	14:57	The cooling of the tropical oceans of 1-2.3 K is described as being "on the cold end of proxy estimates." What is the basis for this statement? The tropical Atlantic panel of Figure 6.2 indicates that the models are warmer than the regional average. Lines 16-17 of the same page characterize the overall tropical cooling as 0-3.5 K. [Anthony Broccoli]	Taken into account.

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6-745	A	14:56	14:56	After inspecting the figure, I think "cold" should be replaced by "hot". [Julia Hargreaves]	Taken into account.
6-746	A	14:56	14:56	Since most of the models (3 out of 5) lie outside the proxy range for the Tropical Atlantic are we therefore to conclude that they are wrong, that this "test-bed" experiment has failed, and that these models should be excluded from the rest of the IPCC report? Isn't this figure also in conflict with the assertion (6-2 l35-38) that the cooling in the tropics has been correctly reproduced? [Julia Hargreaves]	Taken into account, to be consistent with Ch 9.
6-747	A	14:56	15:1	This statement is puzzling. What caused the rest of the cooling? [Iain Colin Prentice]	Accepted, 2nd part only, 1st part rejected, still useful
6-748	A	15:1	15:1	"S.I. Shin et al., 2003; Shin et al., 2003a;" should presumably read "Shin et al., 2003a, 2003b;". [James Crampton]	Noted, to be checked with other chapters.
6-749	A	15:1	15:6	Are the initials 'S.I.' really needed in the citation of 'Shin' on this line and line 6? [C.F. Michael Lewis]	Accepted
6-750	A	15:2	15:7	Need to better explain that different models and experiments provide drastically different simulations of abyssal ocean circulation in the Atlantic at LGM (probably partly due to unsufficient constraints on freshwater balance of the North Atlantic, namely related to uncertainties on river run-off, iceberg melt etc, and partly due to the model itself (intrinsic stability of THC in the model). [Michel Crucifix]	Accepted
6-751	A	15:6	:7	On page 14, lines 21-28, a variety of new paleoindicators provide constrains on the LGM Atlantic THC, yet on page 15, lines 6-7, it is reported that the PMIP-2 modesl simulate a range of responses of the Atlantic deep ocean and overturning cirulation [Robert Webb]	Accepted
6-752	A	15:7	15:7	Line 7 a little misleading. It would be more correct to say that till now there is not a consensus on OAGCM THC response at LGM [GILLES RAMSTEIN]	Accepted
6-753	A	15:9	15:13	This climate sensitvity finding needs to be tied to other sections that discuss climate sensitivity. [Bryant McAvaney]	Rejected, authors believe text is justified.
6-754	A	15:9	15:11	Ice sheets are a feedback under earlier definitions of forcing. So how can they be included in denominator in calculation of sensitivity? [Stephen McIntyre]	Rejected, due to spcae limitations
6-755	A	15:9	15:13	A stronger statement is important here. The fact that the model estimates of cooling agree	Accepted, need a short summary and x-

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				broadly with data-based estimates is a very powerful message about climate sensitivity. In fact, it means that climate sensitivity is better constrained on the basis of LGM observations than it is from recent observations as exemplified by the Stainforth et al. and Murray et al. studies. [Iain Colin Prentice]	ref to Ch. 9.
6-756	A	15:9	15:11	The conclusion is a little bit biased by the reduction of computing sensitivity for the past and future in terms of how much C changes per watt of perturbation. It is too drastic a simplification. The forcing of CO2 doubling and building 3Km high ice sheets is just different. When the first has “a symmetric inter hemispheric” forcing, the second is very different. The conclusion could be: More sophisticated multiproxy analyses give a more realistic view of LGM climate with reduced uncertainties on changes occurring at LGM. Models including ocean and vegetation dynamics are also more able to reproduce these features except in some important areas such as Western Europe. The capability to introduce directly O18 or C13 in models is very helpful to reproduce directly the available data (see Roche 2004 for instance) Roche D., Paillard D., Cortijo E., 2004. Constraints on the duration and freshwater release of Heinrich event 4 through isotope modelling. Nature 432, 379-382 The conclusion is a little bit biased by the reduction of computing sensitivity for the past and future in terms of how much C changes per watt of perturbation. It is too drastic a simplification. The forcing of CO2 doubling and building 3Km high ice sheets is just different. When the first has “a symmetric inter hemispheric” forcing, the second is very different. The conclusion could be: More sophisticated multiproxy analyses give a more realistic view of LGM climate with reduced uncertainties on changes occurring at LGM. Models including ocean and vegetation dynamics are also more able to reproduce these features except in some important areas such as Western Europe. The capability to introduce directly O18 or C13 in models is very helpful to reproduce directly the available data (see Roche 2004 for instance) Roche D., Paillard D., Cortijo E., 2004. Constraints on the duration and freshwater release of Heinrich event 4 through isotope modelling. Nature 432, 379-382 [GILLES RAMSTEIN]	Accepted
6-757	A	15:10	15:10	The use of Climate sensitivity here is in conflict with other parts of the report. In chapter 2 the "Climate Sensitivity Parameter" is defined as the temperature change per change in radiative forcing, oC/(Wm-2). In chapter 8 "Climate Sensitivity" is defined as the	Rejected, authors believe text is clear.

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				temperature change per a doubling of CO2, oC. The different useages of "climate sensitivity" in the climate literature can naturally cause some problems. In this sentence if the sensitivity value is "0.4 to 1.2oC/(Wm-2)" then the correct term to use must be "Climate Sensitivity Parameter". Confusion will be caused if this is not changed. [Gareth S. Jones]	
6-758	A	15:10		As noted above, for consistency with page 2, -4 to -7 wm-2 should be signed negative [Tas van Ommen]	Taken into account, will be in separate section. David
6-759	A	15:11	15:11	"…similar to" --> too unspecific. add range for doubling CO2 [Michael Schulz]	Rejected, confusion between orbital forcing and solar activity
6-760	A	15:15	15:22	Without there being some sections describing what is included as forcings and/or feedbacks, this really is not as helpful as it needs to be. Is the 50 W per square meter at the top of the atmosphere--and does it include albedo effects regarding ice sheets, etc.? Are the forcings relative to glacial or interglacial periods--what is the base amount (is it the present?)? Where it is said there are large latitudinal and seasonal values, the annual, global average value needs to be indicated because that is what the IPCC relationship depends on (perhaps mistakenly--and if this is so then IPCC needs to be thinking about how to express the sulfate forcing differently as that is seasonally and latitudinally varying). It really does seem from the paleo record that the distribution of forcing makes a difference--and if this is true, it needs to be said, and what this means with respedct to the IPCC presumption that all forcings can be added globally to get a response needs to be discussed. [Michael MacCracken]	Taken into account in new box on orbital forcing, and in new section on forcings and feedbacks.
6-761	A	15:16	15:16	“Modulation by the 400 kyr orbital eccentricity period”. Modulation of what? [Andrey Ganopolski]	Text modified.
6-762	A	15:16	15:19	Are this small insolation changes relative to the present? [Andrew Lacis]	Text modified.
6-763	A	15:18		Suggest reword to “ This situation is predicted to continue for the next 50 000 years..” [Brent Alloway]	Accepted, Should read 30000
6-764	A	15:22	15:22	"suggesting a fairly constant GHG radiative forcing" replace by "showing that GHG concentrations are similar over the various interglacial periods". [Michel Crucifix]	Accepted, text modified.
6-765	A	15:24	15:50	It is not clear here what is meant by "today" or "present day" when comparing w/ the LIG, in particular because there has been a nearly 1C global mean warming (and greater warming in e.g. Alaska) over the course of the past century. It is especially important to define the baseline to frame statements, such as the last sentence of the paragraph, where comparisons are drawn between the LIG and the potential not-to-distant future. Given the	Accepted, text modified.

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				difference in forcings governing LIG (primarily summer season w/ possible annual-mean responses due to e.g. albedo-vegetation feedbacks) and today (GHG--with a substantial annual mean radiative forcing), how certain are we based on the evidence cited that global annual mean temperatures are not approaching LIG levels? I think the paragraph is a bit too dismissive of that possibility. Final sentence seems to allude to it however. [Michael Mann]	
6-766	A	15:24	15:35	Marra, M. J. 2003. Last interglacial beetle fauna from New Zealand. Quaternary Research. v. 59. pp. 122-131 [James Shulmeister]	Accepted
6-767	A	15:24	15:35	This paragraph purports to be about warming of the Arctic during the previous interglacial but includes material from all over the world. If you want to extend global coverage you may want to add New Zealand. Marra 2003 has calculated temperatures for the 5e peak based on beetle faunas at 1.6–2.5 C warmer in the summer (January) and 2.3–3.2 C warmer in the winter (July). [Marra, M.J. 2003. Last interglacial beetle fauna from New Zealand. Quaternary Research. v. 59. pp. 122-131.] [James Shulmeister]	Accepted
6-768	A	15:24	:50	We dissect the Holocene curent interglacial in both time and space rather than treating it as a single simultaneous response to compare with climate model simulations but for the LIG, is the density of the data and the relative age control among the terrestrial records good enough to know it is okay to treat it as a single uniform response and rather than a transient response. I looked at the CAPE, Otto-Bliesner and a couple of other papers and only could find regional summaries with no discussion of relative timing and an underlying implication of synchroneity [Robert Webb]	Noted
6-769	A	15:25	15:26	“Averaged insolation in NH summer was higher than today’s by about 12% during the Last Interglacial period (129 to 116 ka)”. In fact, summer insolation during aforementioned period was both higher and lower than at present, and the averaged over the whole LIG summer insolation was only about 5% higher than now (in mid-latitudes of the NH). Probably, the maximum in summer insolation (around 126 kaBP) is meant here. [Andrey Ganopolski]	Taken into account in orbital box.
6-770	A	15:25	:26	This needs a reference. How do we know this? [Lee C. Gerhard]	Noted, see orbital box
6-771	A	15:26	15:28	"the climate of the LIG in both the SH and the NH is inferred to be warmer than today" is incontradiction with lines 6-20, l. 14 "there was no positive global temperature anomaly during the LIG". The truth is probably that we do not have enough information to provide a reliable global temperature reconstruction for that period. The period referred to here is also quite long (129 to 116 ka) and encompasses very different orbital configurations,	Accepted, will be clarified

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				making the quantity "average insolation in NH summer over that period" not so meaningful. [Michel Crucifix]	
6-772	A	15:37	15:38	"polar amplification in the Arctic…" Amplification of what? [Andrey Ganopolski]	Accepted, will be clarified
6-773	A	15:41	15:41	Does the 5 C warming also apply to Antarctica ? What is the appropriate reference? [Michel Crucifix]	Accepted, Watanabe et al.
6-774	A	15:41	15:41	"… with warming of 5C during the LIG" Warming of 5C compared to what? If the authors meant present day, then this is incorrect. [Andrey Ganopolski]	Rejected, correct as it stands, Today is meant as average late Holocene
6-775	A	15:42	15:43	How valid is it to force Greenland with Antarctic temperature changes? That wouldn't have appeared to work for the last glacial maximum, as noted earlier in the text. [Andrew Lacis]	Noted
6-776	A	15:50	15:50	add: Thus leading to a sea level rise of 2.7 to 3.5 m. [Thomas Blunier]	Rejected, cannot quantify future sea level rise due to specifics of the forcings
6-777	A	15:53	15:56	The marine isotopic stages 5, 7, 9 and 11 are quite long (about 50 kyr each). I guess what is meant here are the "warmest phases" of these isotopic stages (that is 5.5, 7.5, 9.3, 11.3). But the word "interglacial" here means something quite specific to Antarctic ice cores that does not necessarily correspond strictly to minima of ice volumes. For instance, the highest sea level was possibly 7.3 and not 7.5. Furthermore, the "duration" of the warm phase in Antartica is not necessarily strictly linked to the duration of the ice minimum, or to the duration of the warm phase in other locations There is a lot of confusion in the community on the words "interglacial" or "glacial" since these words are unfortunately not well defined. In contrast, the LGM is now clearly defined as the maximum ice volume (and not the coldest temperatures). [Didier PAILLARD]	Accepted, will rewrite
6-778	A	15:53	16:21	An interglacial is a period with a minimum and constant ice volume: isotopic stages 5, 7 and 9 and stages with major oscillations in the ice volume. In terms of interglacial, only isotopic stage 5.5 is a strictly speaking interglacial. If this is not correctly mentionned, stage 7 is not shortest than any other interglacial. Stage 7.5 is the shortest but in Antarctica only. This part has to be clarified [Elsa CORTIJO]	Accepted.
6-779	A	15:53	16:21	The whole paragraph is biaised towards ice records. [Elsa CORTIJO]	Taken into account, will be rewritten.
6-780	A	15:53	16:2	This section must be updated with new EPICA data.	Accepted

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				[Per Holmlund]	
6-781	A	16:0		Anyway, overall if the report is as clear and as free from problems as the section I have read, then you've done a nice job! [Sidney Hemming]	Noted
6-782	A	16:1	16:21	This is confirmed on the European continent by Cheddadi et al. (2005) [Joel GUIOT]	Noted, will provide reference
6-783	A	16:1		replace Augustin by EPICA Community Members [Thomas Stocker]	Accepted
6-784	A	16:4	16:20	The last paragraph "The long duration of Stage 11" and the one before could be merged into one. First state that the "long duration of stage 11 results from the interplay between orbital forcing and relatively high greenhouse gases concentrations". When eccentriciy is weak, a glacial inception will only occur if GHG concentration is low enough during relatively small "time windows" opened when northern summer occurs close to the apogeon. This is the reason why it is so important to have accurate chronologies of the ice core records over that period. The analogy Holocene / MIS 11 may be discussed in the "When will the current integlacial end", keeping in mind however that this analogy has limits, especially as regards the variations in insolation that have preceded the interlgacial (i.e. MIS 2 is quite different to MIS 12). [Michel Crucifix]	Noted, may need rewriting
6-785	A	16:4	16:14	From the Vostok record, it seems that the short lived interglacial is warmer than the long lived interglacial periods, such as Stage 11 and Holocene are cooler than stage 5, 7, 9. I am wondering whether the lower temperature would contribute to the length of the interglacial period. [Aixue Hu]	Noted, too detailed to incorporate due to space limitations
6-786	A	16:4	16:10	This argument is completely fallacious. CO2, as has been shown previously, responds on these time scales to the temperature change - so to say that the temperature stayed high because the CO2 was high is very faulty logic! [Andrew Lacis]	Rejected, CO2 is a radiative forcing, it responds as a feedback, it contributes to warmth
6-787	A	16:6	16:6	Paillard (Reviews of Geophysics, 39, 325-346, 2001) could also be cited here, or at line 13-14. [Didier PAILLARD]	Noted
6-788	A	16:7	16:9	This sentence "The recently ...." is misleading, because the Vostok record is heavily distorted. The complete, undisturbed CO2 record over the entire MIS 11 is shown in Siegenthaler et al. 2005 (see details of ref in comment 15). [Thomas Stocker]	Accepted
6-789	A	16:7	16:9	It would be much better to quote the EPICA record, already shown for the start of MIS 11	Accepted

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				in EPICA Community members 2004, but also available in Siegenthaler et al (which, although only submitted is quoted elsewhere). Vostok could not have been reconstructed without seeing the EPICA data, and it will be very dangerous to give the impression that we are happy to turn sections of core upside down at will. Much better to use the simple record that is in the right order. [Eric Wolff]	
6-790	A	16:8	16:9	In that it is said that the Holocene value was essentially constant, this should be revised to read "record shows a CO2 concentration similar to the preindustrial Holocene value over …"--be careful of plurals implying multiple values. [Michael MacCracken]	Accepted
6-791	A	16:10	16:10	Are the climate sensitivities of the models to LGM forcings significantly different than for the same models response to 2xCo2? [Gavin Schmidt]	Comment fits with 15:10, if so comment is accepted
6-792	A	16:12	16:16	The tense here should be changed from present to past--in several spots. [Michael MacCracken]	Taken into account, text will be checked
6-793	A	16:16	16:21	These five lines could be better used than for citing a flawful study. Ruddiman is cited later again. Delete this paragraph. [Thomas Blunier]	Taken into account, if overlap with Holocene section
6-794	A	16:16	16:17	But of course the CO2 was not elevated with respect to other interglacials - a continuation of the same logical lapse. [Andrew Lacis]	Taken into account, the length is determined by the orbital forcing, CO2 contributes to the warmth
6-795	A	16:19	16:21	Statement should be more specific. [Michael Schulz]	Rejected, due to space limitations
6-796	A	16:21		see comment 16 [Thomas Stocker]	Accepted
6-797	A	16:23	16:51	CO2 is a very important player in glacial-interglacial climate change, yet biogeochemistry is not considered here. Also, as pointed out in Box 6.2, we cannot really explain the large atm CO2 variation, so it seems presumptuous to even suggest that we an predict transitions in and out of glacial/interglacial states. [Katsumi Matsumoto]	Rejected, comment not relevant to point being made
6-798	A	16:23		Calov et al. report that they succeeded to reproduce the onset of the last glacial period by considering only insolation change and ice-snow albedo (R. Calov, A. Ganopolski, M. Calussen, V. Petoukhov, R. Greve, Climate Dynamics (2005) 24: 563-576. "Transient simulation of the last glacial inception." Part II: "sensitivity and feedback analysis."). They point out the importance of decreasing the size of the grid to obtain reasonable results. In particular, large grid sizes need spurious large contribution of CO2 while small	Accepted, references inserted, text modified

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				grid makes it minor. [Kiminori Itoh]	
6-799	A	16:23	:38	The answer is "No we cannot predict the transitions out of interglacials and into ice ages" Yes model results are more promising but we do not have a fully coupled dynamical earth system model that successfully simulates either of the transitions. Modeling effort have identified mechanisms that coupled with inferred feedback may result in glaciation but we have not solved this problem and if the authors insist that we have then I challenge them to state that this question is solved and requires no further research. This paragraph lacks a concluding sentence stating we are closer but the answer is still "no". [Robert Webb]	Noted, headline will be changed to say Do we understand..., text of paragraph will be changed
6-800	A	16:24	16:29	This long sentence needs to be broken up. [Michael MacCracken]	Accepted
6-801	A	16:29	16:30	When forced with orbital insolation changes only, past model studies have failed to find the proper magnitude of response to allow for perennial snow cover - this is a very important poinrt. Carry forward to summary. [Stephen McIntyre]	Rejected, not appropriate for summary
6-802	A	16:31	16:37	What isn't said here is that all these different studies come up with different, and in some cases contradictory (more or less NADW production) methods for trying to get glaciers to grow. So while this may be labeled 'more promising', what should be stated is that at this point we do not know which, if any, of these mechanisms are important. Perhaps there is a need for a triggering mechanism such as a series of large volcanic euptions. [Andrew Lacis]	Accepted, will rewrite
6-803	A	16:31	16:31	"more promising" is promotional: do the recent modes succeed or not? [Stephen McIntyre]	Taken into account, text to be rewritten
6-804	A	16:31	16:31	"more promising" - do they achieve perennial snow cover or not? If so, say so. If not, say so. [Stephen McIntyre]	Taken into account, text to be rewritten
6-805	A	16:32	16:32	A prior paper by de Noblet et al. (GRL) first modelled the role of vegetation feedbacks in glacial inception, and should be cited here. [Iain Colin Prentice]	Accepted
6-806	A	16:32		vegetation feebacks : already demonstrated prior to the references given e.g. de Noblet N., I. C. Prentice, S. Joussaume, D. Texier, A. Botta et A. Haxeltine, Possible role of atmosphere-biosphere interactions in triggering the last glaciation, Geophysical Res. Let., 23, 3191-3194, 1996. [Sylvie JOUSSAUME]	Accepted
6-807	A	16:33	16:33	Replace "a coupled dynamical ice sheet model" with "ice sheets that are coupled to the	Accepted

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				model climate, and" [Michael MacCracken]	
6-808	A	16:33	16:33	Shouldn't it be "coupled ice dynamics" rather than "a coupled dynamical ice sheet model" to maintain the symmetry of the sentence construction. [Michael Mann]	Accepted
6-809	A	16:34	67:27	I would suggest referencing the same papers [Robert Webb]	Rejected, comment not relevant to point being made
6-810	A	16:36	16:38	This sentence is not clear. The "cooling took place in a warm North Atlantic/Nordic Seas" seems to be normal. But the major result of the paper by Cortijo et al, 1999 is to show that low latitudes stayed warm while high latitudes cooled before the end of the last interglacial period (sensu stricto, i.e. during the time of minimum and constant ice volume) [Elsa CORTIJO]	Taken into account, text will be modified.
6-811	A	16:36	16:37	"… the initiation of a northern high latitude cooling took place in a warm North Atlantic/Nordic Seas and mid-latitude land environment". It is unclear to me what the authors would like to say here. [Andrey Ganopolski]	Taken into account, text will be modified.
6-812	A	16:36	16:36	When was this cooling? [Michael MacCracken]	Taken into account, text will be modified.
6-813	A	16:40	16:51	"Under a natural CO2 regime, the next glacial period could not be expected to start within the next 30 kyr". It might be improper to say so. Different definations would lead to varied statements. [Guoyu REN]	Rejected, authors believe text is clear
6-814	A	16:40	16:51	Although it is discussed later in the text, it might be worth mentioning the Ruddiman hypothesis here (and pointing forward to the text dismissing it). [Robert Thompson]	Rejected due to space limitations
6-815	A	16:41	16:51	Another exmaple of hubris - the relationship of ice age initiation to cold northern-summer orbital configurations does not work for all of the glacial cycles, as is well known to the authors of this chapter. Therefore to conclude with certainty that the 'highly nonlinear' response to eccentricity variations will not initiate a new ice age is overstepping; what could be said is that it is 'likely' (to use IPCC terminology) that a new ice age is not around the corner. [Andrew Lacis]	Rejected, authors believe text represents a balanced view
6-816	A	16:41	16:43	This is pretty strongly stated--and needs a bit of qualification, I would think. Aside from what might happen as a result of an asteroid impact or possibly with some sort of continental or sea bed uplift, is it really clear that there are no other possibilities?	Taken into account, text will be rewritten.

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				[Michael MacCracken]	
6-817	A	16:41	16:51	I don't understand if this is saying there is no cooling trend at the moment from insolation changes, or if any such trend is very weak, and too weak to offset expected warming. [Neville Nicholls]	Noted, see orbital forcing box for clarification
6-818	A	16:43	16:44	"The Earth's orbit around the Sun can be calculated with high precision for the future, as well as the past, Gallée et al., 1991". The reference here should be Berger, Journ. Atm. Sci, 35 (2362-2367) 1978. This being said, the astronomical forcing is already introduced earlier (p., 8, l. 50-51). The information about astronomical forcing has to be gathered somehow for easy reference, perhaps as a BOX. [Michel Crucifix]	Accepted
6-819	A	16:44	16:44	"the past (Berger, 1978). With low …". The paper by Gallée et al., 1991, is a paper on modeling the response of the climate system to the astronomical forcing NOT on the orbital parameters. Berger (1978) is the most appropriate reference. [André BERGER]	Accepted
6-820	A	16:44	16:44	Gallee et al., 1991 is wrong reference -- should be Laskar, J., F. Jouzel, et al. (1993). "Orbital, precessional, and insolation quantities for the Earth from -20 Myr to +10 Myr." Astron. Astrophys 270: 522-533. [Steven Clemens]	Accepted
6-821	A	16:44	16:44	Gallee et al. (1991) isn't appropriate reference here. [Andrey Ganopolski]	Accepted
6-822	A	16:44	16:44	Gallee et al., 1991 is not the correct reference for insolation calculations, but for the LLN EMIC description (use for instance, Berger, 1978) [Didier PAILLARD]	Accepted
6-823	A	16:47	16:47	Change "could" to "would" [Michael MacCracken]	Accepted
6-824	A	16:48	16:50	The point "Sustained … (Church et al, 2001)" doesn't belong in chapter 6 since it is a projection. It's covered in chapter 10 (more up-to-date than the TAR). [Jonathan Gregory]	Accepted, Church et al. reference deleted, text modified.
6-825	A	16:48		see comment 16 [Thomas Stocker]	Accepted
6-826	A	16:49	16:49	The word "may" needs to be replaced by one of the words in the IPCC lexicon. In addition, this needs to be made more informative, so say something like … "based on indications from Earth history, sustained greenhouse gas concentrations at a level above roughly 500-700 ppm would be likely to lead to a complete ..." [Michael MacCracken]	Taken into account, text will be modified

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6-827	A	16:50	16:51	Well, there;'s plenty of room for speculation in the results quoted - first the CO2 has to stay in the atmosphere for thousands of years (unlikely) to get the Greenland ice sheet to melt; then it has to not regrow when CO2 levels drop and the climate cools. And all of this has a time horizon, using the values here, of some 30 thousand years, plenty of time for anthropogenic influence to be lost in the dust of history. I would suggest this speculation be dropped as being unworthy of a serious scientific report without much greater discussion of the caveats. [Andrew Lacis]	Noted
6-828	A	16:50	16:50	Is Overpeck et al (2005) not also relevant for citation here? [Michael Mann]	Rejected, not relevant since text will be modified
6-829	A	16:50	16:50	delay or prevent [Stephen McIntyre]	Noted
6-830	A	16:55	18:54	This section would be strengthened if it included other possibly types of abrupt change and possible mechanisms that may explain those. A broader discussion could possibly include changes in sea ice (the concept of an ice 'tipping point' has been advanced for the Arctic, see Chapter 4), land surface changes (e.g., in the case of mega-drought?), etc. It may also be helpful to discuss regional abrupt changes versus global abrupt changes in more detail here. Please see chapter 10, box 10.1, where some useful information is given. [Susan Solomon]	Noted. The focus of this section is on abrupt changes "in the glacial-interglacial record"! There are no good records for sea ice changes, although we do think sea ice is an important feedback in the abrupt changes discussed here. Drought is covered in the Holocene section.
6-831	A	16:56	17:2	The spatial domain needs to be associated with the term "abrupt climate change"--is it local, regional, global--it really makes no sense unless domain is stated. [Michael MacCracken]	Noted. We do say that "The repercussions of these abrupt climate changes were global, although out-of-phase responses in the two hemisphere suggest that they were not primarily changes in global mean temperature." Space does not allow detailed discussion of regional patterns.
6-832	A	17:0		I think you did a nice job of summarizing the H events constraints [Sidney Hemming]	Gladly noted.
6-833	A	17:0		Fig. 6.3 in the figure caption it says that Heinrich events are shown, but they are not on the figure. [Sidney Hemming]	Accepted, will be fixed.
6-834	A	17:1	17:12	It's surprising that the most comprehensive review of Abrupt Climate Change, the 2002 NRC Report by Alley et al., is not cited here. [Jochem Marotzke]	Noted. We cite the short summary in Science by the same authors, rather than the long report, as the former is the more accessible reference and of course

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					points to the full report for those looking for the details.
6-835	A	17:2	17:2	…that all abrupt change need not be externally forced. Numerous… [Steven Clemens]	Accepted
6-836	A	17:2	17:2	Do we really know that "not all abrupt change need be forced"? On lines 46-48 the text indicates that a lot remains unknown, and it seems to me that whether forcing greater than a particular size and covering more than a particular domain can occur by a fluctuation or needs to be forced. Of course, this depends on what is called fording and what is called internal--is drainage of meltwater lakes internal or external--if glaciers are external, as is indicated earlier in this chapter, then why not meltwater. What we really want to know is what can happen chaotically--and what happens mechanistically (due to a definable mechanism). It is rather unlikely that we understand all the various mechanisms, so it seems to me a bit strong to be saying that it is clear that not all abrupt changes need to be forced. Do we really know that the instances mentioned following this sentence were natural--or are they just so far unexplained? [Michael MacCracken]	Noted. We say "need not", which does not rule out that perhaps they all are externally forced.
6-837	A	17:2	17:2	suggest 'need be forced by external mecahnisms, such as orbital variations' [Mark Siddall]	Noted. See 835.
6-838	A	17:4	:5	rewrite beginning of sentence starting with "These records" to be 'High latitude records show that [Robert Webb]	Accepted
6-839	A	17:6	17:8	The D/O events are really cycles of a few hundred years length - not just warming events. See earlier note. [Andrew Lacis]	Accepted, text now mentions rapid warming followed by slower cooling
6-840	A	17:6	:6	insert "ice-age" to read 'The most dramatic off these abrupt ice-age climate changes are ……' [Robert Webb]	Rejected, this is already clear from the sentence before and needs not being repeated
6-841	A	17:7	17:8	I think in your section 6.3.2 it would be worthwhile to acknowledge the large contrast between the 8-16 degree temperature changes inferred from the Greenland ice cores and the ~2 degree summer temperature lowerings implied by the glacial moraines. Denton et al. (2005, QSR) have suggested that this apparent discrepancy is due to the seasonal biases of the 2 records. This appears to be a very important observation that is almost certainly going to take us a long way towards understanding DO and Heinrich events as well. [Sidney Hemming]	Noted. Discussion may be extended if space permits.
6-842	A	17:7	17:7	Are we to assume that this large change in Greenland was an abrupt change? Of what scale--regional or global? Earlier it was said that Greenland and Antarctic have been out	Noted. The text says: The repercussions of these abrupt climate changes were

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				of synchronicity--so perhaps this should say it led to an abrupt change (or better, an abrupt shift) of the climate in the North Atlantic region. Similarly for the other examples that follow--are these global or local? When IPCC uses the term climate change it is generally talking about a change in the global climate, so it is important here to make sure to indicate the spatial domain, and whether these are examples of how regional factors can change regional climates--or whether these are global. And are these driven by forcings or feedbacks? [Michael MacCracken]	global, although out-of-phase responses in the two hemispheres suggest that they were not primarily changes in global mean temperature. We think this makes it clear.
6-843	A	17:8	17:12	The difference between Heinrich events and Dansgaard-Oeschger events has to be explained better. One is a North Atlantic event characterized by IRD and accompanied by lower SST while D-O events are temperature events on top of the Greenland ice sheet. [Thomas Blunier]	Accepted, text changed.
6-844	A	17:8		I again emphasise that Heinrich events are IRD events in the marine record. They may be assoicated with cooling in some records, but I feel it is sloppy to talk about them as if they were themselves abrupt climate chnages. [Eric Wolff]	Accepted.
6-845	A	17:9	17:12	see comments 1 and 4, please contact Sidney Hemming over this issue [Mark Siddall]	Noted - see comment 6-832 by Sydney Hemming
6-846	A	17:10	17:10	"the cooling appears to have occured on a century-time scale". Please make clear that this is well the cooling trend itself that takes centuries, and that you do not refer here to the "duration of the cold event". [Michel Crucifix]	Accepted.
6-847	A	17:13	17:14	The best reference in my opinion is Paillard, (2001) because it clearly explains that Ruddiman and Berner may be OK Paillard D., 2001[14]. Glacial cycles: toward a new paradigm. Rev. Geophys. 39, 325-346. [GILLES RAMSTEIN]	Noted, but this is probably a mis-placed comment? Does not seem to fit to the line numbers given.
6-848	A	17:16	17:28	The following may be added. Recent finding of annually laminated sedimentd in Lake Suigetu, Japan, suggest an asynchronous climate changes in the North Atlantic and Japan (Nakagawa et al. 2003, 2005). They concluded that an abrupt warming of ~5? in annual temperature during Late Glacial in Japan led that of the Bolling onset in the North Atlantic. Based on relationships between East-Asian summer and winter monsoon which was estimated from paleo-temperature and precipitation since Last Glacial, Nakagawa et al. (2005) propose the hypothesis that pan-hemispheric cooling events was triggered by North Atlantic forcing, most probably by a melt water pulse and associated changes in the North Atlantic	Noted, but space lacking.

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				thermohaline circulation. Nakagawa, T., H. Kitagawa, Y. Yasuda, P.E. Tarasov, K. Nishida, K. Gotanda, Y. Sawai and Yangtze River Civilization Program Members, 2003: Asynchronous climate changes in the North Atlantic and Japan during Last Termination. Science, 299, 688-691. Nakagawa, T., H. Kitagawa, Y. Yasuda, P.E. Tarasov, K. Gotanda and Y. Sawai, 2005: Pollen/event stratigraphy of the varved sediment of Lake Suigetsu, central Japan from 15.701 to 10,217 SG vyr BP (Suigetsu varve years before present): Description, interpretation, and correlation with other regions. Quaternary Science Reviews, 24, 1691-1701. [Akio Kitoh]	
6-849	A	17:16	17:17	This sentence seems to be indicating that we have an abrupt change when the two hemispheres do something in the opposite direction--so the global average might be near zero. It will be confusing to call this climate change given how the IPCC generally means by this a significant global change. It would have been better to be calling some of these regional variations (and I imagine not everywhere is changing at the same time) "shifts in climate" rather than change--or at least the chapter here needs to indicate that "change" can mean that the global value does not change at all--only hemispheric values. [Michael MacCracken]	Noted. We discussed this in our group, and we disagree that the word "climate change" is limited to a change in global mean temperature.
6-850	A	17:19	17:19	This is an indirect conclusion and should be stated as such. I suggest: Strong changes are found in the global CH4 concentration which may point to changes in the extent or productivity of tropical wetlands. [Thomas Blunier]	Accepted
6-851	A	17:19		the methane reconstructions (Chappellaz and Brook) provide only indirect evidence for changes in the tropical wetlands. Reformulate more precisely [Thomas Stocker]	Accepted
6-852	A	17:23	17:38	Getting out from interglacial, there are two references missing using OAGCM (Khodri 2001) using an EMIC coupled with an ice sheet model (Kageyama et al.2004). Khodri M., Leclainche Y., Ramstein G., Braconnot P., Marti O., Cortijo E., 2001. Simulating the amplification of orbital forcing by ocean feedbacks in the last glaciation. Nature 410. 570-574. Kageyama M., Charbit S., Ritz C., Khodri M., Ramstein G., 2004. Quantifying ice-sheet feedbacks during the last glacial inception. Geophys. Res. Lett. 31. [GILLES RAMSTEIN]	Noted, but this comment seems to refer to some other section.
6-853	A	17:27	17:27	...an increase of ~50 ppb and a decrease of ~30 ppb... [Eva Calvo Costa]	Accepted

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6-854	A	17:27	17:27	The phrasing here seems confusing--should there be an "and" after the first "ppb"? [Michael MacCracken]	Accepted
6-855	A	17:27	17:27	add 'and' after '50ppb' [James Shulmeister]	Accepted
6-856	A	17:27	17:27	Add "and" before "a decrease". [Martin Stendel]	Accepted
6-857	A	17:31	17:32	"Most DO events have a similar, but lower-amplitude, influx of icebergs than the Heinrich events". Discharges of icebergs mainly occur during the stadial (cold) phases, i.e. not during the Dansgaard-Oeschger (warm) events. See Elliot, M., L. Labeyrie, G. Bond, E. Cortijo, J.-L. Turon, N. Tisnerat, J.-C. Duplessy, Millennial-scale iceberg discharges in the Irminger Basin during the last glacial period: Relationship with the Heinrich events and environmental settings, Paleoceanography, 13(5), 433-446, 10.1029/98PA01792, 1998 and van Kreveld, S., M. Sarnthein, H. Erlenkeuser, P. Grootes, S. Jung, M. J. Nadeau, U. Pflaumann, A. Voelker, Potential links between surging ice sheets, circulation changes, and the Dansgaard-Oeschger cycles in the Irminger Sea, 60 - 18 kyr, Paleoceanography, 15(4), 425-442, 10.1029/1999PA000464, 2000. [Michel Crucifix]	Accepted
6-858	A	17:31	17:31	“Most DO events have … influx of icebergs”. DO events (interstadials) are warm events, and influx of icebrgs occurred between DO events during stadials! [Andrey Ganopolski]	Accepted
6-859	A	17:31	17:32	P 6-17, line 31-32; you say that the DO events have a similar but lower amplitude influx of icebergs as Heinrich events, and also significantly reduced surface water salinities. While you might be right about the surface water salinities, I don’t think I know of the evidence to which you refer, so a reference here is necessary. Also, I realize you can’t take into account every nit-picky detail, but I think it’s actually untrue that DO events have similar influxes of icebergs. It’s true that the number of lithic grains per gram increases, and if it’s really true that the salinities decrease then I will acknowledge it’s icebergs. But it’s not just a little bit lower flux of IRD- it’s a totally different thing. [Sidney Hemming]	Accepted - this sentence has been removed, see also previous similar comments
6-860	A	17:31	17:32	Note that here the D/O events are recognized to include cooling. [Andrew Lacis]	Noted
6-861	A	17:32	17:37	To what degree are these « global » and not regional events? [Robert Thompson]	Noted, but this is explained in the text already
6-862	A	17:34	17:34	8.2 ka event [Eva Calvo Costa]	Accepted

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6-863	A	17:35	:36	No mention of the Antarctic Cold Reversal – an important SH abrupt climate change event [Suggest insert after paragraph 3] "There now seems to be growing weight of evidence indicated from key millennial scale climate archives from New Zealand (i.e. speleothem record of Williams et al. 2005) that the region has a distinct palaeoclimatic history that appears to be different in timing, duration and structure to equivalent-aged records of the North Atlantic region and is more likely to be directly affected by Antarctic and south-west Pacific climate influences. For instance, a major abrupt climate reversal (New Zealand Late Glacial Reversal, NZLGR) has been recognised in the 18O speleothem record between bounding warm peaks at 13.53 and 11.14 ka, and culminated at 12.69 ka. It appears that the timing, duration and structure of this reversal is quite different in detail from the YD (Williams et al., 2005). For example, the 18O profile of the NZLGR is shallower than the YD and had a significantly longer duration (2390 years). The NZLGR commenced almost a thousand years later than the start of the Antarctic Cold Reversal (ACR; Jouzel et al., 1995) but continued 1.76 ka beyond it. The NZLGR commenced about 0.83 ka before the YD and finished about 0.36 ka later. This NZLGR spanned the entire Kaipo cold event recognised in the pollen record of eastern North Island (Newnham and Lowe, 2000). Coincident with the timing of the NZLGR the pollen record from south Westland indicates a period of increasing precipitation between 14.4 and 11.4 ka (Vandergoes and Fitzsimons 2003). The pollen evidence is also consistent with the Mt Arthur speleothem trace element record of Hellstrom and McCulloch (2000) which suggests increased precipitation after c. 13.5 ka. Collectively, this multiproxy data broadly supports the conclusion of Turney et al. (2003) that the YD chronozone in New Zealand was characterised by a period of resumed warming, increasingly moist climate, intensified westerly airflow and increased snow accumulation in glacier catchments of the Southern Alps. Hellstrom, J., McCulloch, M., 2000. Multi-proxy constraints on the climatic significance of trace element records from a New Zealand speleothem. Earth Planet. Sci. Let. 179, 287-297. Jouzel, J., Vaikmae, R., Petit, J.R., Martin, M., Duclos, Y., Stievenard, M., Lorius, C., Toots, M., Melieres, M.A., Burckle, L.H., Barkov, N.I., Kotlyakov, V.M., 1995. The two step shape and timing of the last deglaciation in Antarctica. Clim. Dyn. 11, 151-161. Newnham, R.M., Lowe, D.J., 2000. Fine-resolution pollen record of late-glacial climate reversal from New Zealand. Geology 28, 759-762. Turney, C.S.M., McGlone, M.S., Wilmshurst, J.M., 2003. Asynchronous climate change between New Zealand and the North Atlantic during the last deglaciation. Geology 31, 223-226. Vandergoes, M.J., Fitzsimons, S.J., 2003. The Last Glacial-Interglacial Transition (LGIT) in south Westland, New Zealand: palaeoecological insight into mid-latitude Southern	Noted. Again this is a question of space - if space permits we will extend the discussion.

