IPCC Working Group I Fourth Assessment Report

Expert Review Comments on First-Order Draft

Responses to comments included
January 27, 2006

Chapter 10

The following compilation of review comments and author responses is supplied by the Working Group I Technical Support Unit as a record of the process used to prepare the Working Group I report. These comments and responses are not to be edited and/ or re-distributed in part or in full to others.

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

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10-1	A	0:0	0:0	I would like to suggest that the authors try to avoid referring to specific SRES scenarios when dealing with the various stabilization runs. The differences between them reflect the CO2 (and other GHG) concentration (and total CO2-equivalent forcing) at stabilization. This would be better if expressed in that form - e.g., instead of a 'B1 commitment', e.g. 550 ppmv CO2-equivalent stabilization. This would help to be clear that we are not suggesting that these are like the stabilization pathway studies, but rather intended as tests of the physical climate only for specified RF. See suggestion regarding a table summarizing the different stabilization cases. [Susan Solomon]	Noted, change made in revised draft where possible
10-2	A	0:0	0:0	The modelling chapter is always a difficult one, but the authors have succeeded very well in this first draft. My comments here are intended largely to help the readability for the non-expert reader. I think the document could be strengthened by summarizing what conclusions are robust (e.g., mid continental drying? increases in the tropical precipitation max?). A table could be helpful on this. [Susan Solomon]	The executive summary has been re-written, though "robust" is difficult to define for a projection. We list the consistent and notable results in executive summary
10-3	A	0:0		This comment, and those in following rows, refers to the Executive Summary of Chapter 10. In my opinion there are too many references to the meridional overturning circulation. I assume that this topic is also addressed in Chapter 11. Specifically, in the list of 24 findings corroborating the results from the TAR (page 10-3), there are 7 findings dealing with MOC. I got the impression that most of the references deals with the phenomenon in the North Atlantic. The language is not clear regarding when a statement dealing with MOC refers to the North Atlantic or when is a general statement. [PATRICIO ACEITUNO]	Noted. Executive summary has been re-written.
10-4	A	0:0		This is an excellent chapter, but I think the Executive Summary could be improved by getting to the main points faster than it does now. It begins with a long, rather bureaucratic paragraph that is full of acronyms about the various models. This beginning will mean little to most readers. These details should be left to the body of the chapter, and the Executive Summary should get right to the main results of the model runs. I was surprised to see that the summary bullets beginning on line 31 of page 10-3 did not lead off with a statement related to how the models are all projecting a major increase in global mean temperature, consistent with earlier model results as reported in the TAR, and that the rapid and significant warming projected by these models is due largely to human activities, particularly the burning of fossil fuels. I suggest that the Executive Summary begin with a statement about global temperature projections and their causes, and any major changes since the TAR.	Noted. Executive summary has been re-written.

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				[Richard Anthes]	
10-5	A	0:0		In discussing projected global sea level rise due to melting of glaciers and ice caps, it might be useful to include a statement about the rise if all glaciers and ice caps melted—to put a limit on the ultimate sea level rise due to this effect. [Richard Anthes]	Noted. Executive summary has been re-written.
10-6	A	0:0		TSU NOTE: Please see supplementary review material [Simon Brown]	Noted. Executive summary has been re-written.
10-7	A	0:0		Please remind the authors that in the ideal case the first sentence of each paragraph should be such that the reader knows what he can expect in the remainder. [Gerrit Burgers]	Noted. Executive summary has been re-written.
10-8	A	0:0		This chapter has fine content but is punishingly long. [Garry CLARKE]	Noted. Revised chapter streamlined where possible
10-9	A	0:0		There appears to be some randomness as to the materials included in this chapter and the order in which they are presented. [Robert E. Dickinson]	Noted. Executive summary has been re-written.
10-10	A	0:0		Chapter 10 gives bullet points of findings that (i) corroborate the TAR and (ii) are new since the TAR. Are there any findings that have been contradictory or disproven since the TAR and need to be included in these bullet points to ensure balance (perhaps there are not and this should also be pointed out) [Melanie Fitzpatrick]	Noted. Executive summary has been re-written.
10-11	A	0:0		In the Exec Summary of Chapter 10 the changes are qualitative. Is there a way that they could use the format of the TAR to give some quantitative scale (likely, highly likely etc.) [Melanie Fitzpatrick]	Noted. Executive summary has been re-written.
10-12	A	0:0		In Chapter 10 it is worth mentioning (when discussing commitment and present day emissions) that there is a high degree of uncertainty even in quantifying what out present day emissions actually amount to in Gt C. [Melanie Fitzpatrick]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-13	A	0:0		Well documented enabling to non-specialists to better understand global climate projections. [Savitri GARIVAIT]	Noted, thank you.
10-14	A	0:0		Another excellent chapter, that however would benefit from some shortening. I downloaded 60 pages of figures. I realize that one figure is worth 1000 words (and perhaps more), but do we really need them all? [FILIPPO GIORGI]	Number of figures has been reduced in revised draft.
10-15	A	0:0		I did not see anywhere a statement similar to the TAR's "Global temperature change is	Noted. Executive summary has been

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				projected to increase by 1.4-5.6.C by 2100. So much of the uncertainty is due to …, so much is due to …". I was quite surprised by this. The statements about global warming range in the chapter are somewhat vague and certainly convoluted. Was this done on purpose or this range has not been evaluated yet? I am sure the public opinion will expect some sort of clearly stated revision of the global warming range given in the TAR. [FILIPPO GIORGI]	re-written.
10-16	A	0:0		Same comment as the previous one applies to changes in global sea level rise. [FILIPPO GIORGI]	Noted. Executive summary has been re-written.
10-17	A	0:0		I find it difficult to take this Chapter seriously. Any responsible organisation involved in future projections needs to be interested in the success of their past and current projections in order to learn how to improve them.The IPCC is the only body I know which shows not the slightest inerest in whether their projections are successful or not. One is forced to assume that they are intended puirely as propaganda and not serious science or economics [Vincent Gray]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-18	A	0:0		Past and present IPCC emissions scenarios have failed miserably to correspond with actual climate parameters. [Vincent Gray]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-19	A	0:0		The earliest series were the four "Policy scenarios" described in the first IPCC WG1 Report "Climate Change (1990) which dated from 1985. The "Business as Usual" scenario in that Report is now quite unbelievable. It showed, for example, carbon dioxide and methane concentrations well above those now measured. Yet there are those who would continue to quote it [Vincent Gray]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-20	A	0:0		The IS92 scenarios put forward by the 1992 Supplementary Report exaggerated many climate parameters when tested by Gray, V R (1998) . The IPCC future projections, are they plausible? Climate Research, 10 155-162. Instead of revising these for :Climate Change 1995" the IPCC changes some of the early figures without altering the whole scenarios, and pretenede that they were still valid. Further experience since 2000 has therefore shown them to be further out of line with reality [Vincent Gray]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-21	A	0:0		This Chapter continues to sponsor the SRES scenarios, dating from 1990. imposed on "Climate Change 2005, as realistic future projections. Gray, V R (2002) The Greenhouse Delusion: a critique of 'Climate Change 1995' pages 71-78, London, Multi-Science publishers; has shown that the SRES scenarios also contain poor confirmation of the climate parameters of the year 2000, such as methane concentrations and coal production.	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction

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				The scenarios cannot even predict the past, so what is the chance that they can predict the future? [Vincent Gray]	
10-22	A	0:0		The IPCC has consistently rejected the submissions of the senior economists Ian Castles and David Henderson,, who point out that the economic projections used in th IPCC SRES scenarios are technically unsound because the procedure used by the modellists is not permissible under the rules of the internationally-recognised System of National Accounts.. [Vincent Gray]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-23	A	0:0		If you were serious, you should include several paragraphs in the early part of this Reportt discussing the fate of the scenarios and it should lead to a major revision of the SRES scenarios to make them better comply with current economic practice, and with the actual climate parameters currently available from observations [Vincent Gray]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-24	A	0:0		All the results quoted are from models which have never been shown to be capable of accurate prediction, with assumed increases in forcing which have never been observed [Vincent Gray]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-25	A	0:0		Considering the rest of the chapter, I found it very clear and informative. I think it summarizes well the state-of-the-art. [Stéphane Hallegatte]	Noted, thank you.
10-26	A	0:0		It is a great job to assess all the results that have been obtained. In general, cross referencing to other chapters and the validation of the models could get more attention. Biases are shown, but not explained. The results could be weighted with the quality of the models. Some papers are doing this already and this should be mentioned when appropriate (e.g. Schmittner et al 2005 GRL for the MOC, Oldenborgh et al 2005, Ocean Sciences for ENSO). [Wilco Hazeleger]	The issue of model weighting not being appropriate is now discussed in Ch. 8.
10-27	A	0:0		It should be mentioned that Dr. Hansen suggested that the IPCC scenarios are rather pessimistic (http://www.sciam.com/media/pdf/hansen.pdf). I understand that conventional scenarios remain useful, but the question raised by Dr. Hansen is already well known through internet. Thus, the readers of the IPCC report will feel curious if such questions are neglected, and hence, the value of the report might be adversely affected. [Kiminori Itoh]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-28	A	0:0		This chapter summarizes an incredible amount of model output and data and I like in particular the multi-model figures. The chapter is somewhat brief on the projections of concentrations and abundances of various radiative agents. This is probably a	The uncertainty in CO2 concentration is adressed in section 10.4.1 Uncertainties in other greenhouse gases

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				consequence of most AOGCMS using prescribed concentrations. However, from a policy perspective it is important to adress also uncertainties in future abundances and concentrations. There seems to be potential for shortening existing text and streamlining the flow of the chapter. [Fortunat Joos]	or aerosols abundance for a given emission scenario is not adressed here. A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-29	A	0:0		The chapter is silent on ocean acidification, changes in the saturation horizon of aragonite and calcite, and on pH changes. The aragonite saturation horizon is projected to shoal by order 1000 m over this century and SO average surface water becomes undersaturated with respect to aragonite (a metastable form of $CaCO_3$) for CO_2 concentrations exceeding ~600 ppm and thus in most SRES scenarios (Orr et al., Nature, 2005 their Figs. 2 and 5). Similarly, surface water in the subpolar Pacific and the Arctic are projected to become undersaturated. The shoaling of the aragonite lysocline threatens abundant cold water corals in the deep, and calcifying organisms at the surface. Decreases in pH are expected to have consequences on the calcification rates of (warm) water corals and are thus an issue for coral reefs. Though the impact of acidification on organisms is not a WGI topic, the biogeochemical projections of ocean acidification is. Ocean acidification needs to be treated here in the TS, WGI SPM and the Synthesis report. Unfortunately, acidification got not enough attention in earlier assessments.There is already text on acidification in chapter 5 and 7 and in the TS. The present chapter should show a few figures demonstrating the shoaling of the aragonite lysocline and that SO surface water becomes undersaturated for most SRES scenarios (see e.g. Orr et al.2005). The models of intermediate complexity may be used to generate further results. [Fortunat Joos]	Taken into account A section about future changes in ocean pH is added in section 10.4..
10-30	A	0:0		This is arguably the most important chapter in the entire AR4. The strongest and weakest points center around the carbon dioxide input scenarios for the various models, and much of my commentary is tendered on that. It is worthwhile to lead with a quote from Hansen and Sato (2004, Proceedings of the National Academy of Sciences): "Growth rates of climate forcings in the past several years have fallen below all IPCC 2001 scenarios". The growth rate in % carbon dioxide (ppmv) for the decade ending in 2004 is 0.50, in 1994 is 0.41, and 1984 is 0.42. In other words, it has taken more than thirty years for the smoothed growth rate to increase less than ten percent. This has several implications. The use of 1%/year growth rates, while a (perhaps) acceptable common forcing for model intercomparsion studies, is certainly not warranted for studies bounded by 2050 and likely bounded by 2100. Even if the rate of increase eventually reaches or slightly exceeds 1%, it is not likely to do so in the coming few decades. Given the lag time between emissions and ultimate climate response, that means all estimates of climatic change to at least 2050 must be scaled back, at least to scenario	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction

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				B1. Because of the common linearity between model response and percent increase in carbon dioxide, IPCC should note that the results displayed in Chapter 10, where they do not follow B1, should be scaled down proportionally to the B1 scenario. [Jeffrey Kueter]	
10-31	A	0:0		Overall, the chapter has an amazing amount of information included and is written in a quite useful and ordered way. The Executive Summary, however, seems like it has been pasted together without much thought given to a coherent sequencing of information, with some topics covered in quite a number of places. In addition, the seal level section seems to me to be in need of considerable more development as the estimates come to seem quite out of touch with what seems to be actually happening (and with what happened during the 20th century (specific comments are included below regarding that section). [Michael MacCracken]	Some re-ordering has been done in the revised chapter, and sea level rise section has been added to and clarified.
10-32	A	0:0		Opening Comment: In the Chapters that I am reviewing, I choose to not provide an anonomous review. This choice allows the various Chapter authors to contact me directly on matters of errors, concepts, or questions of disagreement. I have already performed thorough reviews of chapters 1-5. Due to the looming November 4th deadline for reviews, I am choosing to review Chapters 6-11 in a drastically shortened way . Rather than going through all of them as I did before, I am choosing to review only the Executive Summaries of chapters 6-11. There are some clear advantages for this strategy, independent of the obvious one of speeding up the very tedious reading and reviewing process. In the previous chapters I have reviewed, I have seen some significant disconnects between two obviously differering reporting strategies. First, it seems obvious to me that the fundamental purpose of these IPCC FAR reviews is to establish the case, or lack therof, for many of the diverse aspects of the human-caused global warming problem. Second, it is noteworthy that this draft WG1 report is roughly twice as long as the WG1 IPCC TAR report. Third, it seems very obvious that the key IPCC assessment-relevant punchlines are hardly double those of IPCC TAR. It seems clear to me that the global-warming research-advancement doubling time scale is a lot closer to twenty years than it is to five years. The obvious conclusion for me is that we don't really need or desire to double the length of the WG1 chapter assessment every five years! For these nearly obvious reasons, and to help me and the other reviewers refocus on the fundamentally important conclusions that are centrally relevant to the IPCC's human-caused climate assessment's goals, I am thus choosing to reduce drastically my own submitted WG1 reviews. And, most importantly, this gives me a good shot at reviewing meaningfully all of remaining chapters 6-11 by the daunting November 4th reviewers' deadline.	Noted. Executive summary has been re-written.

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				[Jerry Mahlman]	
10-33	A	0:0		GENERAL COMMENTS ON CHAPTER 10: GLOBAL CLIMATE PROJECTIONS I found this chapter to be informative, to the point, consistent with their charge from IPCC WG1, and like some of the other chapters, excessive in length relative to the previous IPCC FAR Assessment. It has been an inspiration to see just how far along this climate model development has expanded into a world-wide co-operative effort to learn from each group's strengths, weaknesses, and improvements in understanding of the climate system, and its access to probing through the science and technology of mathematical modelling of earth's climate. From personal experience, all I can say it that the model building, testing, running, and analysing is far more work than anyone outside the enterprise, scientist or not, can possibly appreciate, or comprehend. Indeed, the IPCC assessment process has empowered the advancements in co-operation in climate modelling and diagnosis at a level that the global climate warming policy and emissions mitigations technologists can possibly comprehend. [Jerry Mahlman]	Noted. Revised chapter has been shortened somewhat
10-34	A	0:0		It is very encouraging to see this integrated common-sense evaluation of what can occur within the climate system over the next 100-1000 years. [Jerry Mahlman]	Noted. Thank you.
10-35	A	0:0		TSU NOTE: Please see supplementary review material [Koki Maruyama]	noted
10-36	A	0:0		As a member of TS/SPM drafting team, I am a bit concerned about the usage of the word "commitment". Perhaps to respond to such a concern, in the ZOD of the Technical Summary (21-October-2005) the definition of "Climate Change Commitment" is stated clearly in a box. The essential part of the 3 paragraphs of the definition there is: - Climate change commitment can be defined as the further increase of temperature, or any other quantity in the climate system that continues to change if even if the forcing were to be stabilized. - An alternative aspect of committed climate change is to identify the effect of past emissions by considering climate change model projections in which future emissions are set to zero. - Both ways of viewing climate change commitment are considered in this report. Where the term climate change commitment is used without further qualification it refers to the future commitment with radiative forcing held constant. Where climate change due to past emission is used it refers to the commitment in the absence of further emissions. Though wording may not be the same in the TS and Chapter 10, it is desirable to	Use of commitment has been revised in the chapter to be consistent with the definition in the TS.

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				distinguish conceptually the above two kinds of commitment. .... (TSU - SEE FURTHER COMMENTS IN SUPPLEMENTARY REVIEW MATERIALS FOR CHAPTER 10) [Taroh MATSUNO]	
10-37	A	0:0		Although the Chapter 10 Figures are generally much better than those of Chapter 1, which were extremely poor, they should still be improved. [Lourdes Maurice]	Figures have been revised in second order draft.
10-38	A	0:0		Climate change impacts are solely based on a hierarchy of models. Chapter 9 pointed out many serious issues with reliability on models. Recommend making use of other resources (e.g., data from actual observations). [Lourdes Maurice]	This is a chapter on climate change projections based necessarily on climate models, not observations.
10-39	A	0:0		In general, I found chapter well written with proper articulation of the progress achieved in the research since TAR. Meanwhile there are some comments to few sections of the Chapter. [Valentin Meleshko]	Noted. Thank you.
10-40	A	0:0		Text is difficult to read and jumps from one issue to the next [Axel Michaelowa]	Noted. Executive summary has been re-written.
10-41	A	0:0		Unfortunately, I have not had time to read this chapter fully, hence my comments are limited to the summary, though I have read most of the chapter. There are a lot of results. It is good to see the use of multi model ensembles in looking at changes in variability and extremes enabling something meaningful to be said about at least some changes in variability. I think most of the material is there, but it would benefit from some tightening up as to what are the key new findings and tying the assessment together. For example, grouping changes in temperature, precipitation, sea level and circulation together, then changes in variability and extreme events. Also, there are some groups of diverse results (eg on hurricanes) which need to be assessed overall, not just reported. [John Mitchell]	Noted. Executive summary has been re-written.
10-42	A	0:0		A minor general point - I notice that recent (since the TAR) are cited on mechanisims of change (eg in the Altlantic MOC) which often repeat earlier analyses ((in this example by Manabe and co-workers at GFDL) I understand the need for brevity, but it might be useful to indicate where the recent results are consistent with earlier findings [John Mitchell]	Noted. Executive summary has been re-written.
10-43	A	0:0		In dealing with the contrbiution to sea level rise from the ice sheets, this chapter faces a difficult task because ice sheet models have not been validated in the same sense as are GCMs. Furthermore, ice sheet models have been unable to reproduce key dynamic features of the ice, such as ice streams which must be forced into models. The behavior of ice streams may be a key to projecting the future contribution of Antarctica to sea level	Sea level rise section has been revised.

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				rise. The response of grounding lines and grounded ice to removal of ice shelves may also be a key to projection. Current models have failed to reproduce both sorts of behavior as currently observed, and therefore their value for projection must be questioned. Nevertheless, Chapter 10 is obligated to report the outcomes of model experiments as its chief product. The only way to deal with this situation is to fully assess model uncertainties. The chapter makes honest atttempts in this direction but in the end is still deficient in this aspect. Other approaches (Bayesian or scenario-based assessments of future outcomes) have been explored in the literature, and are reported in WGII Chapter 19. Whether it is within the purview of Chapter 10 to also assess such approaches needs to be decided. If the authors do not wish to do so, they must at least create an easy pathway for readers to reach Chapter 19, either through citations of the literature used in Chapter 19 or by reference to the chapter itself, or both, so that readers are aware of alternative approaches and can read both chapters in an integrated fashion. [Michael Oppenheimer]	
10-44	A	0:0		Please note: Weisheimer, A., Palmer, T.N., 2005: Changing frequency of occurrence of seasonal-mean temperatures under global warming. GRL, 32, L20721 [Timothy Palmer]	Noted. Reference added.
10-45	A	0:0		Overlap and sometimes inconsistent with chapter 8, concerning in particular executive summaries and chapters 8-6 and 10-5 [Michel Petit]	Noted. Executive summary has been re-written.
10-46	A	0:0		A major correction that is needed for this chapter is to avoid indicating that increased precipitation necessarily implies 'wetter' conditions. In a warming world with increased evapotranspiration there are likely to be many regions in which this is not true - especially at high latitudes, where increased rainfall is often accompanied by decreased soil moisture - as discussed by WGII in the TAR. This is of importance in that region for potential methane feedbacks from ground sources, but it is relevant everywhere for future projections of water availability. The explicit avoidance of soil moisture plots except for one figure (10.3.9) means this report can't really discuss which regions got wetter and which did not (drier is not an equal problem, for with less precip, and the likely increase in potential evapotranspiration, drier is less ambiguous). Since the models did provide soil moisture output - even if it may mean somewhat different things in different models - makes its omission in this report even less understandable. [David Rind]	Soil moisture plot and discussion of soil moisture changes is included in Fig. 10.3.9
10-47	A	0:0		There are references in this chapter to Confidence Intervals, eg "95% Confidence Interval". This is technically incorrect, as a Confidence Interval arises from randomisation of the data; it does not describe uncertainty in a parameter or a prediction.	The term confidence interval (or level) is common usage in the climate literature.

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				The correct terminology for a (Bayesian) probabilistic summary is Credible Interval. It would be better to write "95% CI" in all cases, understood to be the range from the 2.5th percentile to the 97.5th percentile. This would save space. [Jonathan Rougier]	
10-48	A	0:0		I feel Chapter 10 still has some problems in some sections and work is needed to pull the whole chapter together rather better. It is difficult to balance the mix between presentation of the scenarios and physical understanding, but it is crucial if the chapter is to be valuable. I have only managed to review some sections in detail (sorry). These are sections 10.1,10.3,10.5, Box 10.2 and question 10.2. A very brief reading of the whole chapter suggests the balance is much better than the 0th draft (reduced section on sea-level rise; variability and extremes better balanced) [Catherine Senior]	Noted.
10-49	A	0:0		Important caveats are clearly made in the text (10-35 2, and 10-42 19 and 10-45 11) yet the text repeatedly uses language that ignores and appears to override them. These assumptions/caveats are fundamental to the entire chapter, indeed the entire manuscript, and might be made clearly and definitively early on, in addition to where they currently surface in the text. [Leonard A. Smith]	Noted. Executive summary has been re-written.
10-50	A	0:0		Technical terms, in particular "PDF", are used with different meanings in different paragraphs. In chapter 10 alone, "PDF" is use to denote (i) a probability forecast of future climate (ii) a relative frequency distribution from a particular ensemble run, (iii) a subjective Bayesian density, complete with prior information, (iv) the probability distribution for a given model and a given sampling strategy. [Leonard A. Smith]	Noted. Specific use clarified where possible.
10-51	A	0:0		In most cases, phrases based on "Range of responses" (used in the titles of sections 10.5.2 and 10.5.3) are more accurate and more appropriate than "PDF", as well as better able to communicate the mathematical content of the science to a non-contributor. [Leonard A. Smith]	Noted. Both are used in the chapter where appropriate.
10-52	A	0:0		The PDF files of the papers suggested for citation are available via ftp to "ccrp.tor.ec.gc.ca" (or 199.212.19.40): Login as "anonymous"; use your email address as the password; enter "passive" (if not passive by default); change to "pub/Papers/Leona" directory by entering "cd pub/Papers/Leona".	More specific information required to act on this comment (i.e. which papers, what application, where in chapter, etc.)

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				[Xiaolan L. WANG]	
10-53	A	0:0		This chapter attempts the impossible in presenting a huge and wide-ranging set of results. Overall I felt the selection of which topics to discuss was good. Inevitably at this stage of drafting, there are places where the presentation reads like a list of disconnected results, and the user desperately wants some synthesis and interpretation. I have tried to indicate some of these areas in specific comments. If this can be tackled I think it would help usabilty and keep length under control. [Richard Wood]	Noted. But this reviewers comments were mis-numbered throughout the comments file. We have attempted to work with him to translate the line numbers and respond if possible.
10-54	A	0:0		The section on quantifying uncertainty (10.5) describes a new area where there has been great progress since thr TAR. I think it is out of poistion within the chapter, as it splits up the sections on projections of specific quantities (especially sea level change, which I think belongs adjacent to 10.3). The uncertainty section decribes methods which are (in principle at least) generically applicable to all projected quantities. So I think would fit better either as the last section of the chapter, or even straight after the introduction. [Richard Wood]	Chapter has been re-ordered, thus switching sections 10.6 and 10.7
10-55	A	0:0		General comment on the sea level part of chapter 10, I am very worried about 10.6.5 in particular. Someone reading this and just wanting a headline number will read the 130-380 mm range of rise by 2100 and take a middle value, say 250 mm. That is half the middle value of about 500 mm from the TAR for the reasons given in the text, and which may indeed be scientifically valid, but which some people will jump on as either 'a retreat by modellers in their doomsday scenarios of coastal flooding' or 'the TAR got it wrong'. They will also conclude that there is now nothing to worry about as sea level has been rising at 2 mm/year for a century so why worry if it is going to do about that for the next century, especially given the high levels of normal decadal variability. I think something is either wrong here (e.g. is 130 plausible for the 21st century given the 20th century rates discussed in chapter 5) or the explanation needs a lot more qualification (e.g. why certain emission scenarios have not been considered, reducing the higher end of the range). [Philip Woodworth]	Sea level section has been revised
10-56	A	0:0		My only other suggestion on the chapter is to put the total predicted amounts into the Exec Summary, not just the steric numbers, although they are not very different it seems. [Philip Woodworth]	Noted. Executive summary has been re-written.
10-57	A	1:0		A few bullets on ocean acidification and pH changes are needed [Fortunat Joos]	Noted. Executive summary has been re-written.
10-58	A	1:0		I'm surprised in the lack of discussion regarding drought. Drought is one of the biggest concerns of society, and it seems that the IPCC - and chapter 10 in particular - should make some clear statements about what climate change might mean for drought. I suggest	Available literature does not address future megadroughts, but we have added clarification that greater dryness

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				being more explicit - rather than dicussing "dryness" in somewhat vague terms, talk about the likelihood of drought - discuss in terms of frequencies, durations, and spatial extent. Chap 6 says some about drought, and makes it clear that changes in drought (metrics above) have occured. Can we cay anything about the future?? Perhaps not, but then yopu should say this explicitly. However, there is one thing you can say for sure - that temperature increases of the future will make droughts more severe - witness the D. Breshears et al., 2005 PNAS paper. The combination of a long (even mega) drought and warmer temperatures could have HUGE consequences. Since we know the former occured in the past - and even in the last several centuries (cite chap 6) - then it seems responsible to mention that they are likely to occur in the future. Some statements should be up in the exec summary - can't get much more relevant to society. [Jonathan Overpeck]	increases the risk of droughts
10-59	A	1:0		As I read the other chapters (started with the most important - 10 - first), I see plenty of assertions that are relevant to projections (e.g., with respect to Atlantic MOC, ice sheets, and sea level - I assume chap 10 will look closely through other chapters to ensure good coordination. Chap 8 has some interesting projections, for example. [Jonathan Overpeck]	Coordination with other chapters has been addressed
10-60	A	1:10	8:	I complement the authors for the thoroughness and objectiveness. However there are some important omissions: 1) The radiative forcing chapter does not sum the forcings; how does this chapter address the net climate change without a value for the net forcing? 2) The summary should state clearly the climate sensitivity values of recent GCMs; it is my understanding the mdoels are converging around 3 K for doubling of CO2; if so why? 3) It is my feeling there is one potential discrepancy between observed global precip trends and modeles trends. The observed global-land average precip increases from 1900 to 1950 and decreases from 1950 to at least 1995 (as per Hulme et al, 1999). The decrease during the latter periopd is clearly inconsistent with GHGs. [Veerabhadran Ramanathan]	1) Ch. 10 shows net longwave forcing; Ch. 2 is out of our purview 2) equilibrium climate sensitivity values now given in table in Ch. 8; Box 10.2 addresses most likely value directly 3) This comment applies to Ch. 9, not Ch. 10
10-61	A	3:0	6:	Structure description of model results better and bring them together in coherent paragraphs instead of listing unconnected bullet points. [Axel Michaelowa]	Noted. Executive summary has been re-written.
10-62	A	3:0	7:	These lists of bullets are very long and detailed. Dividing them up with more frequent section titles by topic rather than pre- vs. post-TAR would be helpful for readers attempting to find specific pieces of information, e.g.: temperature (means and extremes); precipitation (means and extremes); atmospheric circulation; sea level rise (steric and eustatic); atmospheric CO2 [Katharine Hayhoe]	Noted. Executive summary has been re-written.
10-63	A	3:0		It needs to be made very clear up front that the results provided in Lines 30-56 are	Noted. Executive summary has been

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				projections, not observations. "Executives" are virtually certain to mess this up. Please clarify with a more transparent Introduction. [Jerry Mahlman]	re-written.
10-64	A	3:1	7:7	Exectutive summary. This is overly long and unbalanced. There is a mix of statements about scenarios and physical understanding throughout which makes it seem very 'jumpy'. The 'headline numbers' (e.g. ranges) are scattered about (bizzarely sea-level comes before temperature) and very few uncertainty statements. In many cases the conclusions are repeated under results corroborating the TAR results and new results. The balance of statements seems wrong. For example an enormous amount about the MOC. [Catherine Senior]	Noted. Executive summary has been re-written.
10-65	A	3:1		Executive Summary. I found reading the Executive Summary tedious reading. I recommend that to limit the Executive Summary to some 30 bullets that aim more for clarity than completeness, and covers no more than about 2 pages. Chapter 5 gives an example of the type of Executive Summary I am thinking of. [Gerrit Burgers]	Noted. Executive summary has been re-written.
10-66	A	3:1		Executive summary: I like the overall structure of the ES: what is confirmed from TAR, and what is new, but find the 2 long lists of dot points overwhelming. Some are much more fundamental and important than others, some are related to each other yet there there is no overall order of importance or structure. I suggest they be placed under,say, half a dozen or so headings, collecting like points together into a structure. [Robert Colman]	Noted. Executive summary has been re-written.
10-67	A	3:1		Executive summary: suggest most 'important' (fundamental?) changes placed earlier. E.g. last dot point on carbon cycle amplification is very important and needs to go early. e.g Day length changes (p5, line 8) are trivial and should be 'low down' or dropped altogether. [Robert Colman]	Noted. Executive summary has been re-written.
10-68	A	3:1		The executive summary requires a lot more work - a long series of dot points without any form of ranking is not very helpful. [Bryant McAvaney]	Noted. Executive summary has been re-written.
10-69	A	3:1		Exectuive Summary. The readability of the executive summary could be improved by breaking up the findings into groupings based on various topics. Also there is some repetition between the list of findings consistent with TAR and the list reporting new findings that could be eliminated. [Brian O'Neill]	Noted. Executive summary has been re-written.
10-70	A	3:1		Executive Summary: The bullet points seem to have some repeats, and some strange ordering - e.g., why is the bullet on "intensity of rainfall events" not adjacent to the one on "precipitation extremes", and do these need to be two? There is more cases like this - the	Noted. Executive summary has been re-written.

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				bullets need to be consolidated. [Stefan Rahmstorf]	
10-71	A	3:1		Executive Summary - there is some jargon and abbreviations in the first couple of paragraphs such AOGCMs, EMIC, multi-model ensemble, which might make it difficult to read. Can these terms be defined in the text? [David Sexton]	Noted. Executive summary has been re-written.
10-72	A	3:3	3:3	More accurate wording would be "The climate change projections assessed …" as the chapter results we have now and not those that we will get in the future. [Michael MacCracken]	Noted. Executive summary has been re-written.
10-73	A	3:4	3:4	There are a number of cronyms that may well have been defined in previous chapters but are not yet in sufficiently common usage. What is EMIC? [Andrew Lacis]	Noted. Executive summary has been re-written.
10-74	A	3:4	3:4	As most people will read the Executive Summary only, explicite the meaning of AOGCM and EMIC [Michel Petit]	Noted. Executive summary has been re-written.
10-75	A	3:8	3:11	The evolution to multiple-member ensembles of climate model runs has produced major advances in our understanding, both physical and statistical, of the considerable power and diagnostic availability of the approach to a true quantitative, and statistical, understanding of how the climate actually works when it perturbed by anthropogenic radiative forcing. Indeed, this chapter 10 provides us with new insights into how the IPCC assessment process now gives us impressively more information than we could have possibly have known just a decade ago. [Jerry Mahlman]	Noted. Thank you.
10-76	A	3:8	:23	In comparison with other chapters, this paragraph goes into far more detail than is generally provided up front. Perhaps could lead with the main findings (bullets) and either move this section to later in the exec summary or leave in situ but condense to one or two brief statements regarding the importance of the new ensemble databases now available relative to TAR. [Katharine Hayhoe]	Noted. Executive summary has been re-written.
10-77	A	3:11	3:11	I would urge replacing "in" with "set of simulations conducted as part of" [Michael MacCracken]	Noted. Executive summary has been re-written.
10-78	A	3:12	3:12	Change "1% per year forcing" to 1% per year increase in CO2." The two statements are not the same and the underlying chpater (Pg. 8, line 10) indicates that 1% per year CO2 is what was actually used. [Lenny Bernstein]	Noted. Executive summary has been re-written.

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10-79	A	3:12	3:12	A sentence needs to be inserted about the unrealism of the 1%/year forcing. Quoting Covey (2003) from the Global and Planetary Change paper describing the CMIP2 results: "The rate of radiative forcing increase implied by 1% per year increasing CO2 is nearly a factor of two greater than the actual anthropogenic forcing in recent decades, even if the non-CO2 greenhouse gases are added in….Thus the CMIP2 increasing-CO2 scenario cannot be considered realistic…It is also not a good estimate of future anthropogenic climate forcing, except perhaps as an extreme case…" Change the text to: "While the 1%/year forcing gives the models a common and comparable base, observed emissions trends indicate that this likely results in substantial overestimation of climate change, at least through 2050." [Jeffrey Kueter]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-80	A	3:12	3:12	I would urge replacing "idealized" with "these simulations consist of idealized" to give a better sense to the less familiar reader. [Michael MacCracken]	Noted. Executive summary has been re-written.
10-81	A	3:12		1% per year radiative forcing is nonsensical. When will the models deal with plausible possibilities? [Vincent Gray]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-82	A	3:14	3:14	Replace "This presented" by "This set of simulations presented" [Michael MacCracken]	Noted. Executive summary has been re-written.
10-83	A	3:16	3:16	Replace "The second" with "The second set of simulations" [Michael MacCracken]	Noted. Executive summary has been re-written.
10-84	A	3:18	3:21	The Special Report on Emission Scenarios (Pg. 62) carefully stated that scenarios are neither predictions nor forecasts of the future. The report also said that it could not assign probabilities to the likelihood that one or another of its scenarios would occur. These caveats also apply to model projections based on the SRES scenarios and should be included, either in the text or in a footnote. [Lenny Bernstein]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-85	A	3:18	3:21	IPCC is always careful to state that scenarios are not predictions or forecasts of the future (See SRES, Pg. 62). The same is true of climate model proejctions that use SRES scenarios as input. The Executive Summary should remind readers of this fact, either in the text or in a footnote. [Jeffrey Kueter]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-86	A	3:18	3:18	Same as above. What is SRES?	Noted. Executive summary has been

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				[Andrew Lacis]	re-written.
10-87	A	3:18	3:18	Replace "For" with "In simulating" [Michael MacCracken]	Noted. Executive summary has been re-written.
10-88	A	3:18	3:23	Too much detail here. Just say that many integrations performed and archived in AR4 database. This chapter focuses on the analysis of the SRES scenario and 1% CO2 increase integrations. [Ronald Stouffer]	Noted. Executive summary has been re-written.
10-89	A	3:19	3:19	Replace "have been" by "have also been" to indicate that these are additional simulations. And then put a colon after "run" [Michael MacCracken]	Noted. Executive summary has been re-written.
10-90	A	3:19	3:21	"for A1B and B1 scenarios, respectively, for another 100 to 200 years with a vely long integration for 350 years to project change of MOC using the Earth Simulator." is a very importnt and attractive information in AR4. [Koki Maruyama]	Noted. Executive summary has been re-written.
10-91	A	3:22	3:23	The term climate commitment is used in different ways throughout this chapter, some of which are inconsistent with its common use in the literature. Suggest a more careful use of this term along with a clear definition, or perhaps better, suggest that the term be avoided as it really is valid only for a hypothetical case (or cases in the multiple uses in the chapter). Furthermore, given the relative importance of aerosol forcing compared with the currently more limited ocean heat uptake, it may be that the focus should be more (or perhaps equally) on forcing offset from aerosols rather than the oceans. And it would be instructive to add some historical perspective on perceived forcing offset from oceans and aerosols. Apparently, the magnitude of the ocean offset has declined (for example with emergence of ocean heat content data), and aerosol offset has increased with a broader understanding of its potential effects and its role in balancing the radiative forcing budget. [Haroon Kheshgi]	Definition of climate change commitment used in this chapter now consistent with that given in the executive summary
10-92	A	3:22	3:23	I would change this to read "commitment can be assessed in much wider scope and in greater detail than …" [Michael MacCracken]	Definition of climate change commitment used in this chapter now consistent with that given in the executive summary
10-93	A	3:22	3:23	Even if it has been defined in the TAR (p 24-38; p 534), the notion of "climate change commitment" is perhaps unclear for the reader (in particular in an executive summary); a brief definition or a reference to a definition (to 10.7) might be added. [Serge PLANTON]	Definition of climate change commitment used in this chapter now consistent with that given in the executive summary
10-94	A	3:25		Define EMIC, please.	Agreed. Change made.

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				[Jerry Mahlman]	
10-95	A	3:30	3:56	Many of these model-based results need some specific assessment on why these results are expected to be true, with some sense of the confidence why, or why not, they might be expected to be true in the real world, whether they are expected to be "likely", "very likely", or "virtually certain" to be true. This would provide a better connection with the previous chapters. [Jerry Mahlman]	Assessment of likelihood of a climate change projection is difficult, and is related more to consistency of a model projection as discussed in the chapter.
10-96	A	3:30	3:56	It is not readily clear that these"findings" are observationally based, or strictly products from models. This unnecessary confusion needs to be clarified. [Jerry Mahlman]	These are projections from models, not observations.
10-97	A	3:30		If these findings are based on the assumption of a 1% increase in forcing a year, then they are nonsense [Vincent Gray]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-98	A	3:30		While I understand that the guidance for the Executive Summary was apparently to summarize the TAR results and then present the new results, this leads to a very hodge-podge approach to presenting the findings, with many topics spread over the several pages. I would very much urge going to an ordering that is based on a logical sequencing through the key variables (e.g., surface temperature) and then giving the TAR result and the new result in close association, etc. In addition, in doing this it is important that each point really focus on its key variable--a number of these points cover several issues, again leading to particular topics being quite dispersed. Having sub-headings for each of the variables covered would also be useful. [Michael MacCracken]	Noted. Executive summary has been re-written.
10-99	A	3:30		Amazingly, in summarizing the TAR results, there is no mention made of what the temperature projections were. This needs to be done so they can be compared to the new estimates. [Michael MacCracken]	Noted. Executive summary has been re-written.
10-100	A	3:30		For this list of conclusions, it should be made clear whether they apply to all scenarios (SRES and stabilization) or just to some (e.g. SRES only), and also should be clear what time period they apply to since the scenarios cover one or more centuries. [Brian O'Neill]	Noted. Executive summary has been re-written.
10-101	A	3:31	3:57	These results generally agrees with expectation to date from the available observations. However, these model-based projections need to be placed in a meaningful context so that the FAR reader has some context as to what these projections really mean, and for what period of the future these projections being made for. 2050? 2100? Other than the sea	Noted. Executive summary has been re-written.

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				level projections, with their very generous error bars, almost none of the "findings" of lines 30-56 are very well grounded in time, thus confusing the target "executive" seriously. Please repair this and clarify. Essentially, you are "painting" a picture of a future earth for an unspecified era in the 21st Century? I am guessing here. That's not a good sign, because it suggests that more work needs to be done in the Executive Summary. Indeed, consider the plight of our poor candidate "executive" trying to understand what he/she is supposed to be learning from this. [Jerry Mahlman]	
10-102	A	3:31	3:31	Give an order of magnitude to qualify "greatest", e.g. by adding after "northern latitudes" ", about three times larger than the global mean" or refer to the relevant figure [Michel Petit]	Noted. Executive summary has been re-written.
10-103	A	3:32	3:34	The issue of the MOC and its effects is covered in a lot of different points. I would treat it as a separate sub-bullet under temperature, and not mix it in with this point. [Michael MacCracken]	Noted. Executive summary has been re-written.
10-104	A	3:32	3:34	There is some redundancy with p4 lines 12-13; the specific comment on meridional circulation might be suppressed there. [Serge PLANTON]	Noted. Executive summary has been re-written.
10-105	A	3:32	3:32	I think it is a good idea to identify what results corroborate TAR findings and what is new. However, by splitting the bullet points in this way, information on specific topics is spread over two or more places (e.g. to find out about MOC changes one needs to read p3 ll 35-37, p4 ll 6-7, p4 ll 12-24 and p 6 ll 20-31). I also found the format of a succession of unlabelled bullet points hard to navigate. I suggest a more user-friendly format would be to group the bullets by topic (MOC, extremes, cryosphere etc.), and to mark somehow the results that corroborate the TAR (e.g. by an asterisk, a different bullet mark or italic font). [Richard Wood]	Noted. Executive summary has been re-written.
10-106	A	3:34		– overwhelming – This is a passion word. Rephase [Ronald Stouffer]	Noted. Executive summary has been re-written.
10-107	A	3:35	3:37	This point indicates that precipitation increases in the "monsoon regimes" but elsewhere in the assessment (including in this chapter) it is said that the monsoons, at least some of them, are diminished (this point is not clear if the decreases in the subtropics and some midlatitudes refer to the monsoon regimes). If indeed there are decreases in the monsoons in some regions, the physics behind this needs to be indicated, as having the land warm faster than the oceans would seem likely to lead to an increase in monsoons given how monsoons are usually explained to people. [Michael MacCracken]	Noted. Executive summary has been re-written.
10-108	A	3:35	3:37	Give an order of magnitude to qualify the preciptation increases and decreases: a few percents ?, or refer to the relevant figure	Noted. Executive summary has been re-written.

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				[Michel Petit]	
10-109	A	3:36	3:36	Executive Summary. Introduce acronym "(MOC)" after "meridional overturning circulation". [Valentin Meleshko]	Noted. Executive summary has been re-written.
10-110	A	3:36	3:37	general decreases … and some mid-latitude areas – Just say mixed signal in mid-latitude areas. [Ronald Stouffer]	Noted. Executive summary has been re-written.
10-111	A	3:38	3:40	Either this statement or line 51 at page 4 should be omitted, because now the same result is presented both as a TAR result and a new result. [Gerrit Burgers]	Noted. Executive summary has been re-written.
10-112	A	3:38	3:40	The statement about expansion of the Hadley Circulation and poleward shift of storm tracks is here listed as a finding that corroborates results from the TAR but on the next page (10-4, line 51) it is also listed as a new result since the TAR [Garry CLARKE]	Noted. Executive summary has been re-written.
10-113	A	3:38	3:40	Give orders of magnitude for the pressure changes, Hadley cells expansion and shift of storms tracks [Michel Petit]	Noted. Executive summary has been re-written.
10-114	A	3:38	3:40	Mention AO and AAO here? [Ronald Stouffer]	Noted. Executive summary has been re-written.
10-115	A	3:40		The ocean changes need to be better worded. The zonal mean SST increases with a lack of warming in N Atlantic. Heat anomaly penetrates to depth in high latitudes. [Ronald Stouffer]	Noted. Executive summary has been re-written.
10-116	A	3:45	3:45	I would replace "mixing" with "vertical mixing" or something similar to explain what is meant (I.e., the closer connection to the cold, deep ocean). [Michael MacCracken]	Noted. Executive summary has been re-written.
10-117	A	3:46	3:49	These points are examples of ones for which there is not sufficient context. First, I would think the 20th century change and then the projected results of the TAR would be given (for total sea level rise) and then its components would be discussed, but here we have a very partial answer to the sea level impact (e.g., ice sheet effects are left out). On line 48, replace "rises" by "is projected to rise". [Michael MacCracken]	Noted. Executive summary has been re-written.
10-118	A	3:46	3:46	Would it not be fair to mention the opposite effect of precIpitations increase ? See page 6, line 12-14. An alternative would be to add some words such as "(see page 6, lines 12-14 for new information) [Michel Petit]	Noted. Executive summary has been re-written.

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10-119	A	3:48	3:49	Because the numbers are different fromt those of the TAR, this statement should be moved to the "post-TAR" section. An alternative is to use a formulation like "0.13-0.34 is compatible with the range established in the TAR" [Gerrit Burgers]	Noted. Executive summary has been re-written.
10-120	A	3:48	3:49	Is this result identical to that quoted in the TAR? The quantitative nature of the comment sits uncomfortably with the qualitative nature of the other findings reported in this section. [Matthew Collins]	Noted. Executive summary has been re-written
10-121	A	3:48	3:48	Mentioning only thermal expansion is misleading, given the totality of sea level response now expected. Delete this bullet and add the following: "—Because of observed emissions trends and new projections concerning high-latitude ice sheets, estimates of median sea level rise by 2100 have been cut by nearly 50%. While the TAR range was 90-880mm, the new figure is 130-380mm." [Jeffrey Kueter]	Noted. Executive summary has been re-written.
10-122	A	3:48	3:49	"0.13-0.34m for B1 and A1B scenarios, respectively by AOGCMs" may be the exact description. [Koki Maruyama]	Noted. Executive summary has been re-written.
10-123	A	3:48	3:49	I thinkit would be appropriate to extend the comment with and melt of small glaciers and ice caps. If you don't do this it is in contradiction with the previous bullet [Roderik S.W. Van de Wal]	Noted. Executive summary has been re-written.
10-124	A	3:48		this bullet should say that thermal expansion is not the full story [Stefan Rahmstorf]	Noted. Executive summary has been re-written.
10-125	A	3:49		"This range does not represent all modelling and scenario uncertainties." is too vague. Suggest at least giving a hint about what these additional uncertainties do to the assessed range. [Jonathan Overpeck]	Noted. Executive summary has been re-written.
10-126	A	3:50	3:50	Precipitation previously treated in page 3, lines 35-37. Water vapor increase, where ? [Michel Petit]	Noted. Executive summary has been re-written.
10-127	A	3:51	3:51	This statement seems to suggest that the intensity of all rainfall events will increase. I don't think this is really what is meant (is it the average intensity?). [Matthew Collins]	Noted. Executive summary has been re-written.
10-128	A	3:51	3:51	Might be merged with p4 line 5. [Serge PLANTON]	Noted. Executive summary has been re-written.

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10-129	A	3:52	3:52	Delete the first "summer" [FILIPPO GIORGI]	Noted. Executive summary has been re-written.
10-130	A	3:52	3:52	It is very hard for the user to interpret such a sentence – how is he/she to interpret the quoted range? Even (especially?) in the ES, I think the meaning of the range needs to be made clear. [Richard Wood]	Noted. Executive summary has been re-written.
10-131	A	3:54	3:56	Replace "El Nino-like response" by "El Nino-like SST response" because later on section 10.3.5 a distinction is made between the SST response and the "ENSOness" which encompasses not only SST but MSLP and precipitation as well. [Gerrit Burgers]	Noted. Executive summary has been re-written.
10-132	A	3:54	3:56	It is not true that the majority of models show a mean El-Nino response. There is still a wide range of responses from coupled models depending on what collection of models is considered and what analysis technique is used. [Matthew Collins]	Noted. Executive summary has been re-written.
10-133	A	3:54	3:54	I think this bullet point needs to be clarified. Does it refer to extreme rainfall events? [Richard Wood]	Noted. Executive summary has been re-written.
10-134	A	3:54		isn't this the same (recently developing) pattern as highlighed in Hoerling and Kumar's "Perfect Ocean for Drought" paper (Science, 2003), and if so, shouldn't this be mentioned at least in the text? I.e., that likely tropical Pacific change could be more conducive to drought. Note that I don't think coupled models can get the current SST patterns (e.g., the perfect ocean) right enough to get the current/recent Western US drought, whereas perscribed SST's DO get the drought (a point of the H and K 03 paper). This suggests that the possibility of more drought should be mentioned even if the coupled climate models don't indicate this - they can't indicate what they can't simulate. [Jonathan Overpeck]	The Hoerling result is for a La Nina-like SST pattern for drought, not an El Nino-like pattern.
10-135	A	3:54		Add "pattern in the" after "El Nino-like". [Ronald Stouffer]	Noted. Executive summary has been re-written.
10-136	A	3:56	3:56	Is this "eastward shift of precipitation" just over the tropical Pacific or what--it is not really stated very clearly. [Michael MacCracken]	Noted. Executive summary has been re-written.
10-137	A	4:1	4:2	It is very hard to conceive of any model-based or observaion-based case where this statement would not be true. I suggest that it be deleted. MOC can be either atmosphere or ocean, but here it is not defined at all. [Jerry Mahlman]	Noted. Executive summary has been re-written.

