IPCC Working Group I Fourth Assessment Report

Expert and Government Review Comments on the Second-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.

Batch AB (15 June 2006)

No.	Batch	Page:line From	Page:line To	Comment	Notes
6-1	A	0:0	0:0	This chapter exemplifies the issues that the authors must face - there is complexity in the cliamate system that cannot be solely explained by the prevailing humanocentric theory. The inconsistenices deriving from this complexity are the basis of the comments. There is a lot of work to be done. [Lee Gerhard (Reviewer's comment ID #: 83-7)]	Noted
6-2	A	0:0	0:0	A very impressive chapter. Congratulations! [Reto Knutti (Reviewer's comment ID #: 133-50)]	Noted
6-3	A	0:0	0:0	This chapter does not discuss changes in precipitation for which data are available for the last 1000 years. I suggest to discuss this issue in this chaper, for example in section 6.6, which at this point only has a subsection on temperature. A use ful reference would be: Treydte et al., 2006, Nature, p. 1179, doi=10.1038/nature04743 and references therein. [Rolf Müller (Reviewer's comment ID #: 181-36)]	Noted, but data is not available to the extent to make arguments outside of local regions
6-1113	B	0:0	0:0	You should have a clear description of the potential problems with millennial proxy reconstructions: tree rings are well dated but may not be accurate thermometers; reconstructions from nearby sites may differ dramatically and overall results may be undul [Stephen McIntyre (Reviewer's comment ID #: 309-10)]	Noted, issue dealt with with respect to specific comments on this section and the methods chapter
6-1114	B	0:0	0:0	As a matter of prudence, it seems risky to me for IPCC to permit section lead authors to publicize and rely heavily on their own work, especially when the ink is barely dry on the work. In particular, Osborn and Briffa 2006, which is by one of the section lead authors, was published only in February 2006 and is presented in the Second Order Draft without even being presented in the First Order Draft. Nonetheless, it has been relied on to construct the important Box 6.4 Figure 1. This is risky. Osborn and Briffa 2006 uses some very questionable proxies, including the infamous Mann PC1. I have also been unable to verify some of the claimed correlations to gridcell temperature. One of the authors' excuses is that they incorrectly cited the HadCRU2 temperature data set, while they actually used the CRUTEM2 data set and that the some of the HadCRU2 data was spurious. This hardly gives grounds for comfort. The point made in Box 6.4 Figure 1 is also argumentative. If the relative warmth of MWP and modern periods is inessential to any conclusions reached by IPCC, I would urge you to delete this Figure and related commentary. [Stephen McIntyre (Reviewer's comment ID #: 309-11)]	Noted, MWP figure changed. Although much of the claims in the comment concerning the proxies are not share, we have chosen to change the figure somewhat to reduce reliance on a specific paper.
6-1115	B	0:0	0:0	It seems very unwise to me to waive IPCC WG1 policies on publication guidelines, especially for lead authors. For example, Osborn and Briffa 2006 did not meet the December deadline for being published or in print; it was not even mentioned in the First Draft nor was it available from TSU as part of the First Draft process. Other citations in	Rejected, guidlines used for preparing the draft have been followed and new guidlines do not pose problems.

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				the chapter did not meet the December deadline for being published or in press as at the December draft meeting (Osborn and Briffa 2006; Wahl et al 2006; Wahl and Ammann 2006; Hegerl et al "accepted"); several did not meet the February drop-dead date for providing TSU with a preprint (Wahl and Ammann 2006; Hegerl et al "accepted"). The version of Wahl and Ammann 2006 as accepted differeed dramatically from the version provided to TSU for both the First Order and Second Order Drafts, notably in respect to the inclusion of their calculation of MBH verification statistics confirming the results of McIntyre and McKitrick showing failure of MBH verification statistics that had previously been denied.. [Stephen McIntyre (Reviewer's comment ID #: 309-12)]	
16-1116	B	0:0	0:0	The version of Hegerl et al "accepted" has been switched and the proxy reconstruction presented in chapter 6 relies on their submission to J Climate, which had not been accepted as of April 2006, rather than their Nautre article. The articles were switched at the WG1 website between drafts. The Nature article does not provide details mentioned in the Second Order Draft. Non-compliance with WG1 publication deadlines, especially in favor of publications by IPCC lead authors and their associates, is unfair to other authors who might also have sought waivers from published guidelines. [Stephen McIntyre (Reviewer's comment ID #: 309-120)]	Papers cited are within the guidlines for in press papers
6-4	A	0:0		d'Arrigo et al (2006) revisited many high-latitude tree-ring sites in the northern hemisphere, and updated records. Many of their records failed to track the recent instrumental warming. This is the so-called "divergence" problem, and is well-known in the tree-ring community. Many possible explanations exist, including pollution damage recently, an early but time-decreasing CO2-fertilization effect, rising drought stress recently, or nonlinear sensitivity of tree-ring indicators to temperature. (d'Arrigo, in comments to the US NRC panel studying this, noted that "temperature-sensitive" trees are rare and restricted--perhaps with sufficient warming, trees move out of the "temperature-sensitive" band into regions where primary control of growth arises from other factors, with weaker temperature sensitivity.) Notably, with the major exception of the pollution-damage hypothesis, most of the hypotheses for the divergence problem cast doubt on the temperature reconstructions for warm times of the past, allowing the possibility that warming exceeded reconstructed levels and the trees did not capture the full variability. It is clear that the divergence problem is not uniform for all observed tree-ring records in all places, but there is little doubt that a proxy-only reconstruction would not fully capture the instrumentally observed warming of the last two decades of the twentieth century. Omitting discussion of this shortcoming (especially while highlighting shortcomings of other indicators that likely are doing better than the trees--glaciers, for example, are shrinking very rapidly while instrumentally observed temperatures rise), gives a skewed view of the state of the science. I believe that a discussion of the divergence problem is	Noted, text on this problem now inserted in chapter

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				absolutely essential for the chapter. [Richard B. Alley (Reviewer's comment ID #: 4-9)]	
6-5	A	0:0		Despite these comments, the writing team has done an outstanding job with a difficult topic, and should be congratulated. [Richard B. Alley (Reviewer's comment ID #: 4-10)]	Noted
6-6	A	0:0		Overall, this Chapter is well-written and comprehensive. The posing of specific questions at the start of each section is appreciated. The authors do need, however, to check that they answer the questions they pose - even, if only to say that it cannot be answered with current knowledge/data etc. [Govt. of Australia (Reviewer's comment ID #: 2001-300)]	Noted, revised as needed
6-7	A	0:0		Two significant gaps in the relevant palaeo-literature are apparent in a reading of the SOD: First, aside from a good treatment of greenhouse gases and the long record from the EPICA ice core, there is a body of Southern Hemisphere (particularly high-latitude Antarctic) palaeoclimate information that is not represented. Secondly, the issue of abrupt climate change and phasing of hemispheric response lacks generally, a clear definition of what is meant by the phasing issue and specifically, mention of connection between NH Dansgaard Oeschger events and Antarctic counterparts, including a key paper on phasing (Morgan et al, Science, 297:1862-1864, 2002). The additional SH palaeoclimate information is mostly non-temperature related and so is recommended for inclusion in Section 6.6.5, p 6-38 and following. The treatment of abrupt climate change and Antarctic phasing is difficult as it relates to a number of comments spread across the chapter, concentrated on page 6-11 and pages 6-18-6-19. It could involve a box to draw together both the oceanic and ice-core evidence in one place, or it could be treated as an addition around page 6-11, line 28-32 as suggested below for simplicity. [Govt. of Australia (Reviewer's comment ID #: 2001-301)]	Rejected, the chapter is not a text book on paleoclimatology, and with the length limitations a more comprehensive treatment of the phase issues and evidence of abrupt climate change is not possible.
6-8	A	0:0		The authors have missed some useful references for proxy climate information from corals for windows of the more distant past, ie not just the past few centuries (eg Felis et al, 2004, Nature: 429: 164-168; Gagan et al (2004), Quaternay International 118-119: 127-143; McGregor & Gagan, 2004, Geophys Res Lett [Govt. of Australia (Reviewer's comment ID #: 2001-302)]	Noted, considered in revision
6-9	A	0:0		Although I recognize the space constraints, I can't help but think that a figure showing the geologic time scale, and the boundaries, would be helpful. For a general science reader, this would help place much of the material within the chapter in context. For the expert reader, there are references to epoch boundaries throughout the document, and given that	Rejected, see comment 6-7 concerning length issues

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				boundaries change and are redefined (e.g. PETM from LPTM), it may be useful to include a specific definition of the geologic time scale used. Just a simple suggestion... [KB Averyt (Reviewer's comment ID #: 8-3)]	
6-10	A	0:0		Chapter 6 is almost entirely devoted to late Quaternary paleoclimate, with just 2 pages on the pre-Quaternary (6-9, 6-10, Section 3.1). Even those 2 pages are unsatisfactory, because while two of the three sections, on the mid Pliocene warming and the 55 Ma methane discharge are OK as they stand, these are unusual events in the climate history of the last 100 million years. Section 6.3.1 on pre-Quaternary CO2 through Cenozoic times is not OK for several reasons. For the chapter to pass review in an international journal the pre-Quaternary section would need a review of CO2 history that provided authoritative analysis , and an overview that puts the two selected paleoclimate events in the context of a high CO2-high temperature Cretaceous-early Cenozoic climate shifting to the low-CO2 low-temperature state of the last ~30 million years. [Peter Barrett (Reviewer's comment ID #: 12-1)]	Rejected, the authors believe balance is righ, given the strong length limitations. This the chapter focusses on those aspects of pre-Quaternary which are seen as most relevant for the role of the document for policy makers
6-11	A	0:0		1. Chapter 6 focuses on the earth's climate primarily in the recent past - the last 2 million years. It does well in addressing variability during the last two millennia, the Holocene (the last ~10,000 years) and the late part of the Quaternary period (the last 2,600,000 years). For the last ~800,000 years ice cores show that the earth's climate has been characterized by 100,000-year oscillations of temperature and CO2 gas concentration within the narrow range of 5 deg C and 100 ppmv. Sea level varied in step through ~120 m as a consequence of bi-polar ice sheets growing and shrinking. The record can be extended back in time through the deep-sea oxygen isotope record, which shows early Quaternary and older oscillations to have had a 40,000 year frequency with about 1/3 of the amplitude in temperature and sea level change. [Peter Barrett (Reviewer's comment ID #: 12-2)]	Noted, length restrictions do not allow a more detauiled treatment
6-12	A	0:0		2. However this chapter treats the period prior to the ice core record of the last ~800,000 years in just 2 of the 43 pages of text. It is argued in the introduction of the current text that "most space is provided for recent paleoclimatic history because uncertainties become smaller towards the present.", and I accept that data from the distant past are of much lower quality and far more difficult to confidently place in context (apart from growth features like tree rings, corals and varves). However, these data are more than adequate to show us in some detail that climate was profoundly different in earlier times, and that reviewing paleoclimates beyond the last million years should qualify for serious analysis, and maybe even equal space, because we will be living with those CO2 levels in a little more than a decade. [Peter Barrett (Reviewer's comment ID #: 12-3)]	Noted, length restrictions do not allow a more detailed treatment
6-13	A	0:0		3. Indeed by 2015 we will be experiencing CO2 levels of over 400 ppmv, which the earth	Noted, length restrictions do not allow

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				last experienced over 25 million years ago according to estimates of atmospheric CO2 from 3 different marine geochemical proxies covering the last 65 million years (Pearson & Palmer 2000; Demicco et al., 2003; Pagani et al., 2005). Section 6.3.1 reviews the relationship between CO2 and temperature in pre-Quaternary time rather poorly, providing no analysis of the validity of the various proxies. Furthermore Figure 6.1c that supports this section has been changed since the first draft to include CO2 estimates from pedogenic carbonate over the last 30 million years that are as yet unpublished and are also very different from all other records for this interval. The differences are not addressed or resolved in the text. This section makes also little attempt to evaluate the causes of CO2 change through time, omitting for example a seminal review on the topic by Hay et al. (2002). It also obscures the key point made by Crowley and Berner (2001) that the first-order agreement between the CO2 record and continental glaciation continues to support the conclusion that CO2 has played an important role in long-term climate change. [Peter Barrett (Reviewer's comment ID #: 12-4)]	a more detailed treatment
6-14	A	0:0		4. To find out what the earth will be like next century, when CO2 levels have more than doubled it would be useful to review the high CO2 high temperature world of the Cretaceous and early Cenozoic (130 to 34 million years ago), perhaps starting with Barrera and Johnson's compilation on "Evolution of the Cretaceous ocean-climate system" (1999), and Huber et al.'s Warm Climates in Earth History (1999). [Peter Barrett (Reviewer's comment ID #: 12-5)]	Noted, length restrictions do not allow a more detailed treatment
6-15	A	0:0		5. Some will say that the geography of the time was significantly different, but plate movements can be back-tracked over the last 150 million years to recreate continent-ocean geometry as accurate as the best current GCMs, as I mentioned in my response to the first draft of this chapter. Indeed, the petroleum industry funds research into recreating past geography and climate back to 400 million years in the search for more oil. [Peter Barrett (Reviewer's comment ID #: 12-6)]	Noted, length restrictions do not allow a more detailed treatment
6-16	A	0:0		6. From the viewpoint of life on earth Prothero (1992) has argued that the most profound climate change since the Cretaceous has been the shift from "greenhouse" to "icehouse" 34 million years ago, an event comprehensively documented in Prothero et al. (eds.) (2002). This has been more significant than the 5 degC temperate rise from the methane discharge 55 million years ago, which perturbed the climate system for 100,000 years before returning to its previous warm high CO2 state. [Peter Barrett (Reviewer's comment ID #: 12-7)]	Noted, we have focused on the 55 ma event because of the supposed GHG forcing. The chapter does not have room for a general overview of the earth´s climate history
6-17	A	0:0		7. From a geological perspective then, this chapter would be greatly enhanced if it looked beyond its assessment of past behaviour of the present climate system over the last 800,000 years, which is likely to be with us for only another decade or two, and sought	Noted, The chapter does not have room for a general overview of the earth´s climate history

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				insight on what is likely to happen beyond. [Peter Barrett (Reviewer's comment ID #: 12-8)]	
6-18	A	0:0		8. The short section on pre-Quaternary climate is of concern for what it does not consider, giving the impression that paleoclimate has no more to offer than two brief paragraphs, one on what can be gained from the mild mid-Pliocene warming and other on an explosive methane discharge 55 million years ago, an event that has some similarities with the meteorite impact 65 million years ago. The methane discharge is estimated to have injected as much carbon as will the burning of all remaining fossil fuels over the next two centuries, and hence is a useful warning. However the section provides no awareness of the profound change that current IPCC projections indicate from icehouse to greenhouse in the next century, and that seems to me a weakness that needs to be remedied. [Peter Barrett (Reviewer's comment ID #: 12-9)]	Rejected, the authors sympathise with the view but those aspects of paleoclimaology most relevant for policy making have been chosen due to space limitations
6-19	A	0:0		REFERENCES ** Barrera, E., Johnson, C.C. Evolution of the Cretaceous ocean-climate system. Geological Society of America Special Paper 332. ** Crowley, T.J., Berner, R.A. 2001. CO2 and Climate Change. Science, 292, 870 - 872. ** Demicco, R.V., Lowenstein, T.K., Hardie, L.A. 2003. Atmospheric pCO2 since 60 Ma from records of seawater pH, calcium, and primary carbonate mineralogy. Geology, 31, 793-796. ** W.W., Soeding, E., DeConto, R.M, Wold, C.N. 2002. The Late Cenozoic uplift - climate change paradox. International Journal of Earth Science, 91, 746-774. ** Huber, B.T., Macleod, K.G, Wing, S.L. 1999. Warm Climates in Earth History. Cambridge University Press, 1999, 480 p. ** Nairn, A.E.M (ed.), 1961. Descriptive Climatology, Interscience Publishers Inc., New York, London. 382 pp (see: http://www.questia.com/PM.qst?a=o&d=100023339) ** Pagani, M., Zachos, J.C., Freeman, K.H., Tipple, B., Bohaty, S., 2005. Marked Decline in Atmospheric Carbon Dioxide Concentrations during the Paleogene. Science, 309, 600-603. ** Pearson, P., Palmer M.R., 2000. Atmospheric carbon dioxide concentrations over the past 60 million years Nature, 406, 695-99 ** Prothero, D.R., Ivany, L.C., Nesbitt, E.A. 1999. From Greenhouse to Icehouse The Marine Eocene-Oligocene Transition. Columbia University Press, NY. ** Prothero, D.R., Berggren, W.A., 1992. Eocene-Oligocene climatic and biotic evolution: Princeton University Press, Princeton, NJ, 588 p. [Peter Barrett (Reviewer's comment ID #: 12-10)]	See comments above

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6-20	A	0:0		Suggesstion for an new entry in the 6.A Glossary: SOLAR IRRADIANCE: Energy flux of shortwave solar radiation in the ultraviolett and the near infrared expressed in Watt per square meter. [Eva Bauer (Reviewer's comment ID #: 15-14)]	Rejected, belongs in general glossary
6-21	A	0:0		Ok [Tiziano Colombo (Reviewer's comment ID #: 46-14)]	Noted, no basis given
6-22	A	0:0		Throughout - capitalize Northern and Southern Hemisphere [James Crampton (Reviewer's comment ID #: 50-1)]	Accepted
6-23	A	0:0		Throughout - "mid-" should always be followed by a hyphen [James Crampton (Reviewer's comment ID #: 50-2)]	Accepted
6-24	A	0:0		We've been asked by Etheridge to replace references to MacFarling Meure, 2004 (phD thesis) with MacFarling Meure, C., Etheridge, D. M, Trudinger, C. M., Steele, L. P., Langenfelds, R. L., van Ommen, T. D., Smith, A. M. and Elkins, J. W. The Law Dome CO2, CH4 and N2O Ice Core Records Extended to 2000 years BP. Geophysical Research Letters, in press. AS you also reference this work several times, we should probably cite the same thing in both chapters. I'll try and get a pdf off them.. [Piers Forster (Reviewer's comment ID #: 73-38)]	Accepted
6-25	A	0:0		Recent results on faunal and floral behavior are missing. See e.g. :Morin X. et Chuine I (2005) Sensitivity analysis of the tree distribution model PHENOFIT to climatic input characteristics: implications for climate impact assessment. Global Change Biology. 11(9): 1493-1503. Chuine I, Yiou P., Viovy N., Seguin B., Daux V., et Le Roy Ladurie E. (2004) Grape ripening as an indicator of past climate. Nature, 432: 289-290. Osborne C., Chuine I., Viner D., Woodward F.I. (2000) Olive phenology as a sensitive indiactor of future climatic warming in the Mediterranean. Plant, Cell & Environment, 23: 701-710 [Govt. of France (Reviewer's comment ID #: 2010-47)]	Rejected, will add too much to length and chapter needs shortening
6-26	A	0:0		Despite discussing climate sensitivity in several sections, the chapter do not give an assessment of the range or a "best guess" for a global mean of climate sensitivity. As paleoclimate can contribute a lot to such an assessment, many paleoclimatologists attended the IPCC workshop on Climate Sensitivity (Paris, July 2004). The proceedings of this workshop states on page 31: "Past climates offer some guidance to climate sensitivity. Estimates generally fall in a range of equilibrium temperature change of 2–4 K (for 2xCO2)." Therefore we strongly feel that there already is an agreement on as well as an evidence of the climate sensitivity from paleoclimate. Please give a conclusion on the global average of climate sensitivity both in the chapter and in the executive summary	Climate sensitivity, also paleo aspects are treated in Chapter 9

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				(fits best on page 5, line 9) which should be in line with the proceedings of the mentioned workshop. We propose to use the two sentences given above. [Govt. of Germany (Reviewer's comment ID #: 2011-13)]	
6-27	A	0:0		Some of the findings of paleoclimate are highly relevant especially for the attribution of causes of climate change. But the figures illustrating the information are often inappropriate to communicate the robust findings easily (examples are Figures 6.9 and 6.10c). Please put an effort in making the figure message clearer and asily to understand, so that it will be possible to use these figures in the SPM. [Govt. of Germany (Reviewer's comment ID #: 2011-14)]	Noted, figures will be revised
6-28	A	0:0		I know that authors have received plenty of comments and that it is difficult to take all into account. Nevertheless I am frustrated to see that 90% of my comments on FOD were considered as irrelevant. I will then limit my comments on the SOD to a few generalities [Joel GUIOT (Reviewer's comment ID #: 92-1)]	Noted, comments are treated and responded to on an equal basis, noting the severe limitations on length
6-29	A	0:0		Overall this is an excellent chapter. [Danny Harvey (Reviewer's comment ID #: 101-42)]	Accepted
6-30	A	0:0		This chapter has undergone a really great improvement. The ice core data is now involved in the text and the text is pedagogic written. The introducing part with statements is a good approach. In the previous draft the EPICA data was hardly not mentioned but now they are showing up on relevant places in the text. There are though some remains of the old text which needs some more work. [Per Holmund (Reviewer's comment ID #: 108-4)]	Taken into account, with specific comments
6-31	A	0:0		It is hard to see how the chapter could be shortened in any meaningful way - each of the topics is fairly succinctly described and cutting some topics would reduce the overall impact of the chapter. New scientific results are presented and discussed repectively to the TAR, and a credible job is done of summarizing relevant recent work in each of the topics, with enough material on earlier work to in most cases put the newer studies in context without going beyond the specific scope WG1 mandate. I just have a few very minor comments that I listed above. The only quibble about the chapter concerns section 6.6 that might be too wordy with too much details (for expample, too much details are given about each study concerning the data and methods for Northern Hemisphere temperature) but overall the paper does an outstanding job at its stated objectives. [Myriam Khodri (Reviewer's comment ID #: 126-6)]	Noted, see specific comments to Ch 6.6
6-32	A	0:0		Maybe it should be checked for coherency when giving the range of atmospheric CO2 concentration over glacial-interglacial cycle. [page 12 line 54 : 180-300; page 68 line 32 : ~190 - ~280; page 70-line 27: 190-290] [Marie-France Loutre (Reviewer's comment ID #: 148-10)]	Accepted
6-33	A	0:0		Concerning the summary of the McIntyre and McKitrick work, while some of the	Taken into account, the section is

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				problems in the FOD have been corrected, the SOD text still misses the mark. The main points of the debate are ignored or inaccurately summarized and the text appears to place undue reliance on the new Amman and Wahl paper, while skipping much of the discussion that has taken place in the bulk of the literature. This is an important section of the AR4 and will be closely scrutinized, so please take the necessary time to sort it out. I will be cc'ing copies of this section of my comments to some of the responsible officials in the US and Canadian governments so that they are aware of the criticisms that have been lodged of the SOD version when they examine the subsequent draft. [Ross McKitrick (Reviewer's comment ID #: 174-26)]	revised, and rewritten. The Chapter has received almost orthogonal opinions about this section, and has tried to strike a balance.
6-34	A	0:0		The References should be checked. I found a reference in the list that was removed from the text (Usoskin et al., PRL). [Raimund Muscheler (Reviewer's comment ID #: 185-1)]	Accepted
6-35	A	0:0		Once again, I would like to congratulate the authors for their great job. From numeros data sources they have put togehter an excelletn text that nicely illustrates the role of paleoclimate in global-change research. [Michael Schulz (Reviewer's comment ID #: 229-1)]	Accepted
6-36	A	0:0		This chapter differs from earlier ones in that it brings in modelling and attribution as well as paleoclimatological measurements. The arguments for doing it are reasonable, and it makes for a complete, readable chapter, but it does make for a rather uneven report, since we are told here how models (whose fundamental nature is not described until a later chapter) reproduce the more distant past, but we are not told in earlier chapters how well models reproduce the better-observed past 50 or 100 years. [Adrian Simmons (Reviewer's comment ID #: 242-95)]	Noted, the problem is difficult to solve entirely, and a balance need to be obtained
6-37	A	0:0		The general style of the introductory sections of this chapter is rather different than that of previous chapters - with more general introduction to the subject and justification for it, and generalities such as "paleoclimatologists always strive to generate ..." (Page 6-17, line 17). There's no problem with the style per se, but again it makes for a rather uneven report. [Adrian Simmons (Reviewer's comment ID #: 242-96)]	Taken into account, and some revisions are made
6-38	A	0:0		I think that the choice of the authors should have been done while trying to encompass a wider range of scientits in the field, specially in paleoceanography. It strikes me as rather odd that 1 of the lead authors and 3 of the contributing ones belong to the same research group in Gif sur Yvette!, and two more are from Bergen. If the IPCC is to represent the view of the scientific community much more care should be given to represent the views of the whole community. [Govt. of Spain (Reviewer's comment ID #: 2019-105)]	Rejected, the choice of contributing authors has been made based on where the LAs have needed help and where the expertise was readily available. The contents of the report is the important aspect
6-39	A	0:0		There is excessive use of expressions in the text denoting lack of certainty in the research	Taken into account

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				findings discussedd. An example is the following comment. [Govt. of Spain (Reviewer's comment ID #: 2019-106)]	
6-40	A	0:0		The definition of the Quaternary is not consistent with that in chapter 1 and with the latest recommendiations from the Internationl Commission on Stratigraphy and INQUA. The onset of the Quaternary is 2.6 Ma. [Govt. of Spain (Reviewer's comment ID #: 2019-110)]	Accepted
6-41	A	0:0		THE USE OF THE ABBREVIATION LGM FOR LAST GLACIAL MAXIMUM IS NOT CONSISTENT THROUGHOUT THE TEXT [Govt. of Spain (Reviewer's comment ID #: 2019-130)]	Accepted
6-42	A	0:0		I have four chief concerns with this chapter. First, there are numerous important references left out, and an over-emphasis on papers by the authors themselves, which do not accurately reflect the communities' view. In general, the certainty with which this chapter presents our understanding of abrupt climate change is overstated. There is confusion between hypothesis and evidence throughout the chapter, and a great deal of confusion on the difference between an abrupt "climate change" and possible, hypothetical cuases of such climate changes (e.g. Heinrich events). Second, the use of the terms "very likely", "likely", etc. are not in conformance with the rest of the IPCC document -- some things that are virtually certain are listed as "likely" and mere hypotheses, largely untested, are listed as "very likely". This carelessness does not add credibility to this chapter. Third, extensive reference is made to a very few recent papers that have not yet been thoroughly considered by the scientific community, and whose relevance to future climate is, in my judgement, greatly overstated. Finally, the choice of words to define -- or not define -- in the Glossary is strange. A definition (and a very poor one) of Heinrich events is given, but there is no definition for "Holocene", even though that term is used throughout the text. I would additionally note that overall, the chapter does a fine job at dealing with the "Hockey Stick" controversy, but a very poor job dealing with abrupt climate change and its possible relevance to the future. There are numerous glaring omissions of citations -- notably no mention is made of the work by Wunsch, Seager and Battisti, challenging the standard "Broecker-type" hypothesis for abrupt climate change. [Eric Steig (Reviewer's comment ID #: 252-1)]	Taken into account for revisions of the abrupt climate change part.
6-43	A	0:0		Chapter 6 on paleoclimate includes most important aspects of past climate change and the most relevant to discuss for an IPCC assessment. It is well written and easy to follow however sometimes rather short discussion and referencing. Some aspects of proxy records used are missing. [Govt. of Sweden (Reviewer's comment ID #: 2020-10)]	Noted, but chapter has strong page limitations
6-44	A	0:0		Several key researches that addressed the relationship between tree ring width and tropical	Noted and will be considered in

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				climate are absent from the report. Tree ring width is considered a significant indicator. Therefore, these relevant studies could be further incorporated into the report: Pumijumnong, N., Eckstein, D., Sass U., 1995. Tree-ring research on Tectona grandis L. in northern Thailand. IAWA J. 16, 385-392. Pumijumnong, N., Eckstein, D., Sass, U., 1995. Reconstruction of rainfall in northern Thailand from tree-ring series of teak. IGBP-PAGES/PEP II Symposium on Palaeoclimate and Environmental Variability during the past 2000 Years in Austral - Asian Transect, Nov. 28 - Dec. 1, 1995, at Nagoya University. Nagoya/Japan, 1995, 186-191. Pumijumnong N and Wanyaphet T., 2006. Seasonal cambial activity and tree-ring formation of Pinus merkusii and Pinus kesiya in Northern Thailand in dependence on climate. Forest Ecology and Management 226: 279-289. Yadav,R.R., Park, W-K and Bhattacharyya, A., 1997. Dendroclimatic reconstruction of April-May temperature fluctuations in the western Himalaya of India since A.D. 1698. Quaternary research 48, 187-191. Worbes, M. Staschel, R., Roloff, A., Junk, W.J., 2003. Tree ring analysis reveals age structure, dynamics and wood production of a natural forest stand in Cameroon. Forest Ecology and Management 173, 105-123. Stahle, D.W., Mushove, P.T., Cleveland, M.K., Roig, F. and Haynes, G.A., 1999. Management implications of annual growth rings in Pterocarpus angolensis from Zimbabwe. Forest Ecology and Management 124, 217-229. [Govt. of Thailand (Reviewer's comment ID #: 2021-3)]	revision, although chapter has strong length constraints
6-45	A	0:0		The proxy data used to study paleoclimate are quite limited in different study sites. These proxy data can be collected from tree ring, pollen, ice core, coral and stalagmite. The IPCC report does not mention any relevant researches about stalagmite. It is recommended that some of the following studies be included: Baldini, J.U.L., McDermott, F. and Fairchild, I.J. (2002). Structure of the 8200-Year Cold Event Revealed by a Speleothem Trace Element Record. Science, Vol. 296: 2203-2206. (www.sciencemag.com) Betancourt, J.L., Grissino-Mayer, H., Salzer, M.W. and Swetnam T.W. (2002). A test of "Annual Resolution" in Stalagmites Using Tree Rings. Quaternary Research 58, 197-199. Tan, M. and Liu, T. (2003). Cyclic rapid warming on centennial-scale revealed by a 2650-year stalagmite record of warm season temperature. Geophysical Research Letters, Vol. 30 No. 12: 1617-1921. Wang, Y., Cheng, H., Edwards, R.L., He, Y., Kong, X., An, Z., Wu, J., Kelly, M.J., Dykoski, C.A. and Li, X. (2005). The Holocene Asia Monsoon: Links to Solar Chnages and North Atlantic Climate. Science. Vol. 308: 854-857. (www.sciengemag.org.).	Rejected, not enough room due to lengtyh limitations

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				Yuan, D., Cheng, H., Edwards, R.L., Dykoski, C.A., Kelly, M.J., Zhang, M., Qing, J., Lin, Y., Wang, Y., Wu, J., Dorale, J.A., An, Z. and Cai, Yanjun. (2004). Timing, Duration, and Transitions of the Last Interglacial Asian Monsoon. Science. Vol. 304: 575-578. (www.sciencemag.org.). [Govt. of Thailand (Reviewer's comment ID #: 2021-4)]	
6-46	A	0:0		In the case of Chapter 6, the Executive Summary is in the format of five questions. For the Chapter 8 question, there is also a summary paragraph at the end of the reply. In the case of several of the boxes in Chapter 3, there are also summaries. An inconsistent structure conveys a message of lack of coordination between chapters. Secondly, the approach of highlighting key findings in the chapeau provides important points to readers that may be skimming the chapter for salient points. Recommend that the Executive summary of all chapters follow a consistent structure. Chapter 3 serves as a good example to follow. [Govt. of United States of America (Reviewer's comment ID #: 2023-354)]	Taken into account. Most comments are positive to the way Ch 6 has organised the exec. summary.
6-47	A	0:0		Throughout Chapter 6, the authors need to make sure to be absolutely clear whether past climatic conditions cited in the text originate from proxy data, spatial reconstructions, or paleoclimatic models. This is currently unclear in many parts of the chapter, leading readers to believe that modeled temperatures are based directly on proxy data and vice versa. [Govt. of United States of America (Reviewer's comment ID #: 2023-355)]	Accepted
6-48	A	0:0		It is clear in reading this chapter that it was written by multiple authors exercising varying degrees of scientific rigor. The coordinating lead authors need to exercise a stronger role in implementing consistency in both the writing and the scientific integrity of the chapter. For example, if there is not sufficient data to conduct attribution studies of Southern Hemisphere warming over the last 700 years (page 34, lines 48-50), how can page 23, lines 43-48, compare global reconstructions with the late 20th century? Also, the coordinating lead authors need to make sure that the SPM is completely consistent with Chapter 6. [Govt. of United States of America (Reviewer's comment ID #: 2023-356)]	Accepted
6-49	A	0:0		This chapter will go a long way to integrate paleoclimatic data into the climate change debate. However, to inform policymakers, this chapter must reveal the limitations on how well we can truly identify the leads, lags, contemporaneous relations, and rates of change recorded by disparate paleoclimatic proxy records. Dating uncertainties and temporal resolution influence our ability to develop the coherent paleoclimatic reconstructions used to identify the physical mechanisms for the observed changes. To be fully transparent, this chapter must identify the limitations as well as the findings. [Govt. of United States of America (Reviewer's comment ID #: 2023-357)]	Rejected, the authors believe this is done. length limitations limit the detail and rigour.

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6-50	A	0:0		In general the use of tree rings for climate reconstruction is problematic for reasons that are not addressed in the report. There are strong probabilistic relationships between paleoclimatic records, including tree-rings, and climate. Because of this relationship, tree rings provide one of the strongest paleoclimatic proxy records when given through appropriate statistical treatment. This should be addressed in the final paragraph of Section 6.2.1.4. All paleoclimatic proxy methods have limitations and these limitations need to be adequately addressed in Section 6.2.1.4. Chapter 6 needs to provide an explicit explanation of what we know, how well we know it, and what we cannot know through paleoclimatic records. [Govt. of United States of America (Reviewer's comment ID #: 2023-358)]	Accepted, the issue is further addressed, but it is premature to conclude with certainty on what we cannot know, since science progresses rapidly in the field
6-51	A	0:0		The authors of the chapter have done a great job in providing a balanced and concise assessment of paleoclimate information relevant to climate change policy. Add a brief discussion or mention of paleo records for interactions and feedbacks between deglacial and Holocene climate change and terrestrial carbon cycle. Increasing evidence from peatlands suggests that peat carbon store and accumulation rates have responded to climate variations and, as a result, contributed to atmospheric CH4 and CO2 budget during the last 15,000 years. Potential additions could go in Section 6.4.2.1 (p. 18) and 6.5.1.2 (p. 22). Suggested references include: Smith et al. 2004. Science 303: 353-356; Yu et al. 2003; Vitt et al. 2000. Can. J. Earth Sci. 37: 683-693. The Holocene 13: 801-803. [Govt. of United States of America (Reviewer's comment ID #: 2023-359)]	Taken into account, but there are severe limitations on space
6-52	A	0:0		My comments on the paragraph summarizing the contribution of McIntyre and McKitrick constitute one methodological review by climate science outsiders, qualified in the underlying statistics, who apply modern business standards. Secondly these are largely the efforts of one man over a relatively short time, have been a factor (though not the only factor) in a substantial upward revision in the stated warmth of the MWP since the last TAR, and are ongoing. Therefore it would be wise to assume that the sources of uncertainty identified are important, not complete, and not restricted to this part of the chapter, or to the report overall. While this may seem like drawing an unnecessarily large circumference around a problematic area, it is consistent with a trend in other parts of the report towards more conservative estimates of the magnitude of climate change than were described in the TAR, which my time limitations prevent me from detailing [Govt. of United States of America (Reviewer's comment ID #: 2023-360)]	Noted
6-53	A	0:0		Usage of ka and kier inconsistent through chapter [Eric Wolff (Reviewer's comment ID #: 292-16)]	Acepted, will be checked and made consistent
6-54	A	0:0		This chapter summarized recent progress of our knowledge for palaeoclimate which is important for predicting the future changes. Recognition of ice core records as well as surface temperature changes have generally done well. However strongly biassed ideas	Noted, specific comments are dealt with in the sea level section

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				were included in the section where the last glacial sea-level changes are discussed. A number of recent developments done by international community are ignored and the discussions described in the section is single sided. Therefore I am concerned the outcomes if this report will be published as current form since the report is not representing the present status of the international community. I hope the comments that I described below will be considered and included in the new version. [Yusuke Yokoyama (Reviewer's comment ID #: 298-1)]	
6-55	A	1:55	1:55	Delete "robust". [VINCENT GRAY (Reviewer's comment ID #: 88-791)]	Rejected, no basis offered for assertion
6-56	A	2:0	5:	This whoile section consists of a succession of extravagant extreme claims of almost complete knowledge of paleoclimate processes which is not supported by credible literature. [VINCENT GRAY (Reviewer's comment ID #: 88-731)]	Rejected, no basis offered for assertion
6-57	A	2:0		All key finding bullets should have levels of certainty attached. For example, the second (page 2, lines 13-16) and fifth (page 2, lines 26-29) are stated as truisms. [Govt. of United States of America (Reviewer's comment ID #: 2023-361)]	Accepted where possible
6-58	A	2:1	5:28	This Executive Summary should be recast into narrative form as exemplified by the Executive Summaries of all the other Chapters [Richard Soulen (Reviewer's comment ID #: 248-42)]	Rejected, wording has been reviewed twice, prefer to keep as it is
6-59	A	2:1		general comments: this is a verylong chapter - longer than it should be for thepurpose of IPCC. I can only provide partial comments before the deadline because of illness. The chapter is very well written and an excellent source material for someone who wants to know the up to date scoop on paleoclimatology. but that material is not all necessary for ipcc. I think the authors have erred in putting in two much (regardless of the fact I am in keenly interstd in details). at this stage wholesale slash and burn is probably not a good idea, but I think it would be a good idea to go back over the material from the viewpoint as to whether a definitife statement can be made that helps buttress ipcc claims - if not shorten. for example there is a sidebar on the ice age co2 causes and a learned discussion of some of the different positions. but since we do not know the answer to the problem this can be shortened a great deal - causes unknown - a couple of examples of explanations, certainly the lead and lag information is valuable. also with respect to modeling studies for the last glacial cycle,, if we cannot say somethiing definitive keep it shorter. this is just a general guideline. I don't want to sound too harsh on the authors because they have done a lotof work and aas an author myself I know the difficulty of letting go and cutting favorite sections that are actually not critical, but I thik they should at least give it a try. [Thomas Crowley (Reviewer's comment ID #: 51-15)]	Noted, shortened and focussed where possible

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6-60	A	2:1		Executive summary : well established statements should be more direct or affirmative (style more direct with fewer words). See examples below. Are the words "likely", "virtually certain" thoroughly defined and used consistently throughout the IPCC report ? The Executive summary should also put past climate change better in context. For example: define briefly "glacial-interglacial variability", "Last interglacial etc.". [Michel Crucifix (Reviewer's comment ID #: 52-1)]	Accepted
6-61	A	2:1		Section Executive Summary: If not done yet, please add an assessment of the uncertainty (confidence level or likelihood) as far as possible in each bullet point of the executive summary. [Govt. of Germany (Reviewer's comment ID #: 2011-15)]	Accepted
6-62	A	2:1		One point that has probably been already largely discussed is the order of the sections in this summary. As it is, from older time and long time scales towards more recent times and shorter timescales. I was wondering whether the other way round shouldn't be easier for the reader. From more familiar timescales and time intervals (those already discussed in TAR) to less familiar ones. [Marie-France Loutre (Reviewer's comment ID #: 148-22)]	Rejected, existing order resulted from earlier reviews and recommendations
6-63	A	2:6	2:6	Replace "is likely" by "seems possible" [VINCENT GRAY (Reviewer's comment ID #: 88-665)]	Rejected, no basis offered for assertion
6-64	A	2:6	2:6	Delete "all" [VINCENT GRAY (Reviewer's comment ID #: 88-666)]	Rejected, no basis offered for assertion
6-65	A	2:6		it is not a good idea to start with this statemetn because in fact we do not know what the co2 levels from 1-2 Ma. It is just a guess assuming they will continue to lnearly track O18 curves, esp. there was a very nonlinear transition just after 1.0Ma. our own unpublished modeling work suggests such a transition could have been effected by a CO2 level abou t240-250 ppm - ie our model can maintain a different stabel state prior to that with relatively low co2. rather than painting yourself into a corner, state the positive - present CO2 are higher than anything in last 700-800k, and likely already comparable to midPliocene 3 Ma warm period - where we do have proxy stomatal data. [Thomas Crowley (Reviewer's comment ID #: 51-16)]	Acccepted, changed 1 million to 3 million years BP, sentence reworded
6-66	A	2:7	2:7	The first sentence of the para is taken from the underlying text in section 6.3.1., first para. We strongly feels that the conclusion following this information in 6.3.1 shpould also be given in the executive summary. Therefore please insert after "...warmer than present. In the Earth's history warmer climates are to be expected with increased greenhouse gas concentrations, in general." [Govt. of Germany (Reviewer's comment ID #: 2011-16)]	Rejected, made the statement as balanced based on current data and Fig.6.1
6-67	A	2:7	2:7	Delete "also significantly" [VINCENT GRAY (Reviewer's comment ID #: 88-667)]	Rejected, no basis offered for assertion

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6-68	A	2:13	2:13	"have risen far about the natural variability" -> are much higher than [Michel Crucifix (Reviewer's comment ID #: 52-2)]	Rejected, they way it is written takes into accouint that greenhouse gases continue to increase
6-69	A	2:13	2:13	Delete "far" [VINCENT GRAY (Reviewer's comment ID #: 88-668)]	Rejected, no basis offered for assertion
6-70	A	2:13	2:13	Delete "natural" [VINCENT GRAY (Reviewer's comment ID #: 88-669)]	Rejected, no basis offered for assertion
6-71	A	2:15	2:15	stable coupling not easy to understand. May be stable relationship could be better? [Pascale BRACONNOT (Reviewer's comment ID #: 29-1)]	Accepted
6-72	A	2:18	2:19	This statement is not accurate. Replace "at present" with "in recent decades" (line 19). [Govt. of Australia (Reviewer's comment ID #: 2001-303)]	Accepted
6-73	A	2:18	2:18	Replace "It is virtually certain" with "Observations so far indicate" [VINCENT GRAY (Reviewer's comment ID #: 88-670)]	Rejected, no basis offered for assertion
6-74	A	2:18	2:18	Delete "in radiative forcing" [VINCENT GRAY (Reviewer's comment ID #: 88-671)]	Rejected, no basis offered for assertion
6-75	A	2:18	2:18	Replace "well-mixed" by "minor". They are cetrtyainly NOT "well-mixed" [VINCENT GRAY (Reviewer's comment ID #: 88-672)]	Rejected, no basis offered for assertion
6-76	A	2:18	2:18	It is virtually certain: comment is unclear, far too ambiguous specially considering the evidence available on the issue [Govt. of Spain (Reviewer's comment ID #: 2019-107)]	Rejected, is balanced as is
6-77	A	2:19	2:20	maybe replace `at present' by `over recent decades'. As it stands, sentence is untrue. CH4 growth rate is not greater at present than it was a few decades ago. [ian Enting (Reviewer's comment ID #: 63-9)]	Accepted
6-78	A	2:22	2:22	Replace "the current warming will be mitigated by a natural" by " of a current" [VINCENT GRAY (Reviewer's comment ID #: 88-673)]	Rejected, no basis offered for assertion
6-79	A	2:22	2:24	The statements in this section are all related either to past or present climate, mostly related with obervation. This statement is related to long-term future climate change, from modelling evidence. I would suggest to separate it more clearly from the other statements, for example by putting it at the end of the section. Moreover, although the statement is very important and very strong (and must appear in this summary), I do not see very clearly its link with the question raised. [Marie-France Loutre (Reviewer's comment ID #: 148-20)]	Accepted , moved
6-80	A	2:22	:24	Refer to comment 5, quoted here:statement is made that the longest interglacial has been 30K years; in the summary for policy makers, the statement is made that there won't be another cooling event for at least 30K years more, after already having 10K years of interglacial. The year 2022 will be interesting.	Accepted

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				[Lee Gerhard (Reviewer's comment ID #: 83-8)]	
6-81	A	2:23	2:23	Replace "very likely" by "generally expected" [VINCENT GRAY (Reviewer's comment ID #: 88-674)]	Rejected, no basis offered for assertion
6-82	A	2:23	2:23	Delete "naturally" [VINCENT GRAY (Reviewer's comment ID #: 88-675)]	Rejected, no basis offered for assertion
6-83	A	2:23	2:23	rewrite: It is very unlikely that the Earth would naturally enter another ice ..... [Atle Nesje (Reviewer's comment ID #: 190-2)]	Noted, section is revised
6-84	A	2:26	2:29	Delete this paragraph. The authors of this Chapter are not qualified to speculate on possible "projected" temperatures [VINCENT GRAY (Reviewer's comment ID #: 88-676)]	Rejected, no basis offered for assertion
6-85	A	2:26		I have looked through the draft chapter 6 and find it an impressive document. However, bullet 4 on page 6.2, starting "global mean cooling and warming....." strikes me as incorrect and misleading. Whereas the mean rate of temperature change over the Pleistocene may have been 10 times slower than that projected for the next century, there is clear evidence that for specific major climatic transitions, global (or at least hemispheric) temperature changes in the past have been at least as rapid as those projected by climate model simulations and incorporated in the last IPCC report. The most obvious case in point is the global warming at the start of the Holocene, ca. 11.5 ka BP. Russell Coope, more than 20 years ago, showed from beetles that UK temperatures rose faster than could be dated within the errors of 14C dating. Subsequently this was confirmed by Greenland ice cores based on layer counting (full glacial to interglacial in less than 100 years), and by the Cariacos basin marine record. I have worked on varved lake records from both the tropics (Roberts et al Nature 1993 366, 146-148) and the Mediterranean (Roberts et al The Holocene, 2001, 11, 719-734) where this climate transition was accomplished in substantially less than a century. In short, several independent lines of evidence show that the climate system has been capable of flipping from one meta-stable state to another, very different one over timescales that could be experienced by a single human lifetime. This is not an unimportant conclusion in terms of the potential for non-linear responses of future climate to GHG forcing. I also looked for supporting argument for bullet 4 later in chapter 6, but found nothing of substance. In short, this particular bullet seems in need of critical reassessment before the definitive version of the next IPCC report emerges, or simpler still – just cut it. [C Neil Roberts (Reviewer's comment ID #: 216-1)]	Noted, updated main text see comment 6-88.
6-86	A	2:28	2:28	"took place at a rate ten times slower " -> are ten times slower [Michel Crucifix (Reviewer's comment ID #: 52-3)]	Noted, text changed
6-87	A	2:29	2:29	Change "more than ten time slower than this projected future change" to "at least ten times slower than any projected future change. The "any" is the important change.	Noted, text changed

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				740 6-740 1 [Ronald J Stouffer (Reviewer's comment ID #: 258-3)]	
6-88	A	2:29		actually it is a lot greater than ten times, average is close to 80X - don't minimize the difference because of concern about criticism of alarmism - state the numbers as they are [Thomas Crowley (Reviewer's comment ID #: 51-17)]	Rejected, text rephrased , new wording on the safe side
6-89	A	2:31	2:34	Climate models capable of representing the broad-scale regional features. Arguable. Some features, such as thecentral Asian climate, or western African monsoon, nordic seas and North Atlantic meridional overturning cell still pose difficulties for the LGM and mid-Holocene, which may either be due to climate models, forcings or interpretation of data. [Michel Crucifix (Reviewer's comment ID #: 52-4)]	ACCEPT - see revisions
6-90	A	2:31	2:31	Insert after "models" "based on the unlikely supposition that greenhouse gases are the exclusive influence on the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-677)]	REJECT - not in these cases
6-91	A	2:31	2:31	Insert after "proved" "surprisingly" [VINCENT GRAY (Reviewer's comment ID #: 88-678)]	NOTED - paragraph changed
6-92	A	2:31	2:31	Insert after "simulating" "some of" [VINCENT GRAY (Reviewer's comment ID #: 88-679)]	ACCEPT - in effect
6-93	A	2:31	2:33	This statement is one of the very few dealing with modelling work. Unfortunately, it is suggesting that the models are only able to simulate LGM. More precisely, the statement only discuss about LGM and not about the others time slices or time intervals that were also sucessfully simulated by models. This might be misleading. Moreover, it is important that models are able to simulate cold climate (like LGM) but it is probably more important in the context of global warming that they are able to simulate warm climate. This should also be underlined. [Marie-France Loutre (Reviewer's comment ID #: 148-21)]	REJECT - the 'warm' climate referred to here is not from the same forcing as the future climate. No reference here is made of the ability of model's to simulate the Tertiary climates properly - in fact, that is covered in another bullet (bullet 1).
6-94	A	2:31	2:33	This statement is correct except in the representation of Abrupt Climate changes observed in the past. Very specific models and forcings can reproduce the observed differential changes between Northern and Southern Hemispheres but these are dependent on specific parameterisations. In general then I and I think many others do not consider that we have a unique explanation (or means of modelling) abrupt changes such as during stage 3. [Mark Siddall (Reviewer's comment ID #: 238-1)]	TAKEN INTO ACCOUNT - the inability of models to handle many abrupt features is discussed in another bullet.
6-95	A	2:31	:33	This bullet is unclear. Change bullet to read: "Using estimated radiative forcing and land surface changes of the Last Glacial Maximum, climate models can simulate many of the broad-scale patterns of climate change reconstructed from paleoclimatic data." [Govt. of United States of America (Reviewer's comment ID #: 2023-362)]	TAKEN INTO ACCOUNT - the new bullet is more consistent with this suggestion.
6-1117	B	2:31		As I understand it, climate models cannot presently both get into an ice age and get out of	TAKEN INTO ACCOUNT - the

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				it. If this is correct, then the summary here is misleading. [Stephen McIntyre (Reviewer's comment ID #: 309-13)]	more precise specification of what models can do for the LGM should clarify some of this; in addition, another bullet on non-linearities discusses another component of this.
6-96	A	2:32	2:33	I suggest rewording as " … by paleoclimate data IN RESPONSE to the radiative forcing and land surface changes of the Last Glacial Maximum, AND thus INDICATE THAT THEY adequately represent the processes …" [Danny Harvey (Reviewer's comment ID #: 101-34)]	TAKEN INTO ACCOUNT - a good portion of this is included in noting the specification of the land surface boundary conditions including ice sheets.
6-97	A	2:32		There must be a word missing here. Could be "paleoclimatic data due to" [Eric Wolff (Reviewer's comment ID #: 292-3)]	REJECT - 'due to' was not needed but point is moot as it has been changed.
6-98	A	2:33	2:33	Models are not as good as suggested by this sentence. So I would suget to change to : representing the major processes that determine [Pascale BRACONNOT (Reviewer's comment ID #: 29-2)]	TAKEN INTO ACCOUNT - what models can do has been made more specific.
6-99	A	2:33	2:33	Replace "adequately reprenting" by "giving some clues to" [VINCENT GRAY (Reviewer's comment ID #: 88-680)]	TAKEN INTO ACCOUNT - model abilities have been re-stated.
6-100	A	2:35	2:40	"redistribution of heat between the northern and the southern hemisphere". This is not proved. For example, there is no clear signature of D/O events in Antarctica (there is one for Heinrich events). D/O events are probably associated with a redistibution of heat between the surface and the deep ocean. Use therefore the more general statement "redistribution of heat within the system". [Michel Crucifix (Reviewer's comment ID #: 52-5)]	Accepted
6-101	A	2:35	2:35	"climate shift". Rather use "climate change". [Michel Crucifix (Reviewer's comment ID #: 52-6)]	Accepted
6-102	A	2:35	2:41	Emphasise the methane record as an argument on the global character of D/O events. [Michel Crucifix (Reviewer's comment ID #: 52-8)]	Rejected, too much detail
6-103	A	2:35	6:41	This paragraph confuses the abrupt Dansgaard-Oeschger events with the more general millennial scale variability. Evidence for abrupt events -- strictly speaking -- is limited to the Northern Hemisphere, and more likely the North Atlantic region only. There is strong evidence for concommittant changes to North Atlantic abrupt warming in the Southern Hemisphere, but it may not be a response, and evidence for the "abruptness" is certainly not global. To say that the Dansgaard-Oeschger events have "repercussions" is a	Noted, text changes

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				statement of cause and effect which is not by any means universally accepted. A very plausible alternative view is that the Dansgaard-Oeschger events are a response to changes elsewhere, for due to slow changes in the tropics. [Eric Steig (Reviewer's comment ID #: 252-2)]	
6-104	A	2:37	2:37	"temperature likely changed" -> "temperature increased". Be as specific as possible. [Michel Crucifix (Reviewer's comment ID #: 52-7)]	Accepted
6-105	A	2:37	2:37	Replace "likely" by "may have" [VINCENT GRAY (Reviewer's comment ID #: 88-681)]	Rejected, no basis offered for assertion
6-106	A	2:39	2:39	Delete "It is unlikely that" [VINCENT GRAY (Reviewer's comment ID #: 88-682)]	Rejected, no basis offered for assertion
6-107	A	2:39	2:39	Replace "were" with "could hardly have been" [VINCENT GRAY (Reviewer's comment ID #: 88-683)]	Rejected, no basis offered for assertion
6-108	A	2:39	2:41	This sentence is confusing. It is unclear how a redistribution of heat between northern and southern hemisphere wouldn't impact global temperature. The wording of the last sentence of this paragraph is misleading and gives the impression that this heat redistribution was not significant. [Myriam Khodri (Reviewer's comment ID #: 126-1)]	Noted, text changed
6-109	A	2:40	2:40	Replace "instead very likely" by "possibly involved" [VINCENT GRAY (Reviewer's comment ID #: 88-684)]	Rejected, no basis offered for assertion
6-110	A	2:43	2:43	eplace "are very likely" with "could be" [VINCENT GRAY (Reviewer's comment ID #: 88-685)]	Rejected, no basis offered for assertion
6-111	A	2:43	2:43	Remove 'very' from 'very likely' I do not think we can totally rule out atmospheric changes (due for example to changes in albedo, changing ice sheets) as driving abrupt change [Mark Siddall (Reviewer's comment ID #: 238-2)]	Noted, text changed
6-112	A	2:43	6:47	I will accept that it is "likely" that some large abrupt events of the past are linked to changes in the Atlantic ocean circulation. What are the grounds for claiming the science on this falls under the category "very likely". Although not cited in this chapter, there are numerous peer-reviewed papers in high-profile journals that present criticism of this hypothesis. [Eric Steig (Reviewer's comment ID #: 252-3)]	Accepted
6-113	A	2:43	:47	Attribution of abrupt climate change only to changes in Atlantic Ocean circulation ignores other explanations including possible nonlinear responses of tropical Pacific variability to radiative forcing directly overhead (Clement et al. 1997; Cane and Clement 1999; Mann et al. 2005). These two leading theories may be partly reconciled by emerging evidence that big changes in the Atlantic can modulate ENSO frequencies, (see recent paper by	Accepted, Is covered by new text

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				Dong et al. 2006. Geophysical Research Letters), possibly at multiple time scales. Note that allusion is already made to the dynamic ocean thermostat theory on another of the major findings (page 6-3, lines 39-41). [Govt. of United States of America (Reviewer's comment ID #: 2023-363)]	
6-114	A	2:44	2:44	Replace "still under discusion" with "unknown" [VINCENT GRAY (Reviewer's comment ID #: 88-686)]	Rejected, no basis offered for assertion
6-115	A	2:45		Chris Charles David Rind and company pointed out many years ago that there are significant problems of just looking for the conveyor to effect these changes - I agree with them - there may be ice sheet induced changes in steeringn of winds that played a very big role [Thomas Crowley (Reviewer's comment ID #: 51-18)]	Accepted
6-116	A	2:49	2:49	Replace "is likely unprecedented" by "has not been identified" [VINCENT GRAY (Reviewer's comment ID #: 88-687)]	Accepted, bullet deleted
6-117	A	2:50	2:50	Replace is likely" with "could possibly be" 312 6-312 688 [VINCENT GRAY (Reviewer's comment ID #: 88-687)]	Accepted, bullet deleted
6-118	A	2:50	2:50	Delete "enhanced" [VINCENT GRAY (Reviewer's comment ID #: 88-689)]	Accepted, bullet deleted
6-119	A	2:52	2:55	We do not find the underlying text in the chapter and there seems to be a lack of consistency between the main chapter and this bullet. Please carefully check this consistency, as well as the onsistency between this bullet and what appears in the TS and in the SPM. Again, this is not fully consistent and does not reflect what is writen in Chapter 6. [Govt. of France (Reviewer's comment ID #: 2010-48)]	Accepted, bullet and text changed
6-120	A	2:52	2:54	If doubt that an LIG sea-level high stand of 4 to 6 m is consensual. Schellmann G., Radtke U., 2004, A revised morpho- and chronostratigraphy of the late and middle Pleistocene coral reef terraces on Southern Barbados (West Indies), Earth-Science reviews 64, 157-187; Stirling C.H., Esat T.M., Lambeck K., McCulloch M.T., 1998, Timing and duration of the last interglacial: evidence for a restricted interval of widespread coral reef growth, Earth and Planetary Science Letters 160, 745-762., provide reviews of sea level at the LIG and from these a more reasonable range would be between 2 and 6 m, allowing for unceratinty in the uplift rates of Barbados. [Mark Siddall (Reviewer's comment ID #: 238-3)]	Accepted, text changed
6-121	A	2:52	3:2	This statement is based largely on one recent paper and should not be discussed here, as it is not yet a well considered result, and it may not be very relevant. Yes, it was warmer at the last interglacial, and sea level was higher, but the radiative forcing in summer in the Arctic -- and likely SUMMER temperature -- were far greater than anything we expect in the near future. Glaciers care about summer temperature, not mean annual temperature.	Rejected, major conclusions also based on other, earlier published papers

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				[Eric Steig (Reviewer's comment ID #: 252-4)]	
6-122	A	2:53	2:54	"The sea level rise was likely driven" -> be more affirmative on what actually happened in the climate model. [Michel Crucifix (Reviewer's comment ID #: 52-9)]	Accepted, new text
6-123	A	2:53	2:53	Replace "likely" with "may have" [VINCENT GRAY (Reviewer's comment ID #: 88-690)]	Rejected, no basis offered for assertion
6-124	A	2:53	2:53	confirmed" is misleading. In contrast to data, a model can never confirm a hypothesis such as the retreat of the ice sheet. Consider to replace "confirmed" by "corroborated [Michael Schulz (Reviewer's comment ID #: 229-2)]	Accepted, new text
6-125	A	2:54	2:54	Replace "was likely" with could have been" 315 6-315 691 [VINCENT GRAY (Reviewer's comment ID #: 88-2)]	Rejected, no basis offered for assertion
6-126	A	3:1	3:2	Question: If the rate of sea level rise may have exceeded 1m/century during the previous interglacial, then the recent sea level rise meassured by altimeters of ~3mm/year would amount to one third of the previous rise. Is that the case? [Eva Bauer (Reviewer's comment ID #: 15-1)]	Noted, but this element is handled by Chapter 5
6-127	A	3:1	3:1	Replace "likely also" with "may also have" [VINCENT GRAY (Reviewer's comment ID #: 88-692)]	Rejected, no basis offered for assertion
6-128	A	3:1	3:2	Referring to the possiblity that "the rate of sea level rise leading to this [last interglacial] high-stand may have exceeded 1 m/century" in the Excecutive Summary is questionable. This statement is based largely on just one recent paper (Overpeck et al., 2006). What is the likelihood that it is relevant to future sea level rise? This is potentially very misleading and could be considered "alarmist" since at present we really dont' know. I would suggest deleting this statement from the Executive Summary. [Eric Steig (Reviewer's comment ID #: 252-5)]	Bullet rewritten
6-129	A	3:1	3:2	I can't see this statement about 1 m/century backed up in the main text, though I guess it is based on the recent Overpeck et al paper. But in my reading of that paper, this statement refers to the last and penultimate deglaciation, where the main contributor to the RATE of sea level change is the Laurentide, so I find it misleading to use it here in close association with Antarctic ice sheet loss, and the implication about future rise. [Eric Wolff (Reviewer's comment ID #: 292-4)]	Accepteed, new text
6-130	A	3:7	3:7	"likely due to mostly natural processes" Human clearing forests in Europe, China did not impact the GHG concentrations? [Ronald J Stouffer (Reviewer's comment ID #: 258-2)]	Reejected, human contribiution only 2nd order. Not supported by litterature
6-131	A	3:10	3:32	The different paragraphs should be presented in a different order, so that their respective contents better follow the different time intervals considered. Mid -Holocene (24-29) before millenia (30-32) before 20th century (18-22)	Accepted

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				[Pascale BRACONNOT (Reviewer's comment ID #: 29-3)]	
6-132	A	3:10	3:12	The statement "different regions underwent periods warmer and cooler than the 20th century" is trivial (in the case it refers to the global mean temperature of the 20th century) and misleading. Please refer the statement explicitly to the regional temperature change. Proposal: "...warmer and cooler than they have been during the 20th century as regional vcariability often exceeds global variability and because of changes in the Earth's..." 52 6-52 17 [Govt. of Germany (Reviewer's comment ID #: 2011-3)]	See response to comment 134
6-133	A	3:10	3:16	I am slightly disappointed that the importance of the orbital forcing is only pointed out in the context of our interglacial. Moreover it is associted with the Medieval Warm Period,during which the orbital forcing (sensu stricto) is probably the most important to explain the climate changes then. [Marie-France Loutre (Reviewer's comment ID #: 148-23)]	Rejected, no basis offered for assertion
6-134	A	3:10	:16	This finding is a bit overstated given the data limitations. The time scales of all of the Holocene warming events cited here are different than the time scale of the late 20th century warming. There is a lack of interannual resolution at global coverage for practically all of these events. If in hand, comparable warming over a few decades could be discerned at other times in the Holocene. This mismatch in temporal scales and global coverage in the comparison of warm spells in the Holocene with late 19th century warming needs to be addressed in Section 6.5.1.3. Change sentence starting in line 14 to read: "However, data coverage, temporal resolution, and age control of available proxy data make it impossible to discern if the earlier Holocene contained 50 year periods of global warmth comparable to the late 20th century." [Govt. of United States of America (Reviewer's comment ID #: 2023-364)]	Accepted
6-135	A	3:12	3:16	The summary says "there are no known Holocene periods of synchronous global warmth comparable to the late 20th century." This statement is taken from Sct 6.5.1.3, which poses the question of whether the mid-Holocene was warmer than the present, not whether there were synchronous warming periods, thereby insinuating that there is no period in the Holocene in which global warmth is comparable to late 20th century. Such a claim is not made or supported in the text. First of all, section 6.6.1.2 concludes that "deep soil temperature is a good proxy for the annual SAT on continents and that the spatial array of borehole locations is adequate to reconstruct the Northern Hemisphere mean SAT." The GST reconstruction of Huang, Pollack and Shen (1997) clearly indicates substantially higher mid-Holocene Optimum temperatures in a globally-synchronous sample, but this paper is not mentioned in Section 6.5.1.3, where the question is posed. Instead, appeal is made to Figure 6.9, which provides a qualitative, graphical summary of a disparate group of selected proxy-based studies. When reference is made in Chapter 6 to	Accepted

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				another paper using a similar technique (Soon and Baliunas) the findings are dismissed on the grounds that the technique provides no basis for quantitative ranking with the late 20th century. The same is true here, yet the authors still draw a quantitative conclusion. Moreover, globally-synchronous warming in the 7-8kYbp interval is only contraindicated by the blue box representing the tropical Pacific and Indian oceans. The alkenone-based evidence of Kim et al (2000) points to 3C warmer SST during this interval in the South Pacific off Chile, and Lagerklint et al (2005) find similar results for the East Equatorial South Atlantic, so the large blue box is an exaggeration of the spatial extent of the relative cold anomaly. The paper by Lorenz et al only covers the last 7,000 kYbp so it does not provide support for ruling out synchronous changes over the entire interval referred to--a misleading usage of the source. [Ross McKitrick (Reviewer's comment ID #: 174-22)]	
6-136	A	3:12	:16	Argues that previous warm events are local or regional and not sufficiently significant to affect conclusions, but this state flies in the face of a huge volume of current literature that forcefuylly documents that the MWE, for example, was at least a Northern Hemisphere event, if not global. This does not argue well for conclusions that are later reached based on that assumnption. [Lee Gerhard (Reviewer's comment ID #: 83-1)]	Rejected, not supported by litterature, see references in the chapter and on Fig. 6.9
6-137	A	3:14	3:14	Insert after "synchronously" "but our poor samples makes it unwise to derive averages" [VINCENT GRAY (Reviewer's comment ID #: 88-693)]	Rejected, no basis offered for assertion
6-138	A	3:14	3:16	Delete from "Consistent" on lne 14 to "century" on line 16. There are simply not enough samples to make such a confident statement. [VINCENT GRAY (Reviewer's comment ID #: 88-694)]	Rejected, no basis offered for assertion
6-139	A	3:15	3:15	There is evidence for a late Holocene warm period (Medieval warm period) in parts of the Northern Hemisphere and parts of the Southern Hemisphere [for SH evidence see Cooke et al 2000 Clim Dynam 16, 79-91 and Williams et al 2005 Earth & Plan Sci Letters 230, 301-317] [Paul W Williams (Reviewer's comment ID #: 291-1)]	Rejected, not supported by litterature, see references in the chapter and on Fig. 6.9
6-140	A	3:18	3:23	"in response to warming" : which warming do you speak about (Holocene optimum or XXIst century?) [Michel Crucifix (Reviewer's comment ID #: 52-10)]	Accepted, text changed
6-141	A	3:18	3:19	The text ist difficult to understand. Please simplify as follows: " …retreated in the response to warming. As of… (higher summer insolation? - please specify!)… the glaciers were smaller in the early to mid-Holocene … " [Govt. of Germany (Reviewer's comment ID #: 2011-18)]	Accepted, text changed
6-142	A	3:18	3:18	I find this statement confusing and not well discussed in the text. Box 6.3 should make the point that the decrease in summer insolation during the past few millennia should favor	Noted, insolation changes discussed in orbital box

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				glacier growth - and that the observation is that the glaiers are melting. Box 6.3 does make the case that the glacier record is complex and should be interpreted regionally in terms of precipitation and temperture but it does not directly support the idea of a decrease in summer insolation through the Holocene. [Gregory Wiles (Reviewer's comment ID #: 289-1)]	
6-143	A	3:18	:22	Report argues that although past interglacial was warmer, ice less, than current, but that recent climate is caused by different drivers, therefore the fact that the earth is less warm and there is more ice should be disregarded. There will be discussion of this after this report is published. [Lee Gerhard (Reviewer's comment ID #: 83-2)]	Rejected, no basis in litterature
6-144	A	3:20	3:20	Replace "cannot be attrtibuted " with "is difficult to attribute" [VINCENT GRAY (Reviewer's comment ID #: 88-695)]	Accepted
6-145	A	3:22	3:22	Change "the glaciers" to "NH glaciers". [Ronald J Stouffer (Reviewer's comment ID #: 258-3)]	Rejected, reflects a global picture
6-146	A	3:24	3:28	Not all "monsoons" were enhanced. Summer south-tropical american monsoon was probably less penetrative than today. Also, distinguish clearly summer and winter monsoon. Overall, it is probably better to speak about penetration of monsoon on the continent,i.e. "the monsoon can be more or less penetrative" than "enhanced", which is more fuzzy. [Michel Crucifix (Reviewer's comment ID #: 52-11)]	ACCEPT - monsoon changes are now made non-specific.
6-147	A	3:24	3:24	Insert after "models" "based on the unlikely supposition that greenhouse gases are the exclusive influence on the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-696)]	REJECT - that is not the forcing used.
6-148	A	3:24	3:24	Insert after "are" "surprisingly" [VINCENT GRAY (Reviewer's comment ID #: 88-697)]	REJECT - sentence has been changed, so it's a moot point.
6-149	A	3:24	3:24	Replace "most robust" with "some" [VINCENT GRAY (Reviewer's comment ID #: 88-698)]	TAKEN INTO ACCOUNT - altered words (many of) are now used.
6-150	A	3:25	3:25	Replace "observed" by "inferred" [VINCENT GRAY (Reviewer's comment ID #: 88-699)]	REJECT - observed is used in the paloeoclimate context, which by virtue of the use of proxies almost always means 'inferred'.
6-151	A	3:25	3:25	Replaced 'observed' by 'inferred' (we have no record of 'observed' climate changes 6,000 years ago)	REJECT - observed is used in the paloeoclimate context, which by virtue of the use of proxies almost

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				[Michael Mann (Reviewer's comment ID #: 156-59)]	always means 'inferred'.
6-152	A	3:27	3:27	"Coupled models generally perform better than atmosphere-only models" What is meant by this statement? Delete? [Ronald J Stouffer (Reviewer's comment ID #: 258-4)]	TAKEN INTO ACCOUNT - noting that models tend to underestimate changes, the improvement associated with coupled models can be read to imply their obtaining larger changes.
6-153	A	3:30	3:32	Delete this paragraph..We do not need to know "no evidence". The "global warming" of the past 100 years was contaminated by sample bias, from proximity of measuring equipment to human activity [VINCENT GRAY (Reviewer's comment ID #: 88-700)]	Rejected, no basis offered for assertion
6-154	A	3:30	3:32	I find it confusing to state that there is no evidence that centennial to millennial cycles of natural climate variability can cause cooling in the past. Perhaps more explanation is needed or rewording - for surely there are millennial-scale and centennial-scale cycles recognized during the Holocene, however these alone cannot be responsible for the observed warming. [Gregory Wiles (Reviewer's comment ID #: 289-2)]	Rejected, litterature shows no picture of consistent cyclicity between records
6-155	A	3:30	:32	In actuality, the correlation between temperature and solar activity is exceeding good, and much better than greenhouse. [Lee Gerhard (Reviewer's comment ID #: 83-9)]	Rejected, litterature shows no picture of consistent cyclicity between records
6-156	A	3:34	3:38	This paragraph supposes an analogy between the mid-Holocene and the future climate which is not correct. The mid-Holocene was characterised by a different seasonal cycle than today and in the future. [Michel Crucifix (Reviewer's comment ID #: 52-12)]	ACCEPT - Paragraph changed.
6-157	A	3:36	3:36	Replace "under global warming" by "if similat conditions recur" [VINCENT GRAY (Reviewer's comment ID #: 88-701)]	TAKEN INTO ACCOUNT - in effect, by the paragraph change.
6-158	A	3:39	3:41	The statement about ENSO is too strong,because most of the analyses mixe information on the mean state and interannual variability, and results from GCMs are not yet conclusive. [Pascale BRACONNOT (Reviewer's comment ID #: 29-4)]	ACCEPTED - Statement made more equuivocal, and model limitations noted.
6-159	A	3:39	41:	I hae some problems with this statement. We know from the 20th c. that there are already large changes in nature of enso at different times. Paleo coral records are usually very small time slices and the statistical properties of their oscillations may not necessarily be statistically different thanthe range of values implied by the 20th century, if some monte carlo tests were conducted. I think many people have overstated the significance of this due to short sample length. the case has not beenn provven.	ACCEPTED - Statement made more equuivocal.