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				Hemisphere climate change. Quat. Sci. Rev. 22, 1461-1476. Williams, P.W., King, D.N.T., Zhao, J.-X, Collerson, K.D., 2005. Late Pleistocene to Holocene composite speleothem 18O and 13C chronologies from South Island, New Zealand – did a global Younger Dryas really exist ? Earth Planet. Sci. Let. 230, 301-317. [Brent Alloway]	
6-864	A	17:37	17:38	The text should say why these authors feel that models may underestimate the effect of abrupt climate changes; this is an important point, because the prediction chapter (chapter 10) rules them out. [Andrew Lacis]	Noted. This obviously refers to page 18, not 17. There the reference is given where these reasons are discussed - since we do not fully agree with this reference, we only mention it briefly without detailed discussion.
6-865	A	17:37	18:54	The separation "What do we know about abrupt climatic change" and "can climate models simulate these changes" may need to be revisited as modelling contributes (and aims) to understand mechanisms. For example, the present organisation causes confusion and redundancy between lines 5-12 of p. 6-18, and lines 47-55 of the same page. [Michel Crucifix]	Noted. While it is a valid point, we decided to organise the material differently and want to keep it that way - the way proposed by Michel has other disadvantages.
6-866	A	17:37	18:12	Again, despite a half-hearted attempted to indicate there are uncertainties in this picture, the major uncertainties are carefully avoided. Evidence shows that the ocean surface cooling occurred before the ice-rafted debris for both Heinrich and D/O events - so how could the meltwater have caused the ocean dynamical response? Furthermore, discharges from different glaciers appeared to have occurred simultaneously, as if they were all being forced by something else. The explanations thus offered for the 'mechanism' here are bogus in the light of this evidence, and, as usual, no discussion of the contradiction is included. [Andrew Lacis]	Noted. But the reviewer only says "evidence shows...", without providing any reference to such evidence. We are not aware of data with sufficient date control that would support his suggestion.
6-867	A	17:37	18:12	I am surprised that there is no mention at all of possible tropical mechanisms, speculative though they may be (e.g. Cane, Clement, and coworkers). The possibility should at least be mentioned. [Michael Mann]	Accepted. See also comment 869.
6-868	A	17:37	18:12	See comments above regarding north-south asynchrony and mechanisms on page 11. Somwhere here comments should look at north south linkages. Also any mention of the see-saw probably ought to cite Broeker. [Tas van Ommen]	Accepted. Discussion and references for north-south phase relation (see-saw) has been improved, also following comments 870-874.
6-869	A	17:37		This section might address ENSO as a possible mechanism behind initiating abrupt climate change [Steven Clemens]	Accepted.

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6-870	A	17:39	17:40	I don't thing this is an accurate description - actually the start of a gradual cooling trend is in phase between Greenland and Antarctica. An anti-phase, simple seesaw has long been ruled out from methane synchronised ice-core records and modellers have made attempts to explain this (Stokcker and Johnsen 2003; Knutti et al. 2004). A 'southern lagged' seesaw may be the best model we have but it is still only a tentative model. [Mark Siddall]	Accepted.
6-871	A	17:40	17:41	Maybe it could be added the reference: Broecker, W. S. Paleocean circulation during the last deglaciation: A bipolar seesaw? Paleoceanogr. 13, 119-121 (1998), where the expression bipolar seesaw was coined or that from T. Stocker (Stocker, T. F. The seesaw effect. Science 281, 61-62 (1998)) [Eva Calvo Costa]	Accepted.
6-872	A	17:40	17:40	"see-saw" : The see-saw concept has to be used with caution as it does not consider the heat storage in the ocean (which explains the different temporal evolution of temperature in the N and S. See, e.g., Stocker and Johnsen, Paleoceanography, 2005) [Michel Crucifix]	Accepted.
6-873	A	17:40		Broecker (1998) and Stocker (1998) should be cited in refernce to "see-saw" [Katsumi Matsumoto]	Accepted.
6-874	A	17:41	17:41	P 6-17, line 41; you say “During DO events, salinity… I think you should specify warm or cold. From what I understand I assume it’s warm, but… and is it the Irminger Sea that you’re referring to in the section I commented on above? [Sidney Hemming]	Accepted.
6-875	A	17:41		the bipolar seesaw concept needs to be referenced here (Broecker, 1998, Stocker, 1998, Stocker & Johnsen, 2003) Broecker, W.S., Paleocean circulation during the last deglaciation: a bipolar seesaw?, Paleoceanogr., 13, 119-121, 1998; Stocker, T.F., The seesaw effect, Science, 282, 61-62, 1998.; Stocker, T.F., and S.J. Johnsen, A minimum thermodynamic model for the bipolar seesaw, Paleoceanogr., 18, 1087, 2003. [Thomas Stocker]	Accepted. Thanks for the full reference details.
6-876	A	17:46	17:48	I really like your closing sentence of that paragraph (46-48) [Sidney Hemming]	Accepted.
6-877	A	17:46	17:46	should be 'McManus et al. 2004' [Mark Siddall]	Accepted.
6-878	A	17:46		more uptodate is McManus, J.F., R. Francois, J.-M. Gherardi, L.D. Keigwin, and S. Brown-Leger, Collapse and rapid resumption of Atlantic meridional circulation linked to deglacial climate changes, Nature, 428, 834-837, 2004. [Thomas Stocker]	Accepted.
6-879	A	17:50	18:12	This section should mention the work by Roche et al, Nature, 2004 who gives new	Accepted.

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				constrains on duration and ice volume for an Heinrich events. [Elsa CORTIJO]	
6-880	A	17:50	18:2	This paragraph does not address the issue that H events occur in already cold periods. It should therefore be pointed out that they may have caused a shutdown, but that this cannot explain the DO coolings. I am sure all the authors have this in mind, but to avoid confusion it should be spelled out. [Eric Wolff]	Noted. Nowhere is any connection of cooling after DO events with iceberg release mentioned, so we do not see how such a misunderstanding could arise.
6-881	A	17:50		MacAyeal (1993) on binge/purge oscillations is the most appropriate reference, not Hemming 2004. [Katsumi Matsumoto]	Accepted.
6-882	A	17:52	17:55	This complex sentence needs to be simplified, broken in two, or something. [Michael MacCracken]	Accepted.
6-883	A	17:52	17:52	The iceberg volume during an Heinrich event can indeed be estimated from the planktonic oxygen 18 and the result gives a much narrower range than stated here, at least for H4 (Roche et al., Nature, 432, 379-382, 2004): 2 (±1) meters of sea level equivalent . The duration of the event is also quite constrained to 250 (±150) years. [Didier PAILLARD]	Accepted.
6-884	A	17:53		Comment on subsection 6.3.2. Although this section offers a comprehensive view of the mainstream theories on abrupt climate change, it obviates completely other authoritative voices that can be found in the literature, in particular several papers by Wunsch and coworkers: Palaeoceanography vol 15, 417 (2000); Quat. Sci. Rev. vol 22, 1631 (2003). Also, the emphasis in this section is exclusively focused on the ocean circulation at high latitudes: tropical dynamics have been set aside, although recently papers by Philander, Cane and others show some evidence that this could be important. Last, the role of the atmospheric heat-transport is not even mentioned, although recent calculations show that the oceanic heat-transport at mid and high-latitudes is s a very small fraction of the total (Trenberth and Caron. J. Clim. vol 14, 3433 (2001) [Eduardo Zorita]	Taken into account. We do not agree concerning the first point, but included the second point (role of tropics).
6-885	A	18:1	18:1	Models suggest [Gavin Schmidt]	What is the comment?
6-886	A	18:2	18:4	Keep in mind that shifts or local reduction in ocean convection, not necessarily associated with a shut down of the thermohaline circulation, may have global impacts (e.g., Renssen et al., Paleoceanography 2002, cited in chapter). In other words, one does not need a "collapse" of the THC to have global impacts. [Michel Crucifix]	Taken in to account
6-887	A	18:3	18:3	Change "operating" to "occurring" [Michael MacCracken]	Can't find this.

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6-888	A	18:4	18:4	"A similar mechanisms". Which mechanisms? [Andrey Ganopolski]	Accepted
6-889	A	18:4	18:7	Mentioning of Meltwater pulse 1A (MWP-1A) in connection with Younger Dryas is misleading. MWP-1A occurred during Boling and has nothing to do with onset of YD. [Andrey Ganopolski]	Accepted
6-890	A	18:4	18:12	This part overlaps with Section 6.4.2 (page 25 lines 15-40), which is also about the 8.2 kyr event. I suggest it should appear only once, which might reduce duplication. It has to be classified either as glacial-interglacial or Holocene. [Jonathan Gregory]	Rejected. The brief mention here is not enough overlap to be a problem.
6-891	A	18:4	18:4	Do you mean "e.g." rather than "i.e."? [Jonathan Gregory]	No.
6-892	A	18:4	18:5	To avoid confusion, change this to be "(I.e., the Younger Dryas, and the 8.2 ka event)" [Michael MacCracken]	Accepted
6-893	A	18:4	18:6	What does "this natural variability" referring to--is it to "significant changes of relative sea level" in preceding sentence--if so, then calling it change and variation is confusing; if not, need to make clearer. [Michael MacCracken]	Can't locate this.
6-894	A	18:4	18:7	A similar machanism is very unlikely for the YD, at least in connection with MWP1a. Indeed, it is well known that the Meltwater pulse 1a is dated around 14.2 ka, which is about 2 ka before the start of YD. There is therefore no possible direct physical connection between the 2 events (except if 14C can be this much wrong, which I doubt very much). [Didier PAILLARD]	Accepted
6-895	A	18:4	18:12	Meltwater-YD connection: This paragr. should be rewritten considering the latest lit.: J.T. Teller, M. Boyd, Z. Yang, P.S.G. Kor and A.M. Fard, Alternative routing of Lake Agassiz overflow during the Younger Dryas: new dates, paleotopography and re-evaluation, Quaternary Science Reviews 24, 1890-1905, 2005; L. Tarasov and W.R. Peltier, Arctic freshwater forcing of the Younger Dryas cold reversal, Nature 435, 662-665, 2005.; R.F. Spielhagen, H. Erlenkeuser and C. Siegert, History of freshwater runoff across the Laptev Sea (Arctic) during the last deglaciation, Global and Planetary Change 48, 187-207, 2005. [Michael Schulz]	Accepted
6-896	A	18:7	18:7	meltwater input didn't peak during the YD -- instead it was at a minimum [Michael Schulz]	Accepted
6-897	A	18:8	18:9	After 'linked to', it is more correct to write "one or more inflows ranging up to 7x10(power13) m(cubed) (i.e. up to 19 cm of sea level) within a few years (Teller et al., 2002; Clarke et al., 2004). The new reference is to glaciological modeling of the lake	Accepted

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				outburst floods beneath the ice dam. The lesser flood volume is because lake water below sea level at the time could not drain catastrophically. The reference is: Clarke, G.K.C., Leverinton, D.W., Teller, J.T., and Dyke, A.S. 2004. Paleohydraulics of the last outburst flood from glacial Lake Agassiz and the 8200 BP cold event. Quaternary Science Reviews, 23, 389-407. [C.F. Michael Lewis]	
6-898	A	18:8		The latest idea has the 8.2 k Agassiz outburst occurring in as little as 0.5 years (Clarke, G.K.C., D.W. Leverington, J.T. Teller, and A.S. Dyke, Paleohydraulics of the last outburst flood from glacial Lake Agassiz and the 8200 BP cold event, Quaternary Science Reviews, 23 (3-4), 389-407, 2004.). This seems to have been widely accepted and should be included. [Eric Wolff]	Accepted
6-899	A	18:9	18:12	P6-18 lines 9-12; this comes back to whether or not there is evidence for substantial influx of fresh water in these events. If your statement on P 6-17, line 31-32 is correct, then there is a fresh water forcing. Not sure how much large should be and I assume you dealt with that in the modeling section. [Sidney Hemming]	Taken into acount - p17 lines 31-32 were changed
6-900	A	18:11	18:11	Need to change "may" to "can" [Michael MacCracken]	Can't locate this
6-901	A	18:14	18:14	Weaver et al (2002) suggested that meltwater pulse IA at 14,600 yr BP could have come from Antarctica; by freshening the S Ocean it could have stimulated the Atlantic overturning and caused abrupt N Atlantic warming. This could be worth discussing in this section. Meltwater Pulse 1A from Antarctica as a Trigger of the Bølling-Allerød Warm Interval Andrew J. Weaver, Oleg A. Saenko, Peter U. Clark, Jerry X. Mitrovica. SCIENCE VOL 299 14 MARCH 2003 1709-1713 [Jonathan Gregory]	We discussed this in our group and do not find the agreement of this particular modeling result with paleodata convincing enough to include it here
6-902	A	18:18	18:18	8.2 ka event [Eva Calvo Costa]	taken into account
6-903	A	18:19	18:20	...freshwater input of the order of magnitude (?) deduced from... [Eva Calvo Costa]	accepted
6-904	A	18:19		a review of modeling abrupt climate change is Stocker, T.F., and O. Marchal, Abrupt climate change in the computer: is it real ?, PNAS, 97, 1362-1365, 2000 [Thomas Stocker]	noted
6-905	A	18:20	18:22	Wunsch has contested the possibility of freshwater input as causing a shutdown - consider this. [Stephen McIntyre]	noted - but we do not find his argument, which contravenes all evidence, convincing

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6-906	A	18:23	18:23	see #4. Replace "asynchrony..." by the different evolution of Antarctic temperature compared to Greenland temperature during DO events... [Thomas Blunier]	accepted
6-907	A	18:23		in Stocker & Johnson (2003) it was explained in detail why "asynchrony" is not an appropriate notion in north-south connections. "Bipolar seesaw" describes better the link between north and south. [Thomas Stocker]	accepted
6-908	A	18:29	18:29	Suppress reference to Marchal et al., 1999. It does not concern Pa/Th. [Michel Crucifix]	Accepted
6-909	A	18:32	18:32	Köhler et al. : the idea that changes in terr. C uptake modulated deglacial pCO2 was already investigated by M. Schulz, D. Seidov, M. Sarnthein and K. Stattegger, Modeling ocean-atmosphere carbon budgets during the Last Glacial Maximum - Heinrich 1 Meltwater Event - Bölling transition, International Journal of Earth Sciences 90, 412-425, 2001. [Michael Schulz]	Thanks - but we cannot cover all references, need to make a selection
6-910	A	18:33	18:34	Change "an" to "one"--and on next line change "variations" to "variation" [Michael MacCracken]	Accepted
6-911	A	18:37	18:54	It would be more logical to reverse the order of two last paragraphs on this page. [Andrey Ganopolski]	Accepted
6-912	A	18:37	18:37	Does "effect" mean magnitude or likelihood--this should be clarified. [Michael MacCracken]	Accepted
6-913	A	18:37	18:45	I would delete this paragraph. The general point made by Alley et al is a good one and the rebuttal is both complex and somewhat contradictory. It looks like in fighting within the paleo community. [James Shulmeister]	Accepted
6-914	A	18:38	18:38	However, drawing such a general circulation": replace with "Such a general circulation [Michel Crucifix]	Accepted
6-915	A	18:41	18:43	I don't understand this sentence: there is no relationship between ocean heat transport and resolution (it can be high or low, completely independently of resolution). The heat transport in itself is also not simply linked to the amplitude of past changes (it is by the way largely dominated by a wind-driven circulation that will probably not change). And the heat transport is furthermore not linked to the sensitivity of the THC in the future. The authors have here something in mind that should either be stated more clearly (by refering to some actual model simulations) or completely reformulated. [Didier PAILLARD]	Accepted, this text was removed (see also comment # 913)
6-916	A	18:44	18:45	Some model experiments were run under glacial (LGM) boundary conditions. But this	Accepted, see comment 915

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				also is quite incorrect since there is little variability during the LGM. The variability appears to be larger at intermediate glacial stages (stage 3, deglaciation, ...) which is even more problematic from a modeling point of view. [Didier PAILLARD]	
6-917	A	18:44	:45	This sentence makes little sense to me. It reflects a deterministic approach to modeling that we could reproduce these events if we exactly specified the forecings and boundary conditions perfectly [Robert Webb]	Accepted, see comment 915
6-918	A	18:45	:45	This is the right place to refer to Ganopolski and Rahmstorf, Nature, 2001. [Michel Crucifix]	Taken into acount
6-919	A	18:52		the work of Schmittner et al. (2002, Science) and Weaver et al. (2003, Science) needs to be referenced here. Schmittner, A., M. Yoshimori, and A.J. Weaver, Instability of glacial climate in a model of the ocean-atmosphere-cryosphere system, Science, 295, 1489-1493, 2002; Weaver, A.J., O.A. Saenko, P.U. Clark, and J.X. Mitrovica, Meltwater pulse 1A from Antarctica as a trigger of the Bølling-Allerød warm interval, Science, 299, 1709-1713, 2003. [Thomas Stocker]	See comment 901
6-920	A	18:54	18:54	Unless references are given, the phrase "can be" should be changed to "has the potential to be" [Michael MacCracken]	Accepted
6-921	A	18:56	20:22	I am a little surprised to see no references at all to the considerable body of work by Kurt Lambeck in this field. [Mark Siddall]	Noted. The focus of this section is on abrupt changes "in the glacial-interglacial record"! There are no good records for sea ice changes, although we do think sea ice is an important feedback in the abrupt changes discussed here. Drought is covered in the Holocene section.
6-922	A	18:56		Section 6.3.3: The information on sea level in this section and this chapter in total is very incomplete. This section contains only a few references by mainly one author whereas the literature is much richer. [John Church]	Noted. We do say that "The repercussions of these abrupt climate changes were global, although out-of-phase responses in the two hemisphere suggest that they were not primarily changes in global mean temperature." Space does not allow detailed discussion of regional patterns.
6-923	A	18:56		Section 6.3.3: Either in this section or in section 6.5, material needs to be included on	Gladly noted.

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				changes in sea level over the past 2000 years. This has been a growing interest from the paleo sea level community in the last several millenia because of its strong relevance to understanding 20 th century sea level changes and its impacts. [John Church]	
6-924	A	18:56		6.3.3. This section is overly brief. It omits any reference to the work of Lambeck (and co-authors) or Mitrovica since the TAR (for example, studies of sea level in Roman times (Lambeck et al., 2004, Earth and Planetary Science Letters, 224, 563-575; Sivan et al., 2004, Earth and Planetary Science Letters, 222, 315-330), studies of sea level during the holocene (Lambeck, 2002, American Geophysical Union, Geodynamics Series 29, 33-50), studies of sea level change over the last glacial cycle (Lambeck and Chappell, 2001, Science, 292, 679-686), and a general discussion of GIA modelling (Mitrovica, 2003, Quaternary Science Reviews, 127-133)). [John Hunter]	Accepted, will be fixed.
6-925	A	19:1	19:35	some more comprehensive references would be welcome. [Thomas Stocker]	accepted
6-926	A	19:3	19:3	… Even if no anthropgenic changes were currently… [Steven Clemens]	accepted
6-927	A	19:4	19:4	Replace "highly" by "measurable and significant" [Michel Crucifix]	accepted
6-928	A	19:4	19:4	"Relative sea level" may be in the glossary but it might be useful to define it in (). [Jonathan Gregory]	accepted
6-929	A	19:6	19:6	The primary cause of natural variability in sea level… [Steven Clemens]	accepted
6-930	A	19:7	19:10	Rewrite in laymans terms along these lines…. Earth's crust is still rebounding from being depressed by the sheer weight of the LGM ice sheet. [Steven Clemens]	overly colloquial
6-931	A	19:7	19:7	suppress "of the Late Pleistocene ice age". [Michel Crucifix]	accepted
6-932	A	19:10	19:10	For an uninitiated reader, it might help to say explicitly that the restoration of "gravitational equilibrium" means that the land moves vertically. [Jonathan Gregory]	Not good enough, the water in the oceans also moves horizontally, changing the mass distribution
6-933	A	19:16	19:18	Is 'a' needed with '2004' in citation dates of Peltier? There is only one Peltier 2004 in the reference list. [C.F. Michael Lewis]	accepted
6-934	A	19:18	19:18	This is chapter 6; perhaps the ref should be to chapter 5 (Section 5.5, specifically). [Jonathan Gregory]	accepted

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6-935	A	19:18	19:18	should read "(see Chapter 5)" [William Howard]	accepted
6-936	A	19:18		ref. to Chapter 6 not clear [Thomas Stocker]	accepted
6-937	A	19:19	19:19	What is meant by "see Chapter 6"? [Michel Crucifix]	accepted
6-938	A	19:19	:22	This material should be assessed by Chapter 4 and a consistent interpretation agreed. The time scale of when these large rates of sea level rise might occur need to be added here. Consistent information needs to be transferred to chapter 10. [John Church]	Agreed, discussions have taken place with chapter 5 but not with chapter 4 or 10
6-939	A	19:20	19:24	I'm confused by what "this signal" is. The signal measured by GRACE has to do with the movement of mass, but the signal which has to be corrected by -0.36 mm yr-1 is the absolute global sea level change measured by Topex/Poseidon, isn't it? [Jonathan Gregory]	GRACE is measuring the time dependenceof geoid height, ie absolute sea level. There is an intimate connetion between the T/P observation and GRACE
6-940	A	19:21	19:24	Please clarify that this estimate of the GIA contamination is computed for global mean sea-level change. [Donald Forbes]	accepted
6-941	A	19:21	19:21	What is the uncertainty on the -0.36 mm yr-1? [Jonathan Gregory]	The best estimate available is that derivative of comparing the results obtained from the ICE-4G(VM2) and ICE-5G(VM2) models. The formed gives -.28 mm per year, the latter gives -.36 mm per year. Estimates based upon unconstrained variations of the viscosity structure of the Earth model are not useful.
6-942	A	19:21		What are the error estimates on this number. I would be surprised if as many significant digits as presented are appropriate. [John Church]	ICE-4G(VM2) gives -.28 mm per year whereas ICE-5G(VM2) gives -.36 mm per year.
6-943	A	19:22		My understanding that the relevant numbers are already corrected for this effect. If not they should be and this section should be reworded. [John Church]	Steve Nerem and others have been employing these predictions of the correction, on which basis we must assume that they are acknowledged to be required. The issue concerns whether or not the effect is subsumed in the instrumental callibration.

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6-944	A	19:22	:22	line contains sentence that starts with a misplaced pronoun. "This means" [Robert Webb]	accepted
6-945	A	19:26	19:35	Section 4.6.3.2 appears to allow a non-zero contribution in recent millennia, whereas this subsection says "ceased by 4 ka". But in Section 6.4.1 (page 24 line 33-38) you say Peltier and Solheim suggest an ongoing contribution of 0.0-0.5 mm yr-1. That is in fact the same as the TAR estimate and could agree with Section 4.6.3.2; although obviously much smaller than during the main stages of deglaciation, it isn't negligible, so "ceased" is not the best description, I would say. I think it would be helpful to move the later material from Section 6.4.1 to this point, in order to be quantitative and to avoid fragmentation of the subject. [Jonathan Gregory]	It is unclear where this text came from. There is no Peltier and Solheim paper on this particular topic. This reference should be to Peltier (2002) and the text should say (line 37) “cannot contribute more than 0.1 mm per year to the modern rate of global sea level increase of approximately 1.85 mm per year (Peltier , 2002).
6-946	A	19:26	19:35	I agree with the conclusion but there is limited age control on the raised beaches in the Pacific and mid-Holocene ages have been assumed rather than determined in many cases. [James Shulmeister]	These high stands of the sea are in fact quite well dated. William R. Dickinson’s work appears to be the best in this regard, especially completeness of coveage. A good reference to his work is Earth Science Reviews 55 , 191-234, 2001. See especially Figure 5 for the dating control on the age of the highstands.
6-947	A	19:31		Could a more common term than "highstands" be used here? [Neville Nicholls]	Will add a definition
6-948	A	19:37	19:42	Regarding the magnitude of the LGM sea-level, we now know that the global ice volume equivalent sea-level is larger than the 120m (Yokoyama et al., 2001 Palaeo3 v165 p281). This is supported from both North Western Australia data as well as Barbados data after correcting the isostasy (Yokoyama et al., 2000 Nature). Tuning modeling based sea-level reconstructions directly onto the "raw" Barbados coral data poses serious problems since the area are undergone glacial isostatic adjustments due to the Laurentide ice sheets melting , and hence we corrected the effects not only the Barbados data but also other published data sets from Tahiti, Sunda Shelf, New Zealand, Papua New Guinea and North Western Australia (Lambeck et al., 2002 QSR v21p343). [Michel Crucifix]	The original Barbados compilation of U/Th dated RSL records pulished by Fairbanks(1989) did not rule out the additional meltwater pulse proposed by Yokoyama et al on the basis of the J. Bonaparte Gulf data set. However, the extended data set now available from this site includes samples that provide unambiguous control back to the conventional LGM age of 21 ka and these rule out the possibility of the meltwater pulse invoked by Yokoyama et al to argue that the glacial-interglacial rise of sea level was significantly in excess of 120m.

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					Furthermore this Barbados constrint is confirmed by the data set of Hanebuth et al (2000) from the Sunda Shelf. You must also be aware of the analysis of the J. Bonaparte Gulf record by Shennan and Milne (2003) who pointed out the lack of stratigraphic continuity between the cores employed as basis for the suggestion of the existence of a 19 ka meltwater pulse.
6-949	A	19:37	19:42	Gathering the many temporal and spatial data for sea-level are the key to reconstruct the reliable global melt water history curve, and to do that, we MUST not forget to correct glacio-hydro-isostasy (Lambeck et al., 2002 QSR v21 p415). This is also accepted by Peltier, Quat. Sci. Rev. 21 409-414 (2002), who recognises that a 130-135m ice-equivalent eustatic depression of sea level at the LGM is plausible as "the conclusion that [...] observations [...] are consistent with a net LGM depression of ice-equivalent eustatic sea level of about 120 m is dependent upon the details of the way in which the sea-level equation treats the computation of the water load that is added to a broad continental shelf which is initially exposed but which later comes to be inundated by the sea." [Michel Crucifix]	The idea of a 130m-135m LGM depression of eustatic sea level was indeed somewhat plausible in the absence of the extended set of data that are now available from the Barbados location.
6-950	A	19:37	19:42	The magnitude of this larger LGM ice volume equivalent sea-level (ie. 135m or so) was originally not well accepted from Paleoceanographic community because it did not match to the "conventional" sea-level measure ie. deep sea oxygen isotope results (eg., Shackelton, 1988 QSR v6, p183). The larger than 120m sea-level during the LGM required near freezing temperature at the deep sea. Later on, however, independent analyses of pore water oxygen isotope reconstruction done by Schrag et al (2002, QSR v21 p331; 1996, Science v272 p1930) and Adkins et al (2002, Science v298 p1769) also deep sea oxygen isotope data by Waelbroeck et al (2002, QSR v21 p295) and Rohling et al (1998, Nature v394 p162). [Michel Crucifix]	Reading of the Waelbroeck et al paper is that the error bars on the various reconstructions are such as to allow a range of eustatic sea level depressions from about 115m to about 130m. The extended RSL record from Barbados strongly prefers a value near 120m if one accepts the standard correction for local tectonic uplift at this site. The coral records from other locations such as Tahiti, do not extend to LMG and so cannot provide an additional constraint. Once data of the appropriate age from this and other locations become available further adjustments of the inference based upon Barbados alone may be necessary.

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6-951	A	19:37	19:42	In the modelling side, Milne et al (2002, QSR v21 p361) successfully reproduced the LGM sea-level as low as the one that published by Yokoyama et al (2000, Nature) and the glacial isostatic modeling code (cf. Lambeck et al., 2003, QSR v22 p309) was also independently validated by Mitrovica et al (2003, QSR, v22,p127). Peltier QSR 21 409-414 (2002) also established that these results were little dependent on the visco-elastic model of the lithosphere - asthenosphere system. Therefore a large number of researchers in the Palaeoceanography and Palaeoclimatology now recognize the magnitude of the LGM sea-level was larger than 120m. [Michel Crucifix]	Again the arguements in the suite of papers published in the 2002 QSR collection have been superceded by the existence of the extended Barbados RSL record.
6-952	A	19:37	19:52	In summary, the very strong tone adopted in these lines of the AR4, stating that the eustatic sea-level rise is 120 m and, on the other hand, that the occurrence of any rapid rise in melting rate earlier than the meltwater pulse that occured 14.2 k does not reflect the position of a vast community of scientists and can therefore not be accepted as definitive in the context of IPCC WG1. [Michel Crucifix]	The existence of any 19 ka pulse of meltwater is contradicted by both the extended Barbados RSL record and by the Sunda Shelf record. Furthermore the record from the J. Bonaparte Gulf employed to suggest the existence of this pulse is highly questionable on the basis of the lack of stratigraphic continuity between the different sedimentary cores from which the dated specimens were derived. See the paper by Shennan and Milne(2003-QSR).
6-953	A	19:38	19:42	I think the section on glacial-interglacial sea-level changes is biased towards one view. See K. Lambeck and J. Chappell, Sea level change through the last glacial cycle, Science 292, 679-686, 2001. and the discussion in QSR 21, 409-418 for alternative views and discussions [Michael Schulz]	Accepted, will add further references
6-954	A	19:41	19:42	Delete last part of sentence "and the ICE-5G (VM2) model fits to this data set" or rewrite to make clear what it means. [Donald Forbes]	Accepted, will re-write
6-955	A	19:44	19:44	Figure 6.4. The reconstruction of global sea level variations from oxygen isotopes in the Red Sea (Siddall et al., 2003, in Nature) is an independent one which might be useful to compare on the figure, or at least discuss in the text. [Jonathan Gregory]	In the revised Figure the error range derived from the Waelbroeke et al paper is shown on a new inset. These error bars nclude those provided by the Red Sea work of Siddal et al.
6-956	A	19:44	19:44	Figure 6.4: Specmap is a very poor approximation of sea level. It is based on planktonic forams (it is dominated by temperature, not ice volume) and it is quite different from other more precise estimations (like Waelbroeck et al., 2002) that do compare much better	Have added the Waelbroecke et data as an inset in the new Figure.

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				with corals data. This figure tends to suggest that sea level was -40 m during stages 5.1 or 5.3 or -80 m during stage 3, which we know is quite wrong. Why not putting some other data (like Waelbroeck et al., 2002) on this Figure ? [Didier PAILLARD]	
6-957	A	19:46	18:46	"The good fit the model to the data (Figure 6.4)…" I cannot see model results on this figure. [Andrey Ganopolski]	The caption to the revised Figure clearly points out that the curve thorugh the data is that provided by the prediction from the recently published ICE-5G(VM2) model
6-958	A	19:48	19:53	"… appear to rule out the occurrence of any rapid rise in melting rate earlier than … 14.2 ka". What about 10 m sea level rise during 19ka event? [Andrey Ganopolski]	The new data from Barbados extends the record back to the conventional LGM of 21 ka and appear to rule out the occurence of any such event at 19 ka.
6-959	A	19:48	19:48	As I understand it Claire Waelbroeck et al. 2002 provide a review of coral derived sea level estimates and use these estimates to scale benthic isotope records to sea level. I doubt therefore that this is the original reference for the value of 120 m. Deep sea oxygen isotopes have always been adjusted to match coral estimates - there is a risk of circular reasoning to cite this paper as the origin of the 120 m estimate, which I understand comes from submerged Barbados corals. I may be wrong in this but all of the above arguments also go for the citation of the Shackleton 2000 paper as provided here. [Mark Siddall]	The data from Waelbroecke et al are now included in the revised Figure. The issue here concerns the question as to what is being estimated by the coral derived records on the one hand and the deep sea sediment derived records on the other. The deep sea sediment derived inferences are expected to more directly estimate the eustatic (globally averaged) depression of sea level determined by the change in land ice volume. A single coral derived record, on the other hand, may be strongly influenced by local effects (eg on the Huon peninsula where the record is strongly influenced by tectonic uplift). The value of the specific coral record from the island of Barbados derives from the fact that it is only weakly influenced by tectonic uplift and furthermore is expected to provide an excellent estimate of the globally averaged , ice volume related, eustatic

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					depession itself (Peltier, 2002. QSR 21,377-396). Other available coral records are not as useful as they may not extend to LGM (eg Tahiti) or do not explicitly sample the LGM timeslice (eg Huon).
6-960	A	19:48		It would be well worth citing here the paper of Siddall, M., E. J. Rohling, A. Almogi-Labin, C. Hemleben, D. Meischner, I. Schmelzer and D. A. Smeed, 2003: Sea-level fluctuations during the last glacial cycle. Nature, 423, 853-858. [Ian Simmonds]	Limitations of space mitigate against the incorporation of many more references
6-961	A	19:50	19:52	According to Yokoyama and Lambeck , Nature (2000), "ice volumes for the LGM and early part of the late-glacial period remain poorly constrained in glaciologically-based model studies", such that "the rapid rise in sea level noted at 19,000 cal yr BP may provide an answer to the missing-ice problem". In Quat. Sc. Rev., (2002) 415-418, these authors further stress that calendar age uncertainties on the data by Hanebuth et al., 2000 do not allow to reject this hypothesis. Finally, Peltier QSR 21, 409-414 (2002) accepts that "correct this misfit would require some increase in total land ice melting". Furthermore, we now have not only from the North Australian data (Yokoyama et al., 2000;2001) but also from Irish sea area (Clark et al., 2004, Science v304,p1141; McCabe et al., 2005 QSR v24 p1673). [Michel Crucifix]	The extended Barbados record that is now available would appearto rule out the existence of any 19 ka meltwater pulse
6-962	A	19:50	19:50	The reference to an event that occurred at 14.2 ka (BP?) is out of the context and thus very difficult to follow as this event is not mentioned or described before. [Hugues Goosse]	Will clarify
6-963	A	19:50	19:50	what caused the meltwater pulse at 14.2 ka? [Stephen McIntyre]	This is widely believed to have been caused by the Bolling-Allerod warming that was itself due to the sudden re-invigoration of the Atlantic thermohaline circulation following the collapse forced by Heinrich event 1..
6-964	A	19:50	:50	line contains sentence that starts with a misplaced pronoun. "This is" [Robert Webb]	accepted
6-965	A	19:55	20:6	Since the sea level is higher in last interglacial than holocene related to a wamer climate, this implies that the land area may be drier, thus may induce more sand storm (or other processes), which reflect more sunlight. Is it possible this would have helped to precondition the climate situation that is sensitive to the change of Milankovitch forcing? I suspect that warmer climate may be more sensitive to the Milankovitch forcing than a	relevance unclear

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				cooler climate. [Aixue Hu]	
6-966	A	20:2	20:22	Line 2 says 5-7 m, lines 9 and 12 say 4-7 m. Should these not be the same? [Katsumi Matsumoto]	accepted
6-967	A	20:3	20:6	You are ignoring the assertion of NorthGRIP Project Members (2004) that Greenland was 5 degrees warmer but was not much reduced in size in the last interglacial. This contradicts the idea that the ice sheet contributed 3-4 metres of sea level. Their line of reasoning, which involves the small renland ice sheet, is logical, but they have in the end to rely on some disturbed ice at renland. I don't therefore see their contribution as decisive, but the balance of evidence is not represented here. [Eric Wolff]	accepted
6-968	A	20:8	20:22	A sign post should be added here to point the reader to the related discussion in Chapter 19 of WGII. In addition, reference should be made here to Oppenheimer and Alley, Climatic Change, 68, 257-267, 2005. [Michael Oppenheimer]	accepted
6-969	A	20:9	20:9	4-7 metres above modern during the LIG is quoted, but 5-7 metres is quoted in Chpater 4 [Rowan Fealy]	accepted
6-970	A	20:9	20:10	I assume Overpeck et al (in press) and Overpeck et al (2005) are the same? [Michael Mann]	accepted
6-971	A	20:9	20:22	I think the multiple references to Overpeck et al in press in this paragraph could be reduced (possibly to a single mention?). [Neville Nicholls]	accepted
6-972	A	20:9	20:12	As noted above, global sea level was 4-7m above modern during the LIG' (again on line 12). Actually, 'noted above' was '3-7m' on page 15, lines 34 to 35. [Mark Siddall]	accepted
6-973	A	20:9		The implications of LIG sea level highstand for the possibility of a rapid disintegration of the WAIS in the future is grossly overestimated here. While LIG sea level was 4-7 m above present, model simulations (Cuffey and Marshall, 2000; Tarasov and Peltier, 2003) suggest that Greenland might contribute up to 5 m to sea level. Thus, Antarctic contribution might be only one or two meters, which is a small fraction of WAIS. Moreover, the data indicate that LIG sea level was above the present already at the onset of the last interglacial (130 kaBP) and remained rather stable during LIG. Thereby, a smaller than present volume of WAIS might be a result of different dynamic of Termination II, rather than melting of Greenland or warmer LIG climate. [Andrey Ganopolski]	accepted
6-974	A	20:9		sea level is said 4-7m above modern at LIG, and is mentioned 5-7m on line 2	accepted

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				[Sylvie JOUSSAUME]	
6-975	A	20:9	:12	long and confusing sentence that is suggest 4-7m global sea level rise whereas it is noted above to be 5-7m [Robert Webb]	accepted
6-976	A	20:14	20:14	cf. contradition already discussed with p. 6-15, line 29. [Michel Crucifix]	accepted
6-977	A	20:14	20:14	How does one know that the previous IG was not warmer globally than today? With positive feedbacks associated with altered orbital variations, anything is possible. [Andrew Lacis]	accepted
6-978	A	20:14	20:14	citation for "no global" anomaly [Stephen McIntyre]	accepted
6-979	A	20:15	20:18	This seems a bit unlikely to me, as the sea level forcing of 3-4 m is rather small compared with the >100 m of sea level rise that forced most of the WAIS deglaciation. [Jonathan Gregory]	accepted
6-980	A	20:16	20:21	" the melting of the Greenland ice sheet, and perhaps associated oceanographic change, may have triggered the melting of a portion of the West Antarctic Ice Sheet (Overpeck et al., in press), in agreement with evidence found under this ice sheet (Scherer et al., 1998)" This very speculative - the timing of the proposed WAIS collapse is very poorly constrained, and could have occurred any time in the mid-to-late Pleistocene. [William Howard]	accepted
6-981	A	20:18	20:21	This is interesting and provocative, but needs to be considered and, if retained, must be consistent with statements in Chapters 5 and 10. [Donald Forbes]	accepted
6-982	A	20:18	20:21	This sentence doesn't belong in chapter 6, as it is a projection of sea level rise, which is covered by chapter 10 (and should include palaeoclimate inferences). [Jonathan Gregory]	accepted
6-983	A	20:21	20:21	Is the "ongoing collapse" of Antarcitc ice shelves really an accepted fact by everyone ? What are the references ? [Michel Crucifix]	accepted
6-984	A	20:21	20:22	I think it would be more appropriate to deal with this in chapter 4 (probably Section 4.6.2.3, which already has some remarks on it) than in chapter 6. I will point out this sentence in my comments on chapter 4. We can't say th+G39 warming is anthropogenic, in fact, since (a) we haven't successfully simulated the Peninsula warming (b) it is only an inference, rather than measured, that oceanic warming is relevant in the Amundsen Sea. [Jonathan Gregory]	accepted

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6-985	A	20:21	20:22	there have been 120 m of sea level rise in the current interglacial, so collapse is "precedented" [Stephen McIntyre]	The 120 m of sea level rise occurred during the transition into the current interglacial not during it!
6-986	A	20:21	20:22	I think that this sentence needs a rider admitting that the recent collapse of these ice sheets is due to advection of heat from the Southern Ocean due to increased zonal flow and is tied in part to inter-decadal scale variability. Then you can continue that scale of the collapse is unprecedented etc. [James Shulmeister]	Agreed, the issue of the importance of interdecadal variablity to the onoing warmning on the peninsula is an important one
6-987	A	20:21	20:22	Although Larsen B has not collapsed before, you should not ignore the evidence that another ice shelf on the Antarctic Peninsula (George VI, on the west side) is stable now, but apparently did collapse in the early Holocene (Bentley, M.J., D.A. Hodgson, D.E. Sugden, S.J. Roberts, J.A. Smith, M.J. Leng, and C. Bryant, Early Holocene retreat of George VI Ice Shelf, Antarctic Peninsula, Geology, 33 (3), 173-176, 2005.). It may be that a different oceanic regime affected George VI, but also it may be that Larsen B was stable in the early Holocene simply because it was grounded then. While current AP warming probably is important, it is not yet clear that it is unprecedented in the Holocene. [Eric Wolff]	accepted
6-988	A	20:22	20:22	attribution is not part of paleoclimate discussion [Stephen McIntyre]	accepted
6-989	A	20:24	24:38	This part of section 6.4 could benefit from a table summarizing the conclusions and uncertainties in each of these - greenhouse gases, glaciers, monsoon, etc. [Susan Solomon]	Taken into account, but it is hardly possible for the Holocene
6-990	A	20:33	20:33	Replace "changes in the climate response" by "responses of climate" [Michel Crucifix]	noted
6-991	A	20:33		replace "changes in climate responses" with 'and complex climate responses' [Robert Webb]	accepted
6-992	A	20:36		replace "Such large coverage" with 'Such extensive coverage' [Robert Webb]	accepted
6-993	A	20:52		I agree with this statement. However, (Muscheler et al., submitted) refers only to the last 1000 years. There is a paper that discusses the geomagnetic field uncertainties on these reconstructions (Snowball and Muscheler, submitted). The differences between 10Be and 14C records during the Holocene, that are not yet resolved, are discussed in (Muscheler et al., 2005) and (Vonmoos et al., submitted). [Raimund Muscheler]	Ref is changed to Muscheler et al, 2005
6-994	A	20:54	20:54	I suppose it should be “until just before 6 ka”. At least I am not aware about any data suggesting significant sea level increase after 6 ka BP.	accepted