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10-138	A	4:3	4:3	Same as above. What is MOC? [Andrew Lacis]	Noted. Executive summary has been re-written.
10-139	A	4:3	4:4	Better moved after line 15 [Michel Petit]	Noted. Executive summary has been re-written.
10-140	A	4:3	4:4	This point might be placed after the first one concerning meridional circulation (same page, lines 9-11). [Serge PLANTON]	Noted. Executive summary has been re-written.
10-141	A	4:5	4:5	A qualification of the term "most areas" would be helpful (does it refer to the global average). I some areas, models suggest that extreme precipitation can increase at a rate smaller than the increase in the mean. Also the statement seems to imply that precipitation will go up in most areas. In some regions precipitation is projected to decrease. [Matthew Collins]	Noted. Executive summary has been re-written.
10-142	A	4:5	4:5	See comment n 3. [Serge PLANTON]	Noted. Executive summary has been re-written.
10-143	A	4:6	3:6	For clarity, reword the opening few words to be "Sea ice extent and thickness decrease through the course of the 21st century, [Michael MacCracken]	Noted. Executive summary has been re-written.
10-144	A	4:8	4:8	Executive Summary. Statement on precipitation extremes is rather strong and at the same time vague. Whether such increase relates to "most" or to "some" areas and what about areas where precipitation decreases? It should be reformulated in more specific terms. [Valentin Meleshko]	Noted. Executive summary has been re-written.
10-145	A	4:8		"sea ice to become seasonal": would be better to say "to disappear in summer", this makes it more understandable for the general reader [Stefan Rahmstorf]	Noted. Executive summary has been re-written.
10-146	A	4:9	4:9	Suggest insertion of "reasonably" between "models" and "consistent" in l10. In fact, no models are fully consistent with all observations (model error is significant) and some expert judgment is inevitably required to choose what level of verisimilitude is required in order for a model to be credible. [James Annan]	Noted. Executive summary has been re-written.
10-147	A	4:9	4:11	Many would find the concept of a 0% reduction confusing. The wording might more clearly be "…project a range of no change to a 60% reduction..". [Matthew Collins]	Noted. Executive summary has been re-written.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-148	A	4:9	:11	Over what period- to 2100 , 2300 ? [John Mitchell]	Noted. Executive summary has been re-written.
10-149	A	4:9		Add "significant" before :increase in the MOC". [Ronald Stouffer]	Noted. Executive summary has been re-written.
10-150	A	4:11	4:11	It would be useful to give a time frame for this statement and to add the qualifier that these model simulations assume a stabilization of CO2 [Klaus Keller]	Noted. Executive summary has been re-written.
10-151	A	4:12	4:13	This is already stated on page 3, lines 32-34. [Matthew Collins]	Noted. Executive summary has been re-written.
10-152	A	4:12	4:13	This point was already made on page 3, lines 32-34. Again, the points need to be ordered in a more careful and rational way. [Michael MacCracken]	Noted. Executive summary has been re-written.
10-153	A	4:12	4:13	This is a nice result, even if it were projected to be so about 20 years ago. We never did expect a "collapse" of the MOC, unless you give it a century or so. [Jerry Mahlman]	Noted. Executive summary has been re-written.
10-154	A	4:12	4:13	See comment n 2. [Serge PLANTON]	Noted. Executive summary has been re-written.
10-155	A	4:13		overwhelming - This is a passion word. Rephase. [Ronald Stouffer]	Noted. Executive summary has been re-written.
10-156	A	4:15		Role of surface fluxes unclear. What does this imply? [Ronald Stouffer]	Noted. Executive summary has been re-written.
10-157	A	4:16	4:17	The parenthetical statement suggests that if models were to include an interactive ice-sheet, then the melt-water could induce a permanent MOC shut-down. I do not know of any study which suggests Greenland could melt sufficiently quickly to produce the large melt-water pulse required. Indeed, the one study which has gone some way in this direction (Ridley et al. 2005) suggests a negligible impact in the 21st Century. [Matthew Collins]	Noted. Executive summary has been re-written
10-158	A	4:16	4:17	As expected, but nice to know that it is not so easy to generate an MOC collapse. [Jerry Mahlman]	Noted. Executive summary has been re-written.
10-159	A	4:16		instead "none have interactive ice sheets" say: "none include the effect of meltwater from ice sheets" (this is more understandable - also, you could include this effect in other ways	Noted. Executive summary has been re-written.

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				than an interactive ice sheet model) [Stefan Rahmstorf]	
10-160	A	4:17		"(though none have interactive ice sheets)" So? Can't we say more? Is it unlikely that that ice sheet melting in the next 1000 years will be a real player in this regard? Maybe not. [Jonathan Overpeck]	Noted. Executive summary has been re-written.
10-161	A	4:18	4:18	"shut down" is better as two words. [James Annan]	Noted. Executive summary has been re-written.
10-162	A	4:18	4:21	This is a valuable result, simply because it puts a lid on the climate exaggerators who, without justification, scream about "sudden collapse" of the MOC on very short time scales, thus asserting that this is a likely "extreme event". [Jerry Mahlman]	Noted. Executive summary has been re-written.
10-163	A	4:18		in this post-2100 bullet, might want to mention that ice sheet melting could be a influence worth worrying about [Jonathan Overpeck]	Noted. Executive summary has been re-written.
10-164	A	4:20	4:20	I think the significance of the lack of ice sheet models needs to be explained here – it would likely be lost on many non-specialist readers. [Richard Wood]	Noted. Executive summary has been re-written.
10-165	A	4:22	4:26	This statement indicates that the models that do the best job of simulating ENSO show an increase in interannual variability. It does not correspond to the statement in the underlying chapter (Pg. 24, line 49-51) that states that the 6 models that showed the most realistic simulations of ENSO showed no statistically significant changes in the amplitude of ENSO variability in the future. The statement in the Executive Summary should be changed to reflect the underlying text. [Lenny Bernstein]	Noted. Executive summary has been re-written.
10-166	A	4:22	4:26	This could be phrased more positively as "The changes in ENSO amplitude in the 21th century in the most realistic models are of the same magnitude as the observed and modeled variability of ENSO over the last century [Gerrit Burgers]	Noted. Executive summary has been re-written.
10-167	A	4:22	4:22	This does not adequately summarize the text from page 10-25. Just quote directly from the text and say no more. It is not a good idea for the authors to tout specific models, as was done here. Instead, "With regard to ENSO, there is a wide range of behavior among the current models with no clear indication regarding possible changes of future El Nino amplitude or period". [Jeffrey Kueter]	Noted. Executive summary has been re-written.
10-168	A	4:22	4:24	While I agree this is likely, is this statement supported by the chapter text and the literature? It should be stated clearly that the net warming would be relative to	Noted. Executive summary has been re-written.

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				preindustrial conditions (assuming that is what is meant). I also think that the focus on temperature is in danger of missing the point. Impacts on, e.g. North Atlantic sea level and hydrological variables such as subtropical summer drying would add to the already-expected effects from the radiative forcing (see Vellinga, M. and R.A. Wood, 2005: Impacts of thermohaline circulation shutdown in the twenty-first century. Climatic Change (submitted – decision expected soon, copy will be sent to Thomas Stocker). [Richard Wood]	
10-169	A	4:23	4:23	Is the assesment of present day ENSO characteristics based on something from Chapter 8? [Catherine Senior]	Clarify—these results are from papers that assessed ENSO characteristics related directly to how future changes are simulated.
10-170	A	4:25	4:26	Need to change "what the actual possible changes could be" to "what the actual changes will be" as there will be changes and it is the assessment of these that is precluded. [Michael MacCracken]	Noted. Executive summary has been re-written.
10-171	A	4:27	4:27	This point does not belong in chapter 10 (covered in ch 8) and should be removed [Robert Colman]	Noted. Executive summary has been re-written.
10-172	A	4:27	4:27	If a comparison is going to be made, the other sources of uncertainty also need to be listed for it is not clear what is meant here--what other factors are being considered. Does this mean that cloud feedback is larger than emissions scenario uncertainty or just within the set of Earth system processes, or what. When this says "largest" need to say larger than what? [Michael MacCracken]	Noted. Executive summary has been re-written.
10-173	A	4:27	4:27	I totally agreee that "cloud-radiative feedback" is our biggest uncertainty that is of quantitative significance. I am not so sure that the use of the shorter term "cloud feedback" is even scienfically correct. [Jerry Mahlman]	Noted. Executive summary has been re-written.
10-174	A	4:27	4:27	Check for redundance and consistency with chapter 8, page 3, line 30-31, under the heading "Highlights since the TAR include", "Clouds feedbacks have been confirmed as a primary source of inter-model differences, with tropical low cloud the largest contributor" [Michel Petit]	Noted. Executive summary has been re-written.
10-175	A	4:27		Can we identify low cloud changes as the main problem? [Ronald Stouffer]	Noted. Executive summary has been re-written.
10-176	A	4:28	4:30	This is hardly a new conclusion. It has been known for roughly a decade. [Jerry Mahlman]	Rejected The first coupled simulations of this kind appeared in 2000 and 2001.

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10-177	A	4:28	4:36	These lines are much more precise than the vague statement in chapter 8, page 3, lines 48-50. [Michel Petit]	Accepted
10-178	A	4:30	4:30	A more detailed discussion of this is presented in Chapter 8 and its ES (see p8-3 ll 29-31 and section 8.6). To save space this could be omitted from Ch 10. [Richard Wood]	Rejected Chapter 8 is about model evaluation. Here we estimate the impact of the climate-carbon cycle feedback on future projected climate.
10-179	A	4:31	4:33	The error bar from the TAR on the CO2 projections should be given here as well for traceability [Fortunat Joos]	Rejected It will be given in the body of the chapter, not in the Executive summary.
10-180	A	4:31	4:33	I think the sentence would be clearer if the phrase "the SRES A2 … models" was moved to just after "By 2100" [Michael MacCracken]	Accepted
10-181	A	4:31		This point would be much clearer if it started off: "For the SRES-A2 emission scenario…" [Brian Hoskins]	Accepted
10-182	A	4:34	4:36	The uncertainty in e.g. TCR suggests that the 0.7degC extra warming quoted should really be expressed as a range or a probability. [Matthew Collins]	Noted
10-183	A	4:34	4:36	This seems quite reasonable to me. [Jerry Mahlman]	Noted
10-184	A	4:34	4:36	Do you mean something like: "Coupled climate-carbon cycle models suggest CO2 concentrations in the range 730-1020 ppm, for SRES-A2 emissions. This compares with the standard value of 830 ppm used in the AR4 models without an interactive carbon cycle, and provides an indication of the uncertainty due to omission of climate - carbon cycle feedbacks from the standard runs"? Does the 730 ppm value (less than 830) plus the fact that that model has a positive climate-carbon cycle feedback, imply that the present day airborne fraction simulated by that model is too small? Presumably the Bern model used to produce the standard SRES concentrations is tuned to get the present day airborne fraction right, but has no climate-carbon feedbacks. So I would conclude that 730 ppm in 2100 is not possible. As a non-specialist in this area, I found these results confusing, and would welcome more interpretation/synthesis. [Richard Wood]	Taken into account The standard value of 830 ppm is from the "reference" estimate from the BERN-CC model. This estimate accounts for a "reference" positive climate-carbon cycle feedback. Therefore, it is possible for a C4MIP model to simulate a lower CO2 than the "reference" used by the AR4 models.
10-185	A	4:36		Add "where the CO2 concentration is prescribed" at end of sentence. [Ronald Stouffer]	Accepted

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10-186	A	4:38		Just to reiterate, the points included in this listing need to be integrated with those above and ordered in a way that is coherent and logical. [Michael MacCracken]	Noted. Executive summary has been re-written.
10-187	A	4:38		New results section in executive summary: this is a very comprehensive and therefore long list of new results. One way to make it easier for people to locate specific information in this long list would be to divide the list into subsections with subheadings that corresponded to section titles in the rest of the chapter. [David Sexton]	Noted. Executive summary has been re-written.
10-188	A	4:39	4:42	This, along with the further discussion in the main text, will be interpreted as a confident prediction of warming at an accelerated rate of ~0.25C/decade in the near future. This does not seem likely given the evidence available. The 51-year interval used in this analysis (it would be helpful to include the dates in the text here) includes a substantial historical period during which the measured warming rate has been about 0.17C/decade. In order to reach even your lower figure of 0.21C/decade over the stated interval, therefore, a significant and immediate increase in the actual warming rate to about 0.25C/decade would be required from now until 2030. As far as I am aware, no model suggests anything like this, and I suspect that the models with high rates of warming in this analysis probably also overestimate the recent warming somewhat (could this comparison back to 1980 be shown on the related figure?). Although it is not easy to measure by eye, there does not appear to be any sign of significant acceleration in the model outputs over the 2000-2030 interval which is included in the related figure.Do you really mean to contradict the wording of the TAR so strongly, viz that models predict continued warming at close to the current observationally-determined rate, which is close to 0.17C/decade (depending on the precise interval chosen)? I realise that the "assessment" takes place in another chapter, but it would be very helpful if this forecast could at least be given the context of the models' recent trends. [James Annan]	Noted. Executive summary has been re-written. Chapter 3 covers recent warming (last three decades) that shows projected warming for next two decades is roughly consistent with the rate observed over that time period, with the caveat that we are only considering anthropogenic forcing.
10-189	A	4:39	4:42	The model analyses quoted result in an incredibly small range of 0.06C. These analyses only consider the anthropogenic component, however, and the actual range of a forecast will be much wider. Suggest considering all sources that contribute to a forecast range, rather than the hypothetical case for the model intercomparison. For example, Kheshgi and Jain (GBC, 2003, vol.17, 1047, doi:10.1029/2001GB001842) find a much wider range in 2020 than 0.06. This is also apparent in figure 10.5.6. If there is interest in describing the range, suggest including all contributors to the range (e.g. scenarios, sensitivity, natural effects, past matching of model results to temperature history). Perhaps the point that is being made is that of the range of around 0.5C that is seen in model results in 2020, differences in SRES emission scenarios account for a very minor	Noted. Executive summary has been re-written.

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				portion (0.06C). Of course SRES does not consider mitigation, or a broader range of aerosols scenarios, where effects from aerosol emissions may alter temperature by more than 0.06C by 2020. [Haroon Kheshgi]	
10-190	A	4:39	4:42	What period does "early 21st century" refer to? In what way do these conclusions account for how natural variability may affect observed warming over the next few decades (the text makes it sound like we know very precisely how much warming will be observed over the next 10-20 years)? Also, when does the next decade or two begin? 2000, or the publication date of AR4, or some other point? [Brian O'Neill]	Noted. Executive summary has been re-written.
10-191	A	4:39	4:50	The warming values are very precise when the corresponding periods are relatively imprecise. I suggest identifying more clearly these periods. [Serge PLANTON]	Noted. Executive summary has been re-written.
10-192	A	4:39	4:42	Please include the reference period for the warming anomalies. [David Sexton]	Noted. Executive summary has been re-written.
10-193	A	4:39	:41	Not really new- seen in the TAR but the spread over the first few decades is even smaller here. [John Mitchell]	Noted. Executive summary has been re-written.
10-194	A	4:40		What is the year for the values quoted on this line? Also the base period from which warming is measured in this and the next bullet needs to be given explicitly. Is it the same as used in Fig 10.5.17? It would not take many words to clarify this. Also if I am understanding the times and baseline correctly then I infer a central estimate for warming in the next few decades of 0.19C/decade. This is at the very upper end of the range given in the TAR (SPM cited 0.1 to 0.2C/decade). That seems to be a real shift in the new model results and would be worth commenting on explicitly. [Martin Manning]	Noted. Executive summary has been re-written.
10-195	A	4:40		The warming figures of 0.64 to 0.70 C should indicate warming relative to what. [Brian O'Neill]	Noted. Executive summary has been re-written.
10-196	A	4:41	4:41	Change "is similar" to "is projected to be similar" to indicate that this is a projection and not a fact. Also, the phrase "next decade or two" seems quite loose--maybe say over the next few decades if it is intended to be vague. [Michael MacCracken]	Noted. Executive summary has been re-written.
10-197	A	4:43	4:50	Dates for these periods would be useful - eg parentheses after the English: "By mid-century (2046-2065)..." [James Annan]	Noted. Executive summary has been re-written.

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10-198	A	4:43	4:49	Although "committed warming" is now an old concept, its use here provides a valuable tutorial for the policy community. I am not so sure that I like the use of the MOC acronym, since the atmosphere also has its Lagrangian Mean Circulation that is probably more valuable as a diagnostic of the atmosphere's overturning circulation in mid and higher latitudes. [Jerry Mahlman]	Noted. Executive summary has been re-written. MOC is common usage for the ocean in the climate change literature
10-199	A	4:43	4:50	Presumably "mid-century" means in 2050? What does "late century" mean? In particular, the TAR results showed a 1.4 - 5.8 C increase driven by SRES, so the 1.78-3.05 range for late century reported here may seem to be a strong revision of the earlier result unless it is pointed out how it differs from that previous result. [Brian O'Neill]	Noted. Executive summary has been re-written.
10-200	A	4:43	4:50	Likelihood definitions are confusing here, would be better to make clear what the terms mean for this chapter somewhere up front, or to refer readers of the chapter to elsewhere in the report where this is done. [Brian O'Neill]	Noted. Executive summary has been re-written. Likelihood statements are standard for IPCC and defined elsewhere.
10-201	A	4:43	4:50	This type of statement is new and important for policymakers. "very good" [Tatsushi Tokioka]	Noted. Thank you.
10-202	A	4:44	4:44	What does "for early century" mean--which century, and over what period? [Michael MacCracken]	Noted. Executive summary has been re-written.
10-203	A	4:44		First from the wording it is unclear whether the 0.31 refers to the commitment or to the total warming. Second I suggest being more definite than saying "early century" here - a warming value for 2020 or 2030 would make the statement more focused. [Martin Manning]	Noted. Executive summary has been re-written.
10-204	A	4:44		replace "for early century" with some real date? Ditto for "mid-century" etc. [Jonathan Overpeck]	Noted. Executive summary has been re-written.
10-205	A	4:45	4:45	The numbers here don't add up. "...range of 0.31oC from 1.30oC to 1.73oC,.. ". Either the range is incorrect or one/both of the limits. Please correct this. [Gareth S. Jones]	Noted. Executive summary has been re-written to correct this typo..
10-206	A	4:45	4:45	These numbers do not add up. 1.73-1.30 does not equal 0.31. [Jeffrey Kueter]	Noted. Executive summary has been re-written to correct this typo..
10-207	A	4:45	4:45	The numbers given are inconsistent with each other. [Peter Stone]	Noted. Executive summary has been re-written to correct this typo..

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10-208	A	4:45	5:11	This is a very nice exposition of scenario spread vs commitment and scenario spread vs model uncertainty. A follow-on question is whether the choice of scenario now leads to different amounts of commitment in, say, 2030, even though the global warming at that time is similar for all scenarios. I guess that question cannot be answered directly from the runs available, but if any comment can be made (even just to say that we can't answer that question) I imagine it would be useful to the policy community. [Richard Wood]	Noted. Executive summary has been re-written, but this question cannot be addressed from the literature at this time.
10-209	A	4:45		something wrong here the range 0.31 does not span 1.30 to 1.73? [Martin Manning]	Noted. Executive summary has been re-written to correct this typo..
10-210	A	4:47	4:47	For clarity, change "for which" to "depending on which emissions" [Michael MacCracken]	Noted. Executive summary has been re-written
10-211	A	4:51	4:51	The statement about expansion of the Hadley Circulation and poleward shift of storm tracks is here listed as a new result since the TAR but is previously listed as a finding that corroborates results from the TAR (see page 10-3, line 38) [Garry CLARKE]	Noted. Executive summary has been re-written
10-212	A	4:51	4:51	This is already stated on page 3, lines 35-37. [Matthew Collins]	Noted. Executive summary has been re-written
10-213	A	4:51	4:54	Please check whether results are really new, quite a few are duplicated from the results corrobarating the TAR. For instance, the expansion of the Hadley Cell and the slowdown of the MOC is mentioned as an old and a new result. [Wilco Hazeleger]	Noted. Executive summary has been re-written
10-214	A	4:51	4:51	Already mentionned page 3, line 39. To be quantified [Michel Petit]	Noted. Executive summary has been re-written
10-215	A	4:51	4:51	This is already mentioned p3 lines 38-40. [Serge PLANTON]	Noted. Executive summary has been re-written
10-216	A	4:51	4:54	lines 51-54 are a repeat of lines 12-13. [Ronald Stouffer]	Noted. Executive summary has been re-written
10-217	A	4:51	4:51	A summary of changes in Hadley circulation is summarized here, and only the expansion of the Hadley circulation is mentioned. As is described in 10.3.2.4(page20), mean intensity of the Hadley circulation weakens as shown by Tanaka et al(2005) and Yamaguchi and Noda(2005) due mainly to the increase in the static stability in low latitude troposphere. This weakening should also be mentioned here as one of very basic changes in atmospheric circulations. See the comment #4 also. [Tatsushi Tokioka]	Noted. Executive summary has been re-written
10-218	A	4:51	4:51	I didn't understand "… a range of 0.31 from 1.30 to 1.73 ..." Misprint?	Noted. Executive summary has been

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Richard Wood]	re-written to correct this typo.
10-219	A	4:51		The new result indicated in this line is already listed in page 10-3, lines 39 and 40. [PATRICIO ACEITUNO]	Noted. Executive summary has been re-written
10-220	A	4:51		Expands where ? Poleward in the summer hemisohere?, northward, southward? I am not sure what this means [John Mitchell]	Noted. Executive summary has been re-written
10-221	A	4:52	4:54	The new result described in these lines is exactly the same as indicated in the previous page (page 3), in lines 32 to 34. [PATRICIO ACEITUNO]	Noted. Executive summary has been re-written
10-222	A	4:52	4:54	This finding has been presented page 3 as a «finding that corroborates the results from the TAR »; therefore it should probably be removed from the list of « new findings ». [Sandrine Bony]	Noted. Executive summary has been re-written
10-223	A	4:52	4:54	Executive summary: this point is a repeat of the point starting line 12, same page. [Robert Colman]	Noted. Executive summary has been re-written
10-224	A	4:52	4:54	This does not seem to be a new result compared to the TAR [FILIPPO GIORGI]	Noted. Executive summary has been re-written
10-225	A	4:52	4:54	Obviously, there are to many bullets on the AMOC [Fortunat Joos]	Noted. Executive summary has been re-written
10-226	A	4:52	4:54	This is the third time this point is being made--need to consolidate. [Michael MacCracken]	Noted. Executive summary has been re-written
10-227	A	4:52	4:54	Already mentionned page 4, line 12-13 [Michel Petit]	Noted. Executive summary has been re-written
10-228	A	4:52	4:54	This is already mentioned (see comments n 2 and n 6). [Serge PLANTON]	Noted. Executive summary has been re-written
10-229	A	4:52	4:54	When we say "meridional overturning", is it clear that this is for oceanic circulation? [Tatsushi Tokioka]	Noted. Executive summary has been re-written to clarify this point
10-230	A	4:52		this bullet is not a new finding since the TAR, it should move up to the other category. Fromthe TAR: "The shutting off of the THC in either hemisphere could have long-term implications for climate. However, even in models where the THC weakens, there is still a warming over Europe. For example, in all AOGCM integrations where the radiative forcing is increasing, the sign of the temperature change over north-west Europe is positive [Stefan Rahmstorf]	Noted. Executive summary has been re-written
10-231	A	5:1	5:1	Presumably "model tunings" means "simple models tuned to reproduce the results of	Noted. Executive summary has been

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				AOGCMs" [Matthew Collins]	re-written
10-232	A	5:1	5:1	What does "model tunings" mean here--is the result not the mean of the 11 simulations, or perhaps for the mean of the 11 climate model sensitivities? Saying "tuning" here makes no sense. [Michael MacCracken]	Noted. Executive summary has been re-written
10-233	A	5:1	5:7	This is a very instructive and very helpful analysis. [Jerry Mahlman]	Noted. Thank you.
10-234	A	5:1	5:3	Instead of "for all SRES scenarios" , " for B1, A1B and A2 scenarios" may be the exact description. [Koki Maruyama]	Noted. Executive summary has been re-written
10-235	A	5:1		What type of "model tunings" are you referring to? [Jerry Mahlman]	Noted. Executive summary has been re-written
10-236	A	5:4	5:7	I found this point quite confusing. I would really think it would be clearer to use the term "range of estimates" rather than call this the "uncertainty" as it is not clear this is really the uncertainty range, etc. I would also very much favor giving the mean value as well as the range [when just the range is given, there is this ridiculous tendency to then estimate the uncertainty by dividing the top value by the bottom value--as for example dividing 4.5 by 1.5; that this makes no sense can be seen clearly by imagining some perturbation where the range is from 0 to 0.00001, so the ratio ends up at infinity]. [Michael MacCracken]	Noted. Executive summary has been re-written
10-237	A	5:4	5:7	The response uncertainty does not use the range of uncertainty in climate senstivity that is then discussed further down on this page. Shouldn't the partiioning of uncertainty between emissions and response use the full range of climate sensitivity uncertainty? [Brian O'Neill]	Noted. Executive summary has been re-written
10-238	A	5:4	5:5	This was also found in the TAR [Catherine Senior]	Noted. Executive summary has been re-written
10-239	A	5:7	5:7	Strictly it is 'concentrations uncertainty' that is being assessed. 'Emissions uncertainty' would be greater due to the uncertainty in carbon cycle and other 'Chapter 7' feedbacks. [Richard Wood]	Noted. Executive summary has been re-written
10-240	A	5:8	5:9	The new result indicated in these two lines seems quite irrelevant from a practical point of view. I suggest to eliminate it from the list. [PATRICIO ACEITUNO]	Noted. Executive summary has been re-written
10-241	A	5:8	5:9	An academically interesting result, but is it really of high relevance? There is limited space so I would think it important to stay focussed. Also, this result is based on one study under one scenario.	Noted. Executive summary has been re-written

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				[Matthew Collins]	
10-242	A	5:8	5:9	Is this really relevant and worth mentioning in the Executive Summary? [FILIPPO GIORGI]	Noted. Executive summary has been re-written
10-243	A	5:8	5:9	Although interesting as an aside, I don't think it is particularly relevant here. Lengthening of the day by 0.1 milliseconds by the end of the century is nothing compared to other influences e.g. tidal which would lengthen day by ~2.3milliseconds by the end of the century. This should be removed... the report is long enough already! [Gareth S. Jones]	Noted. Executive summary has been re-written
10-244	A	5:8	5:9	This point seems far too trivial to include in the summary. If it is included, at least say "Due to changes in the global wind field" [Michael MacCracken]	Noted. Executive summary has been re-written
10-245	A	5:8	5:9	Cute, but Who Cares? A millionth of a second/year?? [Jerry Mahlman]	Noted. Executive summary has been re-written
10-246	A	5:8	5:9	Funny, but not policy relevant. Could be cited out of context and used against IPCC [Michel Petit]	Noted. Executive summary has been re-written
10-247	A	5:8	5:9	An interesting finding. Is it certain enough to be included in this summary? What is the uncertainty in the estimate? [Ronald Stouffer]	Noted. Executive summary has been re-written
10-248	A	5:8	10:9	Is the change in day length really due to changes in wind? If I recall the paper from the Louvain-la-Neuve group correctly it is related to having more atmospheric mass further away from the centre of the earth [Fortunat Joos]	Noted. Executive summary has been re-written
10-249	A	5:8		this only has curiosity value, zero policy relevance - hence cut, the chapter is too long [Stefan Rahmstorf]	Noted. Executive summary has been re-written
10-250	A	5:9	5:9	The choice of a range of climate sensitivities from 1.7 to 4.2 deg C seems arbitrary (I assume it comes from the range of models used, but at p 5, l 14 a different range is given). I think this would make more sense if the bullet points on climate sensitivity came first to provide context. Maybe the point at p 5 ll 15-16 should be reiterated in this bullet point. [Richard Wood]	Noted. Executive summary has been re-written
10-251	A	5:10	5:12	In fact there is one study (Stainforth et al., 2005) in which high-sensitivity AOGCM versions were found. The general point is, however, that this is comparing apples with oranges. We should not expect ranges from the two approaches to agree as they are derived from entirely different approaches. The statement is phrased such that it is a deficiency of models that they do not sample the range derived from observations. One might equally write "Observations are of insufficient quality and length to constrain the	Noted. Executive summary has been re-written

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				climate sensitivity to within the multi-model range". I think it is sufficient to state that they differ or to omit the statement altogether. [Matthew Collins]	
10-252	A	5:10	5:10	Climate sensitivity is nicely defined later, but it should be defined here, it would not take up much space, e.g. "...climate sensitivity from 2.1-4.4oC for a doubling of CO2.." [Gareth S. Jones]	Noted. Executive summary has been re-written
10-253	A	5:10	5:12	I would urge adding a phrase at the end of line 12 stating "that are allowed, although with low likelihood" in order not to sound too open-ended. [Michael MacCracken]	Noted. Executive summary has been re-written
10-254	A	5:10	5:11	You have to be a little cautious in the use of the word "climate sensitivity" here. There are at least three definitions out there. See NRC, 2003, "Estimating Climate Sensitivity" for a consensus effort to separate out the at least three possible choices. [Jerry Mahlman]	Noted. Executive summary has been re-written
10-255	A	5:10	:25	You appear to give some privileged status to the estimates which you describe as "constrained from observations", which appears to mean those which only use the recent (say ~100 year) climate change averaged over large scales. I comment more substantively on this matter in relation to Section 10.5 where these results are discussed in more detail. I see no reason to single out these estimates as if they are particularly important. [James Annan]	Noted. Executive summary has been re-written
10-256	A	5:12	5:13	Executive Sammary. The sentence should be deleted entirely from Executive Summary. [Valentin Meleshko]	Noted. Executive summary has been re-written
10-257	A	5:13	5:18	What do you mean when you say the pdf is "likely" skewed? If you are talking about an existing estimate (perhaps the "IPCC estimate"), it is either skewed or not. The pdf is not some object to be discovered, it is a description of our uncertainties, and necessarily somewhat subjective. [James Annan]	Noted. Executive summary has been re-written
10-258	A	5:13	5:18	The term "right-skewed" is slightly confusing so is perhaps better expanded to "skewed such that the mode occurs at a smaller value than the median" or some such. Does "maximum probabilities" mean "modal values"? Also this second sentence doesn't seem to make grammatical sense. It would be good to quote the range of the 5%-tile in comparison with the range of the 95%-tile from the various studies to back up the assertion that the lower "bound" is better quantified than the upper "bound". I suspect may readers will be looking for a numerical value for the 5%-tile if it is indeed well constrained. [Matthew Collins]	Noted. Executive summary has been re-written
10-259	A	5:13	5:18	This point is very confusingly expressed. For example, it associates maximum probability with the minimum numbers--in fact with the limits of the likely range. The maximum	Noted. Executive summary has been re-written

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				probability is presumably around 3, which is not, technically, the average of 1 and 4. I am also surprised by all this emphasis on 1 as the lower bound--using only one significant figure here (yet two in other points). Overall, this point really needs to be more simply and clearly--and precisely--expressed. [Michael MacCracken]	
10-260	A	5:13	5:18	This use of undefined PDFs for our semi-literate "executives" , and stated in geek-speak could use some repair work here. [Jerry Mahlman]	Noted. Executive summary has been re-written
10-261	A	5:13	5:25	After careful reading of these two bullets I think I understand why they are stated this way. But a lot of people are probably going to be looking for symmetric confidence limits in the bullet starting on line 19. The reader probably needs to be helped (further) to understand the difficulty of doing that by some change in the language. For example, I would reverse the order in the bullet starting on line 13 to shift the emphasis so that it began with something close to what is now the last sentence. [Martin Manning]	Noted. Executive summary has been re-written
10-262	A	5:14	5:14	Executive Sammary. Before "climate sensitivity" insert "equilibrium". [Valentin Meleshko]	Noted. Executive summary has been re-written
10-263	A	5:15		right skewed – Change to "skewed towards higher values". [Ronald Stouffer]	Noted. Executive summary has been re-written
10-264	A	5:16	5:16	The AOGCMs used here do not sample the full range of sensitivities constrained from observations, or found in GCMs, in particular not the high values. E.g. the high sensitivity CCSR/NIES model is not being used for projections. [Richard Wood]	Noted. Executive summary has been re-written
10-265	A	5:19	5:25	The language is confuse for a non expert. It is mentioned in the same paragraph that the climate sensitivity is very unlikely to be below 1ºC (line 20). Two lines ahead it is mentioned that climate sensitivity is very unlikely below 1.5ºC. It is also mentioned that climate sensitivity is unlikely to be above 6ºC (<33%) and above 4.5ºC (28% probability). [PATRICIO ACEITUNO]	Noted. Executive summary has been re-written
10-266	A	5:19	5:25	I don't think that averaging the pdfs is valid. To the extent that they use independent data, then a product would be a better starting point. Any attempt to combine them has to account for your subjective opinion as to their reliability, and the extent to which they are based on similar assumptions (correlated data, physically similar models). This is of course a very difficult matter to address, but I do not think that hiding behind a clearly wrong method is adequate. If such a judgement cannot be made, then don't make it. See my further comment on p65. [James Annan]	Noted. Executive summary has been re-written
10-267	A	5:19	5:25	Suggest removing the word "conservative" here and in the underlying text and replacing	Noted. Executive summary has been

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				with whatever is meant by this undefined term. In the underlying text a range of distributions are intercompared, each contingent on a host of assumptions that are not clearly defined. Whether the outer bounds, or average, of such ranges is an over- or under-estimate of the width of the distribution would be a poorly-constrained judgment. Suggest reconsidering if the likelihood judgments given are warranted, and, if included, list the leading assumptions implicit in such a judgment. [Haroon Kheshgi]	re-written
10-268	A	5:19	5:25	I don't think it helpful to use the phrase "conservative estimate" (it is a bit ill-defined at a minimum) and it is not at all clear why such a perspective should be taken here in the summary--this just all seems more appropriate for the actual text where it can be developed, and this point should be combined with the preceding point. In any case, why chose "nine PDFs"? Does this mean "PDFs from 9 models? Also, on line 22, change "unlikely below" to "unlikely to be below"--and what is on line 22 seems to duplicate (or conflict with) what is on line 20 about the lower bound. On the finding that best agreement is with a climate sensitivity of 3.0 C (so two figure precision), just a note that that was the value Budyko put forth something like 25 years ago--also with two-figure precision. But overall, this point really needs to be edited down and keep only what is really essential for the summary. [Michael MacCracken]	Noted. Executive summary has been re-written
10-269	A	5:19	5:25	I thought that this lower bound had been set confidently near 1.5C. What are the counter arguments? [Jerry Mahlman]	Noted. Executive summary has been re-written
10-270	A	5:19	5:23	I fully support mentionning the probability values : in this context, it is more policy relevant to explicitely state that the probability of a sensitivity above 6 could reach 33%, than to qualify it as unikely. Why not adding (< 10 %) after "1 ", line 20. [Michel Petit]	Noted. Executive summary has been re-written
10-271	A	5:19	5:25	This is surely one of the main conclusions and deserves to be given a much higher profile in the summary [Catherine Senior]	Noted. Executive summary has been re-written
10-272	A	5:19	5:19	Suggest "most probable values" clearer than "maximum probabilities". [Richard Wood]	Noted. Executive summary has been re-written
10-273	A	5:22	5:22	Suggest "… upper 95% bound …" [Richard Wood]	Noted. Executive summary has been re-written
10-274	A	5:23	5:25	it is unclear what the difference between "best agreement" and "median" are, and how these different climate sensitivities were derived. [Brian O'Neill]	Noted. Executive summary has been re-written
10-275	A	5:23	5:29	It is very valuable to provide these bounds, but I found this bullet point was hard work to	Noted. Executive summary has been

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				understand. It took a while to see that you were giving two estimates of the range, a 'conservative' one and a 'poll of polls' one focusing on the traditional 1.5-4.5 range. If you do this, I think it would help to make what you are doing a bit more explicit. I suggest having two separate bullet points – one giving the 'likely' or very likely' upper and lower bounds, the other dealing with 1.5-4.5. I couldn't follow the last part at all (from "best agreement with observations…" onwards – which observations by the way?). Maybe just give the median value. These are important results, and I think it is worth taking a bit more space to make them clear (as in the summary of Box 10.2) [Richard Wood]	re-written
10-276	A	5:26	5:29	I don't think this is really a new result. It shows we've been working hard but that should go without saying shouldn't it? [Matthew Collins]	Noted. Executive summary has been re-written
10-277	A	5:26	5:29	This is not a result, just a statement of fact [Catherine Senior]	Noted. Executive summary has been re-written
10-278	A	5:28	5:28	Assuming that the average reader is familiar with these ten indices is unrealistic [Michel Petit]	Noted. Executive summary has been re-written
10-279	A	5:30	5:36	Items that corroborate results from the TAR should be moved to the the list that starts on page 10-3. [Gerrit Burgers]	Noted. Executive summary has been re-written
10-280	A	5:30	5:34	This point should be in the 'corroborating TAR' section. [Robert Colman]	Noted. Executive summary has been re-written
10-281	A	5:30	5:34	This does not seem to be a new result compared to the TAR [FILIPPO GIORGI]	Noted. Executive summary has been re-written
10-282	A	5:30	5:34	This seems to be very reasonable to me. [Jerry Mahlman]	Noted. Executive summary has been re-written
10-283	A	5:33	5:34	I'm not sure I understand this sentence. Increased precipitation intensity is the same as an increase in mean precipitation. [Matthew Collins]	Noted. Executive summary has been re-written
10-284	A	5:35	5:37	I strongly suspect that this is a simple statistical result of the mean temperature warming with little change in the underlying statistical variabiliity. [Jerry Mahlman]	Noted. Executive summary has been re-written
10-285	A	5:38		What are "cold air outbreaks"? Cold spells? [Brian O'Neill]	Noted. Executive summary has been re-written
10-286	A	5:39	5:39	This point should be in the 'corroborating TAR' section. [Robert Colman]	Noted. Executive summary has been re-written
10-287	A	5:42	5:45	This finding, which appears to be largely based on results from one model, does not agree	Noted. Executive summary has been

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				with the finding in Chapter 8 (Pg. 52, lines 4-5) which reads: "There is no agreement among models whether global warming will make tropical cyclones more or less intense." The two conclusions need to be harmonized. [Lenny Bernstein]	re-written
10-288	A	5:42	5:53	These conclusions regarding tropical cyclones are somewhat confusing and seemingly contradictory. [Matthew Collins]	Noted. Executive summary has been re-written
10-289	A	5:42	5:45	This does not seem to be a new result compared to the TAR [FILIPPO GIORGI]	Noted. Executive summary has been re-written
10-290	A	5:42	5:45	…have been confirmed using higher resolution (9 km grid) and different model physics configurations, and indicate future increases in tropical cyclone (i.e., hurricane) intensity and precipitation. Similar results have been obtained with a new global atmospheric model run at about 20 km resolution, which can resolve more spatial detaill in individual tropical cyclones. [Thomas Knutson]	Noted. Executive summary has been re-written
10-291	A	5:42	5:45	Delete this paragraph. This conclusion, which presents results from one model, does not agree with the conclusions presented in Chapter 8 (Pg. 52, lines 4-5) on the results from a range of models: "There is no agreement among models whether global warming will make tropical cyclones more or less intense." Chapter 8's assessment should be more robust. [Jeffrey Kueter]	Noted. Executive summary has been re-written, and coordinated better with Ch. 8
10-292	A	5:42	5:45	This is far from a new result. It is roughly a decade old. [Jerry Mahlman]	Noted. Executive summary has been re-written
10-293	A	5:42	:53	Apart from not making an overall assessment of these diverse results, it is worth mentioning that all models (I understand) show more intense precipitation with tropical storms, since much of the loss of life and damage is though flooding and landslides following heavy precipitation. [John Mitchell]	Noted. Executive summary has been re-written
10-294	A	5:46	5:53	Results described in lines 46- 48 and in lines 49-53 refer to the same idea. I suggest to combine those results in one paragraph. [PATRICIO ACEITUNO]	Noted. Executive summary has been re-written
10-295	A	5:46	5:48	"decrease of tropical cyclone *frequency*…" Also append this sentence: Other recent models also show decreases, but smaller in magnitude and with considerable regional variation. [Thomas Knutson]	Noted. Executive summary has been re-written
10-296	A	5:46	5:53	First, these points seem to overlap. Second, am I correct to infer that the point being made	Noted. Executive summary has been

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				is that there will be a decrease in the number of tropical storms? If this is the point to be made, then associated points need to be made that the storms seem likely to be more powerful and put out more rain. However, I would think that just as for the MSU issue, IPCC needs to be pretty cautious in suggesting conclusions about tropical cyclones given how little work has been done on them. To date, it is my understanding that the total number around the world has been remaining roughly constant even with warming--so have these modes reproduced that result. In addition, the new studies indicate a greater tendency to powerful storms that overall dissipate more energy, so just talking about number seems a very limited view. Finally, what is really needed is a breakdown by ocean basin--the Atlantic has recently had some very high numbers of storms, so is this result suggesting that one can get much greater variations in the breakdown of storms among basins, or what? Also, the actual observed trends of intensification are proving to be greater than the models are projecting--a quite troubling result. There have also been some storms appearing in unprecedented locations--like the South Atlantic, so this would need to be mentioned. But overall, I would urge IPCC to be pretty cautious in coming to conclusions here--there is still a lot of work to be done. [Michael MacCracken]	re-written
10-297	A	5:46	5:48	This implied decrease in projected tropical cyclone frequencies is interesting, especially so if it agrees with actual frequency statistics. [Jerry Mahlman]	Noted. Executive summary has been re-written
10-298	A	5:46	5:48	if this result refers to a decrease in frequency of cyclones, it should say so specifically, otherwise it is unclear what it decreasing. [Brian O'Neill]	Noted. Executive summary has been re-written
10-299	A	5:46	5:48	Inconsistent with the statement in chapter 8, page 52, line 4-5 "There is no agreement among the models whether global warming will make tropical cyclones more or less intense", and with the following lines chapter 8, page 52, lines 5-10. [Michel Petit]	Noted. Executive summary has been re-written, and there is now better coordination with ch. 8
10-300	A	5:46	5:48	Are these global atmospheric models or OAGCM results? [Ronald Stouffer]	Noted. Executive summary has been re-written
10-301	A	5:46	5:48	Here, only the decrease of hurricanes in number by 30% is mentioned. I think it is good to mention, besides, that one model (20km resolution MRI model) has shown that the decrease in the total number is explained as the results of substantial decrease in number for relatively weak hurricanes and increase for intense hurricanes. See the results of MRI team. [Tatsushi Tokioka]	Noted. Executive summary has been re-written
10-302	A	5:46	5:49	Better to state the confirmed results explicitly? [Richard Wood]	Noted. Executive summary has been re-written

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10-303	A	5:46	6:2	This was another area where I felt the individual results needed to be synthesised and interpreted for the user. Although the bullet points ae not contradictory the text is complex. [Richard Wood]	Noted. Executive summary has been re-written
10-304	A	5:49	5:53	This finding needs additional explanation, since it does not appear to be logical. An increase or decrease in the number of both strong and weak tropical cyclones is understandable, but what physical mechanism would cause an increase in strong cyclones but a decrease in weak ones? As presented it appears to be a model artifact. [Lenny Bernstein]	Noted. Executive summary has been re-written
10-305	A	5:49	5:53	Results from a global model with about 20 km grid spacing show the strongest tropical cyclones increasing in number while weaker storms decrease in number. The tracks are not appreciably altered, and there is about a 10% increase in maximum wind speeds in future simulated tropical cyclones. (the ending can be deleted, since it is covered in the previous bullet) [Thomas Knutson]	Noted. Executive summary has been re-written
10-306	A	5:49	5:53	This conclusion is not intuitively obvious. The reader could rationalize either an increase or decrease in the number of both strong and weak tropical cyclones, but how does one explain an increase in strong cyclones and a decrease in weak ones? If a physical mechanism can not be provided to explain this apparent contradiction, the conclusion should be dismissed as a model artifact. [Jeffrey Kueter]	Noted. Executive summary has been re-written
10-307	A	5:54	5:56	This does not seem to be a new result compared to the TAR [FILIPPO GIORGI]	Noted. Executive summary has been re-written
10-308	A	5:54	5:56	This needs some explaining because we pretty much accept that we will get more rain out of extra-tropical cyclones(more water vapour available). The wind intensity increase/cyclone rain increase is interesting, but the current observations do not seem to agree. [Jerry Mahlman]	Noted. Executive summary has been re-written
10-309	A	5:54	5:56	This statement is to my opinion to strong, there are also studies, which show no change in midlatitude stominess, e.g., Kharin and Zwiers (J. Climate, 18, 1156-1173, 2005). [Christoph, C. Raible]	Noted. Executive summary has been re-written. This is a synthesis result based on assessment of a number of studies where most show this result.
10-310	A	6:0		Section 6. Because the part on sea-level rise in this Chapter is not finished yet, I expect to receive a finished version for review at a later stage, with the corresponding items in the Executive Summary. Apperently, the authors wish to re-assess the problem of how the errors in various contributions to sea-level rise should be added. I value this important and courageous effort very much! And I hope to find some explanation of the choices	Noted. Thank you.