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				[Thomas Crowley (Reviewer's comment ID #: 51-19)]	
6-160	A	3:43	3:43	Replace "hurricanes" with "tropical cyclones" [Govt. of Australia (Reviewer's comment ID #: 2001-304)]	ACCEPT
6-161	A	3:43	3:45	The terms "abrupt shifts" is misplaced here, and will confuse the average reader with the use of "abrupt" to refer to the Dansgaard-Oeschger events. Certainly there are changes in the frequency of hurricanes, the "abruptness" of such changes is not demonstrated. What is known is that there is low frequency variability that changes the frequency of high-frequency events, and these ARE captured in climate models. [Eric Steig (Reviewer's comment ID #: 252-6)]	TAKEN INTO ACCOUNT - We need to define 'abrupt here'. or remove the word.
6-162	A	3:45	3:45	"nor captured by current climate models" The statement seems too strong. Papers by Hunt and Sieger seem to indicate models can capture the megadroughts of the past. Change to "not captured by models anlayzed to date" or something to modify the statement. [Ronald J Stouffer (Reviewer's comment ID #: 258-5)]	TAKEN INTO ACCOUNT - Phrase describing model capability has been modified.
6-163	A	3:49	3:49	Replace "is virtually certain" by "seems probable that" [VINCENT GRAY (Reviewer's comment ID #: 88-702)]	Rejected, no basis given for assertion consistent cyclicity between records
6-164	A	3:49	3:49	Insert after "in" "the minor greenhouse gases" [VINCENT GRAY (Reviewer's comment ID #: 88-703)]	Rejected, no basis given for assertion
6-165	A	3:49	3:50	This statement was already written in the context of the glacial-interglacial variability. If it is true for the last 20,000 yr, it must be true for the last 2,000 yr. Should it be repeated? [Marie-France Loutre (Reviewer's comment ID #: 148-24)]	Rejected, authors feels it is appropriate to make the statement in this context
6-166	A	3:49	4:7	The use of virtually certain, very likely and likely here seems strange. While I understand that nothing in science is absolutely certain, we do have direct measurements (from ice cores) of greenhouse gases over the last 2000 years. I don't see any need to qualify any of these statements (except perhaps the nitrous oxide where the data are more sparse). Therefore "line 51: "The average rate forcing calculated from these..."; line 55, remove "very likely" (since we observe it); page 6, line 4, remove "very likely". the only reason i can see for any doubt is that there could be very brief high rates of increase hidden in the resolution of the ice core record, but this would be better covered by referring to the average rate of increase on a decadal scale. 813 6-813 5 [Eric Wolff (Reviewer's comment ID #: 292-24)]	Accepted
6-167	A	3:50	3:50	Replace "at present" with "in recent decades". [Govt. of Australia (Reviewer's comment ID #: 2001-305)]	Accepted
6-168	A	3:50	3:51	maybe replace `at present' by `over recent decades'. As it stands, sentence is untrue. CH4 growth rate is not greater at present than it was a few decades ago. [ian Enting (Reviewer's comment ID #: 63-10)]	Accepted
6-169	A	3:51	3:53	Delete from "It is very likely" in line 51 to "era" in line 53. This sentence is misleading.	Rejected, text is OK as it is

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				What is important is the total radiative forcing, including contributions from the major greenhouse gas, water vapour and clouds, not that due to the minor components only, let alone such natural contributors such as the sun and volcanos". 328 6-328 704 [VINCENT GRAY (Reviewer's comment ID #: 88-10)]	
6-170	A	3:51	3:51	this should say "virtually certain", not "very likely" [Eric Steig (Reviewer's comment ID #: 252-7)]	Accepted
6-171	A	3:55	3:55	Replace "very likely" with "possible" [VINCENT GRAY (Reviewer's comment ID #: 88-705)]	Rejected , no basis given for assertion
6-172	A	3:55	3:55	this should say "virtually certain", not "very likely" [Eric Steig (Reviewer's comment ID #: 252-8)]	Rejected , no basis given for assertion
6-173	A	3:55	4:3	Delete. Repetitive of lines 49-53 and not worthy of executive summary. [Govt. of Australia (Reviewer's comment ID #: 2001-306)]	Accepted
6-174	A	3:55	4:7	Group the 3 paragraphs and present them in a more synthetic way. [Michel Crucifix (Reviewer's comment ID #: 52-13)]	Accepted
6-175	A	4:2	4:2	Replace "peaked around 1980" with"has fallen since 1983, and is currently hovering around zero" I suggest you read Chapter 2 [VINCENT GRAY (Reviewer's comment ID #: 88-706)]	Noted bullet removed, subject treated in Ch. 2
6-176	A	4:2	4:2	Replace "when it was very likely" with "The current average concentration is possibly" [VINCENT GRAY (Reviewer's comment ID #: 88-707)]	Noted bullet removed, subject treated in Ch. 2
6-177	A	4:2	4:2	this should say "virtually certain", not "very likely" [Eric Steig (Reviewer's comment ID #: 252-9)]	Noted bullet removed, subject treated in Ch. 2
6-178	A	4:3	4:3	Replace "higher than " by "as high as" [VINCENT GRAY (Reviewer's comment ID #: 88-708)]	Noted bullet removed, subject treated in Ch. 2
6-179	A	4:5	4:5	Replace "likely" with "possible" [VINCENT GRAY (Reviewer's comment ID #: 88-709)]	Rejected, statement is judged correct
6-180	A	4:7	4:7	Insert a dot point referring to CFCs (and other halogens), saying that no natural abundance existed before industrialisation. [Govt. of Australia (Reviewer's comment ID #: 2001-307)]	Taken into account, text has been changed
6-181	A	4:9	4:10	Delete "very likely" [VINCENT GRAY (Reviewer's comment ID #: 88-710)]	Rejected, no basis given for assertion
6-182	A	4:9		"sulfate" not "sulfur" (sulfate is what is measured in the ice, and SO2 is what is emitted). [Eric Wolff (Reviewer's comment ID #: 292-6)]	Accepted
6-183	A	4:10		Remove "very likely". The ice core data ARE consistent with the emissions estimates. There may be other reasons for the similarity, but they are consistent. [Eric Wolff (Reviewer's comment ID #: 292-7)]	Accepted

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6-184	A	4:14	4:14	Add at end "The distribution of samples is, however, still very poor, so there is doubt whether the "averages" can be trusted" [VINCENT GRAY (Reviewer's comment ID #: 88-711)]	Rejected, authors believe stament is supported by current knowledge
6-185	A	4:16	4:21	This statement should let the reader know that the note is in relation to the last 1000 years. [Gregory Wiles (Reviewer's comment ID #: 289-3)]	Accepted
6-1118	B	4:16		You should add that there has been controversy over some of the statistical methods used in the TAR reconstructions. This is one of the most public faces of IPCC TAR and there's no point not acknowledging it. [Stephen McIntyre (Reviewer's comment ID #: 309-14)]	Rejected, it does not change meaning of bullet. Covered in main text
6-186	A	4:21	4:21	Add at end "Again, the poor sample distribution lends considerable doubt to this conclusion" [VINCENT GRAY (Reviewer's comment ID #: 88-712)]	Rejected, no basis given for assertion
6-187	A	4:23	3:28	Should be more direct. Also, the question to be answered for the policy-maker is "What is the likelihood of the present climate warming being of naturalorigin ". This paragraph does not answer that question. [Michel Crucifix (Reviewer's comment ID #: 52-14)]	Rejected, question of causation is a different issue
6-188	A	4:23	4:30	"Insert "Based on proxy data from 26 locations," before the sentence that starts "It is also.." Figure 6-11(a) shows that the conclusion that it is likely that the second half of the 20th century was the warmest period in the NH in past 1000 years is based on extremely limited information, with proxy data from only 26 sites by my count. This information should be included with the conclusion as an indication of its basis. 45 6-45 60 [Lenny Bernstein (Reviewer's comment ID #: 20-14)]	Rejected , issue covered in main text
6-189	A	4:23	4:30	The chapter indicates that the conclusion that the second half of the 20th century is likely to have been the warmest period in the Northern Hemisphere in the last 1000 years is based on proxy data from 26 locations. This fact should be included in the conclusion. [Jeff Kueter (Reviewer's comment ID #: 137-56)]	Rejected , issue covered in main text
6-190	A	4:23	4:30	I assume problems with the paleo-records prevent or hinder statements about longer time scales and confidence. If I am correct, this needs stated somehow. [Ronald J Stouffer (Reviewer's comment ID #: 258-6)]	Rejected , issue covered in main text
6-191	A	4:23	:30	Insert "Based on proxy data from 26 locations," before the sentence that starts "It is also.." Figure 6-11(a) shows that the conclusion that it is likely that the second half of the 20th century was the warmest period in the NH in past 1000 years is based on extremely limited information, with proxy data from only 26 sites. This information should be included with the conclusion as an indication of its basis. Limited geographic coverage of proxy sites is noted in the text (pages 29, 32) and needs to be noted in the executive summary. Authors should double check the number of sites and include the tally.	Rejected , issue covered in main text, which the authors believe is sufficient

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				[Govt. of United States of America (Reviewer's comment ID #: 2023-365)]	
6-192	A	4:24	4:24	Replace from "has provided …. To "very likely" by "shows" 337 6-337 713 [VINCENT GRAY (Reviewer's comment ID #: 88-365)]	Rejected , no basis given for assertion
6-193	A	4:26	4:28	Replace from."It is also" on line 28 to "last 1300 years" on l;ine 28 with " This is, of course, the results of a concentration of measuring equipment to the vicinity of human activity" [VINCENT GRAY (Reviewer's comment ID #: 88-714)]	Rejected , no basis given for assertion
6-194	A	4:26	4:27	It seems an conspicuous omission here not to explicitly acknowledge that this was precisely the level of confidence ("likely" rather than "very likely") that was attributed to this conclusion in the TAR. To prevent the possibility that there be some confusion about the matter, it needs to be explicitly mentioned that the AR4 conclusions are in agreement with those of the TAR on this point. In fact, it should be noted that the conclusion here is stronger than that of the TAR, because the conclusion is being made for the past 1300 years, not just the past 1000 years. [Michael Mann (Reviewer's comment ID #: 156-30)]	Rejected, reference to tar 6k as written
6-195	A	4:26	4:27	The use of "1300 years" here is odd and not justified. Current reconstructions extending back 2000 years (Moberg et al, Mann and Jones) find that late 20th century Northern Hemisphere warmth is likely unprecedented in at least 2000 years. It is therefore "2000" years that should be used here, rather than "1300 years". [Michael Mann (Reviewer's comment ID #: 156-31)]	Rejected, number of records drop off beyond 1300 BP
6-196	A	4:26		For the sentence starting on page 4, line 26, change to read: "It is also likely that in the Northern Hemisphere this was the warmest 50-year period in the past 1000 years and the warmest 100-year period in the past 1300 years." And delete the following sentence "The regional extent … during the last 1300 years". [Govt. of United States of America (Reviewer's comment ID #: 2023-367)]	Rejected, statement supported by literature (e.g. Osborn and Briffa 2006).
6-1119	B	4:26		I disagree that it is "likely" and suggest that you use "likely as not". The conclusion depends on several problematic assumptions and cannot be given that high a confidence statement. These studies are extremely non-independent and the validity of their [Stephen McIntyre (Reviewer's comment ID #: 309-15)]	Rejected, conclusison supported by post TAR published litterature. See also definition of likely in IPCC usage
6-197	A	4:27	4:28	It is quite unclear how the conclusions regarding the spatial extent of warmth are any stronger than those regarding the magnitude of warmth. Both conclusions are based in large part on the same (mostly tree-ring) climate proxy data, and the limitations due to potential loss of low-frequency variability in these data would seem to have equal impact on either conclusion. If the increased information since the TAR allows one of these conclusions to be elevated to the "very likely" category, it elevates both conclusions to that category. However, it would be appropriately conservative to keep both in the "likely" category.	Accepted

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				[Michael Mann (Reviewer's comment ID #: 156-13)]	
6-198	A	4:28	4:30	The basis for this statement is unclear. The reasons for existing uncertainties have as much to do with possible limitations in the retention of low-frequency variability by certain proxies (e.g. tree-rings) as they have to do with limitations the available spatial network of proxy information. If the low-frequency information in proxies such as corals and ice cores--which give us information outside the extratropical land areas and during seasons other than summer---is more reliable that the low-frequency information in proxies such as tree-rings--which are indeed more plentiful, but confined largely to the extratropical land areas, and providing information limited to growing season conditions which in many cases relate to summer temperature---then it is possible that we have better low-frequency information from the regions outside the continental centers, and during seasons other than summer. It is impossible to reject this possibility based on our current understanding, and thus the statement in question as it currently stands is not entirely supportable. [Michael Mann (Reviewer's comment ID #: 156-14)]	Rejected, our current undertanding the the seinsitivity and spatial distribution of existing proxies suggests that the text is most adequate to the best of our understanding
6-199	A	4:29	4:29	Replace "most robust" by "more believeable" [VINCENT GRAY (Reviewer's comment ID #: 88-715)]	Rejected , no basis given for assertion
6-200	A	4:29	:30	Suggest text in page 4, lines 29-30, be changed to read "These conclusions are most robust for summer in extra-tropical land areas and for more recent periods because of the uneven spatial and temporal coverage, and varied characteristics, of the different proxy data." [Govt. of United States of America (Reviewer's comment ID #: 2023-366)]	Noted, text has been changed
6-201	A	4:30	4:30	The final portion of the sentence could read: ...robust in the summer over extra-tropical land areas. [Gregory Wiles (Reviewer's comment ID #: 289-4)]	Noted, text has been changed
6-202	A	4:33	4:34	Delete from ".that" on line 33 to "context" on line 34 [VINCENT GRAY (Reviewer's comment ID #: 88-716)]	Rejected , no basis given for assertion
6-203	A	4:33		likely as not? UGH! YOU ARE GOING TO GET KILLED OVER THIS TERM! How about stating it something like - IT CANNOT BE DEMONSTRATED WITH ANY DEGREE OF CONFIDENCE THAT PAST WARM PERIODS WERE COMPARABLE TO OR GREATER THAN THE LATE TWENTIETH CENTURY. I don't iinsist on this phrasing, but save yourself a lot of grief and choose something - else those 11 letters are going to get you into trouble! [Thomas Crowley (Reviewer's comment ID #: 51-20)]	Taken into acount, text rewritten
6-204	A	4:34	4:34	Insert "southern hemisphere" between "More" and "paleoclimatic". [Govt. of Australia (Reviewer's comment ID #: 2001-308)]	Taken into acount, text rewritten
6-205	A	4:34	4:34	Change "more paleoclimatic" to "more SH paleoclimatic".	Taken into acount, text rewritten

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				[Ronald J Stouffer (Reviewer's comment ID #: 258-7)]	
6-206	A	4:37	4:37	Replace "are" with "can be adjusted to be" [VINCENT GRAY (Reviewer's comment ID #: 88-717)]	Rejected , no basis given for assertion
6-207	A	4:37	4:37	Paleoclimate simulations For clarity, should this refer to model simulations? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-28)]	Accepted
6-208	A	4:38	4:39	Delete both lines. It is untrue. The rise can be explained by the biased sample, because most measuring equipment is situated near human activities, See Gray, 2000 "The Cause of Glogal Warming", Energy and Environment Vol 11 pages 629; McKJitrick and Michaels 2004 "A test of corrections for extraneous signals in gridded surface temperature data". Cliumate Research Vol26 pages 159-173" 342 6-342 718 [VINCENT GRAY (Reviewer's comment ID #: 88-28)]	Rejected, not supported by litterature.
6-209	A	4:39	4:39	As the attribution of causes of climate change is important in the context of the scientific basis of climate change the relevant text in section 6.6.3.4 should be inserted into the executive summary. Therefore please add after "forcings.": "It is (very?) likely that the contribution of natural forcing to observed 20th century warming is small and the solar and volcanic forcings are not responsible for the degree of warmth that occured in the second half of the 20th century." [Govt. of Germany (Reviewer's comment ID #: 2011-19)]	Rejected, belongs in Chapter 9
6-210	A	4:40	4:41	While changes in Asian monsoon strength are a plausible explanation to account for the proxy data, other explanations are possible. Rather subtle shifts in the location of convergence zones can give strong signals in the few paleo-proxies (e.g., speleothems) used to infer monsoon strength, without necessarily having implications for total monsoon strength. [Michael Mann (Reviewer's comment ID #: 156-60)]	Taken into account, text rewritten
6-211	A	4:41	3:46	This paragraph is difficult to understand [Pascale BRACONNOT (Reviewer's comment ID #: 29-5)]	Taken into account, text rewritten
6-212	A	4:41	4:46	We agree fully that IPCC should give an assessment on the magnitude of temperature variation during the last millenium. The text as it stands is very diffficult to understand. Please simplify. Proposal: "It is (very?) likely, that the amplitude of the northern hemisphere temperature variation during the last millenium do not exceed 1 C." [Govt. of Germany (Reviewer's comment ID #: 2011-20)]	Accepted, text is revised
6-213	A	4:42	4:42	Change "last millennium" to "last two millennia". [Govt. of Australia (Reviewer's comment ID #: 2001-309)]	Rejected, we do not have enough data to contsrain this with confidence prior to the last millennium1000
6-214	A	4:44	4:44	Replace "are broadly consistent with" by "show little relation to"	Rejected , no basis given for assertion

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				[VINCENT GRAY (Reviewer's comment ID #: 88-719)]	
6-215	A	4:45		Remove "the" before "Chapter 10". [Eric Wolff (Reviewer's comment ID #: 292-8)]	Accepted, text rewritten
6-216	A	4:48	4:49	Is it possible to be more precise in this sentence and provide an order of magnitude? [Pascale BRACONNOT (Reviewer's comment ID #: 29-6)]	Accepted
6-217	A	4:48		see orior comments about statistically significant differences in enso properties [Thomas Crowley (Reviewer's comment ID #: 51-21)]	Accepted
6-218	A	4:52	4:52	Replace "It is likely" with "There is evidence" [VINCENT GRAY (Reviewer's comment ID #: 88-720)]	Rejected , no basis given for assertion
6-219	A	4:52	4:52	How has the Asian monsoon changed? More/less precipitation? Shifted in space or time? What? [Ronald J Stouffer (Reviewer's comment ID #: 258-8)]	Accepted
6-220	A	4:53	4:53	late Holocene : last 2000 years? Or last 4000 years. The time periode considered should be more precise [Pascale BRACONNOT (Reviewer's comment ID #: 29-7)]	Accepted
6-221	A	4:54	4:54	"captured by climate models" Is it responsible to expect climate models to capture the shift? Is the shift forced or natural variability? If natural variability, why would one expect climate models to capture the shift? [Ronald J Stouffer (Reviewer's comment ID #: 258-9)]	Noted, text will be revised
6-222	A	5:2	5:3	Please specify the text "under a wide range of climate forcing." What caused recent droughts in Africa and N-America? Please give a quantified likelihood for the statement as proposed in the guidance notes for lead authors of the AR4 on Addressing uncertainties (IPCC, July 2005). [Govt. of Germany (Reviewer's comment ID #: 2011-21)]	Accepted – text revised
6-223	A	5:7	5:7	Substitute "can react" for reacts. [Gregory Wiles (Reviewer's comment ID #: 289-5)]	Noted - Sentence now changed.
6-224	A	5:8	5:10	The statement seems overly confident, and requires some degree of qualification. [Michael Mann (Reviewer's comment ID #: 156-61)]	ACCEPT - Paragraph now more humble.
6-225	A	5:9	5:11	Delete from "It is likely" on line 9 to "feedbacks" on line 11. There is no evidence for this [VINCENT GRAY (Reviewer's comment ID #: 88-721)]	TAKEN INTO ACCOUNT - the uncertainty is now expressed.
6-226	A	5:9	5:9	"likely" I believe the total feedback is assessed to be "very likely" positive in other chapters. Need checked for consistency. [Ronald J Stouffer (Reviewer's comment ID #: 258-10)]	REJECT - sentence now removed.
6-227	A	5:9	5:9	It seems that "ocean - atmosphere circulation" should be in the list of feedbacks here. [Gregory Wiles (Reviewer's comment ID #: 289-6)]	ACCEPT - Ocean circulation now included.
6-228	A	5:13	5:13	Replace "paleoenvironmental data indicate that vegetation composition and structure are	Rejected, current wording better

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				..." by " paleoenvironmental and modern environmental data indicate that floral and faunal composition, structure and distribution are ..." [Govt. of France (Reviewer's comment ID #: 2010-49)]	
6-229	A	5:13	5:14	Please add at which rate / magnitude of climate change those fast vegetation changes did occure. [Govt. of Germany (Reviewer's comment ID #: 2011-22)]	Rejected, difficult to give accurate numberss, many exampels with different climatic changes
6-230	A	5:13	5:13	Delete "very likely" [VINCENT GRAY (Reviewer's comment ID #: 88-722)]	Rejected , no basis given for assertion
6-231	A	5:13	5:14	I am unaware of any data showing "changes in vegetation composition and structure" in YEARS. This is a vast overstatement of the evidence for the impacts of Dansgaard-Oeschger events. [Eric Steig (Reviewer's comment ID #: 252-10)]	Accepted, there are, however, changes in years
6-232	A	5:14	5:14	Replace "climate change" with "changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-723)]	Rejected , no basis given for assertion
6-233	A	5:14	5:14	Delete "to climate change" [VINCENT GRAY (Reviewer's comment ID #: 88-724)]	Rejected , no basis given for assertion
6-234	A	5:16	5:18	The sentence is very complicated and remains unclear. Please simplify. Proposal: "During the last glacial period the deposition of wind-born iron into the southern ocean altered millenial scale changes in atmospheric CO2 by less than 25 ppm." [Govt. of Germany (Reviewer's comment ID #: 2011-23)]	Accepted
6-235	A	5:16	5:16	Delete "It is virtual;ly certain that" [VINCENT GRAY (Reviewer's comment ID #: 88-725)]	Rejected , no basis given for assertion
6-236	A	5:16	5:17	While the summary mentions a pCO2 change of up to 25 ppm, the main text gives an amplitudes of 20 ppm (p. 18, line 45; p. 19, line 38) This should be clarified [Michael Schulz (Reviewer's comment ID #: 229-3)]	Accepted
6-237	A	5:17	5:17	Insert after "were" "probably" [VINCENT GRAY (Reviewer's comment ID #: 88-726)]	Rejected , no basis given for assertion
6-238	A	5:19	5:19	Delete "consistent with model results" They are not "consistent. 351 6-351 727 [VINCENT GRAY (Reviewer's comment ID #: 88-726)]	Rejected , no basis given for assertion
6-239	A	5:19	5:20	I think there is a contradiction between the statement "limited role of these processes" and the following bullet. It is likely that the the processes (in a dynamical sense) are responsible for the glacial-interglacial pCO2 variations, mentioned in line 22-23. Hence, the processes by themselves may be not the limiting factor.	Taken into account - text reworded

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				[Michael Schulz (Reviewer's comment ID #: 229-4)]	
6-240	A	5:20		change "climate" to "global climate" [Raimund Muscheler (Reviewer's comment ID #: 185-2)]	Accepted
6-241	A	5:22	5:23	Emphasize that SH oceans were the dominant influence in that change. [Govt. of Australia (Reviewer's comment ID #: 2001-310)]	Accepted
6-242	A	5:22	5:22	Delete "It is very likely that" [VINCENT GRAY (Reviewer's comment ID #: 88-728)]	Rejected , no basis given for assertion
6-243	A	5:22	5:22	Replace "were primarily" with "could have been" [VINCENT GRAY (Reviewer's comment ID #: 88-729)]	Rejected , no basis given for assertion
6-244	A	5:22	5:23	this should say "virtually certain", not "very likely" [Eric Steig (Reviewer's comment ID #: 252-11)]	Rejected, we lack detailed explanation, although
6-245	A	5:25	5:25	Again, we should not let people think that models are perfect. This could be done by : Current models are capable of simulating the major features ? Large scale features? Broad features? Of climate [Pascale BRACONNOT (Reviewer's comment ID #: 29-8)]	TAKEN INTO ACCOUNT - the use of 'in association with large climate forcings' is an attempt in this direction.
6-246	A	5:25	5:29	Delete this whole paragraoh. I just do not believe that this is true. You cannot possibly simulate the major natural inflkuences such as contnental drift, solar changes, volcanic eriptions and ocean circulation changes, whose effects are largely unknown [VINCENT GRAY (Reviewer's comment ID #: 88-730)]	REJECT - That is not the focus of this paragraph.
6-247	A	5:28	5:28	Clarify what is meant by "major unexpected feedback". [Michel Crucifix (Reviewer's comment ID #: 52-15)]	REJECT - due to change in paragraph eliminating the phrase
6-248	A	5:28	5:28	I would strike the last statement "that major unexpected feedbacks are very unlikely to occur...". The concept that models cannot adequately mimic abrupt changes seems to contradict this statement. [Gregory Wiles (Reviewer's comment ID #: 289-7)]	REJECT - due to change in paragraph eliminating the phrase
6-249	A	6:0	27:	Monsoon, a result of strong land-ocean-atmosphere interaction, has significant impact on global climate. The rise of the Tibetan Plateau has established, or more exactly, much strengthened the Asian Monsoon. I believe a more detailed discussion of paleo-monsoon is appropriate in this chapter. 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, or much strengthening, 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	Noted, considered in revision

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				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 (Reviewer's comment ID #: 260-2)]	
6-250	A	6:15	6:25	The insertion of discussions of "policy" here is awkward and inappropriate. It is unclear why discussions of the recent pre-industrial past (i.e., the past 1000 years) are any more or any less relevant to policy than the icehouse or greenhouse climates of the more distant past. It may be true that discussions of climate of the past 2000 years have been *politicized* due to the prominence recent developments in this area were given in the TAR, it is not the case that the subject matter is intrinsically more policy-relevant than any other paleoclimate topics that give us insight in possible furture climate change. [Michael Mann (Reviewer's comment ID #: 156-15)]	Accepted, text changed
6-251	A	6:16	6:17	I would strike "for a number of reasons, but" [Gregory Wiles (Reviewer's comment ID #: 289-8)]	Acepted
6-252	A	6:22	7:23	"analytical and additionnal uncertaincies". Better explain what is meant by these important concepts, and this would be useful not only for dating. "Additionnal uncertaincies" are introduced by the hypothesis related to the data interpretation process (some are not explicitly formulated). This is sometimes called the "structural uncertainty" which it is, due to its nature, difficult to estimate. The idea can also be approached by the notions of "accuracy" and "precision". The analytical uncertainty measures the "precision" (is the measure reproducible). The accuracy (is the measure actually right) can be estimated by comparing the results of different methods. [Michel Crucifix (Reviewer's comment ID #: 52-17)]	Rejected, space limitations, topic brought in in the relevant sub-chapters
6-253	A	6:23	6:24	The statement "2000 years is of great relevance to policy making": the implication of this comment is that events prior to this period are not relevant in the climate debate, which is not the case as shown in this chapter. [Govt. of Spain (Reviewer's comment ID #: 2019-108)]	Acccepted
6-254	A	6:23	:24	It might be worth adding the reason that for the last 2000 years being relevant to policymaking here. Delete sentences from 21-24 "We also … policy making." And replace with "Much of the chapter focuses on the last 2000 years because of the quality and quantity of high-resolution proxy records and similarity to modern boundary conditions makes this period most relevant to climate change policy and decisionmaking." [Govt. of United States of America (Reviewer's comment ID #: 2023-368)]	Acccepted
6-255	A	6:23	:24	It might be worth adding the reason that for the last 2000 years being relevant to policy making here [Connie Woodhouse (Reviewer's comment ID #: 293-1)]	Noted, sentence taken out
6-256	A	6:24	:25	This sentence should be moved to page 6, line 6.	Rejected, text flows well as it is

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				[Govt. of United States of America (Reviewer's comment ID #: 2023-369)]	
6-257	A	6:28	6:28	Delete "state of the art" [VINCENT GRAY (Reviewer's comment ID #: 88-732)]	Rejected, no reason given for suggestion
6-258	A	6:44	6:45	I would strike the two "of this chapter" in the two lines [Gregory Wiles (Reviewer's comment ID #: 289-9)]	Accepted
6-259	A	6:45	7:36	Suggesstion for shortening: Readers are referred to books at three locations. Thus on page 7 sentences in line 12-13 and in line 33-34 can be omitted or shortened. [Eva Bauer (Reviewer's comment ID #: 15-2)]	Accepted
6-260	A	6:49		It is unclear the purpose of the text in this section. Only three forcings are addressed and the message given is misleading. [Govt. of Spain (Reviewer's comment ID #: 2019-109)]	Rejected, purpose is to draw attention to the following sections for detail
6-261	A	6:50	6:52	Suggestion for clarity: Substitute "Time series of astronomically driven insolation change" by "Time series of astronomically driven insolation" and substitute "past solar and volcanic forcing" by "past solar activity and volcanic forcing". [Eva Bauer (Reviewer's comment ID #: 15-3)]	Rejected, text ok as it is
6-262	A	6:51	6:51	Insert after "mechanics" "but possible feedbacks have been little explored" [VINCENT GRAY (Reviewer's comment ID #: 88-733)]	Rejected, comment not addressing the theme of the section
6-263	A	6:56	6:56	Delete "air trapped in". [Govt. of Australia (Reviewer's comment ID #: 2001-311)]	Rejected, more presise as it is
6-264	A	6:56	6:56	aerosol records are obtained from the ice matrix, not from the air in the bubbles. [ian Enting (Reviewer's comment ID #: 63-11)]	Accepted
6-265	A	6:57	6:57	Insert after "ice" "Unfortunately, the very poor sample distribution means we have little knowledge of truly global concentrations" [VINCENT GRAY (Reviewer's comment ID #: 88-734)]	Rejected, this is not the case, trace gases are well mixed
6-266	A	7:2	7:2	Insert after "sampling" "but these also suffer from bias, as they are mainly measured over the sea whereas the paleo samples are on land" [VINCENT GRAY (Reviewer's comment ID #: 88-735)]	Rejected, not relevant
6-267	A	7:3	7:3	"aacuracy": From a statistical point of view, "precision" would be more appropriate. [Michael Schulz (Reviewer's comment ID #: 229-5)]	Accepted
6-268	A	7:5	7:7	Delete from "This potentially" on line 5 to "understood" on line 7 This is unnecessary guff [VINCENT GRAY (Reviewer's comment ID #: 88-736)]	Rejected, text ok as it is
6-269	A	7:11		The writing style of this section could be improved substantially. Dating resolution, accuracy and precision of a sample varies as a function of the time interval to which the sample belongs to. I suggest that the section is rewritten describing the above parameters for specific time intervals. [Govt. of Spain (Reviewer's comment ID #: 2019-111)]	Rejected, space limittaions, more detail in various sub-chapters

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6-270	A	7:13	7:14	Regarding the statement: "In general, time control gets weaker farther back in time."This suggests that there is a gradual decrease in dating reliability, which is not the case. In fact once we cross the time span on which a method is applicable, the reliability in dating a sample decreases sharply. [Govt. of Spain (Reviewer's comment ID #: 2019-112)]	Acccepted
6-271	A	7:14	7:14	"Controls get weaker" : be more specific and quantitative. E.g. : ice layer counting is associated wth an uncertainty of at least 5 % (J. Southon,A radiocarbon perspective on Greenland ice-core chronologies: Can we use ice cores for C-14 calibration?, Radiocarbon 46 (3) : 1239-1259 (2004))). "Time control" is not a well defined phrase. [Michel Crucifix (Reviewer's comment ID #: 52-16)]	Accepted
6-272	A	7:14	7:14	Suggested change: substitute "chronological control is weaker" for "time control gets weaker.." [Gregory Wiles (Reviewer's comment ID #: 289-10)]	Noted, text changed
6-273	A	7:14		"Tree-ring records are generally the best": This sentence seems incomplete, and as it stands it is misleading. Tree-ring records are the best for what?, and why generally. Clearly they are useless to date deep sea sediments. [Govt. of Spain (Reviewer's comment ID #: 2019-113)]	Accepted
6-274	A	7:14		Insert text to read "time control gets weaker farther back in time, making it difficult to address issues of leads, lags, and synchroneity that are critical to evaluate and understand climate processes." [Govt. of United States of America (Reviewer's comment ID #: 2023-370)]	Accepted
6-275	A	7:15	7:16	Change proxies for archives or climatic archives. Corals and ice cores are archives of past conditions where different proxies (isotopes, trace metals) can be measured and related to environmentals parameters (temperature, salinity,...) [Eva Calvo (Reviewer's comment ID #: 37-1)]	Rejected, not necessary to detail
6-276	A	7:15	7:15	Insert after "years)" "but only, of course, for summer, and for constant presumed precipitation and nutrients" [VINCENT GRAY (Reviewer's comment ID #: 88-737)]	Rejected, no basis given for sugegstion
6-277	A	7:17	7:17	Replace "not always" by "rarely" [VINCENT GRAY (Reviewer's comment ID #: 88-738)]	Rejected, no basis given for sugegstion
6-278	A	7:17	7:18	Delete from "Again" on line 17 to "uncertanty" on line 18 [VINCENT GRAY (Reviewer's comment ID #: 88-739)]	Rejected, no basis given for sugegstion
6-279	A	7:18	7:19	RATHER THAN "most paleoclimatic interpretations must take into account uncertainties in time control" ONE SHOULD SAY "ALL paleoclimatic interpretations…" OR JUST DELETE "MOST". [Govt. of Spain (Reviewer's comment ID #: 2019-114)]	Accepted

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6-280	A	7:25		change "15,000" to "12,000" - there is good tree-ring data for the last approx. 12,000 years. Before 12,000 yr BP there are significant uncertainties [Raimund Muscheler (Reviewer's comment ID #: 185-3)]	
6-281	A	7:28	7:28	Remove the word "specific" [Gregory Wiles (Reviewer's comment ID #: 289-11)]	Accepted
6-282	A	7:32	8:9	Could this information be better summarised in a table? Here it is already taking the form of a list [Mark Siddall (Reviewer's comment ID #: 238-4)]	This has been evaluated, but the authors conclude that a list would take too much space and not much is gained
6-283	A	7:32		THIS SECTIONS DOES NOT ADDRESS THE QUESTION. NOTHER IS SAID ABOUT HOW PAST CLIMATE DYNAMICS ARE STUDIED, AND THEIR EFICACY APPRAISED, AND INSTEAD THERE IS A HALF COOKED DESCRIPTION OF PROXIES. [Govt. of Spain (Reviewer's comment ID #: 2019-119)]	Accepted, title changed
6-284	A	7:32		This sectoin never answers the question set out in the title: how well can we reconstruct past climate dynamics. [Eric Steig (Reviewer's comment ID #: 252-13)]	Accepted, title changed
6-285	A	7:42	7:42	proxy grew or existed". A proxy cannot grow (only the underlying signal carrier, if biol. may grow). rephrase to "proxy was formed [Michael Schulz (Reviewer's comment ID #: 229-6)]	Accepted
6-286	A	7:43	7:44	THE STATEMENT IS UNCLEAR: "specific observations, logs, harvest data for reconstructions of past climates." [Govt. of Spain (Reviewer's comment ID #: 2019-115)]	Accepted
6-287	A	7:44		Should read "harvest data, for reconstructions of past climate." [Govt. of United States of America (Reviewer's comment ID #: 2023-371)]	Accepted
6-288	A	7:45		DELETE "biological" and "other organisms" [Govt. of Spain (Reviewer's comment ID #: 2019-116)]	Accepted
6-1120	B	7:49		You need to mention that serious statistical questions have been raised about these calibration procedures from a statistical point of view. [Stephen McIntyre (Reviewer's comment ID #: 309-16)]	Rejected, this is detailed in Ch. 6.6
6-289	A	7:50	:51	"Networks of tree-ring width and tree-ring density are used to infer past temperature changes…" Not only temperature, but moisture-related variables as well! [Govt. of United States of America (Reviewer's comment ID #: 2023-372)]	Accepted
6-290	A	7:50	:51	"Networks of tree-ring width and tree-ring density are used to infer past temperature changes…" Not only temperature, but moisture-related variables as well! [Connie Woodhouse (Reviewer's comment ID #: 293-2)]	Acccepted
6-291	A	7:52		RELIABLE QUANTITAVE SALINITY OR PRECIPITATION PROXIES DO NOT	Acccepted

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				EXIST, AND IT IS MISLEADING TO REFER TO ANY IN THE SAME LEVEL AS TEMPERATURE PROXIES, WHICH ARE FAR MORE CONSTRAINT. [Govt. of Spain (Reviewer's comment ID #: 2019-117)]	
6-292	A	7:53		Should read "functions that are calibrated" [Govt. of United States of America (Reviewer's comment ID #: 2023-373)]	Acccepted
6-293	A	7:57		RELIABLE QUANTITAVE SALINITY OR PRECIPITATION PROXIES DO NOT EXIST, AND IT IS MISLEADING TO REFER TO ANY IN THE SAME LEVEL AS TEMPERATURE PROXIES, WHICH ARE FAR MORE CONSTRAINT. [Govt. of Spain (Reviewer's comment ID #: 2019-118)]	Accepted
6-294	A	8:8	8:8	cf. comment 16. [Michel Crucifix (Reviewer's comment ID #: 52-18)]	Accepted
6-295	A	8:11	8:11	delete "Not suprisingly" at the beginning of this sentence. [Eric Steig (Reviewer's comment ID #: 252-14)]	Accepted
6-296	A	8:13	8:15	THE FOLLOWING SENTENCE IS UNCLEAR AND SHOULD BE REWRITTEN: "the most weight…inferences". [Govt. of Spain (Reviewer's comment ID #: 2019-120)]	Rejected, authors believe it is clear enough
6-1121	B	8:13		this claim is just a "puff" and should be deleted. [Stephen McIntyre (Reviewer's comment ID #: 309-17)]	Noted, text changed somewhat
6-297	A	8:19	8:28	It would be valuable to note that models let us explore amplitudes of variability that are unavailable from the historical record. [Govt. of Australia (Reviewer's comment ID #: 2001-312)]	No material added for lack of space - we feel it is already abundantly clear that some paleoclimatic variations are much larger in amplitude than those of the historic era.
6-298	A	8:21	8:21	"Milankovitch theory". Use, more generally, "astronomical theories of palaeoclimates". Note that Milankovitch tested his theory quantitatively, of course with a very simplified climate model. [Michel Crucifix (Reviewer's comment ID #: 52-19)]	Noted - matter of taste whether to give one specific example (which we did here) or a whole class of theories as example
6-299	A	8:21	8:21	Numerical or quantitative models also check hypotheses for consistency - the narrative or word models used by paleoclimatologists are not always self-consistent or consistent with physics [Mark Siddall (Reviewer's comment ID #: 238-5)]	This comment seems to reinforce what we are saying
6-300	A	8:21		sentence needs reordering: "to test physical hypotheses, such as the Milankovich theory (Box 6.1) quantitatively." [Eric Wolff (Reviewer's comment ID #: 292-9)]	Accepted.
6-301	A	8:27	8:28	Change wording of "there are no direct analogues of the future in the past" to "there may be no direct analogues of the present or future in the past"	

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6-302	A	8:34	8:34	Replace "important" with "vital" [VINCENT GRAY (Reviewer's comment ID #: 88-740)]	rejected - no reason is provided for the suggested change
6-302	A	8:34	8:34	Replace "important" with "vital" [VINCENT GRAY (Reviewer's comment ID #: 88-740)]	rejected - no reason is provided for the suggested change
6-303	A	8:37	8:37	I don't like the word empirical, when in fact the parameterizations are based on physical processes, eventhough idealised or highly simplified. Suppress this word it adds confusion on what is a climate model. [Pascale BRACONNOT (Reviewer's comment ID #: 29-9)]	accepted
6-304	A	8:40	8:41	I don't think this is an accurate statement. Paleoclimate data MAY EVENTUALLY PROVE USEFUL in evaluating the ability of climate models to simulate realistic climate change. To date, though, the most believable validations have been done with modern climate data, not paleoclimate data. [Eric Steig (Reviewer's comment ID #: 252-12)]	Rejected - we feel our statement is accurate.
6-305	A	8:41	8:41	Replacw "anthropogenic" with "greenhouse gas" [VINCENT GRAY (Reviewer's comment ID #: 88-1046)]	rejected - no reason is provided for the suggested change
6-306	A	8:44		Greenhouse gas concentrations are not an external forcing, but internal to the climate system." Delete sentence in 44-46 and end sentence in 43-44 by adding "using differences in proscribed forcing and configuration of oceans and continents. [Govt. of United States of America (Reviewer's comment ID #: 2023-374)]	accepted
6-307	A	8:48	8:48	The PETM may not be nearly as 'rapid' as the current GHG increase [Michael Mann (Reviewer's comment ID #: 156-63)]	cannot locate what this comment refers to
6-308	A	8:49	8:49	add ' and simple' after 'fast' [Mark Siddall (Reviewer's comment ID #: 238-6)]	rejected - "simple" is not correct, often coarse-resolution GCMs are used
6-309	A	8:49	8:49	at least 15-25 m' cannot be correct - it should be 'at least 15 m' or the 'at least' does not make sense [Mark Siddall (Reviewer's comment ID #: 238-8)]	accepted
6-310	A	8:52	8:52	This is a case for which a ealier work could also be cited. Isotope modeling is not new. [Pascale BRACONNOT (Reviewer's comment ID #: 29-10)]	rejceted for lack of space - we were asked to reduce the # of refs
6-311	A	8:52	8:53	Vegetation, as well as terrestrial and marine ecosystem, modules are increasingly included…" isn't it rather: "Vegetation modules, as well as terrestrial and marine ecosystem, are increasingly included… [Myriam Khodri (Reviewer's comment ID #: 126-2)]	accepted
6-312	A	8:55	8:55	Explain the distinction between "offline" and "online" diagnosis. See Prentice and	rejected for lack of space - this would

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				Harrison, 2003 (cited) for an off-line example, and Crucifix, M. and Betts, R. A. and Hewitt, C. D. Pre-industrial-potential and Last Glacial Maximum global vegetation simulated with a coupled climate-biosphere model: Diagnosis of bioclimatic relationships Global and Planetary Change 2005 45 4 295-312, 10.1016/j.gloplach.20, for an on-line example. [Michel Crucifix (Reviewer's comment ID #: 52-20)]	get too technical
6-313	A	8:55	8:55	add 'and isotope' after 'biogeochemical' to allow for 14C, 13C, Pa/Th modelling which is being done and improved [Mark Siddall (Reviewer's comment ID #: 238-7)]	rejected - this is already said in line 52
6-314	A	9:1	9:1	"proxy data from a variety of archives". This is confusing and not well defined. Suggestion : use "palae-environmental records" (e.g. pollen spectra are directly obtained by pollen fossil counting. They are not a "proxy" in this particular case.) [Michel Crucifix (Reviewer's comment ID #: 52-21)]	accepted
6-315	A	9:6	10:57	Sections not in chronological order. Reorder subsections 6.3.1 to 6.3.3 in sequential order. [Govt. of Australia (Reviewer's comment ID #: 2001-313)]	REJECT: The Pliocene represents an equilibrium climate, while the PETM represents a rapid climate change and hence comes second – the same ordering is present in the Quaternary climate section.
6-316	A	9:8	9:8	"Pre-Quaternary climates (prior to 3 Myr)" conflicts with "The Mid-Pliocene (ca. 3.3 to 3.0 Myr) as listed in line 41 on page 6-9.". Question is when is for the late Pliocene. Prior to 2.6 Myr may be appropriate for pre-Quaternary climates. [Govt. of China (Reviewer's comment ID #: 2006-51)]	ACCEPT
6-317	A	9:8	9:8	I would add "Some" in front of pre-quaternay. I think it is misleading to say that all pre-Quaternary" time is warmer than today. This is misleading since Figure 6.1 does not show an estimated temperature record through the deep past. 784 6-784 13 [Gregory Wiles (Reviewer's comment ID #: 289-51)]	TAKEN INTO ACCOUNT: We have moved the reference to Fig. 6.1 up next to the comment in the sentence to indicate which climates we are concerned with. The use of 'by and large' also indicates that not every pre-Quaternary climate can be expected to be warmer than today.
6-318	A	9:14	9:14	I would stike "ingenious" [Gregory Wiles (Reviewer's comment ID #: 289-14)]	ACCEPT: Replced with "on-going'.
6-319	A	9:19	9:20	In general, sentences read better if references are added at the end. [Eva Calvo (Reviewer's comment ID #: 37-2)]	ACCEPT
6-320	A	9:19	9:19	REGARDING THE USE OF ALKENONES TO RECONSTRUCT PCO2, THE PIONERING PAPER THAT DEMONSTRATED THE APPROACH IS BY Jasper, J. P.,	ACCEPT

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				and Hayes, J. M. (1990). A carbon isotope record of CO2 levels during the late Quaternary. Nature 347, 462-464 [Govt. of Spain (Reviewer's comment ID #: 2019-121)]	
6-321	A	9:20	9:21	The reference of Pearson et al. (2001) deals with SST during the Eocene. The correct reference for boron isotopes is Pearson and Palmer (2000), Nature, 406, 695. [Eva Calvo (Reviewer's comment ID #: 37-3)]	ACCEPT
6-322	A	9:22		The stomatal index has periods for which population-level data of extant species is lacking. The empirical relations between stomatal index and atmospheric CO2 concentrations are based on modern species and lots of measurements. Stomatal index tends to vary dramatically within an individual plant and across plants in the same populations. CO2 reconstructions based on a few leaves from an extinct species preserved in a few layers of sediments over millions of years is suspect at best. A complicating factor is that temperature and relative humidy cannot be held constant; these factors also affect stomatal densities (see one exception where attempt was made to hold constant in Van de Water, P.D., Leavitt, S.L., and Betancourt, J.L. 1994, Trends in stomatal density and 13C/12C ratios of Pinus flexilis needles during last glacial/interglacial cycle. Science 264, 239-243). [Govt. of United States of America (Reviewer's comment ID #: 2023-375)]	TAKEN INTO ACCOUNT: We agree that this, and the other, reconstruction methods all have numerous assumptions that can alter the results. That is why we have tried to show a range of reconstructions, so as to make it clear that confidence is not high in any one method. The wide-range of reconstructed results is specifically commented on in lines 24-25.
6-323	A	9:23	:24	THE SENTENCE DOES NOT MAKE SENSE: "magnitudes are generally higher than the interglacial, pre-industrial values seen in ice core data" [Govt. of Spain (Reviewer's comment ID #: 2019-122)]	REJECT: Hard to know what's hard to understand here.
6-324	A	9:25	9:25	"changes in tectonic processes". This is too restrictive. Suppress the word "changes" and/or mention that the long term trend in CO2 is the result of a balance between volcanic activity (production of CO2), silicate weathering and sedimentation. In addition, changes in ocean state may modulate this long term trend. [Michel Crucifix (Reviewer's comment ID #: 52-22)]	ACCEPTED: To a good extent.
6-325	A	9:26	9:26	Needs a paren. In the beginning of the line. [Gregory Wiles (Reviewer's comment ID #: 289-15)]	ACCEPT; Parentheses are now balanced.
6-326	A	9:26		Temperature reconstructions ARE ALSO DERIVED FROM OTHER PROXIS THAN OXYGEN ISOTOPES FOR PRE-QUATERNARY CLIMATES, SUCH AS Mg/Ca IN FORAMS AND ALKENONES [Govt. of Spain (Reviewer's comment ID #: 2019-123)]	ACCEPTED
6-327	A	9:26		Should read, "(e.g., emissions associated with periods of more intense volcanic activity and CO2 drawdown associated with silicate mineral weathering during major episodes of mountain building)." [Govt. of United States of America (Reviewer's comment ID #: 2023-376)]	ACCEPTED to a good extent.
6-328	A	9:27	9:27	Missing ")"	ACCEPTED

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				[James Crampton (Reviewer's comment ID #: 50-3)]	
6-329	A	9:29	:35	My own research (Gerhard and Harrison, 2001) inidcates that the closure of th eIsthmus of Panama and the the Tethyan Seaway is responsible for the initiation of glaciation owing to caused changes in oceanic circulation, and that CO2 or any greenhouse gas is not involved directly - such a major climate change in the earth demonstrated co-incident with tectonic changes is highly more probable than a speculative greenhouse change. [Lee Gerhard (Reviewer's comment ID #: 83-3)]	NOTED: The text indicates that CO2 reduction may be responsible, in part. Other contributions could be mentioned but timing (for example the exact date of the closing of the Isthmus relative to the initiation of the glaciation is problematic.
6-330	A	9:33	9:33	Srike the word "Periods" since the periods are not shown - perhaps show the Eras on the figure and refer to the Mesozoic Era here in the text. [Gregory Wiles (Reviewer's comment ID #: 289-16)]	ACCEPT: Text has been changed.
6-331	A	9:35	9:35	It is confusing to refer to glaciation at 300 million years and talk about surrounding Epochs - perhaps surrounding Periods would be better - but not these are not shown on the Figure 6.1. [Gregory Wiles (Reviewer's comment ID #: 289-17)]	ACCEPT: Text has been changed.
6-332	A	9:42	9:43	" Chandler et al., 1994" should be revised and reorganized into "Sloan et al, 1996; Haywood et al., 2000; Jiang et al., 2005". For, globally annual mean surface temperature differences between the middle Pliocene and the present are 1.4 C in the NH (Chandler et al., 1994), 3.6 C (Sloan et al., 1996), 1.9 C (Haywood et al., 2000), and 2.6 C (Jiang et al., 2005) on a global domain. The reference not list in chapter 6 included: 1) Dowsett, H., J. Barron, R. Poore, R. Thompson, T. Cronin, S. Ishman, and D. Willard, 1999: Middle Pliocene paleoenvironmental reconstruction: PRISM2. USGS Open file Report 99-535, http://pubs.usgs.gov/openfile/of99-535. 2)Jiang, D., H. J. Wang, Z. L. Ding, X. Lang, and H. Drange, 2005: Modeling the middle Pliocene climate with a global atmospheric general circulation model. Journal of Geophysical Research, 110, D14107, doi:10.1029/2004JD005639. [Govt. of China (Reviewer's comment ID #: 2006-52)]	ACCEPT: Text altered and one reference added to the chapter.
6-333	A	9:42		"substantially warmer for a sustained period" (As discussed elsewhere the last interglacial is warmer) [Eric Wolff (Reviewer's comment ID #: 292-10)]	ACCEPT
6-334	A	9:43	9:43	Add at end "the more extreme" [VINCENT GRAY (Reviewer's comment ID #: 88-741)]	REJECT: The magnitudes indicated are in line with the majority of the models.
6-335	A	9:43		Sloan et al 96 should also be refd. [Thomas Crowley (Reviewer's comment ID #: 51-22)]	ACCEPT
6-336	A	9:44	9:44	Replace "will" with "could possibly" [VINCENT GRAY (Reviewer's comment ID #: 88-742)]	ACCEPT IN PART: 'will' is replaced with 'could'.

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6-337	A	9:47		You use ppm throughout the text, but ppmv on Figure 1. You need to decide on one or the other. [Eric Wolff (Reviewer's comment ID #: 292-11)]	NOTED: Figure probably should be altered.
6-338	A	9:49		I think a number of people would be uncomfortable with a 15-25 m sea level high - the latter number is an awful lot - but I can't say that I am tuned into the latest word on this, so maybe is ok. Good to doublecheck though. [Thomas Crowley (Reviewer's comment ID #: 51-23)]	REJECT: Was double-checked.
6-339	A	9:50	9:50	Strike "was much lower" and add "a lower continental aridity". [Gregory Wiles (Reviewer's comment ID #: 289-18)]	ACCEPT
6-340	A	9:53		FOR QUANTITATIVE MARINE SST RECONSTRUCTION CONSIDER CITING THE REFERENCE BELOW WHERE THERE IS COMPELLING EVIDENCE OF PLIOCENE SSTS BEING MUCH HIGHER THAN DURING LATE PLEISTOCENE OFF SOUTHWEST AFRICA: Marlow, J. R., Lange, C., Wefer, G., and Rosell-Melé, A. (2000). Upwelling intensification as part of the Pliocene-Pleistocene climate transition. Science 290, 2288-2291 [Govt. of Spain (Reviewer's comment ID #: 2019-124)]	NOTED: We are limiting the individual site references due to space limitations.
6-341	A	9:53	:54	Delete "Temperature reconstructions for this time period from". Should now read, "Both terrestrial and marine paleoclimate proxies (Thompson, 1991;	

341 A 9:53 :54 Delete “Temperature reconstructions for this time period from”. Should now read, “Both terrestrial and marine paleoclimate proxies (Thompson, 1991;

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6-342	A	10:3	10:3	In parens. "(or even slight cooling)" suggested change "(and even a slight cooling)" [Gregory Wiles (Reviewer’s comment ID #: 289-19)]	REJECT: Data precision not sufficient to distinguish slight coolng from zero.
6-343	A	10:4	10:4	“; Jiang et al. 2005 ” should be added after “Haywood et al., 2000”. Because Jiang et al. (2005) used the IAP AGCM to reproduce middle Pliocene global climate under the PRISM2 2ox2o data set (Dowsett et al., 1999), and corresponding model results are fully consistently with the contents. Additionally, “Jiang et al. (2005) revealed that the reconstructed vegetation have little influence on the middle Pliocene climate on a global domain” should be inserted before “In contrast,” because the above argument is helpful to understand the projected future warmer-than-today climate regime because vegetation feedback having been paid much attention to in the studies related to global warming at present. [Govt. of China (Reviewer’s comment ID #: 2006-53)]	ACCEPT (in part): The Jiang et al reference has been added; the additional discussion has not, due to space limitations, and it being somewhat tangential to the discussion at this point.
6-344	A	10:4	10:6	I am unsure about the usefulness of including a single, isolated, modelling result here apart from to indicate that more work is needed [Mark Siddall (Reviewer’s comment ID #: 238-9)]	TAKEN INTO ACCOUNT: Text now refers to the modeling results of chapter 10 in conjunction with higher CO2.
6-345	A	10:6	10:6	It is not clear from the text why the simulations of the mid-Pliocene give different tropical SSTs than those of the future climate (the prescribed CO2 is similar). This is information would be useful because it tells us on the reason why a past climate is never a "true" analog of the future climate. [Michel Crucifix (Reviewer’s comment ID #: 52-23)]	REJECT: Results are not dissimilar.
6-346	A	10:10	10:13	This relates to a general point about the density and consistency of citation. In some parts of the text citation is adequate, in other parts of the text it is not - here citation is deparately needed after the phrase '…in better agreement with GCM reconstructions from increased CO2 forcing.'. Which reconstructions? There are a great many cases of inadequate citation throughout the whole chapter - another example is below [Mark Siddall (Reviewer’s comment ID #: 238-10)]	ACCEPT: Reference is now given to the modeling results in Chapter 10, which also show tropical and subtropical warming.
6-347	A	10:10		SEE ALSO EARLIER REFENCE BY Marlow, J. R., Lange, C., Wefer, G., and Rosell-Melé, A. (2000). Upwelling intensification as part of the Pliocene-Pleistocene climate transition. Science 290, 2288-2291. [Govt. of Spain (Reviewer’s comment ID #: 2019-125)]	NOTED
6-348	A	10:12	10:12	Use GCM simulation rather than GCM reconstruction [Michel Crucifix (Reviewer’s comment ID #: 52-24)]	ACCEPT
6-349	A	10:14		Should read, “tropical temperature change without strong increases in ocean heat transport (Rind and Chandler, 1991).” [Govt. of United States of America (Reviewer’s comment ID #: 2023-378)]	ACCEPT
6-350	A	10:17	10:30	Note also the hypothesis based on polar stratospheric clouds (for example : Kirk-Davidoff et al., GRL 29 , 1556 (2002), but there may be more appropriate references) to explain high winter temperatures at that time.	NOTED

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				[Michel Crucifix (Reviewer's comment ID #: 52-26)]	
6-351	A	10:22	10:33	The argument here is weak. Whatever change in the THC that is seen for the Pliocene is likely to be an equilibrium response, and although you refer to the change during the 21st century as a transient response, you do not point out that those coupled atm-ocean models that have been run to equilibrium generally predict an increase in THC intensity in a warmer climate after a transient decrease (I could dig up some references if asked). Thus, the response during the Pliocene could be quite different from what the models project as the climate warms over the next century, and the models could still be correct. Comparison with the Pliocene is still a useful validation of coupled models, but it is not the projected change over the next cnetury that should be compared wiht the Pliocene. [Danny Harvey (Reviewer's comment ID #: 101-35)]	REJECT: The several models that have been run longer (and not to equilibrium) do not produce an increase in THC (see Chapter 10).
6-352	A	10:23	10:23	"Thermohaline increase" : be more specific : increase in the intensity of the meridional overturning cell. [Michel Crucifix (Reviewer's comment ID #: 52-25)]	ACCEPT
6-353	A	10:25	10:26	An increase would, however, contrast with the North Atlantic deep water production decreases that are found in several coupled model simulations for the 21st century.'. A statement like this must be supported with citation [Mark Siddall (Reviewer's comment ID #: 238-11)]	ACCEPT: Results from Chapter 10 are now referenced.
6-354	A	10:26	10:30	"The transient response in those models". The transient response of the ocean circulation may be very different to the equilibrium one, because it is determined by a density distribution that is not in equilibrium, thus not stable. 2-D simulations show numerous cases where the "transient" and "equilibrium" responses are opposite each other with respect to the initial state. [Michel Crucifix (Reviewer's comment ID #: 52-27)]	ACCEPT: Comment included.
6-355	A	10:26		you are creating an unnecessary problem for yourself - there are obvious reasons for these differences - one is an equilibrium response (pressumably), the other transient. Also hysteresis effects may apply as the climate stairstepped through the last three million years. I don't think the pliocene results place the future predictions in any kinds of jeopardy. they do however suggest that it got a lot warmer in the polar regions for the present co2 level that we have - transient, equilibriu, whatever. the ocean circulation cannot just spin up on its own - coriolis still steers things eastward at the same latitude. [Thomas Crowley (Reviewer's comment ID #: 51-24)]	ACCEPT: Most of these comments are now included, in one place or another.
6-356	A	10:34	10:55	in line 10, "an abrupt warming" is mentioned. Warming of what? I think this statement should refer to the deep ocean. However, then there is a conflict with line 55, where climatic warming is being referred to. Implicitly this would equate a deep ocean warming with a climatic warming. This may not necessarily be the case if the the deep-water formed a lower latitudes. It should be clarified which part(s) of the climate system	ACCEPT: The location of the warming and climate change influence is now indicated.