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				[Andrey Ganopolski]	
6-995	A	21:4	21:4	Change "increase until" to "increase lasting until" [Michael MacCracken]	accepted
6-996	A	21:4		ref Monnin missing in list: Monnin, E., A. Indermühle, A. Dällenbach, J. Flückiger, B. Stauffer, T.F. Stocker, D. Raynaud, and J.-M. Barnola, Atmospheric CO2 concentrations over the last glacial termination, Science, 291, 112-114, 2001. [Thomas Stocker]	
6-997	A	21:6	21:6	Here Greenland CH4 results are cited. Chappellaz97 and Flückger02 refer to the original work of Blunier et al., Nature 374, 46-49, 1995. Please cite the original work. [Thomas Blunier]	accepted
6-998	A	21:6	21:6	"same level" --need to say what the preindustrial level was. [Michael MacCracken]	accepted
6-999	A	21:6		Flückiger et al., 2002 [Thomas Stocker]	accepted
6-1000	A	21:8		Flückiger et al., 2002 [Thomas Stocker]	accepted
6-1001	A	21:9	21:9	It is confusing here to have "greenhouse gas variations" causing "radiative forcing changes"--need to be consistent about what are variations and what are changes, and using both is really confusing (does it imply some sort of hysteresis?). [Michael MacCracken]	accepted
6-1002	A	21:9		CO2 - subscript [Eric Wolff]	accepted
6-1003	A	21:12	21:12	Figure 6.6: Top: It is impossible to link the references to the data plotted in the figure. From the colors it looks like Mouna Loa goes back to 8kyr BP. Obviously not true. [Thomas Blunier]	accepted
6-1004	A	21:12	21:12	Figure 6.6: Middle: Add to the caption which data belongs to the southern and which to the northern hemisphere. Add to the caption that the difference between north and south is real and not a measurement problem. [Thomas Blunier]	The caption will be improved
6-1005	A	21:12		Figure 6.6: include sulphate record (as done in TAR) [Thomas Stocker]	Rejected, space
6-1006	A	21:14	21:38	Any discussion of preindustrial ghg variations would be noteably incomplete without mentioning the results of Ferretti et al., Science, 309, p1714, 2005 - Unexpected changes to the global methane budget over the past 2000 years. This work shows large changes to the preindustrial methane budget with compensating human and natural factors. The compensation means that the total methane levels remain steady and the mix of	Rejected, not enough evidences, controversial

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				anthropogenic versus natural budget shifts only shows in delta13CH4. [Tas van Ommen]	
6-1007	A	21:14		Replace with “Holocene atmospheric greenhouse gas concentrations” [Vincent Gray]	Rejected, shorter wording
6-1008	A	21:16	21:16	Use "sheet" in place of 'shield'. [C.F. Michael Lewis]	accepted
6-1009	A	21:22	21:22	Change "past 7000 years to "7000 years preceding the Industrial Revolution" as this does not apply to time since then. [Michael MacCracken]	Accepted (pre-industrial)
6-1010	A	21:23	21:23	Change "remained" to "would be expected to remain" as this is hypothetical. [Michael MacCracken]	accepted
6-1011	A	21:26	21:38	It would be only fair to Ruddiman to mention that at least one modelling study (by Kutzbach and Ruddiman) appeared to produce glaciation under "Ruddiman forcing"! [Iain Colin Prentice]	Re-worded
6-1012	A	21:26	21:38	The discussion of the early Holocene anthropogenic release of methane proposed by Ruddimann has also been discussed in Schmidt et al., GRL., 2004, which argued that the correlation with orbital forcing in fact implied that only a very small change in methane would have occured during the Holocence in the absence of human activities. [Drew Shindell]	Noted (space limitations)
6-1013	A	21:26	:38	Keep this paragraph in th report. It outlines current debate about longer time scale anthropogenic warming in a succinct way. [Melanie Fitzpatrick]	accepted
6-1014	A	21:26	:38	The Ruddiman work is deterministic and given the critical evaluation of how long the previous interglacials last on page 15-16 is not worth mentioning and the discarding. [Robert Webb]	accepted
6-1015	A	21:31	21:33	Isn't Ruddiman's hypothesis also in conflict with MIS11 and not only stage 5, 7 and 9? [Eva Calvo Costa]	Noted??
6-1016	A	21:32	21:32	"including the analogy of the three previous interglacials with the Holocene". This sentence adds unfortunate confusion. Ruddiman was precisely using the analogy with other interglacials as a support to his theory, which has been critised because previous interglacials, except perhaps MIS 11, are not good analogs to the present Holocene. [Michel Crucifix]	accepted
6-1017	A	21:32		here it should be mentioned that MIS11 and MIS15.1 appear as rather long interglacials (ref. Siegenthaler et al., 2005, Spahni et al., 2005): Spahni, R., J. Chappellaz, T.F. Stocker, L. Loulergue, G. Hausammann, K. Kawamura, J. Flückiger, J. Schwander, D. Raynaud, V. Masson-Delmotte, and J. Jouzel, Pleistocene records of atmospheric methane	Rejected (to be redirected to the ice age section)

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				and nitrous oxide from Antarctic ice cores, Science, in press, 2005. [Thomas Stocker]	
6-1018	A	21:32		"including the analogy of the three previous". I agree with the argument, but I don't understand what you mean with this phrase. Spell it out more clearly. [Eric Wolff]	accepted
6-1019	A	21:33	21:33	What does "It" refer to--the hypothesis? [Michael MacCracken]	Rejected (the text is clear)
6-1020	A	21:36	21:38	two climate models fail to induce a glaciation- two out of how many models? [Rowan Fealy]	noted, see 1011, re-worded
6-1021	A	21:36	:38	If you must present the Ruddiman work then the last sentence weakens your argument since fully coupled dynamical earth system models do not simulate the onset of glaciation anyway. [Robert Webb]	noted, re-worded
6-1022	A	21:37	21:37	Use "Ruddiman (2003)" in place of '(Ruddiman, 2003). [C.F. Michael Lewis]	accepted
6-1023	A	21:40	22:17	The discussion of the paleotemperature record does indeed suggest warmer early-to-mid-Holocene 20th century during the Holocene" is not supported by the available paleorecords. climates. The fact that climate models do not reproduce a period of enhanced warmth globally seems to be the main rationale for stating that no global warm peiod similar to today occurred. The graphic asociated with this section, Fig. 6.7, suggests that global temperatures were likely warmer than today between about 10KBP and 5KBP. There are several recent articles that suggest tropical SST were warmer than today during times in the Holocene. Given the strong connection between tropical SST and global sfc temps, I would expect that it would be reflected in a warmer globe. [Henry Diaz]	No specific papers are mentioned keep for General discussion
6-1024	A	21:40	22:17	Page 22, line 14, it is mentioned that "…, these warm periods were not of global scale, nor consistent through seasons", while the discussion of the seasonal contrast was hardy mentioned above. This discussion of the seasonal signal is very important and in the discussion of the changes at the end of page 21, it should be mentioned, when possible, for which season the changes have been observed. [Hugues Goosse]	Taken into account - To the introduction
6-1025	A	21:40		"Was any part of the current interglacial period warmer than today?" The authors should be very explicit in this paragraph whether they are talking about annual warming or seasonal (summer) warming. Many of cited in this paragraph proxy data are only representative for the summer period. [Andrey Ganopolski]	Taken into account

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6-1026	A	21:42	21:43	Suggest changing to read "maxima to occur earlier and over shorter periods with increasing …" [Michael MacCracken]	Accepted
6-1027	A	21:50	21:50	As well Cheddadi et al (1997) than Davis et al (2003) have shown that the mid-Holocene period was warmer than today. As written after, a cooling of Southern Europe as well on continents than on Mediterrenean Sea (Kallel et al 1997) shows that the signal is not global.; Kallel N, Paterne M, Labeyrie LD, Duplessy JC, Arnold M (1997) Temperature and salinity records of the Tyrrhenian Sea during the last 18 000 years. Palaeogeogr Palaeoclimat Palaeoecol 135(1—4) : 97—108 ; ; Cheddadi, R., Yu, G., Guiot, J., Harrison, S. P., and Prentice, I. C., 1997. The climate 6000 years ago in Europe. Climate Dynamics, 13, 1-9. [Joel GUIOT]	Taken into account (ref added)
6-1028	A	21:50	21:51	Need to state over what seasons this is the case [Michael MacCracken]	accepted
6-1029	A	21:51	21:51	Kaufmann et al., 2004; Nesje et al., 2005). During this..... [Atle Nesje]	accepted
6-1030	A	21:52	21:53	What is 'northern temperature forest'? Is this northern 'temperate' forest? [James Shulmeister]	accepted
6-1031	A	21:53	21:53	Use "temperate" in place of 'temperature'. [C.F. Michael Lewis]	accepted
6-1032	A	21:53	21:53	Replace "temperature" by "temperate" [Martin Stendel]	accepted
6-1033	A	21:54	21:55	An early warm period has been long established in New Zealand. You might want to add McGlone (1988) [McGlone, M.S. 1988 New Zealand in Huntley, B. and Webb, T. III (Eds.) Vegetation History. Kluwer Academic Publishers pp557-599] alternatively if you want a more recent reference you could use Shulmeister (1999). [Shulmeister, J. 1999. Australasian evidence for mid-holocene climate change implies precessional control of Walker Circulation in the Pacific. Quaternary International 57/58: 81-91.] [James Shulmeister]	Noted, but space limitations
6-1034	A	21:55	21:55	cannot be accounted for by local summer insolation changes". May be better to state "does not seem consistent with ... [Michel Crucifix]	accepted
6-1035	A	21:56	21:57	citation [Stephen McIntyre]	Noted
6-1036	A	21:57	22:1	east Pacific warming. This statement is not backed by the quoted references. Rimbu et al. only consider the Atlantic, while Stott et al. (Nature) document a (this is the title) "decline	New Lorentz et al GRL paper

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				of surface temperature [...] in the western Pacific ocean in the Holocene". [Michel Crucifix]	
6-1037	A	21:57	22:1	Rimbu, Stott: comment [Stephen McIntyre]	rejected, not clear
6-1038	A	22:0		Figure 6.7 does not take into account the diffenrence between northern and southern Europe [Joel GUIOT]	Noted, should be improved
6-1039	A	22:1	22:17	How do you know that when there is no reliable global or northern hemispheric average temperature curve at present? Many paleo-records showed that there was ever warmer periods in summertime sometime during 9000-3000 yr. BP in northern hemisphere, and they probably occurred synchronously at least in northern hemisphere. Caution should be given to the modeling results in this aspect. [Guoyu REN]	Noted, re-worded
6-1040	A	22:2	22:3	Is this any degree of warmth, or (more likely) warmth similar to that of the late 20th century? [Neville Nicholls]	Taken into account, text clarified
6-1041	A	22:3	22:3	Most of the summarized evidence suggests a warm mid-Holocene, with a reorganization warming the south. Synchronous climate is advoced by Majewski et al 2004 - discuss. The negative conclusion rests entirely on dO18 from some ocean sediments - is there a possible explanation for these sediments e.g. salinity changes? If so, discuss. [Stephen McIntyre]	Rejected, Majewski et al., 2004 is cited and discussed further
6-1042	A	22:3		Add at end " but the evidence is not conclusive" [Vincent Gray]	Noted, re-phrased
6-1043	A	22:7	22:17	I suggest that this text should be augmented to address the ability of the models to simulate past moisture conditions. An assessment of this ability is important to understanding the potential meaning of future simulations of climate, especially in arid and semiarid regions. [Robert Thompson]	Accepted, one sentense added to explain
6-1044	A	22:8	22:9	Are these changes with respect to present or preindustrial conditions [Michael MacCracken]	Taken into account
6-1045	A	22:9	22:10	Kitoh et al would seem an appropriate cite here: Kitoh, A., and S. Murakami, Tropical Pacific climate at the mid-Holocene and the Last Glacial Maximum simulated by a coupled ocean-atmosphere general circulation model, Paleooceanography, 17, 1-13, 2002. [Michael Mann]	Taken into account
6-1046	A	22:10	22:12	This is very confusing--this could be read as indicating that the range of the estimated change that occurred from the mid-Holocene to the preindustrial period was from +0.06 to	Accepted, re-worded

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				-0.4, whereas I sense what is meant is that the mid-Holocene was +0.06 above the present climate and preindustrial is -0.4 below the present climate (which seems too small given how much change has occurred). So, I am confused. [Michael MacCracken]	
6-1047	A	22:15	22:17	Thank you - we should drive a stake through the hearts of all terms like "climate optimum" (optimum for what?) [William Howard]	accepted
6-1048	A	22:15	22:17	Is it really up tp IPCC to suggest that some terms no longer get used? It is the case with all these terms that they get abused and misused because they don't have an accepted set of dates. [Philip Jones]	Rejected, but will reprase to make intentions clear
6-1049	A	22:15	22:17	This is a very important statement and it should be told to the authors of other chapters (e.g., chapter 1). I would assume that this also applies to the terms "Little Ice Age" and Medieval Warm Period" as these are similarly limited and not global--but then the chapter goes and does boxes on some of these terms without putting quotes or something to indicate that these named periods are not really real. [Michael MacCracken]	accepted
6-1050	A	22:15	22:17	Yes! [Gavin Schmidt]	accepted
6-1051	A	22:17	22:17	there is a "be" missing between "should" and "abandoned" [Michael Mann]	accepted
6-1052	A	22:17		Replace "should abandoned" with "should be abandoned" [Katsumi Matsumoto]	accepted
6-1053	A	22:23	22:23	....evidence for 20th/early 21st century climate.... [Atle Nesje]	accepted
6-1054	A	22:27		line contains sentence that starts with a misplaced pronoun. "This is" [Robert Webb]	accepted
6-1055	A	22:31	22:31	....understood properly (Nesje et al., 2005). [Atle Nesje]	noted
6-1056	A	22:33	22:39	Records of glacial advance and retreat are one thing; but another is record of glacial non-melting, and for that there are numerous tropcal records that are relevant, as melting homogenizes the glacial core, wiping out any banded structure. These provide a way of determining the uniqueness of current glacial melting, for the banded structure that has now been destroyed was evident in the 1970s and 1980s. [Andrew Lacis]	Rejected, not yet supported by literature
6-1057	A	22:35		line contains sentence that starts with a misplaced pronoun. "This reduces"	Accepted

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				[Robert Webb]	
6-1058	A	22:41	22:41	Box 6.3 Figure 1: It is confusing that larger glaciers are plotted as lower ordinates. Invert the scale. [Thomas Blunier]	Rejected, the reason is that warm periods (higher temp) are normally plotted above the axis and the low temp – below.
6-1059	A	22:43	23:17	A simple diagram of timelines of the various expansions and retreats of glaciers in different regins would help readers understand the relationships between the various regions. This is important in the context of the statement on lines 2 and 3 of page 22. [Neville Nicholls]	Accepted, the fig will be chgnged
6-1060	A	22:49	22:52	Should the term Little Ice Age be used given what was said on lines 15-17 (and elsewhere in text)? Is this not giving credibility that there was really a global cold period when this is not really established? [Michael MacCracken]	accepted
6-1061	A	22:50	22:52	The Little Ice Age had a somewhat different extension in the Northern part of Scandinavia compared to the southern part. In the north we had the maximum glacier extent in the beginning of the 19th century, but that was not the Holocene maximum, which occurred prior to LIA. A good reference for Holocene glacier extent in Northern Scandinavia is : Karlén, W., 1973: Holocene glacier and climatic variations, Kebnekaise mountains, Swedish Lapland. Geografiska Annaler 55A (1): 29-63. Another referens dealing with the termination of the little ice age in the north is : Holmlund, P., 1993: Surveys of Post-Little Ice Age glacier fluctuations in northern Sweden. Zeitschrift f¨r Gletscherkunde und Glazialgeologie 29 (1): 1-13. [Per Holmlund]	taken into account, the paper is already included into cited review
6-1062	A	22:51	22:52	You could omit this comment about advance until the LIA, because glacier fluctuations in recent centuries are covered by Section 4.5.3; perhaps a reference to 4.5.3 would be helpful. [Jonathan Gregory]	Taken into account, the sentense re-written
6-1063	A	22:51	22:51	South of where? [Michael MacCracken]	Accepted
6-1064	A	22:51	22:51	....in the south, where.... [Atle Nesje]	Accepted
6-1065	A	22:52	22:52	Delete [mass balance] [Atle Nesje]	Accepted
6-1066	A	22:52	22:53	Delete the sentence starting with: The higher mountains.....Rewrite to: The distance between the equilibrium-line altitude and the highest topograpical feature suitable for glacier formation may explain the somewhat different mid-Holocene glacial inception	Accepted

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				between southern and northern Scandinavia. [Atle Nesje]	
6-1067	A	22:53	22:53	“mid-Holocene glacial inception” is rather unusual and confusing term [Andrey Ganopolski]	Accepted, re-wroded
6-1068	A	22:53	23:3	Recent analysis of sub-glacial fossil wood and peat in the Swiss Alps shows shorter glaciers than currently at 1450-1150 years BP and earlier at 1800-2300, 3400-5200, 5500-5700, 6500-7100 (peak deglaciation, authors infer almost complete disappearance of glaciers), 8050-9000 years BP. For 1800-2300 years BP an increase in the snowline compared to the current situation by 300 m is inferred. See Christian Schlüchter; Ueli Jörin (2004):Alpen ohne Gletscher? Holz- und Torffunde als Klimaindikatoren, in: Die Alpen, 6, p. 34-47 [Axel Michaelowa]	Rejected/accepted , the paper is not in the peer-reviewed journal , but Hormes et al., 2004 will be added
6-1069	A	22:55	22:55	Cite recent work by xxx [Stephen McIntyre]	See glacier box
6-1070	A	23:1	23:2	You could omit this comment about advance until the LIA, because glacier fluctuations in recent centuries are covered by Section 4.5.3; perhaps a reference to 4.5.3 would be helpful [Jonathan Gregory]	accepted
6-1071	A	23:5	23:5	The LIA, if it existed at all, certainly did not last until the 1920s. I would much prefer leaving out the term LIA and speaking just of the years when things happened. [Michael MacCracken]	Taken into account, text changed accordingly
6-1072	A	23:8	23:8	Use "from Baffin Island" in place of 'from the Baffin Islands'. There is only a single Baffin Island. [C.F. Michael Lewis]	accepted
6-1073	A	23:14	23:14	.....mid-Holocene plant remains allowing a more.... [Atle Nesje]	accepted
6-1074	A	23:15	23:15	Given the previous page's insistence on restricting inappropriate climate descriptors to relevant regions and time periods, associating the Little Ice Age with the 1920s does not really make sense.... [Gavin Schmidt]	accepted
6-1075	A	23:15		There are new results from recessions of Alpine glaciers: Hormes, A., B.U. Muller, and C. Schluchter, The Alps with little ice: evidence for eight Holocene phases of reduced glacier extent in the Central Swiss Alps, Holocene, 11, 255-265, 2001; Joerin, U.E., T.F. Stocker, and C. Schlüchter, Multi-century glacier fluctuations in the Swiss Alps during the Holocene, Holocene, submitted, 2005. [Thomas Stocker]	Accepted, ref. added

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6-1076	A	23:16	23:17	Thompson's dating of Kilimanjaro is very precarious. The assumed accumulation is implausibly low - it's only 50 m thick (as compared to 160 m at Quelccaya), but is dated to 11700 BP versus start of AD440 at Quelccaya. [Stephen McIntyre]	Noted, I know this point concerning the dating of Kili – we have to decide together shall we keep this reference or not – we cannot discuss the dating problem within the Holocene glacier box
6-1077	A	23:20		what is a "demised glacier" do you mean 'retreating glacier' [Robert Webb]	Accepted, the sentense is changed “were small or absent....”
6-1078	A	23:23	23:24	What is meant under “significant reorganization of the climate system”? [Andrey Ganopolski]	Accepted, the sentense deleted
6-1079	A	23:25	23:26	For clarity, change to read "impacts on the mass balances of glaciers" [Michael MacCracken]	accepted
6-1080	A	23:25	23:25	"glaciers" should be "glacial" (or the wording otherwise fixed). [Michael Mann]	accepted
6-1081	A	23:26	23:26	"discrepancy" is the wrong word here. "variations" or "variability" is more appropriate. Who is to say that the regional variations are not real and meaningful?? [Michael Mann]	accepted
6-1082	A	23:26		line contains sentence that starts with a misplaced pronoun. "This is" [Robert Webb]	accepted
6-1083	A	23:27	23:29	This observation is not palaeoclimate, and is covered by Section 4.5.3; I would suggest replacing it with a reference to 4.5.3. [Jonathan Gregory]	accepted
6-1084	A	23:27	23:27	Change "anti-phasing" to "opposing phasing" [Michael MacCracken]	accepted
6-1085	A	23:27	23:27	In addition to Six et al, there should be citation here to: Reichert, B.K., L. Bengtsson, and J. Oerlemans, Recent glacier retreat exceeds internal variability, J. Climate, 15, 3069-3081, 2002. [Michael Mann]	Rejected, ref is more relevant to ch 4
6-1086	A	23:27	23:27	The "anti-phasing" between the Alps and Scandinavia is not obvious on the figure (Figure 1, box 6.3)... May be it would be worth explaining that "shorter timescales" are decadal or interannual ones and therefore cannot be seen on this Figure. [Didier PAILLARD]	accepted
6-1087	A	23:28	23:28	The sentence “illustrated by the recent growth of glaciers in western Norway during the last decades” should be more precise and consistent with chapter 4, page 4-21, lines 7-11. [Hugues Goosse]	Accepted, re-phrased
6-1088	A	23:29	23:29	Addition of possible reference- Fealy, R and Sweeney, J. (2005). Detection of a possible	accepted

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				change point in atmospheric variability in the North Atlantic and its effect on Scandinavian glacier mass balance, International Journal of Climatology, 25, 1819-1833. DOI: 10.1002/joc.1231 [Rowan Fealy]	
6-1089	A	23:29	23:29	....latter example was the result of.... [Atle Nesje]	accepted
6-1090	A	23:30	23:30	.....NAO during the 1990s (Nesje et al., 2000; Nesje 2005). [Atle Nesje]	accepted
6-1091	A	23:30		following implies a post 1999 response so replace "following" with 'associated with' [Robert Webb]	accepted
6-1092	A	23:34	23:42	It is worth addressing the augmentation of the North American monsoon as well. [Robert Thompson]	Taken into account
6-1093	A	23:34	24:32	Same remark as for abrupt events. The division "significance of monson strength" and "what do we learn from equilibrium simulations" does not work well. For example, PMIP simulations, and others with EMICS, have allowed to learn al lot about monsoon dynamics. A separation : observations / modelling would probably work better. [Michel Crucifix]	Taken into account, change the title of the chapter, cross-ref given, restructured
6-1094	A	23:34	24:1	Have the community really solved the question of the mechanisms of changing monsoon strength over the Holocene or are we just closer for the African region with some need for additional work to resolve the issues over Asia and North America. I not sure we have really advanced that much beyond the understandings coming out of Kutzbach ground breaking work. [Robert Webb]	Taken account, added Asian monsoon and North America
6-1095	A	23:36		correct citation: : Jolly et al. 1998 [Joel GUIOT]	accepted
6-1096	A	23:37	23:37	Change "requires" to "resulted from"--this is not a current requirement. Or, if this is what it takes to make the Sahel in models, say so. [Michael MacCracken]	accepted
6-1097	A	23:38	23:38	Where this precipitation threshold (150-300 mm/yr) comes from has to be clarified. [Michel Crucifix]	Taken into account, re-phrased
6-1098	A	23:40	23:40	"Because of the intensified summer land-sea contrast, atmosphere-only models were able to capture a northward shift of the desert-steppe transition". This sentence is quite elliptical. Be more methodical. 1. Models do produce an increase in Sahel precipitation. 2. This is in relation with increased land-sea contrast boosting the African monsoon but not only: elements determining dry vs moist convection, formation of squall lines etc and teleconnections with Indian monsoon may be involved (); 3. The simulated increase in	Taken into account

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				insufficient in AGCM to explain the observed expansion of grass and shrublands, at least regarding our present knowledge of precipitation requirements by steppe and savannah. [Michel Crucifix]	
6-1099	A	23:44	23:54	As avoided so carefully in this paragraph, models are not able to reproduce the estimated change in Indian monsoon strength associated with orbital forcing; this may be relevant for forecasts of future changes in monsoon strength. Assessing the capability of models for future forecasts is one of the things that this chapter is supposed to be doing, and it does not help to avoid saying clearly when models are incapable of responding in a quantitatively realistic way. [Andrew Lacis]	Taken into account
6-1100	A	23:47		Change reference to Liu et al. [Katsumi Matsumoto]	Taken into account, changed as indicated
6-1101	A	23:48	23:49	The “late summer and autumn warming of the surface ocean” can only decrease (not enhance) land-sea (“temperature” is missing in the text) contrast. [Andrey Ganopolski]	accepted
6-1102	A	23:48	23:50	How can late summer warming of the ocean enhance land-sea contrast -- it reduces! [Thomas Karl]	accepted
6-1103	A	23:55	24:1	In the cited here paper of Levis et al. (2004), simulated “sparse grasses” during mid-Holocene do not extend from Sahel to Mediterranean coast, but only up to 22.5N, thereby no agreement with paleobotanic evidences is achieved. Moreover, citing of Levis et al. (2004) paper in the context of climate-vegetation is rather odd, because this is the only modelling study which shows a negative vegetation feedback for the summer monsoon. [Andrey Ganopolski]	Noted, the text changed accordingly
6-1104	A	24:5	24:36	This section about modelling should better emphasise the idea that we have progressed in our knowledge of monsoon and transient climate change during the Holocene, both with the help of increasingly sophisticated GCMs (including more feedbacks) and with the help of EMICS. [Michel Crucifix]	Noted
6-1105	A	24:5	24:31	Why the transient runs over the Holocene are they not discussed?. They could also bring interesting information about climate change during this period. [Hugues Goosse]	Noted, discussed in the following section
6-1106	A	24:5	24:33	There is a strong high latitude bias in this section [James Shulmeister]	Taken into account, sections on low latitudes are extended??
6-1107	A	24:8	24:8	Rather than "change" it would help to say "increase" if this is the case--provide more information. [Michael MacCracken]	accepted

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6-1108	A	24:9	24:13	How about sea surface salinity change? [Akio Kitoh]	rejected
6-1109	A	24:15	24:25	The mention of the assymmetry of treeline advance is confusing, unless it is explained what was asymmetric and what the possible causes might be. A partial treatment is given in the Kaplan et al. (JGR) paper. It would also be nice to hear whether coupled models can reproduce this. [Iain Colin Prentice]	Accepted, re-phrased
6-1110	A	24:17	24:17	Note that PMIP 6k simulations neglect the transient effects due to remanants of ice sheets persisting in Amercia until about 6k. In this case, the fact that PMIP models do not reproduce the observed asymmetry has probably more to do with the experimental setup that with the models themselves. [Michel Crucifix]	Taken into account
6-1111	A	24:17	24:19	Obviously, “coupled atmosphere-ocean models” cannot simulate “shift in the position … of boreal forest”. For this end, the have to be coupled (at least asynchronously) with vegetation model, as it was done in Wohlfahrt et al. (2004). [Andrey Ganopolski]	accepted
6-1112	A	24:21	:25	replace "Observed mid-" with 'The sense but not the magnitude of observed mid-' [Robert Webb]	accepted
6-1113	A	24:22	:25	replace "atmosphere-only models" with 'an atmosphere-slab ocean model' [Robert Webb]	To check with Bette
6-1114	A	24:27	24:31	not clear in which direction is the shift [Joel GUIOT]	Accepted, explained
6-1115	A	24:27	24:29	As a non-expert in this particular topic I cannot understand at all what this sentence says - I suggest a rewrite. [Mark Siddall]	Taken ito account, re-writen
6-1116	A	24:29	24:29	mid Holocene" should be "mid-Holocene [James Crampton]	Accepted
6-1117	A	24:30	24:30	"positive NAO-like shift" what is meant? a shift towards a predominance of the the positive phase of the NAO? [Michael Schulz]	Taken ito account, re-writen
6-1118	A	24:31	24:31	It would be clearer to say "Greater NAO variability" as "more positive" is one direction--or if this means the NAO is more likely to be in the positive phase, this needs to be said. [Michael MacCracken]	Noted
6-1119	A	24:31	24:31	not clear what "more positive NAO variability" means. Greater variability? Needs to be clarified. [Michael Mann]	Noted

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6-1120	A	24:33	24:38	I suggest moving this material to Section 6.3.3 (as remarked on page 19 line 26). [Jonathan Gregory]	Accepted, to move
6-1121	A	24:33	24:38	This entire section is unnecessary and redundant from 3rd paragraph of page 6-19. [Katsumi Matsumoto]	Noted
6-1122	A	24:35	24:35	Change to "by a long-term" [Michael MacCracken]	Noted
6-1123	A	24:36	24:38	Is the value of 0.5 mm/yr consistent with Chapter 4 which quotes a recent value of 0.1mm/yr which increases to 0.2 mm/yr? [Rowan Fealy]	Check for consistency with ch 4 - Dick
6-1124	A	24:36	24:36	Typo in "isostatic". [Jonathan Gregory]	Accepted
6-1125	A	24:36	24:38	This should not be written so negatively. If the long-term changes in the polar ice sheet regions are contributing 25% to the observed sea level rise, that is an important contribution, and must be included when attempting to balance the budget of observed (or estimated) current sea level change. [Andrew Lacis]	To be discusssed in ch 4
6-1126	A	24:36	24:36	isostatic" instead of "isotostatic [Didier PAILLARD]	Accepted
6-1127	A	24:40	25:10	The draft appears to acknowledge that there is cyclic behavior in the climate system, but goes on to state that it is too small to account for recent warming and that because of the lack of consistency in the various data sets, the reasons for this cyclic behavior cannot be established. However, it does not address one critical question: How large a contribution is cyclic behavior making to current warming? This question needs to be addressed, even if the answer is that we cannot make that assessment. [Lenny Bernstein]	rejected, no evidence of clear cyclicity
6-1128	A	24:40	25:10	Cyclic behavior is a major feature of the climate system, even if it is too small to explain recent warming. Policymakers need to have the author's best estimate of the contribution this cyclic behavior is making to current warming, or an explanation of the reasons why that estimate cannot be made. The current text refers to lack of consistency in the data sets, but a fuller disucssion is warrented. [Jeffrey Kueter]	rejected, no evidence of clear cyclicity
6-1129	A	24:44	24:44	Add "at" after 'climate variability'. [C.F. Michael Lewis]	accepted
6-1130	A	24:44	24:47	The text sounds like Bond et al is not right. Is this really so? Otherwise, rephrase sentence to treat them evenly. [Katsumi Matsumoto]	noted

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6-1131	A	24:44	24:44	....Gupta et al., 2003; Nesje et al., 2005). Although..... [Atle Nesje]	accepted
6-1132	A	24:44		add references: Mann et al. 1999, Wapple et al.2002. Mann et al., 2003. Add to the reference list:Mann, M. E., R. S. Bradley and M.K. Hughes, Northern hemisphere temperatures during the past millennium: Inferences, Uncertainties, and limitations, Geophys. Res. Lett., 26, 759-762. 1999. Mann, M. E., and P. D. Jones, Global surface temperatures over the past two millennium, Geophys. Res, Lett., 30 (15)1820, doi:10,1029/2003/GL017814,2003. Wapple, A. M., M. E. Mann, R. S. Bradley, Long-term patterns of solar irradiance forcing in model experiments and proxy based surface temperature reconstructions, Climate Dynamics, 18, 563-578, 2002. [Joan Feynman]	Rejected, unappropriate for Holocene, relevant for last millennium section, sited there
6-1133	A	24:47	24:47	Capitalize "North". [C.F. Michael Lewis]	accepted
6-1134	A	24:50	24:50	IPCC needs to get its story straight was there or was there not a LIA --- this langauge seems to revert back to this notion whereas in 2001 is was discounted. [Thomas Karl]	Accepted, with quotes
6-1135	A	24:50	24:50	Again, should "Little Ice Age" be used--or would it not be better to just give the spread in years (and, of course, earlier it was said there were no proven cyclic fluctuations). [Michael MacCracken]	Taken into account, re-phrased
6-1136	A	24:51	24:51	ca. 1500-1920 AD) is the most [recent evidence suggests to extend the LIA to ~1920] [Atle Nesje]	Taken into account
6-1137	A	24:51	24:51	[Wrong reference used, use:] ... (Nesje and Dahl, 2003). In several.... [Atle Nesje]	accepted
6-1138	A	24:52	24:52	Presumably you mean "glacier" advances. [Jonathan Gregory]	accepted
6-1139	A	24:52	24:52	Add "the North" before 'American Cordillera'. [C.F. Michael Lewis]	accepted
6-1140	A	24:52		Spitzbergen or Svalbard (use consistently thoughout AR4, and in this chapter see 6-23, line 4) [Thomas Stocker]	accepted
6-1141	A	24:54	24:56	to compare Holocene variability with 20th century variability, we need annual resolution proxy series. They are not frequent and when they exist (e.g. tree-rings), they are not always properly indexed. Adequate tree-ring data show that heat waves such known in Europe in summer 2003 occurred many times during the Holocene, but they are not global and then they are not caused by the same mechanisms than for the recent heat waves	Rejected, not relevant for Holocene

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				(Guiot et al., subm). [Joel GUIOT]	
6-1142	A	24:54	24:56	Claim is unsupported. [Stephen McIntyre]	Noted to be revised
6-1143	A	24:54	24:56	We cannot deduce millenial variability for the last century - so this sentence reads somewhat strangely (although I understand what you mean) [Neville Nicholls]	Taken into account, re-phrased
6-1144	A	24:54		Add a sentence before "In most records": Others have stressed the contribution of prolonged periods of low solar activity to European cold periods (Ruzmaikin et al., 2004). Add to the reference list: Ruzmaikin, A., J. Feynman, Xun Jiang, David Noone, Anne M. Waple and Yuk L. Yung, The pattern of northern hemisphere surface air temperature during prolonged periods of low solar output, Geophys. Res. Lett. 31, L12201, doi10,10` 29/2004GL919955,2004. [Joan Feynman]	Rejected, relevant to 6.5/ch 9
6-1145	A	25:1	25:3	This would be a good point to mention that one should not assume, as these cited authors do, that 10Be and 14C variations are necessarily a proxy for solar activity. [Iain Colin Prentice]	Taken into account, ref to the beginning of the section
6-1146	A	25:1		After "correlations between" add: "well established long term changes in solar output (Eddy, 1976) including ", then continue sentence as stands. [Joan Feynman]	Rejected, see ch 3
6-1147	A	25:1	:10	This does idnefiy the solar problem, but gives no insight to where the IPCC will go with the issue. There really needs to be some suggestion as to how to reconcile solar variability with climate change. [Lee C. Gerhard]	Noted, See ch 9
6-1148	A	25:2		Replace the words "solar activity" with "solar radiative and particle outputs". [Joan Feynman]	Rejected see ch 3
6-1149	A	25:2		Magny (1993) has found good correlation between cosmogenic isotopes and lake levels curves in Jura – sentence like in lines 8-10 tends to avoid too quickly the debate existing with solar activity Magny, M. 1993. Solar influences on Hoocene climatic changes illustrated by correlations between past lake-level fluctuations and the atmospheric 14C record. Quaternary Research 40:1-9. [Joel GUIOT]	Noted, but the lack of space
6-1150	A	25:3	25:3	Add following reference: Clemens, S.C., 2005, Millennial-band climate spectrum resolved and linked to centenial-scale solar cycles, Quaternary Science Reviews, 24, 521-531.	Noted, Valerie

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				[Steven Clemens]	
6-1151	A	25:3		Add to the references "Ruzmaikin et al. 2004". [Joan Feynman]	Noted
6-1152	A	25:3		Should Solanki, 2004 Nature get a reference mention here? Also, potentially relevant is the National Research Council report "Radiative forcing of climate change: Expanding the concept and addressing uncertainties." Committee on Radiative Forcing Effects on Climate Change, Climate Research Committee, Board on Atmospheric Sciences and Climate, Division on Earth and Life Studies, The National Academies Press, Washington, D.C. [Tas van Ommen]	Rejected, see forcing
6-1153	A	25:6	25:10	Well said. [Michael MacCracken]	accepted
6-1154	A	25:6		Omit line beginning with "in many records and ending line 8 with the words "submitted". This sentence seems to imply that any solar forcing must demonstrate unchanging periodic variations. However, the Sun is not strictly periodic (e.g. Eddy, 1976; Feynman and Fougere, 1984). In addition since the mean global temperature is subject to many influences, the solar influence may often be overwhelmed, as it is for a few years after volcanoes and also as it has been since 1970. Add to the reference list:Feynman, J. and P. Fougere, Eighty-eight year cycle in solar terrestrial phenomena confirmed, J. Geophys. Res., 89, 3023, 1984. [Joan Feynman]	Noted, see ch 9, and 6.5
6-1155	A	25:8	25:10	Pleass indicate that observational data over the past few decades can help clarify [Thomas Karl]	Taken into account
6-1156	A	25:8	25:10	lack of consistency may simply point to poor proxies [Stephen McIntyre]	noted
6-1157	A	25:8		Omit "the current lack---- attribute". Add: "It is important to find means of clarifying the attribution of" . That last sentence will now read "It is important to find means of clarifying the attribution of "the century and longer time scale climate variations to solar variability ------." I would like to comment further that it does not seem to be so difficult to distinguish between solar forcing and volcanic forcing since volcanic forcing only lasts a few years for any particular eruption but modeling shows that solar forcing is damped by the temperature inertia of the ocean on decadal periods and is more evident for longer term variations of the solar output such as the Gleissberg cycle (60-100 year frequency) or the Grand Maxima and Minima. [Joan Feynman]	Reject, see ch 9
6-1158	A	25:9	25:9	…longer time scale climate variations to direct solar…. [Steven Clemens]	accepted

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6-1159	A	25:9		I do not completely agree since there are records that show a convincing solar climate connection. I agree with the problem of a lack of consistency. Therefore, I suggest to write: … to attribute the century and longer time scale variations in global climate to solar variability … [Raimund Muscheler]	accepted
6-1160	A	25:11	25:12	citation [Stephen McIntyre]	rejected
6-1161	A	25:12	25:12	Perhaps this could be about "variability", not just "abrupt change". The ENSO part isn't really abrupt. [Jonathan Gregory]	Taken into account
6-1162	A	25:14	25:30	8.2 kyr event : given an estimate of the discharged volume. Important because 8.2 event is a validation target for GCMs. [Michel Crucifix]	accepted
6-1163	A	25:15	25:40	This part overlaps with Section 6.3.2 (page 18 lines 4-12), which is also about the 8.2 kyr event. I suggest it should appear only once, which might reduce duplication. It has to be classified either as glacial-interglacial or Holocene. [Jonathan Gregory]	Accepted, a cross-reference is given
6-1164	A	25:17	25:25	Eos article suggests that ourburst was through Coppermine and dates do not match. [Stephen McIntyre]	rejected
6-1165	A	25:20	25:20	....that was [delete 'first'] recognised...[this 'event' was first recognised by Karlén in 1976 in lake sediments in Northern Sweden and later by Dahl and Nesje 1994 and 1996 in peat and lake sediments in southern Norway (they termed it the Finse event), before it was recorded in Greenland ice cores. [Atle Nesje]	noted
6-1166	A	25:21	25:21	“This event [8.2 ka event] is widely believed to have occurred during a cooling period”. I am not sure what the authors would like to say here. My understanding is that 8.2 ka event was just a cold event. [Andrey Ganopolski]	Accepted
6-1167	A	25:21	25:21	This seems strangely phrased--given that Lake Agassiz was likely growing and so it was likely warming, might not the release of the water created the cooling period rather than having occurred during a preexisting cool period? [Michael MacCracken]	Accepted
6-1168	A	25:22	25:23	Replace 'consequence….free of ice cover.' with "consequence of "outburst floods" during which Lake Agassiz drained catastrophically beneath its ice dam into Hudson Bay and the Atlantic Ocean (Rensson et al., 2001; 2002; Clarke et al., 2004; Neje et al., 2004;	noted