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				the authors will make. [Gerrit Burgers]	
10-311	A	6:1	6:2	Executive summary: this point is a repeat of the point starting line 51 page 10-4. [Robert Colman]	Noted. Executive summary has been re-written
10-312	A	6:1	6:3	I find this to be counter-intuitive given that the annular modes are tightening, the amplitude of higher-latitude extra-tropical cyclones should decrease and become more zonal in their structure. Can we have it both ways? If so, what is the argument? [Jerry Mahlman]	Noted. Executive summary has been re-written. The poleward shift and change in frequency are two separate phenomena
10-313	A	6:1	6:3	I assume this shift is seen over the oceans (only). [Ronald Stouffer]	This depends on the region as noted in the studies assessed.
10-314	A	6:3	6:3	Already mentionned twice: page 3, line 39, and page 4, line 51. To be quantified [Michel Petit]	Noted. Executive summary has been re-written
10-315	A	6:4	6:6	Lines 4-6 vs. Lines 9-11. Can we have it both ways? If so, do we know what the mechanisms are? [Jerry Mahlman]	Noted. Executive summary has been re-written
10-316	A	6:6	6:6	That midlatitude storms are likely to intensify is a very important result, and needs to be accompanied by some sort of explanation of how this can happen when the north-south temperature gradient is being sharply reduced. It is not that I doubt the result, but since it is counterintuitive and much has been made about this particular issue, a bit of explanation would be very useful. Does, this mean, for example, that the convective storms are intensifying (as the overall temperature is warmer), or what? [Michael MacCracken]	The mid-tropospheric temperature gradient increases in the future warmer climate thus contributing to the more intense storms as noted in the assessed studies.
10-317	A	6:9	6:11	The conclusion is that many models show a positive NAO trend but the text (page 26, lines 30-40) says that only "more than half" do. Is it likely (>66%) that the NAO trend will be positive? [Matthew Collins]	Agreed—likely that NAO trend will be positive based on majority of assessed models showing that result Xxxx make change
10-318	A	6:9	6:11	This sentence is awkward. Please rewrite it. [FILIPPO GIORGI]	Noted. Executive summary has been re-written
10-319	A	6:9	6:9	Define NAM and SAM [Andrew Lacis]	Noted. Executive summary has been re-written
10-320	A	6:12	6:14	This result, described in lines 12-14 is described again (although with more details) in lines 35-41 of same page. I suggest to eliminate lines 12-14. If these lines are retained, I suggest to be more precise regarding which models suggest that sustained warming will lead to an irreversible meltdown later... All models ? some models ? one specific model ? [PATRICIO ACEITUNO]	Noted. Executive summary has been re-written
10-321	A	6:12	6:12	On line 12, replace "may" by "is likely to" to conform with the IPCC lexicon. Also, this	Noted. Executive summary has been

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				point, while seeming quite reasonable and logical, seems to be in conflict with what is said in the sea level section of this chapter (see further comments below on sea level summary). [Michael MacCracken]	re-written
10-322	A	6:12	6:14	The conclusion of this bullet is contradicted by the results given in section 10.6.4 where it is estimated that Greenland's contributipon to sea-level rise in the 21st century would only be 1 to 7 cm. [Peter Stone]	Noted. Executive summary has been re-written
10-323	A	6:12		Perhaps change to read: "Coupled model simulations show that 21st century warming may be sufficient to melt large portions of the Greenland ice sheet over subsequent centuries" By doing this, you make it clear that the GIS won't melt by the end of the 21st century. [Jonathan Overpeck]	Noted. Executive summary has been re-written
10-324	A	6:14	6:14	It is not clear what "later" means, especially given the points made later with regard to sea level change [Michael MacCracken]	Noted. Executive summary has been re-written
10-325	A	6:14		will lead – No uncertainty? [Ronald Stouffer]	Noted. Executive summary has been re-written
10-326	A	6:15	6:17	"reaches as much as 60%" is misleading. Instead simply state the range, which is from zero to 60%. [Jeffrey Kueter]	Noted. Executive summary has been re-written
10-327	A	6:15	6:26	Too much emphasis on MOC in Ecective Summary [Mojib Latif]	Noted. Executive summary has been re-written
10-328	A	6:15	:17	All MOC projections ? All the new ones since the TAR (Including EMICS?) [John Mitchell]	Noted. Executive summary has been re-written
10-329	A	6:15		15 Change "flux correction" to "flux adjustment". [Ronald Stouffer]	Noted. Executive summary has been re-written
10-330	A	6:16	6:17	This is already mentioned p4 lines 10-11. [Serge PLANTON]	Noted. Executive summary has been re-written
10-331	A	6:17	6:17	Suggest giving the range, and not only one extreme for MOC change. [Haroon Kheshgi]	Noted. Executive summary has been re-written
10-332	A	6:17		19. Page 6, line17 – and reaches as much as 60% - The reduction reaches this value. What is the median value? [Ronald Stouffer]	Noted. Executive summary has been re-written
10-333	A	6:19	6:19	Could 'later' be made more explicit?	Noted. Executive summary has been

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				[Richard Wood]	re-written
10-334	A	6:21	6:23	This is a nice result. [Jerry Mahlman]	Noted. Thank you.
10-335	A	6:21	6:23	20. Page 6, lines 21-23 – Smaller abrupt changes are seen in AOGCMs. Larger abrupt changes seem possible. Uncertainty in statement? [Ronald Stouffer]	Noted. Executive summary has been re-written
10-336	A	6:21	41:	for these ice sheet and sea level issues, there should be some effort to find conformity between chapters 4,5,6 and 10. For example, paleo results suggest the WAIS might be susceptable to collapse, perhaps early on. Also, do the models backing chapt 10 take into consideration processes (not yet well known? - see good sicussion in Cap 4) that could lead to dyanamic instability, and more rapid wasting of ice sheets. It is not clear that these bullets represent the true uncertainties - e.g., that sea level rise in the next few centuries could be more than inferred, and that the WAIS could play a bigger role. Also note that Chap 5 is giving a slightly higher estimate of recent sea level rise that TAR, and that it still appears that sea level rise is accelerating. [Jonathan Overpeck]	Noted. Executive summary has been re-written
10-337	A	6:21		should you add the caveat about not having interactive dynamic ice sheet models, and that this result is likely robust anyhow [Jonathan Overpeck]	Noted. Executive summary has been re-written
10-338	A	6:22	6:23	This is already mentioned p4 lines 16-17. [Serge PLANTON]	Noted. Executive summary has been re-written
10-339	A	6:25		"long term response" is too vague. Could ice sheets make a difference on these longer time scales? Quite possibly… [Jonathan Overpeck]	Noted. Executive summary has been re-written
10-340	A	6:27	6:28	I suggest to be more precise in indicating which regions are involved in the proccesses described in these two lines. [PATRICIO ACEITUNO]	Noted. Executive summary has been re-written
10-341	A	6:27		. ... preciptiation will likely ..... [Jerry Mahlman]	Noted. Executive summary has been re-written
10-342	A	6:27		I think this sounds far more definitive than it should be. Some work indicates that this is NOT the case - see chapter 6. Increased rain on the ice sheet in summer isn't going to slow retreat. At the least, you should quantifiy the time interval for which your assertion is valid, and you should also work w/ Chapter 6 (Overpeck) to make sure that there is agreement between chapters. The problem is that current ice sheet models might not be that good. See recent Alley papers in Science. I'm quite concerned that Chap 10 is	Noted. Executive summary has been re-written

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				underestimating possible future rates of sea level rise. [Jonathan Overpeck]	
10-343	A	6:27		21. Page 6, line 27 – will – No uncertainty? [Ronald Stouffer]	Noted. Executive summary has been re-written
10-344	A	6:29		Changes in the Greenland Ice Sheet out beyond 1200 are discussed in other bullets, but with the WAIS, discussion is limited to this century. Should there be more on what might happen to the WAIS (and EAIS) beyond 2100? This goes with the previous comment - paleo data indicate that the WAIS could collapse sooner than the GIS. There should also be discussion with Chaps 4 and 6 regarding whether ocean warming is the main influence on the WAIS discharge - is this really that well known? Hard to imagine the WAIS sticking around forever if we get serious warming. [Jonathan Overpeck]	Noted. Executive summary has been re-written
10-345	A	6:30	6:31	This is relevant to chapter 3. [Serge PLANTON]	Noted. Executive summary has been re-written
10-346	A	6:30	6:24	22. Page 6, lines 30 – 34 – More needed. Meaning is unclear to me. [Ronald Stouffer]	Noted. Executive summary has been re-written
10-347	A	6:33	6:33	I would suggest changing "precipitation" to "snowfall" [Michael MacCracken]	Noted. Executive summary has been re-written
10-348	A	6:33	6:34	This is a critical place for adding a comment on the limitations of "current ice dynamic models". The existing sentence lacks any statement about the limitations of these models, and so leaves the impression that 2.5 mm yr-1 is the maximum rate that could occur. Larger estimates have been made (Oppenheimer, 1998). At very least, a statement underscoring the deficiencies of these models in dealing with ice streams and grounding line retreat needs to be added, e.g., add to the end of sentence "...although this estimate may be low because these models are unable to reproduce currently observed ice stream and grounding line behavior". [Michael Oppenheimer]	Noted. Executive summary has been re-written
10-349	A	6:33		after "unlikely to outweigh increased precipitation…" add "during this century" in order to clearly separate what may happen in this century from what may happen in later centuries when the situation may well be entirely different. [Michael Oppenheimer]	Noted. Executive summary has been re-written
10-350	A	6:34	6:34	It is not clear what the time period for this rate of change applies to--forever or just during the 21st century [Michael MacCracken]	Noted. Executive summary has been re-written
10-351	A	6:34	6:48	I think it will be very confusing to the reader to use the units mm and mm/yr instead of sticking to meters and m/century. Page 3, line 48 is expressed in meters and that is quite	Noted. Executive summary has been re-written

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				helpful (I think people can understand that a meter and a yard are about the same--but a millimeter, well, that is confusing). [Michael MacCracken]	
10-352	A	6:35	6:41	A quite dramatic increase in sea level is described for the 22nd and following centuries (0.6 m per century) due to melting of the Greenland ice sheet under an scenario characterized by an annual-average warming in Greenland of 8-10ºC (is this possible ?). I question the fact that there are no references in the executive summary to what would be the most likely impact of melting of this ice sheet at the end of the 21st century [PATRICIO ACEITUNO]	Noted. Executive summary has been re-written
10-353	A	6:35	6:41	Suggest considering the full range, and not only one case. The range o f 8-10C seems narrow considering all the various sources of uncertainty. And what is possible under low emissions scenarios, and what is possible with mitigation (which is already occurring)? [Haroon Kheshgi]	Noted. Executive summary has been re-written
10-354	A	6:35	6:35	In the section "New Results since the TAR", an important bullet needs to be added at Page 6, Line 35. "—Because of observed emissions trends and new projections concerning high-latitude ice sheets, estimates of median sea level rise by 2100 have been cut by nearly 50%. While the TAR range was 90-880mm, the new figure is 130-380mm." This is important information for policymakers and needs to be included. [Jeffrey Kueter]	Noted. Executive summary has been re-written
10-355	A	6:35	6:41	This is cryptic. I suggest rewording this. [Jerry Mahlman]	Noted. Executive summary has been re-written
10-356	A	6:35	:41	This result was also in the TAR. [John Church]	Noted. Executive summary has been re-written
10-357	A	6:36	6:39	I am quite confused by the values and the math here. First, did not the TAR say that Greenland would melt with a sustained warming of 3 C would melt Greenland, and 5.5 C would do it in 1000 years--so why do we need to get to 8-10 and the high emission scenarios? Also, if the rate of melting is .6 m/century (much more informative that 6 mm/year) in the first several centuries, then one expect the rate to rise after that time and Greenland would be expected to be mostly gone in well less than 1000 years (unless one is counting the time to get to 8-10 C warming)--please better explain where the estimate for greater than 1000 years comes from for an 8-10 C warming--this seems much, much too long (the Eemian, as I understand it, shows something like 50% melting for only a few degree warming--and apparently over just a few centuries). [Michael MacCracken]	Noted. Executive summary has been re-written
10-358	A	6:38		I wonder about the wisdom of using the explicit figure of 1000 years here in the Exec Summary. It could become a target for contrary views. Also the sense I get from the	Noted. Executive summary has been re-written

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				chapter, and section 10.6.7 in particular, is that there is a lot of uncertainty about the long term evolution of the major ice sheets - probably to the extent that one should think twice before making any quantitative statements beyond a few centuries. Perhaps a more qualitative statement about the issues involved in very long term change in ice sheets would be more robust. [Martin Manning]	
10-359	A	6:39	6:41	This seems an extremely cautious statement--is there any evidence at all that Greenland would reform once melted--certainly there is no analog ice sheet for this at present--it took going into an ice age to generate it. I would suggest changing "medium likelihood"--which implies 50-50 chance, to very unlikely. Also, on line 40, change "could" to "would"--this is not going to be some sort of geoengineering project we undertake, is it? And say "preindustrial climatic conditions" as we are not advocating taking society back to its preindustrial state. [Michael MacCracken]	Noted. Executive summary has been re-written
10-360	A	6:42	6:45	But we have no idea when concentrations will be stabilized. As of now, we can't even justify near-term constant emissions, a vastly simpler goal that we have yet to pursue. [Jerry Mahlman]	Noted. Executive summary has been re-written
10-361	A	6:45		23. Page 6, line 45 – I doubt if "most of this warming is occurring in the first few decades". The tail is very long. See Stouffer 2004 and Stouffer and Manabe 2001. If the rate is in view then the statement is okay. [Ronald Stouffer]	Noted. Executive summary has been re-written
10-362	A	6:46	6:47	This is quite confusing--so this is the amount of rise one would get after getting to stabilization--but give us the value of the increase from present when we are at stabilization for comparison. And on line 54 it says it takes 1000 years to get to stabilization (admittedly after going to zero emissions rather than something like, say 10-20% of current emissions), so this commitment point may well be missing the main change that has occurred. [Michael MacCracken]	Noted. Executive summary has been re-written
10-363	A	6:48	6:50	This is a nice, and new, analysis. [Jerry Mahlman]	Noted. Thank you.
10-364	A	6:48	6:50	<After the sentense, I recommend strongly adding the following sentense> "Overshoot scenario is useful for risk manegement and it implys that the atmosphere temperature will decrease if the GHG concentrations in the atmosphere could be reduced" as described in line 1-11of page 41, Chapter 10. [Koki Maruyama]	Noted. Executive summary has been re-written
10-365	A	6:48	:50	This result was also in the TAR. [John Church]	Noted. Executive summary has been re-written

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10-366	A	6:49	6:49	The word "commitment" should be deleted--not only does the commitment continue (but decline), but the key issue is that sea level keeps rising. [Michael MacCracken]	Noted. Executive summary has been re-written
10-367	A	6:51	:52	This result was also in the TAR. [John Church]	Noted. Executive summary has been re-written
10-368	A	6:52		24. Page 6, line 52 – temperature nearly levels off – The tail is very long. The rate of increase greatly reduces. [Ronald Stouffer]	Noted. Executive summary has been re-written
10-369	A	6:53	6:55	Some rewording is needed here for clarity: "… zero emissions in the year 2100 the climate will take of the order of a thousand years to stabilize, and at that time the temperature and sea level will remain well above their pre-industrial values." Who is expecting them to return--make it clear how different the conditions will be. [Michael MacCracken]	Noted. Executive summary has been re-written
10-370	A	6:53	6:53	Does this mean committed today? [Richard Wood]	Noted. Executive summary has been re-written Xxxx Make change
10-371	A	6:53		some authors have argued that the next glaciation, ~30-50 kyr down the line, could be prevented by anthropogenic CO2 (see Archer and Ganopolski, G-cubed 2005). [Stefan Rahmstorf]	Noted. Executive summary has been re-written
10-372	A	7:1	7:7	Results described in these lines were already presented in page 4, lines 28-36. [PATRICIO ACEITUNO]	Noted. Executive summary has been re-written
10-373	A	7:2		"unanimous agreement" suggests that everybody uses the same set of incorrect parameters [Vincent Gray]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-374	A	7:2		25. Page 7, line 2 – will reduce – No uncertainty? [Ronald Stouffer]	Noted. Executive summary has been re-written.
10-375	A	7:3		...As a result, a growingly large fraction of ... [Jerry Mahlman]	Noted. Executive summary has been re-written.
10-376	A	7:5	7:7	This is confusing as phrased--or maybe I am missing the point. On line 5, should it not say that for a given emissions scenario, consideration of carbon cycle feedbacks can increase the expected CO2 concentration by 50 to 100 ppm, depending on the model (20 to 200 ppm considering the most extreme estimates)? Then on line 7, replace "CO2" by "CO2 increase" [Michael MacCracken]	Noted. Executive summary has been re-written.

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10-377	A	7:6	7:12	This seems to duplicate the bullet p4, ll 31-33. [Richard Wood]	Noted. Executive summary has been re-written.
10-378	A	7:8	7:8	A bullet needs to be added concerning Greenland. Add after the last suggested bullet on page 7, line 8 "--New model results indicate that the Greenland ice sheet would melt completely even if there were no anthropogenically-forced climate change. Human emissions can accelerate this natural process". [this is a result of Toniazzo et al. noted on page 10-21, line 49] [Jeffrey Kueter]	Noted. Executive summary has been re-written.
10-379	A	7:9	7:11	Presumably the ranges given are for a particular emissions scenario [Richard Wood]	Noted. Executive summary has been re-written.
10-380	A	8:5	8:5	Since this chapter was not written until 2005, this should say "new findings with respect to the future climate as compared to the TAR." [Michael MacCracken]	Noted. Introduction has been re-written to take into account reviewers' comments.
10-381	A	8:7	8:21	It would be very helpful to the report if there were a table summarizing the different stabilization cases, and indicating the total CO2 and, for those models that reported RF, the total RF at stabilization (preferably as CO2 equivalent if possible - CO2 equivalent is inexact for many reasons but helpful to the non-expert). Please include the 4x CO2 case in this list - among other things, you show some important results from it for the Greenland ice sheet. This listing will help the non-expert see where each case falls relative to one another. [Susan Solomon]	Noted. Introduction has been re-written to take into account reviewers' comments.
10-382	A	8:9	8:13	This sentence is pretty confusing, and has unbalanced parentheses. [Michael MacCracken]	Noted. Introduction has been re-written to take into account reviewers' comments.
10-383	A	8:10		change "...increase,..." for "...increase),..." [PATRICIO ACEITUNO]	Noted. Introduction has been re-written to take into account reviewers' comments.
10-384	A	8:13	8:13	The "in" is not sufficiently informative. Perhaps say "that were initiated when these concentrations were reached in" [Michael MacCracken]	Noted. Introduction has been re-written to take into account reviewers' comments.
10-385	A	8:15	8:19	The Special Report on Emission Scenarios (Pg. 62) carefully stated that scenarios are neither predictions nor forecasts of the future. The report also said that it could not assign probabilities to the likelihood that one or another of its scenarios would occur. These caveats also apply to model projections based on the SRES scenarios and should be	Noted. Introduction has been re-written to take into account reviewers' comments. Paragraph has been added to address scenarios.

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				included, either in the text or in a footnote. Also, this text assumes a familiarity with the SRES scenarios that many readers may not have. The key features of the three scenarios used for the model intercomparison should be discussed. [Lenny Bernstein]	
10-386	A	8:15	8:21	I think it would be very useful to add a figure here showing (a) the estimated globalannual emissions rates for CO2 (equivalent?) for the next 200 years in the 3 SRES scenarios used; and (b) the estimated annual atmospheric concentrations of CO2 (equivalent?) for the next 200 years that these 3 SRES scenarios would generate. [Chuck Hakkarinen]	Noted. Introduction has been re-written to take into account reviewers' comments. A similar figure appears in the revised text later in the chapter.
10-387	A	8:15	8:19	IPCC is always careful to state that scenarios are not predictions or forecasts of the future (See SRES, Pg. 62). The same is true of climate model proejctions that use SRES scenarios as input. This text should remind readers of this fact, either in the body of the text or in a footnote. [Jeffrey Kueter]	Noted. Introduction has been re-written to take into account reviewers' comments.
10-388	A	8:19	8:20	"climate change commitment should be defined or a reference to a definition given there (see also comment n 1). [Serge PLANTON]	Noted. Introduction has been re-written to take into account reviewers' comments.
10-389	A	8:24	8:24	This paragraph appears to duplicate some of the material in the paragraph starting at p 9 l 14. Suggest merging the material into the later paragraph, which seems a more logical position. [Richard Wood]	Noted. Introduction has been re-written to take into account reviewers' comments.
10-390	A	8:33	8:39	Need to clearly introduce the idea of equilibrium and transient climate changes, time scales of response and etc. [Ronald Stouffer]	Noted. Introduction has been re-written to take into account reviewers' comments.
10-391	A	8:39	8:40	Not sure what is being referred to as a standard benchmark calculation here. The physics ensembles aren't widely done. [Richard Wood]	Noted. Introduction has been re-written to take into account reviewers' comments.
10-392	A	8:54	9:11	This is a good explanation of the sources of uncertainty in climate model projections and should be retained in future drafts. [Lenny Bernstein]	Noted. Thank you.
10-393	A	8:54		The only uncertainties you persistently refurse to address are the discrepancies between the model projections and the actual future behaviousr of the climate as it unfolds [Vincent Gray]	Noted. Introduction has been re-written to take into account reviewers' comments.
10-394	A	9:0		Sec 10.2 The attention paid to aerosol forcing, its uncertainty, and the implications of this uncertainty on total forcing is wholly insufficient.	Rejected. Since there is essentially no information in the IPCC archive

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				[Stephen E Schwartz]	regarding the concentrations of non-sulphate species or the direct and indirect forcings by aerosols, it is not possible for the authors to quantify this uncertainty using forward calculations for the multi-model ensemble. We do, however, discuss the estimates of the aerosol forcing by Forster (2005) for the ensemble A1B simulations. In response to comment 10-470, we have included a table listing model by model what aerosol effects were included.
10-395	A	9:1	9:11	This paragraph, taken together with figure 10.1.1., perpetuates the miss-conception that the calculation of the radiative forcing is a separate step in the modelling process. The radiative forcing is an approximate way of quantifying the radiative impact of a change in concentration in the absence of climate change. The real power of a climate model is that it does not have to make such an approximation and can compute radiative effects as the climate system evolves. Arguably, uncertainties in the calculation of the radiation stream can be considered to be in the same class as uncertainties in other modelling processes. [Matthew Collins]	Noted. However, the AOGCMs produce radiative forcing as a response to concentrations of GHGs and other constituents, and tha is what is being illustrated schematiclly here.
10-396	A	9:1	9:11	This paragraph discusses how emissions are converted to concentrations, concentrations are converted to radiative forcing and forcing affects the climate model response, and that all three stages carry some uncertainty. However, it should be pointed out that some climate models contain their own gas cycle and aerosol models, and that there is some interaction with the climate state. This adds further uncertainty and not using interactive gas cycle and aerosol models is an assumption which also needs to be pointed out. [David Sexton]	In this general discussion this level of detail is unwarranted.
10-397	A	9:3	9:3	Possibly my ignorance but only "Gas cycle" models are mentioned here, but I wonder if it should be "Gas cycle and aerosol models ". [David Sexton]	Noted. Introduction has been re-written to take into account reviewers' comments.
10-398	A	9:13	9:19	The use of multi-model ensembles for climate projections is weakened by the fact that the models are not truly independent of each other, with members of the ensemble sharing common approaches to characterization of climate drivers and outputs. Some discussion of the implications of this fact is needed at this point. [Lenny Bernstein]	It is implied that models cannot sample the full range of uncertainty, but can only estimate it by best efforts of modeling groups.
10-399	A	9:13	9:14	The sentence "This bewildering array of uncertainty…difficult to be able to come to any conclusions regarding possible future climate change" is a fine sentence but, with	Noted. Introduction has been re-written to take into account reviewers'

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				selective quotation, could be put to mischievous use. [Garry CLARKE]	comments.
10-400	A	9:13	9:14	This comment is too negative: "suggests that it is difficult to be able to come to any conclusions". In fact the uncertainties imply only that it is difficult to be definitive or exact. The consistency of modelling results between models, as well as over time, suggests that strong results might be obtained, even though not definitive. [Robert Colman]	Noted. Introduction has been re-written to take into account reviewers' comments.
10-401	A	9:13	9:19	The multi-model ensemble approach is based, in part, on the assumption that the models are independent of each other. This is not the case, since many of the models in the ensemble are derived from each other or a common earlier model. The inter-model comparison programs described in Chapter 8 also drive models to common approahces. Because of this, one would expect that given the same inputs, the outputs of all models in the ensemble would be close. The authors need to discuss the degree to which climate models share common components and the implications of this sharing on the quality of multi-model ensemble outputs. [Jeffrey Kueter]	It is implied that models cannot sample the full range of uncertainty, but can only estimate it by best efforts of modeling groups. Ch. 8 has a more full discussion of this issue.
10-402	A	9:16	9:18	While the "expanded use of multi-models" has been a significant step forward since the TAR, I think it is going too far to say that we are now in possession of "higher quality and more quantitative climate change information". It would be better to say that it has allowed for a more quantitative assessment of climate change projections. [Matthew Collins]	Noted. Introduction has been re-written to take into account reviewers' comments.
10-403	A	9:21	9:22	What is the evidence that sample sizes of order hundreds provide "the means" to quantify parameterization uncertainty? For some cases (e.g., Tol, R. S. J. 2003. Is the uncertainty about climate change too large for expected cost-benefit analysis? Climatic Change 56 (3):265-289.) a sample size of this order can be insufficient. [Klaus Keller]	Noted. Introduction has been re-written to take into account reviewers' comments.
10-404	A	9:21		Add "may" before "provides the means". [Ronald Stouffer]	Noted. Introduction has been re-written to take into account reviewers' comments.
10-405	A	9:26	9:26	Text on projected concentration and abundances may be needed here. A discussion on recent developments in projecting GHGs and aerosol abundances would be nice. At least one needs to clearly state that the AOGCMs were driven be prescribed concentrations. [Fortunat Joos]	ACCEPTED – Section 10.2 discusses the scenarios used and the prescribed concentrations input to the AOGCMs.
10-406	A	9:27	9:27	I would suggest that the title should read "Projected Changes in Radiative Forcing" [Michael MacCracken]	REJECTED – The revised section discusses not only radiative forcing, but also the SRES scenarios used and the correspondence of those scenarios with

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					recent trends. In other words, the section covers both forcing agents and forcing.
10-407	A	9:27	9:27	If (!) short of space I think Fig. 10.1.1 could be omitted. The text explains the issue well. [Richard Wood]	rejected
10-408	A	9:27		I have the impression much of the material in this section really belongs to Chapter 2. Perhaps a clearer explanation as to why it is here would help. [FILIPPO GIORGI]	TAKEN INTO ACCOUNT – Please see response to comment 10-416.
10-409	A	9:27		Section 10.2. This section desperately needs an introduction that describes what it is about. Currently it starts immediately with a detailed assessment of current SO2 emissions in China, before the reader understands: what are the multimodel projections that are being talked about (several different ones are discussed in the intoduction in Section 10.1); what emissions scenarios are used in these projections (e.g., in general this section is describing SRES scenario runs?); what models were used; what assessment of the radiative forcing outcomes of these scenarios is going to be included here vs. in other parts of the report or chapter (e.g. Ch. 2 is referred to later on but it would be good to know up front what was done in that chapter and how the assessment here will be different). [Brian O'Neill]	ACCEPTED – An introduction has been added that describes the scenarios used, the relationship of those scenarios to recent trends (which is not covered in chapters 2 or 7), the relationship of the forcing calculated by the models for present day to the values given in chapter 2, and finally the accuracy of the AOGCMs' forcing calculations.
10-410	A	9:29	9:29	A statement needs to be inserted to the effect that "Some modelers (Hansen and Sato, 2004) have noted that recent emissions trends are below the IPCC marker scenarios from the Third Assessment. Currently, this would make SRES scenario B1 more realistic than the others generally used in this chapter, which are A1B and A2". [Jeffrey Kueter]	ACCEPTED (conditionally) – Section 10.2 and 10.4.2 now discuss the papers by Hansen and Sato (2001), Dlugobencky et al (2003), and van Vuuren and O'Neill (2006) regarding the implications of recent trends for the likelihood of the SRES scenarios.
10-411	A	9:29		I am unsure of the purpose here - is it an implied critque (review) of SRES scenarios or a cmparison of modelled radaitive forcings or intercomparison of radiation codes - it has elemnts of all. Need more assessment and less review. sub-sections need to be put more in perspective. [Bryant McAvaney]	ACEPTED – The purpose of the section has been clarified in the introduction (see comment 10-409). The primary purpose of this section is to relate the forcing at the start of the scenario integrations to the present-day forcings given in chapter 2. This section also explores the implications of errors in the AOGCMs for estimates of equilibrium sensitivity and transient climate response.

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10-412	A	9:31	9:44	Is this paragraph necessary here? Surely this belongs in another chapter as its not about future projections? [Catherine Senior]	TAKEN INTO ACCOUNT – Please see response to comment 10-416.
10-413	A	9:31	9:31	I thought this section (10.2) was a useful discussion of some issues that have been not always been discussed up front in the past. However it needs some introductory material to set the context and explain the context and rationale. The section would benefit from this kind of synthesis of the disparate results, both within the section and by adding a few points to the Executive Summary. How much of the spread in model projections can be attributed to the spread in forcing for a given emissions or concentrations scenario? And how much uncertainty in climate projections is introduced by the new information on emissions uncertainty? The identification of these factors as extra steps in the chain of uncertainty is valuable but I think it would be useful to follow this through or in the same way as has been done for other steps such as model uncertainty. [Richard Wood]	TAKEN INTO ACCOUNT – Please see response to comment 10-409.
10-414	A	9:31	10:24	Totally irrelevant material? [Robert E. Dickinson]	TAKEN INTO ACCOUNT – Please see response to comment 10-416.
10-415	A	9:31	10:13	The title of the section does not reflect its content. The emissions of CO2 and CH4 are discussed for China only. No global view is given for the well-mixed species. [Michel Petit]	ACCEPTED – The relationship of global trends for well-mixed species discussed by Hansen and Sato (2001) and by van Vuuren and O'Neill (2006) is discussed in the revised version.
10-416	A	9:31	24:13	Section 10.2 needs a lot of further work to limit its material to that appropriate to this chapter rather than chapter 2. [Robert E. Dickinson]	REJECTED – The material included in section 10.2 is present there at the request and consent of the CLAs of chapter 2 and 10. However, the revised section now includes an introduction explaining which issues are covered and why they are covered in chapter 10 rather than chapters 2, 7, or 8.
10-417	A	9:31		Section 10.2.1.1 This section should start less abruptly, either by summarizing lines or by aaan introductory line that sets out the problem which is addressed by this section. [Gerrit Burgers]	TAKEN INTO ACCOUNT – Please see response to comment 10-409.
10-418	A	9:32	9:53	This paragraph should give a global overview of emissions or explain why a discussion of China and south Asia is adequate for a global understanding of emissions. CO2 emissions dropped in China but surely they must have risen globally (line 34-35)?	TAKEN INTO ACCOUNT – Please see response to comment 10-415.

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				[Michael Winton]	
10-419	A	9:32	10:2	Why does this look only at China? A global perspective or at least coverage of the countries responsible for high coal use India, Indonesia and South Africa would be warranted. Moreover, since 2001, Chinese coal use and SO2 emissions have increased strongly. [Axel Michaelowa]	TAKEN INTO ACCOUNT – Please see response to comment 10-415.
10-420	A	9:32	:56	It is not clear why this "global" section immediately begins with a long discussion of China. It would be helpful to present the global picture first for context, then explain why the focus is then narrowed to China. [Katharine Hayhoe]	TAKEN INTO ACCOUNT – Please see response to comment 10-415. One reason for the focus on China was the SRES projection for SO2 emissions in 2020, in which Asia is projected to become the dominant source.
10-421	A	9:35	9:36	Indicate the period for the indicated decreases of 32% in BC emissions and 21% in SO2 [Patricio Aceituno]	ACCEPTED – The period spanning the decrease is 1996 through 2000.
10-422	A	9:42	9:42	What "emissions reductions" are being referred to? By how much--are these the Chinese ones only? [Michael MacCracken]	ACCEPTED – these calculations by Streets et al (2001) referred to the effects of reductions just between 1995 and 2000 on climate over the next 100 years. It is confusing and irrelevant in the context of the surrounding discussion and has been removed.
10-423	A	9:43		apparently there is an error in (+0.012+/- 0.02) ºC. Is this a value per year ?.. Or it is the value for the 21st century ? [PATRICIO ACEITUNO]	TAKEN INTO ACCOUNT – Please see response to comment 10-422.
10-424	A	9:43		the global mean surface temperature for the 21st century increases by (+0.012 ± 0.02) C due primarily to the reduced cooling by sulfate aerosols This can hardly be called an increase, and can hardly be attributed to anything. [Stephen E Schwartz]	TAKEN INTO ACCOUNT – Please see response to comment 10-422.
10-425	A	9:47	9:48	This reduction of the emissions estimates does not apply to CO2 ?. [Michel Petit]	ACCEPTED – The Streets article considers BC, SO2 CH4, and CO2. The future trends primarily concern well-mixed greenhouse gases.
10-426	A	9:49		change "…emissions SO2…" for "…SO2 emissions…" [PATRICIO ACEITUNO]	ACCEPTED – The change has been made.
10-427	A	9:50	9:53	A relevant reference to add that compares SRES projections of emissions of various species (including SO2) to recent estimates and to more recent projections is van Vuuren,	ACCEPTED – This paper is now discussed in section 10.2.

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				D. and O'Neill, B.C. The consistency of IPCC's SRES scenarios to 1990-2000 trends and recent projections. Climatic Change, in press. The manuscript is available from the authors, e.g. oneill@iiasa.ac.at. [Brian O'Neill]	
10-428	A	9:53	9:53	clarify by adding SO2 to read 'SO2 emissions', Further one could mention here that nitrate aerosols have not been considered in the AOGCM runs and thus the overall aerosol forcing might still be compatible with the other assumption of the scenario - please check, I do not have the Nox emissions for the two scenarios in my mind. [Fortunat Joos]	ACCEPTED, excerpt for the suggested remark regarding compensation between lower SO2 emissions and the effect of nitrate forcing. This remark is speculative.
10-429	A	9:53	9:53	Is this intended to say that the SO2 emissions in these scenarios are "unrealistically large" or to mean that the emissions projections for all of the species (so also CO2) in these scenarios are "unrealistically large"--this needs clarification. [Michael MacCracken]	ACCEPTED – The descriptor SO2 has been added to limit this discussion to SO2 emissions alone.
10-430	A	9:53	9:53	The fact that the results suggest that emissions in A2 and A1b are unrealistically large would seem worthy of the conclusions [Catherine Senior]	ACCEPTED -- Section 10.2 now discusses recent papers (van Vuuren and O'Neill, 2006; Hansen and Sato, 2001, etc) that conclude that emissions in A2 and A1B are too large.
10-431	A	10:1	10:2	Clearly say that smaller sulfate concentrations imply larger radiative forcing and therefore larger temperature responses. [Ronald Stouffer]	ACCEPTED – The text now notes that lowering the emissions in the A1B and A2 scenarios for consistency with current projections would lead to smaller sulfate radiative forcing.
10-432	A	10:4	:5	Estimation of ozone forcing for the 21st century is complicated by the short chemical lifetime of ozone compared to atmospheric transport timescales This is of course true in spades for aerosols. [Stephen E Schwartz]	ACCEPTED
10-433	A	10:8	10:8	What does "A2p" mean? [Michael MacCracken]	ACCEPTED – It has been noted that the A2p scenario is a "preliminary marker" A2 scenario, in the parlance of the SRES scenarios.
10-434	A	10:8	10:9	What is the SRES A2p scenario? It is not part of the original SRES set -- is this a typo or a new scenario, and if the latter then there should be a pointer to where a description of this scenario can be found. [Brian O'Neill]	ACCEPTED – See response to comment 10-433.
10-435	A	10:8	10:8	It seems that A2p stands for A2. [Serge PLANTON]	ACCEPTED – See response to comment 10-433.

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10-436	A	10:14		Mention lower observations of CH4 which lead to lower radiative forcing and less warming. [Ronald Stouffer]	ACCEPTED – The lower forcing is mentioned in section 10.2.2 and discussed further in section 10.4.2.
10-437	A	10:16	10:16	The reference to the FDH method is not very explicit for those who are not familiar with its application. [Serge PLANTON]	ACCEPTED – This section now begins with a brief description of the differences between instantaneous and adjusted forcing.
10-438	A	10:18		Section #10.4.1. I think this section needs to mention how carbon-cycle feedbacks were treated in the TAR (Section 3.7.3 and Box 3.7 of the TAR), and why the results are different. In the TAR, the carbon-cycle feedbacks were estimated to widen the spread in CO2 concentrations by -14 to +31 %, a spread which was already not centered around zero but still had a negative possibility. This section needs to say what it is in the simple parameterisations tested in the TAR that is no more valid. [Corinne Le Quere]	Accepted
10-439	A	10:21	10:23	In Table 10.2.1, insted of " NCAR", " NCAR, CRIEPI" is strongly recommended and NCAR hase already agreed it through the formal MOU between NCAR and CRIEPI. <Note> CRIEPI completed IPCC runs with CCSM3 using the Earth Simulator before NCAR did. CRIEPI sent the data set to NCAR and NCAR merged the CRIEPI data set and the NCAR data set and sent the aggregated data set to PCMDI according to the official MOU of collaboration between NCAR and CRIEPI. Many scientists in NCAR and other research organizations in the world used the data set provided by CRIEPI and they made many excellent paperes already referred in AR4. The internationl collaboration between NCAR and CRIEPI greatly contributed for IPCC AR4. [Koki Maruyama]	ACCEPTED – CRIEPI has been added to the entry regarding CCSM3.
10-440	A	10:22		Change "…,and the FDH.." for "..,"…,the FDH…" [Patricio Aceituno]	ACCEPTED – The word "and" has been removed.
10-441	A	10:23		The values 4.0 and 7.8 W/m2 seem wrong. Should'n they be 0.40 and 0.78 W/m2, as indicated in lines 11 and 12 of the same page ? [Patricio Aceituno]	ACCEPTED – The typographic error has been corrected.
10-442	A	10:26	10:41	It would be helpful to describe the 20th century values for SO2 emissions, and to indicate whether or not any account is taken of the change in the predominant height of emission of SO2 during the 20th century--from near surface with a few day lifetime to elevated stacks with likely a ten day lifetime. [Michael MacCracken]	REJECTED – The history of SO2 forcing is covered in sections 2.4 and 9.2.1. The purpose of discussing the very recent history of emissions is to indicate whether the SRES scenarios are consistent with present-day trends.
10-443	A	10:26	12:24	The description of RTMIP distracts from what should be the main direction of this	TAKEN INTO ACCOUNT – Please

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				chapter. It would be better placed in Chapter 2 and very simply summarized here. [Robert E. Dickinson]	see response to comment 10-416.
10-444	A	10:26	113:13	Most of this section should be moved to Chapter 2 where the intercomparison of radiative forcings (line-by-line and GCM radiation model results) would be more in line with the topics covered in Chapter 2, rather than being part of "Glo bal Climate Projections" of Chapter 10. [Andrew Lacis]	TAKEN INTO ACCOUNT – Please see response to comment 10-416.
10-445	A	10:44	10:46	Regarding Fig. 10.2.1 it is mentioned that.."The graph also shows the IPCC estimate for the forcing between 1850 to 2000 and the model forcings between the start of the model integrationsand 2000". I do not see this in the graph… [Patricio Aceituno]	REJECTED – The graph clearly has separate symbols for the IPCC and model forcings, which are annotated in the figure and explained in the caption.
10-446	A	10:45	10:45	Somewhere in this paragraph the three scenarios should be named and a statement should be inserted that "current emissions trends indicate that scenario B1 is the most likely of these". [Jeffrey Kueter]	TAKEN INTO ACCOUNT – The choice of scenarios is now discussed in the introduction to the chapter.
10-447	A	10:53	10:53	Figure 10.2.1. should show all three marker scenarios. [Jeffrey Kueter]	REJECTED – The paper by Forster on which this figure is based only discussed the A1B scenario.
10-448	A	10:56		nd elsewhere IPCC estimates etc. Terminology should be clarified and made more specific e.g., IPCC 2001 estimates. [Stephen E Schwartz]	ACCEPTED – "IPCC" has been replaced by "IPCC 2001" throughout this discussion.
10-449	A	10:57	10:57	Text needs to be inserted about the unrealism of the 1%/year transient. "Observed increases in the last three decades were 0.42, 0.41, and 0.50%/year, respectively. Use of the 1% transient substantially overestimates the near-term response. Thermal lag estimates of several decades indicate that this overestimation must continue at least until late in the 21st century". [Jeffrey Kueter]	TAKEN INTO ACCOUNT – The choice of scenarios is now discussed in the introduction to the chapter.
10-450	A	11:0		I'm repeating here the comment for Chapter 10, since it is equally as applicable. Relating increased precipitation to wetter conditions in a warming climate is not justifiable, and there are many regional examples, including those at high latitudes, in which the soil moisture dries out due to increased evapotranspiration regardless of the precipitation increase. Why the soil moisture values from the models were not used to address this question directly, regardless of the uncertainties, is a mystery. [David Rind]	Multi-model changes in soil moisture are shown and discussed in regards to changes in the hydrological cycle in Fig. 10.3.9
10-451	A	11:0		I'll also repeat the comment that while Chapter 10 concludes that over most of the globe tropical storms decrease, the individual region discussions here, whenever they mention the topic, forecast increases.	Better coordination with Ch. 8 and 11.

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				[David Rind]	
10-452	A	11:0		Table 10.2.1, last line should read "mean ± std dev" not "mean ± RMS". [Stephen E Schwartz]	ACCEPTED
10-453	A	11:9	11:23	Please explain the implications of this information for the range in climate sensitivity and transient climate response. Does the table suggest that some of the apparent range in climate sensitivity (when expressed as temperature at CO2-doubling), or in transient reponse, isdue to errors in RT? I believe the former is normalized out already, but the latter is not accounted for. You might want to consider discussing (and showing?) how RT could contribute to the ranges of Figure 10,5.6 and 10.3.1 in 2100. It seems as if it could be at least 20% based upon the results shown, and that would be helpful to indicate. [Susan Solomon]	ACCEPTED – The implications for the range in TCR are now discussed in section 10.2. It is true that, if the true 2xCO2 – 1xCO2 forcing for a given model is used to compute its sensitivity, then the error "divides out". However, many groups use IPCC TAR values, in which case it doesn't.
10-454	A	11:13	11:13	According to table 10.2.1 the range of longwave forcing is 1.25 W/m^2 and not 1.24 W/m^2. [Christoph, C. Raible]	REJECTED – The minimum value in the table is 2.99 W/m^2 and the maximum value is 4.23 W/m^2. This gives a range of 1.24 W/m^2.
10-455	A	11:14		and Table 10.2.1 The range in the longwave forcing is 1.24 W m –2 and the coefficient of variation, or ratio of the standard deviation to mean forcing, is 0.13. [Mean is 3.7 W m-2] This is an important finding and it underscores the reason for not taking 2 x CO2 with nominal value of 4 W m-2 as the basis for the definition of climate sensitivity or CO2 forcing as the basis for global warming potentials. [Stephen E Schwartz]	ACCEPTED – This point is now noted in the text.
10-456	A	11:16		shortwave forcing has a coefficient of variation in excess of 2, This is true but the forcing is small, so the absolute variation is quite small, 0.13 W m-2, so the consequence is rather small. [Stephen E Schwartz]	NOTED.
10-457	A	11:16		The text should specify the physical basis for the shortwave forcing from increased CO2. [Stephen E Schwartz]	ACCEPTED – The shortwave forcing by CO2 is caused by the near-infrared bands off CO2.
10-458	A	11:21	11:21	I have calculated the longwave column numbers for six of these models using the PCMDI archive variable "rlftropa_co2". For four of the models (GISS, MPI and the two UKMOs) my numbers agree perfectly with those listed here. For the two MIROC models my numbers are significantly higher: 3.59 W/m2 for hires and 3.66 W/m2 for medres. This could be a very significant difference because these models are low-liers in the table and likely contribute significantly to the standard deviation. [Michael Winton]	ACCEPTED – The values for the MIROC models have been double-checked and corrected where necessary.
10-459	A	12:0		Table 10.2.3. The identification of the experiments (1a to 4a) is unclear since there is no correspondence with the identification of the set of calculation (1a, 1b, 2, 3).	ACCEPTED – The confusion between the numbered list in the text and the

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				[Serge PLANTON]	calculators listed in the table 10.2.2 has been eliminated by replacing the numbered list with a bulleted list.
10-460	A	12:5	12:5	Insert "Summer" before "mid-latitude" [Serge PLANTON]	ACCEPTED --- text inserted.
10-461	A	12:11	12:12	This finding "that there are no sign inconsistencies in the main forcings" strikes me as much too weak. Surely there is no suggestion of sign inconsistencies (I assume this means disagreement in sign) between model and LBL codes for such forcing as CO2, CH4, N2O etc! Surely there is a stronger finding than this, given the good agreement of many (most?) of the forcings. [Robert Colman]	ACCEPTED – We have replaced this statement with a statement regarding the overall accuracy of the GCM codes relative to the LBL codes for the forcing from changes in WMGHGs from 1860 to present.
10-462	A	12:14		Change "student" for "Student" [Patricio Aceituno]	ACCEPTED
10-463	A	12:20	13:1	Are the instantaneous forcings at 200 mb given in Table 10.2.3 consistent with the results in Figure 10.2.3 ? For doubled CO2, Table 10.2.3 gives 4.28 W/m2 and 4.75 W/m2 for AOGCM and LBL calculations, respectively, while Figure 10.2.3 shows values greater than 5 W/m2. [Andrew Lacis]	ACCEPTED – Table 10.2.3 shows the sum of the longwave and shortwave forcing, not the longwave alone. This has now been clarified
10-464	A	12:25	12:25	Figure 10.2.2. should show all three marker scenarios. [Jeffrey Kueter]	REJECTED – The paper by Forster on which this figure is based only discussed the A1B scenario.
10-465	A	12:25	:26	The forcings from doubling CO2 from its concentration at 1860 AD are shown in Figure 10.2.3 at the top of the model (TOM), 200 mb, and the surface. Page 93 Figure 10.2.3 Figure 10.2.3. Comparison of shortwave and longwave radiative forcings for doubling CO2 from its concentration in 1860 for AOGCMs and line-by-line (LBL) radiative transfer codes (Collins et al., 2005b). The figure caption should specify the substance or process responsible for the shortwave forcing. The implication is that the (negative) shortwave forcing is due to doubling of CO2. [Stephen E Schwartz]	REJECTED – The caption states in the first sentence that the forcing is due to "from doubling CO_2 from its concentration in 1860".
10-466	A	12:33	12:34	"The forcing from the feedback from water vapour…". "Forcing" and "feedback" should not be mixed like this. The forcing should be restricted to the externally imposed changes that affect the radiation, and not used in the context of water vapour response. [Robert Colman]	ACCEPTED – The language now indicates that the fluxes are perturbed in response to the increase in water vapor.
10-467	A	12:34	12:34	"… from water vapour in response to doubling CO2…": this is not the water vapour response to a doubling of CO2, but it is an idealised 20% increase. Need to change	ACCEPTED – See response to comment 10-466.

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				wording to reflect this. Could state perhaps that the perturbation is of a magnitude roughly similar to that expected from water vapour changes under a doubling of CO_2. [Robert Colman]	
10-468	A	13:5	13:20	Please explain how the differences in shortwave and surface fluxes might relate to calculated differences in precipitation compared to temperature changes. Could this explain some of the scatter in the precipitation seen in figure 10.5.1? [Susan Solomon]	TAKEN INTO ACCOUNT – Please see response to comment 10-453.
10-469	A	13:10	13:11	One term between "surface" and "forcing" is missing; likely "longwave". [Serge PLANTON]	ACCEPTED – text added.
10-470	A	13:15	13:22	In the TAR, aerosol effects were the largest source of uncertainty in current radiative forcing, particularly the indirect effects of aerosols. The description of the inclusion of aerosol effects is insufficient to judge the relevance of climate model results presented in this chapter to projections of future climate. Suggest that information be given for all climate model results on how aerosol effects are included with particular attention to indirect effects, whether or not an Albrecht effect is included, carbonaceous aerosols, and cold cloud indirect effects. If such effects are not included, then this presents a gap between climate model simulations and actual climate that is currently glossed over in the current draft. [Haroon Kheshgi]	ACCEPTED – A table has been added summarizing the information on aerosol parameterizations, direct forcing, and indirect forcing obtained from the information submitted to the IPCC archive at PCMDI.
10-471	A	13:17		change "…(2003)parameterize…" for "…(2003) parameterize…" [PATRICIO ACEITUNO]	ACCEPTED
10-472	A	13:23		Seems like most of section 10.2.1.2.1 and 10.2.1.2 belong in chapter 2. Just put summary here. [Ronald Stouffer]	TAKEN INTO ACCOUNT – Please see response to comment 10-416.
10-473	A	13:24		10.2.2 It's not my speciality, but I am surprised to see no explicit mention of CH_4 here or elsewhere. Is it not now widely accepted that the SRES estimates for emissions (or at least the resulting atmospheric concentrations) are substantially too high? A cursory examination of the available data indicates that CH_4 concentrations (and therefore presumably emissions) are roughly stable, not increasing rapidly. [James Annan]	ACCEPTED – The lower methane trends are noted in section 10.2.2 and discussed in more detail in section 10.4.2.
10-474	A	13:24		Again a mixture of review of SRES , missing ingedrients etc without an overall summary purpose. [Bryant McAvaney]	TAKEN INTO ACCOUNT -- Please see response to comment 10-411.
10-475	A	13:34	13:40	The decrease of the forcing efficiency of the sulfate aerosol indirect effect has been shown by Boucher and Pham (2002) and Pham et al. (2005) with the LMDZ model. They show a decrease of -960 to -370 W(g sulfate)^-1 during the periode 1860-1990, and then values evolving from -440 to -210 W(g sulfate)^-1 during the period 2000-2100, and for	ACCEPTED – Both papers are now discussed in this section.

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				the various SRES scenarios. This work should should also be mentioned here (at least Boucher and Pham (2002)), not only Johns et al (2003). Boucher, O., and M. Pham, History of sulfate aerosol radiative forcings, Geophys. Res. Lett., Vol. 29, N. 9, 1308, doi:10.1029/2001GL014048, 2002. Pham, M., O. Boucher, and D. Hauglustaine, Changes in atmospheric sulfur burdens and concentrations and resulting radiative forcings under IPCC SRES emission scenarios for 1990-2100, J. Geophys. Res., Vol. 110, D06112, doi:10.1029/2004JD005125, 2005 [Jean-Louis Dufresne]	
10-476	A	13:44	13:44	I assume that the scaling was done to CO2 emissions not concentrations - right? Otherwise, this would not make much sense. [Fortunat Joos]	ACCEPTED – the word "emissions" has been inserted.
10-477	A	13:54	13:55	As indicated in comments above the characterization of the impact of aviation water emissions and contrails needs to be reconciled throughout the report. In some instances, these are characterized as "insignificant" compared to well-mixed greenhouse gases where in others, they are portrayed as very important. For example, the reference from Minnis et al. [Lourdes Maurice]	REJECTED – The discussion of contrails here, in chapter 2 (executive summary and section 2.6), and in chapter 3 (section 3.4.3.3 on cloud cover) all emphasize that the current and projected impacts are tiny. Current and projected forcings are under 0.03 Wm^-2.
10-478	A	13:54		I suggest to change the word "extant" [PATRICIO ACEITUNO]	ACCEPTED – "extant" has been changed to "recent".
10-479	A	13:55	13:56	I do not understand the phrase "…estimate that the radiative forcing by controls will increase…" [PATRICIO ACEITUNO]	ACCEPTED – "controls" has been changed to "contrails".
10-480	A	13:56	13:56	controls" should read "contrails [Robert Colman]	ACCEPTED – see 10-479
10-481	A	13:56	13:56	typo:contrails [Fortunat Joos]	ACCEPTED – see 10-479
10-482	A	13:56	13:56	Replace "controls" by "contrails". [Serge PLANTON]	ACCEPTED – see 10-479
10-483	A	14:5		I suggest to change "… 1/3…" for … 33%...." [PATRICIO ACEITUNO]	ACCEPTED – text changed.
10-484	A	14:6	14:6	Specify what kind of S-aerosol foring (direct-indirect-total) [Fortunat Joos]	ACCEPTED – The forcing is direct shortwave radiative forcing.
10-485	A	14:13	14:15	The sentence "In every simulation …sign," is misplaced (before the sentence on the emission scenario) and is unclear (perhaps incomplete); in addition, the sentence "Tegen	ACCEPTED – These sentences have been combined and reworded.