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				warmed. [Michael Schulz (Reviewer's comment ID #: 229-7)]	
6-357	A	10:34		SIGNIFICANT POINT - THIS IS ONE OF THE THINGS I IWAS COMPLAINING ABOUT IN THE BEGINNING. We have a learned discussion on the PETM and yet the writeup fails to point out that the ocean response to higher atmospheric carbon loading is almost exaactly as predicted by models for the future - toot your horn here andn include such successes, along with lgm and monsoon simulations as examples of how well the models behave under different boundary conditiosn! [Thomas Crowley (Reviewer's comment ID #: 51-25)]	REJECT: In fact, the response of models seems to underestimate the high latitude warming with respect to the recent Arctic observations.
6-358	A	10:37	10:37	"Carbon isotope excursion" : be more specific : 13C (which will be defined in the appendix) [Michel Crucifix (Reviewer's comment ID #: 52-28)]	REJECT: This is the more general comment; the 13C change, the specific response, is discussed subsequently.
6-359	A	10:38	10:38	"cloud parameters" and "turbulent mixing" are particularly bad examples if one wants to illustrate how the paleo-record can be used to constrain climate models. We have difficulties finding ways to use modern data to test cloud parameterizations and turbulent boundary layer schemes, with numerous measurement campaigns devoted to improve them. It is not clear that proxies will help, given that models are underdetermined given the data (and there are no 'cloud' or 'mixing' proxies). [Michael Mann (Reviewer's comment ID #: 156-62)]	REJECT: NOT IN THIS SECTION
6-360	A	10:54		Delete "excellent" [Govt. of United States of America (Reviewer's comment ID #: 2023-379)]	ACCEPT: Replaced by 'striking'.
6-361	A	11:6	11:6	delta D in EPICA ice core covers the last 740 kyr or so according to EPICA community members (2004). Ice core expected tocover almost 1Myr have been retrieved in Dome Fuji. Suggestion : … glacial-interglacial cycles covering at least the last 650,000 years... [Marie-France Loutre (Reviewer's comment ID #: 148-3)]	Accepted, text revised
6-362	A	11:11	11:12	Long glacial periods' the glacial periods are only considered long because the definition here is equivalent to anything outside the interglacial. The Glacial maxima are a similar length, or shorter than, the interglacial periods. The 'glacial period' as defined in the text therefore refers mainly to the transitory climate regimes between interglacial and glacial maximum - in this case it is not necessarily a very useful definition (the envelope of this definition is between -20 and -140 m sea-level equivalent ice volume). This is espcially the case if we want to make the distinction between the nature of glacial and interglacial - it implies immediately that the interglacials are shorter than the glacials. Infact the climate regimes occurring outside of the interglacials were often shorter lived than the typical interglacial durations. The authors may like to think carefully about how these definitions relate to the following sections on Abrupt Climate Changes. [Mark Siddall (Reviewer's comment ID #: 238-12)]	Taken into account

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6-363	A	11:12	11:12	'There is clear evidence for LONGER interglacial …" seems to be the correct message of the sentence. [Jesús Fidel González-Rouco (Reviewer's comment ID #: 86-1)]	accepted
6-364	A	11:12	11:12	The context seems to indicate that it should say 'There is clear evidence for LONGER interglacial …' [Govt. of Spain (Reviewer's comment ID #: 2019-42)]	accepted
6-365	A	11:13		THE STATEMENT "interglacial periods prior to 450,000 years, but these were apparently colder than the typical interglacials" IS INACCURATE. PRIOR TO THE MID PLEISTOCENE TRANSITION, INTERGLACIALS WERE WARMER THAN PRESENT ONES AS CAN BE INFERRED FROM ANY BENTHIC D18O RECORD SPANNING THE QUATERNARY. [Govt. of Spain (Reviewer's comment ID #: 2019-126)]	Taken into account. We refer now to the period between 430 and 740 ka, which is the period covered by the ice core record
6-366	A	11:14		HOLOCENE EPOCH, NOT PERIOD [Govt. of Spain (Reviewer's comment ID #: 2019-127)]	Accepted.
6-367	A	11:18	:32	You correctly note that CO2 rises only after temperature rises, and cite some of the pertinent literature. But then the statement drifts off to ignore that information and state that the CO2 rise is the cause of the rise. Can't have it both ways. The cited literature essentially falsifys the hypothesis. Better deal with it in a more straight forward manner.There's a problem with the theory, and it has to be faced. [Lee Gerhard (Reviewer's comment ID #: 83-4)]	Rejected. We clearly state that temperature (at least in Antarctica) increases before CO2. This is not surprising because CO2 increase should be forced by some climatic-related parameter under "natural" conditions. We don't state that CO2 or GG is the **initial** cause of temperature rise. We says that GG and mainly CO2 **feedbacks** were amplyfing largely the initial warming corresponding to the shifts from glacial to interglacial modes
6-368	A	11:18		The definitive paper on the correlation between temperature and CO2 in the last 100,000 plus years is Cuffey and Vimeux, 1999. This should be cited here, and elsewhere in this document. [Eric Steig (Reviewer's comment ID #: 252-15)]	Rejected. The paper by Cuffey and Vimeux is not a definitive one on the correlation between CO2 and temperature during glacial-interglacial cycles, but it is dealing with the specific problem of the phase relationship between these two properties during the onset of the last glaciation
6-369	A	11:20	11:20	Insert after " Antactic temperature". "but often anticipating it" [VINCENT GRAY (Reviewer's comment ID #: 88-743)]	Rejected. The ice core record of CO2 does'nt indicate that CO2 often anticipated the Antarctic temperature.

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6-370	A	11:20		DELETE: "CO2 variations over the last 420,000 years broadly followed Antarctic temperature, typically" AS IT REPEATS THE PREVIOUS SENTENCE. [Govt. of Spain (Reviewer's comment ID #: 2019-128)]	Rejected. This sentence doesn't repeat the previous one. It states the phase relationship between CO2 and temperature, when the previous one states the close link between the two properties.
6-371	A	11:21	11:21	"time lag": Taken literally, the statement that CO2 lags T contrasts the main thread of the chapter. This should be clarified by separating between inception and deglac. for which different lead-lag relationships seem to exist. A plausible explanation is offered in Q6.1 (p.68, line 31-37) [Michael Schulz (Reviewer's comment ID #: 229-8)]	Taken into account by deleting "time lag". In fact the lag relationship exists both for deglaciation and inception.
6-372	A	11:27	11:27	Missing word: add "latitudes" after "high northern" [Eva Bauer (Reviewer's comment ID #: 15-4)]	accepted
6-373	A	11:27	11:28	The text should maybe say:" linked with the rapid warming at high northern LATITUDES (Petit et al." [Jesús Fidel González-Rouco (Reviewer's comment ID #: 86-2)]	accepted
6-374	A	11:27	11:27	presumably 'high northern (Pet….' should be 'high northern latitudes (Pet..' [Mark Siddall (Reviewer's comment ID #: 238-13)]	accepted
6-375	A	11:27	11:28	Word is missing: '… linked with the rapid warming at high northern LATITUDES (Petit et al. …' [Govt. of Spain (Reviewer's comment ID #: 2019-43)]	accepted
6-376	A	11:27		"high northern latitudes" (latitudes missing) [Eric Wolff (Reviewer's comment ID #: 292-12)]	accepted
6-377	A	11:28	11:32	Ill defined statement. Suggest text should be something like: "Southern Hemisphere warming at the end of the last glacial period began before Northern Hemisphere deglaciation, although the pattern of deglacial responses in Northern and Southern Hemispheres differ considerably. The glacial period was punctuated by numerous rapid warming events in the North and almost coincident temperature reversals in the Antarctic ice core record (Blunier and Brook, 2001). The general pattern of these North-South changes is out of phase, with cooling in the South starting around the time of abrupt warming in the North. The precise hemispheric timing of most of these events is obscured by dating uncertainties of most records, however it appears that for the last of these events at around 15ky BP, the Antarctic change is seen ahead of the abrupt northern event (Morgan et al, 2002). This suggests the possibility of a southern trigger (Clark, 2002; Knorr and Lohmann, 2003)." By explaining the phasing, this addition then makes much clearer the following parts of the chapter: p6-18li35; Fig6-7;p6-19li37;p6-19li55-57Refs:	The comment is fair but because of space limitation we have to reject it.

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				Blunier, T., and E.J. Brook, 2001: Timing of millennial-scale climate change in Antarctica and Greenland during the last glacial period. Science, 291, 101-112. Clark, P.U., J.X. Mitrovica, G.A. Milne and M.E. Tamisiea, 2002: Sea-Level Fingerprinting as a Direct Test for the Source of Global Meltwater Pulse IA. Science, 295, 2438-2441. G. Knorr, G. Lohmann, Nature 424, 532 (2003). Vin Morgan, Marc Delmotte, Tas van Ommen, Jean Jouzel, Jérôme Chappellaz, Suenor Woon, Valérie Masson-Delmotte, and Dominique Raynaud. Relative timing of deglacial climate events in Antarctica and Greenland. Science, 297:1862-1864, 2002. [Govt. of Australia (Reviewer's comment ID #: 2001-314)]	
6-378	A	11:31	11:31	"out of phase". This phrase, often used in the literature, poses a problem because the temporal evolution of the signals in the North and the South fundamentally differ. It is therefore difficult to properly define a phase lag. Why is it said: "often more pronounced in the NH" ? Isn't it always the case ? [Michel Crucifix (Reviewer's comment ID #: 52-29)]	Taken into account
6-379	A	11:31	11:31	The Younger Dryas cold event in the NH is preceeded by the New Zealand Late Glacial reversal by 0.83 ka, though both cold events end at about the same time [Williams et al. 2005 Earth & Plan Sci Letters 230, 301-317]. The NZ event is not as deep as the YD in the GRIP record. [Paul W Williams (Reviewer's comment ID #: 291-2)]	noted
6-380	A	11:34	11:40	Information on the range of temperature could be added [Pascale BRACONNOT (Reviewer's comment ID #: 29-11)]	The ranges of radiative perturbation and temperature are discussed in 6.4.1.2
6-381	A	11:34		"greatly" not "largely" (English) [Eric Wolff (Reviewer's comment ID #: 292-13)]	accepted
6-382	A	11:38	:40	Should read, "For example, the CO2 increase from ~185 ppm at the Last Glacial Maximum to ~265 ppm in the early Holcene occurred in distinct phases (Stennie et al. 2001) (see Figure 6.4)." [Govt. of United States of America (Reviewer's comment ID #: 2023-380)]	Accepted but "phases" is changed in "rates" and Stennie et al. 2001 in Monnin et al., 2001
6-383	A	11:39	11:40	I am not sure about what is meant here by 'different phases'. As it comes after a sentence on 'rate of change' I was tempted to look at that graph (figure 6.4.d). However, it is about all GHG and moreover it does not change large variations, except over the most recent time interval. Therfore, I guess that 'different phases' must be related to several time interval in figure 6.4.a, such as before 15,000 AD, between 15,000 AD and 9,000 AD, 9,000 AD and 1700 AD. But they are not really drawn on the figure. Therfore, I would suggest either to expand the idea behind this sentence 'different phases', or to withdraww the sentence. [Marie-France Loutre (Reviewer's comment ID #: 148-4)]	The word "phase" has been changed by "rates". The text deals with CO2 and the figure to look at is 6.4.a

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6-384	A	11:40	11:40	Stenni et al., 2001, should be replaced by Monnin et al., 2001. [Govt. of France (Reviewer's comment ID #: 2010-50)]	accepted
6-385	A	11:40	11:40	Shouldn't the ref. for the last transition of CO2 be Monnin instead of Stenni? [Reto Knutti (Reviewer's comment ID #: 133-51)]	accepted
6-386	A	11:40		Reference should be to Monnin et al (2001) NOT Stenni et al. [Eric Wolff (Reviewer's comment ID #: 292-14)]	accepted
6-387	A	11:45	11:45	Insert after "years" "at least for the very few poorly distributed samples available" [VINCENT GRAY (Reviewer's comment ID #: 88-744)]	Rejected, no basis for assertion given
6-388	A	11:46	11:46	Insert after :"gases" "for these samples at least": [VINCENT GRAY (Reviewer's comment ID #: 88-745)]	Taken into account by adding measured before concentrations
6-389	A	11:46	12:49	the section 6.4.1.1 is difficult to read. The comparison respectively to past periods of the current concentration or increase rate in atmospheric CO2, CH4 and N2O, jumps from one timescale to another and I lost track after line 47. At line 47 the percentages are given for the "last (?)" 200 years and compared to what period? [Myriam Khodri (Reviewer's comment ID #: 126-3)]	Taken into account. Missing word "last " added.
6-390	A	11:47	11:47	within the 200 years" should be replaced by "within the last 200 years [Claire Waelbroeck (Reviewer's comment ID #: 279-14)]	accepted
6-391	A	11:47		Should read, "Within the past 200 years,…" [Govt. of United States of America (Reviewer's comment ID #: 2023-381)]	accepted
6-392	A	11:49	11:49	Delete "large and increaseing" [VINCENT GRAY (Reviewer's comment ID #: 88-746)]	Rejected, no basis for assertion given.
6-393	A	11:49	11:49	show effects of the...' I think 'show effects related to the...' reads better [Mark Siddall (Reviewer's comment ID #: 238-14)]	No necessary change
6-394	A	11:53	11:53	Insert after "gases" "but of course, changes in the main greenhouse gas, water vapour, are unknown" [VINCENT GRAY (Reviewer's comment ID #: 88-747)]	Rejected. In this sentence the comparison is restricted to CO2, CH4 and N20
6-395	A	11:54	11:54	Don't capitalize "Era" - i.e., "era" - and elsewhere. "Industrial Era" is not a formal division of the timescale, is it? [James Crampton (Reviewer's comment ID #: 50-4)]	OK
6-396	A	11:54	11:54	Reference section 2.3 directly rather than whole chapter? [Piers Forster (Reviewer's comment ID #: 73-29)]	accepted
6-397	A	11:55	11:56	Note that industrial era increas in radiative forcing also started from an interglacial base-level, compared to previous glacial to interglacial changes - i.e., "… but occurred one to two orders of magnitude faster and started from an interglacial - i.e. higher - base level" [James Crampton (Reviewer's comment ID #: 50-5)]	accepted
6-398	A	11:56	12:2	Same comment here. Why comparing magnitude/rate respectively to the last 650,000	Noted. We added a sentence explaining

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		From	To		
				years and in the following sentence, make the same remark but respectively to the last 20,000 years? The last 20,000 years are included within the last 650,000 years, so I don't understand the point of the second sentence. [Myriam Khodri (Reviewer's comment ID #: 126-4)]	that the GG records have not the same degree of confidence depending on the considered time interval. The degree of confidence is larger for the last 20,000 years than for the last 650,000 years.
6-399	A	11:57	11:57	Insert after "years" "but we are ignorant of the possible changes in the main greenhouse gas, water vapour" [VINCENT GRAY (Reviewer's comment ID #: 88-748)]	No necessary change. We are here comparing the radiative forcing of CO2, CH4 and N2O under natural and anthropogenic modes. The water vapor is part of the climate systeem and a rapid climaticfeedback of any change occuring in the 3 mentioned atmosphericGG
6-400	A	12:1	12:11	Use "larger" and "faster" properly : a rate is larger (a rate is *not* faster) [Michel Crucifix (Reviewer's comment ID #: 52-30)]	accepted
6-401	A	12:1		Should read, "the average rate of increase…." [Govt. of United States of America (Reviewer's comment ID #: 2023-382)]	accepted
6-402	A	12:5	12:5	Add improved reference … MacFarling et al., 2006 in press MacFarling Meure, C., Etheridge, D., Trudinger, C., Steele, P., Langenfelds, R., van Ommen, T., Smith, A. And Elkins, J. The Law Dome CO2, CH4 and N2O Ice Core Records Extended to 2000 years BP., GRL, in press, 2006. [Govt. of Australia (Reviewer's comment ID #: 2001-315)]	Accepted
6-403	A	12:6	12:6	Insert after "CO2" "as determined on these unrepresentative samples" [VINCENT GRAY (Reviewer's comment ID #: 88-749)]	Rejected, no basis for assertion given
6-404	A	12:8	12:9	Replace " peaked around 1980 .when it" with "in 1984, when it was first measured with modern instruments" [VINCENT GRAY (Reviewer's comment ID #: 88-750)]	No necessary change. The level of confidence of the ice core GG data has been added in 6.4.1.1
6-405	A	12:9	12:9	Insert after "Era" "baut it has fallen ever since and the concentratuion seems to be about to decline" 375 6-375 751 [VINCENT GRAY (Reviewer's comment ID #: 88-750)]	Rejected, no basis given for assertion
6-406	A	12:9	12:9	Insert after "rate" "for the year 2000" [VINCENT GRAY (Reviewer's comment ID #: 88-752)]	Rejected, no basis given for assertion
6-407	A	12:11	12:11	Should start with 1 AD not 0 AD. [Gregory Wiles (Reviewer's comment ID #: 289-20)]	Accepted
6-408	A	12:11		Change '0 to 1800 AD' to "1 to 1800 AD' – there is no such thing as 0 AD [Govt. of United States of America (Reviewer's comment ID #: 2023-383)]	Accepted

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6-409	A	12:13	12:51	Box 6.1 should appear in section 6.2 where is first called and the orbital forcing discussed [Eva Calvo (Reviewer's comment ID #: 37-4)]	Rejected. We place Box 6.1 near the sections where paleoclimatic records are displayed (6.4, 6.5).
6-410	A	12:17	12:17	Cite also Laskar et al. for the most recent astronomical solutions [Michel Crucifix (Reviewer's comment ID #: 52-31)]	Taken into account. Reference added.
6-411	A	12:17	12:17	Insert after"confidence" "but several feedback mechanisms are less well known" [VINCENT GRAY (Reviewer's comment ID #: 88-753)]	Rejected. No evidence given to support the assertion.
6-412	A	12:17	12:17	Reference to Laskar et al (2005) should be added. [Marie-France Loutre (Reviewer's comment ID #: 148-5)]	Taken into account. Reference added.
6-413	A	12:19	12:28	what is the referece for "-800kyr to +200kyr"? Does this mean that the earth axis varies between 22.05 to 24.50 degree in a million years? [Aixue Hu (Reviewer's comment ID #: 110-1)]	Taken into account. Sentence made more clear.
6-414	A	12:19	12:19	it would be nice to replace "from - 800 kyr to + 200 kyr" by "from the past 800 kyr to the future 200 kyr" [Claire Waelbroeck (Reviewer's comment ID #: 279-15)]	Taken into account. Sentence made more clear.
6-415	A	12:19		Language found in the Technical Summary (page 13, lines 10-13) should be inserted here. [Govt. of United States of America (Reviewer's comment ID #: 2023-384)]	Rejected. The relationship between orbital forcing and climate is discussed in the text sections; the box is focussing on the orbital parameters and the insolation.
6-416	A	12:20	12:21	The role of obliquity could be misunterpreded. The first role for a given laltitude is to change the annual mean, second one is to modulate seasonal contrast. [Pascale BRACONNOT (Reviewer's comment ID #: 29-12)]	Taken into account. Sentence reordered.
6-417	A	12:20	12:21	two neighbouring quasi-periodicities around 41 kyr'. In the astronomical solutions (Berger, 1978; Berger and Loutre, 1991; Laskar et al., 2005) there are indeed several neighbouring periodicities (NOT QUASI-periodicities) around 41kyr. The first term in the expansion is clearly and strongly dominating the others. In Berger (1978), the first two terms in the expansion are close to 41 kyr; they are three such terms in Berger and Loutre (1991) and two in Laskar et al (2005). My suggestion : ... with a strong quasi-periodicity around 41 kyr. [Marie-France Loutre (Reviewer's comment ID #: 148-6)]	Taken into account. Text modified.
6-418	A	12:28	12:30	Suggesstion for modified sentence: Changes in eccentricity alone have limited impacts on global and annual mean insolation due to periodic annual changes in the Sun-Earth distance. Alternative suggestion: A chaage in eccentricity from 0.002 to 0.050 implies a minor increase in global and annual mean insolation by ~1 Permille (e.g., Berger, A., M.-F. Loutre and C. Tricot, 1993: Insolation and Earth's orbital periods. J. Geophys. Res. 98,	Taken into account. Text modified.

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				D6, 10341-10362). However, changes in eccentricity affect the intra-annual changes in the Sun-Earth distance and modulate thereby significantly the seasonal-latitudinal effects induced by obliquity and climatic precession. [Eva Bauer (Reviewer's comment ID #: 15-5)]	
6-419	A	12:30	12:33	"Due to the precession of the equinoxes and the longitude of the perihelion, periodic shifts in the position of solstices and equinoxes...". This sentence presents as a cause what is actually a definition. [Michel Crucifix (Reviewer's comment ID #: 52-32)]	Noted, text has been rewritten.
6-420	A	12:32		Should read, "There is no consensus, however, about the exact cause and nature of these ocean circulation changes." [Govt. of United States of America (Reviewer's comment ID #: 2023-385)]	Rejected. Not relevant for the lines cited here.
6-421	A	12:33	12:34	Suggestion to avoid confusion: Modify sentence: "As a result, changes in the positions of the beginnings of the seasons on the orbit strongly modulate ...." (The confusion may arise from saying that the duration of the season changes, because the common practice is to define a season as a fixed time interval.) [Eva Bauer (Reviewer's comment ID #: 15-6)]	Taken into account. Text modified.
6-422	A	12:33	12:33	Reword sentence starting "As a result, changes …" - ambiguous. [James Crampton (Reviewer's comment ID #: 50-9)]	Taken into account. Reference to the duration of the seasons removed.
6-423	A	12:33		kyr used here but ka used in page 11, lines 8 and 12. In fact the usage varies throughout the chapter and needs to be regularised. [Eric Wolff (Reviewer's comment ID #: 292-15)]	Noted. Kyr used for durations and ka for dates.
6-424	A	12:34	12:35	Suggestion for shortening: Seasonal changes of insolation can reach 60W/m^2 (Box 6.1, Figure 1). [Eva Bauer (Reviewer's comment ID #: 15-7)]	Taken into account.
6-425	A	12:34		"Seasonal changes" could be replaced by "Changes in seasonal means" as seasonal changes could be read as the changes from summer to winter. Is, however, the seasonal-mean change the interesting one? The seasonal-mean changes quoted to be up to 60Wm-2 are much smaller than the 110Wm-2 mid-June decrease quoted in the following paragraph for the onset of the last ice age. [Adrian Simmons (Reviewer's comment ID #: 242-97)]	Taken into account. Sentence modified.
6-426	A	12:39	12:39	Modify sentence: "Due to the multi-millennial time periods of the orbital ...". [Eva Bauer (Reviewer's comment ID #: 15-8)]	Taken into account. Sentence modified.
6-427	A	12:39	12:40	The orbital forcing may cause abrupt changes if a threshold (non-linear respose) is crossed. E.g. : desertification of the Sahara. [Michel Crucifix (Reviewer's comment ID #: 52-33)]	Taken into account. Sentence removed.
6-428	A	12:42	12:42	Since it is a theory, I would suggest to write "theory proposes that ice ages are…"	Taken into account. Sentence modified

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				[Michael Schulz (Reviewer's comment ID #: 229-20)]	
6-429	A	12:43	12:43	through" -> "through [James Crampton (Reviewer's comment ID #: 50-6)]	Taken into account.
6-430	A	12:43	12:43	should the "trough" be changed to "through"? [Aixue Hu (Reviewer's comment ID #: 110-2)]	Taken into account.
6-431	A	12:43	12:43	misspelling - 'trough' should be 'through' [Mark Siddall (Reviewer's comment ID #: 238-15)]	Taken into account.
6-432	A	12:43		through" not "trough [Eric Wolff (Reviewer's comment ID #: 292-17)]	Taken into account.
6-433	A	12:44	12:45	Suggestion for a correction: "Typically the onset of the last ice age, ~116 kyr ago, corresponds to a 65 N mid-June insolation decrease of ~40 W/m^2 comparred to today." Or alternatively: "... decrease of ~110 W/m^2 compared to ~128 kyr ago." (Both suggestions are in close agreement with sentence on page 6-17, line 24-25. [Eva Bauer (Reviewer's comment ID #: 15-9)]	Taken into account. Sentence modified.
6-434	A	12:44	12:44	Typically, the onset of the last ice age, ~116 kyr ago' - point number 1 here is that the convention is that ages are given as ka and durations as kyr. This is an age so by convention it should be '116 ka ago' this should also have a citation (there are plenty around for this data and most include an error of +/-1 kyr). Point number 2 is that this age is cited later in the text but on at least one occasion as 120 ka ago. I strongly suggest that consistency be sought in these definitions. See later remark for more detail. To the best of my knowledge the date of 116 +/- 1 ka is based on: Stirling C.H., Esat T.M., Lambeck K., McCulloch M.T., 1998, Timing and duration of the last interglacial: evidence for a restricted interval of widespread coral reef growth, Earth and Planetary Science Letters 160, 745-762. [Mark Siddall (Reviewer's comment ID #: 238-16)]	Taken into account. Text modified for consistency.
6-435	A	12:45	12:45	Is 110 correct? [Pascale BRACONNOT (Reviewer's comment ID #: 29-13)]	Taken into account.
6-436	A	12:46	12:46	This sentence is not correct as it is. Suggestion : 'Studies … include spectral analyses of paleoclimatic records identifying orbital periodicities; precise …' [Marie-France Loutre (Reviewer's comment ID #: 148-7)]	Taken into account.
6-437	A	12:47	12:50	Suggestion for shortening: End sentence on line 47 after "climatic transitions." Substitute text from "modelling of ..." until "including monsoon responses." by new sentence: "The modelling of the climate response to orbital forcing includes dynamical, hydrological and biogeochemical feedback mechanisms." [Eva Bauer (Reviewer's comment ID #: 15-10)]	Taken into account.
6-438	A	12:48	12:48	Missing full stop	Taken into account.

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				[James Crampton (Reviewer's comment ID #: 50-7)]	
6-439	A	12:48	12:48	Delete "out" [James Crampton (Reviewer's comment ID #: 50-8)]	Taken into account.
6-440	A	12:48	12:50	Awkward wording: "Current studies point out to other aspects of the orbital forcing than the 65N summer insolation changes to account for paleoclimatic changes including monsoon responses." Meaning is unclear. Amendment required. [Govt. of Japan (Reviewer's comment ID #: 2014-40)]	Taken into account. Sentence removed.
6-441	A	12:48		There should be a period after "biogeochemical feedbacks." [Govt. of United States of America (Reviewer's comment ID #: 2023-386)]	Noted.
6-442	A	12:53		box 6.2 this is what I was complaining about - at least an example of it - you cannot say something definitive, make it shorter, EVEN if what you do say is accurate [Thomas Crowley (Reviewer's comment ID #: 51-26)]	Rejected. The explanation of glacial-interglacial CO2 variations remains an important issue and this box has been favourably reviewed in the FOD review.
6-443	A	13:15	13:15	Reword sentence: "Globally, atmospheric CO2 would be higher if the ocean lacked biological productivity." [James Crampton (Reviewer's comment ID #: 50-10)]	Taken inot account. Text edited
6-444	A	13:15	13:15	Globally, atmospheric…' I suggest replacing with 'Global atmospheric ...' [Mark Siddall (Reviewer's comment ID #: 238-17)]	Taken into account. Text edited
6-445	A	13:22	:53	NEEDS SOURCE REFERENCES FOR THE HYPOTHESIS ON THE CONTROLS ON CO2 DURING GLACIAL TIMES [Govt. of Spain (Reviewer's comment ID #: 2019-129)]	Rejected. Source references are given on page 13, line 2 and 3. Not repeated here for space reason.
6-446	A	13:27	13:27	available sediment data do not … [James Crampton (Reviewer's comment ID #: 50-11)]	Accepted
6-447	A	13:33	13:33	lofted' is a specialised term, please replace e.g. 'carried by winds' [Mark Siddall (Reviewer's comment ID #: 238-18)]	Accepted
6-448	A	13:52	13:52	Adkinson et al. 2002 is given as a reference but it should be Adkins et al. 2002 [Myriam Khodri (Reviewer's comment ID #: 126-5)]	Accepted
6-449	A	13:52	13:52	Ref. Adkinson should be Adkins. There are two versions of that ref. in the ref. section. [Reto Knutti (Reviewer's comment ID #: 133-52)]	Accepted
6-450	A	13:52	13:52	there is a typo related to '(Adkinson et al., 2002)' this should be '(Adkins et al., 2002)' - this is cited as both Adkinson et al. and Adkins et al. in the references so Adkinson et al. should be removed there [Mark Siddall (Reviewer's comment ID #: 238-19)]	Accepted
6-451	A	13:53		REVISE REFERENCE: Köhler et al., in press). [Govt. of Spain (Reviewer's comment ID #: 2019-131)]	Rejected. No basis for assertion given

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6-452	A	13:56	13:56	Delete Kohler et al. (in press) reference. It is not needed. Also, this gives the false impression that Kohler et al. were the first to talk about multiple mechanisms when in fact many people have said this. [Katsumi Matsumoto (Reviewer's comment ID #: 171-1)]	Taken into account. Text edited
6-453	A	13:56	13:56	There is a number of authors who proposed that the synergy between different forcing factors are responsible for G-IG pCO2 change; referring to Köhler et al alone seems a bit unfair to previsous workers; I would at least suggest to put an "e.g." in front [Michael Schulz (Reviewer's comment ID #: 229-21)]	Accepted
6-454	A	13:56	14:2	Replace "the underlying changes in climate" with "our understanding of the global carbon cycle and observations" [Katsumi Matsumoto (Reviewer's comment ID #: 171-2)]	Partly taken into account. Text edited
6-455	A	13:56		Kohler et al (in press) is not in ref list unless you mean the 2005 paper; I suspect you really mean either (Kohler, P., H. Fischer, G. Munhoven, and R.E. Zeebe, Quantitative interpretation of atmospheric carbon records over the last glacial termination, Global Biogeochemical Cycles, 19 (4), 2005), or his new paper in "Climate of the Past Discussions". [Eric Wolff (Reviewer's comment ID #: 292-18)]	Noted, revised
6-456	A	14:13	14:13	Delete "consistently" [VINCENT GRAY (Reviewer's comment ID #: 88-754)]	Accepted
6-457	A	14:17	14:17	Should indicate how the estimate of the radiative forcing is obtained. [Pascale BRACONNOT (Reviewer's comment ID #: 29-14)]	Accepted, Figure 6.4, which includes how radiative forcing is calculated, is now cited here
6-458	A	14:18		Delete "relative to 1750". Radiative forcing is already quoted as relative to 1750 in Chapter 2. [Adrian Simmons (Reviewer's comment ID #: 242-98)]	Accepted
6-459	A	14:19	14:19	Reference section 2.3 directly rather than whole chapter? [Piers Forster (Reviewer's comment ID #: 73-30)]	Accepted
6-460	A	14:25	14:25	Delete "itself" [James Crampton (Reviewer's comment ID #: 50-12)]	Accepted
6-461	A	14:25		"itself partly a consequence" (it is certainly not proved that the dust increase is only due to vegetation changes) [Eric Wolff (Reviewer's comment ID #: 292-19)]	Accepted
6-462	A	14:27	14:27	Replace many by some, because there is still only a limited number of such simulations [Pascale BRACONNOT (Reviewer's comment ID #: 29-15)]	Accepted
6-463	A	14:27		"each contribute": which do you mean by "each"? Vegetation and aerosols? If so, it needs to be stated more clearly.	Accepted, sentence rewritten

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				[Eric Wolff (Reviewer's comment ID #: 292-20)]	
6-464	A	14:30	14:30	Correction: '… half of the know radiative …' --> '… half of the KNOWN radiative …' [Govt. of Spain (Reviewer's comment ID #: 2019-44)]	Accepted
6-465	A	14:48	14:53	CLIMAP, GLAMAP, MARGO - I think these need defining here, even if they are defined elsewhere [Mark Siddall (Reviewer's comment ID #: 238-20)]	Accepted
6-466	A	14:48		For clarity, this paragraph could begin: "The CLIMAP reconstruction of ocean surface temperatures produced in the early 1980s", to help non-specialist readers who do not immediately recognise what the CLIMAP reconstruction is. [Adrian Simmons (Reviewer's comment ID #: 242-99)]	Accepted
6-467	A	14:55	14:55	Delete extra ")" [James Crampton (Reviewer's comment ID #: 50-13)]	Accepted
6-468	A	14:56		to my knowledge Ballantyne et al GRL 2005 give the most thorough estimate of tropical SST changes, including uncertainty - 2.7 ± 0.5 (one sigma) doi:10.1029/2004GL021217, 2005 [Thomas Crowley (Reviewer's comment ID #: 51-27)]	Accepted, summary sentence added
6-469	A	15:1	15:3	The last sentence is unclear [Pascale BRACONNOT (Reviewer's comment ID #: 29-16)]	Accepted, sentence rewritten to clarify
6-470	A	15:7	15:7	"more meridional ocean surface circulation". Is it meant "more southward"? [Michel Crucifix (Reviewer's comment ID #: 52-34)]	Meaning is that surface currents are less zonal and more meridional
6-471	A	15:21	15:21	indermediate" -> "intermediate [James Crampton (Reviewer's comment ID #: 50-14)]	Accepted
6-472	A	15:36	15:36	Add at end "It should be remembered that intercomparison exercises can often do little more than confirm common erors" [VINCENT GRAY (Reviewer's comment ID #: 88-755)]	Rejected, no basis offered for assertion
6-473	A	15:40	15:41	shifts in the Kurishuio and Gulf Stream currents. The Figure 6.5 indeed shows cooling to the North of the Kurishuo and Gulf Stream, but this does not necessarily imply that these have shifted. To show that they have shifted, it is necessary to examine the surface currents. [Michel Crucifix (Reviewer's comment ID #: 52-35)]	Accepted, sentence rewritten
6-474	A	15:52	15:52	All regions (with the possible exception of the polar winter) are affected by radiative forcings. This sentence as written is obvious in the extreme. [Gavin Schmidt (Reviewer's comment ID #: 227-3)]	Accepted, deleted "influenced by radiative forcing"
6-475	A	15:54		MARGO results FROM SOME PROXY ESTIMATES. IN HERE YOU SHOULD CONSIDER RESULTS FROM OTHER COMPILATIONS AND THE REFERENCES THEREIN USING ALKENONES, AS IN THE CITED PAPER IN FIG. 6.5, ALSO	Accepted

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				FOLLOWING MARGO, BY Rosell-Mele, A., E. Bard, K.-C. Emeis, B. Grieger, C.D. Hewitt, P. Muller, J., and R.R. Schneider, 2004: Sea surface temperature anomalies in the oceans at the LGM estimated from the alkenone-UK'37 index: comparison with GCMs. Geophysical Research Letters, 31, L03208. [Govt. of Spain (Reviewer's comment ID #: 2019-132)]	
6-476	A	16:1	16:9	There are two separate questions that have been mixed, here. 1. Do GCMs coupled to a vegetation model reproduce correctly the vegetation patterns of the LGM. Appropraite references are Harrison and Prentice, 2003, and Crucifix, Betts, Hewitt, Glob. Plan. Change 2005 and references therein. The other question is the impact of these vegetation changes on climate (e.g. , Siberian cooling, impact on Monsoon; please down weight the Tibet effect because it is probably less robust). These aspects are discussed in Wyputta Mc Aveney, and Crucifix and Hewitt, Clim. Dyn, 200 (cited) [Michel Crucifix (Reviewer's comment ID #: 52-36)]	Accepted. Paragraph rearranged.
6-477	A	16:12	16:13	I suggest rewording as " … by paleodata IN RESPONSE to the radiative forcing and land surface changes of the Last Glacial Maximum, AND thus INDICATE THAT THEY adequately represent the feedbacks …" [Danny Harvey (Reviewer's comment ID #: 101-36)]	Accepted
6-478	A	16:13	16:13	The feedbacks referred to here are only a subset of the total feedbacks – specifically they do not include ice sheet, vegetation, carbon cycle etc responses, since those values were imposed. A bald statement that 'the' feedbacks are well modelled could be misinterpreted. [Gavin Schmidt (Reviewer's comment ID #: 227-4)]	Accepted, see comment 6-477
6-479	A	16:18		The wrong section of chapter 9 is referred to. It should be 9.6.3.2. [Danny Harvey (Reviewer's comment ID #: 101-43)]	Accepted
6-480	A	16:26		I have a standard gripe that the claims of uncertainties in the Crowley 1995 estimate reflect the fact that people have not read the details of the paper - because in fact an uncertainty analysis was conducted, which is why there is a large spread of values. and I find it notable that the 600-1000 Mt values cited in the ipcc report fig very snugly in the range I state. I furthermore point out in my paper that hte C13 data arenot bulletproof, for glacial stage 6 has very different C13 changes despite the fact that boundary conditiosn were virtually identical to stage 2. [Thomas Crowley (Reviewer's comment ID #: 51-28)]	Accepted. Text deleted.
6-481	A	16:26		"however" serves as a conjunctive adverb here, and should be preceded by a semicolon and followed by a comma. Check all other uses of "however". [Danny Harvey (Reviewer's comment ID #: 101-37)]	Taken into account. Text has been deleted
6-482	A	16:28	16:29	".. yield a reduction in global carbon stocks of ... " : make clear that it is terrestrial vegetation. See also the estimate (compatible with the other references) of Crucifix, Betts, Hewitt, Glob. Plan. Change 2005.	Accepted

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				[Michel Crucifix (Reviewer's comment ID #: 52-37)]	
6-483	A	16:31	16:32	Should read, Bond et al. (2003) [Govt. of United States of America (Reviewer's comment ID #: 2023-387)]	Accepted
6-484	A	16:39	16:39	MIS7 is short in the Antarctic ice core records but poorly defined in the benthic and planktonic isotopes. In terms of sea level (and the original SPECMAP datings) sea level was likely above 15 m below modern but peaked 3 times (Bard et al. 2002, Antoniolo et al. 2004, Thompson and Goldstein 2005; Waelbroek et al) at similar levels (MIS 7a, 7c and 7d). This makes it a bit ridiculous to talk about a MARINE ISOTOPE STAGE simply in terms of ice - it may be that MIS 7d is the 'true interglacial' and this should be referred to as such - although sea level proxies are converging on there being single, well defined 'interglacial' during MIS 7. Similarly MIS 5e is almost certainly the true interglacial over the MIS 5 period and should be referred to as such. [Mark Siddall (Reviewer's comment ID #: 238-21)]	Accepted, willl be revised
6-485	A	16:40	16:57	I suggest making the years cited in Section 6.4.1.5 consistent. Line 40 refers to "~420 to 395 kyr ago) lasted almost 30 yrs." Line 56 refers to insolation maximum at ~427 kyr ago. Did the Stage 11 interglacial begin at an unknown time between 427 and 420 kyr ago? If it lasted "a total duration of 28 kyr," as line 57 reports, then you must know what kyr it began in, right? I suggest referring to 28 kyr on line 40, as you did on line 57. [WG1 TSU (Reviewer's comment ID #: 285-7)]	Accepted
6-486	A	16:40	:40	statement is made that the longest interglacial has been 30K years; in the summary for policy makers, the statement is made that there won't be another cooling event for at least 30K years more, after already having 10K years of interglacial. The year 2022 will be interesting. [Lee Gerhard (Reviewer's comment ID #: 83-5)]	Noted, no changes required
6-487	A	16:52		Quote also Siegenthaler et al 2005 here as well as Raynaud. [Eric Wolff (Reviewer's comment ID #: 292-21)]	Accepted
6-488	A	16:55	16:55	…deglaciation was triggered… [James Crampton (Reviewer's comment ID #: 50-15)]	Noted, no changes required
6-489	A	16:55	16:55	conceptual models 'show' nothing - they are conceptual and indicate a possible mechanism. Please ammend the text to show this - 'deglaciation may be' or 'deglaciation could be' [Mark Siddall (Reviewer's comment ID #: 238-22)]	Accepted
6-490	A	16:56	16:56	…insolation minimum was not sufficient… [James Crampton (Reviewer's comment ID #: 50-16)]	Noted, no changes required
6-491	A	17:6	17:8	Less important remarks follow: the end of the sentence is not clear: to what does "their" refer to in "with their transitions"? This sentence should be modified in order to clarify its	Accepted

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				meaning. [Claire Waelbroeck (Reviewer's comment ID #: 279-13)]	
6-492	A	17:7	17:8	Note that Augustin et al. (i.e., EPICA) align the deglaciation events. The justification by obliquity has been made a posteriori. Furthermore, aligning obliquity implies to align the present on 407 kyr BP, which is not equivalent to the choice of Augustin who align the present with 410 kyr BP. [Michel Crucifix (Reviewer's comment ID #: 52-38)]	Taken into account in revision
6-493	A	17:7	17:7	Ref. Augustin should be something like 'EPICA members' to be consistent with the other refs. in that chapter (e.g. North GRIP Project, etc.) [Reto Knutti (Reviewer's comment ID #: 133-53)]	Accepted
6-494	A	17:7	17:7	Augustin et al. 2004: Elsewhere, this work is cited as "EPICA community member, 2004". Should be consitent. (Note, that Augusting appears elsewhere in the text) [Michael Schulz (Reviewer's comment ID #: 229-9)]	Accepted
6-495	A	17:7	17:8	Please change the citation Augustin et al., 2004 to EPICA Community Members, 2004 [Renato Spahni (Reviewer's comment ID #: 249-1)]	Accepted
6-496	A	17:7		Augustin et al should be "EPICA Community Members 2004" as it is in other parts of the chapter. [Eric Wolff (Reviewer's comment ID #: 292-22)]	Accepted
6-497	A	17:10	:22	This is the data section that comment number 2 was based on - argues that something is different, because we have more ice now than the prervious interglacial, and tha t flies pretty much in the face of uniformitarianism and logic. So we have more greenhouse and more ice? [Lee Gerhard (Reviewer's comment ID #: 83-6)]	Rejected, no basis offered for assertion
6-498	A	17:11	17:11	129 - 116 kyr ago' should be: '129 +/- 1 ka to 116 +/- 1 ka ago' (ka for ages, kyr for durations). This date is almost certainly based on: Stirling C.H., Esat T.M., Lambeck K., McCulloch M.T., 1998, Timing and duration of the last interglacial: evidence for a restricted interval of widespread coral reef growth, Earth and Planetary Science Letters 160, 745-762. [Mark Siddall (Reviewer's comment ID #: 238-23)]	Accepted date ranges;
6-499	A	17:21	17:21	Delete "although" [James Crampton (Reviewer's comment ID #: 50-17)]	Accepted
6-500	A	17:29		insert "they" after "although" [Danny Harvey (Reviewer's comment ID #: 101-38)]	Rejected, no longer relevant with revision suggested in 6-499
6-501	A	17:30	17:31	Rephrase sentence to "Simulated global temperature increase is less than 1 C compared to today?/the pre-industrial period?." [Michael Schulz (Reviewer's comment ID #: 229-10)]	Accepted, text rewritten

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6-502	A	17:35		Section 6.4.1.7 - too many undefined(?) abbreviations in this section [James Crampton (Reviewer's comment ID #: 50-18)]	Accepted, abbreviations defined
6-503	A	17:37	17:37	Note that Milankovitch never referred to 65 N. Milankovitch used "caloric seasons". However, it is true that using 65 N is inspired by his conclusions that northern hemisphere, summer insolation determines the evolution of ice volume. [Michel Crucifix (Reviewer's comment ID #: 52-39)]	Accepted, text clarified.
6-504	A	17:37	17:37	Since it is a theory, I would suggest to write "theory proposes that ice ages are…" [Michael Schulz (Reviewer's comment ID #: 229-11)]	Accepted
6-505	A	17:38	17:39	"Solid" seems an over-statement. There are certainly some sort of association, but the causal connection is still in question. [Michael Mann (Reviewer's comment ID #: 156-64)]	Rejected, no basis offered for assertion
6-506	A	17:39	17:40	the date of 120 ka BP is not dervied independently in the Waelbroeck paper and is based on speculative (although later work has shown at least partly reasonable) assumptions about the onset of the ice ages. I have emailed Claire Waelbroeck directly to ask about this specific issue. This date is not in agreement with the date of 116 ka BP cited twice in the preceding text (page 12, line 44, page 17, line 11). The date of 116 ka BP is likely from Stirling C.H., Esat T.M., Lambeck K., McCulloch M.T., 1998, Timing and duration of the last interglacial: evidence for a restricted interval of widespread coral reef growth, Earth and Planetary Science Letters 160, 745-762. I would agree with the dates from this careful work and them throughout the text as a matter of consistency. If the authors feel it is appropriate they may wish to include a recent review of sea level during the interglacials that I and coworkers have completed: Siddall M., Chappell J., Potter E.-K., in press: Eustatic Sea Level During Past Interglacials, in: `The climate of past interglacials,' F. Sirocko, T. Litt, M. Claussen, M.- F. Sanchez-Goni (eds.), Elsevier, Amsterdam. [Mark Siddall (Reviewer's comment ID #: 238-24)]	Accepted
6-507	A	17:39	17:41	There are two major problems in the sentence "Continental …values". (1) Waelbroeck et al. (2002) do not provide any information on the absolute date of the end of the last interglacial sea level high stand. This reference should thus be removed from this sentence; [Claire Waelbroeck (Reviewer's comment ID #: 279-1)]	Accepted
6-508	A	17:39	17:41	(2) It is contradictory to state on line 39 that sea level lowering started at about 120 ka BP, while the last Interglacial is said to have lasted from ~129 to 116 kyr ago on line 11 of the same page. One of these dates should be retained and appropriate references given. [Claire Waelbroeck (Reviewer's comment ID #: 279-2)]	Accepted, change to 116 ka
6-509	A	17:41	17:41	The 65N June insolation reached a minimum at 116 kyr BP. It is also the case for the NH summer insolation. Thus the continental ice sheet started to grow [12à kyr according to the text] BEFORE the minimum of insolation, and not at the time of the minumim as	Accepted,

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				mentioned in the tex. Thus the regrowth of the ice sheets and the lowering of sea level started during the decrease of NH insolation in the high latitudes. [Marie-France Loutre (Reviewer's comment ID #: 148-14)]	
6-510	A	17:41	17:43	I did not see any definition of glacial inception. Does it mean the restart of ice sheet regrowth, or the beginning of global temperature decrease, or anythin else? Obviously, it is not the first defintion as it is written that inception took place while ice volume is stable. [Marie-France Loutre (Reviewer's comment ID #: 148-15)]	Accepted,
6-511	A	17:53	17:53	Please include estimates of sea level derived from data - I would mention at the minimum: Siddall M., Rohling E.J., Almogi-Labin A., Hemleben Ch., Meischner D., Schmelzer I., Smeed D.A., 2003, Sea level fluctuations during the last glacial cycle, Nature 423, 853-858; Chappell J., 2002, Sea level changes forced ice breakouts in the last glacial cycle: new results from coral terraces, Quaternary Science Reviews 21, 1229-1240;Cutler K.B., Edwards R.L., Taylor F.W., Cheng H., Adkins J., Gallup C.D., Cutler P.M., Burr G.S., Bloom A.L., 2003, Rapid sea-level fall and deep-ocean temperature change since the last interglacial period, Earth and Planetary Science Letters 206, 253-271; The variability in sea level in these records may be too controversial to include here but there is good agreement of a typical MIS 3 sea level of -75 to -85 m between records, which is what is needed to make the point made here - that ice sheet models do not yet agree with reproducable proxy estimates. [Mark Siddall (Reviewer's comment ID #: 238-25)]	Accepted, Cutler et al. reference which is relevant to ice sheet modeling of glacial inception. Rejected, other references which are not within scope of this section.
6-512	A	17:54	17:54	In my opinion Lambeck K., Chappell J., 2001, Sea level change during the last glacial cycle, Science 292, 679 686. MUST be included here and their sea level curve MUST be shown on Fig.6.8a - the paper provides an excellent overview of the problems of deriving sea-level estimates in the past as well as a sea level curve that combines isostatic correction with data. The curve combines careful stratigraphic interpretation with careful dating and isostatic corrections for sea level (from an alternative model to that of Peltier, we should not put too much weight on any one model) - there is no excuse not to include it in a genuinely consensus piece of work. With IPCC we must have a product with which one cannot be left with the suspicion that one school of thought has dominated the outcome at the cost of another. By including the Lambeck and Chappell curve any remaining doubt on this will be gone and there will be a better balance between the three principal techniques available for sea-level reconstructions - fossil reef evidence, benthic oxygen isotopes and ice-sheet modelling. Some may criticise and argue down the Lambeck and Chappell curve in preference for alternatives but in a consensus piece of work our real uncertainty in this is best represented by its inclusion. This will make obvious the range of realistic estimates available.	See comments in 6.4.3

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				[Mark Siddall (Reviewer's comment ID #: 238-26)]	
6-513	A	17:56	17:56	Replace from "could mitigate" to "natural" with "provide a current" [VINCENT GRAY (Reviewer's comment ID #: 88-756)]	Rejected, no basis offered for assertion
6-514	A	17:56		Should read, "There is no evidence of mechanisms that could mitigate…." [Govt. of United States of America (Reviewer's comment ID #: 2023-388)]	Accepted
6-515	A	17:57	18:1	This is completely overstated. We do not know what would cause the current interglacial to end without human intervention. The statement as it stands accepts the Milankovitch theory in its entirety. There are problems with the theory, like the so-called "100 kyr problem" (climate response to eccentricity is largest when its forcing is the smallest) and the "transition problem" (change in the dominant frequency from obliquity to eccentricity about 1 mya). There are more problems, so there should be a recognition that Milankovitch is not all right. This means we cannot rely on the theory to predict how the current interglacial will end. [Katsumi Matsumoto (Reviewer's comment ID #: 171-3)]	Rejected, no basis offered for assertion
6-516	A	18:3	18:3	Never again? This should be qualified with the timescale or rewritten so that it more clearly refers to the next few 10's of 1000s ofyears. [Gavin Schmidt (Reviewer's comment ID #: 227-5)]	Accepted, rewritten to be consistent with bullet
6-517	A	18:6	18:6	Please change the citation Augustin et al., 2004 to EPICA Community Members, 2004 [Renato Spahni (Reviewer's comment ID #: 249-2)]	Accepted
6-518	A	18:6		Augustin et al should be "EPICA Community Members 2004" as it is in other parts of the chapter. [Eric Wolff (Reviewer's comment ID #: 292-23)]	Accepted
6-519	A	18:13	18:51	Nice section, but insist better on timing uncertaincies and explain how records are synchronised (problem of building common time scales). [Michel Crucifix (Reviewer's comment ID #: 52-44)]	rejected for lack of space - we need to shorten, not add
6-520	A	18:16	18:17	Problem with brackets [James Crampton (Reviewer's comment ID #: 50-19)]	fixed
6-521	A	18:17	18:17	Ref. Overpeck: misplaced brackets [Reto Knutti (Reviewer's comment ID #: 133-54)]	fixed
6-522	A	18:22	18:23	Add citation "Huber et al., EPSL, Isotope Calibrated Greenland Temperature Record over Marine Isotope Stage 3 and its Relation to CH4, 2006" for temperature estimations on additional D/O events [Renato Spahni (Reviewer's comment ID #: 249-3)]	number of references needs to be cut, not increased
6-523	A	18:23	18:28	I would strongly support a more careful definition of Heinrich event based on Hemming 2004. My reading of Hemmin 2004 is that the formal definition of a Heinrich (H-event) is an event during which icebergs originating from Hudson Strait deposited iceberg-rafted	rejected - we did base our definition on Hemming 2004, we cite her paper here, and Sydney Hemming commented

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				debris over large areas of the sea floor. Heinrich events occur at the end of a colder period and are infact coincident with warming in Greenland and not cooling. They do occur at the end of the longer and more important D-O stadials - they only last up to a maximum of 600 years and so they do not explain the whole of the cold periods which last several millennia nor do they explain all of the cold periods - there are plenty of D-O stadials without significant amounts of IRD. At EGU this year Luke Skinner proposed the term 'Heinrich Stadials' for these events and I would support this. These careful definitions make all of the difference in our understanding - an H-event at the end of a stadial supports the idea of ice-sheet growth during the cold period followed by a purge. An H-event at the start of a cold period argues against H-events being the freshwater trigger often cited to invoke seesaw type behaviour. Please consider the wording and definitions in this paragraph very carefully. [Mark Siddall (Reviewer's comment ID #: 238-27)]	favorably on this section herself.
6-524	A	18:23		Referring to Heinrich events as "another type of abrupt change", under the general heading "What is the evidence for past abrupt climate changes" is very confusing and misleading. Heinrich events are iceberg discharges. They have been interpreted as linked to climate, but they are not climate changes and should not be presented as such. [Eric Steig (Reviewer's comment ID #: 252-18)]	see previous - the events are in fact presented as iceberg discharge events.
6-525	A	18:24	18:25	Heinrich events are "defined" by the drop-stones in ocean sediments, not just "characterised". Cite the original reference by Heinrich (1988) in Quaternary Research (29) 142-152 [Michel Crucifix (Reviewer's comment ID #: 52-40)]	Rejected - we reflect the somewhat wider usage of the term. Heinrich ref had been there, but had to be cut for space reasons - we refer to Hemming's review paper, which will point the reader to all the classic references
6-526	A	18:25		SEE REVIEW BY Hemming, 2004. [Govt. of Spain (Reviewer's comment ID #: 2019-133)]	noted - we did base this on Hemming 2004, we cite her paper here, and Sydney Hemming commented favorably on this section herself.
6-527	A	18:28	18:28	Problem with brackets [James Crampton (Reviewer's comment ID #: 50-20)]	punctuation error fixed
6-528	A	18:34	18:35	Sentence incomplete? [James Crampton (Reviewer's comment ID #: 50-21)]	punctuation error fixed
6-529	A	18:34	18:34	Suppress "The repercussions of". Cf. comment #29 about the phrase "out of phase", which I do not encourage. [Michel Crucifix (Reviewer's comment ID #: 52-41)]	accepted - replaced repercussions by "effects"
6-530	A	18:34	18:34	"out-of-phase" should be clarified. I guess what is meant is "of opposite sign". Even in the seesaw concept, NH and SATL _react_ at the same time (but in opposite directions); the	rejected - the phase relation is not exactly anti-phase, that's why we use

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				delay is then between ANT and the SATL. [Michael Schulz (Reviewer's comment ID #: 229-12)]	the more general term "out of phase"
6-531	A	18:34	18:36	UNCLEAR STATEMENTS [Govt. of Spain (Reviewer's comment ID #: 2019-134)]	punctuation error fixed
6-532	A	18:34		insert "with" after "although" [Danny Harvey (Reviewer's comment ID #: 101-39)]	punctuation error fixed
6-533	A	18:34		The phrase "repercussions of these abrupt climate changes" assumes that the abrupt changes (in the North Atlantic) happen and spread to the rest of the globe. This is the leading hypothesis, but by no means the only one and it is certainly not proven fact. This section should be reworded to avoid the tendency to confuse observation with hypothesis. [Eric Steig (Reviewer's comment ID #: 252-16)]	accepted - we now say "effects", and it does not say anywhere they start in the Atlantic
6-534	A	18:34		Section 6.4.2.1 may be the place to briefly discuss contributions (to atmospheric CH4 and CO2) from and climate responses of northern (boreal and subarctic) peatlands. Suggested references include: Smith et al. 2004. Science 303: 353-356; Yu et al. 2003; Vitt et al. 2000. Can. J. Earth Sci. 37: 683-693. The Holocene 13: 801-803. [Govt. of United States of America (Reviewer's comment ID #: 2023-389)]	rejected for lack of space
6-535	A	18:34		out of phase responses occurred in the two [Eric Wolff (Reviewer's comment ID #: 292-24)]	punctuation error fixed
6-536	A	18:36		Should read "appears centered" [Govt. of United States of America (Reviewer's comment ID #: 2023-390)]	spelling - accepted
6-537	A	18:38	18:39	Add citation "Huber et al., EPSL, Isotope Calibrated Greenland Temperature Record over Marine Isotope Stage 3 and its Relation to CH4, 2006" for temperature estimations on additional D/O events [Renato Spahni (Reviewer's comment ID #: 249-4)]	rejected for lack of space
6-538	A	18:45	18:46	Figure 6.7 shows clearly that there are Antarctic counterparts to HE, but it is not obvious that there are counterparts to Dansgaard - Oeschger events. [Michel Crucifix (Reviewer's comment ID #: 52-42)]	rejected - new data show these counterparts for DO events
6-539	A	18:51	18:51	Recall that the Younger Dryas is primarily defined by botanical evidence. [Michel Crucifix (Reviewer's comment ID #: 52-43)]	noted - is consistent with what we write
6-540	A	18:54	18:54	Replace 'Sànchez' by 'Sánchez' [JAVIER MARTIN-VIDE (Reviewer's comment ID #: 165-14)]	accepted
6-541	A	18:54	18:54	Replace 'Sànchez' by 'Sánchez' [Govt. of Spain (Reviewer's comment ID #: 2019-74)]	accepted
6-542	A	19:0		Section 6.4.2.3. An additional penultimate sentence could be added (page 20, line 9) in view of preceding reference (in section 6.4.2.2) to large sea level variations, which are difficult to fully attribute to changes in northern ice sheets: "Reconciliation of large	rejected - lack of space and some doubt about the robustness of this conclusion

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				glacial sea level variations (up to 15 m) with likely limited contributions from northern ice sheets has been obtained by prescribing equal and simultaneous melting of the Antarctic ice sheet (Rohling et al. 2004)." Reference: Rohling, E. J., Marsh, R., Wells, N. C., Siddall, M., and N. R. Edwards (2004). Similar meltwater contributions to glacial sea level changes from Antarctic and northern ice sheets. Nature, 430, 1016-1021. [Robert Marsh (Reviewer's comment ID #: 164-1)]	
6-543	A	19:2	19:2	This should read "There is evidence", not "there is solid evidence". [Eric Steig (Reviewer's comment ID #: 252-17)]	accepted in part - changed to "good"
6-544	A	19:3	19:4	The characterization of the abrupt changes as "the South Atlantic warmed when the north warmed, and vice versa" is incorrect. Although this way of describing the data is popular, it is not very accurate. At the very least, the numerous papers pointing this out should be cited. Steig and Alley, 2002; Wunsch, 2003; Huybers, 2003; Schmittner et al., 2003; Roe and Steig, 2004. Furthermore, the purported relationship between N and S can only be demonstrated for the largest events, not for the events generally. [Eric Steig (Reviewer's comment ID #: 252-19)]	Partly accepted. The sentence referred to discusses what the data show, and perhaps the characterisation was too simple. We thus have added "with possible lag". The references proposed do not provide any data inconsistent with this characterisation, so we hope the reviewer's concern is covered in this way. Concerning the final point: the cited Landais paper demonstrates this for a few more events (not just the largest), and the new EPICA core data demonstrate this for events in general (not ready to be cited yet).
6-545	A	19:5		another "me" gripe - I wish people would quit giving Broecker credit for something he did not discover - the proper reference, with explanation for why the see-saw works, is Crowley 1992 NADW cools the southern hemisphere, paleoc. 7:489-497 [Thomas Crowley (Reviewer's comment ID #: 51-29)]	accepted
6-546	A	19:6	19:6	Kreveld et al." should read "van Kreveld et al. [Michael Schulz (Reviewer's comment ID #: 229-13)]	accepted, thanks
6-547	A	19:7	19:7	temperate [Eva Calvo (Reviewer's comment ID #: 37-5)]	accepted, thanks
6-548	A	19:7	19:7	nortward" -> "northward [James Crampton (Reviewer's comment ID #: 50-22)]	accepted, thanks
6-549	A	19:7	19:7	spelling … temperate [Andrew Lacis (Reviewer's comment ID #: 138-8)]	accepted, thanks
6-550	A	19:7		"northward" "temperate" (spelling) [Eric Wolff (Reviewer's comment ID #: 292-25)]	accepted, thanks

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6-551	A	19:10	19:11	Clarify better that 13C is a water mass proxy, and Pa/Th is a kinematic proxy. [Michel Crucifix (Reviewer's comment ID #: 52-45)]	accepted
6-552	A	19:16	19:16	The current sentence implies that H events were triggered by ice sheet instabilities. Although the 'cause' of the H layers are ice sheet discharges, I don't think we know what the initial trigger was that caused the ice sheets to collapse (internal, oceanic, atmospheric, etc.). So I would prefer 'related to ice sheet instabilities', or 'caused by ice sheet instabilities, although the initial trigger remains unclear'. [Reto Knutti (Reviewer's comment ID #: 133-55)]	noted, but we think the current phrasing is correct and does not imply a particular trigger of the ice sheet instability
6-553	A	19:16	19:16	Macayeal" should read "MacAyeal [Michael Schulz (Reviewer's comment ID #: 229-14)]	accepted, thanks
6-554	A	19:16		THE FOLLOWING REFERENCE DISCUSED THE ISSUE BEFORE THE ONE CITED: Broecker,W. S., G. C. Bond, M. Klas, E. Clark, and J. F. McManus (1992), Origin of the northern Atlantic's Heinrich events, Clim. Dyn., 6, 265–273. [Govt. of Spain (Reviewer's comment ID #: 2019-135)]	rnoted - but to save space we refer to a recent review paper rather than the older "classics"
6-555	A	19:18	19:18	Note that the isotope does not vary. This is the ratio of 18O/16O abundances that varies [Michel Crucifix (Reviewer's comment ID #: 52-46)]	accepted
6-556	A	19:18	19:22	I'm not saying those numbers are right, but Siddall et al. estimate up to 35m of sea level rise for H events, not 15. Also, I'm not convinced that the Roche study gets the numbers right. The amount of freshwater is very small, and the duration very short, and both are in sharp contrast to the sea level reconstructions over stage 3 (Siddall, Chappell, etc.), which indicate much larger sea level rise and in particular a rise over more like a thousand years. It might be worth to mention that there is no consensus there. [Reto Knutti (Reviewer's comment ID #: 133-56)]	accepted - added: Volume and timing of freshwater release is still controversial, however. [We are not confident enough to cite the 35 m number]
6-557	A	19:19	19:19	Sidney Hemming estimated a maximum 600 years duration for a Heinrich event - here you say up to 2000 years. This sort of confusion is linked to confusing cold D-O stadials with H-events (see comment above). [Mark Siddall (Reviewer's comment ID #: 238-28)]	accepted - looking at Hemming's paper again, we now cite 250-750 years from her abstract
6-558	A	19:20	19:22	The Roche et al. estimate is based on a 2 dimensional ocean model with the third dimension parameterised. The ocean model has a generally higher sensitivity to freshwater forcing than other models. In general it is incorrect to include quantitative evidence from a single paper using a single model. Similar experiments were carried out using a different (3D) model by: Rohling E. J., Marsh R., Wells N. C., Siddall M. , Edwards N. R., 2004 :Similar meltwater contributions to glacial sea level changes from Antarctic and northern ice sheets, Nature, 430, 1016-1021. These authors found up to 15 m of FW could be injected into the N.Atlantic during FW events and still match the observed d18O there. This work found that their results were highly dependent on the FW forcing needed to 'switch off' the Atlantic MOC, a highly model-dependent variable. I	partly accepted - we cite the example, but added: Volume and timing of freshwater release is still controversial, however.