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				Wiersma and Rensson, 2005)." See comment for p. 18 for the Clarke et al., 2004 reference. Other new references are: Rensson, H., Goosse, H., and Fichefet, T. 2002. Modeling the effect of freshwater pulses on the early Holocene climate: the influence of high frequency climate variability. Paleooceanography, 17(2), 10-1 to 10-18., and Wiersma, A.P. and Rensson, H. 2005. Model-data comparison for the 8.2 ka BP event: Confirmation of a forcing mechanism by catastrophic drainage of Laurentian Lakes. Quaternary Science Reviews, in press. [C.F. Michael Lewis]	
6-1169	A	25:23	25:27	Add to sentence:", and in eastern North America (Spooner et al., 2002). The new reference is: Spooner, I., Douglas, M.S.V., and Terussi, L. 2002. Multiproxy evidence of an early Holocene (8.2 kyr) climate oscillation in central Nova Scotia, Canada. Journal of Quaternary Science 17, 639-645. [C.F. Michael Lewis]	Taken into account
6-1170	A	25:23		the work of Barber et al 1999, Nature, needs to be ref. [Thomas Stocker]	accepted
6-1171	A	25:25	25:25	I cannot see the 8.2 kyr event in the McManus et al. 2004 record [Mark Siddall]	Noted (Eystein)
6-1172	A	25:27	25:28	How can the magnitude and the rate of response by the same? It would be clearer to say, assuming this is what is meant: "The magnitude of the response in Greenland is estimated to have been 6 C, with most of the cooling occurring over a 5-year period. [Michael MacCracken]	Accepted
6-1173	A	25:27	25:27	Barber et al., 1999; Nesje et al., 2000; McDermott et al.,.... [Atle Nesje]	accepted
6-1174	A	25:27	25:27	McDermott et al result has been withdrawn [Gavin Schmidt]	accepted
6-1175	A	25:27	25:27	Risebrobakken et al result is for a cooling at the surface in the East Norwegian Sea - it is not a direct estimate of MOC change. [Gavin Schmidt]	accepted
6-1176	A	25:27		McDermott paper: evidence has been presented at meetings this year that the Crag Cave data were an analytical artefact (the same authors presenetd a parallel analysis with no visible event). It should probably be left out of this list. [Eric Wolff]	accepted
6-1177	A	25:28	25:28	Add to reference citation as "(Alley et al., 1997; Alley and Agustsdottir, 2005)". The new reference is: Alley, R.B. and Agustsdottir, A.M. 2005. The 8k event: cause and consequences of a major Holocene abrupt climate change. Quaternary Science Reviews, 24, 1123-1149.	noted

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				[C.F. Michael Lewis]	
6-1178	A	25:28	25:28	Alley's estimate of 6 deg is local to Summit and relies on a coherence of d18O and temperature at very short time scales that may not be appropriate given an enhanced source of depleted melt water as a source for Greenland precipitation at this time. More recent estimates give a decadal change of around 3 deg C, but that is still unpublished (Severinghaus, pers. Communication). You might want to simply allow for the possibility of revision in the future. [Gavin Schmidt]	Taken into account
6-1179	A	25:28		there is independent evidence from air isotopes giving a cooling of 7.4 C: Leuenberger, M., C. Lang, and J. Schwander, d15N measurement as a calibration tool for the paleothermometer and gas-ice age differences. A case study for the 8200 B.P. event on GRIP ice, J. Geophys. Res., 104, 22163-22170, 1999. [Thomas Stocker]	Rejected, not confirmed
6-1180	A	25:32	25:40	It would be good to point out here the significance of the 8.2 k event as one in which we think that we have some understanding of a freshwater forcing and we can see what were the climate effects under conditions in which mnany of the boundary conditions were similar to today. Model testing etc. [Eric Wolff]	Noted, reference from “abrupt” session
6-1181	A	25:33	25:34	Replace "polar northern hemisphere" with "the Arctic" [Michael MacCracken]	accepted
6-1182	A	25:35	25:38	This sentence seems to me quite unclear--does it mean that the extent of the southward shift depends upon the magnitude of the change in the equilibrium response of the model? And the relationship of the model's high frequency response and the freshwater forcing needs to be explained--that is, does high or low frequency variability lead to more or less of a response to the freshwater forcing--a clearer explanation of the physics is needed. [Michael MacCracken]	Noted to clarify
6-1183	A	25:38	25:38	Insist on Renssen, 2002 bringing up the notion of unpredactibility of the consequences of an abrupt meltwater discharge (it may last 20 or 500 years, depending on the exact initial conditions). [Michel Crucifix]	Taken into account
6-1184	A	25:38	25:40	The lines starting with “Within PMIP2, …” until “… mean states." Should be suppressed as those experiments are not discussed in this report. [Hugues Goosse]	Accepted
6-1185	A	25:38	25:40	PMIP2 future plans are not relevant to the assessment of current research. [Gavin Schmidt]	accepted
6-1186	A	25:39		replace "vulnerability' with 'sensitivity'	Rejected, in climatology “sensitivity”

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				[Robert Webb]	has a specific meaning
6-1187	A	25:42	25:49	Some climate index like sea ice is basically a presence/absence type of index. One cannot have a gradual climate change using such index. The inferred climate change would have to be abrupt. This should be accounted for. [Katsumi Matsumoto]	Noted - to Introduction?
6-1188	A	25:42	25:49	These evidence are not efficient. You could find much more records indicating a warmer period in early to mid Holocene, but you don't want to find them and to say so....Nearly all of the rapid change around North Atlantic Ocean occurred in glacial periods or stages of glacial-to-interglacial transition, and they obviously demanded the condition with ice-sheets in North America and Europe. The posssiblity for the rapid change to occur in current interglacial period would be extremely low. [Guoyu REN]	First part – rejected – covered in 6.4.1. Second part – rejected - see definition of abrupt events 6.3. Glossary
6-1189	A	25:42		5 and 4 ka (and not 5'000 and 4'000). [Heinz Wanner]	Accepted
6-1190	A	25:44	25:46	Rather than saying "transport of moisture to central Greenland" perhaps say that it leads to buildup of snow and ice on central Greenland as transport just does not give a good indication of what is happening. Also on line 44, need to replace "change" with a word giving the sign of the influence; similarly on line 46, given an indication of the types of "changes" in South American climate. And on line 45, it would help to explain what "century-scale" means--does this mean drought conditions typically persisting for about a century? [Michael MacCracken]	Accepted (the sentence is partly re-writen)
6-1191	A	25:45	25:45	....Lauritzen, 2003; Nesje et al., 2005), widespread.... [Atle Nesje]	Accepted
6-1192	A	25:46	25:49	The phrase "under gradual climate forcings" implies that these forcings are somehow responsible for, at the very least, setting the stage for the rapid climate changes. Since the last sentence indicates that we don't know what caused the rapid climate changes, this is a misleading inference. It should be removed. [Andrew Lacis]	Taken into account (sentense removed)
6-1193	A	25:46	25:47	Is it really so clear that the only forcing was gradual? Might there not have been other forcings that we do not yet recognize--land cover changes, methane releases, recovery of sea level, etc.? Do we know these were internal tipping points or nonlinearities--or might they have been externally induced? And are these really large-scale global changes--what the word is generally reserved for--or were these regional shifts that were counterbalanced by opposing shifts elsewhere--do we really have the data to know the scale and magnitude of what happened over the globe? [Michael MacCracken]	Taken into account (sentense removed)

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6-1194	A	25:46		we observe also, between 6ka and 3ka BP, abrupt drying in Africa (Gasse, 2001; Vincens et al, 1999), Europe (Magny, 2004), China (Jiang et al., in press) ; Gasse, F., 2001. Hydrological Changes in Africa. Science, 292, 2259-2260 ; Magny M. (2004). Holocene climatic variability as reflected by mid-European lake-level fluctuations, and its probable impact on prehistoric human settlements. Quaternary International 113,1, 65-79.; Jiang, W.Y., Guo, Z.T., Sun, X.J., Wu, H.B., Chu, G.Q., Yuan, B.Y., Hatté, C., Guiot, J., 2005. Reconstruction of climate and vegetation changes of Lake Bayanchagan (Inner Mongolia): Holocene variability of the East Asian monsoon. Quaternary Research, in press.; Vincens et al., 1999. Forest response to climate changes in Atlantic Equatorial Africa during the last 4000 years BP and inheritance on the modern landscapes. Journal of Biogeography, 26, 879-885. [Joel GUIOT]	Noted
6-1195	A	25:47	25:47	conclusion is not supported [Stephen McIntyre]	Taken into account (sentense removed)
6-1196	A	25:49		Add at end “or that current climate changes could be themselves classified as “abrupt”. [Vincent Gray]	Rejected (sentense removed)
6-1197	A	25:51	25:51	For me, the phrase "past abrupt monsoon change" is quite awkward--what about saying "significance of abrupt changes in monsoons in the past?" [Michael MacCracken]	Accepted
6-1198	A	25:51	25:57	It could be mentioned already here that the collapse of the green Sahara occurred at this time. [Iain Colin Prentice]	Accepted
6-1199	A	25:54	26:8	"All high resolution precipitation-sensitive records reveal that the transitions (not synchronous) from wetter conditions in the early Holocene to drier modern conditions occurred in one or more abrupt steps." It seems that not all records show the transition, and even the transition exists it might not indicate change in climate or monsoon. Some changes found in Asia might have been caused by human interference with terrestrial vegetation, and the vegetation change there was not responding to climate....(please see G. Ren, 2000, Decline of the mid-to late Holocene forests in China: climatic change or human impact? Journal of Quaternary Science, 15 (3), 273-281, and G. Ren, H.-J. Beug, 2002?Mapping Holocene pollen data and vegetation of northern China, Quaternary Science Review, 21 (12-13), 1395-1422). change in vegetation of northern Africa may also been induced mostly by human activities..... [Guoyu REN]	Rejected (papers cited here refer also to the evidences which are not influenced by humans, e.g. marine evidences)
6-1200	A	25:55		I don’t understand what you mean by the parenthetical "not synchronous". [Neville Nicholls]	Accepted, re-phrased
6-1201	A	25:56	25:56	What are "abrupt steps"? Are not all steps abrupt? Likely best to cross out "abrupt"	Accepted, removed

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				[Michael MacCracken]	
6-1202	A	26:10	26:38	I remain dubious about most studies of paleo ENSO behaviour. ENSO has a well-defined temporal and geographical structure, and simply noting that a single region had lower precipitation variability in the past does not imply, by itself, anything about ENSO. Lower precipitation variability would need to be diagnosed at many of the places affected by the modern ENSO. As well, we would need to be sure that this decreased variability was on inter-annual time scales, not century or millenial scales, if we are to deduce ENSO behaviour. [Neville Nicholls]	Noted (one sentense for explanation added)
6-1203	A	26:15	26:15	Could add a reference for central North American early Holocene evidence of ENSO, eg. Godsey, H.S., Moore, Jr., T.C., Rea, D.K., and Shane, L.C.K. 1999. Post-Younger Dryas seasonality in the North American midcontinent region as recorded in Lake Huron varved sediments. Canadian Journal of Earth Sciences, 36, 533-547. [C.F. Michael Lewis]	Noted
6-1204	A	26:18	26:19	"5.4-7.7 ka" should read "7.7-5.4 ka" to be consistent with the format of previously used values [Rowan Fealy]	Accepted
6-1205	A	26:18		[Add amended segment to paragraph – important to provide SH mid-latitude evidence of ENSO onset] "In New Zealand, only one well-dated, high-resolution proxy record has so far been retrieved that is long enough to elucidate the development and date the onset of modern ENSO (Gomez et al., 2004). This proxy record details synchronous textural variation in inter-correlated sediment cores from the Waipaoa flood-plain, continental-shelf (MD97-2122) and continental–slope (MD97-2121) settings of the eastern North Island, New Zealand. This signal which appears in all three sediment cores provides evidence of increased storminess after 4 ka and indicates the impact of intensified atmospheric circulation marking the establishment of the contemporary climate that is strongly ENSO influenced". Gomez, B., Carter, L., Trustrum, N.A., Palmer, A.S., Roberts, A.P., 2004. El Nino-Southern Oscillation signal associated with middle Holocene climate change in intercorrelated terrestrial and marine sediment cores, North Island, New Zealand. Geology 32, 653-656. [Brent Alloway]	Noted
6-1206	A	26:23	26:24	This is a sweeping assertion and I doubt very much that it is true. I suggest consulting someone who has been involved in data synthesis work on Australian pollen data, such as Joh Dodson.	accepted

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				[Iain Colin Prentice]	
6-1207	A	26:26		Replace "Paleoclimate model simulations" with "Paleoclimate simulations", since the word "model" is used immediately thereafter. [Katsumi Matsumoto]	Accepted
6-1208	A	26:29	26:30	If there is no space to explain the 'Bjerknes feedback mechanism' is there any point mentioning it? Would a short explanation be possible - is it described in another chapter for instance? [Mark Siddall]	accepted
6-1209	A	26:36	26:36	should read "Kutzbach, JE" [William Howard]	Accepted
6-1210	A	26:36	26:36	Replace "Otto-Bleisner" by "Otto-Bliesner". [Martin Stendel]	Accepted
6-1211	A	26:36		Correct Citation: Liu et al. 2004 [Joel GUIOT]	Accepted
6-1212	A	26:36	:38	I need to be convinced that this sentence is disconnected with the stated limitations in FAR chapter 8 page 44, lines 33-42: "Along the equator in the Pacific the models fail to adequately capture the zonal SST gradient and typically have thermoclines that are far too diffuse (Davey et al., 2002). Most coupled GCMs fail to capture the meridional extent of the anomalies in the eastern Pacific and tend to produce anomalies that extent too far into the western tropical Pacific. Most, but not all, coupled GCMs produce ENSO variability that occurs on time scales considerably faster than observed (AchutaRao and Sperber, 2002), although there has been some notable progress in this regard over the last decade (AchutaRao and Sperber, 2005) in that more models are consistent with the observed time scale for ENSO. Generally speaking, the models have too little low frequency variability (time scale longer then ENSO). Some of the weaknesses in the simulated amplitude and structure of the variability have been discussed in Davey (2002). " [Robert Webb]	Accepted, re-phrased
6-1213	A	26:37	26:38	"weakening under changed forcing and background state": could there be "forcing or background states" that would induce a strengthening. In other words, is this statement general enough ? [Michel Crucifix]	Taken into account (sentense re-written)
6-1214	A	26:40	32:12	This section is too long. Although I am myself a dendrochronologist, and I enjoyed reading all the information reported here, I believe that this section could be reduced, because it is too detailed. Also, the subtitle is a question (see page 26 line 44), and reading this section I was wondering when the answer was coming. I felt lost. A different structure of the section, maybe introducing some subheadings, would may of some help to the reader.	Rejected, authors believe space is needed for clarity

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				[Paolo Cherubini]	
6-1215	A	26:40		Even though, I was generally very impressed by the compilation of information in section 6.5 "The last 2000 years", I also got the impression that the wording throughout this section is somehow a bit un-balanced. For example, when describing the records combined in figure 6.8b, the D'Arrigo et al. 2005 reconstruction is introduced as including "SUBSTANTIAL AMOUNTS of SIMILAR tree-ring data as Esper et al. (2002) among their predictors", which reduces the value of this new D'Arrigo record. Such wording is then followed by, for example, "Briffa (2005) produced an EXTENDED history ... using MANY tree-ring width records ... using IMPROVED statistical techniques...". I am sensing that this is a difficult issue, but similar examples can be found at various other places in the draft, and should be carefully re-checked (by a native speaker). By the way, with the example of the D'Arrigo record, I do not believe that they used substantial ammounts of similar data than Esper et al. 2002, at least not for their N-American reconstruction. [Jan Esper]	Taken into account
6-1216	A	26:40		Section 6.5 is important but perhaps too long. [Jonathan Gregory]	rejected
6-1217	A	26:40		The readers may have a question whether or not the elevation of CO2 concentration during the 20th century affects the tree-ring. A comment on this point is necessary. [Kiminori Itoh]	Accepted – comment added to uncertainty discussion
6-1218	A	26:40		This undoutedbly will be the most contentious section in the Chapter - particulary in relation to 'hockey sticks'. The section does a good job in explaining the problems and the issues. [Bryant McAvaney]	accepted
6-1219	A	26:42	26:42	It would be helpful to start this key section with a table summarizing the types of data available (tree rings, ice cores, sediments, corals etc.), their strengths and sources of uncertainty (e.g., growing season definition, etc.), and length of records. It would also be helpful to note that the instrumental data of the 20th century generally show largest trends in winter (not summer), while most of the paleoclimate data is most sensitive to summer season temperatures. [Susan Solomon]	Taken into account – will consider in expanded intro section on uncertainties
6-1220	A	26:44		Delete “What do”, capital for “Reconstructions”; delete “tell us?” [Vincent Gray]	reject
6-1221	A	26:45	27:3	Add some words on the (differing) variances of the various instrumental records shown in Fig. 6.8a to avoid confusion by non-expert readers. These variance differences are also the reason why it is not very useful to combine these records for comparison with large scale T reconstructions, as discussed in my third comment of this review.	Accepted – more words

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				[Jan Esper]	
6-1222	A	26:51		Replace "significance" with words like "prominence". At this point in text, the recent warming is just prominent. Significance has not been established yet. [Katsumi Matsumoto]	accept
6-1223	A	26:57		Rather than showing single european instrumental series one could show the European mean temperature covering the last 500 years by Luterbacher et al. (2004, Science 303 1499-1503). [Heinz Wanner]	Reject, figure is instrumental, not proxy reconstruction. Luterbacher cited elsewhere in report.
6-1224	A	27:0	28:	I like the discussion about the Medieval Warm Period and the related Box 6.4. It is used to show that the warm maxima do not temporally correspond in different regions. It is also used to point to historical events like the colonization of Greenland. A large number of publications (see e.g. the PAGES books on the PEP transections) shows that the periods between 4.4 ka and 4 ka or 3.4 and 3 ka were warmer (and possibly drier) than the Medieval Warm Period, and the population growth e.g. in the western European Bronze Age was very high. Is there a plausible argument that these facts, which are regularly discussed by the international press, should not be mentioned? [Heinz Wanner]	Rejected in the specific content of this section – but point to be refered to other Lead Author(s)
6-1225	A	27:0		It should be mentioned that the comparison between "proxy" measurements from the past with surface measurements from the present is unfair since it does not compare like with like. When proxy measurements are used for the whole period (see Figs 6-8 and 6-9 and Box 6.4 Figure 1)recent warming either disappears or is less than was found in 1000 or 1400 AD. At least part of the larger increase for the surface measurements must therefore be due to the close proximity of the measurements to human habitation. The increase in some of the proxy measurements could be due to the increased growth of trees because of increased carbon dioxide in the atmosphere. [Vincent Gray]	Reject – but point about CO^2 fertilization of trees contentious and will be mentioned in revised text.
6-1226	A	27:0		Fig 6-8. It is statistically invalid and visually misleading to overlay the black instrumental line on this diagram. The coloured graph lines show proxy records that end by 1980. If you want a line that continues up to more recent years that then you must use proxy records that continue past 1980, not switch to a different type of series. There are up to date proxy records available, but as I'm sure the authors of this chapter are aware, they depart from the surface instrumental record, as they tend to decline after 1980. If you want a graph continuing to the present, and that's what the proxies show, so be it--let the reader see them. And if the reason for not showing them is that they are hypothesized not to be good representatives of temperature anymore, then by what right does the Figure insinuate that they were good proxies 8-10 centuries ago? It is no defence to claim that MBH99 established a statistically skillful relationship between the proxy network and the	Reject – not true that all proxy records decline after 1980. Neither do this Figure 'switch' to instrumental data post 1980 – it simply portrays the published evidence for calibrated reconstructions, along with independent (borehole and glacier) evidence and shows the instrumental data for comparison

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				instrumental data, since that claim has been refuted.McIntyre and McKitrick (2005a) showed that the pre-1450 RE statistic was incorrectly benchmarked, yielding a spurious inference, and the r2 stat calculated by MB&H themselves, which showed the lack of skill, was simply not reported. The squared correlation between the MBH long proxies and the instrumental record is nearly zero. The mean correlation between proxies and gridcell temperatures in the MBH98 data set (AD1400 portion) is -0.08 (McIntyre and McKitrick 2005b), and the RE significance benchmark is above the MBH98 RE score, using all available implementation of the Mann code (McIntyre and McKitrick 2005c). The surface instrumental record cannot be used as a statistically valid extrapolation for the proxies after 1980. By switching to the instrumental record at 1980, knowing that it provides an inaccurate picture of the continuation of the proxy trend, Figure 6.8 constitues a fundamental deception. [Ross McKitrick]	
6-1227	A	27:0		Fig 6-8. Having established the usefulness and validity of including ground borehole-based records on this compilation graph, an obvious omission is the long term global reconstruction of Huang, Pollack and Shen (1997). The post-1000 portion should be added to this chart. Reference: Huang, Shaopeng, Henry N. Pollack and Po Yu Shen (1997). "Late Quaternary Temperature Changes Seen in Worldwide Continental Heat Flow Measurements." Geophysical Research Letters 24: 1947—1950. References for above cell: McIntyre, Stephen and Ross McKitrick (2005a) Hockey Sticks, Principal Components and Spurious Significance Geophysical Research LettersVol. 32, No. 3, L03710 10.1029/2004GL021750; McIntyre, Stephen; McKitrick, Ross (2005b) Reply to comment by von Storch and Zorita on "Hockey sticks, principal components, and spurious significance" Geophys. Res. Lett., Vol. 32, No. 20, L20714; McIntyre, Stephen; McKitrick, Ross (2005c) Reply to comment by Huybers on "Hockey sticks, principal components, and spurious significance" Geophys. Res. Lett., Vol. 32, No. 20, L20713. [Ross McKitrick]	Must consult w/CA Pollack – reject?
6-1228	A	27:0		Figure 6.8b: I am not very happy to call it "records of Northern Hemispheric temperature variation". Seasonal analyses show clearly that in the midlatitude land areas, and most data are from the midlatitudes, temperature variability is much higher in winter than in summer. If mainly tree-ring data are used: are we sure that we captured the whole annual variability well? In addition, I do not like that Fig. 6.8 b suggests that the uncertainties are lower prior to 700 AD. I do definitely not support the idea to represent temperature time series prior to 700 AD! [Heinz Wanner]	Taken into account with new text added to explain

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6-1229	A	27:5	27:5	Figure 6.8: show land and sea separately. Pseudo-log scale is promotional and should be removed. [Stephen McIntyre]	accepted
6-1230	A	27:7	27:11	Delete this paragraph. It is simply not true. The “Hockey Stick” graph of Mann and Bradley has been shown to be wrongly calculated by McIntyre and McKitrick,(2003), and there is much evidence that the earth was warmer than today during the Medievel Warm Period (800 to 1300 AD) and at other times. [Vincent Gray]	Reject, will make even more clear
6-1231	A	27:9	27:9	This "recent analysis" needs to be indicated in a reference [Michael MacCracken]	accept
6-1232	A	27:9	27:9	citation [Stephen McIntyre]	accept
6-1233	A	27:9	27:11	Please provide source [Axel Michaelowa]	accept
6-1234	A	27:9	27:11	please give references here. [Guoyu REN]	accept
6-1235	A	27:9		: « A recent analysis of instrumental and documentary proxy climate …. « (Chuine et al., 2004) [Joel GUIOT]	accept
6-1236	A	27:9		In brackets after 280 years the two following references could be added: Luterbacher et al. 2004, Xoplaki et al., 2005 (Luterbacher, J., Dietrich, D., Xoplaki, E., Grosjean, M., and H. Wanner, 2004: European seasonal and annual temperature variability, trends and extremes since 1500, Science, 303, 1499-1503. / Xoplaki, E., Luterbacher, J., Paeth, H., Dietrich, D., Steiner N., Grosjean, M., and Wanner, H., 2005: European spring and autumn temperature variability and change of extremes over the last half millennium, Geophys. Res. Lett., 32, L15713, DOI:10.1029/2005GL023424) [Heinz Wanner]	accept
6-1237	A	27:11	27:11	A reference should be given for the "extreme summer of 2003 was very likely warmer that any that occured in at least 500 years". [Hugues Goosse]	accept
6-1238	A	27:11	27:11	A reference is needed for th statement that 2003 summer was the warmest since 1500. [Philip Jones]	accept
6-1239	A	27:11	27:11	Should cite Luterbacher et al (2004) here. [Michael Mann]	accept
6-1240	A	27:11	27:11	citation [Stephen McIntyre]	accept

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6-1241	A	27:11	27:11	This characterisation of the extreme summer of 2003 is likely to cause unnecessary confusion and disagreement. The summer of 1540 was as well extreme in large parts of Europe (e.g., Casty, C., H. Wanner, J. Luterbacher, J. Esper and R. Böhm, 2005: Temperature and precipitation variability in the European Alps since 1500. Int. J. Climatol. (in press), 10.1002/joc.1216), probably as warm as or warmer than 2003. I therefore suggest to replace "at least" by "almost". [Martin Stendel]	accept
6-1242	A	27:11		Any refs from the lit, here? [Paolo Cherubini]	accept
6-1243	A	27:11		Guiot et al (2005) have shown that the last decade has never been reached in Europe during the last 950 years. [Joel GUIOT]	accept
6-1244	A	27:11		Guiot, J., Nicault, A., Rathgeber, C., Edouard, J.L., Guibal, F., Pichard, G. and Till, C., 2005. Last-millennium summer-temperature variations in western Europe based on proxy data. The Holocene, 15, 489-500. [Joel GUIOT]	accept
6-1245	A	27:11		the work of Luterbacher et al. (2004, Science) should be cited here. Luterbacher, J., D. Dietrich, E. Xoplaki, M. Grosjean, and H. Wanner, European seasonal and annual temperature variability, trends, and extremes since 1500, Science, 303, 1499-1503, 2004. [Thomas Stocker]	accept
6-1246	A	27:11		Why do you not insert the corresponding reference (Luterbacher et al., 2004: Luterbacher, J., Dietrich, D., Xoplaki, E., Grosjean, M., and H. Wanner, 2004: European seasonal and annual temperature variability, trends and extremes since 1500, Science, 303, 1499-1503)? [Heinz Wanner]	accept
6-1247	A	27:13	27:16	This seems out of place here and should be moved to a recommendation section if important [Thomas Karl]	reject – just an introduction
6-1248	A	27:16		add after “Mann 2004”, “Soon and Baliunas 2003, McIntyre and McKitrick 2001”) [Vincent Gray]	Reject – describing TAR, pre these references
6-1249	A	27:20	27:23	I do believe that with this IPCC report, it would be useful to be a bit more precise and say that tree-ring data dominate the Mann et al. 1999 record (at least) during the first half of the last millennium, and that the low frequency component is heavily weighted towards the bristlecone pine data from SW USA (as originally stated by MBH99). I know that this is a sensitive issue, but clearly stating this information seems much better then just saying that the record is "based on a range of proxy types". Some counts of the number of proxy types and locations integrated in MBH99 (and some other records) were recently	Taken into account – will say more in discussion

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				published (Esper et al. 2004, EOS 85) that could be cited, if necessary. Further, given the dominance of tree-ring data in the earlier portion of MBH99, the reconstruction (as most others) is certainly weighted towards warm season temperatures back in time. Also, this point should perhaps be emphasized, given the heated discussion on this reconstruction. [Jan Esper]	
6-1250	A	27:21		Exchange historical documentary sources with documentary proxy evidence (Brazdil et al. 2005). Brázdil, R., Pfister, C., Wanner, H., von Storch, H., and Luterbacher, J., 2005: Historical climatology in Europe – The State of the Art, Climatic Change, 70, 363 - 430. DOI: 10.1007/s10584-005-5924-1 [Heinz Wanner]	accepted
6-1251	A	27:24		Mention over which period the trend of 0.15 C is valid and if it is significant. [Heinz Wanner]	Noted – see figure
6-1252	A	27:27		: I do not think that Osborne et al 2005 was discussed in TAR [Joel GUIOT]	Noted - fixed
6-1253	A	27:32	27:32	Disclose that the Briffa reconstruction fails after 1960. [Stephen McIntyre]	Noted – will include in expanded intro on uncertainties
6-1254	A	27:34	27:42	The Soon and Baliunas study should not be addressed with a full paragraph. This gives too much credit to this single paper. I rather suggest to spend one sentence on this study and mention that they conclude differently than the bulk of other studies. [Jan Esper]	Rejected – want to provide clear historical context on important issues
6-1255	A	27:34	27:42	The criticism on p. 6-27, lines 34-42 misrepresents the work of Soon and Baliunas (2003) and Soon et al. (2003) who specifically argued against the construction of spatially extended (e.g., hemispheric) averages of temperatures from diverse proxies. Those authors compared each proxy to itself, not among climatically diverse variables, as the text implies. Those authors detailed climate or ecosystem anomalies, whether quantitative or qualitative, according to expert opinion in peer-reviewed literature, and whether they fit into the framework established by experts like H. H. Lamb. The approach was emphasized as non-quantitative precisely owing to the diverse nature of proxies, as the 4AR itself notes. The statement that the researchers assumed that "relative dryness can be equated directly with warmth" is a substantive misrepresentation of the work and must be removed, as detailed in Soon-Baliunas (2003) and Soon et al (2003). [Jeffrey Kueter]	Rejected – want to provide clear historical context on important issues
6-1256	A	27:34	27:42	Since the authors of the chapter clearly don't find Soon and Baliunas very informative, why spend a whole paragraph on it? Also, the wording conveys some axe-grinding. Since you may want to appeal to space limitations as a convenient way to reject other text suggestions, you need to consider why so much space is given over to S&B. The	Rejected – it is considered important to reflect sufficient of the history of interpretation post-TAR. Also the way in which inference (wso in S&B)

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				complaint that S&B mingle precipitation and temperature proxies is unpersuasive. McIntyre and McKitrick (2005b - see cell G33) showed that the average correlation between long proxies and gridcell temperatures in MBH98 is less than 0 (-0.08), but the precipitation correlation is stronger; so if the pre-1500 hockey stick segment has any climatic information in it, it's precipitation not temperature. Yet its usage by the IPCC presupposes that it suffices to measure temperature. Its usage in this chapter as a temperature chart does precisely the same mischief that this paragraph chides S&B for. It would be better if the paragraph as a whole were deleted, but if something is to be included it should be short, such as: "Soon and Baliunas (2003) and Soon et al. (2003) challenged the conclusion that the 20th century climate was unusually warm in comparison with the past millennium, by surveying regionally diverse proxy evidence and noting the ubiquity of anomalous temperature or precipitation indicators over intervals that are broadly labeled the Medieval Warm Period and the (subsequent) Little Ice Age. Site-specific information does indicate that regional climatic histories are complex and in many cases unexpectedly variable, but their qualitative method did not permit resolution of the question of whether past regional climate events were synchronous on a hemispheric or global scale." [Ross McKitrick]	differs from statistical inference (MBH) represents different method. Will not use suggested wording but will attempt to remove apparent axe-grinding.
6-1257	A	27:34	27:42	Soon & Baliunas actually allowed evidence of EITHER wet or dry conditions to provide evidence of a LIA or MWP. Their analysis would count a location which had been either wet or dry for any 50 year period as evidence supporting a global scale LIA or MWP. I think this paragraph is way too gentle in the criticism of Soon & Baliunas. [Neville Nicholls]	noted
6-1258	A	27:34	27:42	This paragraph (ant the next one on p. 29 L. 7 to 13) should be updated with very recent publications on the “hockey stick” controversy, i.e.: - Huybers, P. Comment on “Hockey sticks, principal components, and spurious significance” by S. McIntyre and R. McKitrick. Geophys. Res. Lett., Vol. 32, No. 20, L20705 - von Storch, Hans; Zorita, Eduardo, Comment on “Hockey sticks, principal components, and spurious significance” by S. McIntyre and R. McKitrick Geophys. Res. Lett., Vol. 32, No. 20, L20701 - McIntyre, Stephen; McKitrick, Ross. Reply to comment by von Storch and Zorita on “Hockey sticks, principal components, and spurious significance”. Geophys. Res. Lett., Vol. 32, No. 20, L20714 - McIntyre, Stephen; McKitrick, Ross. Reply to comment by Huybers on “Hockey sticks, principal components, and spurious significance Geophys. Res. Lett., Vol. 32, No. 20, L20713 Possibly, a conclusion can be given on this issue.	Accepted – will integrate

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				[Philippe Tulkens]	
6-1259	A	27:34	:42	Keep this paragraph in the report. It outlines reasons why the Soon and Baliunas studies have not provided evidence against the 20th century being the warmest on a hemispheric average scale. [Melanie Fitzpatrick]	accepted
6-1260	A	27:34		I feel strongly that using the notion "hockey stick", even in quotes, is a mistake. Such an expression must not enter serious literature on climate change issues. The very wording of this sentence links "hockey stick" with the work of Mann et al (1999). This is not fair, as this notion is now used as to discredit this work. IPCC should not adopt this language. [Thomas Stocker]	Rejected – want to provide clear historical context on important issues
6-1261	A	27:35	27:36	…Soon et al. (2003) challenged the…. [Steven Clemens]	accepted
6-1262	A	27:35	27:36	Replace "attempted to challenge" with " also challenged" [Vincent Gray]	accept
6-1263	A	27:35		Insert after "studies"; "McIntyre and McKitrick (2003, 2005) have identified several serious errors in the compilation of Mann et al (1998), which, when corrected, show a temperature rise in the 15th century much larger than is observed currently [Vincent Gray]	rejected – covered in updated text
6-1264	A	27:37		Delete "qualitative" All the proxy measurerments, including Mann's are "qualitative" [Vincent Gray]	rejected – not accurate
6-1265	A	27:39	27:42	Delete. These remarks are completyely unfair, as Mann and Bradley used precisely the same data, but wrongly assume that they are sufficiently representative to give a fair average.for the whole Norther Hemisphere. Some of the observations they included are actually in the Southern Hemisphere. [Vincent Gray]	rejected – not accurate
6-1266	A	27:39	27:42	Replace with " They showed that the data are not sufficiently representative, even for the Northern Hemisphere, to justify the derivation of an "average" values" [Vincent Gray]	Rejected – not accurate
6-1267	A	27:39	27:39	Insert "or" before "Little Ice Age" to match the other ()s. [Jonathan Gregory]	accepted
6-1268	A	27:39	27:46	Given that what we are learning about the MWP and the LIA are that they were quite different than first described, and are not always coincident around the globe over the times ascribed to them, it seems to me that the terms should really not be used unless put in quotes or otherwise indicated as being different than what they are familiarly explained to be. That is, I would much prefer to see Box 6.4 titled something like "The Climate of the 10th to 12th centuries" or something (preferably perhaps of 800-1200 years before	Accepted – will change text

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				present) and then in the text indicate that there are those who have named it the MWP based on climate conditions in Europe, but that we have discovered that it is really much more complex than that and that the name is simply inappropriate. IPCC should really come up with a new way of referring to these special times. [Michael MacCracken]	
6-1269	A	27:39	27:42	it would be better to give a more balanced statement here. [Guoyu REN]	Rejected – accurate as is
6-1270	A	27:44	29:5	Box 6.4: In TAR, both LIA and MWP were discussed regarding the climatic variations for the last 1000 years. However, in this chapter, almost no description can be found concerning LIA. If the author regards LIA as unimportant for the millennium scale climate variations, any comments about that should be included in this section. [Takehiko Mikami]	Taken into acount – will discus LIA outsider box
6-1271	A	27:46	27:46	The box on the Medieval Warm period is good. It doesn't come up with a series of dates. I agree with this, therefore, it would be better if it wasn't used? [Philip Jones]	Accepted – will change notation
6-1272	A	27:48	27:49	I think you mean that different authors were already proposing that climate had varied in the past (the sentence reads as though these researchers were disputing that climate had changed. In fact they disputed the suggestion that climate was constant). [Neville Nicholls]	accepted
6-1273	A	27:50	27:51	…cores evidence by Rabot (????) of considerable… Needs reference? [Steven Clemens]	Accepted – will find/coite ref
6-1274	A	27:53		suggest change “possible” to “impossible” [Brent Alloway]	accepted
6-1275	A	28:1		The word "worse" is a value judgement - how about cooler or warmer? [Melanie Fitzpatrick]	accepted
6-1276	A	28:4	28:34	Much of this is a repeat of what evidence was available prior to the TAR, but it is important to include this because the position regarding the MWP has been so often mis-represented by so-called sceptics. [Neville Nicholls]	accepted
6-1277	A	28:4	28:34	There is a very long refutal of Lamb's work in this section. It seems to me to be way too detailed a response. A single sentence saying that 'modern analyses have demonstrated that the patterns and scale of change were more complex than Lamb envisioned' would probably suffice. [James Shulmeister]	Rejected – keep to provide historical context
6-1278	A	28:4		If space is a problem, how about "Lamb (1965), who may have been the first to coin the phrase "Medieval Warm Epoch", considered the warmest conditions to have occurred at	Rejected – keep to provide historical context

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				different times…" The point of this comment is that while this section reads nicely, it is more journalistic and uses more space than other important issues" [Tas van Ommen]	
6-1279	A	28:11	28:34	All that is said here about the Medieval Warm Period is equally relevant for the Little Ice Age; that can also be seen in Fig. Fig. 6.4, Box 1. The goal here is to clearly downplay the importance of the MWP, but that same standard should then by applied to the LIA; otherwise, it would appear as if the authors have an agenda they are pursuing. [Andrew Lacis]	Taken into account – will say more about the LIA
6-1280	A	28:14	28:15	these issues apply to subsequent studies as well [Stephen McIntyre]	Noted, but subsequent studies attempted more quantitative than Lamb
6-1281	A	28:21	28:24	I guess, I am a bit skeptical about the Hughes and Diaz (1994) paper, since it is conceptually similar to the Soon and Baliunas study, and even more importantly, the paper was published before the whole discussion on the preservation of low frequency trends in long tree-ring records really started. An evaluation of detrending techniques applied to tree-ring data and the consequences on retained low frequency variations would likely alter the main conclusions of the Hughes and Diaz paper. So, I think that the paper is in some sense outdated. [Jan Esper]	Noted – and in large part accepted. Will considered amending text to reflect this.
6-1282	A	28:21	28:23	Hughes and Diaz [1994] uses proxy series which do not capture centennial trends. It is not usable. [Stephen McIntyre]	Taken into account – will elaborate
6-1283	A	28:26	28:34	The portion of this paragraph starting at the second sentence, "At some times, some regions may have experienced…" is too grudging, and relies for its conclusion on an invalid juxtaposition of proxy and thermometer data in Figure 6-8. If, for the purpose of downplaying the MWP you are going to take the position that the local paleoclimatic evidence is too noisy and uncertain to say more than this, then you can't elsewhere make strong pronouncements about the "very likely" unusual conditions of the late 20th century. The paragraph would do better justice to the large library of paleoclimatic evidence and the profound problems of mapping between instrumental and proxy data if it were amended to read: "It is clear that many regions around the world experienced large climatic variations over the past millennium, including intervals during which conditions would have been perceived as relatively warm compared to those observed at present. The limitation of regional evidence for evaluating present day global trends is that it does not establish hemispheric or global patterns, unless dating is precise enough to support conclusions about whether warming or cooling intervals were synchronous. In recent years a focus of research has been to apply statistical methods to homogenize different types of proxy evidence and attempt to extract common climatic signals from them.	Noted – see edited text

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				However no consensus has emerged about the best way to do this or whether the existing library of paleoclimatic indicators is even sufficient to provide a decisive comparison of the present global climate to that of a thousand years ago. Nor has any study established a sufficiently robust statistical mapping between proxy and thermometer data to support extrapolation of hemispheric temperature averages back to the early centuries of this millennium or beyond. Consequently direct comparison of recent instrumental trends with indirect proxy data of centuries past is not formally possible at this time." [on this point see note in cell 32 on the need to remove the instrumental line in Figure 6-8]. [Ross McKitrick]	
6-1284	A	28:26	28:43	Regional averaged temperature series would be equivalently valubale for the purpose, especially when the regions are representative and proxy data coverage is relatively poor. Some studies from China show that winter mean temperature in the Medieval Warm Period is as warm as the 20 th century (see Ge, Q.-S, Jingyun Zheng, Xiuqi Fang, Zhimin Man, Xueqin Zhang, Piyuan Zhang and Wei-Chyung Wang, 2003, Winter half-year temperature reconstruction for the middle and lower reaches of the Yellow River and Yangtze River, China, during the past 2000 years, The Holocene, 13, 995-1002; Yang, B., A. Braenning, K.R. Johnson et al., 2002, General characteristics of temperature variation in China during the last two millennia, Geophys.Res.Lett, 29(9): 38-1-4; 28. G. Ren, 1998, Pollen evidence for increased summer rainfall in the Medieval warm period at Maili, Northeast China, Geophysical Research Letters, 25, 1931-1934). A recent study shows that annual mean temperature of China is generally warmer from A.D.1000 to A.D. 1310 with a relatively cool episode in 13th century, and it is significantly colder from A.D.1310 to 1910 with minimum anomalies occurring in 15th, 17th and 19th century respectively. Modern warm period beginning from the end of 19th century looks unusual in terms of the 1000-year variation of annual mean temperature, but it is not significantly warmer than the earlier warm period or Medieval Warm Period (MWP)(Z. Chu and G. Ren, 2005, A preliminary reconstruction of mean surface air temperature over the past 1000 years in China, Climate and Environmental Research (in press in Chinese)). [Guoyu REN]	Noted – will explore suggested references and integrate as appropriate
6-1285	A	28:26	29:48	it is worth noticing that the effect of urbanization on surface air temperature records in the past 50 years might have been significant, at least in China. If the effect is excluded, the warming in the last 20 years may not have been so significantly higher than the average warmth of some periods in the “Medieval Warm Period” in eastern China. [Guoyu REN]	Noted
6-1286	A	28:26		In Box 6.4 you might include the following long temperature reconstructions from the Low Countries provided by van Engelen et al. 2001 and Shabalova and van Engelen 2002 covering the last millenium (van Engelen AFV, Buisman J Ijnsen F (2001) A millennium	Accepted – will integrate