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				et al …dependent" is useless if the meaning of the previous is that the changes in dust loading for each type of forcing is of opposite sign when the two models are compared. [Serge PLANTON]	
10-486	A	14:14		change "…sign, These…" for "… sign. These…" [PATRICIO ACEITUNO]	ACCEPTED
10-487	A	14:15	14:15	Replace "2005" by "2004". [Serge PLANTON]	ACCEPTED
10-488	A	14:20		Section 10.3 Comment 1) . On the "commitment" analysis used here for the 21st century : This is based on forcing stabilization and there are other ways of looking at this question that are also relevant to changes over the next several decades such as by using maximum feasible mitigation scenarios (Hare and Meinshausen 2005): these are generally larger than forcing stabilization commitments that over timescales of the next several decades to a century. In other words the "commitment" estimates in Table 10.3.2 are more than lower bounds: on longer timescale of course this picture could reverse. Estimates should therefore be made based on the max feasible scenarios for mitigation using eg MAGICC: fitted to AOGCMs as in the past. [William Hare]	Noted. Further scenarios considered elsewhere.
10-489	A	14:20		Section 10.3 Comment 2) Non mitigation scenarios are used solely in the projections and it would seem essential that a range of mitigation scenarios are computed as well either draw from the literature or pathways with realistic forcing that correspond to a range of scenarios: This is particularly important given the cross cutting Article 2 issue and is relevant to WGII and WGIII. The range of the mitigation scenarios should span the literature. [William Hare]	Noted. Further scenarios considered elsewhere.
10-490	A	14:20		Section 10.3 Comment 3) If possible it would be very useful for this section to also outline based on the mitigation scenarios what warming can be avoided over the 21st century. [William Hare]	Noted. Further scenarios considered elsewhere.
10-491	A	14:20		While a single "figure of merit" for a model is hard to arrive at nevertheless it has been (s.g the Murphy et al QUMP) - what happens to results (when merged) if a weighting (figure of merit) is given to each model in arriving at a "consensus" result - if there is not much change then it would be useful to actually say that. [Bryant McAvaney]	Noted. The use of weighting is considered later. Text modified.
10-492	A	14:23		Section 10.3. Here one finds the kind of introduction that is needed for 10.2 (see previous comment). [Brian O'Neill]	Noted. Improvements to 10.2 made.
10-493	A	14:43		Table 10.3.1 is missing	No, it is after the references.

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				[PATRICIO ACEITUNO]	
10-494	A	14:50	14:51	"… it is anticipated that this is true for climate changes also." The sentence strikes me as too strong as it stands. This MIGHT be true for climate change. Do we have any evidence for this? [Robert Colman]	Accepted. 'Might' added.
10-495	A	14:50	14:50	typo: than instead of that [Fortunat Joos]	Yes.
10-496	A	14:54	15:2	Nice discussion. Move to introduction of chapter. [Ronald Stouffer]	Noted. However, this introduces the section.
10-497	A	15:0		Section 10.3.1: It is unclear if the results discussed in this section are taken from a peer-reviewed paper or not. It appears that it is the authors of this chapter who have done the processing and analysis. Is this the case? If so this really should be made clear. Almost the rest of the IPCC 4AR report uses results from published articles, it seems very odd that this isn't done here. The reason I am particularly concerned here is that I am worried that removing the control drift from the projected temperatures changes, as shown in figure 10.3.1, may not always be appropriate, e.g. a drift may be due to a heat flux trend to/from the oceans, which may change with climate change. At the very least the technique should be referenced (as it would presumably be if published in an article). [Gareth S. Jones]	Accepted. 'Derived by the authors' added early. Removing the control drift is a standard approach for extracting the forced response (as now noted).
10-498	A	15:2	15:2	The average reader needs more guidance for appreciating the synergy between chapters 8 and 10 approaches. Precise cross-references are needed. Chapter 8 should refer in particular to box 10.2 which gives an overall view ; this would temper the negative feeling on models value left by a quick reading of chapter 8. [Michel Petit]	Better coordination between Ch. 8 and 10 appreciated.
10-499	A	15:20	15:21	Don't understand the sentence starting 'Clearly, there is a range of model results at each year…' [Catherine Senior]	Noted. Text modified.
10-500	A	15:26	15:26	According to the results of Douville et al (2002, Cl. Dyn., 45-68), there is not an "acceleration" of the hydrological cycle but an" intensification": on average, the water vapour residence time is increased. This result might be emphasized as it is not intuitive (even in the executive summary); see comment n 25. [Serge PLANTON]	Accepted. Text modified here and later. This reference is added.
10-501	A	15:31		and figure 10.3.1 The trends of the multi-model mean temperature vary somewhat over the century because of the varying forcings, in particular aerosol (see 10.2). Because the plotted temperatures differ from model to model because of both different	Noted. The GHG concentrations are now shown. Further information on forcing in the models is not available.

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				forcings and different model sensitivities, time series of the forcings should be shown, and the model sensitivities should be specified. Given the highly differing treatment of aerosol forcing in the several models, and somewhat different GHG forcing it would be recommended that these be shown separately. Indeed, given the differing forcing per amount of gas (and aerosol or aerosol precursor) emissions, it seems mandatory that time profiles of these quantities be shown as well. Ditto for Figure 10.3.2. One really wants to know how much of the differences among the scenarios are due to ghg forcing vs. aerosol forcing. [Stephen E Schwartz]	
10-502	A	15:39	15:54	As discussed in the Summary, I fear that the model projections of 0.64-0.70 over three decades (even 31 years) will be widely misinterpreted as a confident forecast for the near future, especially since you emphasise that emissions uncertainties play a minor role. In fact it seems more likely to me that either the models are overall a little too sensitive, or the net current forcing is slightly wrong. The stated forecast range implies either a clear, albeit modest, acceleration in the warming rate, or (more plausibly) a modest overestimate in the recent historical warming, which would suggest a similar overestimate in the forecast. It would be useful to include the recent hindcast, along with the historical record, in Figure 10.3.1 in order that this can be checked. [James Annan]	Temperature change ranges are being revised.
10-503	A	15:39	15:54	I am not sure it is justified (or even that instructive) to quote responses to 2 decimal places. Examination of the ensemble mean in this way obscures the considerable uncertainty in global mean change evident in the figures. Perhaps there should be a cross-reference to the uncertainties section (10.5). [Matthew Collins]	Noted. The precision is relevant only to comparison between mean values as is now stated.
10-504	A	15:43	15:43	Typo, presumably intended to be 2046-2065 [James Annan]	Yes
10-505	A	15:43	15:43	Replace "20462065" with "2046-2065". [Aiguo Dai]	Yes
10-506	A	15:43	15:43	20462065" should be split... it should be "2046-2065 [Gareth S. Jones]	Yes
10-507	A	15:43	15:43	"20462065" should be "2046-2065". [Chiu-Ying LAM]	Yes
10-508	A	15:43	15:43	Add a "-" between 2046 and 2065. [Serge PLANTON]	Yes
10-509	A	15:43		change "…20462065.." for "…2046-2065…" [PATRICIO ACEITUNO]	Yes

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10-510	A	15:46	15:54	Comparing the mean warming for each of the SRES scenarios in order to identify when projected climate change becomes significantly different across them is probably fine for drawing the kind of general conclusions stated here. However it may be worth considering that distinguishing between other characteristics of the distributions of projections might be possible earlier: e.g., the 5th or 95th percentiles may become substantiall different earlier. This would be important if, e.g., you wanted to know when do different emissions scenarios differ in their risk of exceeding particular thresholds. Again, this may be too fine a point for this specific text, but perhaps has a place somewhere, especially given the work in WG2 (Ch 19 especially) on associating impacts with particular levels of climate change. [Brian O'Neill]	Noted. Such aspects are included in 10.5 too.
10-511	A	15:49	15:49	"early century" here should be defined. Is it the same as "near future" as defined in line 42? [Gareth S. Jones]	Yes. text modified.
10-512	A	15:50	15:50	The numbers here don't add up. "...range of 0.31oC from 1.30oC to 1.73oC,.. ". Either the range is incorrect or one/both of the limits. Please correct this. [Gareth S. Jones]	Yes.
10-513	A	16:1	16:9	Please add the 1% run to this table and indicate whether the scaling breaks down at all for higher and higher levels on long time scales (e.g., doubling and quadrupling). I would like to suggest that some discussion of 2xCO2 and 4xCO2 in other places would be worthwhile as well - i.e., readers would be quite interested to hear if pattern scaling breaks down at the upper end, or if other scalings such as precipitation begin to behave non-linearly (or not). [Susan Solomon]	The table is specifically for the SRES scenarios and plausible climate changes. A1B and A2 span 2XCO2, and 4XCO2 is large beyond the scope of what is intended for this table.
10-514	A	16:3	16:4	The terms in the M formula should be defined. This M-metric is not a commonly used one so it would be worth spending a couple of sentences somewhere explaining what it is and how it should be interpreted. The current description is rather terse. [Matthew Collins]	Noted. Additional comment in text
10-515	A	16:7	16:7	Figure caption needs to explain the shaded ranges. [Richard Wood]	Noted. Explanation added to caption
10-516	A	16:10		Section 10.3.2. This section is strong in that it pays considerable attention to inter-model variability in the multi-model ensemble. However, I see relatively little on natural internal variability, that is the ratio of the climate signal to natural variability. I realize that there are constraints on the length of this section. Nevertheless, I recommend to include a figure that compares internal decadal variability in precipitation and temperature to the climate change signal. [Gerrit Burgers]	Noted. The aim is to present the forced response, as is now stated early on.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-517	A	16:19		Note special case of sea ice regions. [Ronald Stouffer]	Noted. Text is added.
10-518	A	16:21	16:21	I didn't understand "… a range of 0.31 from 1.30 to 1.73 ..." Misprint? (also at p4 l 51) [Richard Wood]	Yes
10-519	A	16:27	16:27	VERY inappropriate to use only the high SRES scenario. Include the other two. [Jeffrey Kueter]	Reject. This figure shows the extreme curves from A2 and Commit. Other results are for the intermediate A1B.
10-520	A	16:36		change "…stratospheric.." for "…stratosphere…" [PATRICIO ACEITUNO]	Yes
10-521	A	16:37		something seems to be missing in the phrase "…but now additionally given its evolution during the 21st century." [PATRICIO ACEITUNO]	Yes. Text is modified.
10-522	A	16:39		change "…period.. The pattern.." for "…period. The pattern…" [PATRICIO ACEITUNO]	Yes
10-523	A	16:48		Add reference to Stouffer 2004 "Time scales of climate response". [Ronald Stouffer]	Noted, reference added.
10-524	A	17:0		Figure 10.3.5 and Figure 10.3.6. These Figures are probably the most important results in this report. The Figures should be as large as possible (three panels per page) and the contour lines (and shadings) should be as clear as possible. In addition if possible there should be corresponding figures showing the uncertainty of these projections. [Masato Sugi]	Noted. We agree, but are constrained by page length. Further results regarding uncertainty are in later Figures. Detailed regional results are in Ch 11.
10-525	A	17:10	17:10	Figure 10.3.4. Use all three markers. [Jeffrey Kueter]	Noted. The choice is discussed in 10.1
10-526	A	17:12	17:15	Please explain the reason for the enhanced equatorial warming, and how enhanced it is. [Susan Solomon]	Noted. Additional comment offered.
10-527	A	17:17	17:17	No need to quote the pattern correlation to 3 decimal places [Matthew Collins]	Noted. The effective precision is one figure in 1-r, that is 0.006.
10-528	A	17:28	17:29	Does this last sentence imply that we are to expect northern hemisphere cooling in the next 50 years? [Matthew Collins]	No, but the text is clarified.
10-529	A	17:34		It is not clear to me the phrase "They aid the efficient presentation of the broad…" [PATRICIO ACEITUNO]	Noted. Text clarified.
10-530	A	17:37	17:37	A little too vague: what are these exceptions? [Serge PLANTON]	Noted. We do not present exceptions
10-531	A	17:40		change "…extratropical winter.." for "…extratropical Northern Hemisphere winter…"	Noted. We don't imply which is which!

No.	Batch	Page:line From	To	Comment	Notes
				[PATRICIO ACEITUNO]	
10-532	A	17:46	17:46	Figure 10.3.6. Use all three markers. If you can do it for Figure 10.3.5, you can do it for all figures in the same suite. [Jeffrey Kueter]	Noted. We limit the presentation as discussed earlier in 10.1
10-533	A	17:48	17:52	Please consider giving some examples of novel and disappearing climates for which there is high confidence, and indicate the degree of confidence in this result more broadly. [Susan Solomon]	Accepted.
10-534	A	17:49	17:52	Needs more or delete. [Ronald Stouffer]	Accepted.
10-535	A	17:50	17:52	This statement would benefit from a bit more specific details. For example, what are the "certain" regimes? [Klaus Keller]	Accepted.
10-536	A	17:51	17:52	I do not understand this sentence. What are novel climates? [Fortunat Joos]	Accepted.
10-537	A	17:52	17:52	I am not sure what is meant by "novel and disappearing climates". [Matthew Collins]	Accepted.
10-538	A	17:54	18:57	Section 10.3.2.2 needs to make sure it carefully references the large amount of assessment on cloud feedbacks in Chapter 8, more than it currently does. In particular, I can see no value in having the total cloud amount map, when Chapter 8 has shown that we need to look at individul cloud types to understand the feedbacks. Perhaps the height-latitude cross section is more use? [Catherine Senior]	Accepted. Further text added.
10-539	A	17:57	17:57	Dai and Trenberth (2004) should be replaced by Dai, A., K. E. Trenberth, and T. R. Karl, 1999: Effects of clouds, soil moisture, precipitation and water vapor on diurnal temperature range. J. Climate, 12, 2451-2473, with corresponding changes to the refs. list. [Aiguo Dai]	Noted. We need to reference new studies, but have added 'references therein'.
10-540	A	18:0		Figure 10.3.8. Figure 10.3.8 a seems very important. Major differences is sign between different models. Total range almost 4 W m-2. The cloud feedback should have a lot of influence on model sensitivity. This subject seems ripe for discussion. Yet doing a search on the document for "Figure 10.3.8", astonishingly I see no such discussion at all. Do any of these models treat aerosol indirect effects, and can those be showing up in the change in cloud forcing? [Stephen E Schwartz]	Accepted. The figure reference has been corrected.
10-541	A	18:1	18:4	Cloud feedbacks depend on the water content of the cloud as well and clouds are characterized by both an area fraction and an optical depth	Noted. Further text added.

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				[Catherine Senior]	
10-542	A	18:3	18:3	There are two references to chapter 8 in this line; the first one could refer to section 8.6 of chapter 8, and the second to section 8.2 of chapter 8. [Sandrine Bony]	Accepted. Sections given.
10-543	A	18:6	18:8	Does figure 10.3.7a presents an average across the multi-model ensemble or the average of only 2 GCMs (as suggested by the title of the figure: gfdl_cm2_1+ncar_ccsm3)? If the results of only 2 GCMs are shown, then I would suggest to remove this figure because it might not be representative of the multi-model ensemble. [Sandrine Bony]	Accepted. Additional models now included.
10-544	A	18:12	18:14	The sentence « It is worth noting that...16 or more). » is not clear and should be rewritten. [Sandrine Bony]	Noted. The actual number used is given.
10-545	A	18:20	18:22	There is no reason for the cloud cover and the precipitation to be correlated. Much of the total cloud cover is not precipitating (in particular low-level clouds). [Sandrine Bony]	Noted. Text amended.
10-546	A	18:24	18:24	Figure 10.3.7. Use all three markers, if available. If not, say so. [Jeffrey Kueter]	Noted. See 10.1.
10-547	A	18:26	10:40	It will be nice to compare the model cloud forcimng of -22.3Wm-2 with observed cloud forcing -18 Wm-2 for 1985 to 1989 (Ramanathan, Ambio, May 1998 issue, p.187-) and the 1988 value ranging from -14 to - 22 Wm-2(Ramanathan et al, Science, 243, 57-63, 1989; Harrison et al, JGR, Vol. 95, 18687-18,783,1990). [Veerabhadran Ramanathan]	Noted. This comparison belongs in Chapter 8, preferably wth newer evaluations.
10-548	A	18:26	18:37	Global mean changes in cloud radiative forcing (CRF) and their link to climate sensitivity estimates are discussed in much more detail in 8.6; it seems unnecessary to discuss them in chapter 10. [Sandrine Bony]	Noted. The link is made to Chapter 8. These values are not shown there.
10-549	A	18:26	18:37	This paragraph should be extensively modified, or dropped entirely. The discussion of cloud feedback should not be in this chapter, but is covered exhaustively in chapter 8. I have no problems with showing cloud forcing changes, but their connection with cloud feedback is not necessarily straightforward (as discussed in section 8 .6), and, for example, it is not accurate to say that it is even necessarilyindicative of the sign of cloud feedback. I think this paragraph should simply point out the cloud forcing changes, and refer to section 8 .6 for cloud feedback discussion. [Robert Colman]	Accepted. The incorrect statement is removed, and the link to Chapter 8 made.
10-550	A	18:26	18:37	The cloud change picture does not really suggest that the change in cloud forcing would be negative - that needs to be explained more fully. [David Rind]	Noted. The map of forcing is not shown as now mentioned.

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10-551	A	18:26	168:37	A detailed analysis of radiative forcing by clouds as a function of height and optical depth is given by Hansen et al. (1997). [Andrew Lacis]	Noted. This is referenced in Chapter 8.
10-552	A	18:30	18:37	The comments on the sign and uncertainity in cloud feedback are much better handelled in Chapter 8. I would suggest removing this and referencing back. [Catherine Senior]	Noted. We wish to show the new results, which are not included there. We refer to Chapter 8.
10-553	A	18:33	18:33	Figure 10.3.7b should read Figure 10.3.8a. [David Sexton]	Yes.
10-554	A	18:33		change "…Figure 10.3.7b.." for "…Figure 10.3.8a…" [PATRICIO ACEITUNO]	Yes.
10-555	A	18:38	18:38	By "Mean change in diurnal range...has been shown to be decreasing" do you really mean "Diurnal range...has been shown to be decreasing"? There seems to be one level too many of differencing here. [James Annan]	Yes. Text modified.
10-556	A	18:39	18:54	Hansen et al. (1995) showed that the observed decrease in diurnal temperature range could only be explained by a combination of GHG increase along with an increase in continental clouds and aerosols. [Andrew Lacis]	Noted. Reference to Chapter 9 given.
10-557	A	18:42	18:43	Dai and Trenberth (2004) should be replaced by Dai, A., K. E. Trenberth, and T. R. Karl, 1999: Effects of clouds, soil moisture, precipitation and water vapor on diurnal temperature range. J. Climate, 12, 2451-2473. [Aiguo Dai]	Noted –but this duplicated previous point so is cut.
10-558	A	18:55	18:55	Figure 10.3.8 Use all three markers, if available. If not, say so. [Jeffrey Kueter]	Noted, as above.
10-559	A	19:0		Figure 10.3.6. Wang et al. (2004) and Wang and Swail (2005a) also show patterns of change in SLP between 1961-90 and 2070-99 (see their Figures 10 and 14, respectively), which are similar to the patterns shown in this Figure. Thus, I suggest citation of these studies also in this section (Section 10.3.2.4; they are already cited elsewhere in this Chapter). [Xiaolan L. WANG]	Noted. Reference added here.
10-560	A	19:2	19:12	Much of this information seems to belong to Chapter 11 [FILIPPO GIORGI]	Noted. We refer to Chapter 11 for discussion of land precipitation. Sahara point cut.
10-561	A	19:4	19:15	Cloud feedbacks are discussed in depth in section 8.6. Could a reference to that section please be added? [Richard Wood]	Noted. Improved referencing included in 10.3.2.2.

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10-562	A	19:11	19:11	Section 10.3.2. Incorrect reference to figure 10.3.7b. It should be figure 10.3.8a. To further clearness of spread of cloud radiative forcing among individual models I suggest to place the bars in order of increase of appropriate values. [Valentin Meleshko]	Yes. We have improved the figure.
10-563	A	19:11	19:13	Section 10.3.2. The whole sentence appears to be a bit clumsy. It requres re-formulation. [Valentin Meleshko]	Accepted. We leave this for Chapter 11.
10-564	A	19:25	19:25	Figure 10.3.9. Use all three markers, if available. If not, say so. [Jeffrey Kueter]	Noted. See above.
10-565	A	19:27	19:39	The mechanism of the precipitation change should be described in Chapter 9, and the projected change in the precipitation should be given here in more detail. The sentence "the reduction of radiative cooling in the lower troposphere that tends to stabilize the atmosphere" is misleading. Destabilization of the atmosphere due to radiative cooling is the main driving force of convection, and a reduction of radiative cooling directly leads to a reduction of precipitation. Stabilization of the troposphere (more warming in the upper troposphere than the lower troposphere) leads to a weakening of tropical circulation. These explanations should be given in 9.5.2 and 9.5.3. [Masato Sugi]	Further discussion of mechanisms in Ch. 9.
10-566	A	19:36	19:39	The Findell Knutson reference makes important points but appears to be out of place here and needs to be given in the context of other just published papers that report teleconnections (mentioned in Chapter 7). [Robert E. Dickinson]	More discussion of land use change added and references.
10-567	A	19:36	19:39	The relationship between the sentences before and after "However" is not clear. [Masato Sugi]	More discussion of land use change added and references.
10-568	A	19:40	19:40	A comment might be added on the slowing-down of the hydrological cycle; something like: "According to Douville et al, there is an overall reduction of the precipitation efficiency and an increase of the water vapour residence time that result in a slower atmospheric hydrological cycle. This last is more intense but is slowed down." [Serge PLANTON]	Noted. Comment included above.
10-569	A	19:41	19:46	Much of this information seems to belong to Chapter 11 [FILIPPO GIORGI]	Noted. The references are not used there.
10-570	A	19:41	19:46	<Please add the following sentense around line 41 in page 19, Chapter 10> Nishizawa et al. (2005) assess the changes in precipitation, evapotranspiration, runoff, and also runoff-to-precipitation ratio for regions over land, showing that the runoff-to-precipitation ratio tends to decrease in mid-continental regions. <Note>	Noted. Reference and statement added.

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				The paper by Nishizawa et al. (2005) mentioned above has been referred in Chapter 10, page 80, line 14 [Koki Maruyama]	
10-571	A	19:41	19:46	Here and elswhere the specific regional assessments need to be compared with what is discussed in WG2, which also gives specific regional assessments. Since WGII is using output from older GCM simulations, in general, great effort must be made not to come to different conclusions in the regional chapters in the two WG reports. [David Rind]	Accepted. We note WGII.
10-572	A	19:47		Is it possible to make any statement (either here or somewhere else in the chapter) on changes instability and thus convection (and convective precipitation)? [FILIPPO GIORGI]	This is mentioned in reference to changes in tropical cyclones in the extremes section
10-573	A	19:48	20:20	This subsection is similar to the more detailed discussions in section "Changes in Variability" page 24, line 15ff. In particular, the discussion of AO/NAO and ENSO could be merged. [Christoph, C. Raible]	Accepted. The discussion is moved.
10-574	A	19:54	19:56	This pattern is also consistent with trends in the annular patterns of each hemisphere (Osborn 2004, Rauthe et al 2004, Carrill et al 2005, Miller et al 2005). This point also applies to p.3 l.38-40 in the executive summary. [Ron Miller]	Accepted. The discussion in 10.3.5 is noted.
10-575	A	20:0		Figure 10.3.10a. This figure is trying to do too much. It is impossible to distinguish the different models and scenarios. I suggest one panel for each scenario. The interpretation of this figure depends also on knowing the differences in the forcings in the several models, further reason for presenting the time series of these forcings. [Stephen E Schwartz]	The point of this figure is to give a general sense of the spread of the model simulations, not to be able to pick out an individual model or scenario.
10-576	A	20:4	20:4	I think the figure panels are in the wrong order. [Richard Wood]	Accepted
10-577	A	20:5	20:20	This section seems to pre-empt the NAO and El Nino sections in which much more detailed discussion is possible. I do not think the multi-model mean results are consistent with an El-Nino-like change and therefore it is not a "basic response" to a warmer climate. [Matthew Collins]	Noted. The paragraph (from 10-20:5 to 20:20) is moved and merged to the section 10.3.5.
10-578	A	20:5	20:20	This part should be described more clearly, explaining close linkage between a change in one part with a change in another. The increase in the static stability in low latitudes has a close linkage with the decrease in Hadley circulation intensity, which is identical with the decrease in low level equatorial easterly wind. The decrease in low level equatorial easterly explains the decrease in SST contrast in longitudinal direction in the equatorial Pacific, i.e. El-Nino like SST pattern in the background tropical Pacific SST change.	Noted. The paragraph (from 10-20:5 to 20:20) is moved and merged to the section 10.3.5.

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				[Tatsushi Tokioka]	
10-579	A	20:7	20:20	This point was made previously by Shindell et al 2001 and Stenchikov et al 2004, who should be cited. Shindell, D. T., G. A. Schmidt, R. L. Miller, and D. Rind (2001), Northern Hemisphere winter climate response to greenhouse gas, volcanic, ozone, and solar forcing, J. Geophys. Res., 106, 7193-7210. Stenchikov, G., A. Robock, V. Ramaswamy, M. D. Schwarzkopf, K. Hamilton, and S. Ramachandran (2002), Arctic Oscillation response to the 1991 Mount Pinatubo eruption: Effects of volcanic aerosols and ozone depletion, J. Geophys. Res., 107(D24), 4803, doi:10.1029/ 2002JD002090. [Ron Miller]	Noted. However, the paragraph (from 10-20:5 to 20:20) is moved and merged to the section 10.3.5.
10-580	A	20:12	20:12	Please change "monsoon overturning circulation" to "monsoon circulation". [Christoph, C. Raible]	Accepted. However, the paragraph (from 10-20:5 to 20:20) is moved and merged to the section 10.3.5.
10-581	A	20:13	20:13	Replace "El Nino-like change" by "El Nino-like change in SST" because later on section 10.3.5 a distinction is made between the SST response and the "ENSOness" which encompasses not only SST but MSLP and precipitation as well. [Gerrit Burgers]	Noted. The paragraph (from 10-20:5 to 20:20) is moved and merged to the section 10.3.5.
10-582	A	20:13	20:13	Tanaka et al. (2005) is referred but not in the list of reference. [Masato Sugi]	Rejected. There is Tanaka et al. (2005) in the list of reference.
10-583	A	20:16		"…El-Nino …basic response pattern" seems to contradict subsequent material (10.3.5 on p 24?) [Robert E. Dickinson]	Noted. The paragraph (from 10-20:5 to 20:20) is moved and merged to the section 10.3.5.
10-584	A	20:18	20:19	"current models are not deterministic yet"? This does not make sense to me - do you mean that the models do not all agree? [James Annan]	Accepted. However, the paragraph (from 10-20:5 to 20:20) is moved and merged to the section 10.3.5.
10-585	A	20:18	20:20	Needs more or delete. [Ronald Stouffer]	Noted. The paragraph (from 10-20:5 to 20:20) is moved and merged to the section 10.3.5.
10-586	A	20:19	20:19	The average reader would benefit from an explanation how the word "deterministic" should be understood. [Klaus Keller]	Noted. The paragraph (from 10-20:5 to 20:20) is moved and merged to the section 10.3.5.
10-587	A	20:47	20:48	Something is strange with this sentence: increase in heat uptake from reduced vertical mixing? [Fortunat Joos]	Accepted: Text clarified
10-588	A	20:48		36. Page 20, line 48 – Could add Manabe, Stouffer, Spelman, Bryan 1991 to Gregory	Accepted: Reference added.

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				2001. [Ronald Stouffer]	
10-589	A	20:57	20:57	Suggest "consistent" rather than "deterministic". [Richard Wood]	Accepted: Text added.
10-590	A	21:0		Section 10.3.4; What about the southern hemisphere MOC? [John Church]	Accepted: Words added.
10-591	A	21:3	21:3	should read "have little to do with SEAICE model physics among CMIP2 models". Otherwise it reads as if sea ice extent is not controlled by the model physics, which is obviously not correct. [Robert Colman]	Accepted: Reference changed.
10-592	A	21:5	21:5	The proper reference for this comment is really Rind, D., R. Healy, C. Parkinson, and D. Martinson, 1997: The role of sea ice in 2xCO2 climate model sensitivity: Part II: Hemisphere dependence of sea ice thickness and extent. Geophys. Res. Lett., 24, 1491-1494. [David Rind]	Rejected: Illustrative example given from A1B only for sake of brevity.
10-593	A	21:9	21:9	Figure 10.3.11, Use all three markers, if available, If not, say so. [Jeffrey Kueter]	Rejected: Illustrative example given from A1B only for sake of brevity.
10-594	A	21:11	21:11	Figure 10.3.12. Use all three markers, if available, If not, say so. [Jeffrey Kueter]	Rejected: It was referred to at page 10-20 line 39
10-595	A	21:11		apparently Figure 10.3.12 is not referred to in the text.. [PATRICIO ACEITUNO]	Taken into account: Reference added to feedback discussion in Section 4.2
10-596	A	21:20	21:22	Section 10.3.3. This is important statement! But it requires further explanation why ice area does decline more rapidly in summertime. [Valentin Meleshko]	Accepted: A1B added to Caption.
10-597	A	21:36	21:36	Figure 10.3.13. What are the forcings? If 1%/year, need to state that it is likely to be an overestimate by a factor of roughly two, for reasons detailed in earlier comments. [Jeffrey Kueter]	Taken into account: Reference has been added to section 5.5.5.2
10-598	A	21:38	21:41	This statement about Greenland is inaccurate and misleading. Krabill et al. (2000) give a net change in Greenland of "1 +/- <5 mm/year" inches per year, which is simply not distinguishable from zero. It is not negative. In the very same issue of Science, Thomas et al. wrote, "The region as a whole has been in balance, but with a thickening of 21 centimeters per year in the southwest and thinning of 30 centimeters per year in the southeast". And, most recently, Johannessen et al (Science, 2005) reported a substantial increase averaged over most of Greenland, 5.4 cm/year(!). Please change the text to reflect these. [Jeffrey Kueter]	Taken into account: Reference has been added to section 5.5.5.2)

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10-599	A	21:38	21:41	Statement about Greenland is misleading in light of other references. Thomas et al., Science 2001 wrote "The region as a whole has been in balance but with a thickening of 21 centimeters per year in the southwest and a thinning of 30 centimeters per year in the southeast". Johannessen, et al., Science 2005 reported an increase in Greenland averaging 5.4 centimeters per year over the entire landmass. [Jeffrey Kueter]	Rejected: Important to keep in projection chapter too.
10-600	A	21:43	21:48	These points are also made in Ch 8 p 35 ll 28-41. Mayve summarise here and point to Ch 8? [Richard Wood]	Accepted: References added to later sections.
10-601	A	21:45	20:48	The study of Ridley et al (2005) seems highly relevant and some more information would help the reader to better assess this. Some of this information is given later (p. 24, l. 3) but to improve flow, one may want to provide this information at this stage. [Klaus Keller]	Accepted: Text clarified
10-602	A	21:45	21:53	37. Page 21, lines 45-53 – Seems to be a mixed message. What is the assessment? [Ronald Stouffer]	Accepted. Sentence rewritten.
10-603	A	21:45	21:50	Ridley argues that Greenland disappears at 4*co2 Toniazzo ALSO showed…. The two arguments seem to contradict to the reader. I am puzzled whether the author do mena to say that the work by Toniazzo EVEN showed…. Or do they mean to say something else. As it stands it is confusing [Roderik S.W. Van de Wal]	Accepted. Sentence rewritten.
10-604	A	21:48	21:50	The reference to Toniazzo is confusing because it could be read to imply that total melting of Greenland could occur at preindustrial levels, rather than the intended meaning, which I believe is that if Greenland melts at higher temperatures, then reducing CO2 to current or past levels would not allow the ice sheet to return. [Michael Oppenheimer]	Accepted: Word removed.
10-605	A	21:48		Why "only" 0.1 Sv? This is a lot - in chapter 6 we argue this is of the magnitude that caused MOC shutdown in H events during the glacial. It is also a magnitude that can cause a shut-down in some models, even if perhaps not in the HadCM3 model used here. [Stefan Rahmstorf]	Rejected: Beyond scope of chapter. Space limited.
10-606	A	21:50	21:52	It might be interesting for the reader to discuss how the Arctic winter circulation is influenced by Greenland's deglaciation. [Christoph, C. Raible]	Accepted: Figure referenced.
10-607	A	21:52	21:52	I suggest that figure 10.6.4 (which covers this very point) goes here, or is referred to here. [Robert Colman]	Taken into account: Extensive discussion occurred in break out section at the Christchurch meeting. Representatives from Chapters 4, 5, 6, 8 and 10 came to a consensus on how

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					to deal with this.
10-608	A	21:54		Section 10.3.4. This section on ocean circulation changes in my view falls well behind the level of discussion in the TAR, and requires substantial improvements (including the associated bullets in the exec summary). In the TAR, the risk of major ocean circulation changes is recognised as a "low probability - high impact" risk. Evaluating such a risk requires a risk assessment approach - a number of publications during the past years have discussed this, e.g. recently Rahmstorf and Zickfeld, Thermohaline circulation changes: a question of risk assessment, Climatic Change 2005 (http://www.pik-potsdam.de/~stefan/Publications/Journals/rahmstorf&zickfeld_2005.pdf). Much of the criticism of the "naive" approach to risk assessment in the Lomborg Report discussed there could equally be applied to this chapter. It is clear that running a number of "best guess" scenarios of climate change is not a feasible way to assess a risk of very low probability - think of making an assessment of the risk of a nuclear power accident. Yet this chapter practically does not go beyond the point that in "best guess" model scenarios the MOC does not break down - that is trivial, as a breakdown is not a "best guess" but a "small risk" scenario. The TAR - and a number of sensitivity studies - have found that freshwater input to the Atlantic is the key uncertainty with respect to future ocean changes, both in terms of how much to expect, and in terms of how sensitive the models are to freshwater. While the weakening found in most models is predominantly thermal, evidence so far suggests that whether a breakdown threshold is crossed or not depends primarily on freshwater. Runoff from Greenland is the "wildcard" in this respect, with Greenland melting over 1,000 years corresponding to an average flux of 0.1 Sv, a value which is both comparable to freshwater release during Heinrich events, and a critical amount causing shutdown (by itself, without previous weakening due to warming or other water sources) in the more sensitive models. Results from models that do not include Greenland meltwater runoff therefore have practically no value in assessing the risk of a shutdown - I would argue that an AOGCM without Greenland runoff is clearly less useful than an EMIC including this runoff, since the AOGCM misses the key process that can determine the outcome. What this chapter therefore needs, in my opinion is: - A careful discussion of what we have learnt since the TAR about where a possible critical threshold lies in models (my assessment is that we have not made much progress in this respect; despite the intercomparison exercises, we still do not really understand why some models are much closer to a threshold than others). - A careful discussion of what we have learnt since the TAR about how much extra freshwater flux could be expected in future; this would integrate both observations and modeling, and both "best guess" and "worst case" estimates.	Accepted: Text changed

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				Now concerning the impacts of a shutdown. Almost all we learn is that it could not cause an ice age. Since nobody has ever suggested it could (except a Hollywood disaster movie that will look a lot less topical in 2007 when our report appears), this is a red herring - could perhaps be mentioned in passing in Question 10.2 (as it is) but not worthy of any discussion in the main text. Instead, we should learn something here about the actual impacts discussed in the scientific literature, and indeed new work has appeared on this since the TAR, e.g. on the dynamic sea-level impact (Levermann et al. 2005), and a number of papers on the ITCZ shift (which seems robust across GCMs and EMICs and is also seen in paleo data). One would hope that marine ecosystem and fisheries impacts will be discussed in WG2. Overall, the consistency of the current state of knowledge with that described in the TAR should be emphasized (the section now reads as if the risk of a THC change is now considered to be smaller than in the TAR, but I see no scientific reason for that, nor do I know whether this is intended), and it should be made more clear where we have made progress in understanding this risk since the TAR (even though that progress has been disappointingly little). [Stefan Rahmstorf]	
10-609	A	22:2		38. Page 22, line 2 – Add "and inhibit the vertical processes" after "increase their stability". [Ronald Stouffer]	Accepted: Wording changed slightly and CO2 figure added to 10.3.2
10-610	A	22:5	22:6	It would help to specify what is constant and at what level. [Klaus Keller]	Rejected: Only an illustrative scenario was used.
10-611	A	22:5	22:5	Please note in the text that "a lower scenario, B1, is more likely, given emissions trends of the past three decades". [Jeffrey Kueter]	Accepted: Specific details added.
10-612	A	22:6	22:6	'Many of the models' is not particularly informative. How many models are run with flux adjustment, and without? [Michael Vellinga]	Noted. No Change Requested
10-613	A	22:8	22:11	Here the exclusion of two models which are "inconsistent with present day observations" (and presumably the two models which show non-climate-change related drifts) raises a tricky issue. All models are, so some extent, inconsistent with present day observations, yet we use them to make projections of all kinds of quantities in other parts of the report (e.g. the multi-model ensemble means). In some studies, authors have attempted to grade models in a continuous fashion and give each a relative weight. This work is still rather experimental so I guess the most even-handed approach is to consider all models equally likely and exclude none from the assessment unless there is some obvious problem (like	Rejected: We can't selectively remove curves from models. This comment is also at odds with comment 10-613.

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				the aforementioned model drift). [Matthew Collins]	
10-614	A	22:8	22:11	The sheer variety of the simulated MOC at 30N shown in Fig. 10.3.14 gives the impression that there is very little consensus among models - even less so than in TAR. Although model deficiencies are referred to in both text and caption, the impression that the modelling effort has moved backward seems hard to avoid from looking at the Figure. The argument that 'the MOC for these models is shown for completeness' seems irrelevant - a model that does not simulate a plausible MOC has nothing to add to this section but confusion. Therefore they ought to be removed from Figure 10.3.14 [Michael Vellinga]	Accepted: Sentence rewritten
10-615	A	22:18	22:20	is associated with SST and salinity changes … Current'. This sentence is not clear (what does 'associated' mean in terms of cause and effect?), nor particularly relevant to the preceding sentences. More importantly it seemingly contradicts what was said in the opening lines of this section (lines 56 p.10-21 line 2 p. 10-22) where MOC weakening is linked to changes in high-latitude surface fluxes of heat and freshwater. Please clarify this sentence. [Michael Vellinga]	Accepted: Text added.
10-616	A	22:19	22:19	Suggest "see sections 8.3.4 and 8.6.3.4 for evaluation of present-day snow cover…" [Richard Wood]	Accepted: Sentence corrected.
10-617	A	22:20	22:22	South of 60N … 2004a)' The syntax of this sentence is incorrect, and from what I can gather does not add anything informative. Suggest to remove this sentence to increase the clarity of the paragraph. [Michael Vellinga]	Accepted: Sentence corrected.
10-618	A	22:21	22:21	"south of 60 N is repeated. [Serge PLANTON]	Noted.
10-619	A	22:28	22:35	In our model result, both the warmer and fresher sea surface water response to increasing CO2 at the high latitudes of the North Atlantic contribute to the weakening of the THC. For detail, see: Zhou Tianjun, Rucong Yu, Xiying Liu, Yufu Guo, 2005, Weak response of the Atlantic thermohaline circulation to an increase of atmospheric carbon dioxide in IAP/LASG Climate System Model, Chinese Science Bulletin, 50(6), 592-598 [Tianjun ZHOU]	Accepted: Sentence corrected.
10-620	A	22:32	10:33	Period is probably in the wrong place, leaving a fragment of a sentence. [Susan Solomon]	Accepted: Sentence corrected.
10-621	A	22:32	22:33	This sentence seems to be incomplete. [Klaus Keller]	Accepted: Sentence corrected.
10-622	A	22:32	22:33	In addition … Wevaer et al 2003).' This sentence is not complete. Please correct.	Accepted: Sentence modified and

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				[Michael Vellinga]	caveat added.
10-623	A	22:33	22:35	However, no GCM has shown this influence, and it is more conceptual than the discussion in the earlier part of the paragraph. This is an example of combining in the same paragraph concusions with very different levels of certainty, and should be avoided (or acknolwedged). [David Rind]	Accepted: Sentence changed slightly.
10-624	A	22:34	22:35	This is a cryptic formulation. As far as I am aware, the view of a 'fundamental coupling' between the Southern Ocean and NADW production is not widely accepted in the community. A more balanced and accurate formulation would use something like 'this suggests the ability of Southern Ocean processes to impact upon NADW production'. [Michael Vellinga]	Accepted
10-625	A	22:34		39. Page 22, line 34 – fundamental – Is "complex" a better word? [Ronald Stouffer]	Accepted
10-626	A	22:35	22:36	Suggest deleting "and its effect on the North Atlantic Meridional Overturning was minimal". It would be preferable to keep the MOC discussion in one place (paragraph strarting p 24 l 44) [Richard Wood]	Accepted: Reference Added
10-627	A	22:37	22:52	Ref.: Schmittner, A., M. Latif, and B. Schneider (2005): Model projections of the North Atlantic thermohaline circulation for the 21st century assessed by observations. GRL, in press [Mojib Latif]	Noted. Word idealized added.
10-628	A	22:39	22:40	Please note re the Covey quote (2003) from the Global and Planetary Change paper: "The rate of radiative forcing increase implied by 1% per year increasing CO2 is nearly a factor of two greater than the actual anthropogenic forcing in recent decades, even if the non-CO2 greenhouse gases are added in….Thus the CMIP2 increasing-CO2 scenario cannot be considered realistic…It is also not a good estimate of future anthropogenic climate forcing, except perhaps as an extreme case…" that the 1% increase is not at all realistic within any policy timeframe. Add in as a parenthetical after "1%/year (an unrealistically rapid rate, at least through 2050, given trends in recent and coming decades). [Jeffrey Kueter]	Accepted. 1° resolution mentioned
10-629	A	22:42		40. Page 22, line 42 – T85 – The oceanic components are not spectral. Use 1 deg. [Ronald Stouffer]	Accepted. Reference added.
10-630	A	22:48	22:49	With the use of the same model as used in Stouffer and Manabe(2003), Chan et al(2005) showed that MOC finally recovers its intensity in both 4x (and 8x) CO2 cases, although the time required for the recovery is about 1600 yrs (and 6000 yrs) respectively. ; < Chan,	Rejected: Text was correct as written

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				W.L and Motoi, T: Response of thermohaline circulation and thermal structure to removal of ice sheets and high atmospheric CO2 concentration, GEOPHYSICAL RESEARCH LETTERS, 32 (7): Art. No. L07601 APR 5 2005> Abstract: The thermohaline circulation (THC) response to ice sheet removal and quadrupling of atmospheric CO2 in a coupled model and the equilibrium thermal structure are examined. After THC weakening, diffusion of heat and salt to the northern North Atlantic at deep layers increases the temperature and salinity there, in response to CO2-quadrupling. Resulting convective instability induces the exchange of warmer, saltier water in deep layers and cooler, fresher water near the surface. This contributes to a gradual increase in the THC intensity, culminating in its complete and rapid recovery due to positive haline feedback overcoming negative thermal feedback on the THC. Removal of ice sheets prolongs the overall recovery and strengthens the final THC due to precipitation changes over the northern North Atlantic and Labrador Sea. Bottom water and high-latitude sea-surface temperatures are higher without ice sheets, leading to a smaller meridional temperature gradient as indicated by Cenozoic reconstructions. [Tatsushi Tokioka]	
10-631	A	22:50	22:50	these simulations' appears to refer to Manabe and Stouffer, but I think it should refer to the previously discussed AOGCM simulation. Please clarify text [Fortunat Joos]	Rejected: Illustrative example given based on available model output.
10-632	A	22:54	22:54	Figure 10.3.14. Use all three markers. [Jeffrey Kueter]	Rejected: Figure comes from published literature. Can't selectively remove outliers either (see also response to comment 10-614)
10-633	A	22:54	22:54	Figure 10.3.14 The graphical presentation of this Figure is not very clear: there appears to be a concentration of models around the observational estimates, but these simulations are difficult to see. What dominates this Figure are the outliers. Suggest to change the presentation of this Figure to better visualise all models, and allow those within the pack to be clearer. [Michael Vellinga]	Noted: Beyond scope of chapter
10-634	A	22:56	23:13	There could be more discussion on the wind-driven Subtropical Cells here. The STCs carry most of the oceanic heat transport (more than the MOC) and determine the ventilation of the tropical thermocline and hence properties of ENSO and tropical variability in other basins. The results of Hazeleger 2005 (Can global warming affect tropical ocean heat transport? Geophys. Res. Lett. in press) show that the South Atlantic STC does not change but the heat transport in the tropical Atlantic responds to the	Accepted: Wording changed

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				weakening basin-wide MOC. In the Pacific STCs do change, but the heat transport remains constant due to compensating gyre and overturning transports. [Wilco Hazeleger]	
10-635	A	23:3	23:4	Technically, I don't think it is the radiative forcing that is dominating, but the global change response (including feedbacks) induced by the radiative forcing. [FILIPPO GIORGI]	Accepted. Sentence removed.
10-636	A	23:4	23:6	In different models …forcing' This sentence is rather cryptic in its conciseness, and does not convey anything that is directly useful, or essential to the paragraph. It could probably be omitted. [Michael Vellinga]	Accepted: Sentence corrected.
10-637	A	23:9	23:9	Delete first "South of 60N" [Richard Wood]	Accepted.
10-638	A	23:15	23:15	Maybe ref 8.4.6, where this is discussed in more detail. [Richard Wood]	Rejected: Reference was in listr
10-639	A	23:19	23:21	Gregory et al 2005b ref was missing. Assuming this is the GRL CMIP paper, it's important to note that the models analysed were a mixture of GCMs and EMICs, several of them NOT AR4 models. [Richard Wood]	Accepted. Sentence corrected
10-640	A	23:21	23:22	Sentence lacks a verb, or ")." is miplaced. Is the stabilisation/increase always on the century timescale that is of interest here? [Richard Wood]	Noted.
10-641	A	23:24	23:41	This section raises an interesting point. It is a widely held view that variations in the MOC are "driven" by changes in high-latitude ocean convection. Yet the shutting off of convection in some models that show only modest reduction in MOC suggests there are other mechanisms which are important. [Matthew Collins]	Accepted: Reference added.
10-642	A	23:29	23:30	In the parenthesis of the sentense, please add " Bryan et al.,2005, Nakashiki et al., 2005)" like the discriptions in Chapter 10, page 22, line 39 and line 42. [Koki Maruyama]	Accepted: Reference added
10-643	A	23:31	23:31	Such a stabilisation run has also been done with HadCM3, which shows an MOC reduction of about 30% at 4xCO2 followed by very slow recovery relative to the control (still 17% weaker than control after 1000 years). Reference: Wood, R.A., M. Vellinga and R.B. Thorpe, 2003: Global warming and THC stability. Phil. Trans. Roy Soc. A, 361, 1961-1975. [Richard Wood]	Taken into account: Wu et al reference moved; Stocker and Raible left as a summary.

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10-644	A	23:33	23:34	Is it appropriate to quote Stocker and Raible (2005) here? It is a New and Views publication, and does not in its own right support the hypothesis of reduced convection in the GIN Sea, mentioned in the preceding sentence. The reference to Wu et al 2005 does not support this either. I suggest to include the reference to Wu et al directly after the text that refers to projected river runoff, to which Wu et al 2005 obviously does refer. [Michael Vellinga]	Noted.
10-645	A	23:37	23:37	Suggest "Complete shut-downs, long-lasting though not permanent,…" For many policy users shut down for 1000 years is effectively permanent. [Richard Wood]	Accepted. Water vapour feedback mentioned.
10-646	A	23:43	23:52	This paragraph discusses feedbacks amplifying forcing for ice ages. It mentions some feedbacks which amplify the response, but without mentioning them all. However it seems remiss not to mention at least the water vapour feedback, which is the strongest positive feedback under the current climate, and would still be expected to be strong during the LGM. [Robert Colman]	Rejected. Text notes that feedback mechanisms are important,..
10-647	A	23:44	23:47	Actually, there is not a 'relatively solid understanding of glacial inception', unless one uses the word 'relatively' to mean 'poorly'. There are at least 5 different mechanisms people have suggested for how the relatively small reduction in insolation at high northern latitudes in summer could trigger an ice age - including suggestions of both NADW increase and decrease! If one of these NADW responses really did occur, it would not necessary represent a small change - recognize that NADW decreases, or at least colder conditions assumed to be associated with them, in the paleorecord take several hundred years to develop, during which time large climate system responses are possible. So calling it a 'small change' is downplaying its importance inappropriately (for the sake of refuting the film!). [David Rind]	Noted: One sentence in paragraph removed.
10-648	A	23:46	24:3	Much of this paragraph duplicates material in earlier paragraphs in the section. Scope to shorten here. [Richard Wood]	Rejected. It is in reference list.
10-649	A	23:51	23:51	The reference Weaver and Hillaire Marcel (2004b) is not in the reference list. Please add, as it is important. [Jeffrey Kueter]	Accepted.
10-650	A	23:51		change "…Hillaire Marcel.." for "…Hillaire-Marcel…" [PATRICIO ACEITUNO]	Accepted. AR4 result was not available at draft stage. It is now used.
10-651	A	23:54		Why give this old value from the TAR, which is inconsistent with the estimates in AR4? And for a risk assessmment, using the "best estimate" is not very useful. If I ask my doctor about the risk of an operation, and he answers: "my best estimate is that all will go well",	Accepted.