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				include this example to show that the Roche et al. paper is not consensual and should not be included. It is early days for d18O paleo modelling and these early attempts are likely teaching us more about modelling techniques than quatifying reality. [Mark Siddall (Reviewer's comment ID #: 238-29)]	
6-559	A	19:28	19:29	Suggestion for correction: Modify sentence: "... 8.2 kyr event was probably linked to one or more floods ranging between 0.4 to 1.5 x 10^14 m^3 (i.e., 11 to 42 cm of sea level rise) within a few years (Clarke et al., 2004)." (The values given by Clarke et al. (2004) are to my knowledge the most recent hydrological model estimates but note also in section 6.5.2 the previous estimates of flood volumes which are up to a factor of three larger.) [Eva Bauer (Reviewer's comment ID #: 15-11)]	accepted
6-560	A	19:33	:35	The so-called dynamic ocean thermostat model espoused by Clement et al. 1996 and Cane and Clement, 1999, and others needs more explanation. How about, "Some authors have argued that some of the abrupt climate shifts discussed could have been triggered from the tropics. Based on modeling and supported to some extent by compelling evidence of abrupt climate change in the Pacific sector, Clement and Cane (1999) argue for a dynamic ocean thermostat, whereby seasonal insolation maxima and direct radiative heating of the tropical Pacific actually increases upwelling and cooling of the east equatorial Pacific. This reinforces a steepened east-west sea surface temperature gradient and a semi-permanent La Niña–like state with global teleconnections consistent with much of the global evidence for abrupt climate change. This dynamic ocean thermostat model has been invoked to explain the climate of the early-mid Pliocene (Rickaby and Holleran 2005), the early Holocene (Clement and Cane, 1999), and the last 1000 years (Mann et al., 2005). 168 6-168 391 [Govt. of United States of America (Reviewer's comment ID #: 2023-11)]	rejected - no explanation on this basis for the millennial glacial events discussed here has been provided; the reviewer only provides examples of the Pliocene, the Holocene and the last millennium, neither of which is the topic of this section.
6-561	A	19:33	:35	Rickaby, R.E.M. and Holleran, P. 2005. Cool La Niña During the Warmth of the Pliocene? Science 307, 1948 - 1952. [Govt. of United States of America (Reviewer's comment ID #: 2023-392)]	see previous
6-562	A	19:33	:35	An abridged version of the above would also be acceptable. [Govt. of United States of America (Reviewer's comment ID #: 2023-393)]	see previous
6-563	A	19:38	19:39	What is meant here by a "positive" feedback is not obvious to understand. What happens to CO2 when the overturning rate is reduced ? [Michel Crucifix (Reviewer's comment ID #: 52-47)]	CO2 uptake of ocean is reduced, CO2 concentration rises faster
6-564	A	19:38	19:39	should read: "A relatively small feedback between …. formation IS CONSISTENTLY found …" [Danny Harvey (Reviewer's comment ID #: 101-40)]	rejected - too strong
6-565	A	19:46		The discussion of climate models simulating abrupt events neglects to mention that in fully coupled climate models, e.g. Manabe and Stouffer, the magnitude of meltwater	rejected - the reviewer is not correct. Models with ocean GCM component

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				forcing required is many times greater than the greatest amounts estimated to have actually occurred. Most readers will not recognize that the models e.g. of Rahmstorff or Knutti are very simplified. Throughout this section, the type of model being discussed should be clearly stated. Another more general problem is lack of attention to the fact that the leading hypothesis for abrupt climate changes -- flooding of the North Atlantic -- can readily explain only the abrupt cooling events, whereas it is the abrupt warming events that dominate the records. This is a major challenge for the scientific community and should be discussed openly and clearly. Additionally, the modeling work of Chiang, Battisti et al. on the link between the ITCZ and the D-O events should be cited in this section. [Eric Steig (Reviewer's comment ID #: 252-20)]	(including coupled GCM) and with simplified ocean models do not show that simpler models require systematically less freshwater input; also we do not cite any shutdown experiments by Rahmstorf or Knutti so we do not see the need to describe their specific models here. We do cite simulations that explain warm events. Concerning the ITCZ shift, we cite several papers already, space is limited and the reviewer does not suggest a specific additional paper to cite.
6-566	A	19:53		Could "NADW formation" be replaced by "the Atlantic MOC". That would be consistent with earlier notation, and cut down one acronym. Or is there a real distinction between the two? [Adrian Simmons (Reviewer's comment ID #: 242-100)]	accepted
6-567	A	19:55	19:55	Could add Manabe and Stouffer (1995) to the list. Manabe, S., and R. J. Stouffer, 1995: Simulation of abrupt climate change induced by freshwater input to the North Atlantic Ocean. Nature, 378, 165-167. [Ronald J Stouffer (Reviewer's comment ID #: 258-11)]	rejected - can't cite all, chose to select three recent, post-TAR papers.
6-568	A	20:9	20:9	Clarify, for example between parentheses, what is the amplitude of the total change in N2O [Michel Crucifix (Reviewer's comment ID #: 52-48)]	Accepted
6-569	A	20:9	20:9	The model by Goldstein et al. did take into account only the oceanic nitrification source of N2O. Oceanic denitrification could be responsible for part of the variation, too. Therefore, delete the words 'a large' on this line. [Reto Knutti (Reviewer's comment ID #: 133-62)]	Accepted.
6-570	A	20:16	20:16	This explanation is non-unique and this MUST be made clear. Other equally likely candidates include the see-ice mechanism, see papers by Eli Tzipperman and the thermal FW seesaw, Knutti et al. 2004, Stocker and Johnsen 2003. [Mark Siddall (Reviewer's comment ID #: 238-30)]	rejected. We say "may be explained" - this in no way suggests this is the only possible explanation. And both sea-ice (as an important amplifying feedback - it cannot explain changes by itself) and the seesaw are in fact important components of the mechanism cited here.
6-571	A	20:16	20:19	The "stochastic resonance" model of Alley et al. referred to here has been shown to be	rejected. we cite Alley et al as "showing

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				statistically unsupported by the data. Alley et al. used an inappropriate white noise background as their null hypothesis, where standard procedure would be red or colored noise. Roe and Steig (2004) showed that if the more reasonable noise background estimates are used, then the stochastic resonance hypothesis fails to meet statistical confidence. Ditlevsen (J. Climate, 2005) repeated this result, and further showed that the statistical significant of the 1500-year cycle (upon which the stochastic resonance hypothesis depends) was weak. Subsequent work on the North GRIP ice core has further shown that the 1500-year cycle is likely an artifact in the GISP2 ice core (this paper is not yet in press, to my knowledge). These papers should be cited and a more balanced discussion given, if the stochastic resonance hypothesis (which has no basis in climate dynamics) is discussed, despite being discredited. [Eric Steig (Reviewer's comment ID #: 252-21)]	evidence" which suggests stochastic resonance "could have triggered" the events - we do not say this is proven. What Roe and Steig as well as Ditlevsen et al suggest is merely that stochastic resonance is not statistically proven - Alley would be the first to concede that, and so do we. We therefore see no problem with our wording. By the way, the stochastic resonance hypothesis does have a basis in climate dynamics; the mechanism can be shown to work in dynamical climate models (see Ganopolski, Phys. Rev. Let. Phys. Rev. Let. 88(3), 038501).
6-572	A	20:16		this is only one possible explanation for meltwater pulses - Hyde and Crowley demonstrated it could siimply be a response to linear stochastic variations in ice sheet mass balance due to standard atmospheric variability - paleoc. 2002 v 17 doi:10.1029/2001/PA000669, 2002 [Thomas Crowley (Reviewer's comment ID #: 51-30)]	Noted. We write "could have", so do not present this as the only possibility.
6-573	A	20:16		Should read, "although the trigger for the ocean circulation changes remains undetermined." [Govt. of United States of America (Reviewer's comment ID #: 2023-394)]	Accepted
6-574	A	20:21	20:22	The idea that "climate models tend to underestimate the size and extent of past abrupt climate changes" attributed to Alley et al. (2003) is an opinion, not a scientifically demonstrated fact. If this statement is to remain in the document, it should be balanced by the point that "Other authors argue that the magnitude and extent of past abrupt changes, as evidenced in the proxy data, is smaller than generally stated (Wunsch, QR, 2006). 735 6-735 22 [Eric Steig (Reviewer's comment ID #: 252-394)]	Noted. We do in fact provide critical and balancing laguage, when we write: "However, such a general conclusion is probably too simple, ..."
6-575	A	20:35	20:36	The issue of future likelihood should be in Chapter 10. Not here. [Gavin Schmidt (Reviewer's comment ID #: 227-6)]	Rejected, this is a valid cross-reference, chapter 10 has likewise references to what happened in the past, etc.
6-576	A	20:35	20:35	consider "yet been FULLY understand" [Michael Schulz (Reviewer's comment ID #: 229-15)]	accepted
6-577	A	20:38		Section 6.4.3 needs an introduction that simply defines the salient issues to be addressed.	OK, will add if length constraints allow

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				This should be cross-referenced to sea level discussions in Chapter 5. [Govt. of United States of America (Reviewer's comment ID #: 2023-395)]	
6-578	A	20:40	20:51	The current understanding for the Holocene sea-level changes is that at least 3m of ice volume equivalent sea-level is required to explain far-field sea-level observations such as Australia and South China Sea (Nakada and Lambeck, 1989; Lambeck 2005). Direct observation of the Antarctic ice sheet using cosmogenic radionuclides also supported this as continuous melting of Antarctic ice sheets during the last 9000 years (Stone et al., 2003 Science). [Yusuke Yokoyama (Reviewer's comment ID #: 298-2)]	There is no doubt that "at least 3m of ice volume equivalent sea level" rise has occured during the last 9000 years of Earth history. In fact the best estimate over this range of time is approximately 25m. However, a 3m rise could not have occured in the past 4000 years otherwise the 2m highstands observed on equatorial Pacific islands would not be observed as stated in the text and for which refences are provided (e.g. Peltier 2002, QSR 21, 377-396; Peltier et al 202,GJI 148, 443-475).
6-579	A	20:40		Section 6.4.3.1 is repetitive of what is discussed in Chapter 5, where glacial isostatic adjustment is already discussed (in more than one place - see comment #94) and indeed given an acronym which is not used here. [Adrian Simmons (Reviewer's comment ID #: 242-101)]	The acronym GIA was employed in the previous draft of the Sectiion but was removed by the co-ordinating lead authors-it will be re-introduced. Discussin of the GIA process belongs much more naturally in the chapter on paleoclimate than elsewhere.
6-580	A	20:44	22:3	Does the range of SL rise over the past 2000 years prior to the 20th century stated on page 22, line 3 (0 - 0.2 mm/yr) fall outside the range of glacial isostatic adjustment estimated by models stated on page 20, line 47 (-0.28 mm/yr to -0.36 mm/yr) because of natural forcings during this time period? If so, it might be helpful to note this. [Melinda Marquis (Reviewer's comment ID #: 162-10)]	It is well outside this range and in fact has the opposite sign! It is important to understand, however, that the adjustment in the range -0.28 to -0.36 mm/yr is one that is to be applied to the Topex/Poseidon measurment of the rate of absolute sea level rise, i.e. the average value of the rate of sea levl rise measured with respect to th centre of mass of the planet over the range of latitude from 67S to 67N. The number (0.0-0.2) mm/yr is an estimate of the rate of sea level rise, averaged over the entire surface area of the oceans that

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					could have been occuring over the past 2000 years due to the melting of land ice.
6-581	A	20:46	20:48	I carefully looked at Peltier and Solheim (2002) (and even looked at Peltier and Solheim (2004)) but could not see anything related to the contribution of Holocene melting of Antarctic ice to the present-day sea level rise: the cited paper is discussing simulation results of the Last Glacial Maximum climate obtained with the Community Climate System Model of the NCAR with bounadry conditions given by the ICE-4G model of Peltier. The citation must thus be corrected. [Claire Waelbroeck (Reviewer's comment ID #: 279-3)]	Several attempts have been made to correct this reference previously by comunication with the co-ordinating lead authors but the change was never implemented. Hopefully, with the help of this comment, the correction will finally be introduced
6-582	A	20:46	20:49	Overall, the work of Peltier is overcited in these three lines. Results from other earth models should also be cited as this is an assessment report of the current state of research and not someone's personnal list of publications. See also comments below. [Claire Waelbroeck (Reviewer's comment ID #: 279-4)]	A reference to the work of Siddal et. al. will be added, although this work was built-in to the Waelbrocke et al analysis and this is already cited
6-583	A	20:47	20:47	-0.28 to -0.36: Clarify that this is a global correction. [Michel Crucifix (Reviewer's comment ID #: 52-49)]	This was clearly stated in the originasl draft of the material for this section but the wording was changed by the co-ordinating lead authors. The original wording will be replaced.
6-584	A	20:47	20:47	What is Kurt Lambeck or Greg Milne's estimate? [Mark Siddall (Reviewer's comment ID #: 238-31)]	This is an estimate of the correction that would have to be made to the Topex/Poseidon measurment of the rate of absolute sea level rise in order to correct for the influence of the GIA effect. Neither Lambeck nor Milne have estmated this correction to my knowledge. W.R.P.
6-585	A	20:48	:51	Statement is cryptic and potentially incorrect; needs to be rewritten. This should be cross-referenced to quantification of TOPEX/Poseidon corrections in Chapter 5 as they are not cited here. [Govt. of United States of America (Reviewer's comment ID #: 2023-396)]	The magnitude of the correction to the T/P data provided here is supposed to have been referenced also in chapter 5. It will have to be added there.
6-586	A	20:51	20:51	"T/P" is confusing; spell out. [Michael Schulz (Reviewer's comment ID #: 229-16)]	It was spelled out in the original text of this section but was removed by the co-ordinating lead authors
6-587	A	21:0		Figure 6.8. The last sentence of the caption must be removed for the reasons outlined in the two previous comments. Here again, the text is misleading and attempts to convince the reader that ICE-5G(VM2) model results are validated by data: different y-axes should	There is some misunderstanding here. As previously demonstrated in Peltier (2002, QSR 21, 377-396), the relative

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				be used for ice equivalent sea level estimates of Lambeck and Chappell (2001) and for Barbados RSL data of Fairbanks (1989) because ice equivalent sea level is a global value, whereas RSL is local. ICE-5G(VM2) model curve should indeed be compared to Lambeck and Chappell (2001) estimates if it is a global value. [Claire Waelbroeck (Reviewer's comment ID #: 279-9)]	sea level curve from the island of Barbados itself provides a good approximation to the global ice equivalent eustatic sea level history(appropriately defined). This will be made clear by modifying the Figure so as to include the eustatic curve for the ICE-5G model so that the reader may judge for her/himself. A crucial point that follows from the extended RSL curve from the island of Barbados of Peltier and Fairbanks (2006, in press) is that there is no evidence at this location for the existence of the meltwater pulse of 19 ka age that was postulated by Yokoyama et al (2000). The extended record includes data that extends through this time interval whereas the Record published in Fairbanks (1989) did not and so Yokoyama et al were at liberty to make this hypothesis, as mentioned in Peltier (2002, QSR 21, 377-396).
6-588	A	21:4	21:27	At first, the author of this section confuse to use term "eustatic sea-level" and "ice-volume equivalent sea-level". They should be separately used and one have to realized the importance of this differences. 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 has serious problem since the area has been undergone glacial isostatic adjustments due to the Laurentide ice sheets melting. Therefore Lambeck et al. (2002) 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). Relative sea-level curve are different spatially on earth surface so presenting the ice volume equivalent sea-level curve is more relevant. During the course of their compilation of global rsl data (Lambeck et al., 2002), only one	The assertion that the island of Barbados has undergone significant "glacial isostatic adjustment due to the Laurentide ice sheets melting" is simply incorrect. Since this site lies on the trailing edge of the pro-glacial forebulge that exists outboard of the LIS, it in fact has experienced very little influence of GIA but rather itself measures a very good approximation to ice equivalent eusttic sea level history. This is demonstrated on the revised version of the Figure which directly superimposes the ice equivalent eustatic

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				outlaying data was found at the lowest part of the Sunda Shelf data that was in fact radiocarbon data from organic material extracted from chemical reaching from sediments but not from carbonate fossils such as molluscs and corals (Hanebuth PhD thesis, Univ Keil). Therefore we (as international comunity) know that the age determination may have had influenced from "groundwater effects" to shift the age older than the actual data. There are some indication of this at the Melt Water Pulse 1a (Mwp 1a)event. The timing of the Mwp 1a should be the same as in both Barbados and Sunda Shelf but Sunda Shelf data are rugged consistently from Barbados corals (Weaver et al., 2003 Science v299 p1709). In any cases, 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). 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 Waelbeck et al (2002, QSR v21 p295) and Rohling et al (1998, Nature v394 p162). 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). Therefore large number of researchers in the Palaeoceanography and Palaeoclimatology now recognize the magnitude of the LGM sea-level was larger than 120m. Concerning the rapid rise in the sea-level after the LGM at 19ka, 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). Independent numerical analysis using wavelet methods also predicted 19ka termination of the LGM (Hargreaves and Abe-Ouchi, 2003 Paleoceanography v18 Article no.1035). [Yusuke Yokoyama (Reviewer's comment ID #: 298-3)]	curve for the ICE-5G model of the deglaciation process upon the ICE-5G(VM2) model prediction of local RSL history. By ice equivalent eustatic sea level history is meant the history of the change in water depth derived by transforming the time variation of continental ice sheet mass into equivalent ocean volume by accounting for the difference between the densities of ice and ocean water and then dividing the time dependent ocean volume so derived by the assumed time independent surface area of the oceans. To include the time dependence of the surface area of the oceans in this calculation would be to introduce an influence due to the visco-elastic model of the Earth employed to predict the GIA effect—the eustatic sea level history thereby determined would not then be "ice-equivalent". The utility of the Bonaparet Gulf data as a means of inferring the LGM lowstand of the sea has been questioned seriously by Shennan and Milne (QSR, 2003). The Figure presented in this chapter implicitly includes the constraints provided by the pore water measurments of Adkins et al because this informatin was itself employed in the reconstruction by Waelbroecke etal (2002)
6-589	A	21:6	21:10	This statement is not correct and should be removed. In his QSR 2002 paper Peltier uses a version of his earth model that he has tuned to the Barbados relative sea level (RSL) record. This tuned earth model is NOT able to reconstruct the Tahiti, Huon Peninsula, Bonaparte Gulf and Argentine Shelf records (see Fig. 4, 6 and 7). In his last § he acknowledges that examples exist in the current literature of misfits of his model to RSL	The Earth model employed to make the predictions of the GIA effect has not been tuned at all using the Barbados data set. This data set has been employed only to check to see that the

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				observations in certain locations. Therefore, it appears that tuning to the Barbados record does not allow to reconcile all available RSL records around the globe. Note that the citation of Peltier and Solheim (2002) is wrong here again. [Claire Waelbroeck (Reviewer's comment ID #: 279-5)]	net amount of water that has been added to the oceans based upon the local analysis from all of the glaciated regions is acceptible. The parameters of the Earth model are determined by a combination of seismology for the elastic component of the structure and by the anlaysis of the ralaxation time data that can be extracted from ralative sea level histories recorder on the landscape in previously glaciated regions. The methodology employed to perform these analyses was fully reviewed in Peltier (1998). The problem with the Tahiti record as a means of deducing the depth of the LGM low stand of the sea, which is the purpose of this section of chapter 6, is that it does not extend beyond the end of wmp1-a and so provides no constraint upon the depth of the low stand. The probelm with the Huon record is that it is so strongly influenced by the local rate of tectonic uplift that the data are not helpful in this regard either. The Bonaparte Gulf Record, as pointed out in Shannan and Milne (2003, QSR) is based upon data from cores across which stratigraphic continuity has not been established and which, in any event, contain a sharp discontinuity that is ruled out by the extended Barbados record presented in Peltier and Fairbanks (2006, in press).
6-590	A	21:9	21:10	Lambeck et al. (QSR 2002a) have proposed a computation of the global change in ocean and ice volumes since the last glacial maximum (LGM) that reconciles available RSL records from seven different regions. To do so, they chose parameters of their earth model so as to minimize discrepancies between the individual estimates for each region. They	If Professor Lambeck would provide access to his preferred model of the Earth plus the time dependent ice load that he believes it was subjected to, as

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				show that there is a strong gradient in the isostatic effect across the Caribbean region due primarily to the glacio-eustatic contribution of the Laurentide ice sheet implying that Barabados can not be assumed to be an equivalent of the ice equivalent eustatic curve (Fig. 2), contrarily to what claims Peltier (2002). Additional reference: Lambeck, K., Yokoyama, Y.Purcell, T., 2002a. Into and out of the Last Glacial Maximum: sea-level change during Oxygen Isotope Stages 3 and 2. Quat. Sci. Rev. 21, 343-360. [Claire Waelbroeck (Reviewer's comment ID #: 279-6)]	is the case with the ICE-5G(VM2) which is in very wide use internationally by both the climate dynamics and geodesy communities, then it would be possible to assess its reasonableness. What we must assume in the absence of such openness is that his Earth model continues to incorporate the very high value of the lower mantle viscoity he has advocated in the past, a value that is entirely excluded by the voluminous set of RSL data that is available form the region that was once covered by Laurentide ice. Furthermore his model now apparently includes a meltwater pulse equivalent in strength to an ice equivalent eustatic rise of approximately 20m, an event that is ruled out by the exceptional extended set of data from the Island of Barbados that has been assembled in Peltier and Fairbanks (2006, in press). This event is approximately equivalent to "3 Greenlands" and we have never been informed as to where this additional load of glacial ice is to have resided. The ICE-5G model , with its ice equivalent eustatic sea level rise of approximately 120m is in very good accord with the best available glaciological inferences in this regard.
6-591	A	21:10	21:10	Put brackets around "Figure 6.8b" [James Crampton (Reviewer's comment ID #: 50-23)]	OK
6-592	A	21:10	21:13	There is a major problem in this assertion: the value of approximately 120 m can NOT be "inferred (e.g., Shackleton, 2000) on the basis of deep sea oxygen isotopic information"! Shackleton (2000) or Waelbroeck et al. (2002) did not produce any sea level data but only sea level reconstructions calibrated using coral terraces relative sea level data.	Since the reviewers own reconstruction shows an LGM fall of sea level of approximaely 120m, based upon deep sea core del-18O data corrected for the

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				[Claire Waelbroeck (Reviewer's comment ID #: 279-7)]	influence of the variation in deep water temperature, and constrained by coral observations, it is clearly a question what she thinks is the meaning of her own sea level reconstruction. If this reviewer were in agreement with the much deeper low stand estimate of Lambeck and Chappell it would seem to me that her reconstruction should have confirmed its validity. W.R.P.
6-593	A	21:11	21:11	is the precission of the 11.7 m reconstruction really sufficient, to show a value with 4 significant digits? I doubt it. [Michael Schulz (Reviewer's comment ID #: 229-17)]	The number is NOT based upjon a measurment, it is simply, to 4 significant Figures, the value of the ice equivalent low stand of the sea characteristic of the ICE-5G reconstruction.
6-594	A	21:12	21:12	Shackleton, 2000, does not provide any independent information regarding LGM sea level and this citation should not be included here. In fact Shackleton assumed a sea-level lowering of 120 m at the LGM based on Barbados - we must be careful of circular reasoning based on the assumption that the Barbados estimate is correct. [Mark Siddall (Reviewer's comment ID #: 238-32)]	However, the basis of Shackleton's assertion of an ~120 m fall of sea level as characteristic of the LGM state is in accord with the fact that the RSL record at Barbados should record a good approximtion to ice equivalent euststic sea level. Nevertheless there is some possibility of circularity here as the reviewer has suggested and this will be corrected in the re-write if space allows
6-595	A	21:13	21:15	The last phrase 'which scales with the Barbados estimate' could be viewed as correct but I think it is ambiguous in one aspect and is a seriouds miscitation in another. The phrase used could be interpreted as offering new information on LGM sea level (in addition to coral based estimates), especially mentioned in this context. What is more Waelbroeck et al. actually scaled to Yokoyama et al.'s estimate. The phrase should read something like: 'Waelbroeck et al. (2002) produced a sea level reconstruction based on coral evidence and deep sea O-isotopes corrected for the influence of bottom water temperature variations for the entire last glacial interglacial, which is scaled to - 130 m at the LGM (Yokoyama et al. 2000) estimate and therefore offers no indendent information on the magnitude of the LGM low stand (Figure 6.8a).' The lack of independence here means the usefulness of mentioning it in the context of the LGM lowstand is in doubt. The fact is that the	This seems to me to be rather odd in a number of respects. Firstly the RSL history provided to the LA team by Clair Waelbroecke herself is characterized by an LGM lowstand of almost precisely 120m NOT the 130m that one would expect given this reviewer's comment. The error bars surrounding this estimate, which Dr Waelbroecke has also provided, do extend, however, to approximately

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				genuinely independent estimates of the LGM are few and far between. What estimates there are should be included with a fair appraisal of what may be wrong with each. [Mark Siddall (Reviewer's comment ID #: 238-33)]	130m. This section will nevertheless be re-worded in an attempt to capture the spirit of this coment. W.R.P.
6-596	A	21:13	21:15	This sentence is misleading: because I used published relative sea level data from coral terraces in the calibration step of my method, my curve cannot be interpreted as ice equivalent sea levels (see Waelbroeck et al. (2002), section 4, § 2) and has no such value. Only complete earth models like those developed by Peltier or by Lambeck and co-authors can yield estimates of the global impact of ice sheets build up and melting. There is an ongoing debate on the total ice-volume equivalent sea level depression that prevailed at the LGM: Peltier's ICE-5G model yields an estimate of 118.5 m at 21 cal. Ky BP, whereas Lambeck's model yields an estimate of about 140 m at 21 cal. Ky BP (Lambeck and Chappell, 2001; Lambeck et al., 2002a; Lambeck, 2004). Additional reference: Lambeck, K., 2004. Sea-level change through the last glacial cycle: geophysical, glaciological and palaeogeographic consequences. C. R. Geoscience 336, 677-689. [Claire Waelbroeck (Reviewer's comment ID #: 279-8)]	The problem in the literature is that the details of the preferred model of Lambeck et al. have not been made available to the commu nity as have those of the ICE-5G(VM2) model. Where would Professor Lambeck like to store the" ~3 Greenlands" worth of additional ice? We are never informed. A valid model of the GIA process must be able to reconcile not only the global constraints but also those that derive from observations local to the once glaciated regions.
6-597	A	21:20	:51	Avoid use of specialized acronyms such as LIG and GIS. In particular, GIS has another very widely used meaning. [Govt. of United States of America (Reviewer's comment ID #: 2023-397)]	Agreed
6-598	A	21:22	21:22	"that conflicts somewhat with that based upon the extended Barabdos record" is wrong. The correct statement would be " that conflicts with ICE-5G(VM2) results". As explained in comment #5 and 6, Lambeck et al. used RSL data from all around the globe, including RSL Barbados data, to derive their estimate of -140 m for the LGM ice equivalent sea level. [Claire Waelbroeck (Reviewer's comment ID #: 279-10)]	As stated previously the problem is that the model of the GIA process emloyed has never been demonstrated to fit the observations local to the region that was most heavily glaciated at LGM. The problem with the lLambeck et al model is most strikingly manifest in its failure to fit the extended Barbados record which entirely rules out the existence of the 19 ka meltwater pulse that is a crucial component of their argument. To say that their reconstruction fits the requirements of the coral based record from Barbados is simply wrong.
6-599	A	21:24	21:24	"rather than approximately 120 m required by the Barbados data set" is not correct. The sentence should read: " First, the ice equivalent sea level depression is approximately 140 m rather than approximately 120 m computed by the ICE-5G(VM2) model".	This is incorrect as an important signature of the Lambeck et al reconstruction is the 19 ka meltwater

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				[Claire Waelbroeck (Reviewer's comment ID #: 279-11)]	pulse and this is ruled out by the extended Barbados data set of Peltier and Fairbanks (2006, in press).
6-600	A	21:26	21:27	I have looked up Yokoyama et al.(2001) and by my reading this is very unlikely to be the cause of the disagreement. Yokoyama et al. (2001) essentially state that there was a bug in the programme they used but that this makes no difference to their result, they do not event replot their figures because they do not change as a result of correcting the mistake. They state: 'Fortunately, the error does not enter into any other part of the model predictions because the estimate of global sea level rise is based on hte ice volume changes in equation (3).' They conclude by stating: 'The cause for the disagreement must be sought elsewhere, possibly in the different ice and/or earth models used.' The last statement gets exactly at the important point here - we do not know the precise corrections to make for hydro-glacio-isostatic uplift at during glacial periods. Hence estimates using different data sets and models vary (This is described very elegantly here: Potter E.-K., Lambeck K., 2003, Reconciliation of sea-level observations in the Western North Atlantic during the last glacial cycle, Earth and Planetary Science Letters 217, 171-181.). Differences between the Yokoyama et al LGM sea level estimates and the Barbados estimates represent the real uncertainty in this value and should be included with the statement: 'Differences in the LGM values at the two sites may indicate real uncertainty in diffferences in isostatic changes to the relative heights at the two sites in the past' [Mark Siddall (Reviewer's comment ID #: 238-34)]	This is a considered and therfor useful comment. However it misses the central issue. The preferred 140m lowstand estimate of Lambeck et al includes a ~20m contribution from the 19 ka meltwater pulse suggested in the Yokoyama et al (2000) paper. This is ruled out by the extended record from the Island of Barbados. As commented in Peltier (2002, QSR 21, 377-396) this possibility was not ruled out by the Barbados data set as published originally by Fairbanks (1989). However the extended data set contains a large number of samples of corals that provide very important constarints upon the minimum depth to which local sea level (and thus ice equivalent euststic sea level given the location of the Barbados site) could have extended. The difficulty with the suggested last sentence in this reviewer's comment concerns the relative quality of the data sets from Bonaparte Gulf and Barbados. Because there is no estabished stratigraphic continuity between the cores from the floor of Bonaparte Gulf, as pointed out in Shannan and Milne (2003, QSR), there is a very distinct possibility that these samples have been extensively re-worked. The startegy that has been adopted in constructing the ICE-5G(VM2) model has been to make certain that the model fits the "near

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					field" observations of RSL histories as well as the Barbados record.One then tests the global applicability of the model so produced by predicting the RSL histories at sites that were not employed to constrain the model. When this is done the model is found to be in accord with RSL data from the Sunda Shelf (Hannebuth et al, 2001) but not at all with Bonaparte Gulf becuase of the strong meltwater pulse that would have to have occurred in order to reconcile these few data.
6-601	A	21:26	21:27	The last sentence of the § must be removed. It is completely untrue: Yokoyama (2001) and Lambeck et al. (2002b) gave detailed answers to Peltier's comments on this and explained that an error was detected and corrected in their code but that this error did not impact on published results and figures. Additional reference: Lambeck, K., Yokoyama, Y., Purcell, A.Johnston, P., 2002b. Reply to the comment by W.R. Peltier. Quat. Sci. Rev. 21, 415-418. [Claire Waelbroeck (Reviewer's comment ID #: 279-12)]	It is entirely unclear that this is the correct explanation of the error, as the mathematical expressions stated as the basis for the adjustments to the actual depth at which the RSL indicators were found was in accord with the adjustments made, but were in violation of the constraint of mass conservation (see Peltier 2002, QSR 21, 409-414).
6-602	A	21:29	6:30	Is the "Eemian interglacial at ~125,000 years before present" the same period as the "Last Interglacial (LIG, ~129 - 116 kyr ago)," which is referred to throughout Ch. 6 and specifically on page 17, line 11? If so, why are this new name ("Eemian") and new time period (~125,000 years before present) being used here? I suggest sticking with LIG, ~129 - 116 kyr ago, unless you're trying to distinguish the LIG from a different period (in which case, I didn't catch the distinction). [Melinda Marquis (Reviewer's comment ID #: 162-2)]	Accepted
6-603	A	21:29	21:43	The discussion of the Greenland contribution to the last interglacial sea level is still not careful enough because it does not properly present the view recently presented by NorthGRIP Project members 2005. This is a fault shared by the recent Otto-Bliesner and Overpeck papers in Science and it should not be propagated. What is said about models is certainly true. However NGRIP specifically claim that the ice sheet is of similar size today at GRIP, NGRIP, and even in NW and NE Greenland. It is possible to disagree with their line of argument but not to ignore it. If they are right, then the contribution of Greenland cannot be as high as the models suggest, and this should be given as an	Accepted, text has been revised to include this alternative interpretation

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				alternative view. [Eric Wolff (Reviewer's comment ID #: 292-27)]	
6-604	A	21:32	21:32	4-6 m' I think this range should be at least 2-6 m (Stirling et al. 1998) and possibly as high as 2-10 m (Hearty and Kindler 1995)…I am happy with 2-6 m [Mark Siddall (Reviewer's comment ID #: 238-35)]	Rejected, assessment of evidence supports 4-6m
6-605	A	21:34	21:35	NGRIP 2004 certainly did not say that south Greenland became ice free; they specifically suggest that there is still ice, albeit thinner, at Dye 3. Raynaud 2005 is also not a good reference for this question. [Eric Wolff (Reviewer's comment ID #: 292-26)]	Accepted, text has been revised to provide more balance of views
6-606	A	21:39	21:39	Ch. 6 here says Greenland plus other Arctic ice fields contributed between 2 and 3.5 m to sea level rise (as does SPM, page 9, line 27), but the TS (page 33, lines 15-16) refers to a contribution of between 2.2 and 3.5 m. Please make consistent. [Melinda Marquis (Reviewer's comment ID #: 162-100)]	Accepted, all will be revised to no more than 2 to 4 m based on our assessment of several ice model simulations.
6-607	A	21:40	21:41	2-4 C : section 6.4.1.6 speaks about 4-5 C. Note also that Greenland is not necessarily in equilibrium with the interglacial climate because of the long response time of ice sheets. What is meant by "likely" : what sort of uncertaincy does it cover ? [Michel Crucifix (Reviewer's comment ID #: 52-50)]	Accepted 1st part, text corrected to be consistent. Rejected, 2nd part, "likely" defined in TS.
6-608	A	21:40		The 2-4 C warming in Greenland appears inconsistent with the 4-5 C warming discussed in 6.4.1 (page 17, line 20). Authors need to make sure that these inconsistencies are resolved and clarified. Also note that both ranges of values are inconsistent with those in the third bullet under Robust Findings on page 41. Authors should do a global search on temperatures (C) throughout the chapter to ensure consistency among numbers and whether these are based on models or data. [Govt. of United States of America (Reviewer's comment ID #: 2023-398)]	Accepted
6-609	A	21:41	21:43	While it is understandable how the warmth of NH polar regions could lead to deterioration of the much of the Greenland ice sheet, would orbital elements not be leading to the opposite sort of change over Antarctica? Are there hypotheses explaining then how an equivalent of half of the melting of Greenland occurred? I would assume hypotheses might include: rising sea level from the melting of Greenland destabilized some ice shelves and streams in Antarctica; in that the models cited in this report seem to be indicating that warmth would lead to much more snowfall on Antarctica, I guess one could conclude that significant cooling reduced snowfall onto Antarctica, and it was this that allowed SL to rise. If the former is the case, then we should likely be very concerned about an upcoming Antarctic contribution to SL for Greenland melting will be a cause of it; if the latter, then it would suggest that Antarctic snowfall really is sensitive to temperature and so future SL rise might not be so much. Can any hints on this be provided?	Rejected, outside the scope of Chapter 6

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				[Michael MacCracken (Reviewer's comment ID #: 152-262)]	
6-610	A	21:41	21:41	4-6 m' I think this range should be at least 2-6 m (Stirling et al. 1998) and possibly as high as 2-10 m (Hearty and Kindler 1995)…I am happy with 2-6 m [Mark Siddall (Reviewer's comment ID #: 238-36)]	Rejected, assessment of evidence supports 4-6m
6-611	A	21:45		The discussion of the recent results of Overpeck et al. (2006), implying that future warming and its influence on the Greenland ice sheet can be inferred from the paleoclimate modeling results for the last interglacial, may greatly overstate the relevance of these results. The Overpeck et al. results used a state-of-the-art but nonetheless highly idealized ice sheet model, that may not represent the processes correctly. The all important basal conditions of the ice sheet, and resolution of ice stream processes, are simply not realistically simulated yet. Additionally, while Arctic warmth in summer may have been as great at the LIG as in our near future, the radiative forcing during summer was (as stated in the chapter) about 10% greate.r. The effects of CO2 from anthropogenic activities do not come anywhere near this. Without detailed energy balance modeling, which has not been done, it is not at all clear how relevant the LIG results are to the future. While these results should certaintly be discussed, these important caveats deserve more attention. It is also critical that the issue of timescale be discussed. The Overpeck et al. results do NOT tell us how quickly the ice sheet will melt. For policy makers, this is of course the critical issue. [Note that this is all handled much better in the Summary for Policy Makers, and I recommend taking some of the language from there and using it in this chapter.] [Eric Steig (Reviewer's comment ID #: 252-23)]	Noted in revisions
6-612	A	21:46	21:46	Ref. Overpeck: misplaced brackets [Reto Knutti (Reviewer's comment ID #: 133-57)]	Accepted
6-613	A	21:46	:51	This section is poorly written and its point is unclear. The first sentence uses past data to infer future climate and should be deleted. The entire section needs to be rewritten to specifically and clearly define the importance of last interglacial sea level. [Govt. of United States of America (Reviewer's comment ID #: 2023-399)]	Accepted
6-614	A	21:50	21:51	The "analysis of the Earth rotation data" gives, according to page 20, line 56, an UPPER LIMIT of 0.5 mm/yr. Since it is not a definite positive result but just an upper limit, it specifically does NOT support the idea that melt is already occurring (nor the opposite). Please be careful here; there seems to be a selective use of facts here to support a particular idea that remains uncertain. [Eric Wolff (Reviewer's comment ID #: 292-28)]	Considered in revision
6-615	A	21:53	22:3	A more balanced discussion is given in the to sea level rise chapter (10.6). [Govt. of Australia (Reviewer's comment ID #: 2001-316)]	Noted, Ch 6 deals with the paleo-inferences
6-616	A	21:53	22:3	Section 6.4.3.4: while around 8 pages (27-34 of Chapter 6) have been devoted to the	Cross-chapter issues have been

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				thermal hockeystick, only 6 lines appears to be devoted to the sea-level hockeystick. It seems to me to be really important to investigate whether the rate of sea-level rise observed during the 20th century is typical of the previous 2000 years or whether it represents a significant acceleration from a rather weak rise caused by recovery from the last glaciation. Only three references are used to support a view that the rise during the last 2000 years was considerably less that the ~2mm/year observed now. All recent references that give an indication of sea level during the past 2000 years, prior to the 20th century should be given (e.g. 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). [John Hunter (Reviewer's comment ID #: 112-1)]	checked, the space for references is limited
6-617	A	21:53	22:3	(Long comment continued) This is actually pretty well covered in Chapter 5.5.2.5 and represents ones problem associated with spreading the sea-level component through a number of chapters of AR4. Perhaps just a summary and reference to Chapter 5.5.2.5 would be suitable here. [John Hunter (Reviewer's comment ID #: 112-2)]	See above
6-618	A	22:1	22:1	Space between "equatorial Pacific" [James Crampton (Reviewer's comment ID #: 50-24)]	Accepted
6-619	A	22:3	22:3	sea level rise at most 0.2 mm yr-1. This is presumably the eustatic contribution, thus corrected for the isostatic contribution (-0.28 to -0.36). Please clarify. [Michel Crucifix (Reviewer's comment ID #: 52-51)]	Text revised
6-620	A	22:14	22:14	monsoon strength => monsoon dynamics [Michel Crucifix (Reviewer's comment ID #: 52-52)]	Taken into account. Sentence modified.
6-621	A	22:25	22:26	Delete "(see also Section 6.5)" - we're in Section 6.5! [James Crampton (Reviewer's comment ID #: 50-25)]	Taken into account. Reference to Section 6.6.3 on past forcings added.
6-622	A	22:26		tree ring residual 14C" - how is "residual 14C" defined? Bond et al. use the 14C production rate. Suggestion: remove "residual [Raimund Muscheler (Reviewer's comment ID #: 185-4)]	Taken into account. Word removed.
6-623	A	22:26		REFERRING TO SECTION 6.5 DOES NOT MAKE SENSE. [Govt. of Spain (Reviewer's comment ID #: 2019-136)]	Taken into account. Reference to Section 6.6.3 on past forcings added.
6-624	A	22:28	22:28	substantial work is need to disentangle solar from other environmental influences AND interpret them in terms of total solar irradiance. [Michel Crucifix (Reviewer's comment ID #: 52-53)]	Taken into account. Word removed.
6-625	A	22:29		One could add to this discussion: "...and the link to solar irradiance variations remains uncertain"	Accepted. Sentence added.

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				[Raimund Muscheler (Reviewer's comment ID #: 185-5)]	
6-626	A	22:33	22:33	Insert after "gases" "but not, of course, the major greenhouse gas, water vapour, or clouds" [VINCENT GRAY (Reviewer's comment ID #: 88-757)]	Rejected. The word "trace gases" does not apply for water vapour.
6-627	A	22:43	22:43	Insert after "atmospheric" . "minor" [VINCENT GRAY (Reviewer's comment ID #: 88-758)]	Rejected. It is already mentioned "trace gases".
6-628	A	22:43		Section 6.5.1.2: Northern peatlands have accumulated up to 450 GtC during the Holocene, which is a large portion of the (variable) terrestrial carbon inventory during that time period. This C store is large enough to have significant impact on Holocene GHG concentration variations. It is at least as important as forest regrowth and coral reef build-up. Authors should assess the role of northern peatlands (Smith et al. 2004) to determine if it is appropriate to state that terrestrial carbon has remained stable over the past 7000 years. [Govt. of United States of America (Reviewer's comment ID #: 2023-400)]	Taken into account. Sentence on northern peatland added. However, space is not sufficient to discuss the peatland issue in detail. No additional references added for space reason.
6-629	A	22:43		Smith et al. 2004. Science 303: 353-356; Yu et al. 2003; Vitt et al. 2000. Can. J. Earth Sci. 37: 683-693. The Holocene 13: 801-803. [Govt. of United States of America (Reviewer's comment ID #: 2023-401)]	See previous comment
6-630	A	22:56	23:9	"as it did during the previous three g-ig cycles". Here it should be pointed out that we now know that CO2 did not drop by 20 ppm at the start of MIS11 - in fact it increased (Siegenthaler et al 2005). While we can argue about where the correct line up of records between MIS11 and MIS1 should be, we should at least point out that CO2 does not fall in all interglacials. [Eric Wolff (Reviewer's comment ID #: 292-29)]	Accepted. Text has been edited as requested.
6-631	A	22:57	22:57	I am confused by the parenthetical phrase "(in contrast with the observed 20 ppm increase)." Fig. 6.4 (a) shows an increase of CO2 during the Holocene of approximately 115 ppm. Why does the parenthetical phrase refer to a 20 ppm increase during the Holocene? Perhaps you're referring, in the parenthetical phrase, to the 20 ppm increase during the Holocene *prior to the industrial revolution.* If so, I suggest adding this temporal qualifier. [Melinda Marquis (Reviewer's comment ID #: 162-1)]	Taken into account. Term 'Holocene' replaced by 'the past 8,000 years'
6-632	A	23:1		Human activities" should be replaced by "prehistoric agriculture. [Govt. of United States of America (Reviewer's comment ID #: 2023-402)]	Accepted
6-633	A	23:3	23:8	The Ruddiman hypothesis is not "in conflict" with the lack of orbital similarity with the Holocene. In his (numerous) replies, Ruddiman acknowledges stage 11 as a better analogue, and precisely, stage 11 does not invalidate his hypothesis when one considers the precession alignment (which differs from the alignment presented in the EPICA Nature paper). Ruddiman has a global view of the ocean-atmosphere-ice-sheet-biosphere	Noted. Text has been added to clarify that CO2 remained high during MIS11, in conflict with Ruddimann. It was suggested by Ruddimann that land use emissions were directly responsible for

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				system. He considers that the anthropogenic perturbation was amplified by the system, i.e., the anthropogenic perturbation prevented the system to enter glacial inception. With respect to the "natural course" of the system, he says, the ocean warmed and released carbon dioxide. This is why the ?13C varies so little. [Michel Crucifix (Reviewer's comment ID #: 52-54)]	the measured CO_2 rise.We now refer to subsection 6.4.1.5 were these issues are further discussed
6-634	A	23:4		Do not capitalize Industrial. [Govt. of United States of America (Reviewer's comment ID #: 2023-403)]	
6-635	A	23:6		Insert sentence before "This hypothesis requires much larger changes…." as follows: "In a counterpoint to Ruddiman (2003), Broecker (2005) argues that during Marine Isotope Stage 11, which like the Holocene was a time of small orbital eccentricity, atmospheric CO2 stayed above 270 ppm for about 28,000 years (from 420 to 292 kyr B.P.). The Ruddiman hypothesis requires much larger changes…." Broecker, W. S. 2005. The Holocene CO2 rise: Anthropogenic or Natural? EOS, Transactions of the American Geophysical Union 87(3), 27. [Govt. of United States of America (Reviewer's comment ID #: 2023-404)]	Taken into account. Text edited
6-636	A	23:11	23:36	A further problem is that the title of the section (6.5.1.3) poses one question, then answers another. It asks whether any interval was warmer than the present, and gives the answer: There is no conclusive evidence of globally-synchronous warming. This is an evasion. And as far as that goes, there is no evidence of globally-synchronous warming in the present, in that there are regions underoing cooling trends. This seems to be an attempt to mislead readers into thinking that the mid-Holocene optimum was not as warm as the late 20th century, yet there is no evidence provided for such a claim, and the studies as shown indicate the likelihood of the opposite. Even Fig 6.9 could indicate a substantially higher mean temperature in the 7-8 kYbp interval. [Ross McKitrick (Reviewer's comment ID #: 174-23)]	Noted. End of the paragraph rewritten.
6-637	A	23:11	23:36	To prevent any misinterpretations there needs to be a re-write of this section, especially the last sentence. It should read, "When considering the periods of largest temperature changes (Figure 6.9), paleoclimatic records of the Holocene indicate widespread, persistent warm conditions during the mid-holocene, though evidence exists of contrasting patterns between the tropics and the rest of the world. Overall the available evidence is consistent with higher mean temperatures in the mid-Holocene compared to the present." This re-wording should then be used to correct the wording in the executive summary. [Ross McKitrick (Reviewer's comment ID #: 174-24)]	Noted. End of the paragraph rewritten.
6-638	A	23:11	23:36	References for above cells: Junghyun Kim, Ralph R. Schneider, Dierk Hebbeln, Peter J. Müller &Gerold Wefer, "Alkenone-Derived High-Resolution Sea Surface Temperature Reconstruction in the Eastern South Pacific off Mid-latitude Chile over the Past 33 kyr"	First reference rejected. Conference abstracts are not cited in the IPCC report.

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				Journal of Conference Abstracts, Volume 5(2), 584. IM LAGERKLINT, GUNHILD ROSQVIST, OTTO HERMELIN AND KIRK MAASCH, "NEW HIGH-RESOLUTION ALKENONE RECORD OF LAST GLACIAL TO HOLOCENE SEASURFACE TEMPERATURE CHANGE IN THE EAST-EQUATORIAL SOUTH ATLANTIC OCEAN" Geografiska Annaler 87 A (2005); 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. [Ross McKitrick (Reviewer's comment ID #: 174-25)]	Second reference not cited because focus on centennial resolution records (not the case for boreholes).
6-639	A	23:11		Section 6.5.1.3. aims to answer if climate has been warmer than today sometime during the present interglacial. In most localities the influence of increased summer insolation is cleary seen in proxy records. For Scandinavia many records indicate that temperatures were as much as 1-3 degrees higher during summer. But beacuse these periods were not of global scale or consistent through seasons they are not relevant? This section needs to include some temperature estimates to show that regionally temperatures were higher than today in many places. The present post-industrial warming is not consistent through seasons or show a coherent global patterns either. [Govt. of Sweden (Reviewer's comment ID #: 2020-11)]	Taken into account. North Europe temperature change are already included in Figure 6.9.
6-640	A	23:11	:48	This finding is a bit overstated given the data limitations. The time scales of all of the Holocene warming events cited here are different than the time scale of the late 20th century warming. The last interglacial reconstructions discussed in this section are based on regional summaries that include no discussion of relative timing or justification of the underlying assumption of synchronicity. The density of the data and the relative age control among the terrestrial records during the last interglacial is not good enough to treat this period as a single response to a consistent change in climate forcing. The severe limits on dating need to be acknowledged and considered in discussions of climate responses during this period as well as all earlier periods. For the sentence starting on page 23, line 43, replace start of sentence with "Paleoclimatic data reveal that there were places, …". For the sentence beginning page 23, line 44, replace sentence from lines 44 to 45 with "However, current spatial coverage, temporal resolution, and age control of available Holocene proxy data limit our ability to determine if there were 50 year periods of global warmth comparable to the late 20th century." [Govt. of United States of America (Reviewer's comment ID #: 2023-405)]	Taken into account. Text rewritten.
6-641	A	23:24	23:24	"widespread northward expansion". Quantify. Mc Donald 2000 indeed shows excursions of the order of a few degrees (few hundreds of kilometers) but not everywhere. [Michel Crucifix (Reviewer's comment ID #: 52-55)]	Taken into account. "Widespread" removed.
6-642	A	23:27	23:27	Further evidence of an early Holocene warm period in New Zealand is also provided by Williams et al 2005 (Earth & Plan Sci Letters 230, 301-317)	Noted but limited number of references can be cited.

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				[Paul W Williams (Reviewer's comment ID #: 291-3)]	
6-643	A	23:28	23:28	cannot be explained". Replace by "seems paradoxical [Michel Crucifix (Reviewer's comment ID #: 52-56)]	Taken into account. Text modified to "cannot be explained by a linear response to local…".
6-644	A	23:33	23:33	I do not see the link between annual mean insolation at the tropics during the mid-Holocene period and figure 6.5 dealing with the last glacial maximum. [Marie-France Loutre (Reviewer's comment ID #: 148-18)]	Taken into account. The new text refers to the proper figure (Figure 1, Box 6.1)
6-645	A	23:36	23:36	Add at end "But. Of course. None pf these data are really "globally synchronous" because of the poor sample distribution" [VINCENT GRAY (Reviewer's comment ID #: 88-759)]	Taken into account. Last sentence modified.
6-646	A	23:36	23:36	.....(Lorentz et al., 2006). [Atle Nesje (Reviewer's comment ID #: 190-3)]	Accepted
6-647	A	23:36	23:36	"Lorentz" should be spelled "Lorenz" (no T) [Michael Schulz (Reviewer's comment ID #: 229-18)]	Accepted
6-648	A	23:38		Figure 6.9 (which is a substantial revision of Figure 6.7 from the first-order draft(FOD)) The original figure (Figure 6.7 from the FOD) shows a long period, from 3,000 to 10,000 years ago, in which "Siberia and East Russia" are portrayed as being above the preindustrial level by 2?C or more. It is not in Figure 6.9 in this draft. The first-order draft cites this as MacDonald et al, 2000, in Quaternary Research(QR). This citation remains in the captionsd of second-order draft Figure 6.9. My staff enquired to Dr. MacDonald as to why his study was removed from Figure 6.9. Here is his response: "I know of no reason the conclusions regarding Siberia would have changed. The data are extremely robust and the work is widely cited. It is based upon many, many radiocarbon dates from the remains of trees found far north of the present forest—there is very little to question about that. The data came from many different researchers and has consistently been supported by work on other proxies, such as pollen from lake sediments, etc… I think the QR work is highly appropriate and would be sorry to see it excluded for two reasons: 1. It provides for the general timing of maximum warmth across Eurasia and a rough idea of magnitude, 2. The story shows the sensitivity of the northern boreal limits to modest changes in temperature, 3. Positive feedbacks due to boreal forest extension northward are very important for global warming and the data we presented speak directly to this phenomenon. I would add that the 2000 paper shows that the extension of forest in Eurasia in the early through mid-Holocene is very similar to what is anticipated in most global warming projections. I would rather hate to think that this figure was removed because it shows that in this one case (Eurasian forest advance) the Holocene has already experienced an	Taken into accout. North Eurasia data added to the figure.

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				event very much like what is forecast due to greenhouse gas radiative forcing. The perception that 'inconvenient' data were removed from this crucial IPCC report would be a disaster". We then wrote back to Dr. MacDonald, who had not seen the actual illustrations. He responded: "I looked at the figures and it is very curious. The revised figure does not capture the fact that there was a relatively uniform warming response across Eurasia and it thus—in my opinion [is] misleading…. I would finally add that there are a number of studies of a similar nature from Fennoscandia that show the same warming event—in the Holocene marked by treeline advance. It seems to me problematic to leave Eurasia out of this when the evidence is so compelling over such a huge and important high latitude region". Obviously, MacDonald's work has to be re-inserted into this figure. I would anticipate major problems for IPCC if it is not, and he may be the one to object. [Patrick Michaels (Reviewer's comment ID #: 176-21)]	
6-649	A	23:40	23:48	Comment also on the transient modelling of the Holocene : with 2.5 D emics (Crucifix et al., Climate Evolution during the Holocene, a study with an Earth System model of intermdiate complexity, Clim. Dyn., 19, 43-60, 2002; Brovkin et al., Glob. Biog. Cycles; and Wang et al. 2005a) and with 3D emics (Renssen, H. and Goosse, H. and Fichefet, T. Contrasting trends in north Atlantic deep-water formation in the Labrador Sea and Nordic Seas during the Holocene Geophys. Res. Lett. 2005 32 L08711 doi:10.1029/2005GL022462). The 2.5 D emics show the elements of the Earth Response that create a local Holocene optimum (treeline shift, combined influences of ice sheets and vegetation to create an optimum); the 3D shows the contrast between the responses of the Labrador Sea and the Nordic Seas. [Michel Crucifix (Reviewer's comment ID #: 52-59)]	Noted. EMICS are now mentioned in the text.
6-650	A	23:42	23:42	The reference to Y. Wang et al. 2005a is probably an error. This is not a state-of-the-art model. [Michel Crucifix (Reviewer's comment ID #: 52-57)]	Accepted. Reference removed.
6-651	A	23:44	23:44	"... were local temperature was likely as warm of warmer than at the end of the 20th century" : do we speak about models here ? What is the appropriate reference ? [Michel Crucifix (Reviewer's comment ID #: 52-58)]	Noted. Text clarified and proper reference added.
6-652	A	23:44	23:45	Delete from "However" on line 43 to "warming" on line 45> You are in no position to make a statement on any paleo measures "globally" since yous samples are so few and distributed in an unrepresentative manner. The current ones are equally unrepresentative becaiuse they are predoiminantly close to huiman activity, so the two sets cannot be	Taken into account. End of the paragraph rewritten.

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				reliably compared [VINCENT GRAY (Reviewer's comment ID #: 88-760)]	
6-653	A	23:45	23:45	Add at end "poor distribution of samples," [VINCENT GRAY (Reviewer's comment ID #: 88-761)]	Taken into account. End of the paragraph rewritten.
6-654	A	23:48	23:48	references? [Mark Siddall (Reviewer's comment ID #: 238-37)]	Taken into account. End of the paragraph rewritten.
6-655	A	23:52	23:52	Replace "climate change" with "change of climate" [VINCENT GRAY (Reviewer's comment ID #: 88-762)]	accepted
6-656	A	24:4	24:4	.....Scandinavia (e.g., Nesje et al., 2005) (see..... [Atle Nesje (Reviewer's comment ID #: 190-4)]	accepted, the text is simplified
6-657	A	24:10	24:11	Better write "… indicate short or in places perhaps even absent glaciers" (rather than "… show small or absent ..."). A clear distinction must be made between "short" (advance/retreat) and small (volume, area). Length reactions are delayed by typically decades with respect to mass balance. The evidence which forms the basis for reconstructed holocene glacier variability is mostly indirect and related to the glacier tongues (not mass, size or area). This is especially important when comparing past fluctuations with now ongoing rapid changes (mass and area loss much faster than length change). Evidence (drift wood) from today still glacier-covered areas is extremely sparse and uncertain). [Wilfried Haeberli (Reviewer's comment ID #: 94-13)]	Taken into account, some corrections have been done in the text to show that the glacier variations do not directly support the orbital forcing, but coincide with the general trend. The regional aspect of glacial records is underlined in the figure caption.
6-658	A	24:13	24:18	The statement applies not just to decadal-scale variations, but the centennial-scale variations associated with the "Little Ice Age". The same anti-phasing between coastal Northern European and central European regions is observed over these longer-timescales, consistent with the proposition that the NAO is an important driver of glacial mass balance changes on these longer timescales. This is demonstrated by Reichert et al [Reichert, B.K., L. Bengtsson, and J. Oerlemans, Recent glacier retreat exceeds internal variability, J. Climate, 15, 3069-3081, 2002] and should be discussed here. [Michael Mann (Reviewer's comment ID #: 156-16)]	rejected, there is no comparison between the modern and the interglacial volume of ice neither in the bullet no in the text. No data on the sizes of mountain glaciers is avaliable for the last interglacial
6-659	A	24:14	24:14	..driven by complex glacier and climate (mainly precipitation and temperature) interactions. On these.. [Atle Nesje (Reviewer's comment ID #: 190-5)]	rejected, there are no evidence that the retreat might be related to the orbital forcing in 20th century
6-660	A	24:18	24:18	....in the 20th century (Six et al., 2001). [Atle Nesje (Reviewer's comment ID #: 190-6)]	accepted
6-661	A	24:20	24:22	It seems like this section should be consistent with the bullet statement on page 6-3 that notes a decrease in solar insolation over the Holocene should favor the growth of glaciers. The wording should read that "the evidence is not sufficiently well known to identify an	accepted

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				analogous period....." This final statement in the box also seems at odds the earlier statement in the box at lines 10-11 that between 9.0 and 6.0 ka glaciers in some regions were small or absent... [Gregory Wiles (Reviewer's comment ID #: 289-21)]	
6-662	A	24:20		This statement is partly based on the data presented in Box 6.3, fig.1. Here it is stated that the recent glacier recession is a global phenomenon. Even if that is agreed the recession has in most places been going on at least since the early part of the 20th century, sometimes longer (e.g. Luckman and Kearney). Most glaciers used in this compilation have response times > 50 yrs. Most of the recession represented as the sharp up-curve at the right in the diagram represent recession from the advanced positions these glaciers reached during the Little Ice Age centuries and most of the volume loss occurred before 1970, i.e. before the effect of increased emissions of greenhouse gases influenced the radiation balance. It is important to present all aspects of a proxy used to provide a reliable story and this is not the case here. Glaciers are indeed sensitive to climate change but their response is lagged. Of course the recession has continued and in some places accelerated since 1970's but most of the recession shown in the figure is due to a lagged response of post-LIA warming in the first half of the 20th century [Govt. of Sweden (Reviewer's comment ID #: 2020-14)]	accepted
6-663	A	24:24	24:48	Overall, this section feels too descriptive. Better describe mechanisms and/or more generally, better show how these results improve / modify our understanding of climate dynamics and our capacity to predict the future. [Michel Crucifix (Reviewer's comment ID #: 52-64)]	accepted
6-664	A	24:26	24:26	Note that the link between precipitation and monsoon is very likely but not as straightforward as suggested by the text. [Michel Crucifix (Reviewer's comment ID #: 52-60)]	Accepted, the new reference is included, the results of modelling is mentioned
6-665	A	24:31	24:31	Change "models" to "model" [James Crampton (Reviewer's comment ID #: 50-26)]	accepted
6-666	A	24:38	24:38	Soil moisture contributes, and may be actually a major contributor to the change in albedo, but it does NOT counteract the effect of vegetation. They both go in the same direction. [Michel Crucifix (Reviewer's comment ID #: 52-61)]	accepted
6-667	A	24:43	24:43	Which American monsoon ? [Michel Crucifix (Reviewer's comment ID #: 52-62)]	accepted
6-668	A	24:48	24:48	"may be involved in abrupt monsoon fluctuations". Quote references, presumably Claussen and Renssen. Note that Renssen himself has called into question the abrupt shifts shown in his Paleoceanography (2002) paper after a bug has been found in the simulations. Contact him about this.	Rejected. The detaials of glacier variations of the 20th century are discussed in the chapter 4. Here we only briefly discuss the Holocene

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				[Michel Crucifix (Reviewer's comment ID #: 52-63)]	records and provide the longer retrospection. Indeed, the reaction of glacier to climatic changes is delayed and the glaciers act as a low-pass climatic filter, however the modern glacier retreat is clearly contributed by the recent climatic changes. The terminus response time (Paterson, 1994) for a valley glacier of moderate size is estimated from few years to a few decades (Mueller, 1988, Hooker, Fitzharris, 1999, Nesje, Dahl, 2003, Pelto, Hedlund, 2001, Oerlemans, 1994) and is comparable with the accuracy of moraine dating.
6-669	A	25:1	25:4	Mention the snow-albedo feedback as the key mechanisms for cooling amplification (and transform the seaonal forcing in an annual trend), and that this feedback is relevant to future climate change. It is therefore necessary to quantify it properly. See, Crucifix et al., Climate Evolution during the Hlocene, a study with an Earth System model of intermdiate complexity, Clim. Dyn., 19, 43-60, 2002) for a discussion of a separation of the precession and obliquity forcings. [Michel Crucifix (Reviewer's comment ID #: 52-65)]	accepted
6-670	A	25:6	25:15	The fundamental point is a slow cooling trend in the northern oceans, usually reproduced by the models (see, for example, Renssen, H. and Goosse, H. and Fichefet, T. Contrasting trends in north Atlantic deep-water formation in the Labrador Sea and Nordic Seas during the Holocene Geophys. Res. Lett. 2005 32 L08711 doi:10.1029/2005GL022462. [Michel Crucifix (Reviewer's comment ID #: 52-66)]	accepted
6-671	A	25:14	25:15	Gladstone calls on considerable caution on the statistical significance of the results he presents. His paper should actually be read as there is no statistically significant difference between the mid-Holocene and today NAO's. [Michel Crucifix (Reviewer's comment ID #: 52-67)]	accepted
6-672	A	25:17	25:42	The 2 paragraphs do not clearly answer the question posed at the start of this section. Question could be worded better. [Govt. of Australia (Reviewer's comment ID #: 2001-317)]	accepted, the colors are changed
6-673	A	25:27		Here glacier evidence is used, amongst other evidence, to argued that there is no common global climate variability pattern in the Holocene. The fact that maximum Holocene	Rejected, in most cases the lengh changes are unknown, especially for the

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				glacier advances occurred at different times in different places is probably mainly due to the fact that the climatic conditions that cause an advance or retreat were different for glaciers located in different climate regimes (this it actually written on page 23 line 53). [Govt. of Sweden (Reviewer's comment ID #: 2020-13)]	retreats
6-674	A	25:32	25:42	Someone may even wish to cite J. Kirkby, A. Mangini, R.A. Muller, 2004. The glacial cycles and cosmic rays. CERN-PH-EP/2004-027. arXiv: physics/0407005. if finally published. Should one refer to the unlikely extraterrestrial volatile hypothesis ? Deming, D., 1999. On the possible influence of extraterrestrial volatiles on Earth's climate and the origin of the oceans. Palaeogeography, Palaeoclimatology, Palaeoecology, Vol. 146: 33-51. +H63Williams, D. M., Kasting, J. F., & Frakes, L. A., 1998. Low-latitude glaciation and rapid changes in the Earth's obliquity explained by obliquity-oblateness feedback. Nature, Vol. 396: 453-455. Loutre, M. F., & Berger, A., 2000. No glacial-interglacial cycle in the ice volume simulated under a constant astronomical forcing and a variable CO2. Geophysical Research Letters, Vol. 27: 783-786. [Govt. of France (Reviewer's comment ID #: 2010-51)]	First part accepted – the capture is added Second part is rejected – there are very few (if any) non-surging mountain glaciers that increased in the 20th century comparatively to the 19th century.
6-675	A	25:32	25:42	Someone may even wish to cite J. Kirkby, A. Mangini, R.A. Muller, 2004. The glacial cycles and cosmic rays. CERN-PH-EP/2004-027. arXiv: physics/0407005. I do not know if it was finally published, at least one peer review – mine - was strongly negative. Should one refer to the unlikely extraterrestrial volatile hypothesis ? Deming, D., 1999. On the possible influence of extraterrestrial volatiles on Earth's climate and the origin of the oceans. Palaeogeography, Palaeoclimatology, Palaeoecology, Vol. 146: 33-51. In both these cases, it may be appropriate to mention these claims and to be prepared to counter the arguments made in a combination of ignorance, neo-astrological prejudices, and downright bad faith by self-styled skeptics and various non-experts that IPCC is a conspiracy of deep ecologists and who knows what else. Additional – more serious - references ? : Williams, D. M., Kasting, J. F., & Frakes, L. A., 1998. Low-latitude glaciation and rapid changes in the Earth's obliquity explained by obliquity-oblateness feedback. Nature, Vol. 396: 453-455. Loutre, M. F., & Berger, A., 2000. No glacial-interglacial cycle in the ice volume simulated under a constant astronomical forcing and a variable CO2. Geophysical Research Letters, Vol. 27: 783-786. [Robert Kandel (Reviewer's comment ID #: 123-20)]	Accepted, the caption is extended

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6-676	A	25:32		The connection between cosmogenic isotopes and solar activity is more than an assumption. This connection is well understood. [Raimund Muscheler (Reviewer's comment ID #: 185-6)]	Accepted, the explanation is provided
6-677	A	25:39	25:41	The statement is not consistent with the evidence provided elsewhere in this chapter. In particular, much of the centennial-scale variability of the past 1000-2000 years has indeed been related to variations in volcanic and solar forcing. So the use of "century and longer time scale" here is not appropriate. The statement might be more defensible if clearly confined to the discussion of "millennial" scale variability. [Michael Mann (Reviewer's comment ID #: 156-17)]	Accepted
6-678	A	25:39	25:42	The final sentence here may be read to imply that solar, volcanism or internal variability are not drivers in the century and longer climate variations. I think the intention is to say that none of these is solely responsible for such changes. The evidence suggests that they are all viable drivers of climate variability. [Gregory Wiles (Reviewer's comment ID #: 289-22)]	Accepted. Last sentence rewritten.
6-679	A	25:46	26:14	Suggestion for clarity: The studies on the 8.2 kyr event could be structured as follows: i) paleoclimatic evidences of climate anomalies 8200 year ago, ii) hydrological model studies to infer the properties of the likely causes, i.e., of the freshwater flood, iii) climate model studies simulating the induced response. So far, two different climate models were used in studies dedicated to the 8.2 kyr event, i.e., Renssen et al. (2001, 2002) and Bauer et al. (2004). The studies differ in the climate model used and also in the assumption on the meltwater flood volume. Bauer et al. (2004) applied a flood volume of 1.6 x 10^14 m^3 which is one third of the volume in Renssen et al. (2001, 2002) but closer to Clarke et al. (2004), and Bauer et al. (2004) used a pulse duration of 2 years which is shorter than the shortest pulse length in Renssen et al.(2001, 2002). [Eva Bauer (Reviewer's comment ID #: 15-13)]	Taken into account. Text rewritten for clarity.
6-680	A	25:50	26:6	The information about the 8.2 event could be better presented and organised. What is the 8.2 event ? Where is it well recorded with enough time accuracy ? What are the hypotheses ? How have they been tested ? What are the main outcomes of models (unpredictibility and metastates (Renssen, Bauer, LeGrande); complexity of the d18O_precip signal (Werner, and more recently LeGrande, PNAS, 2006) and compensation of sea-water and temperature contributions to the calcite d18O signal (LeGrande). Effect of ocean circulation on 10Be (LeGrande). [Michel Crucifix (Reviewer's comment ID #: 52-68)]	Taken into account. Text rewritten for clarity.
6-681	A	25:50	26:5	the description of the 8.2 kyr event is not very rigorous. Why not quote one of the recent reviews on this topic: either Rohling, E.J., and H. Palike, Centennial-scale climate cooling with a sudden cold event around 8,200 years ago, Nature, 434 (7036), 975-979, 2005 or Alley, R.B., and A.M. Agustsdottir, The 8k event: cause and consequences of a major	Taken into account. Text rewritten for clarity.