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				of Weather, Winds and Water in the Low Countries. In: Jones PD et al. (eds) History and Climate: Memories of the Future? Kluwer Academic Press, New York, Boston, London, 101-124 / Shabalova MV, van Engelen AFV (2003) Evaluation of a reconstruction of winter and summer temperatures in the Low Countries, AD 764-1998. Clim Change 58: 219- 242). [Heinz Wanner]	
6-1287	A	28:30	28:34	Again more recommendations [Thomas Karl]	noted
6-1288	A	28:36	28:36	Box 6.4 Figure 1 - xxx [Stephen McIntyre]	Noted - unclear
6-1289	A	28:40	28:43	The sentence "Studies that have attempted to do this have invariably…" is tendentious in tone. Why "invariably"? Do the authors go looking for that result? Is only one conclusion possible? This should be re-worded: "Some studies that have attempted to do this have found, not surprisingly, that the medieval climate was complex in terms of the precise timing and regional expression of warming events (Crowley etc...." [Ross McKitrick]	Accepted – removed word
6-1290	A	28:41	28:43	The hockey stick shape of Crowley and Lowery [2000] is dependent on controversial bristlecone series, as is Esper et al [2002] on related foxtail series; as is Jones and Mann [2004]. [Stephen McIntyre]	Other reconstructions not including these data show similar shape.
6-1291	A	28:45	28:46	In fact, data are scarce prior to 1700 (not 1600), which impacts the question of how cold the LIA really was. For one to define data scarcity in this arbitrary way, one would have to back it up - how many data ponts are available at various points in time, and what does 'scarce' really mean when there is little tropical data before the instrumental record? [Andrew Lacis]	accepted – see edited text
6-1292	A	28:45	28:49	This para is apparently about uncertainties, but they are not specified. [Thomas Stocker]	Noted – see edited text
6-1293	A	28:45	29:3	As worded, this group of sentences makes a contradictory point. You say that there are very large uncertainties in paleoclimate work, and major deficiencies in geographical coverage, but nonetheless you will confidently draw a precise conclusion for the NH and insinuate that it is true of the world as a whole. [Ross McKitrick]	Noted – see edited text
6-1294	A	28:45	29:3	Moreover, the conclusions are strongly stated on the basis of a small number of references, one of which (Mann 1999) has been conspicuously refuted (on which see comments below starting at G-46). By obstinately clinging to Mann 1999 in light of all the public criticism it has received you are creating the impression of a Panel of authors	Noted – assessment not based on Mann alone, see edited text

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				cemented in their prior beliefs. You also create an impression of an insular group of writers who really don't pay a lot of attention to what goes on outside their own papers. Thus it calls into question the other references: if you placed so much weight on the Mann curve in the TAR, and blundered in doing so, yet you have failed to take on board the published criticisms of the hockey stick in updating your thinking, then chances are your other citations are simply drawn from the same insular milieu and are no more reliable. [Ross McKitrick]	
6-1295	A	28:45	29:3	Finally, following on from a point above in G34-35, the section is written as if the borehole evidence of Huang, Pollack and Shen didn't exist. If it is being ignored because it hasn't been worked over sufficiently in the science literature, then by what right are the more recent articles being used? And if it is being ignored because some authors critiqued it, then why is the Mann work still being used elsewhere, notably in Figure 6-8? [Ross McKitrick]	Rejected – borehole evidence is incorporated
6-1296	A	28:45	29:3	The paragraphs beginning at line 45 should be revised to read as follows. "The uncertainties associated with the paleoclimatic history of the Northern Hemisphere are larger than was appreciated at the time of the TAR: the statistical skill of reconstruction models is not as good and the inherent variability of the climate is likely greater (von Storch et al., 2004; McIntyre and McKitrick 2005a; Moberg et al 2005). Uncertainties are particularly acute prior to 1600, a period for which data are scarce but the comparisons to present day climate are of particular interest (Briffa and Osborn, 2002; Cook et al., 2004a; Osborn et al., 2005). Figure 6-8 shows that the limitations of proxy evidence rules out definitive ranking of the present era to past warming epochs. Unless a geographically diverse sample of long proxy series are updated to the present, or unless a statistically valid methodology for splicing instrumental temperature series to proxy series is successfully established, it is unlikely that a meaningful ranking of the late 20th century climate to that of the 10th and 11th centuries can be made. [Ross McKitrick]	Rejected – see edited text
6-1297	A	28:45	29:3	(proposed text cont'd) "It is certain that further work is necessary to produce many more paleoclimate series with much wider geographical coverage and continuity up to the present. There are far from sufficient data to make any meaningful estimates of global medieval warmth. There are very few long records with high temporal resolution data from the oceans, the tropics or the Southern Hemisphere. The evidence for the Northern Hemisphere supports the view of widespread rising temperatures during the High Medieval time (950--1000) and of widespread cooling conditions in the 17th and 18th centuries. However there is too much variability in results across paleoclimatic methods, too little independence in the dendrochronology-based reconstructions, and too many unresolved issues in statistical methdology, to permit an overall judgment about whether	Rejected – see edited text

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				the northern hemisphere climatic conditions of the late 20th century exhibit truly unprecedented warmth compared to the past 1000 years (Huang, Pollack and Shen 1997; Briffa and Osborn 2002; Cohn and Lins 2005; von storch et al. 2004; McIntyre and McKitrick 2005a.)" [Ross McKitrick]	
6-1298	A	28:45	29:3	References for above cells: Huang, Pollack and Shen, see G35; Cohn and Lins 2005, seeG18; McIntyre and McKitrick 2005a see G33 [Ross McKitrick]	noted
6-1299	A	28:45	29:3	References (cont'd) Hans von Storch, Eduardo Zorita, Julie Jones, Yegor Dimitriev,Fidel González-Rouco, Simon Tett (2004) "Reconstructing Past Climates from Noisy Data" Science, 30 Sept 2004; Anders Moberg, Dmitry M. Sonechkin, Karin Holmgren, Nina M. Datsenko & Wibjorn Karlen (2005) "Highly variable Northern Hemisphere temperatures reconstructed from low- and high-resolution proxy data" Nature Vol 433 10 February 2005 613-617. [Ross McKitrick]	noted
6-1300	A	28:46	28:54	It would be helpful to show what is meant by scarce data. A plot showing where data for this section come from, for different periods, would greatly increase the clarity and strengthen the statements regarding limitations in the medieval warm period, for example. [Susan Solomon]	Noted - will try to develop map and perhaps with distribution of sites through time
6-1301	A	28:47	28:49	The phrasing here seems to me a bit too certain. I would suggest saying "prior to the 20th century likely occurred between 950 and 1100, with annual average temperatures estimated to have been between 0.1 and 0.2 C below ..) [Michael MacCracken]	Noted – see edited text
6-1302	A	28:49		temperatures rather than "levels" [Neville Nicholls]	accepted
6-1303	A	28:51	28:54	Again, these very same comments apply to the LIA before the instrumental record - so again the bias against the MWP is showing. [Andrew Lacis]	Taken into account – will say more about the LIA
6-1304	A	28:51	28:52	Delete "It is certain that" as it is confusing to use the term in describing the need to do research rather than about results, and it would help to connect the first two sentences in that the second sentence is the reason for the first--so say "coverage, because there are ..." [Michael MacCracken]	acccepted
6-1305	A	28:51	28:52	The wording of the sentence beginning "It is clear.." is a bit awkard. Needs a phrase such as "to decrease the substantial uncertainties that still exist in past regional patterns of climate change" or something to that effect. The next two sentences then follow logically as supporting the first. [Michael Mann]	Accept - reworded

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6-1306	A	28:56	29:3	It may be worded like this according to the research result: this evidence does support a conclusion that hemispheric mean temperatures might have been as warm as those in the late 20th century during any period in medieval times. [Guoyu REN]	Noted – see edited text
6-1307	A	28:57	28:57	"High Medieval time" … what is "High"? [James Crampton]	accepted
6-1308	A	28:57	28:57	Maybe "High Mediaeval" could be avoided. When reading it earlier, I assumed it meant in the Middle Ages, rather than the Dark Ages! What a confusing term. [Jonathan Gregory]	accepted
6-1309	A	29:0	30:	more than two pages to discuss validity of reconstructions of the past 1000 years seem disproportionate according to the rest of the chapter. Especially when we know that often the same data are included in the various attempts and often the same people are involved in the various papers. Difficulties pointed out in this section have equivalent for other time periods but nothing is discussed before (I understand that as it is really technical), but why to discuss them here so extensively. [Joel GUIOT]	Rejected based on the important of the issue
6-1310	A	29:1	29:3	This again sounds defensive - one shouldn't be fending off greenhouse critics here. [Andrew Lacis]	accepted
6-1311	A	29:3	29:3	Capitalize "medieval". [James Crampton]	rejected
6-1312	A	29:3	29:3	Discuss Naurzbaev et al ,2004 and evidence of higher medieval treelines [Stephen McIntyre]	Noted – see edited text
6-1313	A	29:7	29:13	As with the Soon and Baliunas paper, I do not see the point to spend a full paragraph on the McIntyre and McKitrick critique on MBH99. The IPCC report is certainly not the place to defend a single paper (Mann et al. 1999), particularly since this (admittedly pioneering) record is now aligned with several other reconstructions as done in Figure 6.8b. I suggest to either remove this paragraph, or alternatively spent one sentence on the work by McIntyre and colleagues saying that some of the methods applied in MBH99 are criticized. Alternatively, it seems more relevant to discuss some reasons for the difference in low-frequency loading (and thus T amplitude) between the records shown in Figure 6.8b, with the differing detrending methods applied to tree-ring data likely being a major source for these differences. The new study by D'Arrigo et al. (2005) would perhaps be a good starting point to highlight the impact of tree-ring detrending on the course of long-term T reconstructions. In their work, D'Arrigo et al. clearly show that "standard" detrending techniques result in reduced T variations reconstructed over the past millennium (admittedly similar to MBH99), whereas the application of RCS ("a statistical technique designed to produce ring width chronologies in which evidence of long-	Noted – see edited text

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				timescale climate forcing is better represented, lines 31-32 on page 29) results in a reconstruction indicating more low frequency loading and thus more T variance over the past millennium. A paragraph addressing the seemingly differing low frequency loadings of the reconstructions shown in Figure 6.8b would be rather relevant, and should not be written as a critique of a single record, but rather as a perspective indicating future paths in palaeoclimatology. [Jan Esper]	
6-1314	A	29:7	29:12	Delete from “produced” in line 7 to “reconstruction” in line 12 [Vincent Gray]	Rejected – see edited text
6-1315	A	29:7	29:13	I assume the authors are aware of the 'comments' and 'replies' on McIntrye and McKitrick in a very recent issue of GRL. [Bryant McAvaney]	Noted – see edited text
6-1316	A	29:7	29:9	McIntyre and McKitrick [2004] did NOT produce a NH reconstruction; they explicitly state that they do not endorse the proxies in MBH98. They showed the results using updated versions of MBH98 proxies and principal components calculated over the maximum period in which all proxies were available. [Stephen McIntyre]	Noted – see edited text
6-1317	A	29:7	29:9	Wahl and Ammann [2004] is not published yet. It does not reproduce MBH98 claims of statistical skill. [Stephen McIntyre]	Noted – see edited text
6-1318	A	29:7	29:13	McIntyre and McKitrick [2005a, 2005b, 2005c] showed that the MBH98 principal components methodology was biased towards selection of hockey stick shaped series; that the MBH98 reconstruction was not robust to the presence/absence of disputed bristlecone pine series; failed R2 and other cross-validation tests; and that the seemingly significant RE statistic was spurious. In particualr, they showed that the IPCC TAR claim that the MBH98 passed cross-validation statistical skill tests was false. [Stephen McIntyre]	Noted – see edited text
6-1319	A	29:7	29:13	The authors seem pretty uninformed about my work with Steve McIntyre. For instance there is no mention of our 2005 GRL or E&E papers, even though these contain the bulk of our arguments; and indeed the paragraph shows that the chapter authors are unaware of what our arguments actually are. The paragraph trots out the straw man that we are selling an alternative climate history, despite our repeated and persistent statements that we are not trying to offer "our" climate history curve. From the outset we have been trying to show what Mann's curve would look like if he had done what he said he had done, using the data he said he used. Lest any reader of this comment think it pejorative for me to suggest that the MBH98/99 data and methods were inaccurately or incompletely disclosed, the Corrigendum ordered by Nature and published July 1 2004 by Mann et al.	Noted – see edited text

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				should settle that. We filed a Materials Complaint with Nature in January 2004, Nature asked Mann to respond, and based on their review of his response Nature ordered a complete restatement of the data and methods of MBH98. The methodology described in the new MBH98 SI differs fundamentally from that presented in MBH98 itself, notably in its use of a highly irregular PC methodology and the splicing of proxy PCs in hitherto undisclosed segments. [Ross McKitrick]	
6-1320	A	29:7	29:13	The last sentence is false. Mann's results have never been reproduced. Ammann and Wahl reproduced the reconstruction PCs of Steve McIntyre to 9 decimal places (no great feat since his code was available on the internet) but got no closer to Mann's final results than McIntyre had, except for their introducing a rescaling step not disclosed in MBH98 but apparently used by Mann. Once added to McIntyre's code the Wahl-Ammann and Mcintyre reconstructions are identical but neither one agrees with Mann's. No one has ever reproduced Mann's results. I know of 3 teams that have tried: McIntyre-McKitrick, Ammann-Wahl and Cubasch, and all failed, but McIntyre and Ammann-Wahl published reasonably close approximations. [Ross McKitrick]	Noted – see edited text
6-1321	A	29:7	29:13	The paragraph also brings up the lack of verification skill of the M&M climate reconstruction, yet ignores the point that Mann's curve also fails verification tests. This is now well-established in the literature: see McIntyre and McKitrick 2005a,c in cell G33. Since you consider the lack of verification skill of (what you term) the M&M2003 climate history to be sufficient cause to reject its results, you are equally bound to reject the MBH98 and MBH99 curves. [Ross McKitrick]	Noted – see edited text
6-1322	A	29:7	29:13	Additionally this paragraph misses the whole issue of the bristlecone pines. The comment about how we "omitted several important proxy series" sounds like you got your material off the realclimate web site rather than from following the debate in the literature. We showed in our E&E2005 paper that the difference between high and low 15th century values is fully explained by the inclusion or exclusion of the Graybill-Idso bristlecone pine series. Since in a proper PC analysis these only appear in PC4 and account for less than 8% of the explained variance of the NOAMER network, as opposed to appearing in PC1 and accounting for 37% in the erroneous Mann PC method, they cannot be considered a dominant climatic pattern. Moreover there is comprehensive evidence (surveyed in our E&E2005 paper) showing that their 20th century growth spurt is not a climatic signal, so they are not proper climate proxies. Yet their usage in the MBH data set swamps the rest of the data set and eliminates the high 15th century values that would otherwise result from the application of the MBH method on the rest of the data. Mann	Noted – see edited text

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				has never rebutted the dependence of his results on the bristlecone pine series, and is hardly in a position to do so since he did an unreported sensitivity analysis and discovered it for himself, but did not report it. So it is not that we "omit" some important proxies and end up with a lousy result, instead we remove some lousy proxies and end up with an important result: the conclusions fall to pieces. The issue, as we have said over and over, is robustness. Mann's conclusions are not robust. They are not statistically robust, nor are they robust to removal of a small network of bristlecone proxies that are widely viewed among dendrochronologists (including Hughes himself in another paper) to be invalid as temperature proxies. What we have shown is not that the 15th century was "warm", but that Mann's results do not provide evidence that the late 20th century was climatologically exceptional. [Ross McKitrick]	
6-1323	A	29:7	29:13	The next 4 cells provide the appropriate alternative wording for this section. I am adding to the length, but what I propose is a mere fraction of the page space given over to the hockey stick in the TAR. Considering the influence it has had, due to its prominence in the TAR, you can hardly begrudge taking adequate space in the AR4 to correct the record. [Ross McKitrick]	
6-1324	A	29:7	29:13	"McIntyre and McKitrick (2003) argued that the data as used in Mann et al. (1998) contained numerous problems, most notably undisclosed editing and duplicate usage of some proxies, and unreproducible principal components (PCs). A Corrigendum by Mann et al. (2004) provided a new listing of data and methods, but to date no one has been able to exactly reproduce the hockey stick displayed in the TAR. The most influential discrepancy between the stated and actual methodology in Mann et al. (1998) was in the calculation of PCs. McIntyre and McKitrick (2005a) showed that Mann et al. used a nonstandard method that is strongly biased toward finding hockey stick patterns in proxy networks. They showed that the method consistently reports a hockey stick-shaped first principal component even in networks of trendless red noise, and assigns significantly inflated eigenvalues to them, thereby exaggerating the variance fraction explained by a hockey stick shape. They also showed that the specific effect of this methodology in Mann's study was to overweight a controversial group of bristlecone pine proxies from western North America. Substantial expert literature precludes reliance on the bristlecones as climatic proxies (McIntyre and McKitrick 2005d), but removal of this subset eliminates the characteristic hockey stick shape in the final climate reconstruction, leaving no apparent basis for viewing the late 20th century as climatologically unique (see also Ammann and Wahl 2005). [Ross McKitrick]	Noted – but many points considered minor and with no substantive effect on interpretation of reconstruction as published. Hence, this version of text will not be used but text will account for many of the points raised.
6-1325	A	29:7	29:13	"The analysis of McIntyre and McKitrick (2005a) also challenged the reconstructive skill	Noted and will consider point in

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				attributed to the hockey stick. Standard skill tests reveal Mann's results to be insignificant at least as of AD1450. The original claim of statistical skill was based solely on reference to an RE score benchmarked without accounting for the influence of the nonstandard PC methodology. Detailed Monte Carlo analysis showed the RE score did not attain the significance cut-off, confirming the inferences of other skill scores, and indicating that the hockey stick graph does not provide reliable guidance as to the climatic history of the Northern Hemisphere in the first five centuries of the millennium. [Ross McKitrick]	revising text.
6-1326	A	29:7	29:13	"Subsequent comments on the McIntyre and McKitrick analysis by von Storch and Zorita (2005) and Huybers (2005) confirmed that Mann's PC method is biased towards hockey stick-shaped results. Von Storch and Zorita presented a simulated example in which the biased PC method would not matter for an overall climate reconstruction, but as it was a different data setting it did not rebut problems in the hockey stick itself (McIntyre and McKitrick 2005b). Huybers argued that introducing a variance rescaling would reduce the RE significance criterion and apparently re-establish significance for Mann et al. in the pre-1450 segment. McIntyre and McKitrick (2005c) showed that this did not address the insignificant R2 score, and if the variance rescaling is introduced in such a way as to properly emulate the hockey stick algorithm the RE benchmark itself remained almost unchanged, thus still indicating insignificance. [Ross McKitrick]	Noted and will consider point in revising text.
6-1327	A	29:7	29:13	In light of the detailed debates over the hockey stick since the TAR it is now clear that it did not provide a basis for concluding that the late 20th century climate is unusually warm compared to the previous millennium, and the IPCC was premature to have given it so much prominence at the time. [Ross McKitrick]	Noted
6-1328	A	29:7	29:13	References for above 4-cells: McIntyre and McKitrick 2005a-c, see G33; Hans von Storch and Eduardo Zorita (2005) "Comment on 'Hockey sticks, Principal Components and Spurious Significance'" Geophysical Research Letters 32(16) 2005GL022753 L20701; Huybers, Peter: "Comment on 'Hockey Sticks, Principal Components and Spurious Significance'" Geophysical Research Letters 32(16) 2005GL023395 L20705; McIntyre, Stephen and Ross McKitrick (2005d) "The M&M Critique of the MBH98 Northern Hemisphere Climate index: Update and Implications" Energy and Environment 16(1)69-100. Mann, Michael E., Raymond Bradley and Malcolm Hughes (2004) "Corrigendum" Nature 430, July 1, 2004 p. 105. [Ross McKitrick]	Noted
6-1329	A	29:7	29:13	How should we make a better assessment on those radically different conclusions? [Guoyu REN]	Text will attempt to clarify

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6-1330	A	29:7	29:13	More information on what was omitted (north american bristlecone pine?) - and why it should not be - would be helpful here. [Susan Solomon]	Text will be modified
6-1331	A	29:7	29:7	Delete comma after "McKitrick". [Martin Stendel]	accepted
6-1332	A	29:7	29:13	This paragraph (ant the previous one on p. 27 L. 34 to 42) should be updated with very recent publications on the “hockey stick” controversy, i.e.: - Huybers, P. Comment on “Hockey sticks, principal components, and spurious significance” by S. McIntyre and R. McKitrick. Geophys. Res. Lett., Vol. 32, No. 20, L20705 - von Storch, Hans; Zorita, Eduardo, Comment on “Hockey sticks, principal components, and spurious significance” by S. McIntyre and R. McKitrick Geophys. Res. Lett., Vol. 32, No. 20, L20701 - McIntyre, Stephen; McKitrick, Ross. Reply to comment by von Storch and Zorita on “Hockey sticks, principal components, and spurious significance”. Geophys. Res. Lett., Vol. 32, No. 20, L20714 - McIntyre, Stephen; McKitrick, Ross. Reply to comment by Huybers on “Hockey sticks, principal components, and spurious significance Geophys. Res. Lett., Vol. 32, No. 20, L20713 Possibly, a conclusion can be given on this issue. [Philippe Tulkens]	Accepted – text will be modified to do this
6-1333	A	29:7	29:13	The critics to McIntyre and McKitrick study are shortly mentioned however the findings related to larger amplitude and higher low frequency variations in earlier centuries and other findings of their study should be incorporated as well. [Heinz Wanner]	Noted
6-1334	A	29:7		The McIntyre and McKitrick saga: here, it should be made clear that 1) Mann et al published the hockey stick curve, 2) the two authors claimed that it was baised, 3) it was shown to be not biased, or at least that it is the best that we can have, now. It must be stated clearly. Period. [Paolo Cherubini]	Noted
6-1335	A	29:7		As it stands now, it is not so clear. [Paolo Cherubini]	Noted
6-1336	A	29:7	:13	Leave this paragrpah in the report. It is a good evaluation of the current debate on the Mann et al work. [Melanie Fitzpatrick]	Noted
6-1337	A	29:7		Insert after “2003”; “2005” [Vincent Gray]	accepted

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6-1338	A	29:7		Insert after " 2005)" "found serious errors in the calculations of Mann et al 1998, which, when corrected, gave temperatures in the 15th century which were higher than any recorded recently.. It is probable that similar errors are present in many of the competing compilations." [Vincent Gray]	Rejected – incorrect statement
6-1339	A	29:7		This has to be one of the most difficult sections of the chapter, because it so clearly attracts controversy. I would rather that the whole "hockey stick" debate were de-emphasised as something that belongs 5+ years ago and is superseded by more current studies. Is it possible to de-emphasise this paragraph and get away from this entirely (footnote? box?) .A neutral tone is important, and von Storch as a template sets a good tone - there are apects of the MBH analysis that may be questioned (eg MM03,05), but the overall conclusion is relatively insensitive to method, as evidenced by several other reconstructions. Regarding the present choice of words, if this much must be said, can it be muted: "...even though they attempted to employ the same method..." might play into certain arguments over the initial availability of material. The word "attempted" is a bit provocative. Also to say that "...to demonstrate any likely validity..." is to use a tone that might be less helpful. As an example, could it be said that " ...arrived at a regression model which estimated temperatures that statistically failed to show agreement with independent observations" or something like this. [Tas van Ommen]	Noted and point accepted in principle. Revised text will attempt to be neutral.
6-1340	A	29:8	29:8	Delete first "in" [Michael MacCracken]	accepted
6-1341	A	29:9		Replace "attempted to employ" with "employed" [Vincent Gray]	accepted
6-1342	A	29:10	29:12	Delete from "and arrived at a regression model" in ;ine 10 to "reconstruction" in line 12. This statement is untrue. [Vincent Gray]	noted
6-1343	A	29:13	29:13	should add to the end of this sentence something akin to "who showed the original Mann et al reconstruction to be robust with respect to the precise details of the method as long as the key underlying proxy data are retained" since this is the crucial point made by Wahl and Ammann. Note also the updated reference: Wahl, E.R. and C. M. Ammann, Robustness of the Mann, Bradley, Hughes Reconstruction of Surface Temperature, Climatic Change (currently in final revision). [Michael Mann]	Noted and will be considered in revising text though this wording will not be adopted.
6-1344	A	29:13	29:13	It would also be useful to note here that Rutherford et al (2005), which has since been published [Journal of Climate, 18, 2308-2329, 2005] reproduces essentially the same	noted

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				reconstruction as Mann et al (1998) using an entirely independent statistical method. In the process, they demonstrate that each of the criticisms raised by McIntyre and McKitrick (2003) are without merit. [Michael Mann]	
6-1345	A	29:13	29:13	The point may potentially be raised by other reviewers that McIntyre and McKitrick made additional claims regarding Mann et al (1998, 1999) in an article "Hockey sticks, principal components, and spurious significance" published in GRL in 2005. Here, they falsely claimed that the Mann et al reconstruction is somehow an artifact of how PCA was implemented in reducing certain tree ring data networks. In this context, it is important to note that those claims have now been independently refuted by 5 separate studies: Wahl and Ammann (cited above), Rutherford et al (cited above) and 3 separate criticisms of McIntyre and McKitrick (2005) that have appeared in or are under consideration in GRL each entitled "Comment on "Hockey sticks, principal components, and spurious significance" by McIntyre and McKitrick": (1) Ammann and Wahl (under review), (2) Huybers(published), and (3) von Storch and Zorita(published). [Michael Mann]	noted
6-1346	A	29:13		Wahl and Ammann 2004 or still in review (see refs) [Paolo Cherubini]	noted
6-1347	A	29:13		The inclusion of the reconstruction of McIntyre and McKitrick in Fig 6.8b shows that there is still some doubt about whether current temperatures are higher than those in the 16th century." [Vincent Gray]	This 'reconstruction' was not considered valid by these authors and will not be included.
6-1348	A	29:13		successful seem not the appropriate word here. Why is a reproduction a success? [Thomas Stocker]	noted
6-1349	A	29:19		19 Insert afer "TAR". "and the reconstruction of McIntyre and McKitrick 2003" [Vincent Gray]	Rejected (see response to 1347)
6-1350	A	29:19		The result of Moberg et al. (2005) on the Northern Hemisphere temperature should be shown in a figure because Figure 6.8b does not show it. The result of Luckman & Wilson (Luckman, B.H. and Wilson, R.J.S. 2005. Summer temperatures in the Canadian Rockies during the last millennium: a revised record. Climate Dynamics 24: 131-144.) is cited in the reference section of this chapter, but it is not mentioned in the text. They show a large temperature fluctuation in Canada, and claim that latewood density is better than tree-ring width to reproduce the past climate. [Kiminori Itoh]	The Moberg et al. curve is included Point on Luckman & Wilson noted
6-1351	A	29:20	29:22	The non-independence should be discussed. This includes non-independence of authors and more detailed discussion of non-independence on proxy series. [Stephen McIntyre]	Rejected – the data series are discussed and point on authors not valid.

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6-1352	A	29:24	29:24	Mann and Jones [2003] uses disputed bristlecone pine series, which may be affected by 20th century fertilization. [Stephen McIntyre]	noted
6-1353	A	29:26	29:28	Is this a correct description of methodology or is their a correlation-weighted system? [Stephen McIntyre]	Text will be clarified
6-1354	A	29:35		Insert after “studies”; “McIntyre and McKitrick (2003, 2005) have identified several serious errors in the compilation of Mann et al (1998), which, when corrected, show a temperature rise in the 15th century much larger than is observed currently [Vincent Gray]	Reject – a different ‘selection’ of predictors does not constitute a “correction”
6-1355	A	29:39	29:39	Please be more specific here about what conclusion the reader should draw about the Esper record. You say it is different from others, but you don't provide enough information for the reader to know if it might be better, or not. [Susan Solomon]	noted
6-1356	A	29:41	29:41	D'Arrigo et al. [2005] does not verify for post-1985 warm values. It cannot be relied upon to record prior warm periods. [Stephen McIntyre]	
6-1357	A	29:41	29:41	Hegerl et al, submitted do not provide any information on proxies and should not be used [Stephen McIntyre]	Rejected – this paper does provide information on proxies
6-1358	A	29:45	29:47	another weakness of Moberg et al (2005) is the lack of identification of the signal recorded by tree-rings and pollen (there is no proper calibration) [Joel GUIOT]	noted
6-1359	A	29:45	29:48	It should be noted here that the two (published) reconstructions which exhibit the greatest amplitude variability (Moberg et al and Esper et al) are actually almost completely uncorrelated--they show very little similarity at all, in terms of the timing of century-scale warm and cold periods, as revealed by a cursory examination of Figure 6.8. In other words, it is misleading to lump these, and other such reconstructions, together as indicating "greater variability" when they actually agree quite poorly with each other (calling into question whether the greater variability is meaningful or an artifact of the data or methodology used). [Michael Mann]	noted
6-1360	A	29:46	29:47	Unless the proxies are calibrated in the warm period of 1980s-1990s, no conclusions can be drawn [Stephen McIntyre]	Unclear what the meaning of this point is
6-1361	A	29:47	29:48	Please be more specific here about what conclusion the reader should draw about the Moberg record. You say it is different from others, but you don't provide enough information for the reader to know if it might be better, or not.	Accepted

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				[Susan Solomon]	
6-1362	A	29:50	29:54	Perhaps this is a spelling mistake, but I am not aware of the Briffa et al. (2005) reconstruction. It also seems that this record is not shown in Figure 6.8b. [Jan Esper]	Accepted – will be amended
6-1363	A	29:50	29:54	This paragraph is contradictory to material on previous page where lines 39-40 argue for global averages [Thomas Karl]	Noted – text may be altered
6-1364	A	29:50	29:54	Briffa et al [2005] was not provided for comment. I was advised that it was withdrawn from consideration. [Stephen McIntyre]	noted
6-1365	A	29:54	29:54	Change to "over the past 2000 years" [Michael MacCracken]	noted
6-1366	A	29:56	30:4	Rutherford et al [2005] uses proxies calculated using the flawed principal components method of MBH98, discussed in McIntyre and McKitrick [2005a]. The flaws have been confirmed by von Storch and Zorita [GRL, 2005] and Huybers [GRL, 2005]. See also McIntyre and McKitrick [2005c, 2005d]. [Stephen McIntyre]	Noted – but method different in two papers
6-1367	A	30:1	30:1	Since the issue of the ability of methods to retain low-frequency variability is raised in the discussion of other studies, it should also be noted here that Rutherford et al (2005) used a method which explicitly calibrates low-frequency (multidecadal and longer-term) variability separately, to preserve low-frequency variations. [Michael Mann]	Noted – but text may not accept this suggestion
6-1368	A	30:6	30:51	This is a very helpful discussion and would be even more helpful if it occurred first in this subsection, along with the table of types of data that I suggested in another comment. Putting this first would make the subsequent description of the range of reconstructions and their uncertainties much easier to understand. [Susan Solomon]	Noted – we will consider the structure of the sections in revised draft
6-1369	A	30:6		Is there an "outside" critical evaluation of the various statistcal techniques that have been used? [Bryant McAvaney]	Not one presently available that could be cited
6-1370	A	30:10	30:10	Should add "Mann and Jones, 2003" after Crowley and Lowery (2000). [Michael Mann]	
6-1371	A	30:12	30:13	Move "explicitly" to before "provides" [Michael MacCracken]	accepted
6-1372	A	30:14	30:14	Should add "Rutherford et al 2005", and delete "Rutherford et al, 2003" which hardly seems relevant (they don't actually produce any proxy-based reconstruction!).	Accepted

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				[Michael Mann]	
6-1373	A	30:18	30:18	Doesn't seem appropriate to cite Mann and Jones (2003) here. While some of the proxy series may represent regional averages, the method used is very much the simple "composite-plus-scale" method used by others--in particular there is no use of any transfer functions or teleconnections or pattern-based calibration techniques. Far more appropriate to lump in w/ Jones et al and Crowley and Lowery approach than with other approaches. [Michael Mann]	Noted
6-1374	A	30:20	30:23	It seems entirely inappropriate to cite Rind et al (2005) here, since that paper provides no test whatsoever of the methods being discussed. Any implications of Rind et al (2005) for pattern-based reconstruction techniques is speculative, at best. If any studies are cited, it should be those which specifically test the stability of relationships between sparse proxy datasets made up of a mix of extratropical and tropical indicators (as is the case with most multiproxy datasets) and large-scale fields, in climate field reconstruction. Such tests are provided by Rutherford et al (2003) and Mann et al (in press): Mann, M.E., Rutherford, S., Wahl, E., Ammann, C., Testing the Fidelity of Methods Used in Proxy-based Reconstructions of Past Climate, Journal of Climate, in press (to appear in Oct 15 issue), 2005. These studies support the conclusion that the sort of non-stationarity alluded to does not seem to be a factor, at least for modeled climate changes of the past thousand years. Another study (Von Storch et al, 2004) comes to different conclusions. But a fundamental error with that study now been established which, as discussed in more detail in a subsequent comment, renders its conclusions very much in doubt. [Michael Mann]	Reject – but reference still considered of sufficient relevance to cite in this context
6-1375	A	30:25	30:51	A big issue is concerned to the fertilization effect of atmospheric CO2 on tree ring width and density for the past 100-200 years. This effect could not be easily distingused from the temeprature effect, and it should be assessed in the uncertainty analysis. [Guoyu REN]	Noted and generally accepted as worthy of some mention
6-1376	A	30:25	30:30	This statement is ambiguous because it is not always true. Transfer functions based on species % data using multivariate analyses of physiochemical data do not require empirical calibration against time line data. [James Shulmeister]	Accepted – text will be modified
6-1377	A	30:26	30:26	Delete "certainly"--virtually nothing is certain here, so this really mistakenly picks out one point rather than saying this about everything. [Michael MacCracken]	Accepted
6-1378	A	30:29	30:31	Most proxy calibrations do not consider autocorrelation. This limitation should be stated and carried forward to summary. [Stephen McIntyre]	Rejected – statement untrue

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6-1379	A	30:32	30:41	It seem relevant to be more specific here, since the current wording is quite difficult to follow for non-specialists ("...specific choice of 'target' and dependent variable."). This is about the differences in reconstructed temperature amplitude between the various reconstructions shown in Figure 6.8b - that is related to the choice of calibration against instrumental data as addressed in my third comment (and should be the same for all reconstructions shown in Fig. 6.8b). If the authors wish to keep the differing scaling approaches in Fig.6.8b, it would be necessary to say that the choice of calibration method, period of overlap with instrumental data, target instrumental data, and using original or smoothed data, all effect the reconstructed absolute temperature amplitude. Such effects were analyzed in Esper et al. 2005 (in GRL), where it is shown that these differences in calibration easily change the reconstructed T amplitude in the order of 0.5 C. From this, the latter discussion of borehole evidence, and other uncertainties highlighted in a recent paper, this upcoming IPCC report should state that the long-term T amplitude is not understood (see Esper J, Wilson RJS, Frank DC, Moberg A, Wanner H, Luterbacher J (2005) Climate: past ranges and future changes. Quaternary Science Reviews 24, 2164-2166.) [Jan Esper]	Rejected – sufficient of the esscence of the problems is conveyed. The point regarding amplitude uncertainty will be made clear
6-1380	A	30:43	30:45	The confidence interval calculatinos are not "clearly" described in any of the publications. [Stephen McIntyre]	Noted
6-1381	A	30:43	30:45	If these are "minimum uncertainty", what is the estimated uncertainty? [Stephen McIntyre]	Somewhat higher but not published
6-1382	A	30:43	30:51	This is the best statement what I have read so far in this chapter…. [Guoyu REN]	Noted
6-1383	A	30:43	40:43	MBH have refused to provide residuals for the controversial 15th century step and should not be included until this data is provided [Stephen McIntyre]	Rejected
6-1384	A	30:46	30:51	While there are many sources of uncertainty, it really also needs to be said that there are thermodynamic constraints that do impose some limits on how much climate can vary from one year to the next, so there are not boundless uncertainties. It might therefore be added that, in the absence of sudden external forcing, there are year-to-year correlations in climate over time that likely limit the range of variations from year-to-year to not much more than plus or minus a half degree--and in the reconstructions, that there is such sparse coverage might well yield more apparent variability than is the case for the world as a whole due to the sampling problem and counteracting variations. So, I would urge not leaving this statement so open-ended. [Michael MacCracken]	Noted – but suggestion probably will not be accepted because no publication qualifies or substantiates the degree to which this can be assured
6-1385	A	30:48	30:51	The statement "in at least some cases, any possible limitations…regression techniques"	Noted – though not entirely accepted

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				appears to inappropriately combine two distinct issues. One issue (as in Esper et al) involves questions regarding the methods used to standardize tree-ring data, and not the regression/calibration methods used (the method used is the simple composite-plus-scale approach discussed earlier in the section). The other issue involves the claim that pattern-based reconstruction approaches systematically underestimate low-frequency variability. That claim is largely based on paper by Von Storch et al (2004) which has subsequently (i.e., since the drafting of this section) been found to have a major error (separate comment below). The claims of the paper are now seriously in question and should not form the basis for any IPCC conclusions. [Michael Mann]	Subsequent work still supports the core of Von Storch's point, though not the degree. Point on structure (different points) will be considered.
6-1386	A	30:48	30:51	The statements here appear to be based in large part on claims made in a study by von Storch et al (2004). These authors claimed that pattern-based calibration approaches significantly underestimate the true low-frequency variabilty, based on the *claimed* application of the method of Mann et al (1998) to synthetic proxies produced from a long model simulation. As noted later in the chapter, there are now known to be major disequlibrium problems w/ the model simulation they used (in fact, there are drifts of several degrees C in the simulation prior to the AD 1000 date, and these appear to continue through the entire simulation). While these problems alone would of course render their conclusions questionable, it has more recently been found that there is a far more fundamental problem with the study. A subsequent study by Mann et al [Mann, M.E., Rutherford, S., Wahl, E., Ammann, C., Testing the Fidelity of Methods Used in Proxy-based Reconstructions of Past Climate, Journal of Climate, in press, 2005] based on application of a pattern-based reconstruction approach (one which is slightly different from, but which yields essentially the same result as the original Mann et al method when applied to the same proxy data set) to a different forced simulation (NCAR CSM1.4 coupled model) of the past 1000 years found no support for the von Storch et al (2004) claims. In this latter study, pattern-based approaches were found to faithfully reconstruct low-frequency variability for a wide range of signal-to-noise ratios. At the time the study went to press, the precise reason for the difference in conclusions from Von Storch et al (2004) was not known. Now it is: Wahl et al [Wahl, E.R., Ritson, D.M. and C.M. Ammann, Reconstruction of Century-Scale Temperature Variations, under review in "Science" (please respect the embargo!)] have shown that there was a fundamental error in the way that Von Storch et al implemented what they *claimed to be* the Mann et al (1998) method. Von Storch et al, for an as-yet unknown reasons, chose to *detrend the low-frequency variability from the data prior to calibration* (this was determined after repeated email exchanges between D. Ritson and H. Von Storch in an attempt to determine precisely what Von Storch et al had done). As shown by Wahl et al, implementation of this erroneous procedure gives a completely different result from the	Accepted – in as much as these issues require further discussion and additional references to be included to bring the representation of the evolving debate to date.