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				how useful is this for me to assess the risk? What I want to know is the worst cases and their probabilities. [Stefan Rahmstorf]	
10-652	A	23:55	23:56	I suggest to eliminate definition of Sv. It is defined in the previous page, line 14 [PATRICIO ACEITUNO]	Noted.
10-653	A	24:1	24:13	41. Page 24, lines 1 -13 – Very nice discussion. [Ronald Stouffer]	Accepted. Sentence completely rewritten.
10-654	A	24:1	24:1	This sentence refers to 'a MOC response' This could be made more informative, and I suggest to add what magnitude/percentage weakening (presumably) is seen in that model. [Michael Vellinga]	Rejected. The assessment is based on numerous model runs and sensitivity experiments.
10-655	A	24:8	24:13	The conclusion on the likelihood of an MOC reduction and its abruptness does not seem well enough qualified, given that it is based largely on model runs drive by a single emissions scenario -- A1B. Thus it can really only be a conditional likelihood, that holds only if the world actually follows A1B. A more general statement of likelihood would also need to account for the likelihood of alternative emissions scenarios (with either larger and more rapid temperature change like A2 or less warming like in B1), and the difference in MOC response to these scenarios. [Brian O'Neill]	Accepted. Sentence removed
10-656	A	24:8	24:9	I didn't really understand this sentence, but it seems to contradict the discussion of stabilistaion experiments at p 23 l 26 onwards. [Richard Wood]	Accepted: Sentence added.
10-657	A	24:12	24:12	Another factor has been shown to be important. I suggest adding "Random internal variability or noise (often not present in simpler models) may also be important in determining the effective MOC stabilty (Monahan 2002).". Reference: Monahan, A.H., 2002: Stabilisation of climate regimes by noise in a simple model of the thermohaline circulation. J. Phys. Oceanogr., 32, 2072-2085. [Richard Wood]	Accepted. Sentence reworded
10-658	A	24:15	27:37	Changes in variability: You might consider adding a subsection regarding interannual surface-temperature variability. [Christoph Schar]	Accepted. New subsection 10.3.5.1 is introduced regarding interannual surface air temperature and precipitation variability.
10-659	A	24:17	24:40	Inconsistent, makes no sense. It cites the CMIP 2005 as stating that the majority of models project an El Nino like pattern, and then says that "CMIP2 models showed that the most likely scenario is for no trend". It is also important, whenever citing CMIP, to	Taken into account. Descriptions here are modified. Different results for paper regarding

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				emphasize that it has the wrong carbon dioxide forcing and therefore its models exaggerate climate change. Also please note that scenario B1, which shows little change, is the most realistic. [Jeffrey Kueter]	SST only and paper regarding SST-precip-slp are explicitly mentioned.
10-660	A	24:17		Section 10.3.5.1. I found this section confusing and I think it can be written more clearly. Geert Jan van Oldenborgh explained to me that in most models SST tends to a more El Nino-like pattern, but that often this is not accompanied by a reduction of the SLP gradient of the Southern Oscillation aspect of El Nino. So if one defines ENSO-ness in terms of e.g. a combined SST+MSLP+Precip. index, as as done in Fig. 10.3.15, there is little change in ENSO-ness in most models. If this is mentioned explicitly in section 10.3.5.1, the section would become much more clear. [Gerrit Burgers]	Taken into account. Different results for paper regarding SST only and paper regarding SST-precip-slp are explicitly mentioned.
10-661	A	24:18	24:40	I really don't think the majority of models produce an El Nino-like response. The conclusion of my CMIP2 paper was for the most common model response of no change to either El Nino or La Nina conditions. This is, I think, backed up by the recent assessments of Van Oldenburgh et al (2005) and Merryfield (2005). This is the safest conclusion. [Matthew Collins]	Taken into account. Different results for paper regarding SST only and paper regarding SST-precip-slp are explicitly mentioned.
10-662	A	24:18	24:40	Mention that many models have still problems in simulating ENSO and the climatology realistically [Mojib Latif]	Rejected. This is not relevant to Chapter 10 (covered in Chapter 8).
10-663	A	24:18	24:22	See the comment #4 above. [Tatsushi Tokioka]	Does #4 mean 10-578? Then: Accepted.
10-664	A	24:25		"…most likely no clear trend…" How related to p 20. The overall question of whether or not more El Nino conditions result is important and merits a less haphazard evaluation than given here. [Robert E. Dickinson]	Taken into account. Descriptions here are modified.
10-665	A	24:33	24:34	El Nino-like changes are associated with deeper Aleutian Lows - I.e., polar lows. How is this a 'non-AO' like response? Both the Pacific and Atlantic polar lows go into making up what is the AO response. [David Rind]	Taken into account. Descriptions here are modified.
10-666	A	24:33	24:42	If the potential of the MOC change to cause an ice age is 'often-cited', I think one or two citations should be given. I do not know of any peer-reviewed scientific literature that makes this assertion. The term 'ice age' is often used in popular articles on this issue, but it is used there in a descriptive and informal way, and I don't think readers of such articles are liklely to be interested in the niceties of definition presented here. If there are some claims like this in the scientific literature, then they should be discussed, otherwise the paragraph seems unnecessarily didactic and I think should be deleted.	10.3.4 (Andrew)

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				[Richard Wood]	
10-667	A	24:38	24:40	Many new reconstructions of SST at LGM suggest greater cooling in the western tropical Pacific than the eastern tropical Pacific. Whether the tropical Pacific exhibited a more La Nina-like or a more El Nino-like pattern of SST is still being debated in the proxy data community. Some cores may also represent more local than regional SST changes. See Lea, Science, 297, 202; Koutavas et al., Science, 297,226; and Rosenthal and Broccoli, Science, 304, 219, for more discussion. [Bette Otto-Bliesner]	Noted. This sentence is deleted as this is relevant in Chapter 6.
10-668	A	24:42	24:42	Figure 10.3.15. Scale the results with a 0.5% per year as well to provide a reasonable bracket. [Jeffrey Kueter]	Rejected. Figure has been changed.
10-669	A	24:44	24:53	I think the argument here is misleading. It's the total fresh water added to the North Atlantic from all sources that's important, rather than the contribution from Greenland alone. If there is a threshold in the system, even a tiny amount of extra water input could make a big difference to the outcome (it looks as if that is what happened in the Fichefet et al. 2003 paper). It's important to note here (and possibly also in Ch 8) that a full detemination of the Greenland source is absent from most of the models used in the main projections. [Richard Wood]	Noted.
10-670	A	24:50		42. Page 24, line 50 – Change "In the most realistic" to "Using the most realistic". [Ronald Stouffer]	Accepted.
10-671	A	24:55	25:3	I thought this was a very nice summary paragraph. [Richard Wood]	Noted. Thanks.
10-672	A	25:3	25:3	The thermocline mode whould be explained. [David Rind]	Accepted. Text is modified.
10-673	A	25:5	25:7	Examples of these models should be given. The general impression is that no coupled atmosphere-ocean model gives highly realistic El Ninos in the present day climate, so how good does 'best' mean, and which models does this pertain to? [David Rind]	Rejected. This is not relevant to Chapter 10 (covered in Chapter 8).
10-674	A	25:12	25:17	Why are results from a single model apparently being given equal weight to results from an ensemble? [Robert E. Dickinson]	Accepted. A single model result is deleted.
10-675	A	25:27	25:28	Replace "El Nino-like conditions" by "El Nino-like SST conditions" because later on section 10.3.5 a distinction is made between the SST response and the "ENSOness" which encompasses not only SST but MSLP and precipitation as well. One may even add "but this change may not be accompagnied by a change towards ENSO-like MSLP	Taken into account. "SST" is added.

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				conditions". [Gerrit Burgers]	
10-676	A	25:27	25:28	The conclusion should really be that there is consistent mean response and a large amount of uncertainty remains. A depressing but fair conclusion I think. [Matthew Collins]	Accepted.
10-677	A	25:27	25:30	The summary of the ENSO section is very helpful. It would be very useful if the authors could indicate what the implications of a shift towards mean ENSO-like conditions are likely to be for e.g., precipitation patterns. [Susan Solomon]	Accepted. Text is added.
10-678	A	25:28	25:30	One may add "The changes in ENSO amplitude in the 21st century in the most realistic models are of the same magnitude as the observed and modeled variability of ENSO over the last century." [Gerrit Burgers]	Taken into account. Text is modified.
10-679	A	25:32		This section appears to apply to contemporary system - why in this chapter? [Robert E. Dickinson]	Rejected. This paragraph is an introduction to future ENSO-monsoon relationship changes.
10-680	A	25:33	25:48	Natural interannual variability could also account for changes in the apparent strength of ENSO teleconnections. Please mention Gershunov (J. Climate 2001) who showed that variations in the apparent strenght of the ENSO-monsoon relationship in the observations are not larger than expected for an underlying constant relationship. Oldenborgh and Burgers (Geophys. Res. Lettrs. 2005, doi:10.029/2005GL023110) have extended this showing that the number of precipitation stations in the world with statistically significant decadal variations in the strength of the ENSO teleconnections is compatible with the null hypothesis of constant teleconnections. [Gerrit Burgers]	Noted.
10-681	A	25:38	25:38	After "(Wang, 2002).", insert "In particular, the East Asian Summer Monsoon is unlikely to be strong in El Nino onset years, and unlikely to be weak in the years following onset (Wu and Chan 2005)." This is to inject into the paragraph an indication of the specific relationship between the East Asian Summer Monsoon and ENSO. [Chiu-Ying LAM]	Noted. Let us know reference details of Wu and Chan 2005.
10-682	A	25:42	25:48	43. Page 25, lines 42-48 – This does not add anything. Delete? [Ronald Stouffer]	Rejected. These sentences explain the mechanism of "global warming" hypothesis in the sentence before.
10-683	A	25:46		change "…warming,, the.." for "…warming, the…" [PATRICIO ACEITUNO]	Accepted.

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10-684	A	25:50	25:56	The CNRM model and the ARPEGE-OPA model is the same model. So the sentence "The Arpege-OPA model also show …(Camberlin,2004)." might be suppressed and at line 50, "the CNRM model (Ashrit et al., 2003)" might replaced by "the ARPEGE-OPA model from CNRM (Ashrit et al., 2003; Camberlin et al., 2004)" [Serge PLANTON]	Accepted.
10-685	A	26:0	27:	Apparently an extensive review of contempory variability - why not in chapter 3? [Robert E. Dickinson]	Noted. Information necessary for projections is included here.
10-686	A	26:10	26:16	The Sahel rainfall is treated much more extensive in chapter 11. It would helpful for the reader to give a reference to that chapter. [Reindert Haarsma]	Accepted. This paragraph is deleted as they are for present climate variability.
10-687	A	26:10	26:16	Some publications are related to climate change but the statements seem to only concern present climate variability analyses. They are relevant to an other report chapter. [Serge PLANTON]	Accepted. This paragraph is deleted as they are for present climate variability.
10-688	A	26:10	26:16	Another paper that that examines the Sahel-ENSO relationship is Rowell (2001), which the authors may want to include. This examines the mechanism of this teleconnection in detail, and also shows that decadal variations in the strength of this relationship are not statistically significant. Rowell, D.P., 2001: Teleconnections between the Tropical Pacific and the Sahel. Q. J. R. Meteorol. Soc., 127, 1683-1706 [Dave Rowell]	Noted. This paragraph is deleted as they are for present climate variability.
10-689	A	26:10	26:16	44. Page 26, lines 10-16 – This discussion does not seem to belong here. Delete? [Ronald Stouffer]	Accepted. This paragraph is deleted.
10-690	A	26:11	26:13	Note the Sahel is also just as sensitive to Mediterranean SSTs as it is to the tropical oceans that are mentioned. The 2 references are: Ward, M.N. 1994 Tropical north-African rainfall and worldwide monthly to multi-decadal climate variations. PhD thesis, Univ. of Reading, UK. Rowell, D.P., 2003: The Impact of Mediterranean SSTs on the Sahelian Rainfall Season. J. Climate, 16, 849-862 [Dave Rowell]	Noted. This paragraph is deleted as they are for present climate variability.
10-691	A	26:18	26:21	The summary of ENSO-monsoon relationships is very helpful. Again, it would be helpful if the authors could indicate what the implications of such a change could be for precipitation. [Susan Solomon]	Accepted.
10-692	A	26:19	26:20	The sentence "However … ENSO" seems limited to present climate variability analysis, without reference to climate projection analyses. [Serge PLANTON]	Accepted. This sentence is deleted.
10-693	A	26:34	26:34	(or AO) in stead of (or NAO). In general the discussion of the different modes is a bit	Accepted.

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				confusing especially because of the use of the different acronyms, with the implicit assumption that they are different names for the same thing. A more clear treatment of the different modes would be appropriate [Reindert Haarsma]	Relationship between AO and NAO is described briefly referring Section 8.4.1.
10-694	A	26:34	26:36	In support of this sentence, none of the 14 models analyzed by Miller et al (2005, revised) exhibit a trend toward a lower NAM index and higher Arctic SLP. [Ron Miller]	Accepted.
10-695	A	26:39		45. Page 26, line 39 – Change "response" to "increase". [Ronald Stouffer]	Accepted.
10-696	A	26:42	26:42	Figure 10.3.16. Scale the results with a 0.5% per year as well to provide a reasonable bracket. If this is 1% per year, state so and provide cautionary comment that it is likely to be a substantial overestimation, at least through 2075, owing to growth in emission rates in the near-term decades and thermal lag. [Jeffrey Kueter]	Rejected. Comment is not relevant to this figure.
10-697	A	26:44	26:47	Add to this paragraph "and by Selten et al. (2005) who studied internal variability from a study where a CGCM was run 62 times from slightly different initial conditions but indentical GHG forcing." [Selten et al. (2005) appears on the list of references of Chapter 10 already]. [Gerrit Burgers]	Taken into account. Selten et al. (2004) is added only briefly, as NAO does not appear in warming signal in Selten et al, while positive AO-like signal appears in Yukimoto and Kodera.
10-698	A	26:44	26:47	Selten et al (2005) found in an 62 member ensemble an individual member which reproduces the observed trend in the NAO over the past few decades. The remark about the results of Selten et al. (2005) on page 44 starting at line 45 may be more appropriate here. [Reindert Haarsma]	Noted. See above. Selten et al. (2004) model is low resolution (T31) and top is low (35km).
10-699	A	26:45	26:47	Beside the mentioned literature there are also studies which show that observed trends of the NAO are not different from internal variability, e.g., Wunsch (BAMS, 80, 245-255, 1999), Schneider et al. (J. Atmos. Sci., 60, 1504-1521, 2003), Raible et al. (2005, J. Climate, 18, 3968-3982, 2005). Moreover, Raible et al. (Climate Dynamics, 18, 321-330) give also some explanation for decadal variability in the North Atlantic region as well as some hints for a connection of the North Atlantic circulation and ENSO-like variability. [Christoph, C. Raible]	Noted. Citing of AGCM experiments are deleted.
10-700	A	26:49	26:51	I don't understand this sentence. Does 'simulated change' mean trends in SLP? Is this sentence saying that the spatial pattern of simulated SLP trends varies among the models? (As indirect support of this statement, Figure 6a from Miller et al (2005, revised) shows that the relative contribution of the leading EOFs to 21C change in SLP varies from model to model.) Also, does the phrase 'in spite of close correlations of the models'	Taken into account. Modified sentences accordingly.

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				interannual (or internal) variability with observations' refer to the models' *leading patterns of* interannual (or internal) variability? It seems to me that the correlation of temporal internal variability among the models' and observations is zero, by definition. [Ron Miller]	
10-701	A	26:51	26:52	In Figure 10.3.6, I do not see significant areas (indicated by stippling) in the Arctic. The only significant area is over the Mediterranean Sea. Thus, Fig. 10.3.6 illustrates that only the southern center of action of the NAO is intensified and shifted eastwards. Another point concerns the stippling itself: Is the interpretation correct that the stippling, where the magnitude of the multi-model ensemble mean exceeds the inter-model standard deviation, is similar to the statement that the stippling denotes areas where the changes are significantly different at a level of 66%. If so, it would be helpful to mention this for the scientific readers of IPCC. [Christoph, C. Raible]	Taken into account. A SLP decrease in the Arctic is not significant, although there are few models with increasing SLP in the Arctic. Six out of seven models show a SLP decrease (Osborn et al. 2004).
10-702	A	27:4	27:8	Thanks for citing our article (Miller et al 2005). In the revised version, we've changed our conclusions with respect to this paragraph to account for the behavior of an updated simulation. If I was going to rewrite this paragraph, I would suggest: 'One of the largest NAM increases among the IPCC AR4 models is exhibited by the model with the lowest upper boundary (at 10 hPa), suggesting that NAM can respond to increasing greenhouse gas concentrations through tropospheric processes (Fyfe et al 1999, Gillett et al 2003). Greenhouse gases can also drive a positive NAM trend through changes to the stratospheric circulation, similar to the mechanism by which volcanic aerosols in the stratosphere force positive annular changes (Shindell et al 2001). However, the multi-model annular response of the IPCC AR4 models to volcanic forcing is significantly less than the observed annular change. This suggests that the models as a group underestimate the coupling between the stratosphere and annular changes at the surface, and thus underestimate a mechanism by which NAM responds to greenhouse gas forcing (Gillett et al 2005, Miller et al 2005).' Fyfe, J. C., G. Boer, and G. Flato, The Arctic and Antarctic Oscillations and their projected changes under global warming, Geophys. Res. Lett., 26, 1601-1604, 1999. Shindell, D. T., G. A. Schmidt, R. L. Miller, and D. Rind (2001), Northern Hemisphere winter climate response to greenhouse gas, volcanic, ozone, and solar forcing, J. Geophys. Res., 106, 7193-7210. Gillett, N. P., R. J. Allan, and T. J. Ansell, Detection of external influence on sea level pressure with a multi-model ensemble, Geophys. Res. Lett., 32, L19714, doi: 10.1029/2005GL023640, 2005. [Ron Miller]	Taken into account. Texts are modified accordingly, using some of suggested sentences.
10-703	A	27:7	27:8	Just to complete the list of studies, Yoshimori et al. (J. Climate, 18, in press, 2005) show	Rejected.

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				in an ensemble modelling study of the Maunder Minimum a positive phase of the NAO 1-2 years after a volcanic eruption. Note that the CCSM model is used in its low resolution. [Christoph, C. Raible]	Relationship between NAO and volcanic eruption is not relevant here.
10-704	A	27:12	27:15	I suggest elaborating on this point by noting that 'The response also depends upon the initial stability of the polar vortex; vortices that are overly stable (compared to observations) in the 20C will be less sensitive to decreases in planetary wave absorption forced by greenhouse gases in the 21C.' [Ron Miller]	Rejected. Model underestimation of coupling is added following comment No.10-702 by the same reviewer.
10-705	A	27:19	27:21	The following phrase seems quite irrelevant in the context of the whole report: "A related effect to changes in winds is the length of the day…(de Viron et al., 2002)." [PATRICIO ACEITUNO]	Accepted.
10-706	A	27:21	27:35	Excellent summary of the NAM and SAM. Please indicate what the implications of the changes in NAM and SAM are expected to be for projections of precipitation and temperature. [Susan Solomon]	Accepted. Implication for temperature and precipitation is added.
10-707	A	27:23	27:35	Why is in this section about SAM (or AAO) no reference made to Figure 10.3.6 in contrast to the section about NAM (or AO) and NAO, whereas the changes in SAM are seen very clearly? [Reindert Haarsma]	Accepted.
10-708	A	27:28	27:30	I suggest rewriting this sentence. 'On averge, a larger positive trend is projected *during the late 20C* by models that include stratospheric ozone changes than those that do not....' I also suggest adding: 'During the 21C, when ozone changes are smaller, the SAM trends of models with and without ozone are similar.' [Ron Miller]	Accepted.
10-709	A	27:32	27:33	I suggest appending this sentence with: 'GHG forcing accounts for the positive SAM trend simulated by the IPCC AR4 models during early winter (May-July), when prescribed ozone depletion is comparatively small (Figure 12 from Miller et al 2005, revised).' [Ron Miller]	Accepted.
10-710	A	27:36		A summary statement concerning changes in NAM and SAM would be useful here [FILIPPO GIORGI]	Accepted. Implication for temperature and precipitation is added.
10-711	A	27:39		I did not see any statements concerning changes in variability (not specifically related to variability modes). I know there are papers (Raisanen, JC, 2002; Giorgi and Bi; GRL, 2005) that have looked at this and will allow you to make some statements in this regard.This in fact would be a nice added information compared to the TAR. [FILIPPO GIORGI]	Accepted. New subsection 10.3.5.1 is introduced regarding interannual surface air temperature and precipitation variability.

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10-712	A	27:40		Section 10.3.6 I question the usefulness of putting drought and flooding together in one section. Physically drought is more naturally linked to extreme heat. Policy makers will be thinking drought/heatwaves together and separate from flooding. The progression of section 10.3.6.1 seems odd, drought - areas of reduced mean ppt but increased extreme ppt - areas with increased mean and increased ppt. Surely the first paragraph which talks about flooding should present the most consistent/ most general/largest impact result i.e. increased mean and increased extreme ppt particularly in NH winter. [Simon Brown]	This section has been revised to separate precipitation and temperature extremes, not drought and flood that are ill-defined in the studies assessed.
10-713	A	27:40		Section 10.3.6 As it stands this section is very selective in the results it reports and does not constitute a balanced review of the results published by many climate centres since the TAR. One study is quoted extensively and provides all the figures for the whole section. Not only is this falling short of providing all the information available to policy makers but it also makes boring reading. The phrase "an 8 member multi-model ensemble" or something similar is repeated 6 times in approx 2 pages of text (p27-157, p28-134, p29-121,131,140,143). Perhaps the best solution would be to have an introductory paragraph before 10.3.6.1 which discusses the various types of ensemble used in the extremes section, the Tebaldi 8 multi model, the Clark 05 and Barnett 05 53 member perturbed physics ensemble and the single model ensembles (e.g. Kharin and Zwiers 05). They could then be referenced by name later without the ensemble description. [Simon Brown]	Tebaldi et al. (2005) provide the only multi-model study using the more generatl Frich extremes indices and is thus used as a synthesis of a larger number of studies. Where appropriate, we cite the studies mentioned by the reviewer.
10-714	A	27:40		Section 10.3.6 Figures. The figures for this section come from one study (Tebaldi et al. (2005b)) which, although tidy, restricts the information to policymakers. The results from the 8 models have all been standardised and, although useful for assessing consistency in the modelled signal, removes information on absolute magnitude. Examples of absolute changes in extremes for global fields are contained within Clark et al 2005, Kharin and Zwiers 2005 and Barnett et al 2005 (full ref given in separate comment). Clark et al 2005 and Barnett et al 2005 being based on a 53 member perturbed physics ensemble arguably offer a more systematic sampling of uncertainty than the 8 multi-model ensemble. [Simon Brown]	Tebaldi et al. (2005) provide the only multi-model study using the more generatl Frich extremes indices and is thus used as a synthesis of a larger number of studies. Where appropraite, we cite the studies mentioned by the reviewer.
10-715	A	27:40		Barnett et al 2005 has not been cited in this section at all and should be. This paper reports changes in frequency of temperature and precipitation extremes in a 53 member perturbed physics ensemble due to doubling CO2. Although equilibrium experiments it arguably provides the most systematic sampling of modelling uncertainty on and the effect of such uncertainty on our ability to project changes in extremes. Suggested inclusions of results from this paper come at relevant locations. Full ref: D N Barnett, S J Brown, J M Murphy, D M H Sexton and M J Webb "Quantifying uncertainty in changes in extreme event frequency in response to doubled CO2 using a large ensemble of GCM	Reference now included.

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				simulations" Climate Dynamics (accepted) [Simon Brown]	
10-716	A	27:40		Section 10.3.6 Tebaldi et al. (2005b) is quoted extensively, presumably because it provides a measure of robustness of results due to it's multi model nature. However, 5 of the 8 models have very similar Transient Climate Sensitivity (their table 1) and one pair of models share common history (GFDL-CM2.0 and 2.1) so the range of modelling error sampled is not as large as it may first seem. The results from the perturbed physics ensemble of Clark et al 05 and Barnett et al 05 are not quoted yet they sample modelling error more systematically and probably more thoroughly. Based on this I would ask the authors whether as currently written, it provides the best synopsis of the understood modelling error affecting extreme events. I do not think so and suggest more weight be given to the results of Clark et al 05 and Barnett et al 05 [Simon Brown]	References now included. Tebaldi et al assess results as provided by the modeling groups themselves—there was no subselection.
10-717	A	27:40		Some references and associated comments are more relevant of chapter 11 on regional climate projections (Christensen and Christensen, …). A reference to this chapter should be done as the question of climate extremes is more widely investigated through regional climate projection analyses. [Serge PLANTON]	Reference to Ch. 11 provided.
10-718	A	27:42	28:45	Related to the above, this section should include the discussion of faster increase of precipitation extremes than the mean in a warmed climate, which is, in FOD, summarized in Ch. 9, Page 49, Line 1-15, rather than here. [Seita Emori]	Relation between mean and extreme precipitation discussed here, and coordinated with Ch. 9.
10-719	A	27:43	27:45	Text only mentions increase in chance of summer drying. Burke and Brown 2005 report substantial increases in percentage of land area experiencing drought at any one time, e.g. extreme drought increasing from 1% of present day (by definition) to 30% by the end of the century under A2 emission with HadCM3. I would have thought this is very policy relevant information. [Simon Brown]	Reference added.
10-720	A	27:43	28:3	Changes in frequency of dry days should be kept with the drought paragraph rather than with the precipitation intensity paragraph as it is more relevant to policymakers concerned with drought. [Simon Brown]	The dry days index may or may not relate to drought (different timescales), depending on how drought is defined. Here we simply note the increased risk of drought, and assess the dry days index separately in relation to precipitation intensity.
10-721	A	27:44	27:47	Suggest referencing the more comprehensive discussion of this in Ch 9.5.2.2 [Richard Wood]	Overlap with Ch. 9 has been worked out.

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10-722	A	27:47	27:48	Inclusion of winter wetness in the drought paragraph seems to be mixing issues and making the text unclear. [Simon Brown]	Discussion now revised.
10-723	A	27:47	27:48	Text implies that "summer dryness" is "in most parts of northern middle and high latitudes". This is not consistent with results from the 53 member ensemble of Barnett et al 2005 (fig1) who find ensemble mean summer wetting for eastern Eurasia, north eastern North America and nearly all land above 60N. [Simon Brown]	More care now taken in referring to specific regions.
10-724	A	27:50	27:52	As a part of the mechanism for the "counter-intuitive" coexistence of increased flood and drought risks, general increase in evaporation from soil in a warmer climate should also be mentioned. [Seita Emori]	Agreed. Discussion added.
10-725	A	27:50	28:45	Discussion on dry days is split between two paragraphs p27-l53 and p28-l36to45 [Simon Brown]	Care taken in relation ot time scale of dry days and summer drying.
10-726	A	27:52	27:55	An aspect not mentioned here is that an increases in the frequency of dry days does not necessarily mean a decrease in the frequency of extreme high rainfall events - see Barnett 05 fig 8 and 9 [Simon Brown]	Agreed. Reference added.
10-727	A	27:54	27:55	Barnett et al 2005 should be included in this list particularly as it shows this result is robust to the modelling uncertainty they sample. [Simon Brown]'	Reference added.
10-728	A	27:55	28:2	These two sentences are almost the same just with different references. Lets have one sentence summarising the general result and a list of references at the end. Barnett 05 should be included in this discussion. [Simon Brown]	Reference added.
10-729	A	28:2	28:2	Barnett 05 states" The ensemble simulations reveal a large uncertainty in the expected changes in extremes in most regions…..so it is not generally possible to identify a change in the frequency of extreme precipitation at an individual location with a high degree of confidence." (section3.2 para9 and+G7 fig 6). The degree of uncertainty which this study finds should be reflected in this section. [Simon Brown]	Agreed. Change made to text.
10-730	A	28:2	28:2	Barnett 05 find that the increase in the frequency of seasonal extremes (seasonal mean rainfall) are greater than the increases in the frequency of daily extremes. This should be reflected in the text. [Simon Brown]	Agreed. Change made to text.
10-731	A	28:5	28:17	In a study of rainfall in the U.S. we found that increases in the heaviest classes are	The paper cited deals with 20th century

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				generally proportional to the increase in precipitation as GHG forcing increases. This should be noted somewhere in the text (Michaels P.J., et al., 2004, Int. Jour. Clim. 24, 1873-1882.) [Jeffrey Kueter]	precipitation characteristics, not projections or GHG forcing.
10-732	A	28:6	28:8	Although Emori and Brown (2005) showed more important and relevant information for this section, its citation is quite insufficient. It clearly showed that precipitation extremes would increase more than the mean only over some part of the globle manily in subtropics, and it is attributable to greater thermodynamic increase for the extremes than for the mean, using a 6-member multi-model ensemble. This work would be assessed better in relation to Meehl et al. (2005a), which also seems to discuss the spatial pattern of the change, the relation between mean and extremes, and the causes of the pattern. Particularly, Meehl et al. (2005a) seems to attribute the high-latitude increase in mean and extreme precipitation to atmospheric circulation (dynamic effect), while Emori and Brown (2005) clearly attributes it to increased water vapror (thermodynamic effect). [Seita Emori]	More discussion is added here to compare to Meehl et al.
10-733	A	28:9	28:9	Discussions of improvements in application of extreme value theory should surely include Kharin and Zwiers 2005 [Simon Brown]	Agreed. Reference and discussion now included.
10-734	A	28:12	28:12	Strike Watterson and Dix ref as it is using a clearly unrealistic scenario. [Jeffrey Kueter]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-735	A	28:19	28:20	Strike Watterson and Dix (2005) ref as it is using a clearly unrealistic scenario. [Jeffrey Kueter]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-736	A	28:45	28:45	If consistency is being discussed then Barnett et al 2005 should be included due to their ensemble being larger and sampling modelling uncertainty more explicitly. Barnett 05 only find limited areas of increased frequency of wet days in July (their fig 9) [Simon Brown]	Accepted. Discussion and reference added.
10-737	A	28:47	28:47	Figure 10.3.18. Scale the results with a 0.5% per year as well to provide a reasonable bracket. [Jeffrey Kueter]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction

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10-738	A	28:47		Section 10.3.6. It gives too black picture on change of climate extremes. I believe it is rather superficial and unbalanced view. There is an impession that any climate change leads to harmful consequences and this is not the case. If we identify some extreme phenomena which are dangerous for environment and sosiety, we must say more specifically where and when and how frequent they might occur. But not simly saying "in most regions". The section should be modified. [Valentin Meleshko]	WG1 does not assess dangerous anthropogenic influence. This discussion is intended to identify general characteristics in changes of extremes, and we refer the reader to Ch. 11 for changes in specific regions.
10-739	A	28:49	28:49	Figure 10.3.19. Scale the results with a 0.5% per year as well to provide a reasonable bracket. [Jeffrey Kueter]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-740	A	28:49		Fig 10.3.19 The stippling which is attempting to indicate robustness of a change across models seems to be set at a very low/liberal level "at least 4 of the 8 models show significant change". How many points have 4 models with significant change and 4 without? Surely the policymakers need to see what the robust results are and I do not think this figure is achieving this. I would have thought at least 6 models showing significant changes would show what results are robust. [Simon Brown]	This is a value judgement on the part of the reviewer. We feel that if at least half of the models are consistent, this provides a qualitative idea of what a consistent response is. But there could be many opinions of how to judge consistency, and we maintain this is appropriate for this application.
10-741	A	28:52		Daily temperature extremes were extensively investigated in Clark et al 05 using a 53 member physics ensemble. They find that the whole ensemble produces increased daily temperature maximums for nearly the whole land surface but the range in magnitude of increases is substantial. This is clearly portrayed in their figure 3 which I would suggest to the authors to be a very policy relevant inclusion to the chapter as it conveys both the magnitude and the uncertainty of the changes to policymakers. [Simon Brown]	Accepted. Discussion and reference added.
10-742	A	28:54	28:57	Section 10.3.6. I do not agree with this statement. Analysis of precipitation (total and convective) for A2 scenario using 14 AR4 indicates that significant decrease of total precipitation in drying regions is accompanied by similar decrease of convective precipitation, as well, in summer over Eurasia during the whole 21st century.We could not identify any single drying region where wherw convective precipitation remains unchanged or increased. [Valentin Meleshko]	This comment actually refers to page 27, not 28. This is interesting information but no reference is given.
10-743	A	28:54	29:1	Clark 05 disagrees with the Kharin and Zwiers 05 finding of max Tmax following daily mean Tmax. This is clearly presented in their figure 4a which shows a complex pattern of max Tmax increases greater and smaller than changes in the mean Tmax	Noted. Qualification added in revised text, and reference added.

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				[Simon Brown]	
10-744	A	29:0	31:	Section on extremes relevant to coastal areas is good. [Robert Nicholls]	Noted. Thank you.
10-745	A	29:3	29:4	Barnett 05 echo these findings with nearly all land areas experiencing increases in frequency of extreme JJA temperatures by at least 20 times and in some areas 100 times more frequent. These results are many times greater than the ensemble spread making it a very robust result. The text should reflect this. [Simon Brown]	Agreed. Discussion and reference added.
10-746	A	29:3	29:3	Which criteria are used to define a"extreme warm season" ? [Michel Petit]	Definition is given in preceding sentence.
10-747	A	29:3		change "…21st century; the probability.." for "…21st century the probability…" [PATRICIO ACEITUNO]	Accepted.
10-748	A	29:8	29:8	Please indicate whether the "winter-time mean" is a running mean or the mean wrt a base period. [Gerrit Burgers]	
10-749	A	29:13	:23	You might want to add that Schär et al. (2004) found an increase in interannual surface temperature variability in RCM scenario simulations (Central Europe, summer season). This would imply an increase in the frequency of extreme warm conditions, as the statistical distribution of mean summer temperatures is not merely shifted towards warmer conditions but also becomes wider. [Christoph Schar]	Accepted. Discussion and reference added.
10-750	A	29:16	29:19	Clark et al 05 do not substantiate the findings of Meehl and Tebaldi 2004 . Clark 05 do not find any consistent circulation changes which drive the increase in heat waves, rather changes in soil moisture is found to dominate. [Simon Brown]	Accepted. Discussion added and reference added.
10-751	A	29:16	29:19	Contrary to Meehl and Tebaldi 2004, Brabson et al 2005 and Clark et al 05 find changes in soil moisture the most significant driver for the changes in heat waves - see Brabson 05 fig 2 and Clark 05 fig 8 [Simon Brown]	Accepted. Discussion added and reference added.
10-752	A	29:21	29:23	Clark 05 find the intensity of 1 in 20 year 10 day heat waves increase for nearly all land points (their fig 6 a and b), with those few points showing little increase occurring only in the tail of the ensemble distribution. The range of potential intensity increase is however large. The text should reflect these results. [Simon Brown]	Accepted. Discussion added and reference added.
10-753	A	29:25	29:25	Figure 10.3.20. Scale the results with a 0.5% per year as well to provide a reasonable bracket.	A clarifying paragraph elaborating on the use of idealized and SRES emission

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				[Jeffrey Kueter]	scenarios has been added to the Introduction
10-754	A	29:27	29:27	Figure 10.3.21. Scale the results with a 0.5% per year as well to provide a reasonable bracket. [Jeffrey Kueter]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-755	A	29:27		Figure 10.3.21 top right panel. The figure or the accompanying text does not give any indication of changes in intensity of heat waves which is very policy relevant. I suggest replacing the top right figure with Clark 05 fig 6 a) and b). the justification of this is i) it provides policy relevant changes in intensity of heatwaves ii) it portrays more clearly the level of modelling uncertainty in such projections than the stippling of the current figure iii) the larger ensemble of Clark 05 provides a more systematic sampling of modelling uncertainty than the 8 models used in the current figure. [Simon Brown]	There are two types of uncertainty here. One is related to inter-model uncertainty, with we illustrate in the figure. The other is parameter uncertainty in one model, which is shown in Clark et al. 2005. We now mention contributions from this uncertainty in the revised text, but maintain that inter-model undertainty is more relevant for the discussion here.
10-756	A	29:29	29:33	The analysis of DTR should not be placed in this section but in section 10.3.2.2. In addition, figures 10.3.20 and 10.3.21 are not related to DTR. [Serge PLANTON]	Accepted.
10-757	A	29:30	29:30	The implication of this sentence is that Stone and Weaver (2002) had examined a variety of models to examine DTR changes. This is not the case, they only looked at one model. Either this sentence should be re-phrased or more references should be added to show that the change in DTR is seen in a variety of models. [Gareth S. Jones]	Accepted.
10-758	A	29:31	29:31	The figure referenced here is incorrect. Fig 10.3.20 is changes in frost days, heat waves and growing season, NOT Diurnal temperature variations as expected in this sentence. The correct figure is needed. [Gareth S. Jones]	Accepted.
10-759	A	29:31	29:31	Reference to fig 10.3.20 irrelevant, except if Fig is complemented by a fourth panel [Michel Petit]	Accepted.
10-760	A	29:33	29:33	The figure referenced here is incorrect. Fig 10.3.21 is changes in frost days, heat waves and growing season, NOT Diurnal temperature variations as expected in this sentence. The correct figure is needed. [Gareth S. Jones]	Accepted.
10-761	A	29:33	29:33	reference to fig 10.3.21 irrelevant, except if Fig is complemented by a fourth panel	Accepted.

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				[Michel Petit]	
10-762	A	29:33		Obviously, Fig. 10.3.2 does not show the change in DTR. Furthermore, the general comment concerning the change in DTR is not consistent with the earlier discussion, and also not consistent with some of the regions discussed in Chapter 11. [David Rind]	Accepted.
10-763	A	29:35	29:45	Should growing season length and diurnal temperature range be included in the extremes section? They are not really extreme events and the space given over them relative to other high impact extremes (extreme temperature & heat waves) seems disproportionate (GSL & DTR taking about half that taken by extreme warm temperatures). Given the extremes section is so short and space is tight I think the space should be used for more mainstream extremes. [Simon Brown]	DTR discussed in 10.3.2.2 as now noted in text. Growing season length is one of the Frich extremes indices and thus assessed as related to extremes with important impacts.
10-764	A	29:47		At the end of section 10.3.6.3, there should be a summary and conclusion of these experiments. It seems that experimental results have reached some consensus: reduction in the total number of tropical cyclone and intensification of tropical cyclones (increase in the number of intense tropical cyclones) in the warmer climate. [Masato Sugi]	Accepted.
10-765	A	29:48	29:53	Some recent modelling studies do not show future increases in wind speeds (e.g. Hasegawa and Emori 2005 and Bengtsson et al. 2005). [Ruth McDonald]	Already noted for Hasegawa and Emori, Bengtsson results now added.
10-766	A	29:48	29:53	Not all of the models show future increases in tropical storm wind speed (e.g. Bengstsson et al 2005 and Haseagawa and Emori 2005). [Ruth McDonald]	Already noted for Hasegawa and Emori, Bengtsson results now added.
10-767	A	29:51	29:51	Figure 10.3.18 and 10.3.19. For precipitation intensity units must be indicated. [Valentin Meleshko]	These are normalized indices.
10-768	A	29:55	29:57	Bengtsson et al. (1996) simulated a reduction of tropical cyclone frequency in the future warmer climate in the T106 ECHAM3 model (about 100km resolution AGCM). Recently… [Masato Sugi]	Rejected. We are emphasizing new results since the TAR.
10-769	A	29:55	30:25	This part cites a lot of time-slice AGCM works, rather than AOGCM works. It should be mentioned somewhere, since the title of this section is "10.3.6. Future Changes in … Global Coupled Climate Models". Or, it would cause confusion. [Seita Emori]	Accepted.
10-770	A	29:55	30:8	I think that the results of Bengtsson et al. (2005) should be mentioned here, to be consistent with section 8.4.3, even though the model used has a low resolution and the cyclones aren't limited to those with warm cores. Benstsson L, Hodegs KI and Roeckner	Accepted.

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				E (2005) Storm tracks and climate change. Submitted [Ruth McDonald]	
10-771	A	29:56	29:56	change "some characteristics" to "more spatial details" Also change "resolution" to "grid spacing" [Thomas Knutson]	Accepted.
10-772	A	29:57	29:57	change "tropical cyclones" to "tropical cyclone frequency" [Thomas Knutson]	Accepted.
10-773	A	30:2	30:2	"indicated global decreases ..." -> "indicated decreases ..." "the tropical north Pacific …" -> " the western North Pacific" as Hasegawa and Emori (2005) only showed the results over the western North Pacific basin. [Seita Emori]	Accepted.
10-774	A	30:2	30:2	delete "global" and change "north" to "northwest" [Thomas Knutson]	Accepted, and we now use "western North Pacific"
10-775	A	30:5	30:8	This is a very big point to be given such a firm conclusion based on this study. Why tropcal cyclone numbers should decrease in a warmer climate is not explained here, or elsewhere. Furthermore, in Chapter 11, many of the regions discussed conclude that there will be an increase in tropical storms, often for both frequency and intensity. Given the heightened sensitivity of this issue, a much fuller representation, with many more caveats, is necessary. Handling (or mishandling) the question of future tropical cyclone is one of the major flaws in this chapter. [David Rind]	Clarification added for stabilization of atmosphere with increased CO2 contributes to decreased numbers in some areas (Yoshimura and Sugi, 2005). Better coordination with Ch. 11.
10-776	A	30:12	30:17	The first sentence of this part seems to be for Sugi et al. (2002), while the second is for Hasegawa and Emori (2005). The author seems to be confused and mixing up the two results. By the way, I guess this paragraph can be combined with the above to reduce redundancy. [Seita Emori]	Accepted.
10-777	A	30:12	30:17	This is confusing. The sentence beginning "A time slice"… I think actually refers to Sugi et al paper discussed in the previous paragraph. The last sentence alone refers to Hasegawa and Emori, but again that was covered in the previous paragraph. So I would delete both these sentences, unless I'm missing something here... [Thomas Knutson]	Accepted.
10-778	A	30:12	30:17	The Haseagawa and Emori (2005) study is only for the WN Pacific. I think the reference is incorrect here. [Ruth McDonald]	Accepted.
10-779	A	30:23	30:24	This finding needs additional explanation, since it does not appear to be logical. An	Accepted—results of Yoshimura and

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				increase or decrease in the number of both strong and weak tropical cyclones is understandable, but what physical mechanism would cause an increase in strong cyclones but a decrease in weak ones? As presented it appears to be a model artifact. [Lenny Bernstein]	Sugi (2005) cited for competing effects of temperature stabilization and SST increase.
10-780	A	30:23	30:24	This conclusion is not intuitively obvious. The reader could rationalize either an increase or decrease in the number of both strong and weak tropical cyclones, but how does one explain an increase in strong cyclones and a decrease in weak ones? If a physical mechanism can not be provided to explain this apparent contradiction, the conclusion should be dismissed as a model artifact. [Jeffrey Kueter]	Accepted—results of Yoshimura and Sugi (2005) cited for competing effects of temperature stabilization and SST increase.
10-781	A	30:26		A summary statement on changes in tropical cyclones would be useful here. [FILIPPO GIORGI]	Accepted.
10-782	A	30:26		Related to the previous comment, it would be useful to provide some physical explanation as to why the number of cyclones is projected to decrease in many models but the peak intensity to increase. [FILIPPO GIORGI]	Accepted—results of Yoshimura and Sugi (2005) cited for competing effects of temperature stabilization and SST increase.
10-783	A	30:26		46. Page 30, line 26 – What is summary of thinking on tropical cyclone changes? [Ronald Stouffer]	Summary added.
10-784	A	30:27	31:14	This section could be much more synthetic; in its present form it is more a review of the literature than an assessment. [Sandrine Bony]	Summary added
10-785	A	30:27		Section 10.3.6.4 Another method of storm track analysis is a storm frequency index based on daily maximum 10m wind speed. This type of analysis has been applied to the ECHAM4/HOPE-G model by Fischer-Bruns et al. (2005) (Fischer-Bruns I, von Storch H, Gonzalez-Rouco JF and Zorita E Modelling the variability of midlatitude storm activity on decadal to centruy time scales. Climate Dynamics (2005) 25:461-476). [Ruth McDonald]	Accepted.
10-786	A	30:27		Section 10.3.6.4 The overall message of this section isn't particularly clear. I suggest that results of all of the studies are grouped together by type of change (e.g. frequency, regional changes and shift in tracks and intensity). [Ruth McDonald]	Some re-writing of this section has occurred to take this into account.
10-787	A	30:28	30:37	To broaden the discussion of future changes in midlatitude storm, Fischer-Bruns et al. (Climate Dynamics, 21, 461-476) conclude that cyclones characteristics are decoupled from temperature and external forcing (sun, volcanoes, greenhouse gas forcing) in simulations of the past 1000 yr, but for continued scenario simulations cyclone frequency parallels the temperature increase. In contrast, Kharin and Zwiers (J. Climate, 18, 1156-1173, 2005) find in their simulations no significant changes of midlatitude cyclone	Accepted, except that Raible et al. Is indeed beyond the scope of this chapter (and no reference is given.