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				Holocene abrupt climate change, Quaternary Science Reviews, 24 (10-11), 1123-1149, 2005. [Eric Wolff (Reviewer's comment ID #: 292-32)]	
6-682	A	25:52	25:54	Suggestion for correction: The value "1.6 Sv in 1-2 years into Hudson Bay" appears to be a missprint in conjunction with the references Rensssen et al. (2001) and Nesje et al. (2004). The sentence in line 52-54 without giving values and references would be appropriate. Renssen et al. (2001) assume for their simulations a fixed flood volume of 4.67 x 10^14 m^3 and releases over 10, 20, 50, and 500 years leading to freshwater fluxes of 1.5, 0.75, 0.3, and 0.03 Sv. Nesje et al. (2004) discuss that freshwater fluxes into the North Atlantic and the Artic Oceans are the most relevant factor among other possible factors for the occurrence of the 8.2 kyr event. [Eva Bauer (Reviewer's comment ID #: 15-12)]	Taken into account. Proper reference added.
6-683	A	25:53	25:53	1.6 Sv in 1-2 years: Sv is a flux, i.e. volume per time, so this sounds like volume per time per time. Suggest 'during' or 'for' instead of 'in'. [Reto Knutti (Reviewer's comment ID #: 133-58)]	Taken into account. Sentence rewritten.
6-684	A	25:53	25:54	To support the 1-2 years you need Clarke et al 2004, and not Renssen et al 2001, who used a much longer lasting event. More recent Renssen et al papers do use short pulses but are not quoted here. [Eric Wolff (Reviewer's comment ID #: 292-30)]	Taken into account. Sentence rewritten.
6-685	A	25:54	25:56	This sentence should read as follows, to accurately reflect what is shown in the literature: "The 8.2 kyr event is INTERPRETED as a brief adjustment of the Atlantic meridional overturning circulation, though direct evidence for such changes in limited due to the small magnitude of the meltwater forcing, compared with e.g. the Heinrich events (Bianchi and McCave, 1999; Risebrobakken et al., 2003; McManus et al., 2004)." It is simply inaccurate to state that the 8.2 kyr event is "recorded" as a change in meridional overturning. [Eric Steig (Reviewer's comment ID #: 252-24)]	Noted.
6-686	A	26:1		The McDermott et al result has now been shown to be an artefact (see correction in Science (2005) 309, 1816, and should not be quoted here [Eric Wolff (Reviewer's comment ID #: 292-31)]	Taken into account. Reference deleted.
6-687	A	26:18	28:19	Replace "limiting the vallue" on line 18 to "review as a" on line 19 by "which means there is no legitimate" [VINCENT GRAY (Reviewer's comment ID #: 88-774)]	Rejected, nos basis given for assertion
6-688	A	26:21	26:26	(Page 26, lines 21-23): "The processes … are due to the recording process in the proxy or to an abrupt change in climate." Doesn't this sentence imply that we don't know if the proxies are valid indicators of abrupt climate change at the end of the first half of the Holocene? But the last sentence of this paragraph (page 26, lines 23-26) implies that, to	Taken into account. Sentence removed.

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				the contrary, the (presumably proxy) observations DO suggest that the climate system can change abruptly. These two sentences appear to me to be inconsistent, but perhaps I am missing something. [Melinda Marquis (Reviewer's comment ID #: 162-3)]	
6-689	A	26:28	26:42	What is meant by the title question ? Is it really answered in this paragraph ? [Michel Crucifix (Reviewer's comment ID #: 52-69)]	Taken into account. Question modified.
6-690	A	26:31		Intertropical convergence zone here, but referred to as ITCZ earlier in chapter. [Govt. of United States of America (Reviewer's comment ID #: 2023-406)]	Taken into account. ITCZ used in the text.
6-691	A	26:34	26:34	Wang et al. 2005a is certainly not the appropriate reference (this is an EMIC). [Michel Crucifix (Reviewer's comment ID #: 52-70)]	Taken into account. Wang et al Science 2005 is the proper reference.
6-692	A	26:34	26:35	Explain the link between monsoon and Mediterranean sappropels. The monsoon precipitation is not drained to the Mediterranea. At best, precipitation associated with the sub-Tropical Easterly Jet barotropic instabilities might have fed some northward run off but the link with monsoon is not obvious at all. [Michel Crucifix (Reviewer's comment ID #: 52-71)]	Taken into account. Sentence modified.
6-693	A	26:47	26:47	Delete "and" in front of the word "lake" [Govt. of Thailand (Reviewer's comment ID #: 2021-1)]	Accepted.
6-694	A	27:0	33:	Section 6.6.1.1 (on 2000-yr proxy reconstructions) is a little too long. It can be either shortened or reorganized into 2 or more shorter sections, say on reconstruction history, debate, and new development. [Govt. of United States of America (Reviewer's comment ID #: 2023-407)]	In order to strike abalance and address the issues raised in the review, the length is considered appropriate. Changes are made, however and much text is rewritten
6-695	A	27:0		Fig. 6.10a. Rather than showing the average of 4 European stations I suggest to plot the available averaged European mean land temperature (using much more than just 4 stations) from Luterbacher et al. 2004 and Xoplaki et al. 2005. This continental scale average would provide a more appropriate overview for the last 250 years. The first lead author has the data or they can be obtained prepared from xoplaki@giub.unibe.ch or juerg@giub.unibe.ch. 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. 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. [Jürg Luterbacher (Reviewer's comment ID #: 151-8)]	Taken into account in revision
6-696	A	27:0		Fig 6.10. I here repeat a point made in my comments on the FOD. 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 at 1980. If you want a line that	See responses to the specific points below. Plotting the instrumental data is appropriate here, and the caption and

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				continues up to more recent years that then you must use the 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, many of them declining after 1980. By failing to show this, and including the surface temperature data in black, it constitutes a misrepresentation, since the black line is an invalid forward extrapolation of the proxy data. If the reason for not showing the updated proxies is that they are not considered 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 instrumental data, since that claim has been refuted, as discussed above. McIntyre and McKitrick (2005a,d) 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 failure of the r2 and CE stats is confirmed by Wahl and Ammann. The squared correlation between the MBH long proxies and the instrumental record is nearly zero (MM05a,c). The mean correlation between the long NOAMER proxies and gridcell temperatures in the MBH98 data set (which dominate the pre-AD1450 portion) is -0.08 (McIntyre and McKitrick 2005c), and the RE significance benchmark is above the MBH98 RE score, using all available implementation of the Mann code (McIntyre and McKitrick 2005d). The surface instrumental record cannot be used as a statistically valid extrapolation for the proxies after 1980. [Ross McKitrick (Reviewer's comment ID #: 174-35)]	lines make it clear that the instrumental data are not the same as an extrapolation of proxy data.
6-697	A	27:1	27:2	Changes in orbital forcing nn which sense lead to a weakening of ENSO? Refs? [Mark Siddall (Reviewer's comment ID #: 238-38)]	Noted. Text clarified.
6-698	A	27:11	27:12	Sentence is difficult to understand. Consider to separate "robust in the modern system" by commas. [Michael Schulz (Reviewer's comment ID #: 229-19)]	Noted.
6-699	A	27:15	34:4	Section 6.6.1 continues to be boring and too much space is spent to justify the curve of Mann et al. In particular too much details are given on the papers in pages 29-31 where hemispheric temperature was reconstructed: it is accessible only for specialists [Joel GUIOT (Reviewer's comment ID #: 92-2)]	Rejected – we have sympathy with the reviewer but it was felt that this level of detail was required to show the degree of progress/independence in work, post TAR – a necessity made clear by other comments
6-700	A	27:17	27:17	In addition to all that is said about the several century record, can anything be said about the early 20th century, when the instrument record suggests high latitude warmth, but mainly because most stations were in the warm Atlantic sector.? Is there paleo evidence that makes clear that the whole Arctic was not warm during this period/ if so, this would	Rejected – this level of detail can not be accomodated because of space restrictions and the issue is addressed in Chapter 3

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				be an important point, making it clear that, as the Arctic Asmt indicated, the current Arctic warming is quite different than from the regional warming of the early 20th century. [Michael MacCracken (Reviewer's comment ID #: 152-263)]	
6-701	A	27:17	33:20	Add additional reference Gerber (in reference list) for independent CO2 proxy of temperature. [Govt. of Australia (Reviewer's comment ID #: 2001-322)]	Rejected – reference not considered relevant at this point
6-702	A	27:24	27:24	Insert after "marine)" "but they do not include a contribution from the bias due to poor spatial sampling" [VINCENT GRAY (Reviewer's comment ID #: 88-763)]	Rejected – sufficient detail given in text to Chapter 3, including details of instrumental uncertainty
6-703	A	27:28	27:28	Insert after "variability" "and is subject to a similar additional uncertainty from biased spatial coverage" [VINCENT GRAY (Reviewer's comment ID #: 88-764)]	Rejected – see response to comment 6-703
6-704	A	27:31	27:31	Correction: '… one North American stations, …' --> '… one North American STATION …' [Govt. of Spain (Reviewer's comment ID #: 2019-45)]	Accepted
6-1122	B	27:37		Show the Briffa et al reconstruction through to its end; don't stop in 1960. Then comment and deal with the "divergence problem" if you need to. Don't cover up the divergence by truncating this graphic. This was done in IPCC TAR; this was misleading and d [Stephen McIntyre (Reviewer's comment ID #: 309-18)]	Rejected – though note 'divergence' issue will be discussed, still considered inappropriate to show recent section of Briffa et al. series
6-1123	B	27:37		I don't think that you should show the Rutherford et al 2005 reconstruction. First there is no "Rutherford et al 2005" reconstruction highlighted in their paper, but a variety of alternatives. The networks are duplicates of MBH98 and Briffa et al 2001, so [Stephen McIntyre (Reviewer's comment ID #: 309-19)]	Rejected – the purpose of showing this is to allow comparison with previous reconstructions but using a different spatial field reconstruction technique.
6-1124	B	27:37		If you do show Rutherford et al, you must show their values after 1960, as with Briffa et al 1960. Not to do so gives a very misleading impression. [Stephen McIntyre (Reviewer's comment ID #: 309-20)]	Rejected – Rutherford et al. did not use the tree-ring density data after 1960 so there are no data to show
6-1125	B	27:37		State that the standard errors have been based on calibration residuals and would be much greater if verification period residuals were used. Not to do so is misleading. [Stephen McIntyre (Reviewer's comment ID #: 309-21)]	Rejected -Taken into account in current text elsewhere
6-705	A	27:44	27:44	Insert after "century "However, this can be mostly explained by the bias caused by the proximity of measuring equipment to human activity (see McKitrick,R and P J Michaels 2004: A test of corrections for extraneous signals in gridded surface temperature data " Climate Research Vol 26, pages 159-173" [VINCENT GRAY (Reviewer's comment ID #: 88-765)]	Rejected – statement is unjustified as is discussed in Chapter 3.
6-706	A	27:44	27:46	Delete from "Recent" on line 44 to "that" on line 46. It is redundant	Rejected – section was included in

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				[VINCENT GRAY (Reviewer's comment ID #: 88-766)]	response to earlier round of comments.
6-707	A	27:46	27:46	Replace "was very likely" with "may have been" [VINCENT GRAY (Reviewer's comment ID #: 88-767)]	Rejected – data justify the present wording.
6-708	A	27:53	27:53	Insert after "2004" "McIntyre & McKitrick 2003" [VINCENT GRAY (Reviewer's comment ID #: 88-768)]	Rejected – sufficient references cited in current text.
6-709	A	27:55	32:	This remark concerns the handling of the Mann- "hockey stick". Traditionally we have had a conflict between paleo climatologists and climatologists work with the present climate. Paleo archives consist of proxy data with different time resolution and different coupling to climate parameters. When Mann et al. presented their hockey stick 6-7 years ago they formatted paleodata in such a way that climate modellers could use it. But very few paleo climatologists agreed to the shape of the curve and now a days we have much better data to use. It is therefore natural to describe the Mann curve in a history of science perspective, but not as a valid data set. A good example of a good modern curve is the one presented by Moberg et al in Nature 2005. It can certainly be improved in the future, but it has at least the the variation seen in almost all paleo climate records for the past millennia. In the present IPCC-text the view described is that we have the hockey stick and then later some scientists have raised critical voices. The basic meaning is that the hockey stick is still the number one description of the past millenia. This is not flattering and it certainly mis-credit the report. I believe that it is rather easy to go through the 5 pages and update the spirit of the text and perhaps make some adjustments in the figure captions. [Per Holmund (Reviewer's comment ID #: 108-5)]	Rejected – the Mann et al. curve is included for consistency and to maintain a historical context for the current state of the art. Also, the low-frequency character of the Moberg et al. series is subject to very large uncertainty – though it is also included to provide a comprehensive representation of the range of published results. The current text does not give uncritical support to the Mann et al (1999) curve – it shows other reconstructions and discusses possible reasons (as far is currently possible) for the differences. Conclusions are then drawn on the bases of all the current data.
6-1126	B	27:57	28:2	Disclose that the early portion of the Mann et al reconstruction is dominated by tree rings [Stephen McIntyre (Reviewer's comment ID #: 309-22)]	Rejected – sufficient pertinent detail provided.
6-710	A	28:11	28:11	Add at end "They can also be regarded with suspician because of the poor distribution of samples" [VINCENT GRAY (Reviewer's comment ID #: 88-769)]	Rejected – insufficient justification provided for including this sentence.
6-711	A	28:13	28:20	Soon and Baliunas was ill-conceived in almost all respects. I understand why it is addressed here but this is too kind (how many other papers have caused multiple editor resignations at the relevant journal?). I would also drop the reference to the 'emphasis placed on [MBH] in TAR' comment – it's not relevant from a scientific point of view. A better paragraph would be something like the following: The "hockey stick" reconstruction of Mann et al. (1999) has been the subject of several critical studies. Soon and Baliunas (2003) attempted to challenge the conclusion that the 20th century was the warmest on a hemispheric average scale by surveying regionally diverse proxy climate data. However, by conflating evidence for relatively warm conditions with alternatively	Accepted (in part) – initial phrase to be deleted but general structure of remaining wording to be retained.

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				dry and wet anomalie occurring at any time within a very wide pre-defined period assumed to bracket the "Medieval Warm Period", their qualitative approach precluded any quantitative summary of the evidence at precise times. Subsequent work supported the MBH conclusions about the relative magnitude of mean Hemispheric 20th-century warmth (Mann and Jones, 2003; Osborn and Briffa, 2006). [Gavin Schmidt (Reviewer's comment ID #: 227-7)]	
6-712	A	28:14	28:20	The studies summarized by Soon and Baliunas mostly missed the latter, warmest years of the twentieth century, so did not provide a full century. And, as the twentieth century started anomalously cold in the Little Ice Age, and finished anomalously warm for human-caused reasons, the mean behavior of the century is really not all that interesting. I would prefer not to see such a poor study highlighted at all, but the failings should be pointed out if reference is made. [Richard B. Alley (Reviewer's comment ID #: 4-1)]	Rejected – present wording presents a balanced view.
6-713	A	28:14	28:14	Insert after "(2003)". " showed that the number and distribution of samples was insufficient to derive a meaningful global or hemiepheric average,and they" [VINCENT GRAY (Reviewer's comment ID #: 88-770)]	Rejected – the paper in question did not provide quantitative evidence to this effect.
6-714	A	28:18	28:18	Delete "qualitative" [VINCENT GRAY (Reviewer's comment ID #: 88-771)]	Rejected – the study being cited was qualitative
6-715	A	28:18	28:18	Replace "precluded" by "showed that" [VINCENT GRAY (Reviewer's comment ID #: 88-772)]	Rejected – suggested change would not make sense.
6-716	A	28:18	28:18	Insert after "times". "is futile" [VINCENT GRAY (Reviewer's comment ID #: 88-773)]	Rejected – insufficient eveidence provided to support suggested change.
6-717	A	28:19	28:20	should also cite here: Mann, M.E., C.M. Ammann, R.S. Bradley, K.R. Briffa, T.J. Crowley, M.K. Hughes, P.D. Jones, M. Oppenheimer, T.J. Osborn, J.T. Overpeck, S. Rutherford, K.E. Trenberth, and T.M.L. Wigley, On past temperatures and anomalous late-20th century warmth, Eos, 84, 256-258, 2003. [Michael Mann (Reviewer's comment ID #: 156-20)]	Rejected – sufficient citations already provided
6-718	A	28:20	28:20	Add at end " McIntyre and McKitrick (2003) have identified several serious errors in the calculatioins made for this paper which , when corrected, show higher tempratires for the years 1400 and 1500 than even the upwardly biased 2oth century global surface temperature record" [VINCENT GRAY (Reviewer's comment ID #: 88-775)]	Rejected – other work has shown the reason for this and to rephrase as suggested by reviewer would give erroneous impression of the 'best evidence available'.
6-1127	B	28:21	28:21	This would be an appropriate spot to mention some of the individual proxy studies with results opposing the views of the multiproxy studies, which you should do in the interests of balance I suggest: A number of studies since IPCC TAR have shown elevated MWP temperatures in diverse parts of the world - Europe (Manino et al 2005 Earth and Planetary Science Letters, 235, 741-751; Siberia (Naurzbaev et al 2004 Quaternary	Rejected – the point is clearly made that some areas likely did have warmth comparable or greater than today (but note also that the evidence is equivocal and there are problems of interpreting

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				Research 62, 126– 133.); Finland (Hiller et al 2001 The Holocene, 11, 491-497); California (Millar et al 2006 Quaternary Research. In Press.), but have not been used in the multiproxy studies discussed below. [Stephen McIntyre (Reviewer's comment ID #: 309-23)]	magnitude as opposed to duration of warmth in systems that repond with lags) – all of the suggested references have imperfect dating control and/or effective lagged/smoothed responses that, makes it difficult to interpret the regional magnitude of 'medieval' warmth.
6-719	A	28:26	28:26	There is something wrong with the sentence. Should be "Pettersson (1914)" rather than "(Pettersson,1914)" [Michael Mann (Reviewer's comment ID #: 156-18)]	Accepted.
6-720	A	28:27	28:28	During the warm period when Norse colonized Greenland, in the South Pacific Polynesian voyagers were colonizing New Zealand (about 700 years ago). This time in NZ was also relatively warm compared to today (Williams et al.2004 The Holocene 14(2), 194-208) [Paul W Williams (Reviewer's comment ID #: 291-4)]	Noted but no amendment to text considered necessary.
6-1128	B	28:36	28:56	This entire discussion of Lamb is very biased and hard to justify if you go back and read Lamb's material. [Stephen McIntyre (Reviewer's comment ID #: 309-24)]	Opinion noted – but considered unfounded. The implication that the text was written without reading Lamb's work is bizarre.
6-1129	B	28:38	28:38	You say that "much is not precisely dated". No reference is provided for this allegation. Documentary evidence for commercial vineyards, for example, is well dated. Treeline changes are sufficiently well dated for low-frequency climate change. [Stephen McIntyre (Reviewer's comment ID #: 309-25)]	No change necessary – also see response to Comment 6-1127
6-1130	B	28:39	28:39	The attribution of "different times" is not a point mentioned in Lamb 1965 and hardly seems like one of the hallmarks of Lamb's position. [Stephen McIntyre (Reviewer's comment ID #: 309-26)]	No change necessary – the text correctly cites Lamb's later opinion.
6-1131	B	28:39	28:41	Lamb 1965 stated: The commonest indications from very diverse types of evidence are that prevailing temperatures in many parts of the world at least between 1000 and 1200, and possibly over a rather longer period, were about 1-2 above present values, though probably less in latitudes under about 40 where increased moisture and precipitation is the main indication. The temperature anomaly was evidently bigger, probably 4 C in places, near the coast of Greenland and possibly elsewhere along the rim of the Arctic Ocean....A 20th century parallel is provided by Spitsbergen, where the average annual mean temperature for the 1930-1940 decade wasal most 4 C higher than for 1912- 1920, with corresponding rises of 1.5-2 C in Iceland and on the southeastern and southwestern coasts of Greenland (at Angmagssalik and Godthaab).	Noted – reviewers's quote is correct but does not alter the basis for the current text that deals with the problem of establishing a Northern Hemisphere mean on a common scale for the late 20th century

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				[Stephen McIntyre (Reviewer's comment ID #: 309-27)]	
6-721	A	28:41	28:41	Replace "was" by "may have been" [VINCENT GRAY (Reviewer's comment ID #: 88-776)]	Rejected – no reason given for suggested change.
6-1132	B	28:43	28:43	I have consulted Lamb (1965) and am unable to locate any use of "historical anecdotes". Lamb used historical information, but it is invidious and incorrect to trivialize this by describing this as "anecdotes". [Stephen McIntyre (Reviewer's comment ID #: 309-28)]	Accepted – it was not the intension to imply trivialization – text amended.
6-1133	B	28:44	28:44	"evidence of vegetation changes" should be "evidence of treeline and vegetation changes". [Stephen McIntyre (Reviewer's comment ID #: 309-29)]	Accepted.
6-1134	B	28:45	28:45	Lamb 1965 makes no reference to Greenland ice cores or European tree ring records. The mention of "early" versions is used invidiously here, but the "early" versions of these records are not material to Lamb's conclusions. Moberg also uses an "early" version of bristlecones, but no invidious mention is made there. [Stephen McIntyre (Reviewer's comment ID #: 309-30)]	No change necessary – the text is factual in the revisions and reinterpretations were later made to some of the records Lamb used. Again , the reference to one (early) Lamb work does not provide a sufficient overview of his or subsequent analyses
6-1135	B	28:46	28:49	Lamb (1965) discussed treeline changes and provided a plausible and quantitative interpretation of treeline changes in terms of lapse rate and vegetation changes in terms of latitude. Approaches not dissimilar have been applied in more recent proxy studies e.g. Millar et al 2006; Naurzbaev et al 2004 mentioned above, or even Pollissar et al PNAS 2006 in print. [Stephen McIntyre (Reviewer's comment ID #: 309-31)]	Noted – and point accepted but no change to text required as is discussed in response to Comment 6-1127
6-1136	B	28:48	28:48	You say that there are "complex lags between forcing and response" for "glacier changes" in an invidious way here, but use glacier changes (Oerlemanns) as an indicator later. What's sauce for the goose is sauce for the gander. If glaciers are usable by Oerlemanns, they are usable by Lamb. [Stephen McIntyre (Reviewer's comment ID #: 309-32)]	Rejected – Oerlemanns' model approach explicitly allows for glacial lag response.
6-1137	B	28:48	31:23	Richard Alley told the NAS panel that temperature was by far the dominant control on glacier [Stephen McIntyre (Reviewer's comment ID #: 309-33)]	Noted – and accepted but no change to text necessary.
6-722	A	28:51	28:51	Add at end "There remains the bias due to poor sample size and distribution which casts doubt on the entire exercise" [VINCENT GRAY (Reviewer's comment ID #: 88-777)]	Rejected – the 'doubt' is discussed at length in the current text.
6-1138	B	28:51	28:51	You use the term "largely on the basis of summer temperature inferences", presumably citing Bradley et al 2003a. They do not say this, so your point is unsupported.	Accepted- The dominance of summer responsive proxies in Lamb's thinking

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				[Stephen McIntyre (Reviewer's comment ID #: 309-34)]	was clear from reading his work but the text has been amended to remove the implied literal support from the citation.
6-1139	B	28:56	28:56	Nearly all of the Hughes and Diaz [1994] proxy series have been processed in a way which do not capture centennial trends e.g. the Guiot series and the Serre-Bachet series, the Polar Urals version of Graybill and Shiyatov. In his comment on the FOD, Esper also pointed out that he was "skeptical" about the Hughes-Diaz paper for the same reason. It is irrelevant and should not be used as supposedly refuting Lamb. [Stephen McIntyre (Reviewer's comment ID #: 309-35)]	Rejected – the citation is a correct one and uses considerably more (and more recent) data than the reviewer refers to. It is not irrelevant.
6-723	A	29:3	29:3	"warmer conditions than those that prevailed throughout the 20th century"--given the large temperature changes in the twentieth century, this statement is ambiguous at best. Is the intention to highlight average temperatures in the twentieth century, warmest temperatures, or something else? [Richard B. Alley (Reviewer's comment ID #: 4-2)]	Rejected – the sense is clear i.e. that mean temperatures (summer and winter), and even extreme short –term temperatures may have been warmer.
6-1140	B	29:14	29:14	Osborn and Briffa 2006 was not presented in the First Order Draft and did not meet IPCC policies on publication deadlines. It is being used in violation of IPCC WG1 Policies. [Stephen McIntyre (Reviewer's comment ID #: 309-36)]	Rejected – this paper can be cited under current rules. Nonetheless the Figure has been removed.
6-1141	B	29:14	29:14	Osborn and Briffa 2006 has just appeared and has not been assimilated. It is not an arms-length article to the section lead authors. It is dangerous to make such a prominent display of a graphic from an article which has just appeared and has not been com [Stephen McIntyre (Reviewer's comment ID #: 309-37)]	Accepted – the graphic has been removed.
6-1142	B	29:14	29:14	The caption says that Box 6.4 Figure 1 excludes "those with an ambiguous relationship to local temperature". This is not the case as set out in some following comments. [Stephen McIntyre (Reviewer's comment ID #: 309-38)]	See responses in appropriate sections
6-1143	B	29:14	29:14	One of the most prominent series on the right hand side of Box 6.4 Figure 1 is Mann's PC1, which uses his biased PC methodology. It is so weighted that the series is virtually indistinguishable from the Sheep Mountain bristlecone series discussed in Lamarche, Fritts, Graybill and Rose (1984). These authors compared growth to gridcell temperature and concluded that the bristlecone growth pulse could not be accounted for by temperature, hypothesizing CO2 fertilization. Graybill and Idso (1993) also stated this. One of the MBH coauthors Hughes in Biondi et al 1999 said that bristlecones were not a reliable temperature proxy in the 20th century. IPCC Second Assessment Report expressed cautions about the effect of CO2 fertilization on tree ring proxies, which were not over-ruled in IPCc Third Assessment Report. At a minimum, the relationship is "ambiguous". In addition, I tested the correlation of this series with HadCRU2 gridcell temperature and obtained a correlation of 0.0. Osborn and Briffa say that they themselves did not verify the temperature relationship for this data. Why not? At any rate, in this	Rejected – the purpose of this Figure is to illustrate in a simple fashion, the variability of numerous records that have been used in published reconstructions of large-scale temperature changes. The text is not intended to give a very detailed account of the specific limitations in data or interpretation for each. Furthermore, though there is an ambiguity in the time-dependent strength of the response of Bristlecone Pine trees to temperature variability, there is other evidence that

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				example, the authors have not excluded an important series with a well-known "ambiguous" relation to temperature. [Stephen McIntyre (Reviewer's comment ID #: 309-39)]	these trees do display a temperature response . Right or wrong, Mann and colleagues do apply an adjustment to the western trees PC1 in their (1999) analysis to account for possible CO2 fertilization. Other authors (Graumlich et al ., 1991) assert that the recent rise in some high elevation conifers in the western U.S. could be explained as a temperature response (she can not confirm the LaMarche et al findings). The issue is clearly complex , as will be noted in a new papragraph on tree-ring problems that will be added to the text .
6-1144	B	29:14	29:14	Another prominent series on the right hand side of Box 6.4 Figure 1 is a foxtail series (which interbreed with bristlecones) from a site within a few tens of miles from the Sheep Mountain bristlecone site. They do not explain why two similar series from so close are used, rather than being composited, if they are to be used at all. I checked the correlation of this data to HadCRU2 gridcell temperature and only obtained an insignificant correlation of 0.04. The authors said that they had cited the temperature data incorrectly, that they had actually used CRUTEM2 yielding a correlation of 0.19 and that HadCRU2 data was spurious in its early portion (1870-1887) because there was no station data. However there is station data at GHCN going back to the data in HadCRU2. D'Arrigo et al 2006 considered using foxtails and rejected the use of this data because it did not meet standards of being correlated to gridcell temperature, expressed in very similar terms to Osborn and Briffa 2006. The contrasting views of D'Arrigo et al 2006 certainly establish that the relationship is "ambiguous" and that this proxy should not be used on multiple grounds. [Stephen McIntyre (Reviewer's comment ID #: 309-40)]	See response to comment 6-1143. Some of what the reviewer says may be true , but is as yet unpublished and the current review is based on multiple strands of evidence, among which the results of Mann and colleagues remains relevant.
6-1145	B	29:14	29:14	The beige series which has the strongest closing uptick in Box 6.4 Figure 1 is the Yamal series. When I plotted this series smoothing with a 30-year gaussian filter, I was unable to exactly replicate the uptick shown in this version. I checked the relationship of this series to gridcell temperature and was completely unable to replicate the claimed (0.49) correlation to temperature, obtaining only a correlation of 0.12. The authors here have used data from Yamal, while they used gridcell data from Polar Urals. There is an updated version of the Polar Urals series, usedin Esper et al 2002, which has elevated MWP values and which has better correlations to gridcell temperature than the Yamal series. since very different results are obtained from the Yamal and Polar Urals Updated, again the	See response to comment 6-1143 and note that the Polar Urals and Yamal series do exhibit a significant relationship with local summer temperature.

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				relationship of the Yamal series to local temperature is "ambiguous" [Stephen McIntyre (Reviewer's comment ID #: 309-41)]	
6-1146	B	29:14	29:14	The van Engeln record only starts in 1251 and is a "shorter record" and does not meet the criteria of the caption. It should be excluded. It obviously wasn't scaled over 800-1995. In addition, it uses instrumental information and contributes to a backdoor use of instrumental information, lending a false authority to the proxy records. [Stephen McIntyre (Reviewer's comment ID #: 309-42)]	Accepted – the van Engelen record will be removed.
6-1147	B	29:14	29:14	Overall, relationships to temperature for many of the series in Box 6.4 Figure 1 are weak at best. [Stephen McIntyre (Reviewer's comment ID #: 309-43)]	See response to comment 6-1143
6-1148	B	29:14	29:14	The authors have not demonstrated that Box 6.4 Figure 1 has not been generated by a form of data mining from a larger inventory of random red noise series, [Stephen McIntyre (Reviewer's comment ID #: 309-44)]	See response to comment 6-1143
6-724	A	29:17	29:17	Replace "complex" with "heterogeneous". [Govt. of Australia (Reviewer's comment ID #: 2001-318)]	Accepted.
6-1149	B	29:18	29:18	Folland et al 2001 as referenced has no connection to medieval discussions and should be deleted. If you mean to cite IPCC TAR, this is inappropriate as they did not do any "studies" and relied on other studies. [Stephen McIntyre (Reviewer's comment ID #: 309-45)]	Rejected – this citation contains reference to other studies.
6-1150	B	29:23	29:23	The same problems characterize these other studies as Osborn and Briffa. You should say: It is also possible that the proxies are so noisy that very little can be concluded from such graphs. [Stephen McIntyre (Reviewer's comment ID #: 309-46)]	Rejected – the presentation of data in the Figure in Box 6.4 allows the reader to gauge the hetergeneity of the data and the reference to Figure 6.10 (and text) provides the reader with a realistic interpretation of the analyses of these data.
6-725	A	29:23		Here it is stated that temperatures were 0.1 to 0.2 degrees below the 1961-90 mean and notiecable lower compared to post 1980 temperatures between 950 and 1100 BP. Such detailed temperature estimates as these are tentative and should be avoided. The proxies series used for these reconstructions indicate that this is indeed a reasonable level. However as we are uncertain if we can even provide as accurate temperature records of the present changes it seems strange to provide this accuracy for estimates based on a wide variety of proxies over a wide area for a period 1000 years ago. [Govt. of Sweden (Reviewer's comment ID #: 2020-12)]	Rejected – the text merely indicates the interpretation of the data as they currently exist, with the appropriate caveates and indication of uncertainty.
6-1151	B	29:24	29:24	The term "very likely" appears to spill over to the assertion that "temperatures were between 0.1 and 0.2 deg C below the 1960-1990 mean". The use of "very likely" is not justified, especially to the 0.1 to 0.2 deg C. At the NAS Panel recently, the majority of	Accepted – and at least for high latitudes agree, but for NH as a whole the last sentence of Box 6.4 is justified.

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				presenters (everyone except Mann) stated that they would not say that we knew temperatures 1000 years ago to within 0.5 deg C. The confidence interval calculations for the reconstructions is very problematic. The difference between MWP proxy index values and modenproxy values is razor thin in some reconstructions and it cannot be said that it is "very likely" or even "likely" that MWP values were below modern values. Creating important unresolved uncertainties are the high medieval tree lines - higher than modern tree lines. I personally think that the probability of MWP temperatures being higher than mid-20th century temperatures range between likely to 50-50. [Stephen McIntyre (Reviewer's comment ID #: 309-47)]	As for the use of "very likely" , this has been changed to "probably" to avoid possible interpretation according to the IPCC definition (which was not the intention).
6-1152	B	29:24	29:24	In addition, you must point out here the "divergence" between proxies and warmth in the 1980s and 1990s. This was an important topic at the NAS panel. None of the presenters could satisfactorily explain the "Divergence" problem and exclude the possibility of a nonlinear relation between temperature and ring width, with declinign ring widths with greater warmth after a certain point (e.g. Davi et al 2004). This it is impossible to make spliced comparisons i.e. saying the proxy levels in the mid-20th were similar or greater than the MWP; the 1980s and 1990s were warmer than the mid-century, ergo the 1980s-1990s were the warmest of the millennium. D'Arrigo et al 2006 reconstruction failed after 1985; Esper et al 2002 reported similar problems; there is evidence of reduced ring widths in the 1990s in many Alaska sites. Without confiming results from the 1980s and 1990s - and the results to data are against a linear relationship continuiing to these temperatures, it is IMPOSSIBLE to make claims about the relationship to past temperatures. The methodologies need to go back to the drawing board and cannot be used in assertions to which confidence is attached. [Stephen McIntyre (Reviewer's comment ID #: 309-48)]	Accepted – in the interests of "balance" a paragraph will be added to the text in which "problems" in the interpretation of tree-ring records are briefly described. HOWEVER- the rest of the point is Rejected. Note that there is not convincing evidence that the "warmth threshold excedence" put forward by D'Arrigo et al. (2004), Davi et al. (2004) is ,in fact, real , widespread, or of import for the interpretation of the current proxy data. Even where a "divergence" between temperature and growth trends is apparent , it was not manifest in the earlier (early 20th C) warm period and there is some evidence to suggest that it was unprecidented (Cook et al., 2004). Many tree-ring records no not exhibit this divergence , including several used in the reconstructions cited in this section.
6-1153	B	29:27	29:27	Take the opportunity here to state that proxies tested against the 1980s and 1990s have shown "divergence" (D'Arrigo et al 2006; Briffa et al 2001). This "divergence" is unexplained and it means that the proxies are not calibrated at higher temperatures contrary to the impression left here and elsewhere. [Stephen McIntyre (Reviewer's comment ID #: 309-49)]	Accepted – see response to comment 6-1152

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6-726	A	29:32	29:35	The portion of the sentence saying that the Medieval warming was "even warmer in relation to the less sparse but still limited evidence of widespread average cool conditions in the 17th century" couild be deleted. [Gregory Wiles (Reviewer's comment ID #: 289-23)]	Rejected – prefer to leave it as it corresponds to the introduction to the box, regarding the early concept of MWP in relation to LIA.
6-727	A	29:36	29:36	"as those in the 20th century as a whole," the same issue arises as in my previous comment--the cold temperatures, the warm, the average, or what? [Richard B. Alley (Reviewer's comment ID #: 4-3)]	Rejected – text clearly relates to mean (i.e. average) of the 20th century.
6-728	A	29:37	29:38	cite also here: Mann, M.E., C.M. Ammann, R.S. Bradley, K.R. Briffa, T.J. Crowley, M.K. Hughes, P.D. Jones, M. Oppenheimer, T.J. Osborn, J.T. Overpeck, S. Rutherford, K.E. Trenberth, and T.M.L. Wigley, On past temperatures and anomalous late-20th century warmth, Eos, 84, 256-258, 2003. [Michael Mann (Reviewer's comment ID #: 156-19)]	Rejected – suggested reference does not add to currently cited information.
6-729	A	29:39	29:39	At some point in the Box, it should be stated that the exisitence of a warm MWP or not, while interesting, has no practical relevence for the detection and attribution of 20th Century climate change. The issue is whether one can explain previous warm periods, but while the uncertianty in the forcings are as large as they are, a warm MWP can easily be accomodated within current ideas about climate response. [Gavin Schmidt (Reviewer's comment ID #: 227-9)]	Rejected – box currently stretching space restriction and this point, while entirely 'correct', is too complex for discussion in this context.
6-730	A	29:40	29:51	This paragraph should be placed before Box 6.4 in order not to break the flow of the main text. This and previous paragraph both deal with criticisms to the hockey stick curve. [Eva Calvo (Reviewer's comment ID #: 37-6)]	Accepted.
6-731	A	29:40	29:40	The statement: "McIntyre and McKitrick (2003) reported that they were unable to replicate the results of Mann, et al." is a misrepresentation. McIntyre and McKitrick (2003) states that the authors had "… substantial success in replicating the MBH98 methodolgy, but some differences remain, possibly due to undisclosed variations in their procedures and assumptions." The specific claim was that the calculations of proxy principle components in Mann at al (1998) were "erroneous." McIntyre and McKitrick concluded that the tempertaure indexes computed using Mann et al (1998) data and methodology were unreliable and could not be used for comparisons betwene current climate and that of past centuries. [Jeff Kueter (Reviewer's comment ID #: 137-66)]	Rejected – the current text represents a factual report of the substantive content of the McIntyre and McKitrick (2003) paper.
6-732	A	29:40	29:51	The opening sentence of this paragraph is a misrepresentation. McIntyre and McKitrick reported that the data as used by Mann et al differed in material respects from what was reported in MBH98, a finding that was upheld by Nature and which led to the Corrigendum of Mann et al (2005), which this paragraph conspicuously fails to mention. As for the inability in M&M03 to reproduce the results of Mann et al 1998, this paragraph fails to cite McIntyre and McKitrick (2005b) which updated the subject and provided a	Accepted - because current text is *apparently* open to misinterpretation, though the subject is too involved and periferal (in the context of further papers regarding the Mann et al (1998,1999) methodology and further

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				detailed, explanatory reconciliation between the M&M03 results and the MBH98 results. In this respect, the suggestion that Wahl and Ammann diagnosed the source of the differences between the M&M and MBH results as an omission of data is false and pejorative, by suggesting that it resulted from a deliberate omission of data. The issues were far more complex, including the failure by MBH98 to explain all their computational steps and a difference in final weighting on a small but influential portion of the (entire) data set. This was all explained in MM05b, more than a year before the Wahl and Amman paper was in press. [Ross McKitrick (Reviewer's comment ID #: 174-27)]	papers providing other reconstructions) to justify over emphasising the many details of the subsequent debate. The text will be further modified to remove the implication the reviewer sees in the use of the word "omission" and to include reference to other MM papers
6-733	A	29:40	29:51	The opening sentence also misrepresents the situation by failing to point out that the "results' of Mann et al were, principally, the supposed findings of unprecedented robustness and statistical significance. Not only have these results NOT been replicated by others, but they have been amply disproven, by teams on both sides. For example, the final version of the Wahl and Ammann paper confirms the findings of M&M05a that the unreported r2 tests show the MBH98 significance claims were untrue (see their Table 1S). The Wahl and Ammann defence of the RE score in MBH98 is based not on material in the CC paper itself, but on a citation to a submission by W&A to GRL which was twice rejected and which remains unpublished. However, the Lead Authors may have been unaware of this point because at the time of release of the SOD, Wahl and Ammann had submitted a version of their paper to the IPCC review web site in which the appendix containing this key information was left out (http://ipcc-wg1.ucar.edu/restricted/review/SOR/SOR-Unpub/Ch06/In_Press/Wahl_&_Ammann.pdf). 577 6-577 28 [Ross McKitrick (Reviewer's comment ID #: 174-27)]	Wahl and Ammann did not submit this paper to the IPCC review site. Rather it was provided to the LAs of Chapter 6 directly. The confusion about which version to cite has now been removed.
6-734	A	29:40	29:51	The second sentence is misleading and pejorative by stating that W&A were able to replicate the results by including all the data, as if to suggest that M&M were so careless as to not use all the data, while failing to mention the reconciling calculations published in MM05b that include all the data in each step (only the weights, as determined by PC algorithms, change, within the range or weights which ought to have yielded robust results according to Mann et al. 2000). The W&A code yields identical results to the McIntyre code (to 9 decimal places) except in one respect. W&A added in a variance rescaling step which is nowhere described in MBH98 or supplementary literature. The W&A program yields temperature PCs identical to those in M&M, but their slightly closer fit in the final NH reconstruction of MBH resulted from their having private information about a final variance rescaling step which Mann et al had omitted from their methodological description, and which was not publicly available, since they refused to release their code. Once this step was added in to the McIntyre code, the results are identical between M&M and W&A, and neither group exactly replicates MBH. The only	Accepted – but note – reviewer is reading too much into the current wording. Differences in the implementation of the PCA inherent in the Mann et al (1998) method, are responsible for whether series were omitted or not in the MM work. It is unfortunate (and misinterpreted) that the reviewer considers that the text is taking 'a cheap shot'. The paragraph has been revised so as to make a dispationate statement of the facts – see also responses to comments 6-1157, a 6-732 and 6-736.

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				improvement W&A achieved in replication came from having private access to an undisclosed MBH programming step. So you have no business using the IPCC report to take a cheap shot at M&M for what amounts to not being telepathic. [Ross McKitrick (Reviewer's comment ID #: 174-29)]	
6-735	A	29:40	29:51	The next two sentences refer to 2 important items: the failure of statistical significance of Mann's results and the biased PC method. But then, with no supporting evidence whatsoever, the first point is dropped by saying "the latter point may have some foundation..." The former point matters acutely and has completely solid foundation, as has been spelled out in papers to which your attention was drawn after the FOD. First, the RE significance benchmark for MBH98 is 0.51, not 0.00, as had been claimed in MBH98. The higher benchmark was established in MM05a and further settled in our exchange with Huybers, notably MM05d, and is the final word in the perr-reviewed literature on the subject. Unfortunately the authors of this section omit any mention of it and ignore MM05d altogether even when referring to Huybers' comment. Second, the r2 test score is 0.0 in the earliest portions of MBH. This is not contested by Wahl and Ammann, indeed they computed the r2 and CE values for each grid step, confirming insignificant skill through to the late 1700s. This is shown in Table 1S of the Wahl and Ammann paper, which was left out of the preprint version supplied to the LA's for the December deadline. For inexplicable reasons, a version without Table 1S is still posted on the IPCC reviewer web page (http://ipcc-wg1.ucar.edu/restricted/review/SOR/SOR-Unpub/Ch06/In_Press/Wahl_&_Ammann.pdf), though at some point after the release of the SOD a different final version (this time including the Appendix and Table 1S) was also posted, so now there are two different versions on the IPCC reviewer web page. Under the circumstances, since LAs have been working off a version that underwent substantial modification after the deadline, we would be justified in demanding all references to W&A be removed. Third, the claim in Wahl and Ammann that an RE benchmark of 0.0 can still be used is not established in their CC paper itself, but is asserted based on a reference to an unpublished submission to Geophysical Research Letters. No pre-print of this paper is available, for the simple reason that their paper was rejected at GRL, twice. Consequently, this paper is not available in the published literature, and no reliance can be placed on it in the AR4. Use of Wahl and Ammann's Climatic Change paper as an indirect means of citing the rejected GRL paper is surely a violation of the IPCC principle. The matter concerning the RE and r2 scores is settled in the peer-reviewd literature: Mann et al 1998/99 presents a climate reconstruction that is statistically insignificant in its pre-1450 portions, thereby providing no quantitative basis for ranking the late 20th century to the medieval period. [Ross McKitrick (Reviewer's comment ID #: 174-30)]	Rejected - the text is based on the authors' interpretation of the current literature (and all papers cited are within current IPCC publication deadline rules). The text gives a balanced view . Please note the following – The MM05d benchmarking method is based on an entirely different analytical framework than that used by MBH98. MBH used the standard method in climatology of making a random time series based on the low-order AR characteristics of the target time series during the calibration period. here the N. Hemisphere mean. This random process is repeated in Monte Carlo fashion and its skill in replicating the actual target time series is evaluated according to any measure of merit in which the investigator is interested. MM's method instead uses the full-order AR characteristics of one of the proxies used in the reconstruction to create pseudoproxies in a Monte Carlo framework. These are then input into the reconstruction algorithm along with white noise pseudoproxies for all the n-1 remaining proxies. This is, in theory, a statistically meaningful procedure, which asks what kind of apparent skill

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					is available in the reconstruction simply from one proxy's noise. However this procedure is not general and would need to be repeated for each proxy set to be examined. Also, it would need the subjective choice of which single proxy should be modelled according to its red noise characteristics each time. Finally, it does not take into account that some of the verifications seen as "skillful" are associated with very poor/exceedingly poor calibrations, which would be rejected on first principles in real world reconstruction applications. This consideration indicates that the 0.51 threshold cited by MM is actually, at least somewhat, overstated. See responses to comments 6-736, 6-1157 and 6-732
6-736	A	29:40	29:51	You refer to the comments of von Storch&Zorita and Huybers, but omit any reference to our rebuttals. This is misleading to readers and unfair to us. If the point is important enough to raise, then treat it properly. Everyone (M&M, vZ&Z, H) agrees that Mann's PC method is biased towards finding hockey sticks in the tree ring data base. The question is whether the PC error "matters" for the final reconstruction. We ourselves explored this in detail in MM05b, which you fail to mention, and we explained in our replies to von Storch and Huybers why their counterarguments do not affect the underlying point. The final reconstruction looks like a hockey stick only if the bristlecones are included in such a way as to dominate the results. This is acknowledged by all parties--M&M, W&A, vonStorch&Zorita, Huybers. All papers on the subject acknowledge this. The reliance on the bristlecones therefore points to the questions of whether the bristlecones are good proxies, whether their influence undermines the claim of robustness in MBH98 and whether they affect the claims of significance. Here, it is significant that the IPCC itself, in the 2nd Assessment Report, examined the topic and specifically warned about the contamination problem in bristlecones. Yet now that the exact problem warned about in the SAR -- a false signal in contaminated data -- has been shown to have undermined	Accepted only as far as the suggestion to cite the reviwer's work more fully. More references will be cited but the text will remain similar in substance. Please note the following – The Wahl-Ammann paper shows clearly that the PC conventions actually have extremetly little impact on the 1400-1449 period of the Mann et al., (1998) reconstruction (MBH). The actual reason that M&M in their 2005 *Energy and Environment* paper get a very different reconstruction for this period is that they indirectly exclude the bristlecone pines by using only N.

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				material prominently emphasized in the TAR, instead of coming clean about it in the AR4 we see a report section that tries to cover it up and cast aspersions on the investigators who discovered it. The reality is that there is very little disagreement remaining among the various authors about the core hockey stick questions. No one defends the bristlecones in print. Everyone agrees that the hockey stick conclusion is not robust to their exclusion (either via direct exclusion or through removal of the PC4 of the NOAMER network). Everyone agrees that the r2 and CE test scores are unambiguously insignificant with or without the bristlecones. Everyone agrees that the RE score is higher when the bristlecones are included. Everyone agrees that a red noise significance benchmark shifts upward when the Mann PC algorithm is used rather than a conventional PC algorithm. Wahl and Ammann argue, implausibly, that only the RE score matters, not the r2. There is no basis in the literature for this. But even if one accepts the claim, they provide no published grounds for setting the significance criterion at anything less than 0.51, which still leaves MBH98 with an insignificant RE even including the bristlecones. That's where things stand in print. [Ross McKitrick (Reviewer's comment ID #: 174-31)]	American tree PCs 1 and 2 in their reconstruction (In this case the PCs were derived from *unstandardized* data input into a PCA algorithm using the variance-covariance matrix). The actual MBH procedure used standardized data. Wahl-Ammann show that when standardized data are used, then the MBH short-segment reference period for centering and scaling does have an impact, but it is very small. Wahl-Ammann do find that the bristlecone pines (BCs)are necessary for 1400-1449 to get a successful verification in the MBH framework. However, Wahl-Ammann also find that over the common (1450-1980) period, the 1400-network results with BCs included gives a reproduction that is very similar to the one determined from the 1450-network where BCs have been eliminated (which successfully verifies). Given this, and because the problematic behaviour of BC series does not occur in the 15th century but is concentrated primarily in the calibration period (which does not change between the cases), these facts alone indicate that the BC data are quite likely not introducing spurious information in the 1400-1449 segment. Wahl-Ammann also make careful arguments for false negative judgements that can be made based on sole application of the r2 statistic in

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					Appendix I. See response to Comment 6-735.
6-737	A	29:40	29:51	The final sentence of this paragraph is disputatious and off-topic. The topic in the paragraph is MBH98/99 and the relative ranking of the late 20th century to the medieval era. The conclusion from the literature is that the hockey stick is insufficiently robust to support a conclusion about such a ranking. The final sentence refers to different studies not considered in the paragraph (and not even listed for the reader) and refers to comparisons of the rate of change, which is not the issue under consideration in the paragraph. The sentence should be deleted. [Ross McKitrick (Reviewer's comment ID #: 174-32)]	Accepted – the sentence has been removed. Though it is certainly not off topic the salient information follows anyway .
6-738	A	29:40	29:51	I propose the following wording for the paragraph, as a more accurate and informative summary of the literature: "McIntyre and McKitrick (2003, 2005a, 2005b) reported that that they were unable to replicate statistical skill and robustess claims of Mann et al (1998, 1999, 2000), additionally reporting use by Mann et al of a biased principal components method that overweighted certain proxies (bristlecones) that were already in question as being affected by CO2 fertilization (Graybill and Idso 1993;Biondi et al 1999, see also SAR). Wahl and Ammann (2006) confirmed the lack of skill in two verification statistics (r2 and CE), but proposed that the RE score was more suitable for evaluating low-frequency skill. They also confirmed that the reconstruction was not robust to the presence/absence of bristlecones, but argued that the addition of bristlecones was necessary to achieve skill in the RE statistic. McIntyre and McKitrick (2005a, 2005d) argued that the traditional "rule of thumb" for RE significance (0.0) did not apply in the presence of the decentered principal component algorithm as used in MBH, and computed a 99% critical value of 0.51 for such a model, implying insignificance in the RE score as well, for the pre-1450 segments of Mann et al (1998, 1999). Bürger and Cubasch (2005) also argued that the MBH98 result lacks robustness by showing that MBH-type reconstructions yield a wide variety of results under slight variations of methodology, none of which could be precluded on an a priori basis. Other proxy reconstructions using different methods to those of Mann have shown high 20th century values relative to the medieval era, but none have made similar claims to statistical skill and none have been shown to skillfully predict warm temperatures in the 1980s and 1990s. In light of the recent debates, the hockey stick graph on its own does not provide a robust basis for claiming that the climate of the late 20th century is warmer than that of the medieval era, and while subsequent studies have obtained visually-similar results, the statistical significance of such a claim has not been established." [Ross McKitrick (Reviewer's comment ID #: 174-33)]	Rejected – see responses to numerous other comments related to this paragraph (6-1157, 6-736, 6-734).
6-739	A	29:40	29:51	References: McIntyre, Stephen and Ross McKitrick (2005a) Hockey Sticks, Principal	Noted – see response to comment 6-

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				Components and Spurious Significance Geophysical Research LettersVol. 32, No. 3, L03710 10.1029/2004GL021750; McIntyre, Stephen and Ross McKitrick (2005b) "The M&M Critique of the MBH98 Northern Hemisphere Climate index: Update and Implications" Energy and Environment 16(1)69-100; McIntyre, Stephen; McKitrick, Ross (2005c) 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 (2005d) Reply to comment by Huybers on "Hockey sticks, principal components, and spurious significance" Geophys. Res. Lett., Vol. 32, No. 20, L20713. Bürger, Gerd and Ulrich Cubasch (2005) "Are Multiproxy Climate Reconstructions Robust?" Geophysical Research Letters, VOL. 32, L23711, doi:10.1029/2005GL024155, 2005. [Ross McKitrick (Reviewer's comment ID #: 174-34)]	738.
6-740	A	29:40	29:51	M&M2003 is a non peer reviewed publication, and as such should not be referenced here. The points raised are almost invaraibly due to misunderstandings, errors in the archived data set (subsequently corrected at Nature) and deliberate obsfucations. The 'state of play' in the MBH versus M&M is that i) claims of non-replicability are completely bogus (Wahl and Amman, 2006; McIntyre's blog), ii) the only substantial criticism was that the PCA normalisation used orginally may have biased the results (M&M, GRL 2005)– however subsequent publications (von Storch et al, 2006; Burger and Cubasch 2006) have demonstrated that this has no actual impact on the final reconstruction. [Gavin Schmidt (Reviewer's comment ID #: 227-8)]	Rejected – it was decided to include references to MM2003 and 2005b in the interests of those readers who wish to follow the historical development of methodological papers relating to Mann et al (1998,1999). The text as revised, will now merely provide a statement that the issue arose but still indicates that this current assessment does not consider the criticisms to have any substantial impact on the interpretation we provide.
6-1154	B	29:40	29:40	We believe that it is important that this particular controversy be accurately represented. The current paragraph does not represent either our claims as represented by us in any publication or an accurate summary of the current status of those claims in the peer-reviewed literature. I propose the following language and will give critical reasons as well: "McIntyre and McKitrick (2003, 2005a, 2005b) reported that that they were unable to replicate statistical skill and robustess claims of Mann et al (1998, 1999, 2000), additionally reporting use by Mann et al of a biased principal components method that overweighted certain proxies (bristlecones) that were already in question as being affected by CO2 fertilization (Graybill and Idso 1993;Biondi et al 1999, see also SAR). Wahl and Ammann (2006) confirmed the lack of skill in two verification statistics (r2 and CE), but proposed that the RE score was more suitable for evaluating low-frequency skill. They also confirmed that the reconstruction was not robust to the presence/absence of bristlecones..[see next item for continuation]. 54 6-54 50 [Stephen McIntyre (Reviewer's comment ID #: 309-8)]	Reject – suggested wording overlong and seemingly phrased in a biased way. The current text (and additional paragraph discussing 'problems' with tree-ring derived data) convey the salient points, hopefully objectively, and cite references for the reader to explore further. See also response to Comment 6-1152 and 6-736

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6-1155	B	29:40	29:40	Suggest: "Other proxy reconstructions using different methods to those of Mann have also shown high 20th century values, but none have made similar claims to statistical skill and none have been shown to skillfully predict warm temperatures in the 1980s and 1990s. Overall, it does not appear possible using currently-available data and methods to say that the climate of the late 20th century is warmer than that of the medieval era by a statistically significant increment or vice versa." [Stephen McIntyre (Reviewer's comment ID #: 309-52)]	See response to comment 6-1157
6-741	A	29:40	:51	Two examples of mischaracterization from this paragraph follow: [Govt. of United States of America (Reviewer's comment ID #: 2023-408)]	See response to comment 6-1157
6-742	A	29:40	:51	McIntyre and McKitrick (2003) reported that they were unable to replicate the results of Mann et al. (1998). [Govt. of United States of America (Reviewer's comment ID #: 2023-409)]	See response to comment 6-1157
6-743	A	29:40	:51	In fact, MM03 stated that there was "substantial success in replicating the MBH98 methodology, but some differences remain, possibly due to undisclosed variations in their procedures and assumptions." Their specific claims were that the calculations of proxy principal components in Mann et al [1998] were "erroneous". They concluded that the temperature indexes computed using Mann et al [1998] data and methodology were unreliable and could not be used for comparisons between the current climate and that of past centuries. [Govt. of United States of America (Reviewer's comment ID #: 2023-410)]	See response to comment 6-1157
6-744	A	29:40	:51	Wahl and Ammann (accepted) demonstrated that this was due to the omission by McIntyre and McKitrick of several proxy series used by Mann et al. (1998). [Govt. of United States of America (Reviewer's comment ID #: 2023-411)]	See response to comment 6-1157
6-745	A	29:40	:51	MM03 did not "omit" any series and Wahl and Ammann (accepted) does not "demonstrate" anything of this nature. On this particular topic, Wahl and Ammann only state the following: "In MM03, the authors describe this result as being developed using the MBH reconstruction methodology, albeit with elimination of a large number of the proxy data series used by MBH, especially during the 15th century." [Govt. of United States of America (Reviewer's comment ID #: 2023-412)]	See response to comment 6-1157
6-746	A	29:40	:51	There is no "description" in MM03 saying that they "omitted" several proxy series used in MBH. Quite the opposite. MM03 reported that some proxy data series said to have been used in MBH were not actually used. Subsequently, they filed a Materials Complaint with Nature, in which Mann et al. admitted that 35 series said to have been used in MBH98 were not actually used. [Govt. of United States of America (Reviewer's comment ID #: 2023-413)]	See response to comment 6-1157
6-747	A	29:40	:51	Consider the following replacement paragraph: "McIntyre and McKitrick [2003, 2005a, 2005b] attempted to replicate exactly the reconstruction of Mann et al [1998] featured in	See response to comment 6-1157 and 6-1154.

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				the SPM of the TAR. While they claimed success in replication of the method except for some details, they also raised statistical questions potentially relevant to determination of the reliability of all reconstruction methods, highlighting the need for greater involvement of statisticians specialized in time series analysis in paleoclimate reconstructions. Firstly, McIntyre and McKitrick [2003] identified sensitivity of the 20th century warming to the presence or absence of specific series bristlecone pines, also considered by Graybill and Idso [1993] as problematic. Secondly, McIntyre and McKitrick [2005a] challenged the reliability of Mann et al [1998], reporting that the earliest portion of the Mann et al reconstruction did not have significant skill under reasonable 'red' noise assumptions. Replies by both Wahl and Ammann [2006], and Huybers [2005], agreed that the RE benchmark statistic is dependent on model assumptions, and can indicate model skill while simultaneously contradicted by r2 and CE statistics, though differ on the appropriate benchmark value for the RE statistic. These and other contingencies in the methodology were elaborated by Bürger and Cubasch [2005] who showed that plausible variations of Mann et al [1998] methodology can lead to a wide variety of results, and argued that verification statistics cannot be used to decide between models. Von Storch and Zorita [2005] also confirmed the bias towards reduced long time scale variability in the Mann et al principal components methodology. These efforts attribute the 'hockey stick' shape of the reconstruction in Mann et al [1998] largely to contestable statistical artifacts in the methodology, but at this point it is unclear to what extent these findings apply to other reconstructions using tree-ring proxies and principal components methodologies." [Govt. of United States of America (Reviewer's comment ID #: 2023-414)]	
6-1156	B	29:40		Also suggest: "Wahl and Ammann argued that the addition of bristlecones was necessary to achieve skill in the RE statistic.McIntyre and McKitrick (2005a, 2005d) argued that the traditional "rule of thumb" for RE significance (0.0) did not apply in the presence of the decentered principal component algorithm as used in MBH, and reported a 99% critical value of 0.51 for such a model, implying insignificance in the RE score as well, for the pre-1450 segments of Mann et al (1998, 1999). Bürger and Cubasch (2005) also argued that the MBH98 result lacks robustness by showing that MBH-type reconstructions yield a wide variety of results under slight variations of methodology, none of which could be precluded on an a priori basis." [Stephen McIntyre (Reviewer's comment ID #: 309-51)]	See response to comment 6-1154 and 6-1157.
6-748	A	29:41	29:42	The use of Wahl and Ammann (accepted) does not comply with WG I's deadlines and all text based on this reference should be deleted. WG I's rules require that all references be "published or in print" by December 16, 2005. Wahl and Ammann was "provisionally accepted" on that date, and not fully accepted until February 28, 2006, at which time no final preprint was available. Substantial changes were made in the paper between	Rejected- the citation is allowed under current rules.