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				correct procedure. It now appears that this methodological error on the part of Von Storch et al (2004) is the primary reason they produces too little low-frequency variability. Thus, it now appears that the Von Storch et al (2004) study is fundamentally flawed, and the conclusions of the study unreliable at best, meaningless at worst. I would thus urge the authors to avoid basing any IPCC conclusions either directly or indirectly, on this study. [Michael Mann]	
6-1387	A	30:53	30:54	This statement is misleading. A number of reconstructions using entirely independent or partially independent data, and different methods, give results that are quite close to those reconstructions (Mann et al, 1999; Briffa et al, 2001; Jones et al, 1998) that were featured in the TAR. The most recent study, using entirely independent data that is not obviously prone to any underestimation of low-frequency trends--global glacial mass balance changes (Oerlemans, H.., Extracting a Climate Signal from 169 Glacier Records, Science, 308, 675-677, 2005) gives a result that is at the lower-end amplitude of variability, similar to Mann et al, 1999 and the other reconstructions shown in the TAR. It is unclear why Oerlemans' NH reconstruction is not shown in figure 6.8 as it is perhaps the *most* independent estimate from the others, aside from the boreholes. Of course, it has its caveats, but this is hardly a reason for excluding it--the same can certainly be said of all other reconstructions. Rutherford et al (2005) obtain reconstructions that are quite similar to those found in the TAR as well. And several reconstructions suggesting more variability (Moberg et al and Esper et al) agree remarkably poorly with each other. Moreover, the methods used in these latter studies have been called into question: Esper et al because of their overly liberal implementation of the RCS tree-ring standardization method, and Moberg et al because of their use of a statistical scaling approach that can artificially inflate low-frequency variability as shown by Mann et al (2005) [Mann, M.E., Rutherford, S., Wahl, E., Ammann, C., Testing the Fidelity of Methods Used in Proxy-based Reconstructions of Past Climate, Journal of Climate, in press, 2005]. Given all of this, it appears, first, factually incorrect to say that more recent studies indicate greater variability than the TAR--this is not the case. Moreover, it is misleading to overly emphasize those studies suggesting greater variability because of the issues raised above. The statement should be reworded to more accurately reflect the current state of our knowledge, which is indeed one of uncertainty, but not one which appears to selectively favor reconstructions that exhibit greater variability. [Michael Mann]	Largely accepted – Oerlaman's temperature series will be included and text modified to incorporate discussion of it. The inference of the timing and interpretation of 'recent' recontructions will be examined to make it neutral.
6-1388	A	30:54	30:55	Is this true? What are not encompassed? [Stephen McIntyre]	Reject – text clear
6-1389	A	30:56	31:4	The statement on the "magnitude of past cool excursions" is relevant, however, as presently addressed is of limited use given that the records in Figure 6.8b are all scaled	Rejected – no systematic recalibration of all records has been published or

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				differently. As said above, it would be useful to scale all the records in the same way, since these differing calibration approaches affect the relative position of single reconstructions within the pool of records, and thus the reconstructed T amplitude. [Jan Esper]	would necessarily be appropriate
6-1390	A	31:1	31:1	"Only one.." The reconstruction CED2004 also has a pronounced max. around yr 1000 -- although less long than the warm phase in MSDDK2005. I would suggest to phrase this a bit more careful. [Michael Schulz]	Accepted
6-1391	A	31:1	31:4	The reader will again wonder why they should not believe the Moberg record more than others. [Susan Solomon]	Noted – text will be modified
6-1392	A	31:6	31:23	This may not be the venue to express this but this paragraph highlights the problems of fucussing on a fairly meaningless climatic parameter - Mean Annual Temperature. We would be much better served if we tried to move to seasonal values and some measurement of evapotranspiration. [James Shulmeister]	Noted
6-1393	A	31:6	31:23	Much of this material will also be more helpful if it is given earlier in the subsection. It would also be helpful to show the coral data, and define what is meant by 'unusual'. [Susan Solomon]	Noted – but not necessarily accepted in reconsidered structure.
6-1394	A	31:6	31:23	This para comes over as very defensive. Would it not be better just to describe and show the evidence. [Eric Wolff]	Accepted
6-1395	A	31:11	31:16	I cannot understand why an important paper of Kaser et al. is not discussed here (G. Kaser, D. R. Hardy, T. Mölg, R. S. Bradley and T. M. Hyera (2004) "Modern glacier retreat on Kilimanjaro as evidence of climate change: observations and facts," Int. J. Climatol., 24, 329-339.) Although this paper deals with a special case where the glacier retreats because of moisture deficiency, it can suggest how you can work out a countermeasure for the reservation of the important glacier. Thus, the sentence, "However, very rapid and apparently unprecedented melting of tropical ice caps has been observed in recent decades, possibly linked to sharply rising SST observed in the tropics after 1976 as well as enhanced warming at high elevations." is not correct for Kilimanjaro. [Kiminori Itoh]	Noted
6-1396	A	31:11	31:13	Oxygen isotope series from high-elevation ice cores provide the longest records, but most represent changes in the source region of precipitation, as well as local temperature. : this should be carried forward to proxy uncertainty summary. [Stephen McIntyre]	Rejected – not considered of sufficient import to justify

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6-1397	A	31:13	31:13	citation [Stephen McIntyre]	Accepted
6-1398	A	31:13	31:13	what is evidence that the melting is "unprecedented": earlier there were statements that these glaciers formed or re-formed in the Neoglacial. [Stephen McIntyre]	Rejected – beyond scope of limited text space
6-1399	A	31:14	31:15	Is it really melting? It might be sublimation. Section 4.5.3 says this is due to insolation, not warming. In any case, "recent decades" is not palaeoclimate, so I suggest replacing this with a reference to 4.5.3. [Jonathan Gregory]	Accepted – perhaps in part only – but cross reference should be made
6-1400	A	31:14	31:23	The discussion of modern changes (post 1976) seems to pop out of nothing. Motivation unclear. [Jochem Marotzke]	Rejected – reference to modern conditions in relation to past is relevant
6-1401	A	31:16	31:18	d18O is affected by both SST and salinity and only in areas where salinity is ctant d18O can be interpreted in terms of SST alone. On the contrary, the Sr/Ca ratio is mainly a proxy for SSTs. On the other hand, changes in salinity are not only associated to precipitation variability but also to evaporation and vertical/horizontal mixing of different water masses. [Eva Calvo Costa]	Accepted
6-1402	A	31:16	31:18	Coral oxygen isotopes and Sr/Ca ratios primarily reflect SSTs, though they are also influenced by salinity changes associated with precipitation variability. - carry forward to proxy undertainy section. Add citation. [Stephen McIntyre]	Noted – but not to be carried forward
6-1403	A	31:20	31:20	Wilson and al." should read "Wilson et al. [James Crampton]	Accepted
6-1404	A	31:25	31:26	This statement is misleading if not false, for the same reason as given above (comment on page 30, lines 53-54). A more accurate statement is that there is now a larger range of reconstructions, some indicating variability remarkably similar to that indicated in the TAR (Oerlemans et al, D'Arrigo et al, Rutherford et al), some indicating greater variability (Moberg et al, Esper et al). Moreover, studies which have controlled for factors such as spatial sampling and seasonality (Rutherford et al, 2005) suggest that restricted sampling may be the reason for enhancement of low-frequency variability in some studies (such as Esper et al, 2002). This is consistent with modeling studies of the dynamical responses to forcing, which have seasonally and regionally-specific patterns of response (e.g. Shindell, D.T., Schmidt, G.A., Miller, R.L., Mann, M.E., Volcanic and Solar Forcing of Climate Change during the Preindustrial Era, Journal of Climate, 16, 4094-4107, 2003). Those reconstructions which suggest greater variability such as Esper et al and Moberg et al, show almost no internal agreement at all, which hardly wins confidence in	Accepted – in part. There are some references and amendments to discussion as it now stands that will inprove the ambiguity in implication that larger amplitude reconstructions are correct – when in fact we do not yet know

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				the meaningfulness of the enhanced low-frequency variability in these studies. The point that should instead be emphasized is that despite the greater range of reconstructions now available, they almost all fall within the uncertainties shown in the TAR, and support the key conclusion. The following sentences (lines 26-28) correctly reflect that conclusion. [Michael Mann]	
6-1405	A	31:25	31:26	What is the conclusion relative to the MWP? This would be less promotional. [Stephen McIntyre]	Reject – next sentence conveys this meaning
6-1406	A	31:25	31:28	This paragraph is likely to be selected into the SPM and needs revision since it overstates the case. The "new" multiproxy evidence heavily overlaps with the old evidence, since the new studies re-use many of the same old proxies. Also, the conclusion entirely rests on splicing the surface observational record onto the proxy record, a step that has no valid statistical foundation. Finally, it overlooks the critique of the basis for the comparable position set out in the TAR. It should be reworded as follows: "There is insufficient new, independent multiproxy evidence since the TAR to settle the question of the state of the present climate in comparison to that of previous centuries. The historical climate appears to have been characterized by greater natural variability than was shown in the TAR, and the statistical basis for using long proxies to extrapolate back from current instrumental records appears to have been weaker than was stated in the TAR." [Ross McKitrick]	Noted but rejected inference. The text makes clear the points made in the text suggested here, but its principal attack, on TAR, is unjustified.
6-1407	A	31:25	31:28	Excellent paragraph (and the discussion leading up to it also - although it is long, it is essential, because of mis-representations of the hockey sticks by some commentators). [Neville Nicholls]	Noted
6-1408	A	31:25	31:28	The conclusion needs to be rewriten in view of the above comments. Don't mention the past 400-500 years, and don't count the past 2000 years either. There are little high-resolusion records for indicating annual mean temperature for the last 2000 years, as has been assessed in this chapter, and concentrate on the past 1000 years as did the TAR. [Guoyu REN]	Noted – and partly accepted – evidence will stress less than 2000 years
6-1409	A	31:26	31:28	I just do not believe that there are enough data to say that the 20th century warmth was unusual in a 2000-year context, at least not, if this statement is made with regard to large scale/NH (which is admittedly not explicitly stated here). I suggest to either limit this statement temporally, e.g. the last 1200, or to add that this statement is made with respect to certain locations of NH from where such long data are available. [Jan Esper]	Noted – and partly accepted – text will be revised to stress little evidence and shorter period.
6-1410	A	31:27	31:27	"Christian era" is not what many people would call it. You could just omit that phrase. [Jonathan Gregory]	Accepted
6-1411	A	31:27	31:27	would be better if this read "new reconstructions that reach back across the past two millennia"	Noted

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				[William Howard]	
6-1412	A	31:27	31:27	Shouldn't IPCC adopt a culturally neutral age scheme? BP(1950) is fine for older times, but either CE, or BCE (common era and before common era) are probably better the millenium of the Christian Era. [Gavin Schmidt]	Accepted
6-1413	A	31:27	31:27	Please avoid the phrase "Christian era" when referring to a timescale [Michael Schulz]	Accepted
6-1414	A	31:27		Section # 6.4: "Christian era" should be replaced by "common era". [Becky Alexander]	Accepted
6-1415	A	31:27		Change "Christian Era" to "Common Era" [Melanie Fitzpatrick]	Accepted
6-1416	A	31:27		"Christian era" should be removed and replaced with some other phrase. [Katsumi Matsumoto]	Accepted
6-1417	A	31:27		is this the best way to define the time interval: first millennium of the Christian era, [Robert Webb]	Accepted
6-1418	A	31:28	31:28	I would suggest saying "in at least a 2000-year context" [Michael MacCracken]	Noted – probable reject in interests of conservalism
6-1419	A	31:28	31:28	Please define what is meant by 'unusual'. [Susan Solomon]	Accepted – will rephrase
6-1420	A	31:30	31:33	There are several large-scale analysis that should be included here. Beltrami (2002a) in references, and Beltrami and Bourlon (2004) references given below including a link to the article [Hugo Beltrami]	Noted Pollock
6-1421	A	31:30	32:12	One extremely important issue that, in my opinion, should be mentioned here is the fact that borehole data allowed for the estimation of the heat absorbed by the ground. This is independent of the method of analysis, and simply reflects the energy stored underground. The first estimates of heat absorbed by the Earth continents showed that the heat absorbed in the last 50 years is of the same order of magnitude than the heat absorbed by the atmosphere (8.0 x 1021 J) Beltrami et al., 2002, Beltrami 2001b, Levitus, 2001, Levitus et al., 2005) and also that the present warming of the planet has a global character. [Hugo Beltrami]	noted. Although topic is important, it is not paleoclimate.
6-1422	A	31:30	32:12	References that should be added: (please see supplemental review doc) [Hugo Beltrami]	noted
6-1423	A	31:30	32:12	This section should also cite borehole temperature reconstructions from ice cores. E.g. Dahl-Jensen, D., et al., 1998: Past temperature directly from the Greenland Ice Sheet. Science, 282, 268-271. And Cuffey, K.M. and G.D. Clow. 1997; Temperature,	noted

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				accumulation, and ice sheet elevation in Central Greenland through the last deglacial transition. J. Geophys. Res., 102: 26,383-26,396 [William Howard]	
6-1424	A	31:30		Replace Heading with “Ground surface temperatures” [Vincent Gray]	Accept Pollock
6-1425	A	31:30		This section seems to miss mention of ice sheet borehole thermometry. There is a wide literature - Dahl-Jensen, Clow etc… In fact, we would still be ignorant of the extent of glacial temperatures in central Greenland if not for the borehole work there. [Tas van Ommen]	noted, discussed in earlier section
6-1426	A	31:31	31:53	This discussion on boreholes is helpful, and would also be helpful to move up along with the other material on methods, to the front of the subsection. [Susan Solomon]	noted
6-1427	A	31:34	31:36	Not all 695 sites are available to the general user. Some of these data are not available on line. [Hugo Beltrami]	noted, all data is available upon request, but some must be requested from the original provider, e.g., British Geological Survey, National Geophysical Research Institute of India, etc.
6-1428	A	31:36	31:39	delete generally [Hugo Beltrami]	noted, sentence has been rephrased
6-1429	A	31:38	31:38	In that "the Earth" is usually taken to mean the entire Earth system (atmosphere, oceans, etc.), I would think it would be better her to say "solid Earth" or "ground" or something to indicate that this means the soils, etc. [Michael MacCracken]	accepted
6-1430	A	31:39	31:41	references are needed here e.g. Barlett et al, 2004, Nitoiu and Beltrami, 2004 [Hugo Beltrami]	Bartlett et al. (2004) added.
6-1431	A	31:41	31:43	eg, Beltrami, 2001a [Hugo Beltrami]	rejected, Belttrami (2001a) not included; it provides only a local example.
6-1432	A	31:43	31:52	However, a couple of recent papers have clarified this issue, Beltrami et al, 2005 have shown that even under conditions of high snow cover variation and in the presence of freezing periods, ground temperatures appear to record long-term SAT trends. Furthermore, an yet unpublished paper (Gonzalez-Rouco et al, 2005) has confirmed the coupling of SAT and GST at long-term scales using three ECHO-G simulations of the Earth’s climate for the last 1000 years. [Hugo Beltrami]	Beltrami et al. (2005) not included; it provides only a local example. Gonzalez-Rouco et al. has been added and discussed.

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6-1433	A	31:45	31:47	Unbalanced number of references compared to other parts of the chapter. [Heinz Wanner]	accepted, three references have been removed
6-1434	A	31:47	31:47	Delete comma before "using". [Martin Stendel]	noted, sentence has been reconstructed
6-1435	A	31:48	31:48	…three-dimentional coupled models are unable to replicate deep soil… [Steven Clemens]	noted, sentence has been reconstructed
6-1436	A	31:49	31:50	Serious problems w/ the ECHO-G simulation discussed here, which are acknowledged later in the chapter (page 34), compromise any conclusions from this study. The unrealistic nature of the forcings used and the serious long-term drift insure that the relationship between changes in different variables is unlikely to be realistic (i.e., the true patterns of covariability between the variables of interest are unlikely to be expressed in this simulation). This caveat should be noted here. [Michael Mann]	Rejected – forcings are 'in line' with other simulations
6-1437	A	31:49	31:49	Add comma before "using". [Martin Stendel]	Accept
6-1438	A	31:50	31:50	Should this not say "that changes in deep soil temperatures were indistinguishable from changes in continental annual SAT"? And perhaps the comparison later in the sentence should also be with respect to "changes" rather than absolute quantities? [Michael MacCracken]	Accept
6-1439	A	31:55	31:55	...average Northern Hemisphere GST (Pollack and Smerdon, 2004). [Eva Calvo Costa]	Accept
6-1440	A	31:55	31:56	What about their MWP reconstructions? [Stephen McIntyre]	noted
6-1441	A	32:0		Report on possible reasons for the last few decades in the tree ring reconstructions of the southern hemisphere which do not indicate a warming (Figure 6.9). Is there some evidence of a stronger SST influence? [Heinz Wanner]	Reject – not sure why but simply representing published evidence
6-1442	A	32:2	32:2	I guess 0.1 here should be 1.0. [Philip Jones]	Reject – no
6-1443	A	32:4	32:5	Should reference here Mann et al (2003) [Mann, M.E., Rutherford, S., Bradley, R.S., Hughes, M.K., Keimig, F.T., Optimal Surface Temperature Reconstructions using Terrestrial Borehole Data, Journal of Geophysical Research, 108 (D7), 4203, doi: 10.1029/2002JD002532, 2003] who show that measures of agreement between spatial patterns of change in 20th century borehole and instrumental temperature data imply far greater uncertainties and potential bias. [Michael Mann]	Reject – large uncertainty in associations between temperature patterns and borehole patterns in this paper

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6-1444	A	32:9	32:12	and Beltrami and Bourlon, 2004 [Hugo Beltrami]	Noted Pollock
6-1445	A	32:9	32:12	In recent months work by Mober (2005) have confirmed the results from borehole temperature reconstructions. This should be mentioned here. [Hugo Beltrami]	Noted Pollock
6-1446	A	32:9	32:10	Rutherford and Mann (2004) reference is wrong. The reference should be: Mann, M.E., Rutherford, S., Bradley, R.S., Hughes, M.K., Keimig, F.T., Optimal Surface Temperature Reconstructions using Terrestrial Borehole Data, Journal of Geophysical Research, 108 (D7), 4203, doi: 10.1029/2002JD002532, 2003. [Michael Mann]	Accepted
6-1447	A	32:9		hemispheric [Hugo Beltrami]	noted
6-1448	A	32:11	32:12	Suggest changing "in less agreement" to "are less consistent" and change "least" to "the least warming". I think "agreement" is a bit like pregnancy"--you are either in agreement or not. [Michael MacCracken]	Accepted
6-1449	A	32:11	32:12	"and in less agreement with those that show least."--isn't that a tautology? Seems unnecessary. [Michael Mann]	Accepted
6-1450	A	32:14		While it is good that a separte evalutin of SH variability is attempted the material presented is very reginally focussed (as is acknowledged in text) and hence is even more difficult to put into a SH context than the individaul studies for the NH. [Bryant McAvaney]	Noted
6-1451	A	32:16	32:29	Chapter 6.5.2 on the Southern Hemisphere starts with a paragraph on the Mann and Jones (2003) SH reconstruction, even though neither this record nor the regional Quelccaya and Law Dome records (which are 2 of the 3 records included in the Mann and Jones SH reconstruction and also mentioned in the text) are shown in the relevant figure 6.9. The reason for not showing the SH record is indicated to be that combining 3 regional datasets is just too little to address SH, and the paragraph consequently concludes that "it is probably more appropriate at this time to consider evidence in terms of limited regional indicators". I fully agree with the conclusion to focus on regional evidence, but then do not see the point of spending a full paragraph on something that is not shown, and from which most of the palaeoclimatic community believes has too little data to be of use. So, it is suggested to remove this first paragraph, which would allow the existing regional evidence to be more completely addressed. [Jan Esper]	Accepted, the paragraph will be substantially reduced to provide just relevant information.
6-1452	A	32:16	32:29	Please show the distribution of SH records, along with the NH ones, if you include the	Accepted. A new figure will be

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				figure suggested in another comment. [Susan Solomon]	included to show location of NH and SH proxies
6-1453	A	32:17	32:17	I would suggest deleting "centuries" or maybe change this to refer to "over the past few thousand years" or something similar. [Michael MacCracken]	Rejected. Current wording appropriate.
6-1454	A	32:21		Ref van Ommen et al - year 2004. Annals of Glaciol 39 is 2004. Also in refs list. [Tas van Ommen]	Accepted. Text modified.
6-1455	A	32:22	32:24	Is this methodology correct? Or is there a correlation-weighted average? [Stephen McIntyre]	Accepted. Text modified. Composite series were formed from weighted combinations of the individual standardized proxy series
6-1456	A	32:26	32:26	If SH was warmer in the period AD950-1000, does this not suggest a global warming (in comination with Naurzbaev et al 2004]? [Stephen McIntyre]	Rejected. The present text is balanced. There are large uncertainties in the SH temperature reconstruction
6-1457	A	32:33	32:39	It seems that the text addressing the Tasmania data is not in line with the record as shown in Figure 6.9. The text refers to the past 2000 and a warm period spanning 900-1500 AD is mentioned, whereas the figure shows the reconstruction back to AD 1000 only. [Jan Esper]	Accepted. Text will be edited to correspond with the figure.
6-1458	A	32:35	32:35	It is not really clear what "it" is--this seems to say that the warming trend is the warmest event, which does not make much sense. [Michael MacCracken]	Accepted. Text modified.
6-1459	A	32:41	33:4	I am very uncomfortable with the assertions of warming in the Southern Hemisphere from these data. An examination of figure 6.9 shows that the existing New Zealand data (bottom graph) cannot be construed to demonstrate any anomalous late 20th century warming. Similarly the data from Patagonia diverges on whether warming is present (top two graphs). In fact the late 20th century flips in NZ and Tassie look more like the alternating phases of the Interdecadal Pacific Oscillation rather than a directional trend. The long term warming trend appears to be derived from from borehole data only. I would regard this singular line of evidence as unsatisfactory to demonstrate a trend. Consequently, I would remove the claim of unusual warmth for the last 50 years [page 33,lines2-4] and replace with a statement that warming may be occurring but that more data is required to verify the apparent trend [ditto for the southern Hemispher bullet point on p4 lines 14-16]. Much more work is needed. Statements about warming trends in this region can be safely made from instrumental records but until we have a better handle on long-term variability the value of such statements is limited. [James Shulmeister]	Accepted, text will be edited to reflect this point.
6-1460	A	32:47	32:52	Discuss 1000 year results.	Accepted.

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				[Stephen McIntyre]	
6-1461	A	32:47		century (lower case) [Eric Wolff]	Accepted.
6-1462	A	32:54	32:57	& Fig 6.9 : the fact that borehole temperatures stop at about 1990 and are prolongated by smoothed observed temperature, increase the impression of hockey stick. This heterogeneous mixture of two sources of data lets the impression that the warming is 1.8 C/500 yrs in S. Africa while borehole alone tells us that it is about 0.7 C. The observed temperature should be smoothed like borehole ones. [Joel GUIOT]	Accepted. Text modified to take account of this.
6-1463	A	32:57		Include the findings of South African past climate variability (Tyson et al. 2002: Tyson, P.D., Cooper, G.R.J. and McCarthy T.S., 2002: Millennial to multi-decadal variability in the climate of southern Africa. International Journal of Climatology 22, 9, 1105-1117) and references therein. [Heinz Wanner]	Rejected. Not sufficient temporal (annual) resolution, not quantitative reconstructions included in this reference.
6-1464	A	33:1	33:3	The conclusion is not supported by the previous discussions of warmer 950-1000 AD period. [Stephen McIntyre]	Accepted. Text modified.
6-1465	A	33:2	33:4	The concluding remark on SH palaeoclimate stating that "the warmth of the last 50 years is unusual in a 350 to 1000 year context" is certainly not supported by the evidence described in the text, and particularly not by the recons shown in Figure 6.9. I suggest removing "to 1000" from this statement, since the two only records spanning the past millennium and shown in Figure 6.9 do not support this conclusion. For Tasmania, there are several pre-instrumental periods visible that are just as warm as the late 20th century. And for New Zealand, several pre-instrumental periods were seemingly warmer than the late 20th century. [Jan Esper]	Accepted. Text modified.
6-1466	A	33:2	33:4	Bearing in mind that uncertainty is a key theme, I do not think that this statement in its present form should be included, based on the figures (fig 6.9) presented. It should be qualified, in that, some data sources from the SH indicate that the warmth of the last 50 years is unusual, but that the data from Australasia does not support this. [Rowan Fealy]	Accepted. Text will be edited to account for this issue.
6-1467	A	33:2	33:4	Suggest removing this statement. It does not add to the more precise statements earlier in this section. Does this statement mean that it is likely that the last 50 years in the SH is warmer than any in the previous 1000? No, the data are not sufficient to prove this. [Haroon Kheshgi]	Accepted.
6-1468	A	33:2	33:4	This statement is not a fair accounting of the evidence just presented; a correct statement is that the evidence supports the concept of general warming conditions over the past 500	Accepted.

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				years, but that warmer conditions prior to that time may well have existed, possibly commensurate with current conditions in the S.H. [Andrew Lacis]	
6-1469	A	33:2	33:2	in AT LEAST a 350 to 1000 year context [Neville Nicholls]	Rejected. Text modified to be more conservative with results.
6-1470	A	33:8	33:12	The simulation of Stendel et al. (Stendel, M., I.A. Mogensen and J.H. Christensen, 2005a: Influence of various forcings on global climate in historical times using a coupled AOGCM. Clim. Dyn. 25, 10.1007/s00382-005-0041-4) should also be included here. In this case, "three" needs to be changed to "four" in chapter 6, page 33, line 11. [Martin Stendel]	Accepted. Reconstruction will be included.
6-1471	A	33:8	34:32	The discussion here, based on Fig. 6.10 and references therein, suggest a definite MWP with maximum warmth in the 1100's AD. Admitedly, this is a reconsturction of NH temo NH temperature, but to the degree that a) the forcing is properly simulated (significant uncertanties) and b) the proxy records are able for that period of time to accurately depict a hemispheric temperature average, one woujld to conclude that within the limits of these uncertainties, the recent warm period is "comparable" to multidecadal periods in the 1100's AD [Henry Diaz]	Noted. Authors believe text represent a balance view.
6-1472	A	33:8	:9	This sentence reads like a figure caption and can be deleted ('F6.10' is the topical noun of the topic sentence for the paragraph and according 4th grade grammer the rest of the paragraph should focus on attributes of 'Fig6.10' such as the nice choice of colors, readability of the axes and text, choice of thickness of the lines, ...) [Robert Webb]	Accepted. Text modified to take account of this.
6-1473	A	33:9	:12	Move sentence to end of following paragraph and reference Figure 6.10 [Robert Webb]	Rejected. Authors believe text is clear.
6-1474	A	33:12	33:12	Ammann et al (2003) only gives the forcings. The CSM 1.4 coupled simulation of the past millennium is described in more detail here: Mann, M.E., Rutherford, S., Wahl, E., Ammann, C., Testing the Fidelity of Methods Used in Proxy-based Reconstructions of Past Climate, Journal of Climate, in press (to appear in Oct. 15 edition), 2005] [Michael Mann]	Accepted. Reference added.
6-1475	A	33:17	33:17	Replace Berger reference with updated following updated reference: Laskar, J., F. Jouzel, et al. (1993). “Orbital, precessional, and insolation quantities for the Earth from -20 Myr to +10 Myr.” Astron. Astrophys 270: 522-533. [Steven Clemens]	Accepted. Reference added.
6-1476	A	33:19	33:19	What, specifically, do "they" and "these" refer to? Some factors? Orbital variations? [Michael MacCracken]	Accepted. Text modified.

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6-1477	A	33:22	33:22	Reference to Figure 6.10: The simulation of Stendel et al. (Stendel, M., I.A. Mogensen and J.H. Christensen, 2005a: Influence of various forcings on global climate in historical times using a coupled AOGCM. Clim. Dyn. 25, 10.1007/s00382-005-0041-4) should also be included. [Martin Stendel]	Accepted. Model results will be included in the figure.
6-1478	A	33:24	33:51	Need here a discussion about the fact that the solar variability depends on the spectrum considered. Variability in the UV range is of the order of 10 % (Rottman et al., Advances in space reserach 27 (12) 1927-1932, 2001), far more than in the visible spectrum. Important consequences for the thermodynamics as well as the dynamics of the stratosphere, with possible consequences on the modes of variability of the troposphere (e.g. NAO). See, reviews ans studies by Labitzke and Matthes, Holocene 13 (3) 311-317 (2003) for influence of 11-year cycle of stratosphere, and Baldwin and Dunkerton, Journal of Atmospheric and solar-terrestrial physics 67 (1-2) 71-82 (2005) about the stratosphere-troposphere dynamical coupling in relation with solar forcing. [Michel Crucifix]	Rejected. Beyond the scope of the section given space limitations.
6-1479	A	33:24	33:51	Stress that it is still difficult, even nowadays, to get an absolute value of the total solar irradiance (difficult to calibrate satellites). This is the reason why it may be difficult to gather different times series valid for different time periods. [Michel Crucifix]	Rejected. Beyond the scope of the section given space limitations.
6-1480	A	33:24	33:28	a discussion of the sulphate record from Greenland ice cores would be useful here. [Thomas Stocker]	Rejected. Beyond the scope of the section given space limitations.
6-1481	A	33:27	33:27	"concentrations and distributions of tropospheric aerosols and ozone are not as well KNOWN" (instead of understood). [Michel Crucifix]	Accepted. Text modified.
6-1482	A	33:30	33:35	A reference dealing with a mechanism is necessary for the statement that the solar luminosity change had been ca. 0.1%. [Kiminori Itoh]	Accepted. Reference added.
6-1483	A	33:30	34:13	A sub-section heading relating to 'solar irradiance changes' or somesuch would help. [Bryant McAvaney]	Noted.
6-1484	A	33:30	34:32	Please coordinate this subsection on solar and volcanic forcing with chapter 2. [Susan Solomon]	Noted.
6-1485	A	33:34	33:35	Why change in citation format here to "J.L Lean et al" rather than "Lean et al"?? [Michael Mann]	Accepted. Text Modified.
6-1486	A	33:37	33:37	Define "reasonably good" [Thomas Karl]	Noted.
6-1487	A	33:37		what are you trying to say "There is generally reasonable-to-good temporal agreement"	Accepted. Text modified.

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				[Robert Webb]	
6-1488	A	33:41	33:41	Change to "Earth's atmosphere"--this is referring to the planet. [Michael MacCracken]	Accepted. Text modified.
6-1489	A	33:42	33:42	should read "sunspot numbers" [William Howard]	Accepted. Text modified
6-1490	A	33:42	33:43	As for the following description, "However, the relationship between sunspot numbers and solar magnetic field is not fully understood," a report of Itoh will give a further insight (K. Itoh, "A novel empirical relation between the aa index and sunspot numbers: theoretical considerations and applications," Japan Geoscience Union Meeting 2001, En-P001 (http://www-jm.eps.s.u-tokyo.ac.jp/2001cd-rom/pdf/en/en-p001_e.pdf). Although the paper is unpublished, it reasonably reproduces the change in the aa index on the basis of double magnetic cycles of the solar magnetic activity. [Kiminori Itoh]	Noted.
6-1491	A	33:42		sunspot numbers and not sunsport numbers [Heinz Wanner]	Accepted. Text modified
6-1492	A	33:45	33:51	The strong differences in the conclusions of Solanki et al (2004) compared to Muscheler et al. 2005a should appear more clearly in this paragraph. [Hugues Goosse]	Not accepted. Do not want to overemphasize the difference as there are also many similarities in the results of the two studies.
6-1493	A	33:49	33:49	should read "without precedent" [William Howard]	Accepted. Text modified.
6-1494	A	33:49		replace "in a similar vein that links" with 'linking' [Robert Webb]	Accepted. Text modified
6-1495	A	33:50		…three periods ….(give start and end years of those periods). [Heinz Wanner]	Accepted. Text modified
6-1496	A	33:55	33:55	Change "has been unable to confirm" to "neither confirms nor denies." In their abstract, Hall and Lockwood state: " While flat activity stars may be in periods of extended activity minima anallogous to the solar Maunder Minimum, a significant reduction in magnetic activity during such periods is not implied (although it is also not rejected) by the data." This section should retained the balanced view of the Hall and Lockwood data that the authors themselves took. Saying that Hall and Lockwood could not confirm Baliunas and Jastrow, when the paper says that it can neither confirm nor deny, is misrepresented the reference. [Jeffrey Kueter]	Accepted. Text modified to account for this point.
6-1497	A	34:1		Solar activity should be replaced with solar modulation since solar activity is not well defined. It could also refer to irradiance variations.	Accepted – changed to ‘open magnetic field flux’

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				[Raimund Muscheler]	
6-1498	A	34:4	34:13	Is there an error here? On line 11 it is stated that most reconstructions attribute a change of 1 W m-2 to the Maunder minimum. On line 13 it states that radiative forcing in chapter 9 is calculated on the basis of a 0.2 W m-2 reduction at the Maunder minimum. This looks contradictory. [James Shulmeister]	Noted. We will revise text.
6-1499	A	34:7	34:12	I think it is indeed helpful to be giving both the percentage change and the actual flux change--this should be done throughout (at least wherever percentages are given) [Michael MacCracken]	Noted.
6-1500	A	34:10	34:13	If the solar luminosity change between the Maunder Minimum and the present is ca. 0.1%, then you cannot explain the temperature changes observed. I think this discrepancy should be mentioned in the text more clearly. In this case, of course (and unfortunately), the model calculations shown in Figure 6.10 cannot be relied on. [Kiminori Itoh]	Rejected. Authors believe text represent a balanced view and the attribution issue is given in Chapter 9
6-1501	A	34:10	34:11	Also state Solanki results [Stephen McIntyre]	Noted.
6-1502	A	34:12	34:12	"the radiative forcing used in Chapter 9" : replace with "the magnitude of the radiative forcing used..." (to avoid ambiguity of "smaller" and "larger" when numbers are not absolute values. [Michel Crucifix]	Accepted. Text modified.
6-1503	A	34:15	34:32	A sub-section entiled 'volcanic forcing' would be helpful. [Bryant McAvaney]	Noted.
6-1504	A	34:15		line contains sentence that starts with a misplaced pronoun. "This derives" [Robert Webb]	Accepted. Text modified.
6-1505	A	34:19	34:19	I would not call the dating uncertainties in the ice cores 'minor'. This is a critical problem with almost all ice core studies. Drop '(in some cases) minor' [James Shulmeister]	Accepted. Text modified.
6-1506	A	34:26	34:26	Add 'either' after 'this, as' [James Shulmeister]	Accepted. Text modified.
6-1507	A	34:28	34:28	Cite here Ammann et al (2003); Mann et al (in press) [Michael Mann]	Noted. One reference added.
6-1508	A	34:29	34:29	Add reference to Stendel et al. (Stendel, M., I.A. Mogensen and J.H. Christensen, 2005a: Influence of various forcings on global climate in historical times using a coupled AOGCM. Clim. Dyn. 25, 10.1007/s00382-005-0041-4) who also prescribe geographic changes in radiative forcing. [Martin Stendel]	Reference added.

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6-1509	A	34:32		Section # 6.5.3: I think the discussion about the uncertainties of volcanic forcing should end by saying that these are sporadic events, and their climate forcing lasts only a couple of years at most (for the larger eruptions like Pinatubo). In other words, they don't contribute to long term climate change. [Becky Alexander]	Reject. Authors believe text represent a balanced view.
6-1510	A	34:34	35:14	The simulations of Southern Hemisphere temperature should be mentioned too as they underline a different behaviour compared to the Northern Hemisphere. This would give a complementary information to observations given in section 6.5.2. An example of the analysis of temperature in the Southern Hemisphere is given in "Goosse H., V. Masson-Delmotte, H. Renssen, M. Delmotte, T. Fichefet, V. Morgan, T. van Ommen, B.K. Khim and B. Stenni, 2004. A late medieval warm period in the Southern Ocean as delayed response to external forcing ? Geophysical Research Letters 31(6) L06203 doi:10.1029/2003GL019140". [Hugues Goosse]	Rejected. Space limitations and inconsistent with regional treatment of the proxy evidences. To discuss in the group.
6-1511	A	34:34		The lack of systematic experiemts to sort out diffference in forcings versus differences between models makes the statements here very subjective. [Bryant McAvaney]	Noted.
6-1512	A	34:36	35:7	Note: The discussion of the different models may have to be slightly modified when the Stendel et al. (Stendel, M., I.A. Mogensen and J.H. Christensen, 2005a: Influence of various forcings on global climate in historical times using a coupled AOGCM. Clim. Dyn. 25, 10.1007/s00382-005-0041-4) paper is included. [Martin Stendel]	Accepted. Text will be modified to correspond to the new data.
6-1513	A	34:36		rewrite figure caption sentence to read 'Northern Hemisphere mean (land and marine) surface temperatures have been simulated by a range of climate models ((Figure 6.10d) using the forcings shown in Figures 6.10a-c." [Robert Webb]	Accepted. Text modified.
6-1514	A	34:44	34:46	It is quite worrying that the models diverge in the more recent part. This could be seized on by skeptics. [James Shulmeister]	Noted.
6-1515	A	34:46	34:49	this simulation also assumes a larger solar radiative forcing than any other. [Michael Mann]	Rejected. Figure 6.10 shows not to be true.
6-1516	A	34:48	34:48	of the large disequilibrium" > "of a large disequilibrium [Michel Crucifix]	Accepted. Text modified.
6-1517	A	34:49		In a recent publication of Mann et al. 2005 (Mann, M.E., Rutherford, S., Wahl, E., Ammann, C., Testing the Fidelity of Methods Used in Proxy based Reconstructions of Past Climate, Journal of Climate, in press, 2005) it is stated that "The long-term model drift in the GKSS simulation contributes an unphysical pattern of variance in early	Rejected. Not relevant to point being made.

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				centuries that is likely almost entirely absent from the later 20th century calibration period used by Von Storch et al. 2004. The large changes in solar forcing assumed by von Storch et al. 2004 also occur largely before the 20th century. These arguably unrealistic features in the GKSS simulation make the simulation potentially inappropriate for use in testing climate reconstruction methods." [Heinz Wanner]	
6-1518	A	35:1		Not only the magnitude is in doubt. There are also uncertainties about the relative solar activity changes. For example, the 10Be record from the South Pole (used by (Bard et al., 2000)) indicates lower solar activity during the Spörer Minimum compared to the Maunder minimum. This cannot be confirmed with the 14C record that indicates that the Maunder and Spörer minima are on a comparable level (Muscheler et al., submitted). [Raimund Muscheler]	Noted. Beyond the scope of section.
6-1519	A	35:5	35:7	Can these models reproduce the emergence from the LGM? [Stephen McIntyre]	Noted. Not relevant to present discussion.
6-1520	A	35:12	35:12	"with the empirical evidence" replace by "with the evidence..." (why is this evidence "empirical"?) [Michel Crucifix]	Accepted. Text modified.
6-1521	A	35:20	35:20	replace "inconsistencies" by "uncertainties" or "structural uncertainties" [Michel Crucifix]	Accepted. Text modified.
6-1522	A	35:27	35:30	note that this reasonning assumes (1) that climate sensitivity to solar variations is similar to that to CO2, which is not demonstrated (2) that there is no bias related to that actual observations of temperature on the one hand, and the response to a change in solar forcing on the other hand, will not project the same way on global mean temperature. [Michel Crucifix]	Noted.
6-1523	A	35:31	35:46	The "sensitvity" of coupled carbon-climate models is a very new area subjected to major uncertainties. The relative 'weighting' of C4MIP results versus various other estimates needs better clarification. [Bryant McAvaney]	Not accepted. There are no other model results available to us.
6-1524	A	35:33	35:33	numerically" : replace by "mathematically" or "formally [Michel Crucifix]	Accepted. Replace with ‘formally’
6-1525	A	35:48	38:2	Additional information on Australian hydrologic variations during the Holocene Water level records from closed lakes in south-eastern Australia have fluctuated widely during the Holocene (Bowler, 1981). Modelling shows that the lakes respond only to large scale changes in climate rather than short-term fluctuations, which can be expressed as precipitation/lake evaporation ratios. During the Holocene these ratios changed rapidly a number of times, fluctuating between >1.1 6,000 years ago to <0.8 at the present time, including one unstable period of five large oscillations in about 700 years (Jones et al.,	Rejected. Comments not relevant to this section.