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				characteristics with a small tendency to a reduction of cyclone intensity. This view is supported by a study (Raible et al., Climate Dynamics, submitted, 2005) who find in simulations of a cold climate state an intensification of strong mitlatitude cyclones. In a linear sense one would assume that midlatitude cyclone intensity will decrease in a warmer climate state (as projected by the scenario simulations). Maybe the last mentioned reference is beyond the scope of this chapter. [Christoph, C. Raible]	
10-788	A	30:39	30:39	Replace "Geng and Sugi (Geng and Sugi)" with "Geng and Sugi (2003)". [Xiaolan L. WANG]	This sentence has been re-written.
10-789	A	30:51	30:54	No reasons for this contrasting response between NH and SH are explained. Is this due to contrasting response of lower atmosphere in polar regions in both hemispheres? [Tatsushi Tokioka]	Mechanism noted related to change in meridional temperature gradients.
10-790	A	31:1	31:1	An order of magnitude of these polar shifts would help the reader [Michel Petit]	Accepted
10-791	A	31:2	31:3	Already Schubert et al. (Climate Dynamics, 14, 813-826, 1998) showed a polward shift of cyclone frequency in scenario simulations, thus this study should be mentioned. [Christoph, C. Raible]	Accepted.
10-792	A	31:10	31:10	Replace this line with "Wang et al. (2004), Wang and Swail (2005a and 2005b), Caires et al. (2005) have shown that for most regions" (see also Comment #32-34 below). [Xiaolan L. WANG]	Accepted.
10-793	A	31:15		A summary statement would be useful here. [FILIPPO GIORGI]	Accepted.
10-794	A	31:16		I wondered if it made more sense to move section # 10.4 between current sections # 10.2 and #10.3. This is because carbon cycle and chemistry generate uncertainties about the projected radiative forcing and I wondered if this was best dealt with directly after section # 10.2. However, I concede that talking about projected forcing, then projected response, and then dealing with the uncertainties afterwards is also logical - but I thought I'd mention it anyway. [David Sexton]	Rejected We prefer to keep the chapter logic as it is now.
10-795	A	31:30	31:30	Point out that scenario A2 is the most unrealistic of the three used generally in this chapter and that the results should be scaled proportionally to the relative change of temperature indicated by B1. [Jeffrey Kueter]	Rejected See scenarios section in the introduction.
10-796	A	31:34	31:35	I thought a large contribution to the positive climate carbon feedback was related to soil uptake processes. Perhaps this could be mentioned as well. [FILIPPO GIORGI]	Accepted Sentence rephrased

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10-797	A	31:34	31:36	The solubility effect is relatively small compared to many other plausible mechanisms. Please mention the other important mechanisms,e.g., as discussed in Joos et al., Science, 1999 or Joos et al, GBC, 2001 [Fortunat Joos]	Accepted Sentence rephrased
10-798	A	31:34	31:36	This sentence describes a feedback mechanism from oceanic carbon cycle. It should be noted that there are also feedback mechanisms from terrestrial carbon cycle, such as possible reduction of NPP due to water stress and enhanced degradation of soil organic carbon due to warming. [Michio KAWAMIYA]	Accepted Sentence rephrased
10-799	A	31:34	31:36	It would help to explain that the projected changes in the terrestrial carbon sink would not compensate for the oceanic changes. [Klaus Keller]	Accepted Sentence rephrased
10-800	A	31:34	31:37	This sentence implies that carbon cycle feedbacks to climate result solely from changes in ocean CO2 solubility. This is only part of the answer. In fact C4MIP simulations have shown that a greater portion of the total feedback is attributable to the terrestrial carbon cycle than the ocean carbon cycle -- i.e. climate changes lead to weakened terrestrial carbon sinks as a result of both decreased vegetation productivity and increased soil carbon loss. [Damon Matthews]	Accepted Sentence rephrased
10-801	A	31:46	31:46	Please give error bar that comes with TAR estimate for completness and traceability. [Fortunat Joos]	Accepted Error bar added
10-802	A	31:49	31:50	It would be more reader-friendly if the author could provide examples of "non-climate feedback uncertainties". [Michio KAWAMIYA]	Accepted Sentence rephrased
10-803	A	31:51	31:51	Knutti et al., CD, 2003 have also considered carbon cycle-climate feedbacks in a probabilistic way. [Fortunat Joos]	Noted
10-804	A	32:6	32:12	Is it possible to scale the uncoupled carbon models to forcings so that the comparison can be better interpreted, at least for a few models? The reader will be looking to find out how much additional forcing is likely to result from carbon feedbacks for a given amount of RF. [Susan Solomon]	Accepted Sentence added
10-805	A	32:27	32:28	Would perhaps be better expressed as "...models ignore the effect of land cover change". This is not philosophically equivalent to assuming the effect to be zero, even if the resulting model design and output is the same! [James Annan]	Accepted

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10-806	A	32:27	32:46	I am aware of two EMIC model studies that have addressed the question of the net effect of historical land-use changes on global temperature, considering both changes to the land surface (albedo, sensible/latent heat etc) and historical emissions of carbon dioxide from land-use change. These are: Brovkin V. et, al. (2004) Role of land cover changes for atmospheric CO2 increase and climate change during the last 150 years. Global Change Biology, 10, 1253-1266; and Matthews, H. D. et al (2004) Natural and anthropogenic climate change: incorporating historal land cover change, vegetation dynamics and the global carbon cycle. Climate Dynamics, 22, 461-479. Additionally Sitch, S. et. al. (2005) Impacts of future land over change on atmospheric CO2 and climate. Global Biogeochemical Cycles, 19, GB2013, has looked at this same issue in the context of land-use and climate change over the next century. [Damon Matthews]	Rejected The effect of land use over the historical period is treated in Chapter 2.
10-807	A	32:33	32:33	The citation to Defries et al (2004) seems to refer to Defries et al (2002) in the references. [Klaus Keller]	Noted
10-808	A	32:40	32:41	There is an inversion between "(2004)" and "(Déqué et al., 1994)": "..AGCM (Déqué et al., 2004) … Maynard et al. (2004)". [Serge PLANTON]	Noted
10-809	A	32:48	32:48	Figure 10.4.1. Misleading because only Scenario A2 is used. [Jeffrey Kueter]	Rejected. A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-810	A	32:48		A summary statement on the importance of land use change vs. GHG forcing would be useful. This has been a widely debated issue in the past that needs some solid assessment and possibly some solid conclusion (also to add to the executive summary). [FILIPPO GIORGI]	Taken into account Summary sentence added.
10-811	A	32:50	32:50	Figure 10.4.2. Misleading because only Scenario A2 is used. [Jeffrey Kueter]	Rejected
10-812	A	32:52	32:52	Figure 10.4.3. Misleading because only Scenario A2 is used. [Jeffrey Kueter]	Rejected
10-813	A	32:54	32:54	Somewhere here it needs to be noted that the model scenarios for methane are almost certainly wrong. It is well known that the growth rate declined to near zero in the last 15 years and that in two of the last five years it was actually negative. Some text about "recent data indicates that scenarios for rapidly increasing methane are almost certainly wrong, at least in the near term". [Jeffrey Kueter]	ACCEPTED – The text now includes a reference to the executive summary of chapter 2 where the decrease in CH4 growth rates is discussed, and it includes a statement that this decrease is not consistent with the SRES

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					scenarios.
10-814	A	32:54	32:54	The title of the section does not reflect its content.CH4 is mentionned in the last sentence only, page 34, line 3. [Michel Petit]	ACCEPTED – This section now includes discussion of how recent methane trends compare to the SRES scenarios, the recent Dentener et al paper that presents more realistic projections of methane out to 2030, and the work by Schindell et al on growth of wetlands.
10-815	A	32:54		10.4.2 In this section, there is extensive discussion of ozone, but almost no mention of the much more significant CH4, despite the title. The apparent disconnect between the SRES (and TAR) and reality must be tackled. [James Annan]	TAKEN INTO ACCOUNT – Please see responses to comments 10-813 and 10-814.
10-816	A	32:54		Section 10.4.2. This section should discuss the more recent results from the large model intercomparison exercise reported by Stevenson et al. (2005). This study assesses new emission scenarios for 2030 in comparison with the SRES A2 scenario and analyzes the corresponding tropospheric ozone budget from 25 atmospheric chemistry models, chemistry transport as well as chemistry climate models. It also studies the coupling between climate change (STE and water vapour feedback) and ozone and estimates the associated radiative forcings. The paper is already referred to in Chapter 7 when discussing the present-day budgets of ozone and precursors: D.S. Stevenson et al. (2005), Multi-model ensemble simulations of present-day and near-future tropospheric ozone, J. Geophys. Res., accepted. [Twan van Noije]	ACCEPTED – The Stevenson paper is now discussed in detail, and a new figure (10.4.4) has been added showing the changes in troposphere ozone burdens between 2000 to 2030 from this study.
10-817	A	33:14	33:14	Replace "Haglustaine" by "Hauglustaine". [Serge PLANTON]	ACCEPTED
10-818	A	33:14	33:14	Change to "Hauglustaine". [Twan van Noije]	ACCEPTED
10-819	A	33:30	33:30	Change "zone" to "ozone". [Twan van Noije]	ACCEPTED
10-820	A	33:30		change "…The zone is also.." for "…The ozone is also…" [PATRICIO ACEITUNO]	ACCEPTED
10-821	A	33:35	33:35	Change "non-methyl hydrocarbons" to "non-methane hydrocarbons". [Twan van Noije]	ACCEPTED
10-822	A	33:37	33:37	See comment n 36. [Serge PLANTON]	ACCEPTED

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10-823	A	33:37	33:37	Change to "Hauglustaine". [Twan van Noije]	ACCEPTED
10-824	A	34:0	35:	Appears to be a sumamry of basic model materials that would logically be in Ch. 8? [Robert E. Dickinson]	While there is some overlap with Chapter 8, we believe a short introduction to the hierarchy of models *placed in the context of uncertainty* is needed here to help the reader interpret the material which follows without needing to make multiple references to other chapters.
10-825	A	34:0		10.5 Overall this is a good summary of the recent research. I have misgivings about one aspect in particular, which is particularly prevalent in this section and "Box 10.2", but pops up elsewhere too. What I object to is the presentation of a particular set of "observationally constrained" estimates in such a way as to indicate that they are really the "right" answer (or at least a particularly important and useful one). In the first paragraph of 10.5.4.4, a wide range of observational constraints are mentioned. However, by the time we get to Box 10.2 Figure 2 and the associated text, the description "observationally constrained" is broadly restricted to the studies that attempted to use the recent large-scale warming to constrain climate sensitivity using what amounts to little more than energy balance arguments. It has long been clear that such attempts are doomed due largely to the limited knowledge of the forcing (eg both Knutti et al 2002 and Gregory et al 2002 make this point), and it is misleading to present these results as if they are particularly privileged or valuable. All of the other estimation methods use observations too! Repeatedly presenting the fact that this type of study does not rule out a climate sensitivity of >6C even at the 66% level does not, in my view, present a realistic or helpful assessment of the uncertainty, even though I acknowledge that alternative figures are also presented. Various lines of evidence point to a substantially lower estimate: for example volcanic forcing (Wigley et al 2005) and paleoclimate data (many refs) clearly indicate lower values as being most likely. Even when allowing parameter values to vary widely, few complex GCMs have been constructed with such high climate sensitivity, and those that have are generally found to be implausible when checked out in more detail (and it's worthwhile to note that the substantial errors they have are very much in line with what would be expected of an overly sensitive model) - for example, the recent analyses of the climateprediction.net results which you cite, the Yokohata et al paper examining the response of the high sensitivity MIROC3.2 model to volcanic forcing, as well as our own recent paper using paleoclimate data (Annan et al 2005). Although I acknowledge that many lines of argument do not comprehensively rule out such high sensitivity, they all	We believe the observationally constrained estimates should be presented as a distinct category, not because they are seen to have a superior status (we do not make such a statement anywhere in the Chapter, because there is no basis for such a claim as the reviewer rightly says), but bercause they are distinct in a methodological sense from approaches which appeal to other types of observational constraint, or methods which place more weight on model results in addition to observational constraints. The climate sensitivity box presents the published pdfs in separate categories precisely because it is recognised that the different methods are based on different choices and assumptions. Further work is needed to find out whether the spread indicated by different types of approach can be reduced by combining the information they contain, but the IPCC can only report the current state of the science, which does not yet provide a basis for quantifying the relative merits of the alternative pdfs. Hence the overall

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				point to a lower value being more likely, and one has to ask what is the likelihood that all independent assessments are substantially biased in the same direction. I suggest that the distinction of these so-called "observationally constrained" estimates be dropped. They have no special status as estimators of climate sensitivity, and give a misleading (in my opinion) estimate of the uncertainty of climate sensitivity, in particular the probability of very high values is exaggerated when these methods alone are used. If you wish to present these estimates as a separate category, then I think at least you need to argue a case for it. I note also that there is substantial overlap with Chapter 9.6 and wonder if some aspects of that section could not be usefully drawn into this section – for instance, the comments on volcanic constraints and Maunder minimum (Ch9 p57-58) [James Annan]	conclusions for the range of sensitivity are based on all the published evidence from models and observational constraints, without attaching special significance to any subset of methods.
10-826	A	34:0		sec 10.4.3 This is a very dissatisfying section. Wholly nonquantitative. Of the twenty-three models represented in the multi-model ensemble of climate-change simulations for IPCC AR4, ten include other tropospheric species besides sulphates. Of these, seven have the non-sulphate species represented with parameterizations that interact with the remainder of the model physics. Nitrates are treated in just two of the models in the ensemble. [Stephen E Schwartz]	TAKEN INTO ACCOUNT – Please see response to comment 10-394.
10-827	A	34:8	34:9	There is something wrong in the phrase "An increasing number of AOGCM's are included multiple types of …" [PATRICIO ACEITUNO]	ACCEPTED – "are included" has been replaced by "have included".
10-828	A	34:9	34:9	Replace "are" (second word of the line) by "have" [Michel Petit]	ACCEPTED – "are included" has been replaced by "have included".
10-829	A	34:43		Another source of uncertainty that is worth mentioning is that due to future "unpredictable" natural forcings (solar and volcanic). [FILIPPO GIORGI]	The discussion here is meant to take the reader through uncertainty in prediction the response to future anthropogenic forcing. Inserted "anthropogenic" in the first sentence of 10.5.1 to clarify the scope. Unfortunately there is scant literature on the effect of future volcanoes or solar variability. GISS has considered eruptions during the 21st century but the paper describing these simulations has not been submitted. We have noted recent statistical analyses of the distribution of

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					eruptions over the past 600 years.'
10-830	A	34:54	34:54	It should be pointed out that observations carry uncertainty which will affect the predictions. For instance, if large observational uncertainties are omitted, then one can obtain erroneously strong constraints on the climate prediction. [David Sexton]	Agreed. Text appended to the first paragraph of 10.5.1 to mention observational constraints and the effect of uncertainty in them.
10-831	A	35:0	36:	figure 10.5.2. The figure would be much more powerful and informative to the correlation of the TCR and equilibrium sensitivity if plotted as a bivariate histogram with one on the x axis and the other on the y axis. As plotted the figures are almost a waste of space. However they could be made much more informative if a labeled point were given for each model. And of course we are dealing with small numbers of models, so give the number of models in each 0.2 degree bin on the right hand axis. suggesting a broadly positive correlation between these two quantities similar to that for equilibrium climate sensitivity for gosh sakes, give the x,y plot (with points labeled according to model) and show the regression line. There is no excuse for vague language such as the above when it would take 5 minutes to do the plot and the calculation. [Stephen E Schwartz]	The histograms indeed do not provide substantial new information and Fig. 10.5.2 has been removed. The scatter plot of sensitivity vs. TCR is given in 10.5.1 as requested. Models are not labeled in the figure due to space constraints. However, a table of sensitivity and TCR for all models will be provided in chapter 8.
10-832	A	35:2		"conditional on" is technically incorrect. One conditions on an event. Thus one might say "conditional on the model being a correct representation of the climate system"; one might otherwise change "conditional on" to "partly determined by" and leave the rest of the sentance as it is. [Jonathan Rougier]	Changed "conditional on" to "dependent upon".
10-833	A	35:2		Distributions "are conditional on the quality of the available models" this is a critical caveat that needs to be stressed much earlier. Later text on the following pages and elsewhere implies that a comprehensive accounting of uncertainties can be obtained, this is plainly false. We cannot "assess the consequences of the uncertainties described above" (10-35 5) if that is taken to imply model inadequacies noted in the preceding paragraph. We can only condition on our current understanding. This is one of the concepts within more public presentations of climate work that is most often misinterpreted, and when misunderstood leads non-climate scientists to think our work claims the impossible, and then disregard the real value of climate research. [Leonard A. Smith]	Agreed. Added sentence to opening para in 10.5.1 to stress the effect of structural model inadequacies, and altered the wording of the first sentence of the next paragraph to emphasise that we can only assess teh range of predicted changes consistent with our current understanding.
10-834	A	35:3		If there was room, a further sentence might be helpful here. "These distributions would	A reference to the effects of structural

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				be wider were uncertainty due to structural errors to be incorporated into the models." [Jonathan Rougier]	uncertainty has been included in response to this and comment 10-833.
10-835	A	35:18	35:19	I think you are a bit too harsh with regards to the regional abilities of EMICs. Variants of the GENIE (C-GOLDSTEIN) model have shown credible behaviour at regional scales at least with respect to MOC slow-down and NW European climate etc (and let's face it, there is little evidence that GCMs can give reliable predictions at a much finer scale). The recent GENIE runs were all at 36x36 (equal area) horizontal resolution. Perhaps Hargreaves et al (Ocean Modelling Vol 11 Nos 1-2 p174-192 2006) is relevant here wrt probabilistic estimation of regional climate change. [James Annan]	Changed "examining" to "quantifying" in the relevant sentence: while some EMICS do allow investigation of uncertainties associated with a subset of the processes driving regional uncertainty, they do not possess sufficient resolution or complexity to be used to provide a basis for quantification of the range of possible regional responses in comparison with current AOGCMs. For example, AOGCMs resolve and simulate internal dynamical variability (e.g. that associated with storm tracks) more comprehensively than EMICS.
10-836	A	35:19	35:19	Is there a reference which can be cited to back up the statement that EMICs are suitable for looking at continental scales. Section #8.8 cite Petoukhov et al 2005 but I am not sure if this is suitable here. I also see that Stocker and Knutti, 2003 is used on p.37, line 18 so that could be used again. [David Sexton]	Inserted a reference to Forest et al (2002), and changed "continental scale" to "large scale" to cover zonally-averaged 2-D EMICS as well as coarse resolution 3-D EMICS.
10-837	A	35:28	35:28	The abbrevation TCR is used here before it is defined in line 52 on the same page. [Gerrit Burgers]	Replaced TCR by "transient climate response"
10-838	A	35:32		While AOGCMs may be the only models even capable of realistic simulation of internal variablitity, extreme events, and feedbacks, one should not give the impression that they do in fact do so without an explicit statement of the temporal and spatial scales below which they fail to do so. [Leonard A. Smith]	Added a sentence to this effect.
10-839	A	35:48		Section 5.2.1. Comprehensive GCMs (if these GCMs truly were comprehensive, there would be no need to improve them further) [Leonard A. Smith]	Not taken into account. This is a term that is well established in the community, even if not perfectly correct.
10-840	A	35:50	35:50	I would suggest "is characterized by" instead of "is related to". [Sandrine Bony]	Changed as suggested
10-841	A	35:50	35:51	Climate sensitivity should be defined in the Glossary and possibly repeated, but in the	Changed to be consistent with the

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				same terms in all chapters. Chapter 9,page 53, line 44-45 says "Precise definitions of climate sensitivity are given in the Glossary and Section 8.6.2.1. "Section 8.6.2.1, page 52, lines 40-43 says "As defined in previous assessments (Cubasch et al., 2001) and in the glossary, the global mean surface air temperature change experienced by the climate system after it has attained a new equilibrium in response to a CO2 doubling is referred to as the equilibrium climate sensitivity (unit is K), and is often simply termed the climate sensitivity." [Michel Petit]	definition in the glossary.
10-842	A	35:50		"related to" -> often summarised by [Leonard A. Smith]	Changed to 'characterized by'.
10-843	A	35:53	35:56	I am confused here about the distinction made between equilibrium climate sensitivity and transient climate response. Shouldn't equilibrium climate sensitivity (on account of its by definition longer timescale than the TCR) also rely heavily on oceanic processes? i.e. the atmosphere equilibrates quickly, but the ocean takes longer, thus ocean changes would show up in the equilibrium climate sensitivity more so than the TCR? [Damon Matthews]	The opposite is true. The ocean takes up heat transiently, and the more efficicent this process is, the lower the transient atmospheric temperature response. In equilibrium, ocean heat uptake is zero, and surface temperature is controlled almost entirely by atmospheric feedbacks. See e.g. Knutti et al. GRL 2005. No change on the text.
10-844	A	36:0		Concerning PDF estimates with the use of models, points would be " to prove that models are sampled in random fashion and to show that they can simulate present climate reasonably well". These points should be clearly stated here, referring relevant parts of the following description, if necessary. [Tatsushi Tokioka]	Noted that models are not sampled in a random way. Model evaluation is covered in chapter 8.
10-845	A	36:15	36:15	Again, here's the 1%/year problem. Please note in the text that this dramatically overestimates the transient climate response, and that the quasi-linearity of model response and oceanic lag means that most of the 1% TCR, at least for the next 50-75 years, should, as a first approximation, be halved. [Jeffrey Kueter]	The scenarios used in the chapter are discussed at the beginning of the chapter in the revised version.
10-846	A	36:21	36:21	Figure 10.5.1. Scale the results with a 0.5% per year as well to provide a reasonable bracket. If this is 1% per year, state so and provide cautionary comment that it is likely to be a substantial overestimation, at least through 2075, owing to growth in emission rates in the near-term decades and thermal lag. [Jeffrey Kueter]	The scenarios used in the chapter are discussed at the beginning of the chapter in the revised version.
10-847	A	36:23	36:38	I do not see how we could assume a normal or log-normal distribution for climate sensitivity or TCR. We are not dealing with probability based on frequency and multiple	Taken into account partly. The figure is removed, and less emphasis is given to

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				realizations, but with subjective probabilities. Why should such probabilities follow a normal or log-normal law ? This method would be correct if the observed climate-sensitivity in models were the result of a random process, of which the mean were the actual climate sensitivity. In other terms, if the climate-sensitivity observed in models were the actual one, perturbed by a normal or log-normal stochastic error... There is no clue this should be the case, and it should be stated. [Stéphane Hallegatte]	the fitting of distribution. However, it is helpful to estimate a mean and standard deviation from these numbers, and some justification for the shape of the distribution is given in the text.
10-848	A	36:25	36:27	I remain unconvinced by the value of fitting a normal (or log-normal) disribution to this data and hence don't believe the numbers generated by this. [Catherine Senior]	Taken into account partly. The figure is removed, and less emphasis is given to the fitting of distribution. However, it is helpful to estimate a mean and standard deviation from these numbers, and some justification for the shape of the distribution is given in the text.
10-849	A	36:25		"Assuming normal distributions". I think it would be more accurate to say "Fitting a normal distribution". This is an example of where my first comment might apply. Rather than write "the resulting 5-95% uncertainty range", it would be shorter and no less accurate to write "the resulting 90% CI". [Jonathan Rougier]	Taken into account.
10-850	A	36:27	36:31	There seems to be some contradiction in this paragraph about what is meant by "best estimate". Line 27, it is the median, but on line 31 it is the "most probable value" which is the mode. Maybe one of these statements is wrong. However, I see that it is possible that both statements are in fact correct, in which case, switching from median to mode, has made the text somewhat confusing and it would be good to use just one measure of best estimate. [David Sexton]	Changed to be consistent, best estimated replaced median throughout the the text.
10-851	A	36:28	36:31	The assumption that the current models cover the full range of uncertainty seems to be questionable. This has been discussed in the document to some extend, but a brief mentioning of this caveat would help to avoid misinterpretations. A citation on this issue (e.g., Draper, D. 1995. Assessment and Propagation of Model Uncertainty. Journal of the Royal Statistical Society Series B-Methodological 57 (1):45-97.) may also be useful. [Klaus Keller]	The caveat that the AOGCMs do not cover the full range of sensitivities is discussed explicitly at the end of the same paragraph. No changes to the text.
10-852	A	36:40	36:40	Figure 10.5.2. Scale the results with a 0.5% per year as well to provide a reasonable bracket. If this is 1% per year, state so and provide cautionary comment that it is likely to be a substantial overestimation, at least through 2075, owing to growth in emission	The scenarios used in the chapter are discussed at the beginning of the chapter in the revised version.

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				rates in the near-term decades and thermal lag. Adjust PDFs. [Jeffrey Kueter]	
10-853	A	36:42	36:45	How do we conclude from figure 10.5.1a that the large uncertainty in the upper limit of sensitivity is not so important for the range of TCR? Assuming we can model the rate of ocean heat uptake using a simple constant (kappa) multiplied by the global mean temperature change, then TCR=F/(kappa+alpha) where F is the radiative forcing and alpha is the feedback parameter. If kappa is relatively constant under 80 years 1% CO2 forcing (as found in many studies) then this makes the TCR PDFs less skewed but the skewness is still there (it's just hard to spot). [Matthew Collins]	Paragraph was clarified.
10-854	A	36:42	46:45	Can this very important point be related to the commitment issue, the known current state of SST and its role in climate of the coming decade, and to the fact that models have now been successfully used in hindcasting? All of these factors would seem to suggest that the state of the ocean, and its slow changes, imply that climate should be well defined for the coming decade at least, in the absence of unusual solar or volcanic activity. Your text is close to saying this, but I am suggesting looking at the language to be completely clear. [Susan Solomon]	Taken into account. Reworded to make it entirely clear and referred to commitment section.
10-855	A	36:44	36:44	There is something inconsistent here. Climate still has to approach its presumably unique equilibirium point (which is the measure of the model's climate sensitivity) whether the forcing is a step function (e.g., instantaneous doubled CO2) or a more gradual increase to doubled CO2. There may well be subjective issues of "linearity" of how well a model responds to small or large radiative forcings, but this should not affect the eventual equilibirum point. It has not been demonstrated that the climate system posseses multiple equilibirum states that may depend on the detailed time dependence of the applied forcing, rather than just its magnitude. Perhaps it was intended to simply state that the transient climate response on time scales well short of equilibirum, the model response is not particularly sensitive to model's climate sensitivity. Perhaps there are climate feedback processes that are slow acting (like sea ice and ocean transport interactions) that don't get a chance to be expressed on transient time scales. [Andrew Lacis]	Paragraph rewritten for clarification. Fast vs. slow feedbacks mentioned.
10-856	A	36:47	36:53	The role of boundary-layer cloud processes in the spread of climate sensitivity is discussed in section 8.6. You should refer to it. [Sandrine Bony]	Accepted. Reference provided to section 8.6
10-857	A	36:47	36:53	Uncertainties in feedback processes are dealt with in chapter 8. [Matthew Collins]	Accepted. Reference provided to chapter 8.
10-858	A	36:47	36:47	You need to show some evidence for the importance of boundary layer processes on climate sensitivity (at least a referencee to chapter 8)	Reference now provided to chapter 8.

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				[Catherine Senior]	
10-859	A	36:48	36:53	The critical role of boundary-layer cloud processes for climate sensitivity has also been pointed out for AR4 OAGCMs (Bony and Dufresne 2005). It should be cited as well. [Sandrine Bony]	Accepted.
10-860	A	36:51	36:52	The word "stratus" should be removed as it is the role of low-level clouds in general that has been pointed out by Webb et al. (2005), [Sandrine Bony]	Accepted.
10-861	A	36:53	36:53	Should add a reference to Bony and Dufresne 2005 [Catherine Senior]	Accepted.
10-862	A	36:55		Paragraph starting in line 55 seems to be misplaced…. [PATRICIO ACEITUNO]	Accepted. Moved upward.
10-863	A	37:2	37:2	It is Chapter 9 that is realy being referred to. [David Rind]	Accepted.
10-864	A	37:5	37:45	This section appears to repeat much of the discussion which has already happened in previous sections. [Matthew Collins]	Moved to the commitment section.
10-865	A	37:5	37:44	A caveat should be expressed to the effect that there is no such thing as "free lunch". Anything that is outside of the range for which EMICs have been tuned is suspect. [Andrew Lacis]	Not taken into acount. Caveats on the limitations of EMICs are already given. Many EMICs are close to AOGCMs and are not tuned to more complex models but to observations as AOGCMs.
10-866	A	37:6	37:19	Any flux adjustments in EMICs needs to be stated. [Bette Otto-Bliesner]	Flux adjustments are discussed in chapter 8, where a table is given with all the details for each model.
10-867	A	37:14	37:14	Replace 'others prescribe radiative forcing' by 'others use simplified equations (see chapter 2) to project radiative forcing from projected concentrations and abundances'. To be correct. [Fortunat Joos]	Accepted.
10-868	A	37:18	37:18	Again, hemispheric to global scale seems pessimistic. [James Annan]	Accepted. Replaced hemispheric by continental scale.
10-869	A	37:22	37:22	When I look at figure 10.5.3, I only see 4X and 1%/year. Is something missing? If it stands, it will be an example of only using unrealistic, extreme cases. [Jeffrey Kueter]	The scenarios used in the chapter are discussed at the beginning of the chapter in the revised version.
10-870	A	37:25	37:25	Replace "all determined" with "largely determined" as Fig. 10.5.3 shows that there is not a perfect rank correlation between surface warming and sea level rise.	Accepted.

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				[David Sexton]	
10-871	A	37:32	37:32	Figure 10.5.3. Quadrupling CO2 is highly unrealistic and implies a world with very little new technological development, a rate far slower than in recent centuries. [Jeffrey Kueter]	The scenarios used in the chapter are discussed at the beginning of the chapter in the revised version.
10-872	A	37:38		should be "some EMICs", not "most EMICs" [Stefan Rahmstorf]	Accepted.
10-873	A	37:41	37:44	"The transient reduction of the MOC in most EMICs is similar to the AOGCMs" is true except for Bern2D-CC model. If this model is the same as the slightly differently named Bern2.5D EMIC used in Fig 10.5.1, then doesn't this reduce the credibility of Fig 10.5.1? If so, then should Fig.10.5.1 be omitted? [David Sexton]	Not taken into account. The model version used in 10.5.1 is different and does not show this prominent MOC reduction (see Knutti et al. GRL 2005).
10-874	A	37:44	37:44	Figure 10.5.1. In legend to figure insert "equilibrium" before "climate sensitivity". [Valentin Meleshko]	Accepted.
10-875	A	37:48	41:51	Section 10.5.3 is very hard to follow. There seems little coherence between paragraphs. [Matthew Collins]	This section is being cut and reorganized.
10-876	A	37:48		The range of response from different scenarios actually varies by region as well (i.e., the difference between A2 and B1 over California as simulated by a given AOGCM is not the same as the global A2/B1 difference simulated by the same model, and different again from the A2/B1 difference for the U.S. Northeast) [Katharine Hayhoe]	Global added to title.
10-877	A	37:48		This section seems very long. The probabilistic material is largely covered in 10.5.4. It would also nice to cite the reference where the all equations of MAGGIC are sumarized. It is for the general reader not clear at the moment which of the many references would be the right reference to lookup. [Fortunat Joos]	The probabilistic figure is now omitted and the author of 10.5.4 consulted. There is no single reference because the model has been developed over a number of years.
10-878	A	37:53		"within the long-standing range of 1.5 - 4.5 advocated by the IPCC" A date needs to be attached to this advocacy, was it in the last century? Or the previous chapter? [Leonard A. Smith]	A reference has been added.
10-879	A	38:0		figure 10.5.6-8 Again a very powerful figure, but its value is diminished by lack of knowledge of the forcing time series for the several models, and the aerosol contribution thereto. Similar considerations apply to interpretation of Figure 10.5.12. [Stephen E Schwartz]	The forcing is described in the text and in panel b of the new Figure 10.5.2
10-880	A	38:7	38:7	"TAR Ch 12" should be " TAR Chapter 12". [Chiu-Ying LAM]	OK, thanks.

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10-881	A	38:8	38:9	Be more specific. Strike "over the next few decades" and say, "through 2040, with model results giving no indication of a sudden upturn immediately thereafter". [Jeffrey Kueter]	This statement is from the Allen paper.
10-882	A	38:23	38:23	Who says their assumption was "controversial"? I would certainly agree, but it seems to me that the scientific community adopted their assumption with alactrity (eg Karl and Trenberth, who quote Wigley and Raper's overall result with none of the necessary qualifications). I'm aware that Stephen Schneider has written around the general area, but not in a way that (in my opinion) justifies your statement. If you have some citations, it would be better to add them. [James Annan]	This has been reworded and see below
10-883	A	38:23	38:33	The literature on assigning probabilities to emissions scenarios is assessed in WG2 Ch 2, Section 2.2.3.3, and it may be worth pointing readers to that section somewhere in this text. [Brian O'Neill]	Thank you we will look into this.
10-884	A	38:23	38:24	I recall that this language used in TAR was the subject of many problems. 'Equally likely' is not correct, I think. It would be better as 'plausible' ('equally plausible' may not be right either). Please check this. [Susan Solomon]	This was an assumption made in a paper not in the TAR. Now reworked
10-885	A	38:33	38:33	Figures 10.5.5. The use of all three scenarios is appropriate. Why can't this be done with many of the other figures? [Jeffrey Kueter]	This figure has been dropped
10-886	A	38:45	38:50	This paragraph ("The aim of this section …") appears a little lost here. Either move it to the start of the subsection 10.5.3, or omit. [Gerrit Burgers]	Done
10-887	A	38:45	38:47	Attribution in Chapter 9 and model assessment in Chapter 8 [Catherine Senior]	Yes, thank you
10-888	A	38:46	38:47	the references to chapters 8 and 9 need to be swapped [Robert Colman]	Done
10-889	A	38:53	38:54	Why and how were four models chosen to tune the simple model to? For the final report will it be tuned to all 21 models? If not how will the 'selected' ones be chosen? [Catherine Senior]	We are doing as many as we can
10-890	A	38:56		Should this line refer to Fig. 10.5.6 a (as it does) or to Fig. 10.5.6 d? [Melinda Marquis]	Figures are being changed
10-891	A	39:8	39:8	Please avoid making any judgement within WG1 as to what the uncertainty is in emissions. We are not qualified to do that. We can only say that the range is represented - not whether this represents the real uncertainty in economics, demongraphics, etc.	Noted

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				Please edit this chapter on the next draft to ensure that we deal with our expertise and not that of others. [Susan Solomon]	
10-892	A	39:10	39:21	With so many pdfs for climate senstivity now in the literature, it seems odd for this chapter to produce the results described here using a model tuned to a set of individual AOGCMs that spans a range of uncertainty in climate sensitivity that the chapter has already concluded is not representative of the full range. At a minimum the existing approach could be complemented by comparing it to results using a wider range of climate sensitivities. There a few options. (1) The text currently says a probabilistic approach is necessary, but this is not strictly true. The same deterministic approach as employed now could be used but with simply a larger range of climate senstivities used in the SCM, that instead of representing only AOGCMs also represented illustrative climate sensitivities based on the new work. For example it could draw on Box 10.2 to use a representative high and low climate sensitivity (1.5 C and 6 or 7 C) to more fully span the range. (2) A probabilistic approach faces the difficult problem of probabilities for emissions scenarios, but one could go part way by showing probabilistic repsonse uncertainty to individual SRES scenarios (i.e., a conditional probabilistic projection, conditional on a given emissions path). For example, later in the chapter this type of result is shown for two SRES scenarios (fig. 10.5.17) and these outcomes could be compared to the deterministic ranges shown in fig. 10.5.6 (a-c). [Brian O'Neill]	• For probabilistic uncertainty the chapter now draws on a later section. This section only attempts a sensitivity study. Yes, the chapter will use such an approach in a later section
10-893	A	39:17	39:17	This "do not span the full range" is again based on the provocative assumption that only the so-called "observationally-constrained" estimates are really valid. See my commments to 10.5 for more on this. One could also plausibly assert that the probability that ~20 independently designed and tested climate models all fall on the same side of the true value of climate sensitivity is only 1 in 2^19, or 2 chances in a million (ie, the true value is almost certainly bracketed by the ensemble)! You could simply say that it is not certain (or not clear etc) that this set of GCMs brackets the response of the climate to anthropogenic forcing. [James Annan]	The wording will be relooked at
10-894	A	39:24	39:24	Fig. 10.5.8 seems to make the assumption that the indirect aerosol forcing has a value of -0.8Wm-2. It is not clear whether this assumption has been made for all 35 SRES scenarios, and if so, why? Surely, indirect aerosol forcing is a very uncertain, very large forcing which should be accounted for in future projections. There needs to be more text either here or in caption of fig. 10.5.8 clarifying what was actually done to indirect aerosol forcing and the affect this has on the results.	We will try to use the best estimate from ch 2 but if it gives a result incompatible with observations we will fall back on the SAR value of –0.8 Wm-2. If possible we will comment on the

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				[David Sexton]	effect of aerosol uncertainty.
10-895	A	39:26	39:26	No reference is given inside the brackets. Include correct reference. [Gareth S. Jones]	Done
10-896	A	39:26	39:26	Insert reference in "( )". [Chiu-Ying LAM]	Done
10-897	A	39:26	39:26	reference missing [David Rind]	Done
10-898	A	39:26		there is a missing reference [PATRICIO ACEITUNO]	Done
10-899	A	39:27	39:27	This sentence is incomprehensible. How was the volcanic forcing included in the 20th century simulations? [Peter Stone]	Rewritten
10-900	A	39:28	39:29	What the dickens does this mean? And why was it necessary?? Please elaborate—otherwise readers are going to think that the result is created by tuning a model that can't estimate the two most recent decades. Perhaps you need to state specifically what the offsets are between the models and the mean for the last two decades. [Jeffrey Kueter]	Rewritten
10-901	A	39:31	39:41	Uncertainty in past natural forcing and future anthropogenic forcing is discussed here. What also should be mentioned is future natural forcing uncertainty, i.e. it is pretty impossible to know if any major volcanic eruptions will occur or if the Sun changes brightness significantly. This is examined in a paper which looks at different future emissions scenerios and looks at the impact of future possible natural forcings impacts. This should be at least referenced, (as is done in Chapter 8 pp65), C. Bertrand, JP Van Ypersele, A. Berger, "Are natural climate forcings able to counteract the projected global warming?", Climatic Change, 55, 413-427, 2002. [Gareth S. Jones]	Thank you, reference is being followed up.
10-902	A	39:38	39:40	This sentence implies that the Sato et al 1993 dataset is flawed in some sense because it doesn't have as good a match (although no mention is made about what it is compared with). If a particular forcing dataset when applied to a model then causes the model not to have as good a match to observations does not imply that the forcing is wrong/incorrect or in error. Other reasons could be that other forcings have not been included or have not been applied correctly, other feedbacks associated with the forcing are not included or not applied correctly or even the model used has flaws. One cannot put more faith in a particular forcing dataset if the model gives a better result than a model with another dataset. To do so would lead to a circular argument and the simulations will naturally end up comparing well with observations because the modeller has made biased choices (e.g. TL Anderson et al 2003 & H. Rodhe, R. J. Charlson and T. L. Anderson, "Avoiding	See below

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				Circular Logic in Climate Modeling", CLIMATIC CHANGE 44 (4), 419-422, 2000) Continued on next row.... [Gareth S. Jones]	
10-903	A	39:38	39:40	... Continued from previous row This sentence also suggests that a simple matter of choice of the forcing dataset can effect the results. I believe it is a bit more complicated than that. Past forcings datasets were created from various sources, but invariably there may be times when there will be large uncertainties in what is known. Choosing a particular data set, because it gives a good result, is not helpful if the choice is incorrect in the first place. Modellers should try to make choices about the available datasets based on the quality of the dataset, independent with what it does to the modelled climate. If possible they should try to sample the uncertainty range, but remember at the same time that there is likely to be just one, unkown, truth rather than a PDF of truths. Both these issues should be taken into account in this part of the section and the sentence should be re-worded to remove the implication that the dataset is flawed and that uncertainty in past forcing and not a simple choice in dataset is the issue. [Gareth S. Jones]	Yes, thank you, we reword the sentence. After consultation with ch 2 we decide to use the Ammann volcanic series
10-904	A	39:38	39:40	The claim about Sato et al (1993) is incorrect (depending on what mystery volcanic forcings the authors are comparing it with). Chapter 2, section 2.7.2.1 and figure 2.7.5 suggests that Sato et al (1993) has lower magnitude forcings for the major volcanic eruptions than two others, Ammann et al (2003) and Andronova et al (1999). This sentence may have to be removed. [Gareth S. Jones]	Text is reworded and sentence removed
10-905	A	39:40	39:41	"Seven different choices" of what? And what does "can be viewed on request" mean? Full references should be given, it is not good enough to leave a vague comment that somone (who?) can be contacted to get more information. [Gareth S. Jones]	This has been removed
10-906	A	39:43	39:43	Figure 10.5.6. The use of all three scenarios is appropriate. Why can't this be done with many of the other figures? [Jeffrey Kueter]	Figure now removed
10-907	A	39:45	39:45	Figure 10.5.7. The use of all three scenarios is appropriate. Why can't this be done with many of the other figures? [Jeffrey Kueter]	Figure now removed
10-908	A	39:47	39:47	Figure 10.5.8. The use of all three scenarios is appropriate. Why can't this be done with many of the other figures? [Jeffrey Kueter]	Figure now removed
10-909	A	39:53	39:53	The statement that "it is not possible to assess the uncertainty in these feedbacks	The sentence is ammended to include

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				individually" is prone to be misunderstood. One minor question is, for example, whether this statement refers to the feedbacks in the model or in reality? [Klaus Keller]	'model'.
10-910	A	40:8	40:9	Avoid stating what future material may be available. Please put text here that is needed, or drop. [Susan Solomon]	This sentence is dropped
10-911	A	40:28	40:28	Figures 10.5.9 Include all three markers [Jeffrey Kueter]	Figure dropped
10-912	A	40:30	40:39	An anthropogenerated molecule of CO2 resides "decades to centuries" (http://www.agu.org/eos_elec/99148e.html.) Assuming anything from the years 2100-3000 is a bit cheeky. I think this analysis really detracts from the report because the even the next 100 years are profoundly uncertain with respect to carbon dioxide concentrations. [Jeffrey Kueter]	This paragraph is dropped and the issue delt with in the c-cycle section.
10-913	A	40:40	40:41	Please avoid saying that material can be viewed on request since IPCC is not in a position to take on such responsibilities, nor should the authors. Please put text here that you think covers what needs to be covered. [Susan Solomon]	OK, the sentence will be omitted
10-914	A	40:41	40:41	Figure 10.5.10. Include all three markers [Jeffrey Kueter]	Figure is dropped
10-915	A	40:43	41:44	The focus of this section is on a narrow range of overshoot scenarios and ignores a broader, and more realistic, literature on long-term emission scenarios that was even included in the IPCC's 1st assessment report. I particular, CO2 emissions following a logistic curve have been analyzed for decades, intended to illustrate possible extents of the fossil fuel era, in the study of carbon cycle and the consequent effects on climate. Such emission cases result in CO2 concentrations that peak, and then decrease over centuries and recent references include [Kheshgi, H. S. and D. E. Archer, A nonlinear convolution model for the evasion of CO2 injected into the deep ocean, Journal of Geophysical Research, 109, C02007, doi:10.1029/2002JC001489, 13, 2004] and [Kheshgi, H. S., Evasion of CO2 injected into the ocean in the context of CO2 stabilization, Energy, 29, 1479-1486, 2004.]. And a broader class of long-term mitigation scenarios has been proposed by [Kheshgi, H. S., S. J. Smith and J. A. Edmonds, Emissions and Atmospheric CO2 Stabilization: Long-term Limits and Paths, Mitigation and Adaptation Strategies for Global Change, 10, 213-220, 2005.] that broadens the restrictive set of stabilization scenarios covered thus far by the IPCC. Suggest that discussion in this section cover the wider set of cases considered in the literature. [Haroon Kheshgi]	Attempts were made to coordinate new mitigation scenarios with WGIII, but the timing of the assessment between WG1 and WGIII did not allow sufficient time for the new WGIII scenarios to be run by models in WG1. These new mitigation scenarios will be assessed by WGIII, and most certainly will be run by WGI models, but not in time for the AR4. They will be part of the AR5. The stabilization and overshoot scenarios considered here are idealized and intended to illustrate processes in the climate system, not plausible economic outcomes.
10-916	A	40:43		Section 10.5.3.2 It strikes me that this section could benefit from a discussion of carbon	Attempts were made to coordinate new

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				cycle uncertainties with respect to stabilization targets -- specifically that the emissions that are consistent with CO2 stabilization are sensitive to the same carbon cycle feedbacks that have been discussed previously in this chapter in regards to atmospheric CO2 increases and warming over the next century. I am aware of two recent studies that have applied C4MIP-type methodology to the question of how emissions targets for CO2 stabilization are affected by positive climate-carbon cycle feedbacks using state-of-the-art coupled climate-carbon models. These are: 1. Jones, C.D., Cox, P.M. and Huntingtord, C. (2005) Impact of climate-carbon cycle feedbacks on emissions scenarios to achieve stabilisation. (To appear as a book chapter coming out of the Met Office "Avoiding Dangerous Climate Change" conference in Feb 2005); and 2. Matthews, H.D. (2005) Decrease of emissions required to stabilize atmospheric CO2 due to positive carbon cycle-climate feedbacks. Geophysical Research Letters, 32, L21707. [Damon Matthews]	mitigation scenarios with WGIII, but the timing of the assessment between WG1 and WGIII did not allow sufficient time for the new WGIII scenarios to be run by models in WG1. These new mitigation scenarios will be assessed by WGIII, and most certainly will be run by WGI models, but not in time for the AR4. They will be part of the AR5. The stabilization and overshoot scenarios considered here are idealized and intended to illustrate processes in the climate system, not plausible economic outcomes.
10-917	A	40:44	40:44	Statement is not true. Enting et al., 1994, the SAR and IPCC Technical Paper III considred already overshoot scenarios [Fortunat Joos]	Accepted.
10-918	A	40:44	40:54	The overshoot discussion should include several additional references to literature in this area. For example the original S and WRE 350 scenarios are overshoot scenarios, which precedes Wigley (2004) although they did not have the same motivation as in this more recent paper. Also, the text states that overshoot scenarios "may lead to greater climate damages and an increased risk exceeding some dangerous interference threshold (where the threshold concept must include rates of change as well as absolute warming)" -- precisely the issue investigated in O'Neill, B.C. and M. Oppenheimer. (2004) Climate change impacts are sensitive to the concentration stabilization path, Proceedings of the National Academies of Science – USA 101(47), 16411-16416. That paper designed new multigas stabilization scenarios (including overshoot), modeled global average temperature outcomes and assessed their implications for impacts. Further, these scenarios were used in a probabilistic assessment of impact potential in Schneider, S.H., and Mastrandrea, M.D. (2005) Probabilistic Assessment of "Dangerous" Climate Change and Emissions Scenarios. Proceedings of the National Academy of Sciences, 102: 15728-15735. Refernce to WG2 Ch 19, Section 19.4.3 could also be made, which is the primary place in which overshoot scenarios are assessed in WG2 for their implications for impacts. [Brian O'Neill]	Accepted. This section has been re-written and shortened to emphasize that we are assessing the physical response of the climate system in idealized overshoot and stabilization experiments.
10-919	A	40:46	40:49	What is the reference for the statement that "overshoot scenarios [..] are more cost-effective in terms of mitigation .."? Does this refer to the expected net-present value of	Accepted. This section has been re-written and shortened to emphasize that

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				mitigation costs? Is this statement true for reasonable ranges of model structures and parameters (e.g., projections of future monetary discount rates, description of induced technological change)? [Klaus Keller]	we are assessing the physical response of the climate system in idealized overshoot and stabilization experiments.
10-920	A	40:46	40:49	Why must this threshold concept include "rates of change"? [Klaus Keller]	Accepted. This section has been re-written and shortened to emphasize that we are assessing the physical response of the climate system in idealized overshoot and stabilization experiments.
10-921	A	40:46	40:54	Please avoid making any judgement within WG1 as to what is cost-effective for mitigation, and related points. We are not qualified to do that. Again, please edit this chapter on the next draft to ensure that we deal with our expertise and not that of others. [Susan Solomon]	Accepted. This section has been re-written and shortened to emphasize that we are assessing the physical response of the climate system in idealized overshoot and stabilization experiments.
10-922	A	40:48	40:48	This text should be deleted as the IPCC has repeatedly stated that the definition of “dangerous interference” is a political, not a scientific decision. [Jeffrey Kueter]	Accepted. This section has been re-written and shortened to emphasize that we are assessing the physical response of the climate system in idealized overshoot and stabilization experiments.
10-923	A	40:53	40:54	"overshoot scenarios are even more important in the WG3 context, as pointed out by Wigley (2005)." This idea was already presented in line 49 and 50 of the same page. [PATRICIO ACEITUNO]	Accepted. This section has been re-written and shortened to emphasize that we are assessing the physical response of the climate system in idealized overshoot and stabilization experiments.
10-924	A	41:1	41:11	Concerning the overshoot scenario experiment, the following infomation in addition to the temperature change is very useful for many scientists and polilymakers and researchers in WG3. "The other climate changes such as the North Atlantic MOC and sea ice volume almost recover to the B1 level in the overshoot scenario experiment, except a significant hysteresis effect is shown in the sea level change due to thermal expansion (Nakashiki et al., 2005; Yoshida et al., 2005)". < Please add the folowing paper in the reference, after line 57 in page 86, Chapter 10. Yoshida Y., K. Maruyama, J. Tsutsui, N. Nakashiki, F.O. Bryan,M. Blackmon, B.A.	Accepted.

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				Boville, and R.D. Smith, 2005: Multi-century ensemble global warming projections using the Community Climate System Model (CCSM3). J. Earth Simulator, 3, 2-10, accepted. (http://www.es.jamstec.go.jp/esc/images/journal200503/pdf/JES3-yoshida.pdf) [Koki Maruyama]	
10-925	A	41:1	41:44	This sectionis very important. It would be greatly improved if it were restructured, lengthened, and the emphasis changed. The paper by Knutti et al. (2005) presents an excellent and generalized physical science framework for dealing with these issues. It should be discussed in more detail, indicating in greater clarity how physical science constraints are useful for consideration of emissions and stabilization. The introduction and conclusions of that paper contain a great deal of information that should be brought into this section, particularly the points made about ocean mixing. The very broad and general figure from the Knutti paper (currently 10.5.12b) should start off this section, coming before discussing less general approaches or figure 10.5.12a, or the work of Meinshausen. [Susan Solomon]	Section has been revised to take into account some of these suggestions.
10-926	A	41:20	41:20	After the end of the sentense, please add the words, as " for risk assessment, as suggested by Yoshida et al.(2005)" <Note> The paper is the same one mentioned above. (see; http://www.es.jamstec.go.jp/esc/images/journal200503/pdf/JES3-yoshida.pdf). [Koki Maruyama]	Accepted.
10-927	A	41:21		Meinshausen - great paper but missing in the ref list. [Stefan Rahmstorf]	Accepted.
10-928	A	41:22	41:22	Replace the word "risk" with "probability" as risk is usually considered to be a product of probability AND magnitude of consequence. [David Sexton]	Accepted.
10-929	A	41:33	41:44	As mentioned above (p37, lines 36-39), EMICs have not adjusted sensitivities to the AOGCMs range of sensitivities. It might thus be misleading to interpret the range of responses in the stabilized scenarios using EMICs as a full range of responses. To avoid this interpretation, it might be recalled there that EMICs have not adjusted sensitivities to the full range of AOGCMs sensitivities. [Serge PLANTON]	Accepted.
10-930	A	41:52	48:42	This is quite a long section. In the closing paragraph, the statement is made that it is too early : the policy-relevance isn't quite there for the AR4. That of course is up to the authors to decide, but the form and scope of what is in the AR4 should then be consistent with that decision. While these topics are scientifically interesting, the attention given in an IPCC report is different from what it would be in research review paper. In view of	The section has been shortened to remove non-essential detail, though some detail is necessary to communicate the choices and assumptions underlying the different

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				that, I would suggest that it needs to be substantially shortened here. [Susan Solomon]	techniques. We also note that the statement in the closing paragraph does not actually say that it is too early to provide probabilistic estimates; rather, it says that a variety of methods is where the state of the art is right now. The fact that different GCMs give different predictions, and that we cannot say which is the best model, does not mean we should not report them in IPCC assessments as policy relevant information. The same applies to probabilistic methods. Indeed, the message that the methods are themselves uncertain is also important information for potential users.
10-931	A	41:52		This section contains very important new material but needs to be distilled [Garry CLARKE]	See response to 10-930.
10-932	A	41:54		Section 5.4 In the first paragraph, the distinction between the model uncertainties mentioned (parameter values etc) and fundamental model inadequacies which are unknown. (Smith, 2002, PNAS 99, 2487-2492; Kennedy and O'Hagan, 2001 J Roy Stat Soc B, 63, 425464) or so poorly represented that the dynamics of the model differ in an important way from the dynamics of the earth system. These model inadequacies cannot be sampled by the various monte carlo methods discussed throughout this chapter. And they are distinct from uncertainties in forcing discussed in the next paragraph (10-42 9). [Leonard A. Smith]	The second paragraph in section 5.4 has been reworded to refer explicitly to the effects of fundamental model inadequacies, including the references given by the reviewer
10-933	A	41:56	41:57	This seems a bit misleading as it stands. It seems to me that model error is a much broader problem than simply discretisation and the resultant need to parameterise sub grid scales. We do not know what the "correct" equations are anyway! The implication of your sentence seems to be that if only we had a powerful enough computer, all our problems would be solved, but there is little evidence that increasing computer power has actually substantially affected the accuracy of model predictions on broad scales (it has, perhaps, increased our confidence that they are doing reasonable things). I suggest something more general, along the lines of "...modelling uncertainties, which arise from both the	This sentence reworded to refer to parameterisation errors in general. The following sentence goes on to distinguish explicitly between errors in model parameters and errors in the fundamental parameterisation equations. See also response to 10-932.