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				December 16, 2005 and February 28, 2006, including insertion of tables showing that the MBH98 reconstruction failed verification with r-squared statsistics, as had been reported by McIntyre and McKitrick in 2003. These tables were not available in the draft considered by WG I when developing the SOD. [Jeff Kueter (Reviewer's comment ID #: 137-67)]	
6-749	A	29:41	29:42	The statement "Wahl and Ammenn (accepted) demonstrated that this was due to omission by McIntyre and McKitrick of several proxy series used by Mann et al (1998)." is incorrect and should be deleted on factual as well as procedural grounds (see previous comment). In their paper, Wahl and Ammenn state: "In MM03, the authors describe their results as being developed using the MBH reconstruction methodology, albeit with elimination of a large number of proxy data series used by MBH, especially during the 15th century." There is no such statement in MM03. Quite the opposite. MM03 reported that some proxy series data said to have been used in MBH98 were not actually used. Subsequently, McIntrye and McKitrick filed a Materials Complaint with the journal Nature. In response to this complaint, Mann et al admitted that 35 series said to have been used in MBH98 were not actually used, but claimed that this did not affect the results. Wahl and Ammenn were able to closely reproduce the original reconstruction when all records were included. However, prior to this, McIntyre and McKitrick (2005a, 2005b) also had reproduced MBH98 results using the flawed principle components method. Wahl and Ammenn reproduced McIntrye and McKitrick (2005a, 2005b), and, in the final version of their paper, also reproduced MM's finding that MBH98 failed r-squared verification. [Jeff Kueter (Reviewer's comment ID #: 137-68)]	See response to comment 6-1157 and 6-1154.
6-1157	B	29:41	29:41	You say that Wahl and Ammann were able to "reproduce the original reconstruction" implying that the reproduced the "rsults". This is completely false. They categorically failed to "reproduce" the MBH claims of statistical skill and MBH claims of robustness to presence/absence of dendro indicators. Their reproduction of a hockey-stick shape used a method almost identical to what we had previously used in our emulations, where e had been emulate the hockey stick shape but only with the flawed PC method OR using a lot of PC series - which enabled the bristlecones to imprint the result. [Stephen McIntyre (Reviewer's comment ID #: 309-53)]	The reviewers opinion is noted and in part accepted – the text in this paragraph is intended to convey a brief and basic assessment of the current balance of evidence regarding the features and likely reliability of the original 'hockey stick'. It is not intended to provide a detailed elucidation of the criticisms or responses, but rather to provide an indication that aspects of the Mann et al (1999) methodology have been challenged and these challenges addressed. This list of references has been extended to include McIntyre and

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					McKitrick 2005b and other minor wording changes made in response to other comments. The reader is also referred to the responses to comments 6-732, 6-734, 6-736, 6-1154 and to the comment 6-740 made by another reviewer.
6-1158	B	29:41	29:41	Wahl and Ammann 2006 did not meet several publication deadlines. Is it fair to use this study when other studies also not meeting publication deadlines were not used? It was not accepted by December 13-15. TSU did not have a preprint by late February. The version available for review was not the same as the accepted verion - in particular, the version made available omitted critical information that MBH98 failed cross-validation r2 and CE statistics. [Stephen McIntyre (Reviewer's comment ID #: 309-119)]	Rejected- the citation is allowed under current rules.
6-750	A	29:41	:42	The use of Wahl and Ammann (accepted) does not comply with WG1's deadlines and all text based on this reference should be deleted. WG1's rules require that all references be "published or in print" by December 16, 2005. Wahl and Ammann was "provisionally accepted" on that date, and not fully accepted until February 28, 2006, at which time no final preprint was available. Substantial changes were made in the paper between December 16, 2005 and February 28, 2006, including insertion of tables showing that the MBH98 reconstruction failed verification with r-squared statsistics, as had been reported by McIntyre and McKitrick in 2003. These tables were not available in the draft considered by WG1 when developing the second-order draft. [Govt. of United States of America (Reviewer's comment ID #: 2023-415)]	See response to comment 6-1158.
6-751	A	29:41	:42	(accepted) should read (in press), pending determination of whether or not WG1 rules regarding inclusion of peer-reviewed articles was violated or not. [Govt. of United States of America (Reviewer's comment ID #: 2023-416)]	See response to comment 6-1158
6-1159	B	29:42	29:42	McIntyre and McKitrick categorically did not "omit" any proxy series used by Mann. Every single one of Mann's series was used. In fact, we used all the series listed in the MBH98 Supplementary Information, whereas Mann et al had themselves "omitted" over 35 series said to have been used in MBH98 (Mann et al Corrigendum 2004). Differences in result exist because of the impact of PC methods on bristlecone weights. [Stephen McIntyre (Reviewer's comment ID #: 309-54)]	See response to comment 6-1157.
6-1160	B	29:42	29:42	Wahl and Ammann 2006 itself does not "demonstrate" that McIntyre and McKitrick "omitted" series. It could not because none were "omitted". They	
6-1161	B	29:42	29:42	There is a fundamental contradiction in what Wahl and Ammann are trying to do. One of the results that we were unable to replicate was the supposed MBH "robustness" to the presence/absence of "all dendroclimatic indicators" - see Mann et al 2000 as well as	See response to comment 6-1157.

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				MBH98. This claim is simply false. It is not robust to the presence/absence of bristlecones. If you "eliminate" the 15th century North American tree rings, you also eliminate the bristlecones and the hockystick doesn't apply. Wahl and Ammann did not demonstrate this; this had been demonstrated by us in McIntyre and McKitrick 2005b. Wahl and Ammann can't claim that they've replicated the robustness claim of MBH, one of its key warranties, while making this argument. [Stephen McIntyre (Reviewer's comment ID #: 309-56)]	
6-1162	B	29:42	29:42	IPCC TAR stated that the MBH reconstruction as having skill in "cross-validation statistics", not just the RE statistic. Mann et al 1998 claimed statistical skill ini verificaiton statistics referrring to the RE, verification r and verification r2 r2, even illustrating the verification r2 statistics for the 1820 step in MBH98. McIntyre and McKitrick 2005a reported that the 15th century step of the MBH reconstruction failed cross-validaiton r2 and CE and other tests. These results have been confirmed in the 2nd version of Wahl and Ammann 2006, but not in the version filed with the Second Order Draft. This has nothing to do with bristlecones being in or out of the reconstruction. The MBH reconstruction fails verification r2 either way. Wahl and Ammann have argued that the verification r2 is not an appropriate measure of low frequency validity. In our opinion, this is a completely ad hoc argument, which will not get much of a hearing from applied statisticians. We think that IPCC would be foolish to rely on this after-the-fact rationalization. [Stephen McIntyre (Reviewer's comment ID #: 309-57)]	See responses to comments 6-1157 and 6-736.
6-1163	B	29:42	29:42	The issue of verification r2 is not just a technicality. The standard errors that enter into the calculation of confidence intervals are also used in r2 calculations. So if you have a very low r2 statistic, you will have very, very wide confidence intervals. With an r2 statistic of nearly zero, you will have no useful reduction of variance from natural variability, whatever that is. So if you have a verification r2 of ~0.0 and you use verificati\on residuals, you are not even clsoe to being able to make a claim about the relation of the MWP to the modern period. [Stephen McIntyre (Reviewer's comment ID #: 309-58)]	See response to comment 6-1157.
6-1164	B	29:42	29:42	McIntyre and McKitrick 2003 already stated that the principal components method in MBH was incorrect, but was unable to precisely diagnose the flaw. In McIntyre and McKitrick 2005a, we precisely diagnosed the error. Neither von Storch and Zorita 2005 nor Huybers 2005 tested the effect using MBH proxies. We did and found a difference. McIntyre and McKitrick 2005b reported that if 5 PCs were used instead of 2 PCs using covariance PCs as recommended in statistical texts in such situations, then you get an MBH-type hockey stick because the bristlecones are in the PCs, but do not with fewer than 4 PCs. Wahl and Ammann obtained a similar results (without crediting our previous	See responses to comments 6-1157 and 6-736.

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				observation of the same effect). So this type of thing matters. This was confirmed and extended by Burger and Cubasch who found a bewildering variety of outcomes with minor variations of MBH methods. [Stephen McIntyre (Reviewer's comment ID #: 309-59)]	
6-752	A	29:47	29:49	There is a severe problem in how this is currently worded. Specifically, the assertion "The latter may have some foundation" has been *definitively* falsified by two other studies that are inexplicably not even referenced in this context. Ammann and Wahl (accepted) is cited above, and yet a key conclusion of that study is ignored here. Wahl and Ammann show explicitly that the Mann et al (1998) reconstruction is not sensitive to how tree-ring networks are represented, as long as correct selection rules are followed (McIntyre and McKitrick did not follow correct selection rule criteria). Moreover, Wahl and Ammann demonstrate that essentially the same reconstruction is achieved whether or not PCA is even used to represent tree-ring networks. This is independently demonstrated by Rutherford et al (2005). So the statement made here is completely indefensible. It must be acknowledged here that both Wahl and Ammann (2005) and independently, Rutherford et al (2005) have explicitly falsified the claim by McIntyre and McKitrick that the main feaures of the Mann et al reconstruction are in any way dependent on how the predictor networks are represented. [Michael Mann (Reviewer's comment ID #: 156-21)]	See response to comment 6-1157.
6-753	A	29:48	29:48	The von Storch paper has been shown to be seriously in error owing to model drift (Osborn et al., 2006, Climate Dynamics) and to failure to replicate the Mann et al technique (Wahl, Ritson and Ammann, Science, 2006; Zorita and von Storch, 2005). I would counsel against respectful citation of a paper so laden with errors. [Richard B. Alley (Reviewer's comment ID #: 4-4)]	Rejected – paper appropriately cited in the context of others.
6-754	A	29:49	29:51	"However" is not appropriate here, as it implies some sort of caveat, which appears to be based on the false statement made above (see separate comment on lines 47-49 of the page page). The previously cited criticism by McIntyre and McKitrick which appears to form the basis of this use of "However", has been decisively refuted by both Wahl and Ammann (2005) and Rutherford et al (2005), and the wording here must be appropriately revised to reflect this. [Michael Mann (Reviewer's comment ID #: 156-22)]	Noted and taken into accoount – wording of this paragraph has been shortened and the 'offending' word does not appear.
6-1165	B	29:49	29:49	None of these other studies has been carefully cross-examined in print yet. You need to note that none of them has laid claim to comparable statistical certainty as MBH. [Stephen McIntyre (Reviewer's comment ID #: 309-60)]	Suggested rejected – wording unnecessary.
6-1166	B	29:55	29:55	For Crowley et al 2003, do you mean Crowley and Lowery 2000? This was mentioned in IPCC TAR but not illustrated. Why has it fallen by the wayside? [Stephen McIntyre (Reviewer's comment ID #: 309-61)]	The reference is correct – the previous reconstruction (C+L 2000) is pre-TAR (and similar anyway).

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6-755	A	30:1	30:3	The so called "hockey stick" was one of three main lines of evidence used in the TAR to justify the conclusion that human activities were the cause of most of the warming observed during the last half of the 20th century. As noted in this chapter, this led to critical analyses of the Mann, et al study that was the most publicized "hockey stick." One of the criticisms of their work was the limited amount of data on which its conclusions were based. The text in this draft indicates that new studies since the TAR "... represent some expansion of the length and geographic coverage of the previously available data." This is a weak statement, suggesting that the expansion of data has not been very great. The reader should be given more information about how much new data has been added to the analysis since the TAR, and why it justifies the strong statement that it is "...very likely that average NH tempertaures were warmer than any other 50 year period in the last 500 years." The TAR conclusion was different, assigning only a likely probability, albeit to the last 1000 years. [Lenny Bernstein (Reviewer's comment ID #: 20-61)]	No change necessary – after reconsidering text it is felt that sufficient detail is provided in the text and Table 6.1 and Figure 6.11 to illustrate what new evidence exists post TAR, and that this evidence is the basis underlying the current conclusions.
6-756	A	30:1	30:3	Proxy studies of Northern Hemisphere temperature, particularly the work of Mann, et al., were a critical part of the support for the TAR's conclusion about the impact of human activities on the climate system. These studies were widely quoted after the TAR. A major criticism of the proxy studies was the limited amount of data they used. This text indicates that ther been "some" expansion of the length and geographic coverage of proxy studies, but does not indicate how much of an expansion has occurred. This is critical information that needs to be included in the chapter. Figure 6-11 shows only scattered data for 1000 A.D, yet the Executive Summary of this chapter, the Technical Summary and the SPM all contain the conclusion that the second half of the 20th century was likely to have been the warmest 50 year period in the last 1000 years. This will be among the most important findings in WG I's report and the reader needs to know how much data support the finding, and whether there has been a significant increase in the amount of data available since the TAR. [Jeff Kueter (Reviewer's comment ID #: 137-57)]	See response to comment 6-755.
6-757	A	30:1	30:3	The "hockey stick" was one of three main lines of evidence used in the TAR to justify the conclusion that human activities were the cause of most of the warming observed during the last half of the 20th century. As noted in this chapter, this led to critical analyses of the Mann et al study. One of the criticisms of their work was the limited amount of data on which its conclusions were based. The text in this draft indicates that new studies since the TAR "... represent some expansion of the length and geographic coverage of the previously available data." This is a weak statement, suggesting that the expansion of data has not been very great. The reader should be given more information about how much new data has been added to the analysis since the TAR, and why it justifies the strong statement that it is "...very likely that average NH tempertaures were warmer than any	See response to comment 6-755.

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				other 50 year period in the last 500 years." The TAR conclusion was different, assigning only a likely probability, albeit to the last 1000 years. This text indicates that there has been "some" expansion of the length and geographic coverage of proxy studies, but does not indicate how much of an expansion has occurred. This is critical information that needs to be included in the chapter. Figure 6-11 shows only scattered data for 1000 A.D, yet the Executive Summary of this chapter, the Technical Summary and the SPM all contain the conclusion that the second half of the 20th century was likely to have been the warmest 50-yr period in the last 1000 years. This will be among the most important findings in WG1's report and the reader needs to know how much data support the finding, and whether there has been a significant increase in the amount of data available since the TAR. [Govt. of United States of America (Reviewer's comment ID #: 2023-417)]	
6-1167	B	30:1	30:1	You allude to the fact that these reconstructions are "not entirely independent inasmuch as there are some predicotrs that are common". This is a very misleading description. For the medieval period, there is massive overlap in all the cited studies. The six series of Briffa (2000) together with bristlecones/foxtails are used in only slightly varying combinations in all of the cited studies. If there are problems with only a few canonical series (as arguably has already been demonstrated with the birstlecones/foxtails) then the entire corpus of studies may fall. Problems can be observed elsewhere e.g. the Yamal series and the Polar Urals Update have very different properties with the Yamal series being a big contributor to HS-ness while the Polar URals series has a strong MWP. The Polar Urals Update correlates better to gridcell temperature than the Yamal series and one cannot help but suspect that the decision to use the Yamal series in all studies except Esper has been done with one eye on the MWP-modern relationship. [Stephen McIntyre (Reviewer's comment ID #: 309-62)]	Accepted – text revised to stress overlap in early centuries of the last millennium. However, please note that the reviewer's "suspicion" is unfounded.
6-1168	B	30:5	30:5	Figure 6.11a does not show many proxies used in R1, R2: e.g. Rio Alerce, Lenca, Morocco tree rings, Quelccaya, Law Dome [Stephen McIntyre (Reviewer's comment ID #: 309-63)]	Accepted – Figure (6.11) now shows a more comprehensive picture of proxy series locations used in the references cited. The reference to "temperature sensitive" proxies has been removed in the caption and additional series, as indicated by the reviewer, have been shown.
6-1169	B	30:5	30:5	Figure 6.11b,c similarly does not show proxies used in R1 e.g. gridcell precipitation in Bombay and Madras. Perhaps these are not "temperature-sensitive" in which case you need to specify which proxies from the earlier studies are not being used. [Stephen McIntyre (Reviewer's comment ID #: 309-64)]	See response to comment 6-1168.

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6-1170	B	30:5	30:5	Figure 6.11 does not show the tropical ice core proxies said in the paragraph to be precipitation proxies but which were used in the studies (including indirectly through the Yang composite used in Mann-Jones 2003, Moberg 2005, Osborn and Briffa 2006.) [Stephen McIntyre (Reviewer's comment ID #: 309-65)]	See response to comment 6-1168.
6-1171	B	30:5	30:5	If you do show "all" the proxies of R1, then check the actual locations. MBH98 used precipitation data from France for North American gridcells. The MBH98 precipitation data said to be from Bombay is not from there; no one knows the geographical locaiton of the data actually used - it might even come from North America. [Stephen McIntyre (Reviewer's comment ID #: 309-66)]	See response to comment 6-1168.
6-1172	B	30:7	30:7	Briffa (2000) used seven sites which recur repeatedly in the other studies. Briffa substituted a ring width series from Yamal for the updated Polar Urals series (later used in Esper et al 2002). If the Polar Urals Update from Esper is used in Briffa instead of Yamal, then the MWP in the reconstruction is higher than shown. The Polar Urals Update has a better correlation to gridcell temperature than the Yamal series (I have so far been unable to confirm the correlation to gridcell temperature of this series reproted in Osborn and Briffa 2006 and suspect that it is wrong.) You need to disclose that this result is sensitive to the choice between using Yamal aor the Polar Urals update. [Stephen McIntyre (Reviewer's comment ID #: 309-67)]	Rejected – these are speculative remarks by the reviewer who incorrectly assumes that there has been a biased 'selection' of data and processing by Briffa versus Esper.
6-1173	B	30:14	30:14	The medieval network of Esper et al 2002 is closely related to that of Briffa 2000. Esper took tree-ring data from 14 sites, but only 7 extended to the medieval period. These 7 sites included 5 of 7 sites from Briffa (2000), plus 2 foxtail sites in California. Foxtails interbreed with bristlecones and may be subject to the same problems as the controversial bristlecone sites of Mann et al 1999. There is no legitmate basis for using TWO nearby foxtail sites, and probably not even one. Their relative MWP-modern level in their reconstruction does not appear to be robust to the presense/absence of these two foxtail sites. [Stephen McIntyre (Reviewer's comment ID #: 309-68)]	Rejected – these remarks are speculative and the current text reviews published papers and is not intended to 'second guess' their content.
6-1174	B	30:17	30:17	I have been unable to confirm that Esper "averaged" the series; this is not stated in the article. Please confirm that this is what he did. [Stephen McIntyre (Reviewer's comment ID #: 309-69)]	Noted- no change to text required (Esper did average data)
6-758	A	30:25	30:30	This paragaph appears to have a missing thought. Did the authors intend on pointing something out about this reconstruction? Commenting on what is significant about the reconstruction was done in the case of the surrounding paragraphs. It seems Figure 6.10 should be cited here. [Gregory Wiles (Reviewer's comment ID #: 289-24)]	Noted – text revised to clarify meaning.
6-1175	B	30:26	30:26	I do not believe that Mann and Jones 2003 "averaged" their proxies. They are weighted some how. Phil Jones did not know how they were weighted. I don't know how. I'd like to	Mann and Jones (2003) explored several variations of 'averaging' their

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				know. [Stephen McIntyre (Reviewer's comment ID #: 309-70)]	selected proxies and produced different reconstructions. The version included in Figure 6.10 used weighting based on regional-extent scaling (i.e. by the cosine of the latitude) and on the strength of the correlation between proxy and local temperature (as measured through decadal correlation). The wording in the current text has been amended to show that 'weighting' was used in the averaging.
6-1176	B	30:27	30:27	Some of the series in Mann and Jones 2003 use more than one site, although "integration" is too pompous a word. However, not a "majority". [Stephen McIntyre (Reviewer's comment ID #: 309-71)]	Accepted
6-1177	B	30:27	30:27	State that the "oxygen isotope records" are from TROPICAL ice cores, which may represent precipitation (as you acknowldege at 6-32-38. Problematic oxygen isotope records from Dunde and Guliya are used in the Yang China composite, where they have a very strong impact. The Yang composite is used in Mann and Jones 2003; Moberg et al 2005 and Osborn and Briffa 2006. Removing the two problematic tropical ice cores from the Yang composite results in a different MWP-modern relationship for this proxy with a knock-on impact for the multiproxy studies, [Stephen McIntyre (Reviewer's comment ID #: 309-72)]	Rejected – they used tropical and polar ice cores.
6-1178	B	30:27	30:27	Mann and Jones 2003 use their problematic PC1, which emphasizes the equally problematic bristlecone site of Sheep Mountain, said by Lamarche et al 1984; Graybill and Idso 1993 etc not to be a temperature proxy. [Stephen McIntyre (Reviewer's comment ID #: 309-73)]	Noted – no revision to text necessary.
6-1179	B	30:27	30:27	Mann and Jones use a 3-series average of Tornetrask, Yamal and Taimyr, which is not robust to the Yamal substitution. [Stephen McIntyre (Reviewer's comment ID #: 309-74)]	Noted – no revision to text necessary.
6-759	A	30:28	30:28	Missing full stop at end of line [James Crampton (Reviewer's comment ID #: 50-27)]	Accepted
6-1180	B	30:29	30:29	Moberg et al 2005 used the Yang composite which includes tropical ice core series and is affected by changing attribution views on tropical glacier dO18 described later. [Stephen McIntyre (Reviewer's comment ID #: 309-75)]	Noted – no revision to text necessary.
6-1181	B	30:32	30:32	The seven tree ring sites in Moberg et al 2005 included three bristlecone sites, including near duplicate versions of the same site, and three other sites used in other studies. It also included other series from Briffa 2000. However the relative medieval	Noted – no revision to text necessary (and comment apparently incomplete?)

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				[Stephen McIntyre (Reviewer's comment ID #: 309-76)]	
6-1182	B	30:32	30:32	In 6-31-41, you emphasize the need to "empirically calibrate" all proxies. This was not done in Moberg et al 2005. They used three low-frequency series which were not calibrated against temperature. There is extreme non-normality in several series, including all 3 series which make the strongest contributions to relative medieval-modern levels. Little confidence can be attached to such calculations. [Stephen McIntyre (Reviewer's comment ID #: 309-77)]	Noted and accepted in principle – but current text (elsewhere) makes the point that Moberg et al. (2005) is not 'formally' calibrated.
6-760	A	30:34	30:34	Remove the word "far" in far-less-accurately [Gregory Wiles (Reviewer's comment ID #: 289-25)]	Rejected – current stress on level of dating accuracy is pertinent.
6-1183	B	30:41	30:41	I recommend that you not use Rutherford et al 2005. It re-cycles the MBH98 proxy network including all the problematic PC series. It adds nothing new. The conclusion sentences representing this study are not informative. The Rutherfor reconstruction as illustrated goes only to 1960, but must be shown up to the end - don't conceal the "divergence problem" [Stephen McIntyre (Reviewer's comment ID #: 309-78)]	Rejected – while true that this reconstruction is based on series common to other recostructions its inclusion is justified on the basis of methodological difference. The 'divergence problem' will be explicitly described in the new text.
6-1184	B	30:41	30:41	D'Arrigo et al in their medieval portion use almost exactly the same network as Briffa 2000 and the other studies: Tornetrask, Yamal, Taimyr, Jasper, Mongolia (plus in their case Coastal Alaska - only one "new" series") 83 6-83 79 [Stephen McIntyre (Reviewer's comment ID #: 309-78)]	Noted – no revision to text necessary.
6-1185	B	30:41	30:41	D'Arrigo et al. [2006] does not verify for post-1985 warm values. This should be disclosed as it raises questions about its calibration on warm periods. [Stephen McIntyre (Reviewer's comment ID #: 309-80)]	See response to comment 6-1183.
6-1186	B	30:41	30:41	The Nature article of Hegerl et al does not describe the network - either where the proxies are or the total least squares method. The article describing these things is their J Climate article which missed the deadlines. It shouldn't be used. [Stephen McIntyre (Reviewer's comment ID #: 309-81)]	Rejected – while the reviewer is entirely correct about the description of the work, the reconstruction itself is in the 'published' literature and is considered salient to the discussion of 'true' amplitude of past changes – after long consideration it was decided to keep it in the chapter, though it is recognised that the work will be subject to later close scrutiny.
6-1187	B	30:41	30:41	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 (Reviewer's comment ID #: 309-82)]	Noted – no revision to text necessary.

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6-1188	B	30:41	30:41	Rutherford et al (2005) studied the proxy networks of Mann et al 1998 and Briffa et al 2001. The Mann et al proxy network as studied used the same principal components methods and series as Mann et al 1998, the methodology of which has been criticized (McIntyre and McKitrick 2005a, Von Storch and Zorita 2005). [Stephen McIntyre (Reviewer's comment ID #: 309-83)]	Noted – no revision to text necessary.
6-1189	B	30:41	30:41	Rutherford et al (2005) incorrectly collated the instrumental data with the proxy data in its consideration of the MBH98 network. This is going to come up at some point and is one more reason not to use this study. [Stephen McIntyre (Reviewer's comment ID #: 309-84)]	Rejected – if the reviewer's comments are correct, a later assessment would be the appropriate place to discuss this.
6-1190	B	30:41	30:41	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 (Reviewer's comment ID #: 309-85)]	Rejected – incorrect interpretation by the reviewer of the implications of the paper he cites. Also see response to Comment 6-736.
6-1191	B	30:43	30:43	The confidence interval calculations are not "clearly" described in any of the publications. [Stephen McIntyre (Reviewer's comment ID #: 309-86)]	Accepted – text amended to omit the word 'clearly' – but note the incorrect location indicated for the comment content
6-1192	B	30:43	30:43	MBH have refused to provide residuals for the controversial 15th century step and rferences to their residuals should not be included until this data is provided [Stephen McIntyre (Reviewer's comment ID #: 309-87)]	Rejected – this does not affect the details of their reconstruction as presented.
6-1193	B	30:43	30:43	If these are "minimum uncertainty", what is the estimated uncertainty? The concept of "minimum uncertainty" is ludicrous - the purpose of uncertainty is to give confidence estimates. Anything relying on "miniumum uncertainty" should be deleted or re-written. [Stephen McIntyre (Reviewer's comment ID #: 309-88)]	Rejected – the text indicates that the uncertainty levels used here are (in some cases) not necessarily representative of the total uncertainty.
6-1194	B	30:45	30:45	Rutherford et al 2005 did NOT study the impact of presence/absence of bristlecones so it is untrue to suggest that they considered robustness to proxy selection insofar as they neglected the most critical aspect for this data. [Stephen McIntyre (Reviewer's comment ID #: 309-89)]	Rejected – the text as presently written is correct.
6-1195	B	30:46	30:46	Unless the proxies are calibrated in the warm period of 1980s-1990s, no conclusions can be drawn [Stephen McIntyre (Reviewer's comment ID #: 309-90)]	Rejected – the text as presently written is correct.
6-1196	B	30:48	30:48	D'Arrigo et al (2006) used only 6 sites in the medieval period, of which all but one overlap the sites of Briffa (2000) used in the other studies. They use the Yamal substitution and their conclusions of relative modern-medieval warmth may not be robust to that. [Stephen McIntyre (Reviewer's comment ID #: 309-91)]	Noted – no revision to text necessary.

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6-1197	B	30:48	30:48	D'Arrigo et al 2006 does NOT verify for the 1980s and 1990s because of the "divergence factor". This issue was picked up in the NAS Panel and no one was able to give a satisfactory explanation for the "divergence factor". I particular, concern was raised that, if the proxies couldn't pick up the warm 1980s and 1990s, how could we be sure that they picked up a possible similar period in the past. D'Arrigo was unable to answer. This is a very important issue which is dodged throughout this section. It's important that you deal with the issue as it's going to be of increasing concern. [Stephen McIntyre (Reviewer's comment ID #: 309-92)]	Noted – text will be revised, elsewhere, to include discussion of this issue – see response to comment 6-1152.
6-1198	B	30:48	30:48	D'Arrigo et al 2006 have not archived their results. Require them to archive all their data and their results as condition of use. [Stephen McIntyre (Reviewer's comment ID #: 309-93)]	Rejected – this is not the purpose of the IPCC assessment.
6-1199	B	30:53	30:53	There has been a bait-and-switch in the Hegerl et al submission. The article in which Hegerl et al describe their network - and the one at the WG1 website for the First Order Draft - is their submission to Journal of Climate, which was described in their Nature article as merely being "submitted". The article at the WG1 website for the Second Order Draft is their Nature article which has been submitted, but which does not provide the information described in this section - which derives from the Journal of Climate submission. Since the Journal of Climate has not met IPCC deadlines, TSU should have removed all references to it in February. In any event, all references to it should be deleted now. [Stephen McIntyre (Reviewer's comment ID #: 309-94)]	Rejected – the new Hegerl et al (2006) paper was accepted in time for new inclusion deadline and provides the necessary information.
6-1200	B	30:53	30:53	From the map in the article at the First Order Draft, I presume that Hegerl et al used all the Briffa 2000 sites; Mann's PC1 and the Yang composite (with tropical ice cores). Conclusions from it as to relative medieval-modern levels will be vulnerable to the same factors as the other studies. [Stephen McIntyre (Reviewer's comment ID #: 309-95)]	Noted – no revision to text necessary.
6-761	A	30:56	31:2	It is stated here with reference to Hegerl et al (in press) that the method of "total least squares" ("TLS"), unlike other methods, can somehow provide statistical reconstructions of past surface temperatures from proxy data that preserve the true variance in the reconstructed series on some desired "low-frequency" timescale.This claim is fundamentally problematic for at least three reasons: [1] Total least squares leads to less biased estimates of regression coefficients (they are unbiased under assumptions that are not satisfied in the Hegerl et al. study, such as known error variances), but reconstructions that fill in missing temperature values with (conditional) expected values always have lower variance than the actual temperatures because the missing temperature values are imputed from the center of the posterior distribution. That is, the sample variance of the reconstruction is *always* smaller than the actual variance by at least the variance of the	Noted – no revision to text necessary as a consequence. It was considered necessary to include this curve in Figure 6.10 and reference to the reconstruction used by Hegerl et al. even though insufficient evidence of their method had been published at the time of writingt. This is not to say that this paper will not be subject to criticism, but any such criticism would be an appropriate subject of a

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				reconstruction residual (see Little and Rubin, Statistical Analysis with Missing Data, Wiley, 2002); [2] Rutherford et al. [Rutherford, S., Mann, M.E., Osborn, T.J., Bradley, R.S., Briffa, K.R., Hughes, M.K., Jones, P.D., Proxy-based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Methodology, Predictor Network, Target Season and Target Domain, Journal of Climate, 18, 2308-2329, 2005] use a regression approach (Regularized Expectation Maximization--Schneider, T. Analysis of incomplete climate data: Estimation of mean values and covariance matrices and imputation of missing values, J. Climate, 14, 853-871, 2001), that is a regularized total least squares regression and have shown that the method performs very well in practice for quite low signal-to-noise ratios [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, 18, 4097-4107, 2005], so others have used similar (and quite arguably superior) approaches to that of Hegerl et al. (2006); [3] Hegerl et al. select proxies on the basis of their correlation with instrumental temperatures, without cross-validation, leading to selection bias, an overestimation of correlations between proxies and temperatures, and an underestimation of imputation errors and hence of temperature variances (which must include a contribution from imputation error variances if expected values are filled in for missing values). [Michael Mann (Reviewer's comment ID #: 156-23)]	subsequent assessment. In the interests of comprehensiveness it was considered 'best' to include the curve here.
6-762	A	31:6	:13	This sentence is way too long and should be broken up into 2 to 3 sentences. [Govt. of United States of America (Reviewer's comment ID #: 2023-418)]	Rejected – while we sympathesise with this comment to some extent it was considered just OK to leave the text as is.
6-1201	B	31:8	31:8	Crowley and Lowery 2000 is not used in any of the graphs. Why do you refer to it here? Why was it not mentioned as one of the canonical reconstructions? [Stephen McIntyre (Reviewer's comment ID #: 309-96)]	Noted – no change to text required . This was a pre-TAR paper (and so not necessarily relevant in a discussion of post-TAR work) and the reconstruction is similar anyway to the subsequent (Hegerl et al , 2006) reconstruction that is included. The citation to Crowley and Lowery (2000) is in the context of a methodological discussion where it is relevant
6-763	A	31:16	31:16	add (D'Arrigo et al. 2006) to the cited references of those studies that regionalized their data prior to final development of Northern Hemisphere reconstructions. [Rosanne D'Arrigo (Reviewer's comment ID #: 56-1)]	Accepted.

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6-764	A	31:16	31:16	it could be added that only a few studies actually prescreened their records with local temps (e.g. D'Arrigo et al. 2006, Osborn and Briffa 2006. [Rosanne D'Arrigo (Reviewer's comment ID #: 56-2)]	Rejected – while the point is true, the paragraph is already complex and this would overcomplicate it.
6-765	A	31:23	31:35	This paragraph seems like it should be before the one above as the surrounding paragraphs are general discussion. [Gregory Wiles (Reviewer's comment ID #: 289-27)]	Rejected – the foregoing paragraphs relate to 'standard' reconstructions using numbers of scaled or calibrated data directly. The glacier interpretation is a different class of information.
6-766	A	31:28	31:35	The discussion of glacier "shortcomings" seems disproportionate. As discussed in chapter 4, glaciers respond to changes in precipitation as well as temperature. If temperature is not changing much, or if precipitation changes are especially large, then glacier changes will not primarily reflect temperature. However, if large data sets from geographically dispersed regions are aggregated, glaciers are rather good paleothermometers. Glaciers are likely much more clearly paleothermometers than are tree-ring records (the glaciers are tracking the late-twentieth-century instrumental warming closely, whereas d'Arrigo et al (2006) and other workers have shown strong divergence in many tree-ring records), but I did not see a parallel list of complaints about tree-ring records. Including more caveats about the more-faithful (if much lower time resolution) recorders does not appear balanced. [Richard B. Alley (Reviewer's comment ID #: 4-5)]	Rejected – the text is justified and the addition of the section on tree' problems' will balance things. See response to comment 6-1152
6-1202	B	31:28	31:28	You should add that these reconstructions are all based on a few selected proxies and that results would be different if other plausible selections were made, such as the updated Polar Urals series being used instead of Yamal or if bristlecones are not used. [Stephen McIntyre (Reviewer's comment ID #: 309-97)]	Rejected – these are the currently available reconstructions – the reviewer's remark is a moot point.
6-767	A	31:29	31:29	The senetence starting "Analyses" should begin with "For example, analyses" [Gregory Wiles (Reviewer's comment ID #: 289-26)]	Accepted.
6-1203	B	31:32	15:32	You state that you are using "two standard error confidence intervals". This really is a misstatement as they are not verification period residuals by calibration period residuals and the true widths may be much greater. [Stephen McIntyre (Reviewer's comment ID #: 309-98)]	Noted – but does not necessitate any change to the text – the basis of these residuals is stated and can be further explored by reading the cited papers and it is stated elsewhere that they may be wider than indicated in some cases.
6-1204	B	31:41	31:41	You state: "For this reason, the proxies must be calibrated empirically…" You then need to state that this has not always been done in past reconstructions. You should also state that there are important outstandihng controversies over whether individual proxies are temperature or precipitation proxies, or whether they are affected by nonclimatic factors. It's not clear to me that the approach discussed here will prove superior in the long run to	Rejected – these points are covered in the existing text.

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				studies of vegetation change based on lapse rate reasoning. [Stephen McIntyre (Reviewer's comment ID #: 309-99)]	
6-1205	B	31:41	31:41	You need to state clearly that proxy series from nearby sites may give very different results e.g. Yamal and the Polar Urals update. [Stephen McIntyre (Reviewer's comment ID #: 309-100)]	Rejected – this would imply a greater instability than current evidence supports.
6-768	A	31:54	31:55	clumsy wording, better: "2 sigma error at the multi-decadal timescale is of order +/- 0.5" [Eric Wolff (Reviewer's comment ID #: 292-33)]	Accepted.
6-1206	B	31:54	31:54	The use of calibration period residuals to calculate confidence intervals cannot be endorsed, especially when there is overfitting in the calibration period, as occurs in many of the reconstructions. Add the following sentence: "All of the above studies used residuals from the calibration period rather than the verification period. Standard errors in the verification period were much higher and accordingly none of the cited confidence intervals can be used with any "confidence" [Stephen McIntyre (Reviewer's comment ID #: 309-101)]	Rejected – the requested insertion is unjustified in most cases, and the veracity of these intervals has been qualified in the current text anyway.
6-1207	B	31:54	31:54	None of the confidence intervals consider the impact of the following issues: validity of bristlecones as a proxy; biased selection of proxies (e.g. the Yamal substitution), mis-specified use of tropical dO18 series as a temperature proxies. Should any of these issues be proved to apply, reported confidence intervals are meaningless. [Stephen McIntyre (Reviewer's comment ID #: 309-102)]	Rejected – the reference to "Yamal substitution" is unfounded (and offensive) and the other remarks are open to debate. The current text refers to published confidence limits.
6-769	A	31:54	32:2	This paragraph is somewhat obscure. We suggest it is reviewed and possibly re-written. [Govt. of Australia (Reviewer's comment ID #: 2001-319)]	Rejected – the paragraph has been reviewed and the reviews considered. It is considered essential to qualify the indication that these confidence limits are necessarily correct in all interpretations and the references cited allow the reader to explore this issue in greater detail.
6-770	A	31:54	32:2	It might be clarifying for the reader to include a reference in the list on lines 56-57 presenting uncertainties for a low pass filtered version of the Mann et al (1999) reconstruction: Gerber et al (2003), Fig. 1b, already in ref. list. I think it would be instructive to show this different approach and even to discuss it in terms of its implications to interpret low frequency variations in comparison with the other references provided. Also, Crowley (2000) shows different low frequency uncertainty bands for the Mann et al. reconstruction. These discrepancie [Jesús Fidel González-Rouco (Reviewer's comment ID #: 86-3)]	Accepted.
6-771	A	31:54	32:2	Comment: it might be of use for the reader to include a reference in the list on lines 56-57 presenting uncertainties for a low pass filtered version of the Mann et al (1999)	See response to comment 6-770.

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				reconstruction: Gerber et al (2003), Fig. 1b, already in ref. list. I think it would be instructive to show this different approach and even to discuss it in terms of its implications to interpret low frequency variations in comparison with the other references provided. [Govt. of Spain (Reviewer's comment ID #: 2019-46)]	
6-1208	B	31:56	31:56	Mann et al have not archived their individual reconstruction steps and their residuals are not calculable. Require them to archive the individual steps and residuals as a condition of referring to them here. [Stephen McIntyre (Reviewer's comment ID #: 309-103)]	Rejected – this is not an appropriate approach to be followed soley in this case and no such requirement is made for other papers.
6-772	A	31:57	31:57	add D'Arrigo et al. (2006) to ths list of those studies that quantified the uncertainties of their reconstructions. [Rosanne D'Arrigo (Reviewer's comment ID #: 56-3)]	Accepted.
6-773	A	31:57	32:2	The statement made here as currently worded is absolutely false. Regression residuals of course take into account the uncertainty in the degree to which the proxy accurately record the climate variables of interest, i.e. the uncertainties in the proxy climate signal, as this is estimated from calibration/verification. What they don't take into account is a possible degradation of that signal prior to the interval used for calibration/verification. The latter may or may not be significant depending on the data being used, for example the extent to which tree-ring estimates are based on large samples and multiple replication of chronologies. I believe it is this latter sort of uncertainty which is being alluded to here by the authors, but that is very different from what is stated. The wording here should be revised to be more accurate and precise. [Michael Mann (Reviewer's comment ID #: 156-39)]	Accepted – text modified to further clarify the point.
6-774	A	31:57	32:2	A neglected contributor to uncertainties (at least where cross-validation is not used) is the selection bias mentioned earlier with respect to e.g. the Hegerl et al reconstruction (i.e., use only of proxies which are highly correlated with temperatures over the calibration period). This leads to underestimation of uncertainties. Even in relatively simple test cases with simulated temperatures with a small fraction of missing values, this selection bias can lead to a significant underestimation of error variances (cf. Schneider, T. Analysis of incomplete climate data: Estimation of mean values and covariance matrices and imputation of missing values, J. Climate, 14, 853-871, 2001) [Michael Mann (Reviewer's comment ID #: 156-65)]	Noted – but no change will be made to the text.
6-775	A	32:4	32:5	TAR data series are not identified in Fig. 6.10b, therefore it is not possible to see how the currently available reconstructions compare with TAR [James Crampton (Reviewer's comment ID #: 50-28)]	This is apparent from the text and the curves in 6.10 can be cross-referenced easily – but TAR series will be indentified in caption.

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6-776	A	32:4	32:29	Some key points appear to be lost in this discussion. For one, although it is correct to say that many recent reconstructions suggest greater variability than was shown in the TAR, an overwhelmingly important caveat is not stated: since the reconstructions which suggest the greatest low-frequency variability (Moberg et al and Esper et al) don't even remotely resemble each other (they are essentially anti-correlated on centennial timescales, with the first showing pronounced cold in the later centuries and the latter showing pronounced cold in the earlier centuries), it is highly implausible that both or in fact either one, reflect meaningful estimates of past annual northern hemisphere mean temperatures. More likely, the differents are due to differential seasonal and spatial sensitivity of the underlying proxy data. This is only vaguely alluded to, yet it is perhaps the primary reason for the observed differences. Secondly, it is shown in the figures (Figure 6.10a) but left unstated in the discussion that the available instrumental record in earlier (i.e., 17th-19th) centuries agrees far better with the reconstructions shown in the TAR than many of the more recent reconstructions (e.g.Moberg et al or Huang, Pollack et al boreholes) which suggest far greater cooling than do the instrumental recods. The same is true with the Oerlemans glacier-based estimate, which is entirely independent of all other proxy data, and shows estimates quite close to those shown in the TAR over the available interval back to AD 1600. The latter cast significant doubt as to whether the "newer" proxy reconstructions suggesting a colder "little ice age" are at all accurate. The discussion here therefore requires considerably greater circumspection than is currently present. [Michael Mann (Reviewer's comment ID #: 156-38)]	Rejected – the caveat is not considered an appropriate one to state explicitly in this level of assessment, particularly as it is not possible to judge which, if any, is more likely to be realistic. That some/many of the differences between reconstructions are attributable to different targets/predictors is clearly implied and can not be taken further on the basis of current understanding. The final remark regarding the similarity or otherwise to early instrumental data is also a debatable point, with the bias towards European early instrument locations – and as a result of CLA discussion, the indication of the (dotted) early instrumental record on Figure 6.10b has been removed anyway.
6-777	A	32:31	32:34	Could be supressed, because this information is provided not far above. In general the whole section is very interesting, but too long. [Pascale BRACONNOT (Reviewer's comment ID #: 29-17)]	Rejected – the length and content are the product of balancing multiple reviews of earlier drafts.
6-778	A	32:31	32:31	Did the authors mean to say Southern rather than Northern Hemisphere? If the intention was Northern then relatively should be removed late in the sentence. [Gregory Wiles (Reviewer's comment ID #: 289-28)]	Accepted.
6-1209	B	32:31	32:31	Limitations of other proxies is discussed, but not tree rings. You need to discuss problems with site chronologies: the "divergence factor"; lack of homogeneity through Modern Sample Bias, changing altitudes, non-monotonicity of response. [Stephen McIntyre (Reviewer's comment ID #: 309-104)]	Accepted.
6-779	A	32:35	32:35	Note that some studies consider that tree-ring width data from some northern sites can integrate climatic conditions on an annual basis (e.g. Jacoby and D'Arrigo 1989, Climatic Change 14: 39-59; D'Arrigo et al. 2006). [Rosanne D'Arrigo (Reviewer's comment ID #: 56-4)]	Noted – but no change to text considered necessary.
6-1210	B	32:40	32:40	You need to explicitly state, according to this interpretation, some tropical glacier ice cores used in previous multiproxy studies measured precipitation	

1210 B 32:40 32:40 You need to explicitly state, according to this interpretation, some tropical glacier ice cores used in previous multiproxy studies measured precipitation

				cores used in previous multiproxy studies measured precipitation and this may require re-interpretation of multiproxy studies using tropical dO18 series (MBH98-99, J [Stephen McIntyre (Reviewer's comment ID #: 309-105)]	text.
6-1211	B	32:40	32:40	The Oman offshore diatoms have also been interpreted as a precipitation proxy (Treydte et al 2006). If you are not showing some precipitation proxies e.g. Tropical ice cores, maybe you should not show this one. [Stephen McIntyre (Reviewer's comment ID #: 309-106)]	Meaning of point not clear
6-1212	B	32:41	32:41	You say "apparently unprecedented". If you don't have evidence, change the wording. The glaciers in the North American Rockies are considered to have formed in the Neoglacial, those in the Venezuealan Andes in the LIA, maybe these ones are recent as well. [Stephen McIntyre (Reviewer's comment ID #: 309-107)]	Rejected – the qualification is meant to indicate the nature of the "unprecedentedness".
6-780	A	32:42	32:42	Possibly should be changes to the stronger "likely" [Gregory Wiles (Reviewer's comment ID #: 289-29)]	Accepted.
6-781	A	32:44	32:44	Is chapter 3 really the right citation for tropical-glacier mass balance, or should it be chapter 4? [Richard B. Alley (Reviewer's comment ID #: 4-6)]	Accepted.
6-782	A	32:44	32:45	"primarily reflect SSTs' - reword; coral oxygen isotopes can reflect SST or salinity or a mixed signal; coral Sr/Ca ratios are primarily SSTs. 21 6-21 320 [Govt. of Australia (Reviewer's comment ID #: 2001-6)]	Accepted – sentence reworded.
6-783	A	32:44	32:45	Well it is well known that both temperatures and salinities influence delta o18 in corals, I am not familiar with any evidence indicating that Sr/Ca is influenced by salinity. Can the authors provide a citation to support this claim? [Michael Mann (Reviewer's comment ID #: 156-37)]	Noted – and text revised to account for this point.
6-784	A	32:47	32:50	Need to clarify these statements as majority of coral records to date are based on oxygen isotopes which can contain a mixed SST/salinity signal. This can result in an apparent inflation of the magnitude of recent warming from coral oxygen isotope records, eg Lough (2004) Palaeo. Palaeo. Palaeo. Vol 204: 115-143. [Govt. of Australia (Reviewer's comment ID #: 2001-321)]	Noted – and text revised
6-785	A	32:52	32:52	Briefly develop "pseudo-proxy network" [Michel Crucifix (Reviewer's comment ID #: 52-72)]	Rejected – not sufficient space.
6-786	A	32:52	32:57	The wording here is not objectively defensible. The use of "indicate" in line 5 implies that there is established validity to the claim in question. For reasons discussed subsequently, there is no validity to the claim. At the very least, "indicate" must be replaced by "claim". As shown by Wahl et al (which is incorrectly referenced later as "Wahl and Ritson"), the claims of Von Storch et al 2004 are erroneous, because their analysis was fundamentally compromised by an undisclosed error in what they falsely claimed to be an implementation of the method of Mann et al. This same error (detrending data prior to calibration) was also inexplicably made (and justified by a "personal communication" to H. Von Storch) by the two referenced papers by Burger and Cubasch and Burger et al,	Rejected - the purpose of including this citation in the text (despite their unfortunate error in not disclosing the detrending) is because it still has some potential significance for several reconstructions. It is a moot point whether the details of the model implementation they used is that significant for the existence (if not the

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				which compromises all conclusions in those papers as well Furthermore, several of these papers (Von Storch et al, 2004; Burger et al, 2006) used a deeply flawed ("Erik") simulation wherein the model (GKSS) was incorrectly initialized at 900 AD with modern day (i.e., anthropogenic) initial conditions. As discussed later in the chapter (page 37) it has been shown by Osborn et al (2006) that this error leads to a drift of more than 1C in the first century, and most of the long-term variability in the simulation is due to a persistent long-term drift. The model also suffers from an exagerrated 20th century warming due to absence of anthropogenic tropospheric aerosol forcing. Independent analyses using *correctly-implemented* climate field reconstruction (CFR) methods and a *well-behaved* simulation of the past 1000 years (NCAR CSM1.4 coupled model forced by estimated natural and anthropogenic radiative forcing, w/ any spatial drift removed) shows no evidence of any systematic low-frequency variability using even lower signal-to-noise ratios than VS04 (Mann et al, 2005a). The best available evidence is that the claims by Von Storch, Burger, Cubasch and collaborators are simply false, and it is incumbent upon the AR4 report to accurately reflect where this matter currently stands. [Michael Mann (Reviewer's comment ID #: 156-24)]	magnitude) of potential bias in reconstructions. The full history to date of the claims and rebutals are trackable in the citations and the implication as regards this assessment is still clear. On balance, therefore, the paragraph should remain. The word "indicate" is clearly accompanied by "may" so the sense is clear.
6-1213	B	33:0		caption for supplemental figure (BELTRAMIHugo_AR4ERSOD_Ch06_Sup.png): Figure 3. The resulting temperature anomalies for each region from the forward modelling of the ECHO-g control (red), FOR1 (green), FOR2 (blue) runs, and the mean temperature anomaly (black) for each region: (a) British Columbia/Yukon, (b) Manitoba/Saskatchewan, (c) Quebec/Ontario, (d) Atlantic Canada. Enhanced EPS. [Hugo Beltrami (Reviewer's comment ID #: 306-11)]	Not relevant as Figure will not be included
6-1214	B	33:2	33:2	You don't have evidence to say that the bias is "likely" not as large. The matter is in controversy. I've read all the articles closely, am very familiar with the literature and the arguments and I think that the comment by Wahl et al is completely beside [Stephen McIntyre (Reviewer's comment ID #: 309-108)]	Rejected – this statement is based on analyses showing the effect of the disequilibrium "spin up" used in the German simulation that provided the pseudo proxies – see 6-787.
6-787	A	33:3	33:3	The von Storch et al estimate of a 2x error in reconstructions should be rejected more strongly than done here. Osborn et al (2006) showed that some of the 2x estimated by von Storch arose from climate-model drift associated with initiating the model with modern conditions for a preanthropogenic run, so that the model had to cool initially. Furthermore, the Wahl et al (2006) piece shows that von Storch et al simply failed to implement the Mann et al procedure, and so did not test that procedure (a point already made by Zorita and von Storch). The text should reject the von Storch et al estimate of 2x error with high confidence based on known shortcomings in the study. [Richard B. Alley (Reviewer's comment ID #: 4-7)]	Rejected – the current statement is clear and the papers detailing the 'detrending' step in von Storch et al analysis are cited – see 6-1214.

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6-788	A	33:3	33:7	The discussion here does not accurately reflect where the science currently stands (based on papers that were accepted or in press by the official IPCC deadline). Both Rutherford et al (2005) and Mann et al (2005a) show that the hybrid RegEM method that has been used by Mann and collaborators in all work done during the past 5 years yields essentially the same reconstruction as the original Mann et al (1998) method, applied to the same multiproxy network.Furthermore, Mann et al (2005a) show that the RegEM method applied to synthetic proxy data with even lower signal-to-noise ratios than those assumed by Von Storch et al (2004) and Cubasch et al (2006) yield no evidence at all of a systematic bias in the reconstructed low-frequency variability of the sort originally claimed by Von Storch et al (2004). Therefore, it cannot honestly be argued that there is any legitimate evidence that the methods in question systematically estimated low-frequency variability. As discussed above, the arguments by Von Storch and associates to the contrary are based on erroneous work that (a) incorrectly implements the Mann et al (1998) method as shown by Wahl et al (2006) and (b) makes use of a seriously flawed model simulation. The discussion here is not accurately reflective of what has actually been demonstrated in the peer-reviewed literature, and must be revised if the AR4 report is to maintain the level of rigor and accuracy that has been the hallmark of past IPCC reports. As a side-note, it is worth noting that Von Storch et al now claim that, even though Von Storch et al (2004) was entirely erroneous, they can still get statistical methods to underestimate low-frequency variability if they assume an extremely red proxy noise component. This is both disingenous, since it reflects an assumption about limitations of the proxy data and not the method, and it is also false. As shown in recently submitted work, the latest claims by Von Storch and associates, like their earlier claims, are simply false and likely the product of additional errors or undisclosed erroneous procedures. It is additionally worth noting that the specious criticisms leveled by Cubasch and associates (Burger and Cubasch; Cubasch et al) are demonstrably not even remotely plausible criticisms of the RegEM approach used by Mann and coworkers in climate field reconstruction over the past 5 years, since that method provides no room for the so-called "flavors" (essentially, subjective distortions of methodology) introduced by Cubasch et al. In this method, regularization guards again statistical fitting, and the regularization parameters are objectively chosen by generalized cross-validation ("GCV"). The error structure is moreover explicity modeled, and an unbiased estimate of unresolved variance is obtained. Given that Rutherford et al (2005) using the same proxy data obtain a nearly identical reconstruction to that of Mann and coworkers based on earlier statistical methods, the Cubasch criticisms are neither legitimate criticisms of the earlier work,, nor are they relevant having effectively been already discredited by the findings of Rutherford et al (2005) and Mann et al (2005a). [Michael Mann (Reviewer's comment ID #: 156-26)]	Accepted – text now indicates that Rutherford et al (2005) is potentially non-biased – and the references to the bias issue are not formed in any way that impunes, RegEM – rather the text makes general points about the possibility of 'several' methods being somewhat suspect in this retrospect. Overall, the balance is considered fair and (though very brief) factual and sufficient references are provided to help those seeking further details.

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6-789	A	33:4	33:4	The reference is incorrect and out of date. The correct reference is: "Wahl, E.R., Ritson, D.M. and C.M. Ammann, Comment on 'Reconstructing Past Climate from Noisy Data', Science, 312, 529b, 2006. 519 6-519 25 [Michael Mann (Reviewer's comment ID #: 156-26)]	Accepted.
6-1215	B	33:4	33:6	The Wahl et al (accepted) article and Osborn and Briffa 2006 did not meet IPCC policies on publication deadlines and should not be cited.. [Stephen McIntyre (Reviewer's comment ID #: 309-109)]	Rejected – revised deadlines mean that these papers are citable.
6-790	A	33:10	33:11	The continued reference to the Hegerl et al (2006) reconstruction as supposedly using a method (total least squares) which yields a reconstruction which better retains low-frequency variability is absolutely preposterous. If the AR4 report makes such a claim, it will be open itself up to charges that the lead authors have absolutely no idea what they are talking with regard to statistical estimation. At the risk of being overly repetitive, I will repeat some crucial points which I have made elsewhere in the report where this issue has been raised. All proxy-based reconstruction methods which estimate expected missing values of the quantity of interest (e.g. surface temperatures) from noisy or sparse predictors (e.g. proxy data), must necessarily underestimate the true variance. This is because the variance about the estimated *expected* values is not reflected in the estimated values themselves, but nonetheless contributes to the true variance. Variance estimates should therefore be derived separately, as in e.g. the Expectation Maximization (EM) algorithm and its regularized variants such as the "RegEM" algorithm of Schneider(2001) [Schneider, T., Analysis of incomplete climate data: estimation of mean values and covariance matrices and imputation of missing values, Journal of Climate, 14, 853-871, 2001] which employs ridge regression for regularization. The "RegEM" method was first applied in the context of climate field reconstruction by Schneider (2001), and subsequently used by Rutherford et al (2005) [see Rutherford, S., Mann, M.E., Osborn, T.J., Bradley, R.S., Briffa, K.R., Hughes, M.K., Jones, P.D., Proxy-based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Methodology, Predictor Network, Target Season and Target Domain, Journal of Climate, 18, 2308-2329, 2005; see also the independent tests of the algorithm in reproducing long-term trends described by 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, 18, 4097-4107, 2005]. Indeed, if TLS is regularized (as it should be!) and the assumption is made of homogenous relative errors (which is reasonable using normalized proxy data, in the absence of any further information about the error structure in the proxy data), it simply leads to the RegEM algorithm of Schneider (2001) which achieves regularization through ridge regression. It is therefore highly implausible that the TLS method alluded to here is in practice more reliable than e.g. the RegEM algorithm, or that reconstructions based on TLS more reliably reconstruct past long-term climate changes than those based on	Noted – the text has been amended to include the word "possibly". The assessment does not state that Hegerl et al (2006) have correctly accounted for error variance in their use of the total least squares method – the statement here merely indicates that such an approach has the potential to avoid bias in the estimation of the regression coefficients. There is no implication in the revised text that the TLS approach is better or worse than RegEM.

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				RegEM (e.g. the reconstructions described by Rutherford et al, 2005). The claim that TLS somehow produces a reconstruction of past variability that magically retain the true underlying variance on some particular timescale is simply nonsense, and the authors of this chapter open themselves up to potentially severe critcism if they so uncritically repeat this absurd argument. [Michael Mann (Reviewer's comment ID #: 156-27)]	
6-1216	B	33:11	33:11	Once again Hegerl et al 2006 should not be used. I'm dubious that total least squares has much to do with this particular issue, but until Hegerl et al is published, it's impossible to say. [Stephen McIntyre (Reviewer's comment ID #: 309-110)]	Rejected – the cited paper has been through peer review and this reviewer's intuition is not sufficient grounds to exclude reference to this work.
6-1217	B	33:12	33:12	Here you need to have a full discussion of calibration in warmer periods. Bürger and Cubasch 2005 point to the need for calibration to include calibration across the range of temperatures being studied. This hasn't been done in the multiproxy studies cited here. [Stephen McIntyre (Reviewer's comment ID #: 309-111)]	Rejected – many reconstructions are based on scaling or regression where the calibration predictions include at least some relatively warm (recent) data. The current assessment can not speculate on the extent to which individual reconstructions may contain estimates that represent unrealistic extrapolations beyond the calibration predictand data – the reference mentioned is ,anyway, cited in the text.
6-1218	B	33:12	33:12	I suggest language like: "Virtually none of the proxies used in the multiproxy studies have been calibrated against temperatures of the 1980s and 1990s. There is evidence that some proxies reverse their response as temperatures rise. This would make the existing reconstructions invalid insofar as estimates of prior warmth was concerned." [Stephen McIntyre (Reviewer's comment ID #: 309-112)]	Noted but specific wording unjustified – these points will covered in a balanced way in additional text and the point is highlighted already in the list of 'Key uncertainties'.
6-1219	B	33:13	33:13	You have no basis under peer reviewed literature for this sentence. The presence of bias may dampen proxy indexes from warm periods and prevent these proxy choices from reflecting past warmth. This is a big issue and is not dealt with candidly here. [Stephen McIntyre (Reviewer's comment ID #: 309-113)]	Rejected – this statement is based on the behaviour of bias in regression and the current understanding of existing reconstructions – no change to text justified.
6-1220	B	33:17	33:17	These studies are extremely non-independent and the validity of their interpretation of modern-medieval differentials stands or falls on a few issues, all of which there is either considerable uncertainty or actual evidence against the interpretation relied upon in hte studies. [Stephen McIntyre (Reviewer's comment ID #: 309-114)]	The caveats and non-independence of the studies have been clearly discussed in the text.
6-791	A	33:18	33:19	Instead of saying "the few new reconstructions" please give the actual number of new	Accepted – stated 4 new

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				reconstructions. [Lenny Bernstein (Reviewer's comment ID #: 20-62)]	reconstructions beyond 1000 years.
6-792	A	33:18	33:20	It seems an conspicuous omission here not to explicitly acknowledge that this was precisely the level of confidence ("likely" rather than "very likely") that was attributed to this conclusion in the TAR. To prevent the possibility that there be some confusion about the matter, it needs to be explicitly mentioned that the AR4 conclusions are in agreement with those of the TAR on this point. In fact, it should be noted that the conclusion here is stronger than that of the TAR, because the conclusion is being made for the past 1300 years, not just the past 1000 years. [Michael Mann (Reviewer's comment ID #: 156-28)]	Rejected – this is clearly implied in the current text
6-793	A	33:18	:19	Instead of saying "the few new reconstructions" please give the actual number of new reconstructions. [Govt. of United States of America (Reviewer's comment ID #: 2023-419)]	See response to comment 6-791.
6-794	A	33:20	33:20	The use of "1300 years" here is odd and not justified. Current reconstructions extending back 2000 years (Moberg et al, Mann and Jones) find that late 20th century Northern Hemisphere warmth is likely unprecedented in at least 2000 years. It is therefore "2000" years that should be used here, rather than "1300 years". [Michael Mann (Reviewer's comment ID #: 156-29)]	Rejected – the large reduction in available data prior to AD 700 precludes this statement.
6-795	A	33:22	33:44	The information compiled by Pollack and Smerdon (2004) does not include much data from the Arctic. There are some recent papers that do consider surface temperautre histories for the Canadian Arctic such as Taylor et al. 2006 Taylor, A.E., Wang, K., Smith, S.L. and Burgess, M.M., Judge, A.S. 2006. (Canadian Arctic Permafrost Observatories: detecting contemporary climate change through inversion of subsurface temperature time-series. Journal of Geophysical Research. 111, B02411, doi:10.1029/2004JB003208.) There are also some recent papers by Majorowicz (with others) that also present results for northern Canada, for eg. Majorowicz et al 2004 (Majorowicz, J.A., Skinner, W.R., Safanda, J. 2004. Large ground warming in the Canadian Arctic inferred from inversions of temperature logs. Earth and Planetary Science Letters 221: 15-25.) [Sharon Smith (Reviewer's comment ID #: 244-74)]	Accepted– additional citations added
6-796	A	33:22	33:44	This section does not really discuss spatial variability. The changes in ground surface temperature will not be uniform and a number of papers have examined this. For example a recent paper by Taylor et al. 2006 (see comment 74 for reference) found for sites in the Canadian High Arctic, the cooling during the Little Ice Age and the warming that followed it was largely buffered in the central Archipelago by the maritime climate. Since shallower ground temperatures were used in the reconstruction it was also possible to obtain information on ground surface temperature in the latter 2 decades of the 20th	Noted – no aditional change to text required after noting response to Comment 6-795 .

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				century (something that the authors of Chapter 6 mention was not possible with the deeper temperatures used for the surface temperature reconstructions presented in this chapter) [Sharon Smith (Reviewer's comment ID #: 244-75)]	
6-797	A	33:22		Section 6.6.1.2 - The title should be changed as the information is not obtained from ground surface temperature measurements. The ground surface temperature histories are determined (or extrapolated) from deeper ground temperature profiles. A record of several centuries may be obtained through the use of mathematical inversion techniques. There is not a set of measurements of ground surface temperatures over these long periods (in fact, the reconstructions may be made from a single temperature profile collected at one point in time). [Sharon Smith (Reviewer's comment ID #: 244-73)]	Accepted – see response to comment 6-1221.
6-798	A	33:22		The title of Section 6.6.1.2 (in italics) should be changed to "What do ground surface temperature reconstructions derived from subsurface temperature measurements tell us?" [Govt. of United States of America (Reviewer's comment ID #: 2023-420)]	Accepted.
6-1221	B	33:22		Please change ground surface to subsurface: What do large-scale temperature histories from subsurface temperature measurements tell us? [Hugo Beltrami (Reviewer's comment ID #: 306-1)]	Accepted.
6-799	A	33:26	33:26	Delete extra ")" [James Crampton (Reviewer's comment ID #: 50-30)]	Accepted.
6-1222	B	33:29	:31	I suggest you change the paragraph to read: Because the solid Earth acts as a low-pass filter on downward-propagating temperature signals, high frequency noise, typical of SAT is filtered out in the shallow subsurface. The ground temperature then records preferentially the sustained trends of the energy balance at the ground surface. This preferential filtering caused by heat diffusion implies that borehole reconstructions portray only multi-decadal to centennial changes. [Hugo Beltrami (Reviewer's comment ID #: 306-2)]	Rejected - current text expresses sense intended.
6-800	A	33:32	33:32	the use of "surface temperature history" is problematic in its ambiguity. This could easily be confused with "surface air temperature". It should be explicilty acknowledged that boreholes provide an estimate of "ground surface temperature" which may or may not be similar to "surface air temperature" (which is what instrumental thermometer records provide). [Michael Mann (Reviewer's comment ID #: 156-33)]	Accepted – "ground" added before "surface".
6-1223	B	33:32	:33	Subsurface temperatures are not proxy, but direct measurements of energy. Please delete other from below to read; "reconstructions provide independent estimates of surface temperature history with which to compare multiproxy reconstructions."	Accepted.