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				1998). Less precise records from elsewhere on the continent generally seem to mirror the driest and wettest phases (e.g. Stanley and De Deckker, 2003). The last such change was about 1840, when conditions reverted to dry conditions not see since the early Holocene (Jones et al., 2001). This latter change appears to be coincident with a warming of the East Australian Current, perhaps signalling a poleward movement of ocean-atmosphere systems (Thresher et al., 2003). Stanley, S. and P De Deckker (2003) A Holocene record of allochthonous, aeolian mineral grains in an Australian alpine lake; implications for the history of climate change in southeastern Australia, Journal of Paleolimnology, 27, 207-219 Jones, R.N., J.M. Bowler and T.A. McMahon (1998) A high resolution Holocene record of P/E ratio from closed lakes in Western Victoria. Palaeoclimates, 3, 51–82. Bowler, J.M., 1981. Australian salt lakes: a paleohydrologic approach, Hydrobiologia, 82, 431–444. Jones, R.N., T.A. McMahon, and J.M. Bowler, J.M. (2001) Modelling historical lake levels and recent climate change at three closed lakes, Western Victoria, Australia (c.1840-1990), Journal of Hydrology, 246, 158-179. Thresher, R., S.R. Rintoul, J.A. Koslow, C. Weidman, J. Adkins and C Proctor (2004) Oceanic evidence of climate change in southern Australia over the last three centuries, Geophysical Research Letters, 31, doi:10.1029/2003GL018869 [Roger Jones]	
6-1526	A	35:48		The various regional variability discussions need to be drawn together somehow otherwise it is a long list without much seeming purpose. [Bryant McAvaney]	Noted.
6-1527	A	35:48		Cullen et al. 2002 should be Cullen et al. 2001: Cullen, H., D'Arrigo, R., Cook, E., and Mann, M.E., 2001: Multiproxy-based reconstructions of the North Atlantic Oscillation over the past three centuries, Paleoceanography, 15, 27-39 [Heinz Wanner]	Accepted. Text modified.
6-1528	A	35:50	36:37	Regarding decadal and multi-decadal variability, this section dealing with ENSO dynamics could include a brief description of the Pacific Decadal Oscillation (PDO) and its role and apparent modulation of ENSO events, particularly ENSO teleconnections, on decadal timescales. [Eva Calvo Costa]	Noted. See chapter 3 for a large discussion on this topic. The extent to which Pacific decadal variability is independent of ENSO is not yet clear.
6-1529	A	35:50	36:37	This section needs to decide if there has been "a relatively consistent history of El Niño in past centuries" or "decadal variability intensified, suggesting that the frequency domain characteristics of ENSO are sensitive to background conditions" and "striking evidence of nonstationarity in ENSO teleconnections, showing a distinct absence of the typical pattern	Take in account. Text modified to clarify.

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				of tropical Pacific warming" I am concerned that some of the multidecadal "evolution of the ENSO's global climate imprint" may be an overly deterministic interpretation of ENSO impacts (see Wolter, K., R.M. Dole, and C.A. Smith, 1999: Short-term climate extremes over the continental U.S. and ENSO. Part I: Seasonal temperatures. J. Climate, 12, 3255-3272 and • Sardeshmukh, P.D., G.P.Compo, and C. Penland, 2000: Changes of probability associated with El Niño. J. Climate, 13, 4268-4286). [Robert Webb]	
6-1530	A	35:50		Delete “What do” and “system tell us?” [Vincent Gray]	Accepted. Text modified
6-1531	A	35:50		This section on ENSO contains valuable information but about different things. A summary assessment statement is called for. [Bryant McAvaney]	Accepted.
6-1532	A	35:53		You might include the publication of Song (1998) using dryness wetness information from China to reconstruct seasonaly resolved SOI back to 1429. (J. Song, 1998: Reconstruction of the Southern Oscillation from dryness/wetness in China for the last 500 years, International Journal of Climatology, Volume 18, Issue 12 , Pages 1345 - 1355). [Heinz Wanner]	Rejected. The SOI signal in China has not been clearly identified and sometimes results controversial.
6-1533	A	36:9	37:2	There is a general tendency for more negative NAO during the 17th and 18th centuries than in the 20th century, thus indicating that the warmth of Europe and Asia in the 20th century might has something to do with the more obvious zonal atmospheric circulation. The low frequency change of PDO, NAO and AO and the implication for attribution of climate change should be more emphasized. [Guoyu REN]	Rejected. Not relevant to this section. No clear relationships between low frequency changes in NAO (AO) and climate in the North Atlantic have emerged yet from the proxy records
6-1534	A	36:17	36:20	This (Mann et al. 2005b) is with a simple ZC model result, and is not supported by AOGCMs. [Akio Kitoh]	Noted.
6-1535	A	36:21	36:22	Is this thermostat effect robust accross different models ? (not sure, for example, in CCSM3.0) [Michel Crucifix]	Noted.
6-1536	A	36:23	36:25	The sentence beginning with "However" is not quite correct, and doesn't reflect the most recent work. The statement appears to be based on the comparison made by Cobb et al (2003) with Crowley (2000)'s *global* radiative forcing. This volcanic forcing series has a significant component due purely to extratropical eruptions. Yet such eruptions do not impose any dust veil forcing over the tropics (or therefore, the tropical Pacific). Only the tropical sub-component of the volcanic forcing is relevant to understanding the forced response of the tropical Pacific ocean-atmosphere. Mann et al (2005b) estimated the actual volcanic radiative forcing acting on the tropical Pacific based on the tropical-only	Noted. Text will be properly edited.

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				component of Crowley's (pers. comm.) chronology--the associated forcing series turns out to be completely different in character from the global tropical radiative forcing. Indeed, the low-frequency changes in both amplitude of variability and mean state indicated by the Cobb et al (2003) estimates were found to correspond remarkably well with the response of the Cane-Zebiak model to *tropical-only* volcanic radiative forcing changes over the past 1000 years (with solar forcing playing a secondary role). [Michael Mann]	
6-1537	A	36:23	36:23	citation should be to "Mann et al (2005b)" not "Mann et al (2005a)". [Michael Mann]	Accepted
6-1538	A	36:28	36:37	The strength of ENSO-drought relationships (wrt the US) also occurs in the instrumental period. This point is made in Ch 3 for all these types of circulation indicators. The links aren't stable across time. [Philip Jones]	Accepted.
6-1539	A	36:28	36:37	Should mention here the apparently changing influence of ENSOs on the Indian monsoon; although it is not in the paleocontext, it emphasizes the point. [Andrew Lacis]	Noted. Cross-reference to check Chapter 3.
6-1540	A	36:28	36:37	Using a long-distance teleconnection (US moisture) to demonstrate non-stationarity of ENSO is rather weak. There is evidence of non-stationarity within the West Pacific Warm Pool described in McGregor and Gagan 2004 a and b (already cited). [James Shulmeister]	Rejected. Macgregor and Gagan (2004) paper's refers to lack of stationary in the frequency of ENSO events, but not in its teleconnections
6-1541	A	36:40	36:48	Is not the problem of really being able to pin down how the NAO has behaved perhaps an indication that it should not be called an "oscillation"--but is rather an indication of, perhaps, some sort of hysteresis or bifurcated state or something. Too often, calling something an oscillation seems to me to be giving the impression that it will just switch aback and forth no matter what, when there is certainly the possibility the circulation could get stuck in one state or the other--or even switch to some alternative state, given various types of forcings and climate states. I would suggest being a lot more qualified in talking about these variations--indicating more that there are tendencies to different modes rather than being so sure that it is called an oscillation with a capital "O" [Michael MacCracken]	Noted.
6-1542	A	36:42	36:43	What the NAO has done in the last couple of decades is not palaeoclimate. This could probably be replaced with a reference to chapter 3. [Jonathan Gregory]	Taken in account. Present NAO behavior is used to infer past relationships with regional climate
6-1543	A	36:49	36:50	Should note here model studies indicating more negative NAO/AO conditions during colder climates (e.g., Rind et al., 2004, J Climate, which also contains the appropriate caveats). This has implications for the future climate (for which most models show a more positive AO/NAO).	Noted.

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				[Andrew Lacis]	
6-1544	A	36:51	36:52	The formulation regarding the winter of 1708/09 is meaningless. Of course, an extremely cold winter in Europe can only occur when the NAO index is negative. To infer this, it is not necessary to cite Luterbacher et al. I suggest to remove the first part of the sentence ("The coldest reconstructed European winter in 1708/1709, and"). [Martin Stendel]	Noted.
6-1545	A	36:52	54:2	I find it perplexing that several recent studies that both precede and are more relevant to the discussion here regarding the evidence for a relationship between radiative forcing and the negative phase of the NAO (and its influence on Europe) are completely ignored. These include: Schmidt, G.A., Shindell, D.T., Miller, R.L., Mann, M.E., Rind, D., General Circulation Modeling of Holocene climate variability, Quaternary Science Reviews, 23, 2167-2181, 2004; Shindell, D.T., Schmidt, G.A., Mann, M.E., Faluvegi, G., Dynamic winter climate response to large tropical volcanic eruptions since 1600, Journal of Geophysical Research, 109, D05104, doi: 10.1029/2003JD004151, 2004; Shindell, D.T., Schmidt, G.A., Miller, R.L., Mann, M.E., Volcanic and Solar Forcing of Climate Change during the Preindustrial Era, Journal of Climate, 16, 4094-4107, 2003; Shindell, D.T., Schmidt, G.A., Mann, M.E., Rind, D., Waple, A., Solar forcing of regional climate change during the Maunder Minimum, Science, 7, 2149-2152, 2001. [Michael Mann]	Noted. References will be added.
6-1546	A	36:55	36:55	Given how variable the NAO can be, it seeming to be sensitive to a lot of even very distant anomalies (like Indian Ocean SST), it seems to me that the text needs to leave open the possibility that regional scale human activities (e.g., changes in land cover, changes in the amount of aerosols) could be affecting the NAO--at least indicate that these variations are unexplained and be very careful in asserting that they are all natural (or particularly that they are all internal as volcanic aerosols could also be influencing them). [Michael MacCracken]	Beyond of the scope of this section.
6-1547	A	36:55	37:2	It is unclear why the study of Reichert et al (2002) [Reichert, B.K., L. Bengtsson, and J. Oerlemans, Recent glacier retreat exceeds internal variability, J. Climate, 15, 3069-3081, 2002] is not cited here. The study predates Luterbacher et al (2002), and provides a stronger physical/theoretical basis, in attributing the changes discussed in European precipitation and glacial mass balance to changes in the NAO. [Michael Mann]	Accepted. Text will be edited to include Reichert et al (2002) work.
6-1548	A	37:1		Consideration of vulnerability and adaptive capacity needs also to distinguish between the probability of extreme/catastrophic events and the chronic impacts of an increase in average water level. In Venice, measures are required for both a flood protection system against extreme high tides as well as routine protection of the city from the degradation	Rejected. Not relevant to this section.

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				(physical, economic and social) caused by tides, waves and saltwater infiltration. This is described in several chapters in Fletcher C. and Spencer T. (2005) (Eds) Flooding and Environmental Challenges for Venice and its Lagoon: State of Knowledge, Cambridge University Press [Pierpaolo Campostrini]	
6-1549	A	37:2	37:2	....Nesje et al., 2000; Nesje and Dahl, 2003). [Atle Nesje]	Accepted.
6-1550	A	37:4	37:10	There are much more publications about Asian precipitation change in Holocene period. As I learned, evidence for the rapid change in monsoon precipitation is relatively weak. The changes in vegetation and sedimentation in mid to late Holocene have not been induced mainly by climate change, and they could not be used to indicate climate change for the past 6000 years for many parts of the old world. [Guoyu REN]	Noted.
6-1551	A	37:4	37:17	Paleaoenvironmental and paleaoclimatic evidences suggest that a predominant temperature drop and an aridification occurred at ca. 2200BC. Paleoclimate studies in China supported these results. The collapse of ancient civilizations at ca. 4.0ka BP in the Nile Valley and the Mesopotamia has been attributed to climate aridification. A widespread alternation of the ancient cultures was also found in China at ca. 4.0ka BP in concert with the collapse of the civilizations in the Old World. Numerical experiment of AGCM with SST forcing in simulating the weakening of the Thermohaline Circulation (THC) indicates a significant reduction of precipitation in East Africa, the Mid East, the Indian Peninsula and Yellow River Valley,supporting the idea that coldness and aridification caused by weakening of the Thermohaline Circulation have greatly contributed to the changes of ancient civilizations at ca. 4.0ka BP. Reference: Wang Shaowu, Tianjun Zhou, Jingning Cai, Jinhong Zhu, Zhihui Xie, and Daoyi Gong, 2004, Abrupt Climate Change around 4 ka BP: Role of the Thermohaline Circulation as Indicated by a GCM Experiment, Advances in Atmospheric Sciences, 21(2), 291-295. [Tianjun ZHOU]	Rejected. Comment not relevant to point being made.
6-1552	A	37:4		Replace with “Asian monsoon variability” [Vincent Gray]	Accepted. Text modified.
6-1553	A	37:5	37:10	This paragraph overlaps with Section 6.4.2 (page 25 line 52 onwards). Can you avoid covering it twice? [Jonathan Gregory]	Accepted. Contents in both paragraphs will be revised and edited.
6-1554	A	37:5	37:10	It seems to me that the potential for land cover change or dust aerosols to be having an effect needs to be allowed for--or at least that not all of the variations may be due solely to internal variability. [Michael MacCracken]	Accepted.

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6-1555	A	37:10	37:10	Remove extra brackets around references. [James Crampton]	Accepted
6-1556	A	37:12	37:17	Or it could be related to the recent warming of the Indian Ocean, perhaps an anthropogenically-influenced occurrence. [Andrew Lacis]	Noted.
6-1557	A	37:17	37:17	Remove "A.K." before "Gupta et al." [James Crampton]	Accepted.
6-1558	A	37:17	37:17	Is this millennial-scale mode of monsoon variability captured in models? [Stephen McIntyre]	
6-1559	A	37:19	37:28	The draft says, "… evidence is not conclusive, particularly given that the relationship between hypothesized solar proxies and variation in total solar irradiance remains unclear." This may certainly be a kind of mystery. But, this mystery can be solved if we consider that solar activity factors other than the luminosity are affecting the temperature. In fact, for example, there are reports on a relation between solar wind and AO (D. R. Palamara and E. A. Bryant (2004) "Geomagnetic activity forcing of the Northern Annular Mode via the stratosphere," Annales Geophysicae 22: 725–731) or NAO (F. Boberg and H. Lundstedt (2002) "Solar Wind Variations Related to Fluctuations of the North Atlantic Oscillation," Geophys. Res. Lett., VOL. 29, NO. 15, 1718, 10.1029/2002GL014903). It is well known that AO (or NAO) governs the climate and temperature of the Northern Hemisphere. And, a recent study on the AO reveals that the AO can be excited by various kinds of external forces (H. L. Tanaka & M. Matsueda, J. Meteorol. Soc. Jpn., 83, 611-619 (2005)). Thus, the solar magnetic activity possibly affects the climate through interacting with AO (or NAO). Such a suggestion seems worth to note. [Kiminori Itoh]	Noted. Comment relevant to the point being made.
6-1560	A	37:19		Replace with" Eastern African hydrological variability" [Vincent Gray]	Accepted. Text modified.
6-1561	A	37:22	37:22	Remove extra brackets around references. [James Crampton]	Accepted.
6-1562	A	37:25	37:27	I think this is ambiguous and disputable. It seeems to suggest that we can expect the prolonged Sahel drought to cntinue, and I cant see how the paleo data can tell us this. [Neville Nicholls]	Accepted.
6-1563	A	37:30	27:56	Very nice section, but seems to find better its place in section 6.4 [Michel Crucifix]	Take in account. Emphasis here is in changes in the past 2000 years.
6-1564	A	37:42	:43	rewrite sentence "Thus, the paleoclimatic record of multi-year, decadal, and even century-scale drier periods is likely to remain a feature of future North American climate, particularly in the area to the west of the Mississippi River.	Accepted. Text modified.

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				[Robert Webb]	
6-1565	A	37:45	37:48	This sentence supports greater droughts and warmer temperatures in the MWP Some statements made earlier in the Chpter tend to question the MWP as globally warm. However, if it was a hemispheric phenomenon, it would be more logical to assume some degree of similarity in the SH, which would then lead to a definite global-scale signal. Also the mention of mega-droughts in the past, does not support statements made elsewhere, particularly in Chpter 3 that droughts are increasing, with the underlying suggestion that this might represent climate change. [Henry Diaz]	Taken in account. Text modified..
6-1566	A	37:45	37:48	How much of an influence does the warmer North American continent, which likely results from the synoptic conditions producing the drought, have on the determination of the NH warmer-than average summer temperatures? While North America is not big, there is a sparcity of data for the hemisphere as a whole, and it may contribute to the concept disproportionately. [Andrew Lacis]	Rejected. Most recent global temperature reconstructions are weight-area estimates.
6-1567	A	37:45	37:48	As discussed by Cook et al (2004b), the pattern of more extensive drought in the western U.S.during the Medieval period is consistent with a La Nina-like state in the tropical Pacific that is indicated by Cobb et al (2003) and predicted by the modeling experiments of Mann et al (2005b). Independent evidence for this conclusion has more recently been provided by Rein et al [Rein, Bert; Lückge, Andreas; Reinhardt, Lutz; Sirocko, Frank; Wolf, Anja; Dullo, Wolf-Christian, El Niño variability off Peru during the last 20,000 years, Paleoceanography, Vol. 20, No. 4, PA4003, 2005] and Castiglia and Fawcett [Castiglia, P.J. and Fawcett, P.J., Large Holocene lakes and climate change in the Chihuahuan, Geology (in press)]. The physical connection therefore is not between large-scale warmth and western U.S. drought but, rather, that both are responding in their own way to changes in radiative forcing (the latter through the influence of tropical radiative forcing changes on ENSO). The previously cited studies of Hoerling and Kumar and Seager et al support the interpretation that such drought changes are closely related to (potentially forced) tropical Pacific SST changes. [Michael Mann]	Noted.
6-1568	A	37:45	37:47	discussion of MWP should be carried forward to MWP discussion [Stephen McIntyre]	Rejected. The MWP Box deals with temperature patterns, whereas this section deals with precipitation changes during the MWP
6-1569	A	37:50	37:56	Again, this assumes that we know what the forcing was. While no obvious climate forcing would have step-functions, there may well be ones we don't know about (galactic dust clouds or internal solar variability - who knows, maybe even cosmic rays). The point is	Noted.

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				that when one doesn't know what physical phenomena forced these specific changes, nor what forcing provoked it, it's inappropriate to make statements about "more gradual forcing". [Andrew Lacis]	
6-1570	A	37:53	37:56	Given the very limited spatial representativeness of the data, it seems to me that great caution needs to be used in drawing a general conclusion--perhaps there was a change in the track of the hurricanes rather than in their frequency--and do we really know that these changes were due to gradual forcing rather than something else. Do we really have long-enough statistics to be sure that these are shifts rather than just statistical fluctuations. I would suggest using some words from the IPCC lexicon to give a better sense of the level of confidence to place in these results. [Michael MacCracken]	Noted. There is no proxy evidences for past changes in hurricane frequencies or intensities
6-1571	A	38:3	39:45	There is a great degree of overlap between many of these paragraphs and Box 6.3 - seems unnecessary. [Andrew Lacis]	Accepted. Section will be incorporated into other existing sections and boxes.
6-1572	A	38:4	39:44	I was unclear about the purpose of the entire section 6.6, which lacked the clear connection to the lessons learned of the rest. The discussion is on mechanisms, rather than an assessment. The section, and thereby the Chapter, seems simply to peter out in the last paragraph.. [Jochem Marotzke]	Accepted. See 6-1571
6-1573	A	38:4		I think the information in this section is somehow repetitive and some parts would serve better the reader if included in other sections (chronologically, as the chapter is designed). For example, the iron hypothesis would fit nicely in Box 6.2: What caused the low atmospheric CO2 concentrations during glacial times?. [Eva Calvo Costa]	Accepted. See 6-1571
6-1574	A	38:6	38:7	Actually, Arrhenius calculated that doubling of CO2 would cause surface warming between 5C (low latitudes) and 6C (high latitudes) (Arrhenius, 1896). [Andrey Ganopolski]	Accepted. Text on Arrhenius dropped
6-1575	A	38:6	38:18	I would suggest to remove this more or less historical amount. Especially, not to refer to Arrhenius. since he got the right number for the wrong resaons (nevertheless he was no doubt a genius) [Michael Schulz]	Accepted. Text on Arrhenius dropped
6-1576	A	38:16	38:18	“globally rather subtle, orbital changes (?) must be amplified by climate feedbacks…” Which “orbital changes” were amplified by climate feedbacks? Do the authors believe that this is changes in globally averaged annual solar insolation of the order of 0.5 W/m2 caused by eccentricity variations? If so, this is very unusual point of view on the Quaternary climate dynamics with which very few workers would agree.	Accepted. ‘globally rather subtle’ deleted

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				[Andrey Ganopolski]	
6-1577	A	38:16	38:18	why does the amplification not run away our of control? [Stephen McIntyre]	No action. There are also negative feedbacks.
6-1578	A	38:17	38:17	"orbital changes must be amplified by climate feedbacks". This sentence does not make sense. Climate feedbacks do not amplify the variations of obliquity, eccentricty. The climate system filters, in a non-linear way, the variations of insolation induced by changes in the orbit. Changes in the biogeochemical cycles are part of this response, and probably contribute to produce the 100-kyr cycle. [Michel Crucifix]	Noted.
6-1579	A	38:20	38:32	Althoug not known as a 'forcing', to be consistent with the previous discussion (and, by the way, why is this discussion occuring here at all?) one should note the possibility of ocean thermohaline circulation changes on inducing climate feedbacks. [Andrew Lacis]	Not accepted. Section is on biogeochemical an biophysical feedbacks. See also 6-1571
6-1580	A	38:20	38:20	Rather than making such a bald assertion, I would suggest modifying the text to read "A variety of evidence indicates that biogeochemical cycles played …" [Michael MacCracken]	Accepted
6-1581	A	38:20	38:23	This is one of the places where I think some confusion might well arise over what is a forcing and what is a feedback--and having the distinction seem to rest on whether the model can run the full cycle or just part of it does not seem very satisfying to me. Thus, changes in vegetation is more and more being called a feedback, yet here is a forcing. Similarly, I think the discussion of the CO2 changes during the glacial cycling are often referred to as a feedback that amplifies the orbital forcing--yet here the GHG changes are a forcing. It might help here to use the word "radiative influences" instead of "radiative forcing", keeping the latter term for external influences. [Michael MacCracken]	Noted
6-1582	A	38:26	38:26	Actually we don't know that the continental ice sheets had high albedo. They existed at low elevations, and may have ground up a lot of dirt (as the Malaspino glacier in Alaska does today) and hence may have been relatively dark (as the Malaspino glacier is). [Andrew Lacis]	Noted.
6-1583	A	38:34	38:35	Box 6.3 says we don't know what caused the glacial to interglacial change in CO2 so it's incorrect to say here that marine biogeochemical cycles are mainly repsonsible for it. [Andrew Lacis]	Not accepted. It is clearly explained in Box 6.2 that the changes in CO2 are related to the ocean.
6-1584	A	38:34	38:34	Again, I would start the sentence saying "Evidence indicates that change in the marine …" to provide support for the conclusion. [Michael MacCracken]	Accepted.
6-1585	A	38:35	38:35	The reference should be to Box 6.2 instead of Box 6.3	Accepted.

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				[Philippe Tulkens]	
6-1586	A	38:35		line contains sentence that starts with a misplaced pronoun. "This suggests" [Robert Webb]	Noted.
6-1587	A	38:37	38:41	Very awkward sentence. [Jochem Marotzke]	Accepted. Sentence modified
6-1588	A	38:39	38:39	Comma required after "(Knuti et al., 2004)"; this sentence clumsy, could be rewritten. [James Crampton]	Accepted.
6-1589	A	38:43	38:43	…have only modest direct effects… [Steven Clemens]	Accepted – 'direct' added, 'only' deleted
6-1590	A	38:47	38:47	Replace "aeolian" with "wind-borne," more readers will understand what you mean. [Lenny Bernstein]	Accepted
6-1591	A	38:47	38:56	This paragraph should also cite the 2004 Science paper by Kohfeld et al, which presented empirical evidence (not model-based) that changes in dust input to the ocean could only, at most, account for the 30-40 ppm reduction in CO2 between stage 3 and stage 2. Given we now have both an empirical and a modelling basis for quantifying the contribution of dust (including evidence for enhanced export production at the LGM), I think it is now possible to make quite a strong statement about this topic. In fact, the situation is well stated in the conclusion of the Kohfeld et al. paper. [Iain Colin Prentice]	Accepted. See also 6-1571
6-1592	A	38:47	38:56	The role of aeolian iron deposition into the oceans in regulating past atmospheric CO2 are evidenced by many researches, and these should be mentioned. We could not find a better explaination for the lower level of atmospheric CO2 concentration in glacial period than this at present. It might be improper to use this argument here to support the claim that fertilization of the ocean with iron to mitigate anthropogenic climate change may not be very effective. Science is science, and you should objectively cite what have been learned by paleo-community. [Guoyu REN]	Text incorporated into box 6.2. Language reflects scientific findings.
6-1593	A	38:56	38:56	I would suggest to cite K.E. Kohfeld, C. Le Quere, S.P. Harrison and R.F. Anderson, Role of Marine Biology in Glacial-Interglacial CO2 Cycles, Science 308, 74-78, 2005. for a limited role of iron-fertilization in glacial-interglacial CO2 changes [Michael Schulz]	Accepted
6-1594	A	39:7	39:7	Should note that it is an orbitally-induced increase in land/ocean contrast. [Andrew Lacis]	Accepted
6-1595	A	39:9	39:9	Full stop required after "…Ducoudre et al., 2000)". [James Crampton]	Accepted
6-1596	A	39:11	39:11	Adams and Faure should not be cited as it is not a reliable source.	Noted.

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				[Iain Colin Prentice]	
6-1597	A	39:14	39:14	Do not forget that the Younger Dryas is primarily defined from paleobotanical evidence. [Michel Crucifix]	Not important in the context of this discusson on rate of change
6-1598	A	39:21	39:30	Levis et al., JGR (1999) and Crucifix, Hewitt, Betts, Glob. Plat. Change, 45 (4) 295-312 (2005) are the appropriate references for simulations of the vegetation with dynamical vegetation models. Jolly and Haxeltine (Science 276 786-788 (1997)) were among the first to document the effect of CO2 with a biome model in the context of paleoclimate reconstructions, but the reference Harrison / Prentice 2003 remains appropriate. Concerning the impacts of vegetation changes on the climate of the LGM, useful references are Kubatzki et al., Clim. Dyn., 1998, Levis et al (1999, but not really a state-of-the-art GCM), Wyputta and Mc Aveney 2001 and the Crucifix and Hewitt, Clim. Dyn., 2005. The latter indeed considers the remote effects, nicely summarised in the section. I would perhaps not mention the impact of vegetation change over Tibet as this is presumably a less robust feature. [Michel Crucifix]	Noted
6-1599	A	39:21	39:31	Now I find mention of vegetation feedbacks at the LGM!! I think this part could be incorporated into 6-14 and then referred to again here. [Julia Hargreaves]	Accepted. See also 6-1571
6-1600	A	39:25	39:25	How does the tropical warming over land during the LGM help better reproduce the observed data, which suggests large cooling in the tropics over land? [Andrew Lacis]	Noted.
6-1601	A	39:25	39:25	I would suggest giving some explanation of the result that "the tropics warm where the tropical forest is replaced by savannah" in that there are (at least) two competing feedbacks--the albedo goes up, which should reflect more solar and make the region cooler, but this is overwhelmed by the drying out and the warming that occurs due to less soil moisture. So, perhaps actually mention what the feedbacks (influences) are. [Michael MacCracken]	Noted.
6-1602	A	39:28	39:30	Word inclusion used twice… [Andrew Lacis]	Accepted
6-1603	A	39:28	39:31	A more local-scale modelling study by Midgely et al., poublished in GCB alongside the Harriosn and Prentice work, also showed a major response of vegetation structure to the glacial-interglacial CO2 shift. [Iain Colin Prentice]	Noted
6-1604	A	39:33	39:36	Rearrange sentence - not clear as written that the LGM C inventory was 300 to 700 GtC lower than pre-industrial (i.e., these are relative, not absolute values). [James Crampton]	Accepted

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6-1605	A	39:33	39:44	Carbon storage at LGM estimated from data is slightly lower than simulated from models (Peng et al., 1998).; Peng, C.H., Guiot, J., Van Campo, E., 1998. Estimating changes in terrestrial vegetation and carbon storage : using palaeoecological data and models. Quaternary Science Reviews, 17, 719-735. [Joel GUIOT]	Noted
6-1606	A	39:35	39:35	Rather than saying "terrestrial biosphere" I would suggest saying "living vegetation" as I am not at all sure that this statement is including, or is meant to include< the soil carbon reservoir. [Michael MacCracken]	Text clarified. Soil carbon is included
6-1607	A	39:35	39:36	I am rather confused here. I thought the amount of carbon in preindustrial vegetation totaled about 700 GtC, so I am confused about how it could have been reduced by 300-700 GtC (unless this is perhaps including some of the below surface carbon--but I wonder if we really have a good inventory of this. [Michael MacCracken]	Text clarified. Soil carbon is included
6-1608	A	39:36	39:39	I think it would be really helpful here to be giving estimated amounts of carbon in order to give meaning to the differences in amounts. [Michael MacCracken]	Accepted
6-1609	A	39:39	39:41	As above - rearrange sentence to make it clear that LGM values were 600 to 1000 GtC lower relative to pre-industrial values. [James Crampton]	Accepted
6-1610	A	39:48	39:50	If the named people in the acknowledgements have assisted in the making of the chapter, should they not be "contributing Authors" and named on pp 1? [Gareth S. Jones]	Policy cleared with TSU
6-1611	A	39:48	39:50	The people listed here should be listed on page 1 in the contributing author's list. Chapter 6 is the only chapter having such aknowledgment section. Is there any reason to list these authors separately ? [Philippe Tulkens]	Policy cleared with TSU
6-1612	A	40:0		The section "References" is sloppy (I know, this is just the first draft). Anyway, I show here some examples which I just noticed: p42,l21: Bonani and not Bonami; p42,l47: Briffa et al in prep ?! should it be quoted? p48,l45 Holzahauser 1998: ETH Züricvh is not a publisher and I can't imagine an ETH at Stuttgart-Jena-New York! p49,l3: Hughes and Diaz1994: a "?" is missing. [Paolo Cherubini]	Noted
6-1613	A	40:0		There are many errors and inaccuracies (including spelling errors) in the reference list. The references have therefore to be checked carefully. [Atle Nesje]	Accepted

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6-1614	A	40:34	40:34	Archer : year is 1998. DOI : 10.1029/98GB00744 [Michel Crucifix]	Accepted
6-1615	A	40:45	40:53	Should be listed as EPICA community members 2004, as indeed it also is. [Eric Wolff]	Accepted
6-1616	A	40:46		REWRITE opening sentence to improve clarity: A key problem for Venice, Italy, is the increasing frequency of floods due to increased relative sea level in the past century of about 23 cm, consisting of about 12 cm of land subsidence, both natural (3 cm) and anthropogenic (9 cm), and 11 cm of sea-leve rise (Carbognin, Teatini, Tosi , Journal of Marine Systems 51, 2004,pp. 345-352). The anthropogenic subsidence was caused by groundwater withdrawals, which began in 1930 and became significant between 1950 and 1970 when it was stopped. The subsidence of the city of Venice is presently limited to about 0,4 mm/yr. The overall rate of sea-level trend between 1896 and 2002 is 2.50 mm/yr and includes all the effects. It must be noticed that the time series of yearly mean sea level presents wide oscillations and a cospicous increase during the last decade, especially considering the values recordered in 2001 and 2002. Significant and consistent evidence of sea level rise over previous centuries is also found by analysis of longer term data sets (Camuffo e Sturaro, 2004). [Pierpaolo Campostrini]	Rejected, does not belong here
6-1617	A	40:56	40:56	The Holocene,..[add also 'The' in other places in the ref. List where referred to this journal.] [Atle Nesje]	Accepted
6-1618	A	42:36	42:36	This should be written as 2003a to match the citation on p. 6-29, line 3. [Henry Diaz]	Accepted
6-1619	A	44:10		6.6 somewhere this section should mention the possibility of raising ground levels in response to s.l.r. over a broad area. In the case of Venice there is a prospect of "reclaiming" a relative difference of about 30cm w.r.t. sea level by means of deep injection of fluids in the subsoil. Comerlati A. et al (2003) Can CO2 help save Venice from the sea? EOS 84 (49) 9 Dec 2003 546, 552-553, American Geophysical Union [Pierpaolo Campostrini]	Rejected, does not belong here
6-1620	A	44:56	44:56	the Crucifix-Hewitt paper has now a full reference: 25 (5) 447-459. [Michel Crucifix]	Accepted
6-1621	A	44:57	44:57	The reference given is incomplete, the issue of Climate Dynamics is not given. [Philippe Tulkens]	Accepted
6-1622	A	45:4	45:4	The reference given is incomplete, the issue (69) of climatic change is not given. [Philippe Tulkens]	Accepted
6-1623	A	46:6	46:7	EPICA Community Members (not the hyphens and capitals)	Accepted

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				[Eric Wolff]	
6-1624	A	48:13		Hays et al. reference is incomplete [Andrey Ganopolski]	Accepted
6-1625	A	48:46		Hoyt and Schatten reference contains typos (the title repeated twice). [Andrey Ganopolski]	Accepted
6-1626	A	48:53		Huang et al. reference. It should be “over the past five centuries”. [Andrey Ganopolski]	Accepted
6-1627	A	51:15	51:17	The full reference is as follows: Kucera, M., Rosell-Mele, A., Schneider, R., Waelbroeck, C. & Weinelt, M. Multiproxy approach for the reconstruction of the glacial ocean surface (MARGO). Quat. Sci. Rev. 24, 813-819 (2005). [Eva Calvo Costa]	Accepted
6-1628	A	51:21	50:21	Kukla : give full author list. [Michel Crucifix]	Accepted
6-1629	A	52:47	52:56	The MacDonald et al., 2000 reference on lines 47-50 is repeated on lines 53-56. [C.F. Michael Lewis]	Accepted
6-1630	A	54:24		Insert Reference; McIntyre, S , and McKitrick, R., 2005 Hockey sticks, principal components and spurious significance. Geophys Research Letters 32 LO3710, doi:10.1029/2004GL021750. [Vincent Gray]	Accepted
6-1631	A	54:39		It is unclear why the title of Milankovitch (1941) monograph is translated into English. This book was published in German. [Andrey Ganopolski]	Noted
6-1632	A	55:13	55:13	Add: Nesje, A. 2005: Briksdalsbreen in western Norway: AD 1900-2004 frontal fluctuations as a combined effect of variations in winter precipitation and summer temperature. The Holocene 15, 1-8. [Atle Nesje]	Noted
6-1633	A	55:22	55:22	Add: Nesje, A., Ø. Lie and S.O. Dahl 2000: Is the North Atlantic Oscillation reflected in Scandinavian glacier mass balance records? Journal of Quaternary Science 15, 587-601. [Atle Nesje]	Noted
6-1634	A	55:53		paleoclimatic ("o" missing) [Eric Wolff]	Noted
6-1635	A	59:44	59:44	Add reference to Stendel et al. (2005a) after line 43: Stendel, M., I.A. Mogensen and J.H. Christensen, 2005a: Influence of various forcings on global climate in historical times using a coupled AOGCM. Clim. Dyn. 25, 10.1007/s00382-005-0041-4. [Martin Stendel]	Noted
6-1636	A	63:0		In the ZOD, the authors of this question suggested they might add a figure illustrating	Accepted

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				Milankovitch cycles, combined wuth a schematic of orbital variations. Is this still a possibility - it seemed a nice idea ? [David & David Wratt & Fahey]	
6-1637	A	63:1		I very much like the "question" sections [Michael Schulz]	Noted with glee
6-1638	A	63:4	63:4	The opening phrase seems much too encompassing, and generally has the effect of giving the misimpression that human induced change is therefore of no consequence. This opening sentence needs to be qualified by indicating that above some modest level, all of the changes appear to be driven by particular factors, some of which operate at some times, some at others--and so what climate history tells us is that if we change some important factor, there will be a response. [Michael MacCracken]	Accepted
6-1639	A	63:4		References are not generally being used in answers to the "Climate Change Science Questions" - the TSU is likely to provide guidance on this. [David & David Wratt & Fahey]	Leaving them in for now, until asked to cut them.
6-1640	A	63:8	63:9	I would suggest replacing the phrase after the comma with "with quantitative model simulations that are driven by reconstructions of identified forcings showing good agreement with observations. [Michael MacCracken]	Rejected. Too clumsy
6-1641	A	63:8	63:9	This statement seems overconfident to me. Which models can reproduce the ice ages with confidence (or indeed at all)? [Eric Wolff]	Noted. Our chapter gives the references to realistic simulations of ice age climate.
6-1642	A	63:11	63:16	It seems to me there are some omissions here, including mention of the locations of continents and ocean passages, and the efficiency of heat transport by the atmosphere and oceans (which can be affected by the shape of ocean basins, the heights and locations of mountains, etc.). It is for this reason that I think the word "local" in line 16 is mistaken--not only local climate depends on how heat is distributed by winds and ocean currents--the global (or at least continental scale) climate can also be so affected. [Michael MacCracken]	Noted. But "local" is here used for "at a given location", as opposed to global mean - this does not rule out continental-scale. But the point is that changes in ocean currents have little effect on the global mean, as stated in the chapter.
6-1643	A	63:12	63:13	...change: (1) changing the distribution of incomming solar raiation... [Steven Clemens]	Rejected. Changes in solar output do not change the distribution, but the amount of incoming solar radiation.
6-1644	A	63:14	63:14	…radiatin that is reflected back to space (this… [Steven Clemens]	Accepted
6-1645	A	63:19	63:30	Good to rapidly re-explain why the 100-kyr pops up in the Earth response to the astronomical forcing. This is a consequence of non-linear components in the climate	Noted, but too complex for the popular style of the questions

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				system, including certainly the isostatic response (there is a paper Crucifix et al. in Earth and Plan. Sci. Letters, 184 (623-633) 2001, but the hypothesis has been discussed since Ghil and Le Treut, JGR 1981, and Tarasov and Peltier, JGR 1997) and biogeochimal cycles (Paillard, Earth and Plan. Sci. Lett. 2004 is certainly to quote for the southern ocean ventilation hypothesis). [Michel Crucifix]	
6-1646	A	63:19	63:30	This is a wild overstatement, and in many ways indicative of the weakness of this chapter as a whole. There are still as many questions concerning the Milankovitch driving of ice ages as there have ever been. Thre is continuing and new evidence that the previous interglacial started prior to the Milankovitch solar peak. Milankovitch variations taken literally (solar insolation at high northern latitudes during summer) cannot explain some of the previous ice age occurrences. Models cannot agree on how to get solar radiation forcing at 115K to force ice sheets to grow (some even using opposing mechanisms). There is still no understanding why the miniscule radiation variation associated with the 100K eccentricity cycle should generate ice ages with that frequency. Statements like the ones given here, to those knowledgeable about the subject, have the effect of invalidating the whole chapter and staining the IPCC report. Were all the caveats and uncertainties to be properly indicated it would, on the contrary, make the report seem more honest and balanced. If the idea here is that the audience cannot be trusted with the truth, then this mirrors the Bush Administration's approach, which basically follows the same line. [Andrew Lacis]	Noted, but there are many statements here that we have to disagree with. E.g., the 100 kyr eccentricity cycle does not produce "miniscule" radiation variations, but a large amplitude modulation of the precession cycle. It is somewhat surprising that a reviewer obviously unfamiliar with basic facts resorts to completely inappropriate polemics, even comparing the authors with the Bush administration. We do clearly state the uncertainty, e.g. in phrases like "There is still some discussion how exactly ice ages are initiated and terminated, but the most likely scenario is..." etc.
6-1647	A	63:20	63:20	…well established that these are initiated (paced) by regular... [Steven Clemens]	see 1648
6-1648	A	63:20	63:20	Change "caused" to "driven" [Michael MacCracken]	Accepted
6-1649	A	63:22		but hardly the global, annual mean" is better written "but with minimal impact on the global annual mean [Tas van Ommen]	Accepted
6-1650	A	63:23	63:23	Change "they" to "the changes in radiation" to be clearer. [Michael MacCracken]	Accepted
6-1651	A	63:23		perhaps better "There is still discussion on exactly how ice ages are intitated and terminated, but… [Tas van Ommen]	Accepted
6-1652	A	63:24	63:24	Refer to Berger, J. Atm. Sci, 1978 and Berger and Loutre, Q.S.R., (10) 297-317, 1991. [Michel Crucifix]	Refs not allowed in questions
6-1653	A	63:24	63:24	What is "this" referring to? Need to clarify.	Accepted