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				numerical errors due to a finite resolution mesh, and also from uncertainties in representations of physical processes..." [James Annan]	
10-934	A	42:24	42:26	This is an interesting proposition, because it assumes that the physics inherent to each model is also equally wise. That is not likely to be true. An example can be gained from hurricane forecasting models. They, too, are (somewhat) independent, but some (notably GFS) are consistently better than others (notably GFDL) and there is no demonstration that regression to the model mean is a preferred forecast. It's equally easy to discriminate between AOGCMs by examining the RMS errors for the period, say, 1950-2000. [Jeffrey Kueter]	This statement does not actually require that all models are equally credible. It says only that if model errors are partially independent (in ensembles of models of either uniform or varying credibility), then there is potential for a partial cancellation of errors when forming ensemble means.
10-935	A	42:28	42:28	Both Chapter 8 of the Tar and AR4 use the multi-model approach and show it to be better than any individual model [Catherine Senior]	Added a reference to Chapter 8. The TAR discussion is covered by the reference to Lambert and Boer.
10-936	A	42:37	42:38	The multi-model approach is also susceptible to outliers – another way of saying that it is difficult to determine what the prior is in the Bayesian framework. [Matthew Collins]	This point is made later, in section 10.5.4.6.
10-937	A	42:37	42:38	There is only mention of the drawbacks of the methods and not of its advantages. For instance it relies on carrefuly validated models with a particular attention to limit long-term drift in a multi centennial control simulation (that is not the case for each individual member of the perturbed physics ensembles). [Serge PLANTON]	Reworded accordingly.
10-938	A	42:37		Suggest instead "However, members of a multimodel ensemble share common systematic [is "structural" better?] errors (Lambert and Boer, 2001), and, except in the case where the number of parameters is small, cannot span the full range of possible models, due to resource constraints." [Jonathan Rougier]	Reworded accordingly, omitting the reference to a small number of parameters since this is never the case for AOGCMs.
10-939	A	42:41	43:17	The work of Annan et al 2005b seems relevant here - this is also a "perturbed physics" ensemble using a GCM, along roughly similar lines to Murphy and Stainforth. I realise it is mentioned immediately following, but its existence does directly falsify your statement on p43 116-17, since our work does use a different model and we also make a (perhaps rather naive) stab at the model error problem. [James Annan]	The reference to the use of the same model has been reworded to make it clear that it applies to the discussion of the pdfs of figure 10.5.13 (now 10.5.3 in the second order draft), not to perturbed physics ensembles in general.
10-940	A	42:52	42:53	Murphy et al did not assume "that effects of individual parameters combine linearly and independently" as they placed an amount of uncertainty about their predictions which accounted for nonlinear interactions, as estimated from their ability to predict the response of 13 runs where they had perturbed several parameters at once. Only if they had set this	Reworded: "assuming the effects of individual parameters were additive but making a simple allowance for the effects of non-linear interactions".

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				extra uncertainty to be internal variability, would they have assumed linear combination of effects from parameters. So "assuming ... independently" should be replaced by "assuming the effects of individual parameters were additive but allowing for uncertainty due to nonlinear interactions,". [David Sexton]	
10-941	A	43:1	43:17	I would add in the text that the comparisons between these perturbed-physics models and observations are very crude, especially when compared with the precise calibration and validation techniques used for AOGCMs. It seems very likely that most of the perturbed-physics simulations could be ruled out by the validation techniques usually used on AOGCMs (as demonstrated by the work of Knutti and Meehl using seasonal cycle), even if such a work is impossible in the case of very large set of simulations. [Stéphane Hallegatte]	Perturbed physics ensembles are indeed designed to sample model uncertainties rather than to identify a single, best-guess model version. Nevertheless Murphy et al (2004) published verification statistics for their perturbed ensemble of similar scope to those used in a typical AOGCM model description paper. In any case, those and other verification techniques can inevitably rule out ALL models, due to the failure to date to eradicate systematic biases. For example, even the simple seasonal cycle measure of Knutti and Meehl is sufficient to show that most AOGCMs (as well as most perturbed physics members) fail to lie within the limits of observational uncertainty. Those AOGCMs and perturbed physics members which pass this particular observational test are bound to possess substantial systematic biases in other variables (e.g. cloud). The question is, which biases matter for climate prediction ? More work is needed before the IPCC can make evidence-based assessments of the relative weights to attach to multi-model, perturbed physics or other types of ensemble, and to their constituent members.
10-942	A	43:11	43:12	but most of the simulations with low sensitivity underestimate it [Catherine Senior]	Noted, but the main discussion point here is the investigation of the high

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					sensitivity simulations, since this is where the Stainforth et al ensemble differs from other GCM ensembles.
10-943	A	43:16	43:17	Piani et al (2005) do NOT account for structural uncertainty because their results were based on HadAM3 models only and they only perturbed 6 parameters. Claims that they have found an emergent constraint have yet to be tested on a multimodel ensemble. If any paper has investigated structural uncertainty then it is Murphy et al who include in their list of perturbations, extra processes such as cloud area scheme, rhcrit parameterisation scheme, canopy decoupling and anvil scheme that are not standard HadAM3 physics and are extra processes. But I don;t think this needs to be discussed. I would just like to see this sentence "Only Piani..." deleted as it is wrong. [David Sexton]	Agreed. Piani et al did not investigate structural uncertainty related to fundamental model error (the sense in which the term is used in this section). Sentence deleted as requested.
10-944	A	43:16		To consider the possibility of structural uncertainties is not to account for them in any meaningful sense. [Leonard A. Smith]	Sentence deleted (see response to 10-943).
10-945	A	43:21	43:23	The type of model that is used in this application should be defined. [Serge PLANTON]	Done.
10-946	A	43:23	43:23	The ensemble Kalman filter is only resource efficient once you know exactly the form of the cost-function used to compare models with observations. It must be re-run for each new observable introduced. The MOC results could be cross-referenced with earlier sections. [Matthew Collins]	Sentence deleted (see 10-947).
10-947	A	43:27	43:28	As mentioned above, this work (Annan et al 2005b) also uses present day climate – the LGM constraint is in addition to this. I suggest moving this citation to the previous paragraph, and deleting the claim in l16-17 about model error and all such work being based on a single model. I'm not sure that such a detailed description of the algorithm is worth including – the sentence of l23-24 could be dropped. Hargreaves and Annan 2006 is a better reference – full reference given below. [James Annan]	Sentence of detailed description dropped, and reference to THC work replaced by a sentence referring to Hargreaves and Annan (2006). See also response to 10-939.
10-948	A	43:27	43:27	If Annan et al (2005b) is mentioned one should also cite Schneider von Deimling et al. (2005), referenced in the back of Chapter 10, who used importance sampling that is as efficient as the Kalman filter in such application. Replace furthermore "Chapter 8" by "Chapter 9". [Hermann Held]	Inserted reference to Schneider von Deimling et al and updated reference to fuller discussion in section 9.6.2.3.
10-949	A	43:27	43:28	The last sentence is not useful in this chapter. [Serge PLANTON]	Disagree. A short sentence is needed to cross-reference discussion of perturbed

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					physics ensembles in Chapter 9 (see 10-948).
10-950	A	43:30		Section 10.5.4.3. Wang and Swail (2005b) also analyzed the relative importance of model differences and forcing differences in explaining differences in a 60 year transient response to increasing GHGs in an ensemble of 3 AOGCMs (CGCM2, HaCM3, and ECHAM4/OPYC3), which should be cited in the first paragraph of this section. Thus, I suggest add the following sentence in line 37, before the sentence "These conclusions are ...": Wang and Swail (2005b) also analysed the relative importance of model differences and forcing differences in explaining differences in a 60 year transient response to increasing GHGs in an ensemble of 3 AOGCMs. They found that internal variability explains more of the Canadian CGCM2 ensemble spread than the forcing-induced variability in ocean wave heights in most areas of the oceans, and that model differences explain much more of the AOGCMs ensemble spread than forcing differences as a source of uncertainty in ocean wave height (and sea level pressure) changes." [Xiaolan L. WANG]	Included a slightly shortened version of the suggested text.
10-951	A	43:34	43:37	The results about the internal variability v ensemble spread was also shown in the TAR [Catherine Senior]	Figure 10.5.15 (10.5.4 in the second order draft) has been updated to report results from the AR4 models, and the discussion has been modified to refer to the TAR.
10-952	A	43:55	44:6	This part should be shortened and merged in 10.5.4.2 since it concerns methodology of perturbed physics ensembles and not the diagnostic of uncertainty drivers. [Serge PLANTON]	Rejected. A brief description of the experimental design of the Collins et al ensemble (which is different from those discussed in 10.5.4.2) is needed to provide the reader with key information needed to assess the significance of the spread in the ensemble results.
10-953	A	44:1	44:1	"a high quality" is a little subjective (see comment n 36) [Serge PLANTON]	"high quality" replaced by "credible". The ensemble members were chosen on the basis that they should simulate present climate with skill comparable to that of the standard, unperturbed version of HadCM3.
10-954	A	44:6	44:8	Since this sentence concerns the uncertainty linked to cloud forcing, it should be displaced in the next paragraph on the same topic. [Serge PLANTON]	Changed as suggested.
10-955	A	44:10	44:10	Figure 10.5.16. This important figure shows that, after thirty years of warming (where we	The 1% per year forcing is an idealised

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				are) even at 1%/year, the warming is linear. Text should note that the 1%/year assumption makes the results of the first 75 years or so too warm. [Jeffrey Kueter]	scenario used for scientific understanding – the results are not intended as predictions. The interpretation of forcing scenarios is now explained more fully in the Introduction.
10-956	A	44:17	44:24	I disagree with the Palmer et al. (2005) statement: increasing the resolution will be helpful but will probably not solve the problem of cloud feedbacks...unless we are able to perform global climate simulations at the resolution of a LES! [Sandrine Bony]	Reworded to refer explicitly to the importance of cloud microphysical properties, and to avoid giving the impression that very high resolution is necessarily the main requirement for reducing uncertainty.
10-957	A	44:26	44:26	A few words explaining what you mean by "constraining" would be welcome. [Sandrine Bony]	Text has been added to the first paragraph of 10.5.1 to introduce the idea of using metrics of agreement with observations to determine, or partly determine, the range of predicted changes obtained from models.
10-958	A	44:26	44:51	Section 10.5.4.4 I would like to see something more explicit in here about process based observational constraints e.g. Williams et al 2003 and 2005 [Catherine Senior]	This section briefly lists a large number of potential constraints. If we were tro make the change suggested, we would also need to expand on the physical basis of the other constraints, for the sake of balance. Unfortunately there is insufficient space to do this.
10-959	A	44:27		Again - if models are ranked by "figure of merit" against observations then does the range of climate sensitivity sensitivre to this ranking? [Bryant McAvaney]	This has been tried for a perturbed physics ensemble (Murphy et al, 2004), which is mentioned here and discussed in section 10.5.4.2 and box 10.2. It has not yet been tried for a multimodel ensemble, so this remains a question for future research.
10-960	A	44:45		Without wanting to extend the length of the chapter, I think this paragraph could be augmented with: "There are also methodological issues to be resolved in these types of "calibrated projection" concerning the role and quantification of model structural error (Goldstein and Rougier, 2005)." The reference is to: M. Goldstein and J.C. Rougier (2005), Probabilistic formulations for transferring inferences from mathematical models	Inserted the following text: "There are also methodological issues to be resolved in observationally constrained model projections concerning the role and quantification of structural model

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				to physical systems, SIAM Journal on Scientific Computing, 26, 467-487. [Jonathan Rougier]	errors (Goldstein and Rougier, 2005)"
10-961	A	44:52		I think it would be useful to explain how this observational "constraint" works. It is mentioned a lot, but I am not sure it is clear how it actually works [FILIPPO GIORGI]	See response to 10-957.
10-962	A	45:4	45:56	This part of the section seems lengthy and somewhat repetitive. It could easily be shortened [FILIPPO GIORGI]	The text has been revised to remove non-essential material.
10-963	A	45:23	45:23	You should refer to Chapter 9, not chapter 8 (observational estimates of climate sensitivity are discussed in chapter 9). [Sandrine Bony]	Changed.
10-964	A	45:25	45:25	You should refer to Chapter 9, not chapter 8. [Sandrine Bony]	Now refers to the climate sensitivity box (10.2)
10-965	A	45:27	45:31	Rather than use "Frame and Allen (2005) and Allen et al (2002) argue", you could say that "Piani et al (2005) SHOW that many observables…", as this paper clearly demonstrates this point. [David Sexton]	Text changed to reflect this point..
10-966	A	45:28	45:29	Since climate sensitivity is a direct function of feedback strength, this dichotomy makes no sense as written. [David Rind]	Reworded.
10-967	A	45:30	45:30	I don't think the TCR scales directly with the feedback parameter. In fact it scales as the inverse of the feedback parameter, just like the climate sensitivity doesn't it? [Matthew Collins]	The reference to TCR has been removed, though it is indeed the case that to first order the transient response is expected to scale in proportion to feedback parameter (e.g. Hansen et al, 1985).
10-968	A	45:33	45:56	This section seems also related to the previous section on scenario uncertainties. Perhaps there should be some cross-referencing. [Matthew Collins]	Cross-reference to 10.5.3 included.
10-969	A	45:33	45:56	Somewhere here it must be noted that A2 is the least likely of the three commonly-used (in AR4) marker scenarios and that B1 is what we are near, and that everything that isn't B1 should be divided by somewhere around two for realism. [Jeffrey Kueter]	The interpretation of emissions scenarios is discussed in the revised Introduction.
10-970	A	45:36	45:37	This sentence would benefit from an amplification. [Klaus Keller]	Reworded to clarify.
10-971	A	45:40	45:40	typo "0.91.9" - the text, including caption for Fig 10.5.17, seems rather unclear too.	Corrected typo. Caption for Fig 10.5.17

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				[James Annan]	(10.5.6 in second order draft) reworded.
10-972	A	45:40	45:40	Add a "-" between "0.91" and "1.9 C". [Serge PLANTON]	Corrected.
10-973	A	45:40		error in "…0.91.9ºC…" [PATRICIO ACEITUNO]	Corrected.
10-974	A	46:10	46:11	It is not clear what you mean by "primarily constrained by observations". In fact they all rely in a rather fundamental manner on using a model to relate historical observations to future predictions (projections). This may be the source of my disagreement over the presentation of the "observationally constrained" estimates as if they are the ultimate arbiters of truth. In reality, there is no way to avoid a somewhat subjective judgement, other than perhaps by throwing out a large amount of potentially useful information (in itself a subjective decision). [James Annan]	Revised text to clarify: the point is that the answers are designed to fill the space consistent with observational uncertainties. However they do indeed depend on relationships obtained from a set of models, which are assumed to be robust. This caveat is now explicitly stated. We agree that there is no a priori reason to regard these methods as superior to other techniques, but we do not agree that the wording of our discussion implies such a view.
10-975	A	46:24	46:24	The reference of Stott et al (2005b) is discussed in the paragraph on Figure 10.5.17, yet this reference is not cited in the legend of the figure. [Klaus Keller]	Reference was erroneously labelled "Kettleborough". Corrected.
10-976	A	46:28	46:28	I am not sure what it means to say that the Harris et al. study neglects forcing uncertainty. Is it simply saying they only considered a single forcing scenario? [Matthew Collins]	The point is that the distributions based on ensembles of physical climate system AOGCMs (true for the AR4 models as well as the Harris et al perturbed physics ensemble) do not consider uncertainties in converting emissions of GHGs (for a given SRES scenario) into concentrations. Reworded to clarify.
10-977	A	46:37	46:37	If the figure 10.5.17 shows normal fits to (at least some) of the distributions (as the legend implies, but this is somewhat unclear) why, then, is the agreement in the shape worth mentioning. Is this not (at least in part) by design? The width is another story, of course. [Klaus Keller]	Only one of the curves (AR4 AOGCMs, shown only for the A23 scenario) is based on a normal fit. The multimodel fit should not be regarded as a pdf, and this curve was not part of the assessment of shape similarity on 46:37, which was intended to refer only to those methods which do provide

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					pdfs. The multimodel fit can, however, be compared to the pdfs in terms of width. Text reworded to clarify.
10-978	A	46:46	46:47	By construction, the range encompassing all PDFs should be wider than all individual PDFs. [Serge PLANTON]	Reworded to say just that the range encompassing all pdfs is significantly wider than that implied by the multimodel ensemble spread.
10-979	A	46:51	48:44	Section 10.5.4.6. This is a mixture of methodology and results. The methodology parts would sit better in section 10.5.4.5 (although need summarising - see details below) and this section could be shorter and concentrate on the geographical results [Catherine Senior]	A case can certainly be made for including all the methodology aspects in 10.5.4.5, but then there would be a potential confusion of why global results were discussed alongside methodology in 10.5.4.5, whereas regional results were separated from the methodology in 10.5.4.6. If the assessment of methodology is separated from the results, there is a danger readers might read the results without appreciating the attendant caveats and assumptions, so we suggest keeping the existing structure. However, 10.5.4.6 has been shortened where possible, to sharpen the focus on the geographical results.
10-980	A	46:51		Not to sound like I am complaining, but the first papers that did ensemble model weighting and probabilities based on this approach were those of Giorgi and Mearns, JC, 2002; Giorgi and Mearns GRL 2003. Minimally, they should be mentioned here. [FILIPPO GIORGI]	A reference to these papers has now been included at the beginning of section 10.5.4.6.
10-981	A	47:12	47:45	The description of the Furrer et al. study is rather long. [Matthew Collins]	This description is now substantially shortened.
10-982	A	47:12	47:36	These two paragraphs on the details of the Furrer et al method are too detailed to be included directly in the text. The emphasis suggets some superiority of this method over the others which have been summarised, which I don't believe is true(?). If this text has to stay it should be put in an annex, but I would prefer it to be briefly summarised along with the other methods [Catherine Senior]	See response to 10-981.
10-983	A	47:12	47:45	Three paragraphs on one paper Furrer et al (2005) seems to be disproportionately long	See response to 10-981.

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				compared to space devoted to other papers. This seems to be because several sentences describe "a scientific review of how the knowledge was derived" rather than a "conscise assessment of the current knowledge" (from instructions in the IPCC letter to reviewers. [David Sexton]	
10-984	A	47:25		It is usual to write MCMC as "Markov chain Monte Carlo". [Jonathan Rougier]	Corrected.
10-985	A	47:47	47:47	Figures 10.5.18. Use all three markers. [Jeffrey Kueter]	Rejected. The aim here is to show illustrative examples of probabilistic techniques. Unfortunately there is not space for a comprehensive set of maps showing a range of scenarios.
10-986	A	47:49	47:49	Figure 10.5.19. Use all three markers. [Jeffrey Kueter]	See response to 10-985.
10-987	A	47:51	47:51	I am sorry to inform that Räisänen (2005b) was rejected and should not be referenced to. [Jouni Räisänen]	Noted. Relevant text removed.
10-988	A	47:56	48:27	You show results from three methods (Furrer et al and two by Raisanen) which all give rather similar results. There is a lot of uncertainty in the assumptions in these methods, but you have not shown it. Could one of the figures by replaced by a method (e.g. Harris et al 2005?) which will truely show the range of uncertainty? [Catherine Senior]	Figure 10.5.18 (10.5.x in the second order draft) has been modified to include results based on Harris et al.
10-989	A	48:0		Section 10.6: Gregory has done his usual excellent job in writing this section! As noted, this section does not include the full range of scenarios. I would encourage the inclusion of the full range of scenarios ASAP so that there is less opportunity for critics to accuse IPCC of inflating the ranges. [John Church]	
10-990	A	48:11	48:12	Some effort should be devoted to understanding the reason behing "outliers". Is it really the case that it is not possible to distinguish between "reasonable" and "unreasonable" model results? It may then be possible to eliminate "outliers" for just cause, and thus perhaps get an improved result. [Andrew Lacis]	Agreed: this is a good subject for future research, but there is at the time of writing no published basis to eliminate outliers, so this cannot be assessed in the text. Note also that it does not necessarily follow that a model with an outlying climate change response should be expected to possess a poor simulation of present climate.
10-991	A	48:29	48:42	This summary section need its own sub section heading [Catherine Senior]	Done.

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10-992	A	48:31	48:31	I would prefer to emphasise that although widely differing methods and models are used, the results are in fact highly consistent. Of course methods that use only a small subset of the available information will tend to give a wider spread of uncertainty than methods that use more constraints (a problem which must be set against the danger of overconfidence through not adequately accounting for deficiencies in both data and models), but when all is said and done, almost(?) all credible research point to a most likely value of around 3-4C, and I am not aware of any significant evidence that actually points towards a likely value of above ~5.5C or lower than about 1.5C, even though some methods do not rule such extreme values out. It would in my opinion be silly to overemphasise the possibility of very high climate sensitivity when no-one has managed to produce a model of any level of complexity that can realistically represent volcanic forcing, paleoclimate changes, the seasonal cycle, while also having a climate sensitivity of about 6C or less. Most attempts to do so seem to fall over at the first hurdle. The recent evidence that supports (or at least permits) high sensitivity (>6C) appears to consist of two planks: "observationally-constrained" calculations that by design throw away almost all of our knowledge about the detailed physics and history of the climate system, and a handful of GCM runs with extreme parameter values which were shown to be implausible as soon as anyone bothered to check even their seasonal cycles. I would expect the response of these extreme models to volcanic forcing and LGM simulations to be similarly unreasonable. [James Annan]	Added a comment pointing out that there are some areas of consistency between the results of different methods. However, we note that the reviewer's comment seems to refer specifically to pdfs of climate sensitivity, whereas the summary at this point in the text refers to uncertainty and probabilistic methods in general, including regional changes. Assessment of pdfs of climate sensitivity is pulled together in box 10.2, which gives a summary of evidence for the most likely value and the range which broadly concurs with the views expressed in the comment. In particular, the summary is based both on evidence from observationally constrained pdfs and from published climate model results. However, the risk of a high value of sensitivity can only be based on available published evidence, and there is as yet no objective basis for weighting the information from alternative pdfs.
10-993	A	48:35	48:42	Given the likely delayed publication of Annan and Hargreaves (possibly also Rougier, but I don't know), I suggest this could be replaced with something along the lines of: "A good example concerns the treatment of model error in Bayesian methods, the uncertainty in which affects the calculation of the likelihood of different model versions (Kennedy and O'Hagan, Craig et al). This is yet to be thoroughly addressed in the field of climate prediction, but some initial steps are being taken in this direction (Annan et al 2005, Knutti and Meehl 2005)." [James Annan]	Deleted the reference to Annan and Hargreaves, but retained Rougier, which has been accepted for publication. Quoted Knutti and Meehl and Annan et al as evidence that the problem of structural model error has been recognised in climate prediction.
10-994	A	48:44	49:21	The discussion should include results from Hansen et al. (2005) regarding the comparison of modeled and observed heat storage in the oceans along with its implications for global energy balance and sea level changes. [Andrew Lacis]	Rejected. The comparison of observed and modelled ocean heat uptake and thermal expansion (i.e. in the past) is dealt with in chapters 5 and 9.

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10-995	A	48:44		Section 10.6: Where is the material on storm surges and other extreme events. There is a little in Chapter 11 but clearly insufficient. [John Church]	Noted. Chapter 11 is revising the coastal box, and more information will be added.
10-996	A	48:44		Section 10.6: I would like to see more on the regional distribution of sea level. [John Church]	Noted.
10-997	A	48:44		The entire section on sea level change is excellent [Garry CLARKE]	Noted - thanks!
10-998	A	48:44		The impression I got from reading this section is that, compared to the TAR, projections of sea level rise are going to be lower (even though, as I mentioned above, no range is yet given in this draft but hopefully will be in the next), and thus sea level rise is less of a problem for the future. Is this really the case? In earlier discussions during the AR4 process I actually was gathering an opposite message coming from the AR4, that is sea level rise being more of a problem. It is important to deal with this issue and how it relates to the TAR conclusions very clearly. [FILIPPO GIORGI]	Taken into account. We agree that we need to give figures which can be compared with the TAR, and explain the differences.
10-999	A	48:44		Section 10.6--Sea Level Change. I think there are serious problems with this section. It is nice to try to become fully based on model results, but it must be remembered that models do less for precipitation than for temperature, less well for snow than rain, and less well in rough orography as opposed to open areas. In addition, were the modeling approach used here applied to the 20th century, it appears that it would be far off of what is observed (perhaps only a third of what is observed). Yet neither of these points is really discussed and covered--instead there is a barreling ahead with the modeling approach (and as a former modeler I would like nothing more than for it to work). It really seems to me that before relying on modeling for the 21st century, it is essential to do better on the 20th century (and especially on the increase in the rate from the early part of the century to the higher rate found by satellite data since 1993). The idea that IPCC would bring the rate down as much as this section does without somehow explaining the 20th century and the full observational record just does not make sense to me. [Michael MacCracken]	Taken into account. We agree that the discrepancy between the sum of contributions and the observed C20 sea level rise, and the difference between the 1990s and C20 rates of level rise, are issues which have to be addressed, but note that they are relevant not only to projections, and are discussed in chapter 5 as well. Discrepancy between modelled and measured contributions is also important to discuss.
10-1000	A	48:44		Section 10.6 I feel there is a serious problem with sea level in this report. Observed SLR (3.2 mm/yr, if you subtract the Pinatubo rebound according to Church et al., 2.7 mm/yr) is above any TAR scenario for the past decade, and shows that the TAR has underestimated SLR. Yet now it is suggested to revise the projection downward. This makes no sense. If I consider what the main policy-relevant knowledge about SLR is, it would be the following: - Sea level is increasing, and faster than expected in the TAR. - SLR is a long-term problem, starting slowly but continuing very likely for centuries,	Taken into account. We agree that the difference between the 1990s and C20 rates of level rise is an issue which has to be addressed, but note that it is relevant not only to projections, and is discussed in chapter 5 as well. The second and third points are already made in the 1st draft, but are given

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				being very hard to stop. So looking just up to 2100 is not giving a realistic idea of the problem. - There is a significant risk that unabated global warming would lead to several meters of sea level rise over the coming centuries (say, 300 years), leading to loss of island nations and coastal cities. [Stefan Rahmstorf]	more prominence in the 2nd draft by introducing a subsection on the subject of sea-level rise in the long term.
10-1001	A	48:46	49:21	Section 10.6.1: Of the subsections on sea level change, this seems the most appropriate for the use of models, but the ranges given in this section seem not to mesh well with what appears to be happening since 1993 and this needs to be remedied as indicated in Chapter 5. Chapter 5 gives the overall rate from 1993-2003 as 3.1 plus or minus 0.4 and says that 2.6 plus or minus 0.2 is due to thermal expansion plus ice melt. And it says that glacier and ice cap melting contributed 0.76 plus or minus 0.14 during 1992-2003 (all in mm/yr)--as noted above, I wish rates were in m/century, but will work with mm/yr if necessary. So, if the ice sheet term is zero (and this section says it is actually negative, which would make the situation worse), the current rate due to thermal expansion is roughly 1.8 mm/yr (though there is a place in Chapter 5 that says it is 1.3 plus or minus 1.8--which seems impossible given the total rate of rise). But on page 10-49, line 10, this chapter gives a lower bound for 2000-2020 of 0.6 mm/yr and the upper bound as 2.1 mm/yr. There seems to be a serious mismatch here, for it does seem as if the thermal expansion term has no where to go but up from its present value, yet that is not allowed by the bounds given. And the situation only gets worse going to later in the century, when the projection for 2080 to 2100 is 1.3 to 4.9--showing an acceleration, but the lower bound is still less than what is said to be going on today. Somewhere, there are some serious problems and mismatches. [Michael MacCracken]	Noted--comment addressed in revised version
10-1002	A	48:46		Section 10.6.1 COMMENT. 1) There needs to be further discussion of the model vs obs discrepancy. If the steric SLR of the last decade or so is due in the main to anthropogenic heat uptake the implication is that the projections are too low. Modelling of ocean heat uptake remains a substantial issue in most ocean models. 2) a brief comment on how these estimates compare to the TAR steric SLR projections would be good [William Hare]	Noted--comment addressed in revised version
10-1003	A	48:50		47. Page 48, line 50 – reference sea level chapter in TAR. [Ronald Stouffer]	Accepted.
10-1004	A	49:7	49:7	What exactly is meant by "committed": constant forcing from 2000 levels or what? [William Hare]	Taken into account by removing the word "committed" (though the reviewers' deduction is correct).

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10-1005	A	49:16		How does this range compare with similar scenarios in the TAR? [John Church]	
10-1006	A	50:2	50:8	48. Page 50, lines 2-8 – Seems like this discussion belongs in chapter 9. [Ronald Stouffer]	Rejected, since chapter 9 doesn't look at the similarity of past and future patterns for other quantities; its concern is with observed and modelled patterns for the past only.
10-1007	A	50:29	50:29	Figure 10.6.2. Use all three markers. [Jeffrey Kueter]	Rejected. We wish to show modelling and scenario uncertainty separately. This figure shows the former for a particular scenario (A1B), chosen because it's the one for which we have the largest number of models.
10-1008	A	50:31	50:40	The introductory section should mention the following fundamentally important aspects and make clear how the model calculations cited in the following paragraphs (especially p. 51, lines 8 - 15, 35 - 40 and 42 - 48) deal with these questions (suppression of information about the corresponding difficulties would not be honest): The credibility of any scaling critically depends on two basic physical aspects: the firn/ice temperature and the size/dydnamics effect (Haeberli et al., 2002, Haeberli 2004; References: Haeberli, W., Maisch, M. and Paul, F. (2002): Mountain glaciers in global climate-related observation networks. WMO Bulletin, 51/1, 18-25. Haeberli, W. (2004): Glaciers and ice caps: historical background and strategies of world-wide monitoring. In: Bamber, J.L. and Payne A.J. (eds): Mass Balance of the Cryosphere. Cambridge University Press, Cambridge, 559-578): 1. Firn temperature: Under polar and dry-continental conditions, firn areas are cold and react to atmospheric warming by firn warming (not mass loss). Such firn warming relates to latent heat exchange involved with percolation/refreezing of surface meltwater and is known to be strongly overproportional with respect to air temperature change. Once the firn becomes temperate, mass loss starts taking place with continued warming of the air. This means that the sensitivity of large firn areas in the Canadian Arctic or in Central Asia, etc., could (a) strongly increase during the coming decades and thereby (b) reduce the regional differences in sensitivity. 2. Size/dynamics effects Glacier volume is calculated by multiplying area times thickness. Thickness depends on slope (via the basal shear stress driving flow) and basal shear stress depends on vertical extent times the mass balance gradient (via the total mass turnover determining continuity	Noted--comment addressed in revised version

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				and flow). As a consequence, area/volume relations are neither constant in space nor in time (statistical approaches neglecting this involve misunderstanding of the basic physical processes involved). For the same reasons, the response time as calculated from thickness and ablation at the terminus is not a primary function of size but of slope (size is, however, indirectly related to slope via the hypsometry of mountain valleys). This means that the large and relatively flat glaciers around the Gulf of Alaska or in Patagonia, where the most important sea level contribution comes from, have response times beyond the century and cannot dynamically adjust by tongue retreat to rapid forcing but rather waste down in place with little area loss. This, in turn, causes the mass balance/altitude feedback to become important. As an example, a cumulative surface balance of about 50 to 100 meters within a century or so could easily increase the mass balance sensitivity by a factor of two, correspondingly double the surface lowering and, hence, lead to a runaway effect. The corresponding growth in size of the ablation area on such glaciers may overcompensate the effect of shrinking total area on small glaciers elsewhere. This means that the sensitivity of the main meltwater producers is likely to strongly increase during the coming decades and strengthen regional differences accordingly. Calving instabilities tend to strengthen the positive feedback in these cases even further. These effects would, however, be reduced to some degree by the fact that important parts of such large maritime meltwater producers are below sea level (their melting lowers sea level). [Wilfried Haeberli]	
10-1009	A	50:33	50:33	add in the frist sentence immediately excluding greenland and Antarctica which are discussed in 10.6.4 to prevent confusion about the fact that some people consider greenland and antarctica as ice caps and don't see the difference between an ice cap and an ice sheet [Roderik S.W. Van de Wal]	Accepted.
10-1010	A	50:38	50:40	I don't understand this sentence at all. What does "locall" mean here? Which "controls"? [Richard Hindmarsh]	Accepted. Rewritten for clarity.
10-1011	A	50:42	51:24	Glaciers are sensitive to temperature and to precipitation, but also to other meteorological variables like wind, atmospheric moisture and radiation. This is generally not taken into proper account. Estimations of glaciers sensitivity to temperature are often based on observations or on models calibrated on observations (e.g. degree-days), that only account for temperature as an observable, thus at best fudging possible changes in other met variables into the temperature observable. Gerbaux et al. 2005 (Surface mass balance of glaciers in the french Alps, distributed modeling and sensitivity to climate change, Journal of Glaciology, in press) use a model that accounts for all surface meteorological	Noted--comment addressed in revised version

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				parameters to separately evaluate mass balance sensitivity to not only temperature (and temperature alone) and precipitation, but also wind, moisture and various terms of radiation. For instance, they find that a 1 C warming has the same impact as a 28% precipitation decrease, an almost doubling of wind speed or a 22% increase of solar radiation. Thus, although temperature is probably a dominating factor, changes in various other met variables, not just preciptiation, can modulate the impact of climate warming on glaciers. [Christophe Genthon]	
10-1012	A	50:42		The appearance of partial derivatives makes this section an outlier -- it is the only one in Chapter 10 that has any actual math (apart from Table 10.6.1). Is it necessary for the exposition? Chapter 4 (on the cryosphere) has not maths in it. [Garry CLARKE]	Accepted. Partial derivatives replaced with less scary notation.
10-1013	A	50:44	50:45	add (b) in brackets after mass balance and (T) after temperature [Roderik S.W. Van de Wal]	Noted. This comment actually refers to page 51. The clarification suggested is not needed because the notation has been simplified following comment 10-1012.
10-1014	A	51:0		Section 10.6.3.3: I am surprised by the significant differences with the TAR. Also the AOGCMs significantly under predict the observed glacial reponse. what are the implications of the discrepancies. Given these discrepancies, what should we make of the smaller predictions of future glacial contributions here compared with the TAR? The growing discrepancy of the AOGCM results (0.3 to 0.7 mm/yr) would suggest some deficiency in the modelling of glacier melting and raises concern about discrepancies for the 21st century. Could there be an issue of a faster dynamic response with warmer temperature allowing ice to move more rapidly to lower altitude where it can melt more rapidly? What about the impact of a greater percentage (and amount) of precipitation occurring as rain rather than snow? Also, in the cryosphere chapter there is reference to glacier changes which may affect their sensitivity. [John Church]	Noted--comment addressed in revised version
10-1015	A	51:1	50:6	This comment is not in the right position, the paragraph is about mass balance sensitivity to temperature here there is presented an argument that the albedo might change by soot. On itself this might be correct but it is not related to the mass balance temperature sensitivity [Roderik S.W. Van de Wal]	Accepted. Paragraph moved.
10-1016	A	51:8	51:15	These model calculations use a small number of very small mountain glaciers which are only weakly representative for the large meltwater contributors. How do they treat the firn-temperature and size effects as mentionned in comkment 21?	Rejected. The work of Oerlemans and others is based on only 12 glaciers but they are representative of a wide range

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				[Wilfried Haeberli]	of climatic regimes. The recent work of Braithwaite and others uses a sample of 61 glaciers. Both methods make allowance for refreezing of meltwater i.e. warming the firn. The results stated are for static sensitivity, not including dynamic effects, which are discussed separately.
10-1017	A	51:8	51:15	I am surprised to see that there is presented a sensitivity without warning for the mass balance as a function of global temperature change. There have been several papers addressing the importance of NOT using global temperature change, but rather local changes in changes of temperature (and precipitation) e.q gregory and oerlemans 1998 and Wal and Wild 2001. So if the authors insist on remaining to express this quantity, they should explain that you can not use Tg to calculate the change in volume of glaciers. [Roderik S.W. Van de Wal]	Accepted. Caveat inserted.
10-1018	A	51:29	51:29	Van de Wal et al 2001 should be Van de Wal and Wild 2001 [Roderik S.W. Van de Wal]	Accepted.
10-1019	A	51:30	51:30	Voklume/area scaling is not a generall accepted scientific concept. The problems should be made clear: (a) from a statistical point of view, it makes no sense to correlate a variable with itself (area is contained in volume) – volume/area-scaling just suppresses the large scatter (roughly 30% standard deviation for mountain glaciers) in area/thickness relations, which are statistically more reasonable. The large scatter in ice thickness data as related to area is due to (i) the small number and often incomplete coverage of accurately measured glaciers and (ii) the fact that not area is directly/physically related to ice thickness (an indefinitely wide but very short glacier would be very thin and not very thick!) but shear stress as governed by mass turnover or the mass balance gradient times the altitudinal extent of glaciers (inverse flow law). Volume/area relations – besides being physically and statistically questionable – are neither constant in time nor in space if climatic conditions change from maritime to continental or vice-versa. [Wilfried Haeberli]	Noted--comment addressed in revised version
10-1020	A	51:30	51:30	The equation is wrong V scales with A^1.375 or V=cA^1.375 [Roderik S.W. Van de Wal]	Accepted. Correction made.
10-1021	A	51:30	51:33	The deviations of the steady state are not larger than 20% as estimated by Van de Wal and Wild 2001, by using a simple flow model calculation [Roderik S.W. Van de Wal]	Accepted. Text inserted.
10-1022	A	51:35	51:38	Good remark concerning the size effect - but how can we know that area loss is more important? Explain how exactly this has been estimated and what the relative contribution	Taken into account by inserting a reference to Schneeberger et al. They

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				of each part may be. [Wilfried Haeberli]	do not make this point explicitly, but it can be seen from their Table 3 for instance. Comparison of the "Static" column with "Step 1" shows the effect of neglecting area reduction. Comparison of "Step 1" with "Step 2" shows the (smaller) effect of lowering the surface.
10-1023	A	51:42	51:48	provide an estimate of the effect from ice below sea level or - if not considerer - mention the problem. [Wilfried Haeberli]	Rejected. Ice below sea level is mainly an issue for the West Antarctic ice sheet, which is dealt with at length in its own section.
10-1024	A	51:47	51:48	The argument that the subgrid hypsometry approach by Marshall and Clarke can be used to solve the issue that not each individual glacier can be treated separately is not adequate. Using a subgrid parameterization might provide a forcing for each individual glacier but that is not the only information needed to treat each glacier individually, one still needs a thickness and elevation distribution for each glacier which is not available. So the sentence should be omitted [Roderik S.W. Van de Wal]	Accepted.
10-1025	A	51:50	52:18	Section 10.6.3.4: This section projects the contribution of glaciers and ice caps during the century as 26-58 mm and indicates this is much less than for the TAR. Indeed, at the rate since 1993 cited in chapter 5 of .76 plus or minus 0.14 mm/yr, this rate, even without any acceleration, would give a rise of 76 mm or roughly double the mean of the projection--and there is every indication from observations of glaciers and ice caps that this rate is going to increase (and that the glaciers and ice caps of Antarctic and Greenland are left off does not seem to be thought to make the difference--their contribution is projected as very small). It would be very interesting to know of the models can reproduce what is currently happening to the ice caps and glaciers. Even with high resolution models, it is hard to understand how they can be applied to this issue as the models really do smooth mountain ranges and so would have the wrong reference heights--and being lower would likely put a lot more snow on the glaciers than would actually happen (given water vapor decrease with height, mountain winds, etc.). The explanation given for the lower estimate also seems a bit questionable: (a) the amount of melting in the TAR was not very dependent on the emissions scenario, and I think this was because only the central climate sensitivity was used, not the full range; (b) it seems doubtful there is so much less that this would have such a large effect on the total melt in the coming century. It seems to me that to really justify the counterintuitive results here, several things need to be justified: (a)	Noted--comment addressed in revised version

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				how well the models reproduce changes during the 20th century; (b) that the model estimates of average glacier and ice cap height actually matches the observations; (c) that snowlines and mountain heights match up; (d) that the seasonal cycle matches up; (e) that the effects of mountain winds are accounted for; (f) that the rough topography of may glaciers that would lead to multiple reflections of solar and exposure to IR from adjacent mountains, etc., and (g) that the models include the process the Lonnie Thompson indicates is the cause of the accelerated melting that is seen (namely that if meltwater runs off--even into the glacier--the available heat can melt several times more snow and ice because energy does not have to be diverted from melting (heat of fusion) to evaporation (heat of vaporization--which is several times larger)--temperature and snowfall are not the only key parameters; the higher downward flux is key, the lower albedo around the glaciers that raises temperature, evaporates soil moisture, and lowers atmospheric humidity. There would seem to be potentially a lot of processes that AOGCMs are unlikely to include that might raise the melting rate and get better agreement with the 20th century--at the very least this section needs to recognize how uncertain the model results may be, and this does not seem to be the case. [Michael MacCracken]	
10-1026	A	51:50		Section 10.6.3.4 COMMENT. 1) The projected loss of GSIC ice over the next century using the global techniques here appear low compared to more detailed studies. There needs to be more discussion and review of these other approaches before concluding projections. See Schneeberger, C., H. Blatter, et al. (2003). "Modelling changes in the mass balance of glaciers of the northern hemisphere for a transient 2 x CO2 scenario." Journal of Hydrology 282(1-4): 145-163. and Bohner, J. and F. Lehmkuhl (2005). "Environmental change modelling for Central and High Asia: Pleistocene, present and future scenarios." Boreas 34(2): 220-231. [William Hare]	Noted--comment addressed in revised version
10-1027	A	51:50		Section 10.6.3.4 COMMENT. 2) The projections do not include the large areas of GSICs adjacent to Greenland and Antarctica, This was discussed in Chapter 11 of the TAR however the issue needs to be fully revisited, including for the Antarctic Peninsula, in terms of making or not making estimates. [William Hare]	Noted--comment addressed in revised version
10-1028	A	51:50		Section 10.6.3.4 COMMENT. 3) Projections in Section 10.6.3.4 seem too low. Dyurgerov and Meier report mass loss rates from glaciers recently (last decade) of around 0.9 mm/yr. Such loss rates are consistent with the projections made by Schneeberger et al and Böhner and Lemkuhl (2005) but are much lower than the estimates Section 10.6.3.4.	This comment repeats comment 10-1026.