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				[Hugo Beltrami (Reviewer's comment ID #: 306-3)]	
6-801	A	33:37	33:38	It should be noted here that these sampling error estimates do *not* take into account potential systematic biases as pointed out by Mann et al (2003). [Michael Mann (Reviewer's comment ID #: 156-34)]	Rejected – not clear what the reviewer is referring to.
6-1224	B	33:41	:42	There has been at least one more analysis of the dataset already referenced in the text [Beltrami and Bourlon, 2004], which confirms the analysis of Huang et al (2000) and Pollack and Smerdon (2004). Please add this as suggested below. 19th and early 20th centuries. A geospatial analysis of the Huang et al. (2000) results by Mann et al.(2003) (see correction by Rutherford and Mann, 2004) argued for significantly less overall warming, a conclusion contested by Pollack and Smerdon (2004) and by Beltrami and Bourlon (2004), who reach the same conclusions as Pollack and Smerdon (2004) in an independent analysis of the same borehole data set . [Hugo Beltrami (Reviewer's comment ID #: 306-4)]	Accepted (in part) - text revised.
6-1225	B	33:42	:44	This is statement is not quite right. I suggest you delete this sentence. The subset of data from Canada measured after 1980 contains that warming. In fact those measurements have been included in the Beltrami and Bourlon (2004) analysis as well in the "newer data" analysis for Canada (Beltrami et el., 2003). This does not alter the results. [Hugo Beltrami (Reviewer's comment ID #: 306-5)]	Noted – text amended but new reference not included for 'balance' in number of borehole citations.
6-802	A	33:46	34:4	The discussion of shortcomings in borehole paleothermometry is clear, and might even include the possibility that groundwater motion affects temperature profiles. However, as noted next, the lack of a similar discussion of tree rings is not appropriate. The chapter is at some pains to point out possible errors in certain indicators (borehole temperatures, glaciers) while failing to make a similar treatment of tree rings, which are the primary indicators in most of the reconstructions. And yes, the chapter does have brief and general discussion of shortcomings, but I did not see the same detail as for the glaciers and boreholes. [Richard B. Alley (Reviewer's comment ID #: 4-8)]	Accepted – tree-ring data problems will be discussed in additional paragraph.
6-803	A	33:46	34:4	Comment: First attempts to compare actual borehole profiles and model simulations have recently been published (Beltrami et al. 2006: Geophys. Res. Letters, 33, L09705) suggesting that variations in external forcing factors are needed to account for the trends observed in borehole profiles. [Jesús Fidel González-Rouco (Reviewer's comment ID #: 86-4)]	Noted – but no change to text required.
6-804	A	33:46	34:4	The authors seem to dismiss the influence of snow cover on the ground surface temperature. While they may be right that a few extreme years may have little effect on	Rejected- not so – see text line 51 page 6-33, but the Taylor et al (2006)

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				the long-term trend, they do not consider what the effect may be of a longer-term change in snow cover. The surface temperature will reflect all changes that occur in the surface energy balance or local climate which include changes in snow cover. Taylor et al. 2006 (see comment 74 for ref) found that surface temperatures increased with an increase in total snow (provides insulation) and this appears to have been sufficient to conteract a decrease in air temperature over the same period. These changes in snow cover, therefore could buffer changes in air temperature. Osterkamp and Romanovsky (1999) also found that significant warming of permafrost (which would be in response to increases in ground surface temperature) in response to an increasing trend in snow cover (ref. Osterkamp, T.E. and Romanovsky, VE. 1999. Evidence for warming and thawing of discontinuous permafrost in Alaska. Permafrost and Periglacial Processes 10:17-37). [Sharon Smith (Reviewer's comment ID #: 244-76)]	reference has been added.
6-805	A	33:46	34:4	Comment: First attempts to compare actual borehole profiles and model simulations have recently been published (Beltrami et al. 2006: Geophys. Res. Letters, 33, L09705) suggesting that variations in external forcing factors are needed to account for the trends observed in borehole profiles. [Govt. of Spain (Reviewer's comment ID #: 2019-47)]	See response to comment 6-803.
6-806	A	33:49	:52	On lines 49 and 52 there is a reference to "Smerdon et al., in press". This paper has now been published, so substitute "2006" for "in press". [Govt. of United States of America (Reviewer's comment ID #: 2023-421)]	Accepted.
6-1226	B	33:49	:49	There is one new paper confirming the long-tern SAT ground temperature coupling using a cluster of boreholes in Canada (Beltrami et al., 2005). Please add that reference in this line. [Hugo Beltrami (Reviewer's comment ID #: 306-6)]	Rejected – need to curtail references in this section and one study of a limited region is not adequate to draw a general conclusion..
6-807	A	33:51	33:51	Should reference Mann and Schmidt (2003) here along with the other two references cited. [Michael Mann (Reviewer's comment ID #: 156-35)]	Rejected – this reference appropriately cited on line 55.
6-1227	B	33:52	:53	A recent paper, Beltrami et al., (2005). Have shown this with 80 years of SAT data. Please include this in that sentence. (Smerdon et al., in press). Observational time-series of ground temperatures are not long enough to establish whether the mean annual differences are stable over long time-scales, although Beltrami et al., (2005) have shown that SAT and borehole data are coupled for at least 80 years, even in regions with variable snow cover, in Canada (Beltrami et al., 2005). [Hugo Beltrami (Reviewer's comment ID #: 306-7)]	Rejected – the sentence refers to observational time series of ground temperatures , not SAT.
6-808	A	33:55	33:55	Insert comma at end of line	Accepted.

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				[James Crampton (Reviewer's comment ID #: 50-31)]	
6-809	A	33:57	34:4	It must be acknowledged here as it is later in the chapter (page 37), that the conclusions of Gonzalez-Rouco et al are compromised by a major error in the simulation as identified by Osborn et al (2006), which renders most of the long-term variability in the simulation (which forms the basis for their interpretation about ground surface temperature changes) unphysical in nature. The conclusion by Mann and Schmidt (2003) strongly contests the Gonzalez-Rouco claim, and is based on a model simulation that does not depend on spinup errors. Gonzalez-Rouco et al (2006) claim to reach a similar conclusion to Gonzalez-Rouco et al (2003), but as this simulation two may contain serious errors (there is certainly an error in their 20th century forcing, which does not include tropospheric aerosols), any work by Gonzalez-Rouco and collaborators is suspect until independent analyses are done by other modelers to determine whether Mann and Schmidt (2003) or Gonzalez-Rouco et al are closer to the truth with regard to the disconnect between GST and SAT on long timescales. Given other work as cited here which has found significant differences in the presence of seasonal snowcover and land-use change, the Gonzalez-Rouco et al conclusions seems dubious at best. It is dangerous for the AR4 to give them these dubious studies the weight that are currently given them, given the multiple errors and undisclosed erroneous procedures that are already known to have riddled the Von Storch, Zorita, Gonzalez-Rouco et al studies to date. [Michael Mann (Reviewer's comment ID #: 156-36)]	Rejected – reviewer seems to be over-critical of the potential value in some of the papers referred to here despite the known (and described as the reviewer mentions) problems with the German simulation. It is not clear whether these problems seriously compromise conclusions being refered to here - this assessment is attempting to provide a dispationate view of the issues.
6-1228	B	33:	34:	Section 6.6.1.2 Comment: In addition, an important point that should included in this section. Borehole temperature have been instrumental in showing that the ground has absorbed as much energy as the whole atmosphere in the last 50 years (Levitus et al., 2001, Beltrami et al., 2002, Levitus et al., 2005 end references therein , and Beltrami et. al., 2006a) . This is extremely important because there is no doubt in here that this is real energy since, there is little mathematical modeling involved when measuring the heat gain underground Recall also that the effects of surface effects are not involved here. It is a simple a nd plain calculation of increased heart iunderground.. These are very solid results, and confirm along with the work of Levitus for the ocean, that the all climate subsystems have gain energy in the recent past and provide strong evidence that the present warming has a global character. This I believe is a very important point and must be included in this section. It should also be included in the table in page 6-41 as a robust finding. [Hugo Beltrami (Reviewer's comment ID #: 306-9)]	Rejected – many of these issues are relevant for Chapters other than 6 (i.e. 3, 4 and 5) and the cummulative knowledge gained is not most appropriately summarised , here
6-1229	B	33:	34:	Comment continuation: Please add to the text the results of Beltrami et al. (2006b) that show the first comparison of borehole temperature and the output from a GCM for an	Rejected – the results desribed by the reviewer are interesting but the various

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				extensive area of Canada. This could go here in line 47 page 6-33 or in the section 6.6.3 Paleoclimate Model-Data or in one of its subsections. General circulation models (GCMs) and past climate records should ideally provide similar views of the recent climate evolution. One such record is temperature versus depth profiles logged within continental boreholes which provide information for the past long-term temperature evolution from an integrated history of surface heat dissipation through the crust. To conduct such a comparison, Beltrami et al. (2006) used three millennial simulations with the ECHO-g GCM. One simulation kept present conditions of climate forcing constant, and two other simulations incorporated estimations of the evolution of some external forcing factors (solar variability, volcanic aerosols, and greenhouse gases). Using surface air temperatures generated by these models, the authors constructed simulated temperature versus depth profiles, which they then compared with existing borehole data from Canada. Their results suggest that the warming observed in the continental subsurface across Canada cannot be explained by solely by internal variability of the Earth's climate. I enclose one of the figures (BELTRAMIHugo_AR4ERSOD_Ch06_Sup.png) with the comparison. A higher resolution figure can be provided. [Hugo Beltrami (Reviewer's comment ID #: 306-10)]	assumptions made, implicitly or explicity, can not be accommodated without substantial addition to the current text –which is not possible given the severe space restrictions now being imposed.
6-1230	B	34:1		The Gonzalez-Rouco (2006) paper does more than demonstrate long-term SAT and ground temperature coupling, it also shows that the borehole method of climate reconstructions works well although with decreasing resolution back in time. [Hugo Beltrami (Reviewer's comment ID #: 306-8)]	Noted – but no text change necessary.
6-1231	B	34:12	34:12	What happened to the Law Dome proxy? Why isn't it shown? [Stephen McIntyre (Reviewer's comment ID #: 309-115)]	Past temperature variations at Law Dome have been inferred from isotopic and borehole records. (1) Jones and Mann (2004) showed an isotope record from Law Dome based on O18. This record has a "cold" present-day and "warm" 1000-1750 period. Dahl-Jensen et al. (1999) showed temperature variations at Law Dome obtained by inverting the borehole temperature profiles. This record has a colder interval (peaking in 1250 and 1850) relative to the recent period, followed by a steady recent warming. Therefore, the opposite trends recorded in these reconstructions do not

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					allow reaching a final consensus on temperature variations at Law Dome during the past millennium.
6-810	A	34:16	34:19	Well-dated proxy records in the SH include Williams et al 2004 (The Holocene 14(2), 194-208) and Williams et al. 2005 (Earth & Plan Sci Letters 230, 301-317). Both show evidence for warm period around 700 years ago. [Paul W Williams (Reviewer's comment ID #: 291-5)]	Rejected. These stalagmite records are not high-resolution records. Comparisons with the reconstructions included in the SH section are not straighforward.
6-1232	B	34:25	34:25	Mann and Jones 2003 said that Cook's NZ proxy was not a temperature proxy. Why is it used? [Stephen McIntyre (Reviewer's comment ID #: 309-116)]	Mann and Jones (2003) did not stated that Cook's NZ record was not a temperature proxy, they said that the reconstruction do not significantly correlated with gridded instrumental temperature records.
6-811	A	34:25	35:7	This section in large part replicates the dicussion in Chap. 2. I suggest that the two sections be merged (in Chap 2) and only the implementation issues discussed here. [Gavin Schmidt (Reviewer's comment ID #: 227-10)]	Accepted. Discussion in this sections are reduced to implementation issues.
6-812	A	34:28	34:28	The longest tree-ring reconstruction for New Zealand is for kauri (Agathis australis) and extends from 1998 to 1724 BC (3723 years). See Boswijk et al 2006 The Holocene 16(2), 188-199. [Paul W Williams (Reviewer's comment ID #: 291-7)]	Boswijk et al (2006) report on the development of a long kauri chronology in New Zealand. Its potential to reconstruct past climate variations has not been explored yet.
6-813	A	34:34	34:39	What is written here about South America is correct but gives a biassed view because you only quote the positives. In N Patagonia, while the 20th century is warmest, the maximum is in about 1930. This should be presented more even-handedly. [Eric Wolff (Reviewer's comment ID #: 292-34)]	Accepted, text modified to reflect the comment.
6-814	A	34:36	34:36	The reference to figure 6.12 should be moved to the next sentence. [Marie-France Loutre (Reviewer's comment ID #: 148-25)]	Accepted, text modified to reflect the comment.
6-815	A	34:43	:44	This section is dealing with the southern hemisphere. The sentence "…these both indicate unusually warm conditions prevailing in the 20th century (Pollack and Smerdon, 2004)", and the reference therein are both incorrect. The ground surface temperature changes over the last 500 years do not indicate unusually warm conditions prevailing in the 20th century in Australia and southern Africa. This is because the unusually warm conditions developed late in the century, after most of the boreholes had already been logged. What the borehole reconstruction for Australia does show is very good correspondence with the Cook et al. (2000) reconstruction for Tasmania and the Cook et al. (2002) reconstruction for New Zealand. The Australia work is described in a manuscript "Five centuries of	Accepted. Current text clearly states that warmer conditions in south Africa and Australia post-dated the time when the boreholes were logged and thus the most recent warming is not recorded in the borehole time series.

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				Climate Change in Australia: The View from Underground", by Pollack, Huang and Smerdon, accepted for publication in the Journal of Quaternary Science. The Africa work by the Pollack group is unpublished. [Govt. of United States of America (Reviewer's comment ID #: 2023-422)]	
6-816	A	34:46		change "no" to "not" [Raimund Muscheler (Reviewer's comment ID #: 185-7)]	Accepted, text modified to reflect the comment.
6-817	A	34:46		not registered [Eric Wolff (Reviewer's comment ID #: 292-35)]	Accepted, text modified to reflect the comment.
6-818	A	34:52	36:44	First part of section 6.6.3 is in fact a discussion on the forcings of the last millenium and not really a data-model comparison and could be integrated with section 6.6.4; I do not understand why section 6.6.3.4 is not integrated in the section 6.6.1 as the simulations discussed there are already discussed in 6.6.1 as pseudo proxies. Space should be saved with a better integration [Joel GUIOT (Reviewer's comment ID #: 92-3)]	Noted - this issue will be reviewed , though the discussion of forcings must come before that of comparison of simulation results.
6-819	A	34:56	35:2	I do not understand the rationale for the references quoted here. Indeed, some models quoted in table 6.2 and which results are used in figure 6.13 are not cited here (e.g. Osborn et al; Goosse et al.; Gonzalez-Rouci et al.; Stendel et al) [Marie-France Loutre (Reviewer's comment ID #: 148-26)]	Noted - the text is intended to provide examples only and will be modified to refer to Table 6.2 , where details of all simulations used are provided.
6-1233	B	34:		Section 6.6.2. Comment: The magnitude of warming in the SH, as far as I recall, are smaller than those in the NH. Please check the work of Pollack et. al (2006). Reference is below to an in press paper. [Hugo Beltrami (Reviewer's comment ID #: 306-12)]	Accepted. Text modified to reflect the comment.
6-820	A	35:0		Figure 6.13 and table 6.2. The curve AJS is not included in the figure. [Eduardo Zorita (Reviewer's comment ID #: 304-2)]	Rejected – the curve is included , though it is covered by others in 6.13c
6-821	A	35:4	35:10	Of course, the differing sensitivities to forcing among the different models (e.g. arising from different parameterizations of various processes such as clouds which influence the representatinos of important feedbacks) is also a key factor in explaining the differences among simulation results. [Michael Mann (Reviewer's comment ID #: 156-40)]	Noted – true , but no alteration of text required
6-822	A	35:6	35:6	Use consistent referencing for the astronomical solution. This is Berger, Journ. Atm. Sci., 1978 [Michel Crucifix (Reviewer's comment ID #: 52-73)]	Rejected – current referencing arose as a result of previous comments
6-823	A	35:7	35:7	..." in terms of the latitudinal and seasonal changes in incoming shortwave radiation at the top of the atmosphere" [Michel Crucifix (Reviewer's comment ID #: 52-74)]	Accepted
6-824	A	35:12		FIGURE 6.13 presents radiative forcings relative to a 1500-1899 mean. In Chapter 2,	Noted – the reference period used in

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				radiative forcing is defined relative to 1750, and this is how time series are presented earlier (e.g. FIGURE SPM.1). [Adrian Simmons (Reviewer's comment ID #: 242-102)]	Chapter 6 was considered the appropriate one in the context of a disc ussion of Paleodta over the last 500-1300 years
6-825	A	35:18	35:18	Replace "good" by "strictly limited" [VINCENT GRAY (Reviewer's comment ID #: 88-778)]	Rejected – suggested wording not appropriate
6-826	A	35:25	36:7	Include a discussion on the effect of changes in total solar irradiance on coupled stratosphere-troposphere dynamics and stratosphere chemistry (ozone). This is relevant because this may explain how the relatively weak solar forcing during the Holocene and during the last millennium (typically 0.3 W/m2) may have had a relatively large imprint on continental temperatures. [Michel Crucifix (Reviewer's comment ID #: 52-77)]	Rejected. This topic is discussed in Chapter 2, section 7.
6-827	A	35:27	35:27	Reference section 2.7 directly rather than whole chapter? [Piers Forster (Reviewer's comment ID #: 73-31)]	Accepted
6-828	A	35:30	35:30	Add at end "The effects of various feedbacks, such as the influence of cosmic rays, or influences on cloud cover, are less well known" [VINCENT GRAY (Reviewer's comment ID #: 88-779)]	Rejected. No basis for assertion given. This topic is discussed by chapter 2
6-829	A	35:37	35:40	Which relationships are considered as well-known, and which are more speculative is not entirely clear from the text. Do I understand well that the Sun's open magnetic field is associated with the sunspot number, and that the sun's closed magnetic field is associated with the energy input ? [Michel Crucifix (Reviewer's comment ID #: 52-75)]	Noted.Sentence is clear that these relationhsips are not fully understood.
6-830	A	35:47	35:48	Muscheler et al. (2005) shows only the last 500 years. If 1137-1146 should be included one should refer to the Muscheler et al. (accepted) paper. There are uncertainties, time resolution differences, ... Therefore, I think it is not good to define the periods of increased solar activity as it is done in the present version. Suggestion: ... for the last millenniuim, three periods (around AD 1785, 1600, 1140) when solar ... [Raimund Muscheler (Reviewer's comment ID #: 185-8)]	Accepted
6-831	A	35:50	35:50	Specify better what is meant by "long-term" trend (give an explicit time scale) [Michel Crucifix (Reviewer's comment ID #: 52-76)]	Rejected. It is clear from the context that multi-decadal to centennial trends are discussed
6-832	A	35:51	35:51	Correction: '… or refute the analysis by (Baliunas and Jastrow, 1990)…' --> '… analysis by Baliunas and Jastrow (1990)…' [Govt. of Spain (Reviewer's comment ID #: 2019-48)]	Accepted.
6-833	A	35:51		Should read Baliunas and Jastrow (1990). [Govt. of United States of America (Reviewer's comment ID #: 2023-423)]	Accepted.

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6-834	A	35:52	35:52	This title is not an accurate description of the section which only discusses a small class of transient model runs for the last millennium. There are many more ways in which to do a model-paleodata comparison (some of which are discussed elsewhere. I suggest that the title be changed to something like “Comparisons of millennial simulations with paleo-data” or something. [Gavin Schmidt (Reviewer’s comment ID #: 227-11)]	Assume it is p. 34, line 52. Accepted
6-835	A	35:53	35:53	Reference section 2.7 directly rather than whole chapter? [Piers Forster (Reviewer’s comment ID #: 73-32)]	Accepted.
6-836	A	36:6	36:6	Reference section 2.7 directly rather than whole chapter? [Piers Forster (Reviewer’s comment ID #: 73-33)]	Accepted.
6-837	A	36:21	36:21	Correction: '… in global radiative forcing with no altitudinal or spatial …' --> '… in global radiative forcing with no LATITUDINAL or spatial …' [Jesús Fidel González-Rouco (Reviewer’s comment ID #: 86-6)]	Taken into account. Text deleted
6-838	A	36:21	36:21	Correction: '… in global radiative forcing with no altitudinal or spatial …' --> '… in global radiative forcing with no LATITUDINAL or spatial …' [Govt. of Spain (Reviewer’s comment ID #: 2019-49)]	Taken into account. Text deleted
6-839	A	36:30		It seems unnecessary to quote the unpublished Mieding paper here when you already have two good references. [Eric Wolff (Reviewer’s comment ID #: 292-36)]	Accepted.
6-840	A	36:32	36:33	I have not been able to check the Stern (2005) reference, but an important factor in raising the sulfate concentration was not just the total emissions in the regions, but also the later shift of emission to tall stacks, which in effect increased SO2/SO4 lifetime from a couple of days to perhaps 10-14 days, so creating an increase far greater than caused by just the increase in emissions. I am wondering if this has been accounted for, and whether this general parallel evolution is just by glancing at the record or a result of a thorough analysis. Thus, should not tall stacks also be mentioned in this sentence along with the general increase in emissions? [Michael MacCracken (Reviewer’s comment ID #: 152-264)]	Taken into account. Text edited and shortened. Discussion of shift to tall stacks not included for space reasons.
6-841	A	36:40	36:40	Add at end "All this was done by assuming that the large temperature peak in 1999 usually attributed to an El Niño anomaly, can be considered to generate a spurious "linear trend" ever since 1978,, implyiong its depenedence on human-induced forcing" [VINCENT GRAY (Reviewer’s comment ID #: 88-1265)]	Rejected – the suggested wording is not appropriate and no justification is offered by the reviewer to support its inclusion.
6-842	A	36:43	36:43	Reference section 2.9 directly rather than whole chapter? [Piers Forster (Reviewer’s comment ID #: 73-34)]	Accepted
6-843	A	36:50	36:50	Delete "generally good" [VINCENT GRAY (Reviewer’s comment ID #: 88-780)]	Rejected – the current txt is appropriate

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6-844	A	36:54	36:54	"is constrained to be small" : develop briefly (the records have been aligned and normalised) [Michel Crucifix (Reviewer's comment ID #: 52-78)]	Rejected – the data have been centered but not normalised , and the citation is provided to provide more detail of the effect.
6-845	A	36:56	36:56	The reader cannot identify the ECHO-G simulation on Fig. 6.13, as implied in the text [James Crampton (Reviewer's comment ID #: 50-32)]	Noted – the Figure is being modified and the identification of this simulation will be reviewed. The text has been modified to change "dotted" to "dashed".
6-846	A	37:1	37:9	Comment related to this piece of text and Figure 6.13d and Table 6.2: The text and figure illustrate that for the post-1990 period only the ECHO-G simulations exhibit greater early 20th century warming in comparison to the other simulations and this is associated to not including troposferic aerosols among the forcings. Subsequently, the text states that ' All of these simulations, therefore, appear to be consistent witht he available evidence from reconstructions ...' This argumentation could be missleading:while it is true that the ECHO-g steps out of the rest of the simulations in the post-1990 interval, the other simulations include different arrays of forcing and thus are hardly comparable. Some only include a partial representation of greenhouse gases (C instead of G) as is the case of the climber2 simulations. More greenhouse gas (ghg) load would presumably rise the level of warming in these runs for the post-1990 period. Also, these simulations do not include aerosols as in the case of ECHO-g, though they include land use changes, factors that count in the opposite direction to ghg. It is uncertain whether omitting some ghg forcing and aerosols will equilibrate. The situation is that, since the different simulations are not considering comparable sets of forcing factors, the purported 'consistency' is subject to be based on the casual coincidence of model responses to different sets of forcing factors. [Jesús Fidel González-Rouco (Reviewer's comment ID #: 86-7)]	Noted - the text is correct in stating a general consistency exists between the simulations and our current knowledge of past temperatures – though it is true that the precise role of specific forcings , as opposed to differing model sensitivities, is not discussed. There is currentlty no published literature that explores this issue and a new analysis is beyond the scope of this assessment. A clear statemnt is , however, made about the limited implications that can interpreted through this comparison. We assume the reviwer means 1900 and not 1990 in the comment.
6-847	A	37:1	37:9	Comment related to this piece of text and Figure 6.13d and Table 6.2: The text and figure illustrate that for the post-1990 period only the ECHO-G simulations exhibit greater early 20th century warming in comparison to the other simulations and this is associated to not including troposferic aerosols among the forcings. Subsequently, the text states that ' All of these simulations, therefore, appear to be consistent witht he available evidence from reconstructions ...' This argumentation could be missleading:while it is true that the ECHO-g steps out of the rest of the simulations in the post-1990 interval, the other simulations include different arrays of forcing and thus are hardly comparable. Some only include a partial representation of greenhouse gases (C instead of G) as is the case of the climber2 simulations. More greenhouse gas (ghg) load would presumably rise the level of warming in these runs for the post-1990 period. Also, these simulations do not include	See response to Comment 6-846

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				aerosols as in the case of ECHO-g, though they include land use changes, factors that count in the opposite direction to ghg. It is uncertain whether omitting some ghg forcing and aerosols will equilibrate. The situation is that, since the different simulations are not considering comparable sets of forcing factors, the purported 'consistency' is subject to be based on the casual coincidence of model responses to different sets of forcing factors. [Govt. of Spain (Reviewer's comment ID #: 2019-50)]	
6-848	A	37:2	37:2	The reference to the ECHO-G simulation should be to Fig. 6.13d, not 6.11d? [James Crampton (Reviewer's comment ID #: 50-33)]	Accepted
6-849	A	37:2	37:2	"dotted red line in Figure 6.11d" I think it is a dashed line actually. [Gareth S. Jones (Reviewer's comment ID #: 121-61)]	Accepted
6-850	A	37:2	37:7	Among all the simulations shown in Figure 6.13, the ECHO-G simulation is the only one performed with a GCM that covers the whole millennium. Apparently, the figure should include the curve AJS with the CSM model simulation, but unfortunately it does not. Therefore, this fact in theory, could explain part of the differences to the other simpler models in the initial centuries of the millennium. According to my own calculations, the Northern hemisphere temperature simulation in AJS is very close to the second simulation with the model ECHO-G mentioned in this paragraph. The AJS simulation includes also aerosol forcing in the 20th century, so that the explanation for the warmer 20th temperatures in the second ECHO-G simulation may not hold.. If the figure had include the AJS the reader could conclude that the ECHO-G simulations are not as atypical as this paragraph seems to indicate. Furthermore, other simulations apart from the ECHO-G simulations also lack other important forcings. For instance Bauer et al. omit methane forcing. [Eduardo Zorita (Reviewer's comment ID #: 304-3)]	Rejected – Figure 6.13 does indeed include the AJS simulation . The "other forcings" curve (Figure 6.13c) is masked by other simulations. The differences between this Figure and the analysis undertaken by the reviewer very likely relates to the use of different base periods (1500-1899 here and perhaps a shorter, recent period in the reviewr's analysis). The specific forcings used in the models are described in Table 6.2.
6-851	A	37:2		You must mean Fig 6.13 not 6.11 [Eric Wolff (Reviewer's comment ID #: 292-37)]	Accepted
6-852	A	37:4	37:5	"anomolies are likely … in that simulation (see Figure 6.13c)." I can't see the ECHO-G simulation (GSZ2003, GSZ2006) in Figure 6.13c. Is it there, buried under other simulations? Can you please clarify the graphic. [Melinda Marquis (Reviewer's comment ID #: 162-4)]	Rejected – the forcings for the ECHO-G model are masked by those of other runs – at present we can not see a way to overcome this problem.
6-853	A	37:8	37:9	Reformulate sentence ("evidence" appears twice) [Michel Crucifix (Reviewer's comment ID #: 52-79)]	Accepted
6-854	A	37:12	37:15	"…the magnitude of which is currently in doubt. ...they are not a powerful test..." seems to be an attempt to describe the uncertainty of the information. In order to communicate this uncertainty in a consistent way please use a quantified likelihood for the statement as proposed in the guidance notes for lead authors of the AR4 on Addressing uncertainties (IPCC, July 2005).	Noted, will attempt a rewrite

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				[Govt. of Germany (Reviewer's comment ID #: 2011-24)]	
6-855	A	37:16	37:20	Very nice to see models like this being used to investigate forcing uncertainty. However what is the original references for the plots described. As far as I can tell the Petoukhov, Plattner and Montoya papers only describe the models used, not the simulations. If there is no published work (or whatever IPCC criteria for acceptance) then sadly these plots might have to be excluded? [Gareth S. Jones (Reviewer's comment ID #: 121-62)]	Rejected. It is a tradition that IPCC is using published models to perform specific simulations and evaluate scenarios.
6-856	A	37:30		The simulations are shown in 6.13 d, not 6.13b.It's the irradiance change that is in 6.11b. [Eric Wolff (Reviewer's comment ID #: 292-38)]	Accepted
6-857	A	37:37	37:40	How can the temporal evolution be similar while the amplitude of the Maunder Minimum is three times as much as in BARD25 ? Or is it just the overall temporal pattern that is similar ? Please clarify. [Michel Crucifix (Reviewer's comment ID #: 52-80)]	Taken into account. An additional figure has been added that clarifies this point.
6-858	A	37:43	37:43	There is a mistake in the number of the figure. Please change 6.11c to 6.10c [Govt. of Germany (Reviewer's comment ID #: 2011-25)]	Accepted
6-859	A	37:52	37:56	Comment: While this reads strictly true from figure 6.13, it might be appropriate to highlight that both in the case of reduced and non-reduced solar forcing, natural forcings seemed to have a non-negligible contribution to warming since greenhouse gases forcing started increasing (~1800 AD) [Jesús Fidel González-Rouco (Reviewer's comment ID #: 86-5)]	Noted. Peak warming in the middle of the 20th century is already mentioned on line 52/53 of page 37.
6-860	A	37:52	37:56	I think that you are justified in making a stronger statement about the inadequacy of natural forcings and the importance of anthropogenic forcings in explaining the warming of the last century. There is a very thorough discussion of natural forcings in Chapter 2 (Section 2.7), and the evidence really is becoming overwhelming that solar forcing is nowhere near large enough (and the volcanic forcing trend is negative). You should also cross-reference the discussion of radiative forcings in Chapter 2. I think that Chapter 6 is a very important chapter, so you do need to make strong statements (as strong as the science justifies). [Danny Harvey (Reviewer's comment ID #: 101-41)]	Noted. The text on page 38, line 4 to 8 summarises the results and provide our conclusion.
6-861	A	37:52	37:56	Comment: While this reads strictly true from figure 6.13, it might be appropriate to highlight that both in the case of reduced and non-reduced solar forcing, natural forcings seemed to have a non-negligible contribution to warming since greenhouse gases forcing started increasing (~1800 AD) [Govt. of Spain (Reviewer's comment ID #: 2019-52)]	See comment 859
6-862	A	38:0	41:	Good to see some mention of hydroclimatic variability in this chapter, but it almost seems like a footnote. The main focus is on past temperatures and the drivers of temperature variability in order to place current warming into a long-term context and to differentiate	Noted, but not much available space to elaborate on tis issue

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				the roles of different forcing mechanisms. In parts of North America, and perhaps elsewhere, the future scenarios for changes in moisture seem to be varied, and unlike temperature, currently clear hydroclimatic responses to global warming are not as obvious as temperature responses. As is mentioned in this section, some paleo records suggest radiative forcings have influenced ENSO, which has an important influence on precipitation in many regions. In other cases, the causal mechanism for drought events and precipitation regime changes are not clear. That being said, it might be good to point out that warming temperatures, by themselves, will have large impacts on hydrology and water availability, even with no changes in moisture regimes. Breshears et al. (2005) suggest that the recent drought in the western U.S. was perhaps a taste of what is to come: global-change type droughts. Increased temperatures can alter hydrographs, change the precipitation to snow ratio, increase demand, evaporation, and evapotraspiration, and lead to persistence of drought conditions (e.g., Oglesby and Erickson 1989). If 20th-21st century warming exacerbated the recent drought, what would be the impact of this amount of warming (or more, as projected) on a drought such documented in the paleoclimatic record at the end of the 16th century? If a temperature increase was superimposed over this widespread and severe drought (which did occur during a period of generally cooler conditions), the chances are it would be even more widespread and persistent. It would be interesting to re-calculate the reconstructed gridded PDSI values for western North America (Cook et al. 2004b) with increased temperatures for this period. Maybe it would not make too much difference since temperature is a not a dominant factor in PDSI, but it might be an interesting exercise, if it could be done. As is mentioned in Section 6.6.5.5., proxy records show that the range of drought characteristics in the 20th century do not contain the full range of variability in the past 150-2000 years. These records contain evidence for relatively short droughts (4-6 years) that exceed the severity of droughts of similar length in the 20th century (e.g., the 1950s drought) as well as runs of years with below average conditions that persist for many more years than seen in the modern period. In both cases, these droughts under warmer conditions would likely result in more widespread, persistent, and/or severe events. The impacts of these paleo-type droughts under warmer conditions are apt to be far reaching, as has been hinted at with the recent drought. Just a very few mid-level water managers in the western United States, with the foresight and courage to start considering the implications of the paleo records in concert with the regional impacts on temperature from global warming, are beginning to incorporate this information into planning. It would be enormously helpful to bring up these points in the SPM in order to get this kind of thinking and planning elevated to a higher level of decisionmaking. [Govt. of United States of America (Reviewer's comment ID #: 2023-424)]	
6-863	A	38:0	41:	Additionally, section 6.6.5.5 considers ENSO impacts in a manner that is far too	Rejected, assesment based on current

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				deterministic. There is great variability in ENSO and its impacts. All El Niño events do not look like the canonical El Niño. [Govt. of United States of America (Reviewer's comment ID #: 2023-425)]	evidence
6-864	A	38:0	41:	Add a short section on Central Asia and Middle East region. Page 40, line 24 would be an appropriate place for such a section. There is adequate proxy data for this. [Govt. of United States of America (Reviewer's comment ID #: 2023-426)]	Rejected, quality of data makes this problematic
6-865	A	38:0	41:	Section 6.6.5. I'm glad to see some mention of hydroclimatic variability in this chapter, but it almost seems like footnote to the chapter, and it might be good to highlight this section a bit more. I realized that the main focus is on past temperatures and the drivers of temperature variability in order to place current warming into a long-term context and to differentiate the roles of different forcing mechanisms. In parts of North America, and perhaps elsewhere, the future scenarios for changes in moisture seem to be varied, and unlike temperature, currently clear hydroclimatic responses to global warming are not as obvious as temperature responses. As is mentioned in this section, some paleo records suggest radiative forcings have influenced ENSO, which has an important influence on precipitation in many regions. In other cases, the causal mechanism for drought events and precipitation regime changes are not clear. [Connie Woodhouse (Reviewer's comment ID #: 293-3)]	Noted, no changes needed in text
6-866	A	38:0	41:	That being said, it might be good to point out that warming temperatures, by themselves, will have large impacts on hydrology and water availability, even with no changes in moisture regimes. Breshears et al. (2005) suggest that the recent drought in the western U.S. was perhaps a taste of what is to come: global-change type droughts. Increased temperatures can alter hydrographs, change the precipitation to snow ratio, increase demand, evaporation, and evapotraspiration, and lead to persistence of drought conditions (e.g., Oglesby and Erickson 1989). If 20th-21st century warming exacerbated the recent drought, what would be the impact of this amount of warming (or more, as projected) on a drought such documented in the paleoclimatic record at the end of the 16th century? If a temperature increase was superimposed over this widespread and severe drought (which did occur during a period of generally cooler conditions), the chances are it would be even more widespread and persistent. [Connie Woodhouse (Reviewer's comment ID #: 293-4)]	Noted, no changes needed in text
6-867	A	38:0	41:	It would be interesting to re-calculate the reconstructed gridded PDSI values for western North America (Cook et al. 2004b) with increased temperatures for this period. Maybe it would not make too much difference since temperature is a not a dominant factor in PDSI, but it might be an interesting exercise, if it could be done. As is mentioned in 6.6.5.5., proxy records show that the range of drought characteristics in the 20th century do not contain the full range of variability in the past 150-2000 years. Proxy records contain	Noted, no changes needed in text

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				evidence for relatively short droughts (4-6 years) that exceed the severity of droughts of similar length in the 20th century (e.g., the 1950s drought) as well as runs of years with below average conditions that persist for many more years than seen in the modern period. In both cases, these paleo-documented droughts under warmer conditions would likely result in more widespread, persistent, and/or severe events. [Connie Woodhouse (Reviewer's comment ID #: 293-5)]	
6-868	A	38:0	41:	The impacts of these droughts under warmer conditions are apt to be far reaching, as has been hinted at with the recent drought. Just a very few mid-level water managers in the western US, with the foresight and courage to start considering the implications of the paleo records in concert with the regional impacts on temperature from global warming, are beginning to incorporate this information into planning. It would be enormously helpful to bring up this point in the summary for policy-makers in order to get this kind of thinking and planning elevated to a higher level of decision making. [Connie Woodhouse (Reviewer's comment ID #: 293-6)]	Noted, no changes needed in text
6-869	A	38:4		somewhere in here it is pertinent to point out that hegerl et al 2003 conducted the most thorough detection and attribution study to date with several different paleoclimate reconstructions and found that whereas the response to volcanism is highly significant, the response to solar is iffy at best, and that with the longer records the ghg signal can be detected by mid 20th c. - hegerl et al j clim submitted quantiy that about one-third of the mid 20th c warming can be attributed to ghg. ref is grl 2003 doi:10.1029/2002GL016635, 2003 [Thomas Crowley (Reviewer's comment ID #: 51-31)]	Rejected – severe space constraints prevent this and the issue is more appropriate for Chapter 9.
6-870	A	38:14	38:14	Insert after "climate" "when human effects on the surface, such as the building of cities, the emission of energy and land-use changes have been ignored" [VINCENT GRAY (Reviewer's comment ID #: 88-1281)]	Rejected, no basis for assertion, not relkevant for Ch 6
6-871	A	38:22	38:35	ok, but note, however, the dependency of the forcing / response ratio on the type of forcing (e.g., efficacy of climate forcings, Hansen, J. et al. J. Geophys. Res. 2005 110 D18104 doi:10.1029/2005JD005776. [Michel Crucifix (Reviewer's comment ID #: 52-81)]	Rejected. Appropriate caveat added on line 24 to 26. Differences in efficacy for solar and volcanic forcing compared to CO2 as found by Hansen et al. are small compared to the overall uncertainty of solar and volcanic forcing.
6-872	A	38:30	38:31	Add improved reference … MacFarling et al., 2006 in press MacFarling Meure, C., Etheridge, D., Trudinger, C., Steele, P., Langenfelds, R., van Ommen, T., Smith, A. And Elkins, J. The Law Dome CO2, CH4 and N2O Ice Core Records Extended to 2000 years BP., GRL, in press, 2006. [Govt. of Australia (Reviewer's comment ID #: 2001-323)]	Accepted

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6-873	A	38:41	38:51	Please explain how the CO2/climate sensitivity arises in these models: which process is driving it? [Eric Wolff (Reviewer's comment ID #: 292-39)]	Taken into account. Reference to section 7.3 added. Not enough space to give detailed explanation here.
6-874	A	38:43	38:43	Specify explicitly the sensitivity obtained in Gerber et al. 2003 [Michel Crucifix (Reviewer's comment ID #: 52-82)]	Noted. Sensitivity not added. Too much detail for this chapter. The Bern model is also part of C4MIP. The sensitivity of the Bern model with respect to NH temperature change is 12 ppm/K.
6-875	A	38:48	38:51	This paragraph looks strange. Assuming that the mean sensitivity among models is 8 ppm/C and the mean change in CO2 concentration is 10 ppm, the estimated mean temperature change should be 0.8K and not 0.6 K. This can be confirmed by Montecarlo simulations of the ratio between two gaussian probability distributions with mean 8 and standard deviation 1 and mean 10 and standard deviation 2, respectively. The ratio between both yields a median of 0.8 K with a 5%-95% range of 0.55K-1.1K. The assumption of 0.6K of NH T temperature range is therefore biased towards low variations. A value of 0.8 K supports more strongly reconstructions with high past variability. The recent paper by Scheffer et al (Geophys. Res. Lett. 33 doi 10.1019/2005GL055044) also supports higher past temperature variations: whereas the reconstruction by Mann et al (1998) would imply a CO2 sensitivity as high as 41 ppm/K, the one by Moberg et al (2005) yields a value of 12 ppm/K, clearly more in agreement with model estimations. [Eduardo Zorita (Reviewer's comment ID #: 304-1)]	Taken into account. Text edited to make clear that the value of 0.6 K is an illustrative example.. It is not clear that the mean model sensitivity is 8 ppm/C and that the mean change in CO2 is 10 ppm as asserted by the reviewer. Chapter 7 gives sensitivities with respect to global surface temprature change, whereas here sensitivities with respect to NH surface temperature are given. The models suggest a range of 4 to 16 ppm/K for the CO2-NH temperature sensitivity and the ice core data suggest CO2 changes of 6 to 10 ppm. Ice core data have also an uncertainty that should not be neglected.
6-876	A	38:49	38:50	Add improved reference … MacFarling et al., 2006 in press MacFarling Meure, C., Etheridge, D., Trudinger, C., Steele, P., Langenfelds, R., van Ommen, T., Smith, A. And Elkins, J. The Law Dome CO2, CH4 and N2O Ice Core Records Extended to 2000 years BP., GRL, in press, 2006. [Govt. of Australia (Reviewer's comment ID #: 2001-324)]	Accepted
6-877	A	39:1	39:14	Evidence of paleo-ENSO activity in New Zealand has been derived by Fowler et al.(2000) and Fowler (2005) from kauri (Agathis australis) tree-rings (Fowler, A. et al. 2000 Journal of the Royal Society of New Zealand 30(3), 277-292; and Fowler, A. 2005 Climate Research 29, 73-84). [Paul W Williams (Reviewer's comment ID #: 291-6)]	Rejected. Fowler et al. (2000) concluded that the kauri chronology is an imperfect ENSO proxy, which limites it use as a proxy of ENSO past variations. In Fowler (2005), The potential for the ENSO reconstructions is indicated but quantitative estimates

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					of ENSO past variability are not presented.
6-878	A	39:7	39:9	"These reconstructions share significant common variance.." - is this with instrumental records or with each other? Make it clearer if independent reconstructions of ENSO show consistent and stable relationships back to a given date in the past. [Govt. of Australia (Reviewer's comment ID #: 2001-325)]	Accepted. Text modified. The reconstructions share common variance with instrumental and with each other. Correlations between Sthale et al., Mann et al., and D'Arrigo et al., reconstructions range between r = 0.59 and r = 0.77. However, these reconstructions are partially based on the same proxy records, thus they are not totally independent reconstructions.
6-879	A	39:9	39:11	"In most coral records……" repeat of statement made earlier & see associated comments (Page 32). [Govt. of Australia (Reviewer's comment ID #: 2001-326)]	Accepted. Text modified to reflect the issue on coral's limitations.
6-880	A	39:10	39:14	Since the section is on variability, it should be made clear that the restults presented in this paragraph is about the mean state. [Pascale BRACONNOT (Reviewer's comment ID #: 29-18)]	Accepted. Text modified.
6-881	A	39:16	39:20	References for the "several coral and tree-ring studies"; only Urban et al (2000) cited in this paragraph; Also note proxy climate evidence for changes in the strength of ENSO teleconnections (as observed in the instrumental records, 1920s-1940s), also evident in proxy climate records from earlier time periods (eg Hendy et al (2003) The Holocene 13: 187-199). [Govt. of Australia (Reviewer's comment ID #: 2001-327)]	Accepted. Text modified. The paragraph has been deleted and the information integrated trough the ramaining text in the section.
6-882	A	39:16	39:20	This paragraph does not provide an assessment. We suggest it is reviewed and possibly re-written. Proxy data from Antarctic ice cores show a polar expression of ENSO that identifies a link to southeastern Pacific sea-ice extent variations (Meyerson et al, 2002). In general higher frequency of El Niño events is associated with increased sea-ice extent. Ref: Eric A. MEYERSON, Paul A. MAYEWSKI, Karl J. KREUTZ, L. David MEEKER, Sallie I.WHITLOW, Mark S.TWICKLER, The polar expression of ENSO and sea-ice variability as recorded in a South Pole ice core. Annals Glaciology. 35, 2002. [Govt. of Australia (Reviewer's comment ID #: 2001-328)]	Accepted. The paragraph has been deleted and the information integrated trough the ramaining text in the section.
6-883	A	39:16	39:16	drier central Pacific... the ocean is never dry. [Michel Crucifix (Reviewer's comment ID #: 52-83)]	Accepted. Text modified. The paragraph has been deleted and the information integrated trough the ramaining text in the section.

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6-884	A	39:20	39:20	references? [Mark Siddall (Reviewer's comment ID #: 238-39)]	Accepted. This point is still controversial. Sentence deleted.
6-885	A	39:22	39:32	It should be added that some of the conclusions are drawn on one model results (even though an ensemble simulations was performed). Other mechanisms could enter into play that are not reproduced in this particular model, and could change part of the conclusions. [Pascale BRACONNOT (Reviewer's comment ID #: 29-19)]	Accepted. Text modified to reflect the comment.
6-886	A	40:5		This is a correct reference to Luterbacher et al. However, I don't see why Luterbacher assume increased solar irradiance at the end of the 17th century. It is still within the Maunder minimum. [Raimund Muscheler (Reviewer's comment ID #: 185-9)]	Noted. According to Luterbacher et al (2004), the increased solar irradiance at the end of the 17th century and through the first half of the 18th century might have induced a shift toward a high NAO/AO index, which in turns increase winter temperature across Europe.
6-887	A	40:7	40:7	apart from Luterbacher et al. 2004 there should also be a reference to Xoplaki et al. 2005. 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. [Jürg Luterbacher (Reviewer's comment ID #: 151-9)]	Rejected. Based on IPCC document size limitations, only the most relevant references should be included.
6-888	A	40:7	40:7	Cite also here Shindell et al (2001). [Michael Mann (Reviewer's comment ID #: 156-41)]	Rejected. Based on IPCC document size limitations, only the most relevant references should be included.
6-889	A	40:13	40:13	apart from Nesje and Dahl, 2003 the author might also cite Pauling et al. (2006) who present 500 year seasonal precipitation for Europe. They point to the fact that at the end of the 17th century/beginning of the 18th century Europe experience a trend towards more winter precipitation. Pauling, A., Luterbacher, J., Casty, C., and Wanner, H., 2006: 500 years of gridded high-resolution precipitation reconstructions over Europe and the connection to large-scale circulation, Climate Dynamics, 26, 387-405. [Jürg Luterbacher (Reviewer's comment ID #: 151-10)]	Rejected. Based on IPCC document size limitations, only the most relevant references should be included.
6-890	A	40:14		This section starts by mentioning the NAO and AMO as the main sources of Atlantic variability, then spends the rest of the section talking about the NAO to exclusion of the AMO. This could be corrected by adding the following paragraphs: [Govt. of United States of America (Reviewer's comment ID #: 2023-427)]	Rejected. Although valid, this comment deals with AMO variability during the instrumental period, a topic that is covered in Chapter 3.
6-891	A	40:14		"The AMO is the leading mode of quasi-periodic, multidecadal North Atlantic SST variability related to oceanic thermohaline circulation (Delworth and Mann, 2000; Sutton and Hodson 2003; Knight et al. 2005). Over the instrumental period (1856-Present), the	Rejected. Although valid, this comment deals with AMO variability during the instrumental period, a topic that is

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				AMO exhibited a 65-80 yr cycle (0.4 º C range), with warm phases at roughly 1860-1880 and 1930-1960 and cool phases during 1905-1925 and 1970-1990. The AMO appears to have returned to a warm phase beginning in the mid 1990s. AMO phases tend to be very persistent but the transitions from one phase to the other tend to occur quickly. The AMO has been associated with multi-year precipitation anomalies worldwide (McCabe and Palecki, 2006; Sutton and Hudson 2005). The AMO is thought to play a role in Atlantic hurricane formation (Golenberg et al. , Caribbean and NE Brazil rainfall, African and North American drought frequencies (Folland et al. 1986; McCabe et al. 2004), and temperatures in Europe (Sutton and Hodson 2003). [Govt. of United States of America (Reviewer's comment ID #: 2023-428)]	covered in Chapter 3.
6-892	A	40:14		Instrumental observations capture only two full cycles of the AMO, but a longer AMO reconstruction (A.D. 1567-1990) is now available from tree rings in eastern North America, Europe, Scandinavia and the Middle East (Gray et al., 2004). AMO phases in the reconstruction tend to average 20 years in duration (ranging from 9 to 53 years), except in the 18th century when AMO variability was noticeably dampened. Enfield and Cid-Cerrano (2006) estimated probability distribution functions from the Gray et al. (2004) reconstruction to calculate the probability of future shifts in AMO. AMO variability has been correlated to tree-ring reconstructions of precipitation, PDSI and fire occurrence in the western U.S. (Gray et al., 2003; Hidalgo 2004; Sibold and Veblen 2006). Correlations between AMO and winter climate in the western U.S. in both the instrumental and reconstructed record beg for a mechanism." [Govt. of United States of America (Reviewer's comment ID #: 2023-429)]	Rejected. Gray et al. (2004) reconstruction of AMO substantially differs from Delworth and Mann (2000) estimations of temperature variability across the North Atlantic during the past 300 years. Additional reconstructions are needed to reach a general consensus of past AMO variability.
6-893	A	40:14		Delworth, T. L., and M. E. Mann (2000), Observed and simulated multidecadal variability in the Northern Hemisphere, Climate Dynamics 16, 661–676. [Govt. of United States of America (Reviewer's comment ID #: 2023-430)]	Gray et al. (2004) reconstruction of AMO substantially differs from Delworth and Mann (2000) estimations of temperature variability across the North Atlantic during the past 300 years. Additional reconstructions are needed to reach a general consensus of past AMO variability.
6-894	A	40:14		Enfield, D. B. and Cid-Serrano, L. 2005. Projecting the risk of future climate shifts. International Journal of Climatology 10.1002/joc.1293. [Govt. of United States of America (Reviewer's comment ID #: 2023-431)]	Rejected. Although valid, this comment deals with AMO variability during the instrumental period, a topic that is covered in Chapter 3.
6-895	A	40:14		Enfield, D. B. and A. M. Mestas-Nuñez and P. J. Trimble. 2001. The Atlantic multidecadal oscillation and its relation to rainfall and river flows in the continental U.S.	Rejected. Although valid, this comment deals with AMO variability during the

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				Geophysical Research Letters 28, 2077-2080. [Govt. of United States of America (Reviewer's comment ID #: 2023-432)]	instrumental period, a topic that is covered in Chapter 3.
6-896	A	40:14		Folland, C.K., T.N. Palmer, D.E. Parker. 1986. Sahel rainfall and worldwide sea temperatures. Nature 320, 602-606. [Govt. of United States of America (Reviewer's comment ID #: 2023-433)]	Rejected. Although valid, this comment deals with AMO variability during the instrumental period, a topic that is covered in Chapter 3.
6-897	A	40:14		Goldenberg, S. B., C. W. Landsea, A. M. Mestas-Nuñez, and W. M. Gray2001), The recent increase in Atlantic hurricane activity: Causes and implications. Science 293, 474–479. [Govt. of United States of America (Reviewer's comment ID #: 2023-434)]	Rejected. Although valid, this comment deals with AMO variability during the instrumental period, a topic that is covered in Chapter 3.
6-898	A	40:14		Gray S.T., J.L. Betancourt, C.L. Fastie, and S.T. Jackson, 2003. Patterns and sources of multidecadal oscillations in drought-sensitive tree-ring records from the central and southern Rocky Mountains. Geophysical Research Letters 30, 49-1. [Govt. of United States of America (Reviewer's comment ID #: 2023-435)]	Gray et al. (2004) reconstruction of AMO substantially differs from Delworth and Mann (2000) estimations of temperature variability across the North Atlantic during the past 300 years. Additional reconstructions are needed to reach a general consensus of past AMO variability.
6-899	A	40:14		Gray, S.T., Graumlich, L.J., Betancourt, J.L. and Pederson, G.T. 2004. A tree-ring based reconstruction of the Atlantic Multidecadal Oscillation since 1567 A.D. Geophysical Research Letters 31, L12205, doi:10.1029/2004GL019932. [Govt. of United States of America (Reviewer's comment ID #: 2023-436)]	Gray et al. (2004) reconstruction of AMO substantially differs from Delworth and Mann (2000) estimations of temperature variability across the North Atlantic during the past 300 years. Additional reconstructions are needed to reach a general consensus of past AMO variability.
6-900	A	40:14		Hidalgo, H.G., 2004. Climate Precursors of Multidecadal Drought Variability in the Western United States. Water Resources Research 40:W12504:10 p. [Govt. of United States of America (Reviewer's comment ID #: 2023-437)]	Rejected. Although valid, this comment deals with AMO variability during the instrumental period, a topic that is covered in Chapter 3.
6-901	A	40:14		McCabe GJ, Palecki MA (2006) Multidecadal climate variability of global lands and oceans. International Journal of Climatology. DOI 10.1002/joc.1289. [Govt. of United States of America (Reviewer's comment ID #: 2023-438)]	Rejected. Although valid, this comment deals with AMO variability during the instrumental period, a topic that is covered in Chapter 3.
6-902	A	40:14		McCabe, G. J., Palecki, M. A., and Betancourt, J. L. 2004. Pacific and Atlantic Ocean	Rejected. Although valid, this comment

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				influences on multidecadal drought frequency in the United States. Proceedings of the National Academy of Sciences 101, p. 4136-4141 [Govt. of United States of America (Reviewer's comment ID #: 2023-439)]	deals with AMO variability during the instrumental period, a topic that is covered in Chapter 3.
6-903	A	40:14		Sutton, R.T. and D.L.R Hodson. 2003. Influence of the ocean on North Atlantic climate variability 1871-1999. J. Climate 16:3296-3313. [Govt. of United States of America (Reviewer's comment ID #: 2023-440)]	Rejected. Although valid, this comment deals with AMO variability during the instrumental period, a topic that is covered in Chapter 3.
6-904	A	40:14		Sibold, J.S. and T. T. Veblen, 2006. Relationships of subalpine forest fires in the Colorado Front Range with interannual and multidecadal-scale variation. Journal of Biogeography 33, 833-842. [Govt. of United States of America (Reviewer's comment ID #: 2023-441)]	Rejected. Although valid, this comment deals with AMO variability during the instrumental period, a topic that is covered in Chapter 3.
6-905	A	40:14		Sutton, R. T., Hodson, D. L. R. 2005. Atlantic Ocean Forcing of North American and European Summer Climate. Science 309, 115-118. [Govt. of United States of America (Reviewer's comment ID #: 2023-442)]	Rejected. Although valid, this comment deals with AMO variability during the instrumental period, a topic that is covered in Chapter 3.
6-906	A	40:16	40:16	"poorly" should be changed into "not well" in the sentence of "and for poorly understood reasons". [Govt. of China (Reviewer's comment ID #: 2006-54)]	Accepted. Text modified.
6-907	A	40:33	34:34	Several studies (e.g. Zeng, N. and J. D. Neelin, Lau, K.-M. and Tucker, C. J., Enhancement of interdecadal climate variability in the Sahel by vegetation interaction, Science, 286, 1537-1540 (2000) point to the role of vegetation in determining a meta-stable state characterised by drought (i.e., leading to a long drought) [Michel Crucifix (Reviewer's comment ID #: 52-84)]	Noted, text considered
6-908	A	40:37	40:40	"… periods with more frequent, longer and/or geographically more extensive drought in North American ..." More frequent that what? More extensive than where? Longer than when? Can you please clarify this? [Melinda Marquis (Reviewer's comment ID #: 162-5)]	Accepted. Text modified.
6-909	A	41:0		Key uncertainties in 6.7 Robust finding: replace "articulated" with "explained". The problem is not a matter of articulation but explanation! We don't have the answers. [Katsumi Matsumoto (Reviewer's comment ID #: 171-4)]	Noted, text considered
6-910	A	41:0		Third item should read "Global sea level rise due primarily to …". Also there is disagreement between this temperature range and those listed elsewhere in the text. Fourth item should read "associated with", not "linked to". Linking infers causation. Eighth item needs clarification because dry periods last decades to centuries and droughts do not. Last item should read "Models are capable of simulating many aspects of climate and vegetation change for past periods of different forcings." [Govt. of United States of America (Reviewer's comment ID #: 2023-444)]	Noted, text considered

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6-911	A	41:0		Last item under Key Uncertainties should read "The lack of extensive networks of proxy data that are resampled and updated to the present day means …". Proxy data do not run. [Govt. of United States of America (Reviewer's comment ID #: 2023-445)]	Noted, text considered
6-912	A	41:5	41:5	There should be two additional sections (6.6.5.6) covering the various published reconstructions of the Pacific Decadal Oscillation (eg Biondi et al (2001); D'Arrigo et al (2001); MacDonald & Case (2005); Shen et al (2006)). (6.6.5.7) covering changes in Antarctic and Southern Ocean climate. While there is a relative paucity of climate data for the Southern Hemisphere, proxy data from Antarctic ice cores provide a range of additional indicators for climate indices at mid- to high-southern latitudes. Major parameters that have been reconstructed or estimated include precipitation, atmospheric circulation/pressure fields and sea-ice extent. Recent data show evidence of increased snow accumulation since 1970 in parts of West Antarctica (Kaspari et al., 2004), particularly the Pine Island, Thwaites Glacier region, as evaluated against the last 200 years. On longer timescales increases in snow accumulation of up to 80% occurred during the first half of the Holocene at Law Dome, in coastal East Antarctica: despite a relatively stability in climate forcing and inferred temperatures (van Ommen et al., 2004). The change in accumulation at this site from the last glacial maximum to present, more than 1000%, points to significant climate shifts in cyclonicity in the region. Variations in past sea-ice extent have been inferred from sulphur compounds in ice cores, and these suggest that large decadal scale variations in the latter 20th century are superimposed upon a 20% decline in overall sea-ice extent in East Antarctica since 1950 (Curran et al., 2003). The correlation extends to total Antarctic sea-ice extent and suggest overall decline in the period since the mid-20th century. Other data show a connection between increases in south-west Pacific sea-ice extent and frequency of El Niño events (Meyerson et al., 2002). Changes in Southern Hemisphere atmospheric circulation are also recorded in ice core proxy records. Records show that the strength of Southern Hemisphere westerly circulation in the Australian sector is as strong now as any time since the last glaciation (Shulmeister et al., 2004). Proxy data for the dominant atmospheric pattern known as the Southern Annular Mode (SAM) show that recent levels are enhanced (stronger SAM) relative to the past 700 years (Goodwin et al., 2004), consistent with the increased westerly circulation in the region. Refs: Mark A. J. Curran, Tas D. van Ommen, Vin I. Morgan, Katrina L. Phillips and Anne S. Palmer. Ice Core Evidence for Antarctic Sea Ice Decline Since the 1950s, Science, 302: 1203-1206, 2003. I. D. Goodwin, T. D. van Ommen M. A. J. Curran and P. A. Mayewski. Mid latitude winter climate variability in the South Indian and south-west Pacific regions since 1300	Rejected. Although a number of proxy-based reconstructions of decadal and multi-decadal Pacific climate variability has been developed, they markedly differ in their estimations of PDO past variability. There is not yet a clear consensus on PDO variability during the past centuries. Regarding Antarctic and Southern Ocean Climates, the comments deal with recent (instrumental) climate variability that should be discussed on Chapter 3. On the other hand, most of the proposed records of past climate variability have not been calibrated against instrumental records and in most cases they just provide preliminary views of past climate variability across Antarctica.