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				[Michael MacCracken]	
6-1654	A	63:26	63:26	Suggest changing "as more and more snow accumulates" to "leading to greater and greater accumulation of snow and ice." [Michael MacCracken]	Rejected - a matter of simple style
6-1655	A	63:26	63:29	The text considerably overstates the confidence one may have in the simulations of ice age inception. I take issue with the term that the models "confirm" the hypothesis, which to me requires that a full interglacial/glacial cycle be simulated with a model based on first principles, to the extent possible. While valuable, the models quoted here fall short of this. Loutre et al. is a reduced-complexity model of only the Northern Hemisphere, Khodri et al. showed that snow cover increases under the right conditions, and the Paillard model is too simple to call it a "hindcast". As this Question appears to be directed at non-experts, it is crucial to be precise. [Jochem Marotzke]	Taken into account. "confirm" changed to "indicate", see 1657. "Hindcast" changed to "reproduced".
6-1656	A	63:26	:27	To accurate represent the findings rewrite "Climate model simulations confirm that an Ice Age can indeed be started in this way ...." with 'Climate model simulations identify a mechanism of increased delivery of snow to high northern latitudes that coupled with vegetation feedbacks can be used to explain how Ice Ages are started ...." [Robert Webb]	Noted - but we also note that the questions section is meant to be simple in style, so the suggested impenetrable jargon is not appropriate here.
6-1657	A	63:27	63:27	Change "confirm' to "indicate" [Michael MacCracken]	Accepted
6-1658	A	63:32	63:32	Change "the Ice Ages" to "ice age cycling." [Michael MacCracken]	Rejected, as a matter of style.
6-1659	A	63:33	63:33	…(Petit et al., 1999)… [Steven Clemens]	no refs allowed
6-1660	A	63:33	63:33	Change "show that" to "indicate that the" [Michael MacCracken]	Rejected - these data are so certain that we can use "show"
6-1661	A	63:34	63:34	…atmospheric CO2 follows the temperature chages with a lag of some hundreds of years (Caillon et al., 2003) but leads changes in ice volume. [Steven Clemens]	Accepted
6-1662	A	63:34	63:34	Change "in the warm" to "during the"--and aren't all interglacials warm, just say "interglacials" [Michael MacCracken]	Accepted. But this is for lay people - we need to tell them that interglacials are warm
6-1663	A	63:35	63:35	Add a phrase to the end of the sentence to the effect "(Caillon et al., 2003), indicating that the changes in CO2 are a feedback rather than the driving force for the change, as is the case with human emissions of CO2." [Michael MacCracken]	Rejected - the following explains its a feedback

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6-1664	A	63:36	63:36	Replace "take" with "occur over" [Michael MacCracken]	Accepted
6-1665	A	63:38	63:39	The sentence, "Model simulations of Ice Age climate yield realistic results only if the role of CO2 is accounted for." should be reconsidered when the result of Calov et al. is taken into account (R. Calov, A. Ganopolski, M. Calussen, V. Petoukhov, R. Greve, Climate Dynamics (2005) 24: 563-576. "Transient simulation of the last glacial inception." Part II: "sensitivity and feedback analysis."). They have succeeded to reproduce the onset of the last glacial based on only the insolation changes and the ice-snow albedo feedback. They point out the importance of decreasing the size of the grid to obtain the reasonable result. In particular, large grid sizes need large contribution of CO2 while small grid makes it minor. [Kiminori Itoh]	Rejected. We are talking here not about inception but about LGM simulations
6-1666	A	63:39	63:39	Question 6.1: Would it be feasible to add one or two sentences here outlining the processes thought to lead to increasing CO2 as a result of increasing temperature ? [David & David Wratt & Fahey]	Noted - but this is a complex problem, not fully understood, so very tough to treat it for lay readers in two sentences
6-1667	A	63:41	63:41	Within the last ice age (MIS 30) over 20 abrupt and dramatic climate shifts known as DO cycles have… [Steven Clemens]	Accepted
6-1668	A	63:42	63:42	The reference to section 6.4.2.1 is incorrect, there is no such section. [Philippe Tulkens]	Accepted, fixed
6-1669	A	63:48	63:50	Unfortunately, the data show that the ocean appeared to cool prior to the ice sheet instabilities. [Andrew Lacis]	Noted, but what is unfortunate about this?
6-1670	A	63:49	63:49	"triggered by …" although this is probably correct for Heinrich events, the situation is less clear for DO events to which this paragr. also refers. Should be stated more precisely [Michael Schulz]	Rejected - the text says "some of these changes" for a reason
6-1671	A	63:53	63:53	…can tell from marks ice leaves on bedrock), [Steven Clemens]	Accepted
6-1672	A	63:53	63:53	The absence of past ice sheets is inferred from more than just the lack of marks on rocks - presence/absence of tills and dropstones, inferred sea level, and geochemical evidence. I would say: "(geologist can tell using various lines of physical and chemical evidence preserved in sedimentary rocks). [James Crampton]	Accepted
6-1673	A	63:55	63:55	Replace "analysis" with "analyses" [Michael MacCracken]	Accepted
6-1674	A	63:56	64:2	The alkalinity balance also matters to understand the long term evolution of CO2. See E.	Noted but too complex for this purpose

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				Sundquist, Quat Sci. Rev. (10) 286-296 (1991) for a review. In summary, CO2 is determined by a chemical equilibrium betwen decarbonation (production of CO2 by volcanoes), weathering and sedimentation. [Michel Crucifix]	
6-1675	A	64:4	64:12	This paragraph must have been written prior to much of the rest of the report, since it represents old thinking about solar radiation variations, rather than the more nuanced presentation elsehwere in this chapter and in the report. [Andrew Lacis]	Noted, but this suggests no specific revision so we can only guess what kind of changes the reviewer had in mind
6-1676	A	64:4	64:7	Given the chapter covers time back 500M years, mention should be made of the slow change in solar output and not just of the sunspot cycles. [Michael MacCracken]	Rejected for space limitations - sorry Mike but we can't cover it all...
6-1677	A	64:10	64:12	On line 11, change "are" to "were" as this is the case in the past--and not likely in the future. A phrase should also be added indicating that the volcanic and solar forcings are going to be much smaller than the human forcings of climate change, so that in the future these factors will not be dominant. [Michael MacCracken]	Accepted the second part
6-1678	A	64:14	64:18	On line 15, change "or" to "and". On line 16, the parenthetical phrase seems pretty strong--it should be qualified with a "likely", and the phrase "until then" seems to imply that this has been happening since time immemorial, instead of since perhaps sometime in the 19th century--but this is all pretty uncertain. And on line 18, the last phrase should be changed to "and neither can a cessation of volcanic activity" so that the sign of the change is consistent. [Michael MacCracken]	Accepted
6-1679	A	64:15		Insert “all” after “explain” [Vincent Gray]	Rejected
6-1680	A	64:16	64:17	Same comment as above [Andrew Lacis]	The phrase was removed, as comment 1678 also took issue with it
6-1681	A	64:17		(Usoskin et al., 2003) is not a good reference in this context since their record peaks around 1970 AD (green curve in their figure 2). This is in contrast to neutron monitor and sunspot data and clearly points to a climatic influence on the 10Be record that they use. A reference to the sunspot or the neutron monitor data would be more appropriate. [Raimund Muscheler]	refs have to go from the questions
6-1682	A	65:0		Question 6.2 - Keep this question in. It is a good summary [Melanie Fitzpatrick]	Thanks
6-1683	A	65:0		Question 6.2: We note there is no figure - although in the ZOD there was a suggestion you might provide a figure showing rates of warming derived from paleo data. Is this still	Currently we do not have such a figure - so probably, no.

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				a possibility ? [David & David Wratt & Fahey]	
6-1684	A	65:4	65:4	The opening sentence to this answer is virtually identical to that for Question 6.1, and suffers from the same flaw. Saying "all" will give the misimpression that human influences are not therefore something unusual or different--it needs to be said that the natural variations are comparatively small unless there is external forcing--and humans are adding an important external forcing. [Michael MacCracken]	Accepted
6-1685	A	65:4	65:4	Replace "Some" with "From the perspective of the geological history of the Earth, some" to give context. [Michael MacCracken]	Accepted
6-1686	A	65:4		Suggest comparing carefully with 9.2 to make consistent and to avoid overlap [David & David Wratt & Fahey]	Accepted
6-1687	A	65:7	65:9	Recast sentence beginning "And faster rates…" - clumsy. [James Crampton]	Accepted
6-1688	A	65:7	67:40	It might be correct that GLOBAL temperatue rised in the past were not more than 0.19 per decade . But REGIONALLY the rates of temperature increse have been much higher at times.For instance for west and central Europe at the transition from Younger Dryas to Holocene, mean annual temperatures rose from c. -2 to c. +8 in about 50 years (= c. 2 per decade). But of course there were completely different conditions in comparison with the present (although not close to the ice sheet).Data in Renssen, H. & Isarin, R. 2001 (The two major warming phases of the last glaciation at ~14.7 and ~11.5 ka cal BP in europe: climate reconstructions and AGCM experiments. Global and Planet. Change 30, 117-153; and Bohncke, S. & Vandenberghe, J. 1991 Palaeohydrological development in the southern Netherlands during the last 15000 years. In 'Temperate Plaeohydrology' (eds. Starkel, L., Gregory, T.J. & Thornes, J.B.), 253-281. [Jef Vandenberghe]	Of course. That's why we specifically wrote: *faster rates of global-mean warming*
6-1689	A	65:8	65:8	It is not clear what the phrase "at least" is doing here--it should likely be deleted. [Michael MacCracken]	Accepted
6-1690	A	65:13	65:13	Poor example - nobody would ever say climate change is defined by the CO2 level. [Andrew Lacis]	Rejected
6-1691	A	65:15	65:17	I would think that IPCC should stick to referring to regional to global scales and not talk about local scales, which can have many more influences than just changes in circulation. And suggesting that sea-ice feedback is a local influence seems to me to be quite confusing--first, it occurs over a regional, and second, what happens over such a large region will have hemispheric consequences (and we include its effect on global temperature change).	we use the term local here not in contrast to regional, but as in "at a given location" rather than for a global mean

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				[Michael MacCracken]	
6-1692	A	65:17	65:19	This statement is just wrong and contradicts the discussion of the chapter. The power of the Milankovitch forcing explanation of ice ages lies just in the fact that hemispherically nearly antisymmetrical forcing causes global temperature change such as ice ages. It is particularly important here, in the "tutorial" part, as it seems to be intended, to be precise. [Jochem Marotzke]	Noted. But this is a "forcing" vs "feedback" confusion, which we well clarify elsewhere in the chapter. Milankovich forcing alone, without changes in the global radiation budget due to ice albedo and CO2, would have very little effect on the global mean temperature - that is our point.
6-1693	A	65:19	65:21	Using continental drift as a factor having an influence over millions of years seems a real stretch--at least say "tens of millions of years" [Michael MacCracken]	Accepted
6-1694	A	65:23	65:30	It looks like here you did not want to quote the submitted EPICA (Siegenthaler et al) paper, which is extensively quoted elsewhere in the chapter. This needs to be made compatibel with the rest of the chapter. [Eric Wolff]	no refs in questions
6-1695	A	65:24	65:24	For clarity, in parenthesis, say "(which covers about …" and start the next sentence with "The time history of the CO2 concentration ... [Michael MacCracken]	Accepted
6-1696	A	65:28	65:28	Change "it" to "the CO2 concentration" [Michael MacCracken]	Rejected
6-1697	A	65:29	65:29	Change "the past Ice Ages" to "past glacial maxima" or "the Last Glacial Maximum" as "Ice Ages" is not really well defined. [Michael MacCracken]	The EPICA record shows several glacial terminations, for which we here use the lay term "end of ice ages"
6-1698	A	65:32	65:32	Should "Temperature" not be "Temperature change" as that is what we really are focusing on? [Michael MacCracken]	Accepted
6-1699	A	65:34	65:34	I would suggest changing "Local" to "Regional" [Michael MacCracken]	Rejected, since we talk about a single paleo record, which by definition measures a local change
6-1700	A	65:39	65:39	is is really the 20th century that is referred to ? [Philippe Tulkens]	absolutely - is the 20th century not the past century??
6-1701	A	65:42	65:42	I would suggest changing this to read "more meaningful for understanding global change is an …" [Michael MacCracken]	Accepted
6-1702	A	65:45	65:45	Actually 2004 is the fourth warmest; and 2005 may well be the warmest.	Accepted

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				[Andrew Lacis]	
6-1703	A	65:47	65:47	I would suggest changing "between those reconstructions" to "between the temperature changes generated by those reconstructions" as we are talking about the results and not the techniques for doing the reconstructions. [Michael MacCracken]	Rejected
6-1704	A	65:50	65:51	Delete from “and has thus” in line 50 to “since then” in line 51 [Vincent Gray]	Accepted
6-1705	A	65:51	65:52	Being driven by solar forcing is not necessarily a positive statement, given the uncertainty concerning what the solar forcing was - this needs to be changed to a caveat ('though driven by solar forcing'). [Andrew Lacis]	Don't understand this comment.
6-1706	A	65:52	65:54	The sentence: "Since proxies indicate …possible amplifying mechanisms" seems reather technical for the expected readers. Can it be written less tersely, without recourse to words like "robust" or "scaling". [David & David Wratt & Fahey]	Accepted
6-1707	A	65:53	65:53	Change "or any" to "and" [Michael MacCracken]	Accepted
6-1708	A	66:1	66:2	At least in Greenland and Antarctica, it was warmer at the peak of the last ig than now. Are you sure about this statement regarding 125 kyr? [Eric Wolff]	Accepted - changed to "clear evidence"
6-1709	A	66:2	66:3	The previous sections have emphasized the Milankovitch forcing does not drive past warm climates, just past warm latitudes in certain seasons. Past warm climates are in the Tertiary and earlier - and models cannot reproduce these very well at all, in particular they cannot produce the extreme high latitude amplification that is implied by the (somewhat uncertain) paleodata. [Andrew Lacis]	Accepted - incriminating sentence deleted
6-1710	A	66:3	66:3	Change "is accounted" to "and other external changes in forcing are accounted" as there are multiple influences to consider. [Michael MacCracken]	sentence gone
6-1711	A	66:6	66:6	Change "longer" to "much longer" and "tectonic activity" to "tectonic activity, continental drift, and other factors" to give a better sense of things. [Michael MacCracken]	Accepted
6-1712	A	66:8	66:15	A similar comment is valid for the transition from the last full glacial (=Pleniglacial) to the Late Glacial at around 14.7 kyrs BP. At that time the mean annual temperature changed from c. -1 to c. +7 in a few hundreds of years (NOT 5000 years!!!). This applies also to the Summary on p. 6-2, lines 31-33. Reference again to Renssen & Isarin 2201	Global mean temperature?

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				(ref. in previous comment). [Jef Vandenberghe]	
6-1713	A	66:8		Replace “0.19K” with “0.02-0.18” C.(see Chapter 3: surface and lower troposphere readings; the figure is wrong) [Vincent Gray]	Changed to 0.15-0.18 (Quote Chapter 3: "from 1979 to 2004 the linear trend is 0.15–0.18 K per decade")
6-1714	A	66:11	66:11	Change "local" to "local to regional" [Michael MacCracken]	see earlier response to same comment
6-1715	A	66:14	66:15	Ocean circulation changes do affect the global mean temperature; they do not affect the temperature of the globe everywhere in the same way, but that doesn't mean they don't have some global average response. [Andrew Lacis]	model simulations suggest the effect on the global mean temperature, even of a full collapse of NADW formation, is minimal
6-1716	A	66:15	66:15	Change "which would hardly affect" to "that altered regional temperatures, but that likely had little effect on" [Michael MacCracken]	Accepted
6-1717	A	66:17	66:24	This reads like crude propaganda in the face of enemies of greenhouse warming. While the point is appropriate, it should be more subtly presented. [Andrew Lacis]	Rejected - polemics with no constructive suggestion for change
6-1718	A	66:24	66:24	Change "of" to "over" [Michael MacCracken]	Accepted
6-1719	A	67:0		Table 6.1. Note on ocean cooling at LGM in North Atlantic: may also need to recall that there is lots of differences between data reconstructions. To some extent, the LGM North Atlantic ocean might be a weak test. In cryosphere changes : specify that the Antarctic cooling is simulated with the right magnitude. Ideally, we should aim at this table containing also more "counter-intuitive" results, such as the winter warming at northern high latitudes during the mid-Holocene in response to the vegetation feedback (Wohlfart et al., Clim. Dyn, 2004), although there are only two models so far in the PMIP 2 database (UBris-HadCM3M2 and FOAM), too few to make a concensus. [Michel Crucifix]	Noted, but space liimitations apply
6-1720	A	67:0		Table 6.1 under "Cryosphere Changes" should mention expansion of sea-ice in the LGM Southern Hemisphere: "Data indicate expansion of perennial and seasonal sea-ice in the circumpolar Southern Ocean, with seasonal sea-ice extending nearly to the modern Polar Front Zone (Gersonde et al., 2005)" [Gersonde et al. already included in references]. For a modelling, use PMIP-2 output and/or Weaver, A. J., M. Eby, A. F. Fanning, and E. C. Wiebe (1998), Simulated influence of carbon dioxide, orbital forcing, and ice sheets on the climate of the Last Glacial	Accepted

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				Maximum, Nature, 394, 847-853. Weaver et al. do capture sea-ice expansion in the LGM So. Ocean. [William Howard]	
6-1721	A	67:0		Table 6.1 on page 67 a PMIP-2 concensus of 0-3 C for LGM tropical ocean cooling is presented while a concensus of 0-3 C warming for the range of future global or tropical ocean temperature is the type of heated debate that the IPCC is attempting to reconcile. [Robert Webb]	noted
6-1722	A	67:4	67:4	Regarding figure 6.8: Remove the gray envelope that indicates uncertainty. Most paleoclimate experts would acknowledge that this measure, popularized by Mann et al, is misleading because it considers only some of the errors, and is more indicative of precision than accuracy. This figure perpetuates this misleading measure, and makes it worse by calculating a vague composite standard error, and then discounts the statistic in the caption ('this is a purely indicative representation'). It is not even that. [David M Anderson]	Accepted, figure changed
6-1723	A	67:4		Replace “Consensus” with “Results” [Vincent Gray]	Noted, to be considered in SOD
6-1724	A	67:6	67:6	In Table 6.1, note that the second column is "region" and not "Local" and the text should also be saying region and not local. More generally, based on the results here, the point should be made that models are as likely to be under as over estimating the response of climate to forcing--and maybe even more likely to be under estimating it. In the fourth column, for ocean cooling in the tropics, I would suggest in line 3 changing "find" to "indicate" and in the last line changing "cannot generally reproduce" to "underestimate" if that is the case--at least say how the result is being missed. [Michael MacCracken]	Noted, to be considered in SOD
6-1725	A	67:6		Replace “Result” with “Section” [Vincent Gray]	Noted, to be considered in SOD
6-1726	A	67:6		Replace “Consensus” with “Results” [Vincent Gray]	Noted, to be considered in SOD
6-1727	A	69:0		Figure #. 6.1Stage 11 and stage 7 should be labeled to go along with the text. Also, there should be some information on how DT was calculated. [Becky Alexander]	accepted
6-1728	A	69:0		Figure 6.1 is a good opportunity for a third question that will focus readers on the fundamental figure that best depicts the potential for significant global warming. Unlike the vast majority of other figures in this and other chapters, the implications of the figure are easily understood by all. In my experience, when I show this figure to the layperson, it creates a "now I get it, we really could be in for it" response. The question...	Noted,

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				Considering figure 6.1, why has modern temperature not increased proportionally with the increase in greenhouse gasses as it did, naturally, over the past 450,000 years? Are modern climate conditions sufficiently different fom those of the past half million years that we don't expect a similar temperature increase? [Steven Clemens]	
6-1729	A	69:0		This figure is too important to lose fidelity in an effort to save space. It is the one figure that engenders a visceral response from those who see it. Use full scales for each record, don't let the red, green, and blue lines overlap! Use a whole page if necessary so that it is perfectly clear that the temperature line isn't just buried behind the CO2 or CH4 lines in the modern. Address the reasons why temperature has not increased proportionally. [Steven Clemens]	noted
6-1730	A	69:0		Figure 6.1 Need to point out that this is not a global mean delT on the left-hand axis but a local/regional value. It is worth explaining either in the caption or in the text that there is a conversion necessary to global mean delT. [Melanie Fitzpatrick]	accepted
6-1731	A	69:0		Figure 6.1. Most readers do not know which is MIS 5, 7, 9 and 11 in Fig. 6.1. Please add information in the figure. [Akio Kitoh]	accepted
6-1732	A	69:0		Figure: I am uncomfortable here with the use of Vostok temperature data for early MIS11. Vostok could not have been reconstructed without seeing the EPICA data, and it will be very dangerous to give the impression that we are happy to turn sections of core upside down at will. My preference would be to cut the temperature record at the end of the original Vostok record. [Eric Wolff]	Accepted, new data will be shown
6-1733	A	69:7		Fig. 6.1 In first sentence of caption, spell out "four," in place of numeral "4." [Melinda Marquis]	accepted
6-1734	A	69:8	69:8	Does the record not go back further than 450 ka (if not in this graph, then in total)--note that the text on page 6-11, line 45 says 650 ka. [Michael MacCracken]	Accepted, new data now published
6-1735	A	69:13		EPICA Community Members (not the hyphens and capitals) [Eric Wolff]	accepted
6-1736	A	70:0		Figure #. 6.2: Bottom plot shows change in SST, why not change in ice sheet elevation (versus absolute elevation)? How well is this known? [Becky Alexander]	Noted, new figure produced
6-1737	A	70:0		Figure 6.2., (Forcings): the origin of the error bar remains unclear. For vegetation and aerosols, there is a risk of future experiments yielding estimates that are outside the error	Taken into account in revision

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				bars. What is the uncertainty source for CO2 forcing (the change in concentration is well known)?. Ice sheet forcing: various PMIP2 experiment give a forcing between 2.44 and 4.04 W/m2. Dust forcing : Claquin et al. provide indeed two possible values (given by the error bar drawn on the graphic) but, clearly, they did not explore the different sources of uncertainty, such as those related to the geometric form of dust (round or flat-shaped) and the dust source (Werner et al. 2002 provide a more systematic exploration of the latter uncertainty, but this was not taken into account by Claquin et al.) [Michel Crucifix]	
6-1738	A	70:0		Figure 6.2 Ice Sheet Elevation and SST scales are too similar in colour - it's difficult to tell them apart, even though one is on land and the other in ocean. [Melanie Fitzpatrick]	accepted
6-1739	A	70:0		Fig. 6.2 Upper left graph Y axis units: W m-2. In fourth sentence of caption, omit "a" from "...denote a best estimate values …" [Melinda Marquis]	accepted
6-1740	A	70:2	70:17	The text does not give any real discussion of the right hand side of the figure. Actually the figure appears to suggest that sea level was lower at 25K than 21K, and the maximum reduction was closer to 135m (using the center of the blue lines). If the figure is left to stand like this without any further explanation, it's hard to see how people owuld not come to that conclusion. [Andrew Lacis]	This Figure has been revised. The issue concerns the fact that the only data that suggest sea level could have been lower prior to LGM are based upon the monastrea annularis species of coral which may live at great depth below the level of the sea. These samples therefor provide only a lower bound on the LGM depression not a usul measurment of it. Since the Barbados record now extends back to the conventional LGM of 21 ka, however, they do rule out the occurence of the large meltwater pulse suggested by Yokoyama et al (2001) to have occurred at 19 ka.
6-1741	A	70:11		Figure 6.2 It is not clear from the caption over what time scale the ice sheet reconstruction is for - is it at the max of the LGM? [Melanie Fitzpatrick]	accepted
6-1742	A	71:0		Fig. 6.3 Add Y axis label to middle graphic (about Antarctic debris). [Melinda Marquis]	Noted
6-1743	A	71:5	71:12	Need to explain numbers on top panel ("17", "14", "12", "8"), and mark D/O and Heinrich events on the actual plots [James Crampton]	Accepted

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6-1744	A	71:9	71:10	The Heinrich events are not shown and strictly are not recorded simply as IRD but as concretised layers (see comments 1 and 4 above) [Mark Siddall]	Noted
6-1745	A	71:9		Heinrich events are not shown on any of these plots [Eric Wolff]	Noted
6-1746	A	72:0		Figure 6.4: The curve for pre-LGM sea-level is drawn from SPECMAP data and most of the accumulated data sets from coral reefs have been ignored (Yokoyama et al., 2001 EPSL, v193 p579; Cutler et al., 2003 EPSL, v206 p253, Potter et al., 2004 EPSL, v225 p191). The coral based sea-level histories were also reproduced from a Physical oceanographic modeling using Red Sea deep sea oxygen isotopes (Siddall et al., 2003 Nature, v423 p853). The working group should use this data as well as the compilation by Lambeck et al (2002 QSR v21 p343) to draw the figure like Fig6.4. [Michel Crucifix]	This Figure has now been redrawn so as to show at large scale the extended Barbados data set and the fit to it by the ICE-5G(VM2) model. Also included, however, as an inset, is the constrained history of ice equivalent eustatic sea level produced in the paper by waelbroecke et al (2002) which includes input from all of the earlier work cited n this comment.
6-1747	A	72:0		Figure 6.4 also has problems for the last deglaciation. The error bars represented by coral living depth in the figure are large and we cannot conclude neither the magnitude of the LGM sea-level nor Mwp1a if we use this data only. I belive most of the researchers in the Paleoceanographic communities who know the nature of the sea-level observation will not accept this curve. As the general knowledge in the community, people should use to draw sea-level curve using only by most reliable sea-level indicators. In this case they should have used Acropora palmata only since it is most reliable sea-level indicator during the deglaciation period in the Atlantic for this purposes. Also citing Shackleton (2000,Science v289 p1897)as "reliable" LGM sea-level data is misleading since the LGM sea-level estimation has uncertainties of 10-20m (Shackleton, per. comm). It is hoped the AR4 WG will modify this curve before the publication. [Michel Crucifix]	This Figure has been redrawn so as to make clear that different error bars are fixed to the different samples that make up the extended barbados data set. Although some species, such as monastrea annularis may live at great depth below the level of the sea they may also grow near sea level and in such circumstances they provide an important constraint upon the maximum amount by which sea level could have been depressed.
6-1748	A	72:0		Fig 6.4 Spell out LGM: Last Glacial Maximum. Spell out RSL: Relative Sea Level. Interpretation of data in graphic, i.e., in last sentence of caption, would probably be better moved to text in chapter. "KBP" differs from more commonly used axis label of Years Before Present. [Melinda Marquis]	accepted
6-1749	A	72:14	72:14	For several decades it has been realised that SPECMAP alone shows the trend of sea level but not absolute values. Both Waelbroeck et al. 2002 and Cutler et al. 2003 gave scalings that can explain the differerences with coral data of the simple scaling shown here. PLEASE show one of these (I favour Waelbroeck et al. in this context since this paper is focused solely on this issue). Both methods show substantial agreement with each other	The Waelbroecke et al curve has been added to the Figure as an inset.

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				and measurments of LGM deep water temperature from pore water estimates and previous estimates of d18O variation with sea level. There is no need whatsoever to resort to the old SPECMAP curve in this context. [Mark Siddall]	
6-1750	A	73:0		Fig. 6.5 Interpretation of data in graphic, i.e., in last sentence of caption, would probably be better moved to text in chapter. [Melinda Marquis]	accepted
6-1751	A	73:1		Figure 6.5. I wonder if this fugure adds much information from what is given in the text. If it is not the case, dropping it could be considered. [Philippe Tulkens]	accepted, figure deleted. Note new orbital box and new figure.
6-1752	A	73:12		December, not January [Eric Wolff]	accepted
6-1753	A	74:0		Figure 6.6: the time axis should preferably be BP not AD, since the time period displayed is Holocene. [Katsumi Matsumoto]	Noted
6-1754	A	75:0		Fig. 6.7 Add units to Y axis, i.e., degrees. [Melinda Marquis]	accepted
6-1755	A	75:0		Figure 6.7: Consider adding data from Sarnthein et al. (M. Sarnthein, S. van Kreveld, H. Erlenkeuser, P. Grootes, M. Kucera, U. Pflaumann and M. Schulz, Centennial-to-millennial-scale periodicities of Holocene climate and sediment injections off the western Barents shelf, 75 N, Boreas 32, 447-461, 2003.) for Barents Sea [Michael Schulz]	noted
6-1756	A	75:2	75:3	The text in yellow is hardly visible. [Hugues Goosse]	Noted, will be fixed
6-1757	A	76:0	76:	See also comment on page 30, lines 53-54):The Oerlemans (2005) Northern Hemisphere temperature reconstruction should be shown--it is far more independent than the other estimates shown, and for this reason of particular significance. [Michael Mann]	Accepted – Oerlaman's curve will be included in Figure and discussed
6-1758	A	76:0		Figure 6.8b: Scaling the T reconstructions to differing instrumental target records, using different seasonal means (annual, summer), periods, and methods – according to the papers mentioned in the legend – is perhaps not the best way of combining these timeseries. These differing calibrations can have rather huge effects on the resulting T amplitude (see Esper et al. 2005; referenced in the report). I suggest scaling all the records in the same way against annual mean temperatures using the maximum period of overlap with instrumental data (likely 1856-1979) to avoid this "scaling bias" on the reconstructed T amplitude. This could be done using the land only data, averaged over the NH or a 20-	Issue noted – but expressing curves as individually, originally published considered optimum, though issue will be noted in revised text

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				90 N latitudinal band. [Jan Esper]	
6-1759	A	76:0		Figure 6.8b: Use a "normal" linear x-axis, rather than some non-linear time scale. The scale, as applied in the draft version, is confusing and misleading. Most importantly, it leaves the visual impression that early temperature changes (e.g. during the first millennium AD) were more rapid than later ones. [Jan Esper]	Accepted
6-1760	A	76:0		Figure 6.8b: The instrumental record shown together with the proxy-based reconstructions is very misleading. Figure 6.8a nicely shows the various temperature records and their differing variance. Just combining these records, as done in Fig. 6.8b, leaves the visual impression that temperatures became less variable towards recent times, not too mention several other methodological problems that arise from this straight-forward averaging. The artificially increased variance back in time (in the instrumental record shown in Fig. 6.8b) is not in line with any of the reconstructions. The most striking example that the instrumental record is misleading, is, that it is clearly outside the confidence range (displayed in gray) during about the 1820s. The instrumental record shown in Figure 6.8b needs to be replaced by a (shorter) instrumental mean series representing NH, or alternatively the 20-90 N latitudinal band (using CRUTEM2v or HadCRUT2v). [Jan Esper]	Rejected – it is considered informative to show some early instrumental data provided sufficient cavecttes inform the reader of these problems
6-1761	A	76:0		Figure 6.8b: Replace the acronym "CED2004" with "ECS2002", since the Esper/Cook/Schweingruber paper that appeared 2002 in Science is the original paper introducing this record. Using the follow-up paper will be rather confusing to the readers, and is not in line with the other acronyms that all utilize the original and not subsequent papers where the records were re-calibrated. [Jan Esper]	Accepted
6-1762	A	76:0		Figure 6.8b: I am not very convinced that the figure should extend back over 2000 years, given the very limited data available for the first millennium AD (and the non-linear time axis is not an appropriate way to deal with this issue as suggested (see comment above)). I suggest to reduced the time scale to 800 AD, or alternatively show the long records alone in an extra figure, if possible. [Jan Esper]	Accepted
6-1763	A	76:0		Figure 6.8b: This last comment (5) is also related to the confidence range, that is narrower during the first millennium in comparison to more recent times, an effect that stems from the way uncertainty is currently calculated in combination with the reduced number of reconstructions back in time. A narrower confidence range during the first millennium AD in no way reflects the reality of our understanding of past climate. [Jan Esper]	Accepted

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6-1764	A	76:0		Figure 6.8 p. 6-76 Plot panel (b) as linear in time. There is no year zero in the Gregorian calendar. The truncation of Buffa et al (2001) (light blue) should be restored. To eliminate clutter in panel (b), remove several of the largely non-independent curves. Remove, as they are now questionable, reconstructions that use the non-conventional de-centered or short-segment-centered principle component calculations (see e.g., McIntyre-McKitrick 2005 GRL). Remove, as they are questionable, reconstructions that overly weight series like the bristlecone pines, which are known to exhibit a 20th century growth spurt unrelated to temperature. This is especially pertinent for those reconstructions that overweight such series by means of the short-segmented PC calculation. [Jeffrey Kueter]	First remarks accepted and noted but changing the curves shown is rejected as considered more impartial to express all available published reconstructions
6-1765	A	76:0		Fig. 6.8 Is there some way to shorten this lengthy caption? [Melinda Marquis]	Will attempt to do so
6-1766	A	76:4		This whole series should be redrawn and displayed using proxy measurements throughout (i.e. Including from 1900) to show the “anthropogenic” influence of weather stations since 1900 [Vincent Gray]	Rejected – point unclear
6-1767	A	77:4		The corrected proxy record of Mann et al by McIntyre and McKitrick (2003) should be added to Figure 6.8b. [Vincent Gray]	Rejected – these authors do not consider it valid
6-1768	A	78:0		Figure 6.9: Even though I am not an expert for SH temperature reconstructions, I am a bit concerned about the two records spanning the past 1000 years (Tasmania and New Zealand). This concern is related to the differing pre-instrumental variance that is much larger for the New Zealand record, and the differing low frequency components around 1000-1100 AD indicating much colder conditions in New Zealand in comparison to the Tasmania record. This latter difference is perhaps not entirely satisfying, given the vicinity of these locations, and might call for the truncation of either one of these reconstructions. Similarly, it would perhaps be useful to re-check the variance difference between these reconstructions. The issue should be discussed with Ed Cook. [Jan Esper]	Noted – the point will be considered and discussed (as suggested) with the names contributing author
6-1769	A	78:0		Fig. 6.9 Clarify Y axis label: (…wrt mean temperature during 1961-1990). [Melinda Marquis]	Accepted
6-1770	A	78:5		There needs to be a Table of North island tree ring results as well as Fig 6.9 [Vincent Gray]	Rejected
6-1771	A	79:0		Note that "Louvain EMIC" is MoBidiC. This is important to avoid confusion with	Noted and accepted

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				ECBILT-CLIO, which is another EMIC from Louvain-la-Neuve. [Michel Crucifix]	
6-1772	A	79:1	80:12	Figure 6.10: Add simulation by Stendel et al. (2005a). [Martin Stendel]	Accepted
6-1773	A	80:1	80:10	A link between model names and the description provided in Chapter 8 (Climate Models and their Evaluation) should be provided. [Hugues Goosse]	Noted and will be considered
6-1774	A	80:1	80:10	A reference should be given for NCAR CSM GCM and ECBilt-CLIO EMIC. I guess that the first one has been published in Jones and Mann 2004 and the second one in Goosse et al. 2005 (both already cited in the report). [Hugues Goosse]	Accepted
6-1775	A	80:5	80:5	Caption of Figure 6.10. "Louvain EMIC" is not a published name for the model. It should be replaced with "MoBidiC EMIC". MoBidiC corresponds to the name of the model given in the publications and given in chapter 8 (figures 8.8.1 and 8.8.2) [Philippe Tulkens]	Accepted
6-1776	A	81:0	81:	Box 6.1. Figure 1. References in bottom panel are not included in reference list. [Eva Calvo Costa]	ACCEPTED (will be added)
6-1777	A	81:0	81:	Bottom panel; Boron isotopes as a paleopH were analysed by Pearson and Palmer (2000), Nature 406, 695-699. Demicco et al. (2003) used their data but adopted different assumptions (probably more accurate) to infer pCO2. Maybe the reference of Pearson and Palmer could be added to the figure. [Eva Calvo Costa]	TAKEN INTO ACCOUNT (will investigate)
6-1778	A	81:5	81:8	I think it is incorrect to be saying "myr" rather than "Myr" and note on line 7 it says "MY"--there needs to be consistency and correctness here. [Michael MacCracken]	ACCEPTED
6-1779	A	82:0		Figure, top part, y-axis, C has become accidentally superscripted. [Eric Wolff]	Noted.
6-1780	A	83:13		Concordia is the station, Dome C is the place, Dome Concordia is incorrect usage. Change to Dome C here. [Eric Wolff]	Noted Figure deleted for space reasons
6-1781	A	84:0	84:	Box 6.3, Figure 1 suggests that temperature in the NH were at least as warm in the mid-Holocene as today. While these glacier advances/retreats may only be indicative of temperatures in Europe, they are also consistent with paleorecords from other regions. So, the statement early on in this chpter that the here an now is the warmest time globally for the Holocene does not appear to be supported--as I also noted above. [Henry Diaz]	Noted. Indeed, the glaciers in most mountain regions were small in the Early-Mid Holocene. However, in the tropics the retreat was due to the lack of precipitation, the temperature reconstructions show a cooling.

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6-1782	A	84:0	84:	Update Swiss glacier line in Box 6.3, Fig. 1 with data from Christian Schlüchter; Ueli Jörin (2004):Alpen ohne Gletscher? Holz- und Torffunde als Klimaindikatoren, in: Die Alpen, 6, p. 34-47, which suggests many more periods where glaciers were shorter than today. [Axel Michaelowa]	accepted, the Alpine curve will be updated and replaced by a composite curve based on Holzhauser et al. (2005), Holzhauser and Zumbühl (2003) Schlüchter; and Jörin, 2004, Hormes et al., 2001
6-1783	A	84:0		Figure #. 1 Box 6.3: Define "today" [Becky Alexander]	accepted
6-1784	A	84:0		Box 6.3, Fig. 1 Clarify X axis label: What is "Cal."? [Melinda Marquis]	accepted
6-1785	A	84:0		Box 6.3, Figure 1: You show the Holzhauser (1998) reconstruction. In the main text (p.6-22,line 38 you refer to Leemann and Niessen (1994). To be consistent with the other records in the figure, you should either replace the Holzhauser (1998) record with the Leemann and Niessen (1994) record in Box 6.3, Figure 1, or refer to Holzhauser (1998) in the text p. 6-23 in the section starting with line 4 and ending with line 17. [Atle Nesje]	Accepted, the alpine reconstruction will be updated
6-1786	A	84:0		Since there not a single curve representing the European Alps, you should also show the Holocene record presented by Holzhauser and Zumbühl (2003) Nacheiszeitlische Gletscherschwankungen. Sonderdruck zum 54. Deutschen Geographentag Bern, aus: Hydrologischer Atlas der Schweiz, 2003. [Atle Nesje]	accepted
6-1787	A	85:0		Box 6.4, Figure 1: I like the figure very much, but believe that another comment needs to be added to the legend. The seemingly increased variance (between the records) back in time can also be related to the decrease in replication (sample depth) that is rather significant with at least some of the reconstructions displayed. If not properly accounted for, changes in sample replication with time will result in time dependent changes in the variance of the individual records which are unrelated to climate. Lower sample replication also generally reduces the signal strength of the reconstructions back in time, an effect that would increase the chance for more random fluctuations, and would at least partly explain the heterogeneous variations during MWP. I personally believe that these biases have a substantial effect on the figure, and that the increased variance back in time should not (solely) be used as a foundation to suggest a more heterogeneous nature of climate during MWP. [Jan Esper]	Comments noted and partially accepted (signal strength does not reduce in proportion to replication – though its expression does) The implication of these curves is considered reasonable despite this.
6-1788	A	85:0		Figure 6.4 p. 6-85 Plot panel (b) as linear in time. There is no year zero in the Gregorian calendar. The truncation of Buffa et al (2001) (light blue) should be restored.	This is a repeat of No. 1764 – see the response to that

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				To eliminate clutter in panel (b), remove several of the largely non-independent curves. Remove, as they are now questionable, reconstructions that use the non-conventional de-centered or short-segment-centered principle component calculations (see e.g., McIntyre-McKitrick 2005 GRL). Remove, as they are questionable, reconstructions that overly weight series like the bristlecone pines, which are known to exhibit a 20th century growth spurt unrelated to temperature. This is especially pertinent for those reconstructions that overweight such series by means of the short-segmented PC calculation. [Jeffrey Kueter]	
6-1789	A	85:1	85:3	In Figure 1 of Box 6.4, the graph of "E Asia" should be refered to "China" because reconstructed temperature graph curve might be quite different from those in Japan and Korea. [Takehiko Mikami]	Noted and accepted