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				AD. Climate Dynamics, 22(8):783-794, DOI: 10.1007/s00382-004-0403-3, 2004. Susan KASPARI, Paul A. MAYEWSKI, Daniel A. DIXON, Vandy Blue SPIKES, Sharon B. SNEED, Michael J. HANDLEY, Gordon S. HAMILTON, Climate variability in West Antarctica derived from annual accumulation-rate records from ITASE firn/ice cores, Annals Glaciolgy 39, 2004. Eric A. MEYERSON, Paul A. MAYEWSKI, Karl J. KREUTZ, L. David MEEKER, Sallie I.WHITLOW, Mark S.TWICKLER, The polar expression of ENSO and sea-ice variability as recorded in a South Pole ice core. Annals Glaciology. 35, 2002. J. Shulmeister, I. Goodwin, J. Renwick, K. Harle, L. Armand, M.S. McGlone,, E. Cook, J. Dodson, P.P Hesse, P. Mayewskij, M. Curran. The Southern Hemisphere westerlies in the Australasian sector over the last glacial cycle: a synthesis. Quaternary International 118–119 (2004). Tas D. van Ommen, Vin Morgan and Mark A. J. Curran. Deglacial and Holocene changes in accumulation at Law Dome. Ann. Glaciol., 39:359-365, 2004 [Govt. of Australia (Reviewer's comment ID #: 2001-329)]	
6-913	A	41:6	41:6	The authors should make sure this is a "key uncertainty". Somewhere in this chapter, there should be a greater emphasis on not only developing more proxy climate records with greater spatial coverage BUT also high quality and reliable proxy climate records. There are many published "proxy climate records" which have little or no relationship with local climate variables and/or are poorly calibrated against instrumental records. Inclusion of such records can compromise the reliability of long-term climate reconstructions. [Govt. of Australia (Reviewer's comment ID #: 2001-330)]	Taken into account in revison of text
6-914	A	41:6	41:7	Robust finding : "global sea level rise due to primarily to ice sheet retreat likely exceeded 4 m the last time the Arctic was 3 to 4 oC warmer than present". This sentence, presented as a robust finding, gives the misleading impression that a future warming of 3 to 4oC in this region will cause the same sea-level rise. Yet, the ice sheets responded, during the LIG, to the orbital forcing which has a very strong imprint on summer temperatures. Furthermore, they were probably not in equilibrium with the climate because of the dynamic evolutions of both forcing and ice sheet volume. [Michel Crucifix (Reviewer's comment ID #: 52-85)]	Taken into account – changed to highlight importantance of summer Arctic temperatures in main text. Deleted SOD "Robust Findings"
6-915	A	41:6	41:6	Delete "Robust" and "Key", also in the Table [VINCENT GRAY (Reviewer's comment ID #: 88-781)]	Rejected – no justification offered.
6-916	A	41:7	41:7	The Section 6.7 "Robust Findings and Key Uncertainties" should be a Table [Govt. of Australia (Reviewer's comment ID #: 2001-331)]	Taken into account – Deleted table in favor of new, shorter, section focused on key uncertainties. Findings in Exec Summary
6-917	A	41:7	41:7	Robust Findings Column- "Global sea level rise due to primarily to ice sheet retreat likely	Taken into account – Deleted table in

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				exceeded 4 m the last time the Antarctic was 3 to 4 degrees C warmer than present." with "Global sea level rise due primarily to ice sheet retreat likely exceeded 4 m the last time the Antarctic was 3 to 4 degrees C warmer than present, possibly due to melting of the Greenland icesheet". [Govt. of Australia (Reviewer's comment ID #: 2001-332)]	favor of new, shorter, section focused on key uncertainties. Findings in Exec Summary
6-918	A	41:7	41:7	Add "on centennial timescales" to the end of "There is no evidence for a natural interglacial climate cycle that could explain recent global warming, or that the current warming will be mitigated by a natural cooling trend". [Govt. of Australia (Reviewer's comment ID #: 2001-333)]	Taken into account – Deleted table in favor of new, shorter, section focused on key uncertainties. Findings in Exec Summary
6-919	A	41:7	41:7	Replace "feedbacks" with "processes" and "amplified" with "contributed to" in the paragraph "Biogeochemical and biogeophysical feedbacks have amplified climatic changes in the past and are likely to do so in the future". [Govt. of Australia (Reviewer's comment ID #: 2001-334)]	Taken into account – Deleted table in favor of new, shorter, section focused on key uncertainties. Findings in Exec Summary
6-920	A	41:7	41:7	Replace "Droughts lasting decades to centuries are a recurrent feature of climate in North America and northern Africa under a wide range of climate forcing" with "Regional droughts lasting decades to centuries are a recurrent feature of climate (e.g. in North America and northern Africa) under a wide range of climate forcing". [Govt. of Australia (Reviewer's comment ID #: 2001-335)]	Taken into account – Deleted table in favor of new, shorter, section focused on key uncertainties. Findings in Exec Summary
6-921	A	41:7	41:7	Insert "aspects of" in between "simulating" and "climate" in the last paragraph "Models are capable of simulating climate and vegetation change for past periods of very different forcings and climate". [Govt. of Australia (Reviewer's comment ID #: 2001-336)]	Taken into account – Deleted table in favor of new, shorter, section focused on key uncertainties. Findings in Exec Summary
6-922	A	41:7	41:8	Will the authors please decide on the wording of their finding about the NH temperature during the 20th century. In the Executive Summary and in WG I's higher level summaries, the finding is that the second half of the 20th century was the warmest 50 year period in the NH in the last 1000 years, and unusually warm compared with the last 1300 years. The Executive Summary also states that the the regional extent of NH warmth was very likely greater during the 20th century that any other century in the last 1300 years. Pg. 6-33, lines 19-20, state "... it is likely that (in the NH) the 20th century was the warmest in at least the past 1300 years." This table states that "it is also likely that this was the warmest 50-year period in the past 1300 years." While these three wordings are similiar they have non-trivial differences that should be resolved. [Lenny Bernstein (Reviewer's comment ID #: 20-63)]	Taken into account – Deleted table in favor of new, shorter, section focused on key uncertainties. Findings in Exec Summary
6-923	A	41:7	41:7	Models are capable of simulating climate and vegetation changes for past periods of very different forcings and climate. This is true, but lots of progress need to be made, before we are sure we simulate this for the good reasons. It should be added that there is a lack of GCM simulations with vegetation feeback and coupling with the biogeochemical cycles.	Taken into account – Deleted table in favor of new, shorter, section focused on key uncertainties. Findings in Exec Summary

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				[Pascale BRACONNOT (Reviewer's comment ID #: 29-20)]	
6-924	A	41:7	41:7	In the key uncertainty column I suggest to add : Progress need to be made to better understand and assess changes in variability and extremes. [Pascale BRACONNOT (Reviewer's comment ID #: 29-21)]	Accepted – changed text and format.
6-925	A	41:7	41:8	It would help if the wording in the chapter and the wording in the summaries were the same concerning whether the second half of the 20th century was likely to have been the warmest 50 years in the last 1000 years or the last 1300 years. [Jeff Kueter (Reviewer's comment ID #: 137-58)]	Accepted – see 6-922
6-926	A	41:7	41:7	The use of "1300 years" here is odd and not justified. Current reconstructions extending back 2000 years (Moberg et al, Mann and Jones) find that late 20th century Northern Hemisphere warmth is likely unprecedented in at least 2000 years. It is therefore "2000" years that should be used here, rather than "1300 years". [Michael Mann (Reviewer's comment ID #: 156-32)]	Noted – need to make sure justification is more clear in text
6-927	A	41:7	41:7	table: 'Observations of changes in climate' - 'The rates and processes by which ice sheets disintegrated in the past are not well known.' This could be improved: 'Neither the rates nor the processes by which ice sheets grew and disintegrated in the past are well known.' [Mark Siddall (Reviewer's comment ID #: 238-40)]	Taken into account - changed text and format
6-928	A	41:7		Table Robust findings: Please specify the text regarding the droughts in Africa and N-America "under a wide range of climate forcing." What caused recent droughts in Africa and N-America? Please give a quantified likelihood for the statement as proposed in the guidance notes for lead authors of the AR4 on Addressing uncertainties (IPCC, July 2005). [Govt. of Germany (Reviewer's comment ID #: 2011-26)]	Noted – recent droughts not paleo and thus not topics of this chapter. Causes of earlier droughts not known.
6-929	A	41:7		Another robust finding is in my sense that one: "Natural warming during periods like Medieval or mid-Holocene optimum are neither global or even synchroneous, at the contrary of the warming of the last 50 years" [Joel GUIOT (Reviewer's comment ID #: 92-4)]	Noted - Deleted table in favor of new, shorter, section focused on key uncertainties. Findings in Exec Summary
6-930	A	41:7	:8	Decide on the wording of finding about the NH temperature during the 20th century. In the Executive Summary and in WG1's higher level summaries, the finding is that the second half of the 20th century was the warmest 50 year period in the NH in the last 1000 years, and unusually warm compared with the last 1300 years. The Executive Summary also states that the regional extent of NH warmth was very likely greater during the 20th century that any other century in the last 1300 years. Page 6-33, lines 19-20, state "... it is likely that (in the NH) the 20th century was the warmest in at least the past 1300 years." This table states that "it is also likely that this was the warmest 50-year period in the past 1300 years." While these three wordings are similiar they have non-trivial differences that should be resolved. A concerted effort needs to be made to ensure consistency within	Taken into account in main text and executive summary. Deleted table in favor of new, shorter, section focused on key uncertainties.

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				Chapter 6 and among IPCC chapters when making statements of this sort. [Govt. of United States of America (Reviewer's comment ID #: 2023-443)]	
6-931	A	41:41	41:41	The TS (page 32, line 18) says that NH temps of the second half of the 20th century … and likely the warmest in the past 1,000 years, as does the SPM (page 9, line 24). But Ch. 6 (RF&KU, page 41, line 41) cites past 1,300 years. Please make consistent in all (three) places. [Melinda Marquis (Reviewer's comment ID #: 162-130)]	See 6-930
6-932	A	41:43	41:45	The robust finding about drought in North America and northern Africa is very interesting. However, this point seems to be more detailed than the other eight robust findings listed. The other eight robust findings address more general findings, it seems to me. So that the point about drought in N. America and northern Africa seems not to belong in this list. One might wonder, for instance, why you include this point about drought, but not also the "intriguing finding ... that the South Asian (Indian) monsoon has, in the drier areas of its influence, recently reversed its millennia-long orbitally-driven low-frequency trend toward less rainfall" (page 40, lines 19-20). This point about the South Asian monsoon seems comparable to me to the point about drought. [Melinda Marquis (Reviewer's comment ID #: 162-6)]	Noted - Deleted table in favor of new, shorter, section focused on key uncertainties. Findings in Exec Summary
6-1234	B	42:1		References: Beltrami, H., C. Gosselin*, and J.C. Mareschal (2003). Ground surface temperatures in Canada: Spatial and temporal variability, Geophysical Research Letters, 30 (10), 10.1029/2003GL017144. Beltrami, H., G. Ferguson and R. N. Harris (2005). Long-term tracking of climate change by underground temperatures, Geophysical Research Letters, 32, L19707, doi:10.1029/2005GL023714, 2005. González-Rouco, J.F., H. Beltrami, E. Zorita, and H. von Storch (2006) Simulation and inversion of borehole temperature profiles in surrogate climates: Spatial distribution and surface coupling, Geophysical Research Letters, 33, L01703, doi:10.1029/2005GL024693, 2006. H. N. Pollack, S. Huang and J. E. Smerdon (2006) Five centuries of climate change in Australia: The view from underground Journal of Quaternary Sciences. In Press. Beltrami, H., J. F. González-Rouco and M. B. Stevens. (2006b) Subsurface temperatures during the last millennium: Model and observation , Geophysical Research Letters, Vol. 33, L09705, doi:10.1029/2006GL026050, 2006. Beltrami, H., E. Bourlon, L. Kellman and J.F. González-Rouco. (2006) Spatial patterns of ground heat gain in the northern hemisphere, Geophysical Research Letters, Vol. 33, L06717, doi:10.1029/2006GL025676, 2006. Levitus, S., J. Antonov, J. Wang, T. L. Delworth, K. Dixon and A. Broccoli, Anthropogenic warming of the Earth's climate system. Science, 292, 267-270, 2001.	Will be consisdered in final draft

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				Levitus, S., J. Antonov, and T. Boyer, Warming of the world ocean, 1955-2003, Geophys. Res. Lett., 32, L02604, doi:10.1029/2004GL021592, 2005. Beltrami, H., J. Smerdon*, H. N. Pollack and S. Huang (2002). Continental heat gain in the global climate system. Geophysical Research Letters, 29 (8), 10.1029/2001GL014310.. [Hugo Beltrami (Reviewer's comment ID #: 306-13)]	
6-933	A	42:7	42:8	Adkinson et al. does not exist - this is a typo on Adkins et al. (see comment 21) [Mark Siddall (Reviewer's comment ID #: 238-41)]	Accepted
6-934	A	42:39	42:47	Do not use this citation, use the citation on page 48, line 47-48 instead [Renato Spahni (Reviewer's comment ID #: 249-5)]	Accepted
6-935	A	42:39	42:47	Remove Augustin et al, since it is in correctly as EPICA Community Members 2004. [Eric Wolff (Reviewer's comment ID #: 292-40)]	Accepted
6-936	A	43:15	43:15	correction to reference: Paleooceanography, 19, PA3014, doi:10.1029/2004PA001030. [Eva Bauer (Reviewer's comment ID #: 15-15)]	Accepted
6-937	A	43:17	43:17	correction to reference: Geophysical Research Letters, 30(6), 1276, doi:10.1029/2002GL016639. [Eva Bauer (Reviewer's comment ID #: 15-16)]	Accepted
6-938	A	50:49	50:49	Insert "Gray. V.R., 2000. "The Zcause of Global Warming" . Energy and Environment Vol 11 pages 613-629" [VINCENT GRAY (Reviewer's comment ID #: 88-782)]	Rejected, not referred to in text
6-939	A	52:0		Should read Esper et al. (2005). [Govt. of United States of America (Reviewer's comment ID #: 2023-446)]	Accepted
6-940	A	53:38	53:38	Typo [Govt. of Thailand (Reviewer's comment ID #: 2021-2)]	Accepted
6-941	A	56:1	56:2	same as previous remark [Govt. of France (Reviewer's comment ID #: 2010-52)]	Accepted
6-942	A	56:22	56:24	The reference of Luterbacher et al. 2002 should be changed to: Luterbacher, J. , E. Xoplaki, D. Dietrich, P. D. Jones, T. D. Davies, D. Portis, J. F. Gonzalez-Rouco, H. von Storch, D. Gyalistras, C. Casty and H. Wanner, 2002: Extending North Atlantic Oscillation Reconstructions Back to 1500. Atmos. Sci. Lett., 2, 114-124, doi: 10.1006/ asle.2001.0044 [Jürg Luterbacher (Reviewer's comment ID #: 151-11)]	Accepted
6-943	A	58:4	58:4	Insert "McKitrick, R & P.J. Michaels 2004 "A test for extraneous signals in gridded surface temperature data" Climate Research Vol 26, pages 159-173" [VINCENT GRAY (Reviewer's comment ID #: 88-783)]	Rejected, not referred to in text

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6-944	A	62:46	62:47	The authors' names are: M. Sánchez Goñi, I. Cacho, J. Turon, J. Guiot, F. Sierro, J. Peypouquet, J. Grimalt and N. Shackleton [JAVIER MARTIN-VIDE (Reviewer's comment ID #: 165-13)]	Accepted
6-945	A	62:46	62:47	The authors' names are: M. Sánchez Goñi, I. Cacho, J. Turon, J. Guiot, F. Sierro, J. Peypouquet, J. Grimalt and N. Shackleton [Govt. of Spain (Reviewer's comment ID #: 2019-73)]	Accepted
6-946	A	64:4		Change "in press" to "2006" [Govt. of United States of America (Reviewer's comment ID #: 2023-447)]	Accepted
6-947	A	64:6		Delete "In press." Add "Journal of Geophysical Research – Atmospheres, 111(D07), art. no.-D07101." [Govt. of United States of America (Reviewer's comment ID #: 2023-448)]	Accepted
6-948	A	68:0		I can not find any indication of where Question 6.1 Figure 1 is to be placed. I suggest that it be placed after line 29 on this page. [Wilmer Anderson (Reviewer's comment ID #: 5-38)]	accepted
6-949	A	68:0		question 6.1. There is no 'call for' Question 6.1, Figure 1 in this section. However, I am not sure that it is the right place for this figure. The orbital parameters are not really discussed in this section. [Marie-France Loutre (Reviewer's comment ID #: 148-9)]	accepted second part rejected, the figure illustrates for lay people the orbital cycles causing ice ages
6-950	A	68:1		The logic behind the selection of references is not easy to follow. References seem to have been picked at random. E.g. : Climate simulations confirm that an Ice Age can be confirmed that way : two references are cited, which are perhaps not the most appropriate. While other important informations, such as : changes are not synchronous and have opposite sign in the North and South Atlantic: this statement, perhaps more controversial, is not supported by appropriate references. [Michel Crucifix (Reviewer's comment ID #: 52-86)]	Indeed not much care was given to choice of references since it remained unclear at time of writing whether the common questions would have references at all, or not. Will be improved if references remain in.
6-951	A	68:4	68:4	Sentence should read: …on all time scales, including long before … [Wilmer Anderson (Reviewer's comment ID #: 5-35)]	accepted
6-952	A	68:6		The body of the answer shows that changes in radiation balance are indeed the principal driver of past climate change. (see Question 1.1) The cause of such changes has multiple sources. So suggest that the sentence be rewritten as: 'Although changes in Earth's radiation balance are the principal driver of past climates, the cause of such changes are varied.' [David Wratt & David Fahey (Reviewer's comment ID #: 67-45)]	accepted
6-953	A	68:8	68:9	this sentence seems very over-positive to me. We are not confident about the "causes" of	rejected. There are many cases where

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				the changes in the late Quaternary, let alone earlier ones, and they certainly can't be produced buy quantitative models without specifying many of the changed parameters. [Eric Wolff (Reviewer's comment ID #: 292-41)]	we have good confidence in attributing causes - e.g., orbital cycles causing ice ages or Eemian or early Holocene warmth.
6-954	A	68:11	68:15	This sentence sounds like it is possible for man to change the earth's orbit or the solar energy output. I suggest it be changed to read as follows: … There are three fundamental ways the earth's radiation balance can change thereby causing a climate change: (1) changes in the incoming solar radiation (e.g. by changes in the earth's orbit or changes in the energy emitted by the sun itself), (2) changes in the fraction of the solar radiation that is reflected (this fraction is called the albedo - it is changed e.g. by changes in the cloud cover, aerosols, or land cover), and (3) changes in the long wavelength back radiation (e.g. by changes in the greenhouse gas concentration). [Wilmer Anderson (Reviewer's comment ID #: 5-36)]	accepted
6-955	A	68:12		Suggest word 'ways' in ln 12 and 'factors' in ln 17 be the same word since they are being equated. [David Wratt & David Fahey (Reviewer's comment ID #: 67-47)]	rejected, they are not equated, there are a lot more factors than ways
6-956	A	68:15		The concept of 'back-radiation' will not be understood by many non-experts. Suggest adding a defining/descriptive phrase like '..the long-wave energy radiated back to Earth….." 255 6-255 46 [David Wratt & David Fahey (Reviewer's comment ID #: 67-47)]	accepted
6-957	A	68:19	68:29	Perhaps a triggering mechanism is needed to kick off an ice-age, such as a combination of a cyclical minimum in solar luminosity and a string of large volcanos, superimposed on the Milankovitch forcing. [Andrew Lacis (Reviewer's comment ID #: 138-9)]	rejected - no evidence given
6-958	A	68:22	68:22	Change this sentence to read as follows: …season (but hardly affect the global, annual mean) … [Wilmer Anderson (Reviewer's comment ID #: 5-37)]	accepted
6-959	A	68:29	68:29	The statement that the next ice age will commence in 50 kyr is in conflict with the statement on p. 18, line 5 according to which the onset should occur in 30 kyr. Should be harmonized. [Michael Schulz (Reviewer's comment ID #: 229-22)]	accepted
6-960	A	68:29	68:29	FAQ 6.1 says the next large minimum in northern summer insolation, similar to ones that started past Ice Ages, is due in ~50,000 years. But Ch. 6 (Ex. Sum., page 2, lines 23-24) says, "It is very likely that the Earth would not naturally enter another ice age for at least 30,000 years." And the TS (page 13, lines 44-45) also cites 30,000 years for this phenomenon. Please cite these numbers consistently in all (three) places. [WG1 TSU (Reviewer's comment ID #: 285-8)]	accepted

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6-961	A	68:31	68:31	Insert after "cause" "since the changes in past ages of the most important greenhouse gas, water vapour are unknown" [VINCENT GRAY (Reviewer's comment ID #: 88-784)]	rejected - the reason is that orbital cycles are the primary cause. Water vapour cannot be, it can only act as a feedback.
6-962	A	68:33	68:34	atmospheric CO2 follows the climate changes with a lag of some hundreds of years. This is true for the Antarctic temperature, not for climate in general. For example, the glacial inception is associated with a cooling in the NH, and changes in Antarctic and CO2 only occur much later. [Michel Crucifix (Reviewer's comment ID #: 52-87)]	accepted - added "in Antarctica"
6-963	A	68:39		Within the last ice age, [Eric Wolff (Reviewer's comment ID #: 292-42)]	accepted
6-964	A	68:46	68:46	FAQ 6.1: Would it be clearer if "instabilities in the ice sheets" were changed to something like "rapid release of freshwater"? [Melinda Marquis (Reviewer's comment ID #: 162-76)]	accepted, reworded
6-965	A	68:51		For clarity change to 'greenhouse gas abundances' [David Wratt & David Fahey (Reviewer's comment ID #: 67-48)]	accepted
6-966	A	69:4		cosmic rays usually don't reach the Earth's surface. The products of the nuclear reactions induced by cosmic rays can reach the Earth's surface. [Raimund Muscheler (Reviewer's comment ID #: 185-10)]	accepted
6-967	A	69:13		I would change "since 1940" to "since the 1950's" since sunspots reached their maximum in 1957 AD. Since it is difficult to define the start and end of a trend, the present sentence is not really wrong. [Raimund Muscheler (Reviewer's comment ID #: 185-11)]	accepted
6-968	A	69:14	69:14	Missing ")" [James Crampton (Reviewer's comment ID #: 50-34)]	fixed
6-969	A	69:14		Add parentheses [David Wratt & David Fahey (Reviewer's comment ID #: 67-49)]	fixed
6-970	A	70:1	71:15	FAQ 6.2: It would be helpful to include a figure for FAQ 6.2. One suggestion for a figure is a graph showing carbon dioxide concentration and temperature for a few time periods, e.g., mid-Pliocene, LGM and last interglacial minimum, and present. Something like a simplified version of Figure 6.4(a). [Melinda Marquis (Reviewer's comment ID #: 162-81)]	Consiered in revision
6-971	A	70:4		Suggest adding a figure for Q6.2, perhaps a time line with some essential features related to CO2 and temperatures as discussed in the answer. [David Wratt & David Fahey (Reviewer's comment ID #: 67-53)]	will think about it
6-972	A	70:8	70:8	Is this ["of the past 30 years (about 0.19 deg C per decade")] consistent with the TS (page	GISS data: linear trend 1976-2005 =

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				18, line 43) and SPM (page 6, line 41), both of which refer to global warming for 1979-2005 as 0.17 deg C per decade? Shouldn't 0.19 be changed to 0.17? [Melinda Marquis (Reviewer's comment ID #: 162-19)]	0.21 per decade Hadley data: 0.17 per decade. I had used the average of the two, but now changed to 0.17. Part of the difference is also that for this audience I like to use a round number ("past 30 years"), not past 27 years as in the SPM.
6-973	A	70:8	70:8	Is this 0.19 deg C per decade consistent with the TS (page 18, line 43) and SPM (page 6, line 41), both of which refer to 0.17 deg C per decade. Please cite consistently is all (three) places. [WG1 TSU (Reviewer's comment ID #: 285-9)]	see above
6-974	A	70:18		Suggest for clarity changing to 'global mean conditions' [David Wratt & David Fahey (Reviewer's comment ID #: 67-50)]	accepted
6-975	A	70:19	70:20	Large changes in global mean require some global forcing. Not quite exact, because the Earth may respond non-linearly to a seasonal forcing. Typically the Milankovitch hypothesis : global, annual mean forcing close to zero, but the Earth radiative response is global. Furthermore (cf comment #93) : the energy balance of the system may be out of equilibrium during a few years (so, annual mean temperature change without global forcing), for example during D/O events. [Michel Crucifix (Reviewer's comment ID #: 52-88)]	noted - we are of course well aware of these caveats, but tried to convey the bottom line in a simple manner for lay persons here without space to go into all details
6-976	A	70:23		"The main reason for the current concern about climate change is the rise in atmospheric CO2 concentration" AND OTHER GREENHOUSE GASES. [Govt. of Spain (Reviewer's comment ID #: 2019-137)]	accepted
6-977	A	70:24	70:26	The EPICA papers are published. Please update the sentence. [Reto Knutti (Reviewer's comment ID #: 133-59)]	accepted - reference will be added if refs in common questions allowed.
6-978	A	70:25	70:26	For consistency with the rest of Ch. 6, and with the TS (page 6, line 54) and with the SPM (page 3, line 26), it seems preferable to change the tense of this statement from future tense to present tense (" … the new EPICA ice core will provide a record 700,000 years back in time ..." to " ... the EPICA ice core provides a record 650,000 years back in time ..." Note that in addition to changing the verb tense, I've changed 700,000 to 650,000 to make the period consistent with that given elsewhere in the report. [Melinda Marquis (Reviewer's comment ID #: 162-20)]	accepted
6-979	A	70:25	70:26	"almost half a million years" and "700,000" are inconsistent with "650,000" years cited in the TS (page 6, line 54) and with the SPM (page 3, line 26). Please cite consistently in all (three) places. [WG1 TSU (Reviewer's comment ID #: 285-10)]	accepted
6-980	A	70:25		accurately 650,000 years back in time	accepted

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				[Eric Wolff (Reviewer's comment ID #: 292-43)]	
6-981	A	70:27		Suggest for clarity changing to 'concentrations have' [David Wratt & David Fahey (Reviewer's comment ID #: 67-51)]	accepted
6-982	A	70:28	70:28	Reference section 2.3 directly rather than whole chapter? [Piers Forster (Reviewer's comment ID #: 73-35)]	accepted
6-983	A	70:32	70:33	it has not the same value all over the globe" : be more specific : "its variations are not the same throughout the global [Michel Crucifix (Reviewer's comment ID #: 52-89)]	rejected - matter of style.
6-984	A	70:32		It is a bit of an oversimplification to imply that CO2 is a well mixed gas with the same value all over the globe. [Adrian Simmons (Reviewer's comment ID #: 242-103)]	rejected - it is a very good approximation for the purpose of this discussion
6-985	A	70:35	70:35	For consistency with SPM, page 6 (line 39), it seems preferable to change "~0.6" to "~0.65" for the "global warming signal of the past century." [Melinda Marquis (Reviewer's comment ID #: 162-21)]	made it 0.7 (two significant digits suggest more accuracy than this is known at!) - need to monitor whether this remains consistent with SPM
6-986	A	70:37	70:37	The two sentences starting "Although they must not be over-interpreted...." and then "For example,..." is a confusing example that needs either more explanation or it should be deleted. [Gregory Wiles (Reviewer's comment ID #: 289-30)]	although it is not at all clear what is confusing here, we tried to simplify statement for clarity
6-987	A	70:38	70:39	Referring to oxygen-18 isotopes may be too technical for an FAQ. Please reconsider. [Melinda Marquis (Reviewer's comment ID #: 162-77)]	accepted
6-988	A	70:42	70:44	Recall that these reconstuctions are biased towards the northern hemisphere, and towards Europe. [Michel Crucifix (Reviewer's comment ID #: 52-90)]	noted - this is discussed in chapter 6 but no space in this box for such details
6-989	A	70:44		Suggest replace "~150 years" by "about 150 years". [David Wratt & David Fahey (Reviewer's comment ID #: 67-112)]	accepted
6-990	A	70:45	70:45	You can update the list with 2005 as the warmest year. [Andrew Lacis (Reviewer's comment ID #: 138-10)]	accepted
6-991	A	70:45	70:45	warmest years on record to be completd by 2005 [Marie-France Loutre (Reviewer's comment ID #: 148-11)]	accepted
6-992	A	70:45	70:45	Don't you want to add "2005" to the list of warmest years, e.g., for consistency with Chapter 3 and with the SPM (page 6, line 34). [Melinda Marquis (Reviewer's comment ID #: 162-22)]	accepted
6-993	A	70:45		What about 2005? We are told in Chapter 3 that two estimates place it as the warmest year on record, and a third estimate places it as the second warmest on record. [Adrian Simmons (Reviewer's comment ID #: 242-104)]	accepted

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6-994	A	70:50	70:50	Delete the word thus so that the sentence reads as follows: …and has been exceeded ... [Wilmer Anderson (Reviewer's comment ID #: 5-41)]	accepted
6-995	A	70:50		Suggest deleting 'thus' [David Wratt & David Fahey (Reviewer's comment ID #: 67-52)]	accepted
6-996	A	71:1	71:2	The statement about the last interglacial is wrong; as per 6-17, there is clear evidence for a warmer Arctic and Antarctic, and it's likely the global mean was of order 1 degree warmer. [Eric Wolff (Reviewer's comment ID #: 292-44)]	accepted - statement removed
6-997	A	71:6	71:6	"changes in tectonic activity" : suppress "change", and/or be more explicit. Changes in the configuration of the continents, run-off rate, sedimentary processes (weathering), and volcanic eruptions. [Michel Crucifix (Reviewer's comment ID #: 52-91)]	accepted
6-998	A	71:8	71:8	As in my earlier comment regarding Ch. 6, FAQ 6.2 (page 70, line 8), it seems to me that page 71 calls for the same comment: Is this ["current rate of warming of 0.19 deg C per decade"] consistent with the TS (page 18, line 43) and SPM (page 6, line 41), both of which refer to global warming for 1979-2005 as 0.17 deg C per decade? Shouldn't 0.19 be changed to 0.17? [Melinda Marquis (Reviewer's comment ID #: 162-23)]	see response above
6-999	A	71:8	71:8	0.19 deg C is cited as 0.17deg C per decade in TS (page 18, line 43) and SPM (page 6, line 41). Please make consistent throughout. [WG1 TSU (Reviewer's comment ID #: 285-11)]	see response above
6-1000	A	71:15	71:15	There are probably global annual mean changes in temperature during D/O, due to the fact that, during a short time, the radiative balance of the system is not in equilibrium (the ocean releases heat during a short time). [Michel Crucifix (Reviewer's comment ID #: 52-92)]	noted and agreed, but they are very small according to models
6-1001	A	71:17	:19	Needs to be rewritten to make it clear that (1) prior to ice cores we cannot measure rates of climatic change comparable to today's, and (2) for that reason we have no evidence if similar rates were seen before 600,000 years ago. The current text seems to imply that past rates were not as fast as today. Additionally, most older paleoclimatic records are from single points. This makes global-scale inferences questionable. [Govt. of United States of America (Reviewer's comment ID #: 2023-449)]	the text says just that already very clearly: "Further back in time, beyond ice core data, the time resolution of sediment cores and other archives does not resolve changes as fast as the present warming."
6-1002	A	73:0		table 6.2./ sorry to bother abouth spelling : MoBidiC (only MBC in capital letters) [Marie-France Loutre (Reviewer's comment ID #: 148-8)]	Accepted
6-1003	A	73:0		Table 3.7. Tropical cyclons, Definition, where it says '58 to 69 ms-1' it should say '59 to 69 ms-1' [Govt. of Spain (Reviewer's comment ID #: 2019-70)]	Accepted

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6-1004	A	73:2	73:2	AJS..2006 is refferenced in Table 6.2 but does not appear in Fig. 13d. [Jesús Fidel González-Rouco (Reviewer's comment ID #: 86-8)]	Accepted
6-1005	A	73:2	73:2	AJS..2006 is refferenced in Table 6.2 but does not appear in Fig. 13d. [Govt. of Spain (Reviewer's comment ID #: 2019-51)]	Accepted
6-1006	A	73:3	73:7	Should add if the Tropospheric sulphate aerosol (A) includes the direct and indirect effects of aerosol or not. A simulation just including the direct effect will have different forcing to one with direct and indirect effects [Gareth S. Jones (Reviewer's comment ID #: 121-63)]	Accepted
6-1007	A	74:1		Table 6.3 : Wang et al. 2005: specify reference more accurately [Michel Crucifix (Reviewer's comment ID #: 52-93)]	Rejected – citation considered appropriate
6-1008	A	75:1		Appendix 6.A : Glossary. Generally the definitions are too vague, or lack rigour (e.g. : alkalinity, eccentricity). When climatic events are defined (e.g.:Bølling, Younger Dryas, Dansgaard-Œshger) say how the event was originally defined (e.g. : from botanical evidence, or as an anomaly in the ?18O signal). [Michel Crucifix (Reviewer's comment ID #: 52-94)]	Accepted
6-1009	A	75:1		Appendix 6.A : Glossary : eccentricity is (not well) defined, but climatic precession and obliquity are not. I am happy to provide definitions if needed (contact me) [Michel Crucifix (Reviewer's comment ID #: 52-95)]	Noted, will be considerered
6-1010	A	75:1		Interglacial (even last interglacial) are defined. Interglacial is defined in term of'ice age glaciation. However a definition for glaciation or ice age glaciation is missing. [Marie-France Loutre (Reviewer's comment ID #: 148-2)]	Rejected, due to space limitations
6-1011	A	75:1		Although definition for Pleistocene, Pliocene and other epochs is given, there is no definition for Holocene [Marie-France Loutre (Reviewer's comment ID #: 148-16)]	Accepted,
6-1012	A	75:8		REPLACE "temperature" "BY SEA SURFACE temperatures temperatures OF THE OCEAN SURFACE" [Govt. of Spain (Reviewer's comment ID #: 2019-138)]	Accepted
6-1013	A	75:33		shield Earth from cosmic rays [Eric Wolff (Reviewer's comment ID #: 292-45)]	Accepted
6-1014	A	75:36	75:38	DO event : give original definition : anomaly in the ?18O of ice in Greenland, interpreted as… [Michel Crucifix (Reviewer's comment ID #: 52-96)]	Rejected, original definition is too narrow
6-1015	A	75:36		I agree with the definition of Dansgaard-Oeschger (DO) events as "Abrupt warming events followed by gradual cooling." However, since the evidence for ABRUPT warming is restricted to the North Atlantic region, it should not be stated that the DO events are recorded "elsewhere". The definition should simply read .. "recorded in Greenland ice	Taken into consideration, the correspondance with changes outside the Atlantic should, however, be addressed

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				cores". [Eric Steig (Reviewer's comment ID #: 252-25)]	
6-1016	A	75:40	75:42	... their distribution ... => their species distribution [Michel Crucifix (Reviewer's comment ID #: 52-97)]	Accepted
6-1017	A	75:41	75:41	"silt"-sized appears a bit too much jargon for a glossary; replace by diameter in mm [Michael Schulz (Reviewer's comment ID #: 229-23)]	Accepted
6-1018	A	75:51	75:52	... their distribution ... => their species distribution [Michel Crucifix (Reviewer's comment ID #: 52-98)]	Accepted
6-1019	A	75:52	75:52	"sand"-sized appears a bit too much jargon for a glossary; replace by diameter in mm [Michael Schulz (Reviewer's comment ID #: 229-24)]	Accepted
6-1020	A	75:55	75:56	A force is not balanced by a pressure (pure heresy for a physicist). Anyway, the definition is probably superfluous. [Michel Crucifix (Reviewer's comment ID #: 52-99)]	Noted, will be considerered
6-1021	A	76:4		This is a very poor definition of Heinrich event, confusing fact with hypothesis. Stating that Heinrich events are "indivative of cold periods" is both unimportant and potentially misleading. Furthermore, there is room for debate about how many Heinrich events there are. By some measures, there are only four, by others there are 8 or more. The definition should simply read as follows: Heinrich event: An interval of rapid flow of icebergs from the margins of ice sheets into the North deposition of sediment eroded from the land. [Eric Steig (Reviewer's comment ID #: 252-26)]	Accepted
6-1022	A	76:6	76:6	Often, though not always, coincident with the conclusion of cold events' instead of 'indicative of cold events' [Mark Siddall (Reviewer's comment ID #: 238-42)]	Accepted
6-1023	A	76:23	76:23	Recall that this is a shortcut for incoming solar radiation [Michel Crucifix (Reviewer's comment ID #: 52-100)]	Noted, will be cosisdered
6-1024	A	76:46	76:46	...and AD 1900 when most glaciers their maximum Neoglacial extent and temperatures in the northern.. [Atle Nesje (Reviewer's comment ID #: 190-7)]	Accepted
6-1025	A	77:5		REPLACE "float" BY "LIVE", GIVEN THAN SOME ALSO SINK OR SWIM [Govt. of Spain (Reviewer's comment ID #: 2019-139)]	Accepted
6-1026	A	77:7	77:10	The oxygen istopic ratio concerns in general all oxygen isotopes (16, 17 and 18). So first recall that 18 and 16 are the most abundant, and briefly say why they are useful (fractionnation during phase change, depending on temperature) [Michel Crucifix (Reviewer's comment ID #: 52-101)]	Accepted
6-1027	A	77:26	77:28	The 'top' age of Pleistocene is given according to the beginning of the Holocene, i.e.	Accepted

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				10,000 years ago. However, this is radiocarbon age and not calendar age. This should be mentioned. In ch 6, page 25, line 47 it is indeed written 'the beginning of the Holocene, approximately 11,600 years ago', here in calendar year. [Marie-France Loutre (Reviewer's comment ID #: 148-17)]	
6-1028	A	77:26	77:28	PLEASE REVISE DEFINITION ACCORDING TO THE LATEST PROPOSALS BY THE ICS AND INQUA. [Govt. of Spain (Reviewer's comment ID #: 2019-140)]	Accepted
6-1029	A	77:43	77:45	The definition of Quaternary is very much discussed and disputed nowadays. 'Quaternary',as a formal chronostratigraphic unit disappeared from the Geological Time Scale 2004. However there are several suggestion to include the Gelasian Stage in the Quaternary. This suggestion will most probably be discussed in 2006 or 2007. Then the beginning of Quaternary would be 2.59 Myr BP. On the other hand, Tertiary is not anymore a chronostratigraphic unit. Quaternary should not be defined according to something that does not formally exist anymore. Suggestion : according to the most widely accepted defintion in 2006, Quaternary is formed of two epochs, the Pleistocene and the Holocene, and it extends from 1.8 milion years ago into the present. [Marie-France Loutre (Reviewer's comment ID #: 148-1)]	Accepted
6-1030	A	77:43	77:45	PLEASE REVISE DEFINITION ACCORDING TO THE LATEST PROPOSALS BY THE ICS AND INQUA. [Govt. of Spain (Reviewer's comment ID #: 2019-141)]	Accepted
6-1031	A	77:44	77:44	The chronological term "Tertiary" is no longer in use; should be replaced by Cenozoic [Michael Schulz (Reviewer's comment ID #: 229-25)]	Accepted
6-1032	A	77:45		"from 1.8" (actually there is discussion at present whether the Quaternary is really 1.8 Myr or 2.6 myr but I guess this is OK for now. [Eric Wolff (Reviewer's comment ID #: 292-46)]	Accepted
6-1033	A	77:53	77:54	I do not support including see-saw in the definition list. Even one the original users of the concept (T. Stocker) tends not to use it anymore because it is misleading (the temporal evolution of the signals largely differ in the North and in the South). [Michel Crucifix (Reviewer's comment ID #: 52-102)]	Accepted
6-1034	A	78:2	78:2	represent => reconstruct [Michel Crucifix (Reviewer's comment ID #: 52-103)]	Accepted
6-1035	A	78:6	78:6	"early wood of the next" => "of the following spring" (this is more clear) [Michel Crucifix (Reviewer's comment ID #: 52-104)]	Rejected, text is clear as is
6-1036	A	78:10	78:12	The Younger Dryas is characterised by the reappearance of Dryas Octopetala in Europe, which has then been interpreted as a return to glacial conditions. There is now evidence that this return is hemispheric and associated with a change in the ocean circulation.	Noted, will be consisdered

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				[Michel Crucifix (Reviewer's comment ID #: 52-105)]	
6-1037	A	78:13	78:14	Explain what 13C is useful for [Michel Crucifix (Reviewer's comment ID #: 52-106)]	Noted, to be consisdered
6-1038	A	78:18	78:18	"its variation in time" -> be more accurate. the variations in its production are influenced by magnetism. The variations in its concentration are affected by its production rate and the ocean uptake (e.g. : the Younger Dryas plateau). Hence, sometimes, difficult interpretations (same problem for 10Be) [Michel Crucifix (Reviewer's comment ID #: 52-107)]	Noted, to be considered
6-1039	A	78:19		ADD "IN PRODUCTION" AFTER "variation" [Govt. of Spain (Reviewer's comment ID #: 2019-142)]	Accepted
6-1040	A	78:19		"Sun" "Earth" with capital letter (as with other planets) [Eric Wolff (Reviewer's comment ID #: 292-47)]	Accepted
6-1041	A	78:21	6:23	231Pa/230Th is not (to the best of my knowledge) used for longer term dating (U-Th is!). Pa/Th is a circulation proxy. Pa and Th are uniformly produced throughout the ocean but Th is removed quickly by adsorption onto particles. Pa is only weakly adsorped by particles and therefore is transported around the ocean by advection/diffusion and convection before it is removed to the sediments in areas of high particle or opal flux (Pa shows a strong affinity for opal). The Pa/Th ratio in the sediment is therefore a record of this transport and of past changes in ocean circulation. [Mark Siddall (Reviewer's comment ID #: 238-43)]	Accepted Accepted
6-1042	A	78:21	78:23	Pa/Th : also mention its use as a kinematic proxy (because of scavenging) [Michel Crucifix (Reviewer's comment ID #: 52-119)]	Accepted
6-1043	A	78:23		Surely Pa/Th is used mainly for assessing ocean circulation strength, not for "longer term dating" (and if the latter, longer than what?) [Eric Wolff (Reviewer's comment ID #: 292-48)]	Accepted
6-1044	A	78:25		KYR [Govt. of Spain (Reviewer's comment ID #: 2019-143)]	Accepted
6-1045	A	78:25		8.2 kyr event in 6.5.2.1, not 8.2k [Eric Wolff (Reviewer's comment ID #: 292-49)]	Accepted
6-1046	A	78:26		It's not an oscillation (excursion?), and it's more like 200 years not 400 years. [Eric Wolff (Reviewer's comment ID #: 292-50)]	Accepted
6-1047	A	80:0		Figure 6.1 middle panel: The temperature scale is unclaer.What is T=0 C refered to? [Govt. of Germany (Reviewer's comment ID #: 2011-27)]	Accepted, caption will be updated
6-1048	A	80:0		For Figure 6.1, middle and lower panels, scale lines on Y axes need to either be outward (as in the top panel), or at least as a layer above the data so that they are not covered. Many of these figures might benefit by the scale lines graphed outward rather than	Accepted

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				inward. [Govt. of United States of America (Reviewer's comment ID #: 2023-450)]	
6-1049	A	82:8	82:8	The d18O record cannot measure "global" warming; should be stated more prcisely [Michael Schulz (Reviewer's comment ID #: 229-26)]	Accepted
6-1050	A	84:0		In Figure 6.3, the meaning of the three stars at the top right is not explained. [Govt. of United States of America (Reviewer's comment ID #: 2023-451)]	Accepted
6-1051	A	84:16	84:16	Add a sentence to explain the stars in Fig 6.3 such as: The stars in the top right corner represent the today's levels of atmospheric greenhouse gas concentrations . [Renato Spahni (Reviewer's comment ID #: 249-6)]	Accepted
6-1052	A	85:0	85:0	Fig. 6.4: Add refs. [Monnin et al., Science, 2001] and [Flückiger et al., GBC, 2002] for Dome C CO2 in figure and figure caption. [Reto Knutti (Reviewer's comment ID #: 133-61)]	Rejected. The reference to Monnin, 2001 is in the caption. The reference to Flueckiger 2002 is given twice in the caption. Monnin et al. 2004 is the right reference for the figure as the age scale of the 2004 paper is used.
6-1053	A	85:0		Figure 6.4 : Very nice figure and shows quite clearly the big jump in rate of change of forcing. However I can't seem to find the a,b,c and d labels on the figures. [Gareth S. Jones (Reviewer's comment ID #: 121-64)]	Accepted. Labels to be added. Thank you.
6-1054	A	85:0		Fig 6.4: Year AD is horrid and confusing; not consistent with the rest of the chapter. A good rule is to use AD only when we don't get beyond 0 AD. [Eric Wolff (Reviewer's comment ID #: 292-51)]	Accepted.
6-1055	A	85:4		Figure 6.4: add labelling to the different figure panels (a), (b), (c), (d) [Renato Spahni (Reviewer's comment ID #: 249-7)]	Accepted.
6-1056	A	85:13		Dome C, not Dome Concordia (Concordia is the station not the dome) [Eric Wolff (Reviewer's comment ID #: 292-52)]	Accepted.
6-1057	A	85:15	85:16	sentence about the arrow makes no sense. I think you mean: "The arrow shows how the anthropogenic rate of change would be recorded in an ice core" (but it depends on the accumulation rate of the ice core so I don't really know what that means either). Explain better. [Eric Wolff (Reviewer's comment ID #: 292-53)]	Accepted, will be reworded
6-1058	A	86:16	86:22	The right part of Fig. 6.5 (regional dT vs. global dT) is still nowhere discussed or called. [Michael Schulz (Reviewer's comment ID #: 229-27)]	Accepted, figure called out where right part discussed
6-1059	A	86:22	86:22	Kucero" should be spelled "Kucera [Michael Schulz (Reviewer's comment ID #: 229-28)]	Accepted
6-1060	A	87:5	87:7	Not clear what reference state was used for producing the anomalies (present-day or pre-industrial?)	Accepted, CCSm and ECHO-G simulation specifics given in figure

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				[Michael Schulz (Reviewer's comment ID #: 229-29)]	legend
6-1061	A	87:11	87:11	As far as I can see, western Canadian glaciers are NOT shown in this figure. [Marie-France Loutre (Reviewer's comment ID #: 148-13)]	Accepted, Devon and Agassiz ice cores in Eastern Canada included in figure
6-1062	A	87:15		NGRIP said there was last ig ice at Dye 3, implying it should be a white dot. This came from the experts on greenland ice cores; you can disagree with them but not ognore them. [Eric Wolff (Reviewer's comment ID #: 292-54)]	Accepted, Dye 3 now colored gray to indicate evidence inconclusive if ice remained through the LIG
6-1063	A	88:0		Fig. 6.7a: I would suggest to replace the ARM data by a more widely accpected NADW proxy, e.g. benthic d13C from Shackleton, N.J., Hall, M.A. and Vincent, E., 2000. Phase relationships between millennial-scale events 64,000-24,000 years ago. Paleoceanography, 15: 565-569. [Michael Schulz (Reviewer's comment ID #: 229-30)]	Will be considered. Decided not to replace due to need to use proxy that is linked with circulation not water mass chemistry
6-1064	A	88:1		Figure 6.7 : may be useful to grey out Heinrich Events, if possible [Michel Crucifix (Reviewer's comment ID #: 52-108)]	Noted. Time scales are too uncertain to do so.
6-1065	A	88:5	89:11	Several comments here on the caption to Figure 6.7. First, page 88, line 6, change "panels e to f" to "e to g." Second, page 89, lines 8-11, I suggest moving info about parts of the figure already mentioned in the caption to those respective sections of the caption: I suggest moving "The Dansgard/Oeschger ...17" to line 6 (still page 89) of the caption, after citation of NorthGRIP. I suggest moving "the Heinrich events ... H6" to page 88, line 8, after "(Dokken and Jansen, 1999)." I suggest moving "the Antarctic warm events ... A4 are all shown" to line 7 (still page 89), after "2001." [Melinda Marquis (Reviewer's comment ID #: 162-7)]	Accepted. Accepted for D/O and A events, but not for H events as not directly visible in panel a).
6-1066	A	89:11	89:11	Sepcific how the ocean record is dada and aligned. [Michel Crucifix (Reviewer's comment ID #: 52-109)]	Accepted
6-1067	A	89:11	89:11	CH4 is *NOT* well mixed in the atmosphere, otherwise the concentration would be the same everywhere [Michel Crucifix (Reviewer's comment ID #: 52-110)]	Rejected. It is well mixed, but not completely mixed.
6-1068	A	89:16		Meridional (spelling) [Eric Wolff (Reviewer's comment ID #: 292-55)]	Accepted.
6-1069	A	90:1	90:1	In my opinion Lambeck K., Chappell J., 2001, Sea level change during the last glacial cycle, Science 292, 679 686. MUST be included here and their sea level curve MUST be shown on Fig.6.8a - the paper provides an excellent overview of the problems of deriving sea-level estimates in the past as well as a sea level curve that combines isostatic correction with data. The curve combines careful stratigraphic interpretation with careful dating and isostatic corrections for sea level (from an alternative model to that of Peltier, we should not put too much weight on any one model) - there is no excuse not to include it in a genuinely consensus piece of work. We need a product with which one cannot be	Since Lambeck was a co-author of the paper by Waelbroecke et al it is reasonable to assume that he was in accord with its conclusions. In any event the actually data points that define th Lambeck and Chappel curve are in fat shown on part b of his Figure where the complete history over the

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				left with the suspicion that one approach to the problem has dominated the outcome at the cost of another. By including the Lambeck and Chappell curve any remaining doubt on this will be gone and there will be a better balance between the three principal techniques available for sea-level reconstructions - fossil reef evidence, benthic oxygen isotopes and ice-sheet modelling. Some may criticise and argue down the Lambeck and Chappell curve in preference for alternatives but in a consensus piece of work our real uncertainty in this is best represented by its inclusion. This will make obvious the range of realistic estimates available. [Mark Siddall (Reviewer's comment ID #: 238-44)]	period since Last Glacial Maximum is shown.
6-1070	A	90:1	90:23	Figure 6.8 The figure should be drawn using isostatically corrected sea-level (ice equivalent sea-level) instead of relative sea-level since the latter is not identical where samples were corrected. The figure have still used SPECMAP data. For the last 5 years or so, large number of data sets are reported from coral reefs but they 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) but again this was ignored. The working group should use coral data as well as the compilation by Lambeck et al (2002 QSR v21 p343) to draw the figure because this is the one of the major development from the previous IPCC report. The larger figure (B) which described the sea-level history was also not correct. At first the sea-level should be drawn using isostatically corrected value ie. ice equivalent sea-level. 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). I hope the AR4 WG will modify this curve before the publication. [Yusuke Yokoyama (Reviewer's comment ID #: 298-4)]	As previously demonstrated in Peltier (2002, QSR 21, 377-396)) there is no significant isostatic correction required at the Barbados location in order to obtain euststic ice equivalent sea level from the local relative sea level history, if by ice –equivalent one means ice mass translated to water depth by applying only a correction for the difference between the density of ice and the density of water and employing the surface area of the present day ocean to map ice volume into water depth. If, on the other hand, one includes the time dependent variation in the surface area of the ocean due to coastline migration then a small correction is required but this is less than 5m at the conventional 21 ka age of LGM. According to either definition the difference between the eustatic depression at LGM for the ICE-5G(VM2) model and the LGM eustatic depression in the Lambeck and Chappell model is approximately 20m. This will be made clear in a revised version of the Figure on which the actual ie-equivalent eustatic curve of the ICE-5G model is superimposed

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					upon the loca RSL curve predicted by the model of the isostatic adjustmet process. Because several of the coral based records employed in the Lambeck et al reconstruction are contaminated by extremely high rates of tectonic uplift (eg Huon) or do not in fact extend to LGM, it is highly questionable as to whether these records should be employed o re-constrct euststic sea level history.
6-1071	A	90:1		Figure 6. 8 : Why not directly providing the the eustatic sea-level curve given by the model, rather than the Barbado's one ? [Michel Crucifix (Reviewer's comment ID #: 52-111)]	A revised vrsion of the Figure has been produced on which both the eustatic cure of the ICE-5G model and the GIA predicted RSL history are superimosed. so as to directly demonstrate their essential equality.
6-1072	A	91:0		Do we have any idea whether theses estimated warmest temperatures are annual or seasonal? The text talks about 'proxy records more sentitive to specific seasons'. Maybe this should be reminded in the caption of this figure. [Marie-France Loutre (Reviewer's comment ID #: 148-19)]	Accepted
6-1073	A	91:0		Fig. 6.9: Barents sea data: the reconstruction of Sarnthein et al (2003. Centennial-to-millennial-scale periodicities of Holocene climate and sediment injections off the western Barents shelf, 75 N. Boreas, 32: 447-461.) suggests an earlier warm peak than that of Duplessey et al. --> should be included [Michael Schulz (Reviewer's comment ID #: 229-31)]	Accepted
6-1074	A	92:0		Figure 6.10: the figure summarizes the current knowledge of the papeoclimatology about the development of the northern hemisphere temperature during the last 1400 years. The figure illustrates the high agreement and much evidence of different paleoclimate series. Therefore we see the figure to be important in the communication of climate change to policy makers and the public. But for this purpose figure 6.10c is not well designed. Please consider the possibility of plotting a graph (solid line) into the overlapping range where it reaches highest values. [Govt. of Germany (Reviewer's comment ID #: 2011-28)]	Rejected, important not to give the indication of one being more important than the others
6-1075	A	92:1	92:4	The authors of this chapter should request an explanation from the lead authors of the SPM of why there is not a single graphic from the chapter shown in the SPM. Every other major section of the SPM has at least one supporting graphic. The lack of a supporting	Noted

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				graphic in the "A Paleoclimate Perspective" section is effectively a slap in the face to chapter 6 authors. It also sends a disturbing message that AR4 is somehow backing away from paleoclimate-based claims made in the TAR where the results from paleoclimate studies were highlighted. Yet, a reading of chapter 6 shows no such thing, and in fact reveals more robust evidence in support of the key conclusions. Chapter 6 highlights the fact that there are now a large number of different paleoclimate studies which all lead to the same key conclusion that northern hemisphere mean temperatures in recent decades are likely unprecedented in at least a millennial timeframe. Moreover, several of the newer studies extend these conclusions back to at least the past 2000 years. It was a mistake for the authors of the SPM in the TAR to show only one reconstruction (that of Mann et al, '99) when in fact there were multiple reconstructions shown in the body of the report (chapter 2) which supported the main conclusion regarding anomalous late 20th century warmth. This clearly set up one study as a straw man for attack. AR4 has an opportunity to undo the damage of that unfortunate decision, and show in the SPM Figure 6.10 which indicates that the key conclusions regarding recent hemispheric warmth in a millennial context are now supported by more than a dozen different reconstructions taking into account the ensemble of uncertainties associated with the different reconstructions. [Michael Mann (Reviewer's comment ID #: 156-55)]	
6-1076	A	92:6	92:6	It may be helpful to add to the caption (Figure 6.10 a) "since 1850 (land and marine) and since 1781 (land only)." Page 27 (lines 21-30) provides this distinction and level of detail; it may be helpful if the caption does also. [Melinda Marquis (Reviewer's comment ID #: 162-8)]	Rejected – the caption was considered too long and had to be modified and shortened. The reference to theTable (6.1) is clear and provides such detail.
6-1077	A	92:7	92:7	gray shading: standard errors : I suppose the standard error is on the smoothing, not the annual values. Please clarify [Michel Crucifix (Reviewer's comment ID #: 52-112)]	Rejected – the interpretation is considered obvious anyway
6-1078	A	92:9	93:3	It is not easy to identify particular studies from the record abbreviations - this is a problem because particular records are referred to in the text (e.g., Chapt. 6, p. 32, line 24) [James Crampton (Reviewer's comment ID #: 50-29)]	Noted – and we have some sympathy for this view but severe space problems have led us to the curent formulation , and specific records can be traced throug the Table 6.1
6-1079	A	93:6	93:6	replace "region" by "temperatures" [Michel Crucifix (Reviewer's comment ID #: 52-113)]	Accpted – the text has been revised anyway
6-1080	A	93:9	93:11	The use of smoothed filtered curve right upto the end of the data is misleading. The top plot seems to suggest serious downturns in the upward trend of temperatures at the start of the 21st century. This gives an incorrect impression of the real underlying trend, and may be misused by some	Rejected –this issue is dealt with in the responses to comments to Chapter 3 . Sufficient information on the operational characteristics of this

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				[Gareth S. Jones (Reviewer's comment ID #: 121-65)]	smoother (and the specific end effects of the chosen "padding") are supplied to allow the reader to fully appreciate the issue.
6-1081	A	93:12	93:12	Add "Soon and Baliunas (2003) have pointed out that the poor number and distribution of samples, particularly for the early period, is such that these reconstructions have to regarded with suspicion" [VINCENT GRAY (Reviewer's comment ID #: 88-785)]	Rejected – sufficient caveats have been supplied.
6-1082	A	94:0	94:0	Figure 6.11 could show symbols for kauri tree-rings records (Boswijk et al. 2006 The Holocene 16(2), 188-199) and speleothems (Williams et al 2004 The Holocene 14(2) 194-208) in North Island of New Zealand and speleothems in the South Island (Williams et al 2005 Earth & Plan Sci Letters 230, 301-317). [Paul W Williams (Reviewer's comment ID #: 291-8)]	Rejected – a clear criterion for inclusion had to be chosen – and it was decided t show those series/sites that had been incorporated specificlly in large-scale temperature reconstructions.
6-1083	A	94:5	94:5	Should be clarified that 1000 etc. denote years AD [Michael Schulz (Reviewer's comment ID #: 229-32)]	Rejected – Section deals with last 2000 years so context considered clear – but will comply with general format adopted
6-1084	A	94:9	94:9	Add at end "The inadequate number and distribution of these samples, particularlky for the earlier period means that the hemispheric and global averages should be regarded with suspicion (Soon & Baliunas 2003)" [VINCENT GRAY (Reviewer's comment ID #: 88-786)]	Rejected – see reponse to Comment6-1081
6-1085	A	95:11	95:11	Add at end "The absence of any overall temperature rise from 1350 to 2000 in the last sample suggests the possibility that sites remote from human habitation are exempt from the surface warming claimed for the 20th Century" [VINCENT GRAY (Reviewer's comment ID #: 88-787)]	Rejected – not sufficient evidence to make this statement
6-1086	A	96:0		Figure 6.13: All panels of the figure (a-e) should be shown on the same page in order to assure a comprehensive overview. The title of panel c) should be consistent with the underlying text on page 37. Please use instead of "all other forcings" "anthropogenic forcing". [Govt. of Germany (Reviewer's comment ID #: 2011-29)]	Accepted
6-1087	A	96:0		Figure 6.13: As the last 200 years are of special interest (an additional forcing has exceeded the natural forcings by a factor of 3) in communicating the attribution of causes of climate change we urge strongly to add a zoomed figure containig the information of figure 6.13e for the last 200 years only. [Govt. of Germany (Reviewer's comment ID #: 2011-30)]	Rejected- space restrictions meant that it was hard to include this Figure in the first place and further space can not be allocated to accommodate this suggestion
6-1088	A	97:0	97:0	Fig. 6.13: There are no panels a to e, but just one, please update the caption and the corresponding text.	Rejected – the caption was considered too long and had to be modified and

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				[Reto Knutti (Reviewer's comment ID #: 133-60)]	shortened. The reference to theTable (6.1) is clear and provides such detail.
6-1089	A	97:0		Figure 6.13. The thick lines are not recognizably different from the thin lines. Suggest that either the thick or thin lines also be made dashed. [Govt. of United States of America (Reviewer's comment ID #: 2023-452)]	Rejected – the interpretation is considered obvious anyway
6-1090	A	97:6	97:7	As written it looks like I am expecting forcings in a-c and e. Better to put ";" after a-c. Then: "; (e) Annual mean NH temperature using the same forcings in a set of experiments designed…" [Eric Wolff (Reviewer's comment ID #: 292-56)]	Noted – and we have some sympathy for this view but severe space problems have led us to the curent formulation , and specific records can be traced throug the Table 6.1
6-1091	A	97:10	97:10	Should be Figure 6.10c [Marie-France Loutre (Reviewer's comment ID #: 148-12)]	Accpted – the text has been revised anyway
6-1092	A	97:10		Figure 6.13, legend, line 10. In the parenthetical statement "(modified from Figure 6.11c)" you really mean Figure 6.10c. [Govt. of United States of America (Reviewer's comment ID #: 2023-453)]	Rejected –this issue is dealt with in the responses to comments to Chapter 3 . Sufficient information on the operational characteristics of this smoother (and the specific end effects of the chosen "padding") are supplied to allow the reader to fully appreciate the issue.
6-1093	A	98:0		Fig 6.14: Traufetter is not listed in the caption or ref list, and is anyway an odd choice; I think F. Joos was obtaining a record from EPICA from Udisti or Castellano: if this is not already obtained it can be provided. [Eric Wolff (Reviewer's comment ID #: 292-59)]	Taken into account. Reference added. No basis for assertion given that reference is an odd choice. Will try again to get high-resolution data from Udisti.
6-1094	A	98:1		Figure 6.14 : Why is there so little apparent agreement between Bigler et al. and Miding ? [Michel Crucifix (Reviewer's comment ID #: 52-114)]	Noted. Main features discussed in the text. are common among both data sets. Remaining differences are probably related to different procedures to remove volcanic spikes and analytical uncertainties.
6-1095	A	98:3		Fischer et al is not shown on the graph [Eric Wolff (Reviewer's comment ID #: 292-57)]	Accepted. Ref. Deleted.
6-1096	A	98:4		Mieding (only available as a report) is a very strange choice and not needed since you already have Bigler for Greenland; there is no Antarctic reference listed [Eric Wolff (Reviewer's comment ID #: 292-58)]	Rejected. Like to show results from independent laboratories.

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6-1097	A	99:1		Figure Box 6.1 Figure 1 : Clarify the references. Is it really the BER90 solution which is shown here ? Berger and Loutre usually argue that for these time scales, the BER78 is to be preferred. Clarify how the seasons are defined. [Michel Crucifix (Reviewer's comment ID #: 52-115)]	Rejected – not sufficient evidence to make this statement
6-1098	A	99:10		W/m^2 or W m^-2 [Eric Wolff (Reviewer's comment ID #: 292-60)]	Accepted
6-1099	A	100:0		Box 6.3 Mt Kenya is misspelled [Govt. of Sweden (Reviewer's comment ID #: 2020-15)]	Rejected- space restrictions meant that it was hard to include this Figure in the first place and further space can not be allocated to accommodate this suggestion
6-1100	A	100:0		Box 6.3, figure 1, needs a caption, not just a list of places. [Eric Wolff (Reviewer's comment ID #: 292-61)]	Accepted
6-1101	A	100:1	100:3	Figure for Box 6.3 (Figure 1): Do the arrows on the ends of the vertical color bar have any meaning? If so, it's unclear to me what that meaning is. If not, I suggest removing the arrowheads and leaving just a plain color bar (an elongated rectangle). [Melinda Marquis (Reviewer's comment ID #: 162-9)]	Noted- issue will be reviewed with regard to clarity and attempt madeto improve the situation if deemed necessary
6-1102	A	100:1		Box 6.3 Figure 1 : why different colors for Svalbard and New Zealand ? Clarify color code in the legend. [Michel Crucifix (Reviewer's comment ID #: 52-116)]	Noted – this Figure will be changed to separate (e) which will go in a different Figure . Caption will be reviewed.
6-1103	A	100:5	100:5	There needs to be an indicator of length on the vertical scale [VINCENT GRAY (Reviewer's comment ID #: 88-788)]	Accepted
6-1104	A	100:5	100:5	Add at end "Records of length changes in glaciers". It should be noted that there are many glaciers that have increased in the 20th Century" 413 6-413 789 [VINCENT GRAY (Reviewer's comment ID #: 88-788)]	Accepted
6-1105	A	100:5	100:12	Caption seems incomplete (only refs) [Michael Schulz (Reviewer's comment ID #: 229-33)]	Accepted
6-1106	A	100:6	100:11	The term "calibrated" should be explained in the text or in the figure caption - are these glacial records somehow calibrated with instumental records? And if so what do these show? There is not place this is explained. [Gregory Wiles (Reviewer's comment ID #: 289-31)]	Noted, text will be considered, calibrated has to do with transfer from radiocarbon dated age to calendar ages
6-1107	A	100:9	100:9	..Matthews et al., 2005; Nesje et al., 2005). [Atle Nesje (Reviewer's comment ID #: 190-8)]	Accepted
6-1108	A	102:1		Box 6.4, Figure 1 : "scaled to have zero mean and unit standard deviation over the period 800-1995". Is it easy to justify ? Variance depends on the region and so all records are not supposed to have the same variance.	Noted – the reviewer is correct , but the curent approach is designed not to over-emphasise specific records (or to

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Michel Crucifix (Reviewer's comment ID #: 52-117)]	contradict the more appropriate interpretaion shown in Figure 6.10).
6-1109	A	102:20	102:20	Add "The inadequate number and distribution of these samples, particularlky for the earlier period means that the hemispheric and global averages should be regarded with suspicion (Soon & Baliunas 2003)" [VINCENT GRAY (Reviewer's comment ID #: 88-790)]	Rejected – sufficient caveats have bee provided in the text
6-1110	A	103:0		Comment on Question 6.1 Figure 1. This figure is somewhat misleading. The earth's orbit is not this ellipitical, and the sun is shown as being too near the center of the ellipse rather than at a focus unless the earth's orbit is intended to be on a plane with respect to the plane of the paper. If this is the case a coordinate system might help illustrate the idea that the earth's orbit is at an angle to the plane of the paper. In addition the figure shows that the eccentricity of the orbit is changed without changing the locations of the perihelion or the apehelion. Also the figure does not show either the precession of the perihelion of the earth's orbit or the time variation of the tilt of the earth's orbit with respect to the plane of Jupiter's orbit. [Wilmer Anderson (Reviewer's comment ID #: 5-39)]	Rejected, the figure has been changed, but the purpose is illustrative rather than being strictly accurate since this is to convey the point made in a schematic fashion
6-1111	A	103:1		Question 6.1 Figure 1: The Figure contains several mistakes. 1. Precession: the ellipse showing the precession movement should be round the axis. Even though, it would still imperfectly represent the notion of climatic precession. The important is to show that perihelion occurs alternatively in Spring; Summer, Autumn etc. Sun : it is essential to put the Sun at one of the two focii of the ellipse (here, it is almost centred). Obliquity (more widely used than tilt) is defined between the axis and the perpendicular to the ecliptic. This is not well shown either. [Michel Crucifix (Reviewer's comment ID #: 52-118)]	There are no mistakes, but a misunderstanding: this is supposed to be a perspective view from diagonally above. The figure has been changed to make this clear.
6-1112	A	103:5		Comment: I would like for the figure caption to describe the changes in the orbit a bit more so that the figure is a bit more understandable. [Wilmer Anderson (Reviewer's comment ID #: 5-40)]	Accepted - while trying to keep the caption short.