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				See Schneeberger, C., H. Blatter, et al. (2003). "Modelling changes in the mass balance of glaciers of the northern hemisphere for a transient 2 x CO2 scenario." Journal of Hydrology 282(1-4): 145-163. and Bohner, J. and F. Lehmkuhl (2005). "Environmental change modelling for Central and High Asia: Pleistocene, present and future scenarios." Boreas 34(2): 220-231. [William Hare]	
10-1029	A	52:4		These values are considerably less than values observed for the latter part of the 20th century! The implication would seem to be that a warmer climate results in slower glacier wastage, presumably as a result of greater precipitation? [John Church]	Noted--comment addressed in revised version
10-1030	A	52:5	52:6	Here the argument is made that TAR found higher values for SLC because precipitation was not included. At least for ECHAM4 this is not a sound argument Van de Wal and Wild showed that including precipitation changes does not make a difference for the global mean SLC, regionally this might be important but not on the global scale. [Roderik S.W. Van de Wal]	Rejected. Van de Wal and Wild included the (indirect) effect of precipitation on the sensitivity to temperature, but not the (direct) sensitivity to precipitation.
10-1031	A	52:8	52:9	I am not sure about the steady state argument as an explanation for the difference between TAR and this work. I believe they both assume steady state in late 19th century so what is the argument? I believe too many arguments are thrown on a big pile here. Argumentation should be more specific [Roderik S.W. Van de Wal]	Taken into account by rewriting for the 2nd draft. This big pile of arguments was supplied more as explanation to the reviewers of the 1st draft than as intended final text.
10-1032	A	52:12	52:18	The new estimates of Morris for the sensitivity of the AP mass loss to warming are higher than in the TAR (0.012 mm of sea level rise/°C of local warming) but still not as high as is implied by other work and do not take account of the ice dynamics being observed at present See eg Rau, F., and M. Braun (2002). "The regional distribution of the dry-snow zone on the Antarctic Peninsula north of 70 degrees S." Annals of Glaciology 34: 95-100. and may not take account fully of the observed increase in ablation area Torinesi et al. (2003). "Variability and trends of the summer melt period of Antarctic ice margins since 1980 from microwave sensors." Journal of Climate 16(7): 1047-1060.. Rignot, E., G. Casassa, P. Gogineni, W. Krabill, A. Rivera, and R. Thomas (2004). "Accelerated ice discharge from the Antarctic Peninsula following the collapse of Larsen B ice shelf." Geophysical Research Letters 31(18). [William Hare]	Noted--comment addressed in revised version
10-1033	A	52:12	52:18	Van de Wal and Wild 2001 made an estimate of the contribution of small glaciers and ice caps around the Greenland ice sheet being 6% of their total estimate [Roderik S.W. Van de Wal]	Noted--comment addressed in revised version

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10-1034	A	52:20		Section 10.6.4 COMMENT 1) This section deals with projected change in the ice sheets mainly over the 21st. It would better if before the projections sub section 10.6.4.2 the ice sheet responses over multi century timeframes and the issues raised in Sections 10.6.6 and 10.6.7 are described along with implications for the uncertainty in these projections. The GIS projections from 10.6.6 are relevant to a discussion of 21st and 22nd century SLR projections (see my specific comments on Section 10.6.6) and the rapid ice dynamics issue and the degree to which models capture the potential mechanisms is relevant to a discussion of the uncertainty in projections using the ice sheet models cited in this section. [William Hare]	Taken into account by rearranging the material so that the 21st century and further future can be discussed together.
10-1035	A	52:20		Section 10.6.4: Again, there is a great reliance on model results, without somehow verifying that the models have the right sensitivity. For the ice sheets, the main test might seem to be to see if they can replicate the apparent disappearance of half of the Greenland and West Antarctic ice sheets during the Eemian. Do these sensitivities match what must have been those from the past? And do the models reproduce the melting that has been going on, particularly in Greenland, since the early 1990s? Most of the model factors mentioned as possible shortcomings for glaciers and ice caps apply here as well (an additional one might be the potential energy effect of meltwater runoff into crevasses, etc.). But again, if the meltwater is not present on the surface, then the absorbed energy does not have to be used to vaporize the meltwater and can all go to melting more snow and ice--and this is a huge effect. Even if there is a refreezing (and densification?) at the base, this ice will be at a much lower altitude and so more vulnerable to later melting. But, given the measurements of what is going on with the Greenland Ice Sheet, suggesting that the contribution of Greenland might be 10 to 70 mm during the 21st century seems very loiw to me--the upside potential would seem to be much higher, and the later text talks about getting up to a rate of 600 mm per century once the temperature gets several degrees higher--there is either a serious tipping point here, or the estimate being given is much too low--either situation is very serious. And the notion that Antarctica will have a negative effect of -20 to -200 mm (page 10-53, line 40)during the 21st century with the range not even considering the possibility of a contribution to sea level rise seems also hard to accept. It is generally agreed that Antarctic has been growing smaller during the Holocene, and we know from the geological record it was not present when the world was several degrees warmer, so this projection that during roughly only the 21st century there will be a buildup of the ice seems to place far more confidence/certainty in the model results than would seem justified given paleohistory and the accelerating rate of rise of sea level. If this range is indeed, the two-sigma limit, then I think that the uncertainties in the various modeling studies must be greatly understated. Again, for such a counterintuitive result, it seems to me that there must be much, much more careful examination of the mechanisms and model representations--and testing of	Noted--comment addressed in revised version

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				them against some observed (or reconstructed) situations. [Michael MacCracken]	
10-1036	A	52:24	52:24	Add Surface before Mass balance and (SMB) after mass balance [Roderik S.W. Van de Wal]	Accepted.
10-1037	A	52:26	52:28	Here lies the main problem with Chapter 10's treatment of ice sheets. This section refers the reader to 10.6.7 for discussion of observations that cast into doubt the value of current models because they are unable to produce recent, rapid changes in the ice, as well as the longstanding problem of them not reproducing ice streams. While 10.6.7 (p.10-55, line 8) does discuss these issues in details, it also refers the reader back to 10.6.4 where at least a synopsis of this discussion is needed, but is absent. Without it, the model outcomes stand more or less unchallenged, as if they are to be taken at face value. There is a need to be more explicit right here (p. 52) with regard to the limitations of the models. Otherwise, the section could be read as reporting the model outcomes as if there are no real dobuts about them. One can easily see one consequence of segregating doubts about the models in this fashion: they are nowhere to be found in the executive summary, as I noted in my comment above. [Michael Oppenheimer]	Taken into account by rearranging the material so that the 21st century and further future can be discussed together.
10-1038	A	52:30		Section 10.6.4.1. Precipitation surface mass balance and accumulation are used as synonyms throughout the text. For Antarctica, I would recommend to use net accumulation (precipitation minus evaporation) instead of precipitation throughout the section including the table. [Nicole van Lipzig]	Taken into account. These terms are not generally used as synonyms, but refer to three different quantities.
10-1039	A	52:30		Section 10.6.4.1. I miss mentioning the water vapour feedback in this section. The description is restricted to the sensitivity of the SMB to land ice temperature. However, the expected SMB sensitivity to an global temperature change might be much larger. This is illustrated by van Lipzig et al (2002), who show that the land ice temperature increases by 1.7 times the forcing which is applied at the lateral boundaries and sea surface of the model domain. The sensitivity of the SMB of Antarctica to the applied temperature forcing (15% per K) is therefore larger than expected. I understand that you need to be very restrictive in adding text, but I would argue that for a complete understanding of the SMB temperature sensitivity, the water vapour feedback is of importance and needs to be mentioned. [Nicole van Lipzig]	Rejected. We agree that the water vapour feedback is important for understanding regional climate change, but it is not immediately relevant in the quantification presented here, which is all in terms of warming over Antarctica. The result of Van Lipzig et al. has been converted (using the factor of 1.7 from their paper) to be comparable with the others.
10-1040	A	52:34	52:34	This line is ambiguous: It is not the orographic effect that is overestimated, but the precipitation. [Nicole van Lipzig]	Accepted.
10-1041	A	52:42	52:42	The work by Lipzig et al 2002 is not a degree-day model or energy balance model. It is a	Taken into account by clarifying that

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				regional atmospheric model [Roderik S.W. Van de Wal]	the classification as energy balance or temperature index refers only to the method of surface mass balance calculation.
10-1042	A	52:50	52:52	I do not understand where the 6% per K, which is mentioned here, comes from. Van Lipzig et al (2002) estimated the sensitivity of the saturation vapor pressure to temperature to be 18 mm per yr per K, which is 12% of the surface mass balance per K. If it is from a different source, please specify. [Nicole van Lipzig]	Taken into account by removing the numbers, which weren't intended to be attributed to the paper cited.
10-1043	A	53:0		Section 10.6.5: I note the projections are relative to the base year of 2000 rather than 1990 as in the TAR. If you are going to change the base year a means of comparison with the TAR is required. These values are low compared with the TAR. There seems to be two difference - substantially lower glacier contributions (see comments on section 10.6.3.3) and the full range of scenarios are not yet considered. Note that quadratic fitted to the 1970 to 2002 sea level gives an extrapolation similar to the top end of this range. Even a linear extrapolation would give about 180 mm. For the 2020 projections relative to 2000, I note we are already (in 2005) past the low end of the range! [John Church]	Noted--comment addressed in revised version
10-1044	A	53:0		Table 10.6.1 For sea level, the SMB over the grounded ice is of importance and therefore the caption should read "comparison of grounded ice sheet…". [Nicole van Lipzig]	Accepted.
10-1045	A	53:0		Table 10.6.1: I wonder a bit about the differences in the accumulation change in the different studies using the same ECHAM4 model, since they are using essentially the same data but just interpolated to different grids. In the last column, the value 7.4 should be shifted half line down to be on the same line as the neighboring value 0.47. With the regional model of Lipzig et al. 2002, the driving GCM should be mentioned as well for completeness [Martin Wild]	Noted concern about different methods. Noted comment on position of the number; it is as the referee suggests in the word document, but not in the pdf. Rejected remark about RACMO; it isn't driven by a GCM.
10-1046	A	53:5	53:12	The text should be modified taking into account the recent satellite data showign a significant ice thickenning in the Greenland interior, some thinning at the margins and an area average growth of the Greenland Ice Sheet at the rate of about 5 cm/ year (Johannessen et al., Science Express, 20 October 2005, 10.1126/science.1115356). Only a small fraction of the margins of the Greenland Ice sheet is directly affected by global warming (Chylek and Lohmann, Ratio of Greenland to global temperature change:	Taken into account to some extent by adding discussion about the decadal variability in the rate of sea level rise, but noting that this is relevant not only for projections and is mentioned in chapter 5 as well. However, we do not

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				Comparison of observations and climate model results, Geophysical Research Letters, 32, doi:10.1029/2005GL023552, 2005), the rest is dominated by North Atlantic Oscillation. So the future behaviour depends more on the future state of the NAO than on the projected global warming. [Petr Chylek]	agree with the inference that the NAO will be dominant in the future because it is important in the past, since the signal of climate change is projected to become much larger than internal variability.
10-1047	A	53:5		There is a bold statement here, which could be taken out of context. I would suggest that a qualifier is put in which makes it clear that we're not including increases in flux in this calculation. Actually, I think that to simply assume that all increases in flux can be folded into the issue of West Antarctic ice sheet collapse is not reasonable. It's not reasonable because there is a citable aceleration in glaciers around the Amundsen Sea, which may or may not lead to collapse but are certainly giving a sea level rise contribution (climate-related or not), and it is clearly possible that this will increase, or decrease and this needs to be folded into the overall uncertainty. [David Vaughan]	Accepted. "Surface mass balance" has been inserted in several places.
10-1048	A	53:9	53:10	Wild et al. (2003) is not the only reference showing no net ice loss in Greenland. Thomas et al., Science 2001 wrote "The region as a whole has been in balance but with a thickening of 21 centimeters per year in the southwest and a thinning of 30 centimeters per year in the southeast". Johannessen, et al., Science 2005 reported an increase in Greenland averaging 5.4 centimeters per year over the entire landmass. [Jeffrey Kueter]	Taken into account by removing the comparison with observed changes, in order to avoid confusion, and giving reference to chapter 4.
10-1049	A	53:14	53:20	It should be noted somewhere that the sensitivities should be used with care as they are depending in some cases of the magnitude of the pertubation itself. The sensisitivity for the melt of the Greenland ice sheet roughly doubles for a 4K positive perturbation relative to a 1K pertubation. So the sensitivities are non-linear quantities. [Roderik S.W. Van de Wal]	Accepted; noted in the table caption.
10-1050	A	53:22		Section 10.6.4.2 COMMENT The estimated projections for Greenland do not include an assessment of the effect of at least two uncertainties a) fast ice stream dynamics and b) uncertainty in the warming over Greenland. The present loss rates from Greenland are only partly due to the SMB losses which dominate the calculations here and some way needs to found of showing the effect of uncertainty in the modelling of these processes (see eg Parizek and R.B.Richard B. Alley Implications of increased Greenland surface melt under global-warming scenarios: ice-sheet simulations Quaternary Science Reviews, Volume 23, Issues 9-10, May 2004, Pages 1013-1027) b) The polar amplification over the GIS in the main models used in the Huybrecths 2004 work is in the range 1.2-1.4 and this may be too low when compared to observations which indicate a factor of about 2 (Chylek, P. and U. Lohmann (2005). "Ratio of the Greenland to global temperature	(a) Taken into account by rearranging the material so that the 21st century SMB changes and the dynamical changes which are particularly relevant for the further future can be discussed together. (b) Reference to Chylek and Lohmann inserted. However we have not commented on this in particular because it is only one study based on a very small number of sites and a small climate signal; given these

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				change: Comparison of observations and climate modeling results." Geophys. Res. Lett. 32(14): 1-4.). This could have substantial implications for the mass balance of the GIS in the scenarios used and would increase the loss rate (see eg the Ridley et al work cited later, which implies that a 7oC warming around the GIS corresponds to loss rates of order 3-4 mm/yr in the first few centuries). [William Hare]	uncertainties, we take it to be some confirmation of amplification of the warming over Greenland but not a precise result.
10-1051	A	53:34		The way of writing, using "we" (i.e. We have used… or we project…(line39)) seems not to be coherent with the rest of the chapter… [PATRICIO ACEITUNO]	Accepted.
10-1052	A	53:38	53:42	Maybe just a few words to explain the proportion of ice dynamics in the sea-level-rise estimate. [Richard Hindmarsh]	Noted--comment addressed in revised version
10-1053	A	53:44	54:18	This section will be read widely -- the reasons for difference to the TAR could be more explicit and standalone. If the same emission assumptions has been made in the TAR -- what would the range of sea-level rise have been? [Robert Nicholls]	Taken into account. We agree that we need to give figures which can be compared with the TAR, and explain the differences.
10-1054	A	53:44	54:1	Is it ilkely that the global average sea-level rise be smaller, throughout the 21st century, than the presenly observed (and increasing) 3mm per year deduced from satellite altimetry ? [Michel Petit]	Taken into account. We agree that the difference between the 1990s and C20 rates of level rise is an issue which has to be addressed, but note that it is relevant not only to projections, and is discussed in chapter 5 as well. It could be caused by internal decadal variability and is not necessarily indicative of a significant acceleration, so there may not be an inconsistency.
10-1055	A	53:46	54:18	The authors of this chapter are commended for their efforts to place new estimates in time frames beginning in 2000. It would be preferable if the effect of including A1FI in the estimates were stated for the convenience of readers familiar with the TAR estimates. The further refinement promised in this section is needed and it will be important to provide the most unequivocal statement possible about the range of potential sea-level rise. [Donald Forbes]	Noted. Thanks for the commendation. In the 2nd draft the scenario uncertainty is evaluated for an average model.
10-1056	A	53:46	54:6	As stated by the authors, this section remains a work in progress. Nevertheless, two directions are troubling: changing the comparison basis to year 2000 from 1990, and restricting the scenario range tested. Either the full range ought to be reported, or perhaps it is the authors' intention to use only the simple models to explore the full scenario range?	Noted. Yes, in the 2nd draft the scenario uncertainty is evaluated for an average model.

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				If so, to avoid confusion, there ought to be a placeholder for those results here. [Michael Oppenheimer]	
10-1057	A	54:1		Section 10.6.7 COMMENT The quantitative scaling example given in this section of the magnitude and rate of loss of ice from fast ice stream responses to loss of ice shelves is not very convincing. The exclusion of main ice shelfs from consideration appears to be unwarrented given observed ocean warming and projected surface warming over the Ross Ice Shelf. (Robertson, R., M. Visbeck, et al. (2002). "Long-term temperature trends in the deep waters of the Weddell Sea." Deep Sea Research Part II: Topical Studies in Oceanography 49(21): 4791-4806) and studies of the implication of warming for basal melting and stability if these ice shelves (Grosfeld, K. and H. Sandhager (2004). "The evolution of a coupled ice shelf-ocean system under different climate states." Global and Planetary Change 42(1-4): 107-132. and Williams, M. J. M., R. C. Warner, et al. (2002). "Sensitivity of the Amery Ice Shelf, Antarctica, to changes in the climate of the Southern Ocean." Journal of Climate 15(19): 2740-2757) [William Hare]	Noted--comment addressed in revised version
10-1058	A	54:2	54:3	Did the TAR really test up to 5.8 C warming--I thought not for as I recall only the central sensitivity was used. It really is important for AR4 to be using the full range of possible model projections of temperature, etc. [Michael MacCracken]	Noted. Yes, the TAR sea level projections used the full range of scenarios and model uncertainties. In the 2nd draft the scenario uncertainty is evaluated for an average model.
10-1059	A	54:4	54:6	This set of neglected terms seems to me unlikely to be the explanation that there is not a match to the 20th century--these terms are likely quite small. [Michael MacCracken]	Noted and agreed.
10-1060	A	54:14	54:18	Indeed, reconciliation with Chapter 5 is needed--and I think the approaches used in this chapter likely would explain well less than half of the observed rise during the 20th century--and likely not the rate since 1993 9even with observed SST, etc.). [Michael MacCracken]	Noted. The reconciliation of the observed and modelled contributions to sea level rise is an issue that is relevant not only to projections, and is discussed in chapters 5 and 9 as well.
10-1061	A	54:25	54:25	Is there not already ablation going on over an increasing area of the Ice Sheet? [Michael MacCracken]	Noted. The comment is correct but is not inconsistent with the text.
10-1062	A	54:26		Did Huybrects report the 2.7K "threshold" as representing the reponse of SMB only, or did it implicitly include some degree of dynamic reponse? In any case, the continual reporting of this value at 2.7K is a bit absurd, even if that is what Huybrechts found, because no one really believes the second significant figure. [Michael Oppenheimer]	Noted. Huybrechts et al. (1991) considered only SMB changes, without dynamics. Many model results are stated in the literature to two significant figures; as always, there has to be an assessment of modelling uncertainty, which is provided by the reassessment

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					of the threshold which follows.
10-1063	A	54:30	54:32	This really makes one want to have some detailed checks of the models done, for if a global warming of several degrees is not causing net loss over Greenland, it is really hard to imagine given the current state of what is occurring and over how much of Greenland some melting is occurring. [Michael MacCracken]	Taken into account by specifying "surface mass balance" rather than "mass balance".
10-1064	A	54:34	54:38	This is a bit strong - most people would countenance the disappearance of the middle sector. Maybe the Marshall-Cuffey modelling of the last-interglacial should be mentioned. [Richard Hindmarsh]	Noted--comment addressed in revised version
10-1065	A	54:42	10:42	It sounds to me as a ridiculous experiment to keep the Greenland ice sheet fixed for a 9.5 degrees warming experiment, what is the point in mentioning it, it is clear that transient effect are important for these large changes. [Roderik S.W. Van de Wal]	Taken into account by replacing "fixed", since this could be misleading. Ridiculous or not, a fixed ice sheet is what most AOGCM experiments have.
10-1066	A	54:42	54:44	This estimate really seems quite questionable, given how much melting occurred during the Eemian and apparently how fast this occurred. I really think that the models must be neglecting runoff of meltwater, for including this could increase the overall melt rate by a factor of several. At least some of the models should be trying that to see its effect. [Michael MacCracken]	Rejected. The rate is the right order of magnitude (several mm yr-1) for deglaciation episodes, given that the meltwater pulses probably came from much larger ice sheets than Greenland. The degree-day scheme used in the ice sheet model of Ridley et al. does include allowance for refreezing of meltwater.
10-1067	A	54:42		Sentences like "the sea level contribution was 5.5 mmyr-1 over the first 300 years" do not really tell a general reader (I tend to think of my mother or brother) what is at stake. How about presenting the same information as: "The sea level rose by 1.6 meters over the first 300 years." [Stefan Rahmstorf]	Taken into account in the sea-level commitment subsection of the 2nd draft, in which we give some numbers in metres. The units of mm yr-1 are useful for comparison with all the other rates of sea level rise previously stated.
10-1068	A	54:46	54:51	State that Toniazzo et al. find that the Greenland ice sheet disappears with pre-industrial carbon dioxide concentrations. [Jeffrey Kueter]	Accepted. In fact Toniazzo et al. did not simulate the removal of the ice sheet, just the failure of regrowth.
10-1069	A	54:47		49. Page 54, line 47 – I believe Broccoli and Manabe 1982 could be referenced here. [Ronald Stouffer]	Accepted, but equally Crowley and Baum (1995) could have been cited. Hence we have removed the first Toniazzo citation. The AR4 should focus on recent literature.

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10-1070	A	54:50	54:50	Lunt et al - date? [Andrew Lacis]	Accepted. Date is 2005.
10-1071	A	54:50	54:50	After "Lundt et al.", insert year in "( )". [Chiu-Ying LAM]	See 10-1070.
10-1072	A	54:50	54:50	lunt () [Roderik S.W. Van de Wal]	See 10-1070.
10-1073	A	54:50		The year is missing for the reference, Lund et al. () [PATRICIO ACEITUNO]	See 10-1070.
10-1074	A	54:55	54:55	Figure 10.6.4. 4 x CO2 constant for 3,000 years is a highly unrealistic scenario and any result from that scenario will mislead policymakers. [Jeffrey Kueter]	Rejected. It is a standard, although idealised, scenario for assessing the long-term commitment to climate change.
10-1075	A	55:0	56:	For the record, and to provide support, I approve of the way that the upper bound on dynamic contribution of Antarctica is calculated. It seems a better approach that relying on one specific model. [David Vaughan]	Noted and appreciated.
10-1076	A	55:1		Section 10.6.7. As a non-expert, I found this a very accessible and clear exposition that appears to be well-balaned: trying to indicate a firm position without claiming to give the ultimate answers. I would like to see many more such sections in the 4-AR! [Gerrit Burgers]	Noted and appreciated.
10-1077	A	55:1		Since it is not clear what the forcing is, I don't much like the word "response" in the section head. (Response to what?) It would be better to focus on "dynamic change", and then tell us what the change might be responses to. [David Vaughan]	Rejected. The "response" is "to climate change".
10-1078	A	55:6	55:8	"mechanisms responsible are not completely represented…" is a massive understatement. The grounding line representation in the Huybrechts WAIS model (and all others) is simply wrong, as demonstrated by recent findings. Ice streams are absent. The processes responsible for the behavior of Jakobshavn in Greenland are nowhere to be found in these models; the Zwally (2002) inference of surface-to-base meltwater lubrication is also missing. How important each of these may prove to future whole-ice-sheet behavior is a matter of current discussion, but they indicate that the models are not just incomplete but fundamentally wrong. This sense of inadequacy needs to be clearly stated. [Michael Oppenheimer]	Taken into account by stating simply that the phenomena observed do not occur in these models; the possible reasons are discussed later. Obviously the models aren't wrong in *every* respect, but it's not easy to summarise the complexity of the following discussion in one sentence!
10-1079	A	55:19	55:21	This sentence is probably wrong and in any case calls for a fuller explanation. Wild et al 2003 have such temperatures (summertime -2C isotherm) occuring around the time of doubling. Once this occurs, the ice shelf may only survive on the order of a century, so	Noted--comment addressed in revised version

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				"several" seems too long. This would be a good place to refer to Oppenheimer and Alley 2004. [Michael Oppenheimer]	
10-1080	A	55:21		I would argue that "several centuries" will be required to begin the process of collapse on Filchner-Ronne or Ross. Given the extraordinary rates of warming, and increases in melt-days on the Antarctic Peninsula we have seen over the last few decades, the northern corner of Ronne could begin to suffer summer melt in a hundred years. [David Vaughan]	See 10-1079.
10-1081	A	55:23	55:24	The Shepherd conclusion that bottom melting was the key is not universally accepted. [Michael Oppenheimer]	Noted--comment addressed in revised version
10-1082	A	55:34	55:47	The wording here is a bit careless (for instance the use of "will" in line 46). There needs to more clarity in statements about what models project may happen (ie, steady-state reattained), what has already been observed, and what alternative outcomes may happen in the future. Just because a model projects an outcome doesn't mean it "will" happen. [Michael Oppenheimer]	Taken into account by inserting a further qualifier, but note that this is a model-independent statement - if there is a steady state to be attained, by definition the rate of sea level contribution will decrease as it is attained.
10-1083	A	55:44	55:44	Typo "thomas" [Richard Hindmarsh]	Accepted.
10-1084	A	55:51	55:51	resistance" rather than "traction [Richard Hindmarsh]	Accepted.
10-1085	A	56:0	57:	Section 10.7 Climate Change Commitment. This section entirely misses the consequence of stabilizing CO2 concentration. Because CO2 stabilization would require essentially halting emissions, in view of the long residence time of CO2, then concommitant emissions of aerosols and aerosol precursors would also greatly diminish, most notably sulfate. Now suppose that aerosols are at present offsetting as much as 70% of GHG forcing. Then there will be a step function increase in forcing, followed by rapid increase in temperature, as a consequence of halting CO2 emissions. Note language of Chapter 9, Page 28, line 24 which calls attention to consistent estimates for the greenhouse gas attributable warming of 0.7 to 1.3 C offset by cooling from other anthropogenic factors (associated mainly with cooling from aerosols) of 0.2 to 1 C Because of the short lifetime of aerosols (a week) and the long lifetime and exponential growth (40 year 1/e time < atmospheric residence time) of CO2, a week's worth of aerosol emissions is offsetting some fraction of 40 years of CO2 emissions. Hence stop the aerosol emissions and the step function change in total forcing. It is not known what fraction the aerosols are offsetting, but it could be substantial.	Noted--comment addressed in revised version

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				Even more important, and adding to the above phenomenon, is the possibility that the climate sensitivity has been severely underestimated because of failure to adequately account for aerosol forcing. This possibility is explicitly noted in the language of Chapter 9, Page 60, line 51 that a high sensitivity cannot be ruled out because it is possible that a high aerosol forcing could nearly cancel greenhouse gas forcing. These considerations demand a discussion here. [Stephen E Schwartz]	
10-1086	A	56:7	56:7	grammar "not than" [Richard Hindmarsh]	Accepted.
10-1087	A	56:13	56:19	This discussion presents an interesting, although too-limited approach to bounding the long term ice sheet contribution. Indeed a key question is "What happens to the now-static ice in the future?", particularly for WAIS if ice streams discharge their ice. The answer is given in terms of what the Huybrechts model allows, "this being an upper limit...". But this is only an upper limit within the Huyrechts model, the limitaitons of which have been noted (Alley et al 2005). In this context, the failed grounding-line representation of Huybrechts is particularly problematic because it goes directly to the issue of future behavior of now-static ice. Other assessments (Oppenheimer and Alley, 2004, 2005; Oppenheimer 1998; Hansen, in Climatic Change, 2005) suggest that higher discharge rates are plausible. At this point, since the chapter has gone outside the normal modeling framework already by making several back-of-the-envelop estimates, there is really no excuse for not mentioning this literature and also linking to the discussion in WGII Ch.19 where this material is amply covered. [Michael Oppenheimer]	Noted--comment addressed in revised version
10-1088	A	56:17	56:19	It is by no means obvious that the results of Huybrecht and de Wolde represent an upper limit of the loss rate of ice arising from loss of ice shelves and rather represent an upper limit of this model, generic physical and numerical problems of which led Vieli and Payne in a recent review to conclude that there is presently no reliable model available Vieli, A. and A. J. Payne (2005). "Assessing the ability of numerical ice sheet models to simulate grounding line migration." J. Geophys. Res. 110(F1): 1-18. [William Hare]	See 10-1087.
10-1089	A	56:30	56:30	Is this value of 0.15 to 0.4 a rate per century--or total amount. As a total amount, it sure seems small. [Michael MacCracken]	Rejected. It is a total amount, as indicated by the units. That is what the paper says.
10-1090	A	56:34		A summary paragraph here would be useful. [FILIPPO GIORGI]	Taken into account by reorganising the material in order to bring the long-term projections together.

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10-1091	A	56:35	58:24	Nicholls and Lowe (2004) Global Environmental Change include the commitment to sea-level rise if greenhouse gas concentrations were immediately stabilised at 2000 using the HadCM3 model: the ultimate rise is 1-m. Compare with Friedlingstein and Solomon (2 [Robert Nicholls]	Noted. HadCM3 is among the models used here.
10-1092	A	56:35		Section 10.7 COMMENT The discussion about commitments is very useful. There are several different ideas in the literature about what "commitment" means (see eg Hare, W. L. and M. Meinshausen (2005). "How much warming are we committed to and how much can be avoided?" Climatic Change, accepted.) and it may be useful to distinguish between these in the text (eg the main commitment discussed is a constant forcing commitment). [William Hare]	Accepted, and reference added.
10-1093	A	56:35		Section 10.7. It would be useful to include a caveat when introducing climate change commitment calculations that these do not represent estimates of unavoidable climate change -- a confusion that may be likely to occur for readers. For example, unavoidable climate change over the next half century is surely greater than what occurs in a commitment run, because forcing can not be instantly stabilized. Furthermore, in the very long term it is plausible that climate change could be less than in a commitment run since forcing could plausibly be reduced below current levels. This distinction is made in WG2, Ch 2, 2.3.1.2, in order to discourage their use in adaptation studies as a kind of "unavoidable climate change scenario", rather than seeing them as a useful device for better understanding models. [Brian O'Neill]	Accepted.
10-1094	A	56:39	56:51	Hansen et al. (2005) give the current energy imbalance of the Earth as 0.85 W/m2, implying that the unrealized global warming is about 0.6 degrees C without any further increase in radiative forcing. [Andrew Lacis]	Accepted.
10-1095	A	56:39		The definition of climate change commitment here is the one referred to most often in the chapter and is the one that matches the constant RF runs done with the GCMs. However, the last paragraph of section 10.7.2 refers to the other way of using the term commitment in the literature, i.e. the future climate change caused by past emissions if there were to be no further emissions. The chapter needs to be careful to use language that differentiates these cases. [Martin Manning]	Accepted.
10-1096	A	56:43	56:53	and also by SIEGENTHALER U, OESCHGER H TRANSIENT TEMPERATURE-CHANGES DUE TO INCREASING CO2 USING SIMPLE-MODELS ANNALS OF GLACIOLOGY 5: 153-159 1984. Is the Wigley 84 reference indeed peer	Accepted.

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				reviewed literature? [Fortunat Joos]	
10-1097	A	57:18	57:21	Please add the following sentence; "The drastic thawing of the near-surface permafrost particularly in Alaska and Siberia are projected in A1B scenario, using coupled models (Kitabata et al, 2005; Stendel et al, 2002). Kitabata et al. (2005) suggest that the rapid thawing still occure even in the 20th century stabilization case and annual mean soil moisture will decrease in the permafrost regions due to increase of subsurface drainage, which may cause the drought in these regions. " <Please add the following paper in the reference, after line 21 in page 77, Chapter 10> 1) Kitabata, H., K. Nishizawa, Y. Yoshida and K. Maruyama, 2005: Permafrost Thawing in Circum-Arctic and Highland under Climatic Change Scenarios projected by CCSM3, SOLA, Meteorological Society of Japan, submitted (http://210.189.77.208/Result/Kitabata.pdf) 2) Stendel, M., and Christensen, J.H., 2002: Impact of global warming on permafrost conditions in a coupled GCM, Geophys. Res. Lett., 29, 13 [Koki Maruyama]	These references more appropriate in section 10.3.3, and have been added.
10-1098	A	57:22	58:55	Please bring some of the material on limitations - e.g., the possible rapid dynamic response of the ice sheets - to the front of the sea level section, so that the reader can understand the limitations before the rest of the discussion and presentation of numbers begins. [Susan Solomon]	This comment seems to refer to the sea level section, not commitment. Sea level has been revised.
10-1099	A	57:34		change "…the deep ocean will warm up more.." for "…the deep ocean will warm up more slowly…" (I am not sure if this is the idea…) [PATRICIO ACEITUNO]	Accepted.
10-1100	A	57:36		To help the reader I suggest to define NADW and AABW [PATRICIO ACEITUNO]	Accepted.
10-1101	A	57:40	57:56	The phrase "commitment" was perhaps introduced in Ramanathan (Science, Vol 240, P. 293, 15 April 1988 issue). [Veerabhadran Ramanathan]	Accepted.
10-1102	A	59:0		Box 10.1 I suggest that other possible high impact, but low probability natural events that could cause abrupt climate change should be mentioned. For instance a future volcanic eruption the same size as Tambora (1815) could offset some future warming for decades or an impact from an bolide (comet/asteroid) or a volcanic super-eruption could cause huge and rapid climatic changes that may force the climate into a new state (e.g. KO Pope, KH Baines, AC Ocampo, BA Ivanov, "Impact winter and the Cretaceous/Tertiary extinctions: Results of a Chicxulub asteroid impact model", Earth and Planetary Science Letters, 128	REJECTED: volcanic eruptions have climatic effects with short life times of 3-5 years. This is well documented e.g. by simulations of the Pinatubo eruption. Covered in 8.7.2.3

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				(1994), 719-725; C Oppenheimer "Climatic, environmental and human consequences of the largest known historic eruption: Tambora volcanic (Indonesia) 1815", Progress in Physical Geography, 27,2 (2003) 230-259; MR Rampino, S Self , "Volcanic winter an accelerated glaciation following the Toba super-eruption", Nature, 359, 50-52; GS Jones, JM Gregory, PA Stott, SFB Tett, RB Thorpe, "An AOGCM simulation of the climate response to a volcanic super-eruption", Climate Dynamics, 2005. [Gareth S. Jones]	
10-1103	A	59:1	59:34	It would be helpful to show the different time scales for sea level and temperature rise under commitment cases together, to elaborate the point made on line 50 regarding the slower rate of the later. Please consider producing a single figure showing both. [Susan Solomon]	Not clear what this comment refers to. Misplaced?
10-1104	A	59:1	62:9	Box 10 .1. This whole section needs to be consolidated with chapter 8 (section 8.7), as there is a fair bit of duplication between the two. [Robert Colman]	The Box is meant to pull together from various chapters what we know about abrupt climate change. So there is necessarily some overlap.
10-1105	A	59:1		As for the possibility of sudden change in ocean currents, the opinion of C. Wunsch is important (Carl Wunsch, Science, What Is the Thermohaline Circulation? Vol 298, Issue 5596, 1179-1181, 8 November 2002). He points out as follows, "The conclusion from this and other lines of evidence is that the ocean's mass flux is sustained primarily by the wind, and secondarily by tidal forcing." His discussion seems physically sound, and hence, readers of AR4 will feel uncomfortable to see that the Wunsch's report is neglected. [Kiminori Itoh]	The point is that the major forcing is from winds and that they provide the necessary energy input for the circulation. This is addressed on 10-68:34. However, changes in the buoyancy forcing clearly affect the rate of overturning and deep wster formation as plenty of models demonstrate. This was also addressed by Rahmstorf (2003, Nature)
10-1106	A	59:5		That definition of abrupt climate change has an earlier source, which I believe is the original. Also, we need not just use one definition. In Chapter 6 we write: "Abrupt climate changes have been variously defined either simply as large changes within less than 30 years (Clark et al., 2002), or in a physical sense, as a threshold transition or a response that is fast compared to forcing (Rahmstorf, 2001; Alley et al., 2003) or duration of the subsequent climatic regime (Overpeck and Trenberth, 2004)". [Stefan Rahmstorf]	A cross-chapter topic meeting at LAM3 has decided to use the NRC definition.
10-1107	A	59:29	59:30	This is an example of rhetoric that is designed to inflame rather than inform, i.e. "reaches as much as 60%". Instead simply state the range, which is from zero to 60%. [Jeffrey Kueter]	wording changed
10-1108	A	59:37		50. Page 59, line 37 – complete shutdown – S+M do not believe that the THC was	MOC is actually slightly below 0.

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				completely shutdown in their 4XCO2 run. [Ronald Stouffer]	
10-1109	A	59:41		"would not be abrupt" - that does not apply to all model scenarios, see Rahmstorf and Ganopolski 1999, their Fig. 2 (http://www.pik-potsdam.de/~stefan/Publications/Journals/rg99.pdf) [Stefan Rahmstorf]	for MOC the wording is correct - it is temperature for the "0.2" scenario that decreases over a few decades.
10-1110	A	59:44	59:45	This is a common misconception, which should not be promulgated here (and doing so, by the way, backs up the 'movie' that the chapter takes great pains to denounce). It is not the 'shutdown' in NADW that takes decades - that takes centuries (or for the Heinrich events in the Bond cycles, thousands of years) - it is the recovery that may take only a few decades (something no model can get to happen). [David Rind]	agreed - added a sentence on abrupt warmings and refer to Ch 6
10-1111	A	59:46		What about the DO warm events? These are clearly abrupt climate warmings par excellence - what do we know about their forcing? They are not caused by ice sheet instabilities, I think. [Stefan Rahmstorf]	agreed - added a sentence on abrupt warmings and refer to Ch 6
10-1112	A	59:51	59:52	51. Page 59, line 51-52 – long term and hemispheric to global scale effects …not investigated. – I am confused. I thought that is what all the important papers have done over the past 15 years. Clarify. [Ronald Stouffer]	REWORDED, meant are not full collapses of the MOC, but changes in e.g. LAbrador DWF.
10-1113	A	59:57		52. Page 59, line 57 – overwhelmed – Passion word. [Ronald Stouffer]	word replaced
10-1114	A	60:5	60:5	It would seem appropriate to cite some of the relevant studies (e.g., Matear, R. J., A. C. Hirst, and B. I. McNeil. 2000. Changes in dissolved oxygen in the Southern Ocean with climate change. Geochemistry, Geophysics, Geosystems 1:Paper number 2000GC000086.). [Klaus Keller]	citation added
10-1115	A	60:14	60:14	Obviously it is a weaker circulation that models are getting, so it represents a negative feedback, not a positive one for sea ice. [David Rind]	REJECT: merid heat flux in subarctic decreases, but heat flux into the Arctic increases.(Hu et al, 2004)
10-1116	A	60:17	60:19	A "personal communication" that "sea ice cover can rapidly reduce in a few years" is not an acceptable IPCC reference. [Jeffrey Kueter]	AGREE: will be clarified. If no paper is available this sentence will be removed.
10-1117	A	60:18		53. Page 60, line 18 – Idea of variability and signal combined? [Ronald Stouffer]	see 1116
10-1118	A	60:19		Sorry - but citing personal communications is not allowed in the next draft.	see 1116

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				[Martin Manning]	
10-1119	A	60:24	60:34	This section does not appear to be either an abrupt change or a surprise, rather it is a "tipping point" and perhaps the section head should be changed to reflect this. [David Vaughan]	added "Irrreversible Changes" to Box title.
10-1120	A	60:29	60:30	How is the firn temperature considered in this odel calculation? The estimated volume loss concerns small and predominantly low-latitude mountain glaciers with possibilities of adequate model parameterization. Because of thze firn-temperature and size effects described ion comment 21, however, these glaciers are not representative for the large glaciers which essentially contribute to sea level. [Wilfried Haeberli]	we dont address issue of sea level here. Schneeberger et al. glaciers are all north of ~40°N, not low latitude.
10-1121	A	60:32	60:34	The last sentence of this paragraph is hardly understandable. What exactly should be expressed? [Wilfried Haeberli]	demonstrate different time scales and potential irreversibility
10-1122	A	60:36	61:14	Suggest this material should be brought into line with section 10.6.6 and 10.6.7. [John Church]	not clear what is NOT in line, as it is a summary of those sections.
10-1123	A	60:41	60:42	Does the threshold temperature of 2.7 degree C indeed refer to a globally mean temperature change? If so, how does this statement compare to the information provided on page 54, line 26? [Klaus Keller]	corrected
10-1124	A	60:50	61:14	Obviously, a paragraph is repeated here with some subtle differences between the two versions. Both versions end strangely by focusing only on the near term and indicating that ice sheet models contain "no information" on the long term future of the now-static ice. Of course, this is literally incorrect: the models do say a few things on this point. The problem is that they may well be wrong! Instead, this discussion should tie to the improved discussion I have recommended above for 10-56 lines 13-19, where not only the models but other approaches (including paleo-climatic evidence) are discussed. If the language "the fast-flowing areas are limited in extent, and could discharge only a small fraction..." is meant to be retained in the rewrite, it would be better to replace it with "areas that are currently fast-flowing contain a volume of ice that is small compared to 6m sea level rise". Otherwise, the statement would seem to rule out the possibility that additional ice from upstream would pass through the same drainage area. [Michael Oppenheimer]	Thanks - first paragraph deleted.
10-1125	A	60:50	61:14	The two paragraphs contain some almost repeated text, and should be edited to make them read better. [David Vaughan]	done
10-1126	A	60:50	61:14	We should try to be consistent about the terminology for ice shelf retreat. You've used breakup and break-off. I believe that "retreat" is a better term. Break-up ususally seems	done

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				to apply the final-stage loss of the ice shelf which could be dynamic in origin, while retreat refers to the long period of climate-driven shrinkage that occurs prior to that. Later in the chapter you also use the term "disintegration" which adds another layer of confusion pg69-line26. [David Vaughan]	
10-1127	A	60:52		change "…surface melting. or which are.." for "…surface melting, or which are…" [PATRICIO ACEITUNO]	done
10-1128	A	61:4	61:14	There is a high degree of duplicated content with the preceding paragraph. [Garry CLARKE]	done
10-1129	A	61:4	61:14	Paragraph repeated. [David Rind]	done
10-1130	A	61:4	61:14	54. Page 61, line 4-14 – Almost repeat of what is said earlier. [Ronald Stouffer]	done
10-1131	A	61:4		The idea of paragraph starting in line 4 is the same as that of paragraph starting in line 50 of page 60. [PATRICIO ACEITUNO]	done
10-1132	A	61:5	61:6	Why does the uncertainty change from the last page? (" 4 to 6 meters" p. 61 compared to "about 6 m" on the previous page (l. 55)? [Klaus Keller]	para now deleted
10-1133	A	61:17	61:18	I think the word "irreversible"needs to be defined here. On long timescales, few things are completely irreversible. I assume the authors mean centennial or millennial scales, and should state the timescales involved. I also question the term "frequently", on the same grounds -- have irreversible changes really been a frequent occurrence on anything other than extremely long (geological) timescales? [Robert Colman]	added an explanatory sentence at the beginning of the box
10-1134	A	62:7	62:9	The Dorn et al. (2003) study showed no long-term trend in NAO caused by global warming, only interdecadal variability. The present way of citing it gives the misleading impression that models suggest large NAO-related long-term changes in European temperatures. [Jouni Räisänen]	text modified
10-1135	A	63:0	32:	The upper bound is difficult to constrain because of the limited length of the observational record and uncertainties in the observations, which are particularly large for ocean heat uptake and for the magnitude of the aerosol radiative forcing. Studies that take all the important uncertainties in observed historical trends into account cannot rule out the possibility that the climate sensitivity exceeds 4.5 C This is an important conclusion.	Constraints on climate sensitivity from the observed warming are discussed in detail in section 9.6, to which a reference is given. No change on the text.

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				I would quarrel with characterizing uncertainty in aerosol forcing as an uncertainty in the observations. It is an uncertainty in the estimate. [Stephen E Schwartz]	
10-1136	A	63:0		Box 10.2 This should discuss Lorius et al. Nature 1990 - deriving climate sensitivity from multivariate regression from data. [Stefan Rahmstorf]	Simple estimates from LGM data (Lorius, Lea, Hoffert and Covey) are discussed in the LGM section 9.6. They do not provide sufficiently quantitative uncertainty estimates to be used for the synthesis.
10-1137	A	63:0		Box 10.2 and associated summary bullets. PDFs definitely must not be averaged or interpreted in any average fashion, as is done here. It is logically wrong (we discussed this in Trieste…) Imagine you have one data constraint that rules out a CS above 4 ºC at 99% confidence, but a 50% chance it is below 2 ºC. You have another data constraint that rules out a CS below 2 ºC at 99% confidence, but gives a 50% chance it is above 4 ºC. Then, if you take your results seriously, you can have 98% confidence that CS is within the interval 2-4 ºC. If you average the two pdfs, though, you get a totally different (and what's more important, completely wrong) result. [Stefan Rahmstorf]	Taken into account. Average PDF is replace by expert judgement.
10-1138	A	63:1	63:19	The discussion of climate sensitivity should differentiate between (1) the temperature equivalent of the applied radiative forcing (This is the Delta-T-zero factor in Hansen et al., 1984; Hansen et al., 1997). (2) the feedback magnification of the applied forcing leading to the equilibrium temperature response, and (3) the time rate of approach to equilibirum - ocean heat capacity and heat transport into the deep ocean. [Andrew Lacis]	Not taken into account. This box only discusses climate sensitivity, the definition of which is unambiguous. Amplifying feedbacks are discussed in chapter 8. The transient response is discussed in section 10.5.2
10-1139	A	63:1	64:56	There is much duplication here with the results presented in Chapter 9. [Matthew Collins]	Not taken into account. The intent of box 10.2 is to summarize all material from the many different chapters and sections and provide a synthesis.
10-1140	A	63:1	65:12	Box 10.2 I felt this was well written and summarises the results very well. [Catherine Senior]	No changes requested.
10-1141	A	63:1	65:1	The discussion of climate sensitivty and TCR in this report is very important. While the authors have done a great job summarizing the available approaches and data, expressing the final result in a way that is clear is important. As it is currently expressed, the bottom line could be interpreted as a range of 1.5-4.5 (end of the Box). That is exactly the same as TAR, SAR, and FAR and doesn't seem to do justice to the fact that we no longer believe that a value of 1.5 is as likely as 4.5. I realize that there is information given on what is likely and more likely - but that's not the format that would be expected here.	Taken into account. New range based on expert judgment is provided.

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				What we are saying will probably only be fully understood if the authors express the result differently. Would it not be possible to say 3, or 3.4, plus X, minus Y, where these are determined in the standard way. Appropriate caveats and limitations are already in the text and are excellent; these could be expanded if needed but without such an approach our findings are sure to be misinterpreted. [Susan Solomon]	
10-1142	A	63:8	63:8	How many AR4 slab models? [Catherine Senior]	Taken into account. 18 models, will be updated for final draft if necessary.
10-1143	A	63:9		Box 10.2 "provide only general guidance concerning how large" [Leonard A. Smith]	Not taken into account. Meaning of comment is entirely unclear.
10-1144	A	63:11	63:12	Determining what is dangerous climate change is not a scientific question. I'd be inclined to leave this sentence out. [Richard Wood]	Misplaced comment, probably relates to abrupt change box.
10-1145	A	63:31	63:32	Given that the temperature equivalent for doubled CO2 is 1.2to 1.3 degrees C (Hansen et al., 1984; Hansen et al., 1997), a "climate sensitivity" below 1 degree C is a clear statement of negative overall feedback - which really has no physical justification. [Andrew Lacis]	Taken into account. Added a sentence confirming net positive feedbacks. Some studies have indeed argued for net negative feedbakcs (e.g. Lindzen and Giannitis, Douglas and Knox).
10-1146	A	63:34	63:37	A statement that some studies "take all the important uncertainties in observed historical trends into account" seems somewhat problematic. Do we know all the important uncertainties? [Klaus Keller]	Taken into account. Changed to 'all important known uncertainties'.
10-1147	A	63:34	63:35	Suggest replace "zonal" and "meridional" by "east-west" and "north-south" for the target audience. [Richard Wood]	Misplaced comment, probably relates to abrupt change box.
10-1148	A	63:37	63:38	This is not "A further difficulty" - it is implicit in the previous results - but is worth emphasising as "An important point". The constraint on transient change is a useful output. [James Annan]	Taken into account.
10-1149	A	63:41	63:48	If you are going to claim a 33% chance of climate sensitivity exceeding 6C, then I would argue that paleoclimate evidence does provide a useful constraint. On the other hand, if you accept that exceeding 6C is very unlikely, I would agree that the existing paleoclimate research probably doesn't help further. I think the text needs clarifying here. [James Annan]	New expert judgment of the likely range is given and consistent with the LGM statements.
10-1150	A	63:41	63:48	This section should consider if the LGM is consistent with the very high climate sensitivities that parameter estimation based solely on the instrumental record allows. For	The LGM constraint is discussed in detail in chapter 9, to which the text

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				this to be true would require close to zero forcing in the LGM with higher GHG concentrations and greater ice cover…and higher aerosol loading. Furthermore, it is disappointing that this section is solely based on literature that is only submitted. Suggest if the long string of literature on this subject and if is consistent with the probability of high climate sensitivity proposed in this chapter. [Haroon Kheshgi]	now refers. All literature cited is available for review and will be in press by the time the SOD goes in review.
10-1151	A	63:42	63:48	Please note that the Schneider von Deimling et al. study also used Antarctic paleo data as independent constraints and obtained almost identical results than for tropical SST. Hence, closer inspection of what both groups have done may well allow to draw further conclusions than just indicated in this §. [Hermann Held]	Not taken into account. It is questionable to what degree such a simplified atmospheric model can capture the relevant processes over Antarctica. The authors themselves make this caveat in their paper.
10-1152	A	63:44		Rumour has it that the discrepancy between those two studies could be due to a bug in the Annan et al. model - this needs to be clarified. If true, there would be no reason to question the suitability of LGM data to constrain climate sensitivity as found by Schneider et al. [Stefan Rahmstorf]	Inappropriate comment, and not enough details given. Lead authors are not supposed to find bugs in published papers.
10-1153	A	64:1	64:10	As mentioned before, this type of analysis _has_ been done with other models, in particular Annan et al 2005b which uses climatological constraint similar to Murphy et al, and also uses the LGM as validation. Schneider von Deimling et al (2005) covers similar ground with a third model. [James Annan]	Taken into account partly. The corresponding sentence is removed. Neither Annan 2005 nor Schneider 2005 provide PDFs, and both of them do not sample the full range of possible sensitivities. The interpretation of these results wrt structural uncertainties is thus not straightforward.
10-1154	A	64:1	64:2	Indeed, climate sensitivity is not a tuneable quantity. Rather, it is the embodiment of the entire model physics of feedback interactions, parameters, parameterizations, and physical formulations. Again, it is important to differentiate between (1) the temperature equivalent of the applied radiative forcing, (2) the feedback magnification, and (3) the time rate of approach to equilibirum. The temperature equivalent of the applied radiative forcing is a quantity that in principal has a "correct" answer that is based only on laboratory measurements of the absorption coefficients (and line-by-line radiative transfer modeling) of atmospheric gases. The relative error of GCM radiation calculations is readily correctable by absorber scaling or "tuning", and should not be considered to be a part of the model's "climate feedback sensitivity". The sum total effect of the feedback processes in magnifying the applied radiative forcing as the model approaches equilibrium is the climate sensitivity. And this is distinct from the time rate of approach	Not taken into account. This box only discusses climate sensitivity, the definition of which is unambiguous and given in the glossary. The box is just a synthesis of other chapters and sections, not supposed to explain the concept of climate sensitivity. Amplifying feedbacks are discussed in chapter 8. The transient response is discussed in section 10.5.2

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				to equilibrium, which is a ocean heat capacity and heat transport into the deep ocean issue. [Andrew Lacis]	
10-1155	A	64:1		"Climate sensitivity is not a tuneable quantity in AOGCMs" : this statement is false, unless "quantity" is taken to mean an explicit 'sensitivity' parameter value in which case it is misleading. Enough is known about model responses to changes in various parameters that one could indeed tune the sensitivity of an AOGCM in a manner not uncommon to the way one tunes the global mean temperature of such a model. [Leonard A. Smith]	Taken into account. Modified to 'not a single easily tunable parameter'
10-1156	A	64:2		"observed present-day climatology provides a constraint" : there is no single clear way to apply this constraint, given that the models are unable to realistically reproduce the observations. See the discussion of "state-of-the-art" models in Stainforth et al 2005. [Leonard A. Smith]	Reviewer is correct in his statement. Space is limited in the summary box and caveats are discussed in section 10.5.4
10-1157	A	64:4	64:4	I think the irreversibility of MOC spindown, and its implications, are an important issue that should be discussed here. [Richard Wood]	Misplaced comment, probably relates to abrupt change box.
10-1158	A	64:7	64:10	See comments on p 24 ll 33-42. Here, I think it may be appropriate to say something about ice ages. The way the para starting at p 63 l 33 deals with the common and scientific uses of "Gulf Stream" is very neat, and I suggest a similar approach would be good here, i.e. say that the effect of MOC shutdown is sometimes portrayed as an ice age, say briefly what an ice age is, say that the impacts of MOC shutdown, while large, would not trigger a climate change as large as the last ice age. [Richard Wood]	Misplaced comment, probably relates to abrupt change box.
10-1159	A	64:7		change "…shown in ,Box 10.2.." for "…shown in Box 10.2…" [PATRICIO ACEITUNO]	Corrected.
10-1160	A	64:8		"They constrain the lower bound": they merely agree or coincide. [Leonard A. Smith]	Taken into account. Sentence reworded.
10-1161	A	64:14	64:18	While small enough perturbations about some reference point should always be representable as being sensibly linear, Hansen et al. (1984) showed that climate feedbacks do not combine in linear fashion. Rather, the different feedbacks (e.g., water vapor, clouds, snow-ice albedo) combine in a multiplicative fashion since any emperature increase due to say, increase in water vapor, will act on cloud and snow-ice processes, and	No change requested. Feedbacks are discussed in chapter 8. More details on the Stainforth et al. 2005 study is given in section 10.5

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				temperature responses due to changes in these processes will again act to change the water vapor amount, etc. [Andrew Lacis]	
10-1162	A	64:20		Note that without new significant information, the a postiori reweighing of targeted monte carlo ensemble members almost always violates statistical good practice. It is difficult to see how this would not be the case given only "state-of-the-art" members with the definition and properties given in Stainforth et al (2005) [Leonard A. Smith]	The chapter authors are not in the position to (dis)prove published papers. No specific changes requested on the text.
10-1163	A	64:21		"they have low probabilities": this is confusing; these values of sensitivity (?they?) have low probabilities in the sense that there are few model runs at those values, but as individual model runs that alone does not imply they have "low probabilities attached to them". As noted in my comment on 10-64 20, the down-weighting via a postiori comparison of specific models selected based on their sensitivity is, at best, statistically questionable. Unless some model runs are arguably realistic and other of arguably un physical, any quantitative down weighting is ambiguous. [Leonard A. Smith]	Taken into account partly. Reworded to make clear that probabilities are not attached to ind. model runs. The chapter authors are not in the position to (dis)prove published papers. Although all model are wrong to some extent, some model runs are clearly more realistic than others.
10-1164	A	64:32	64:33	This final sentence could be (mis-)interpreted in an alarmist way. The more conservative text on p 67 seems more appropriate. Surely the key point is that while our modelling is imperfect, there is no evidence of a threshold that is likley to be passed. [Richard Wood]	Misplaced comment, probably relates to abrupt change box.
10-1165	A	64:43	64:44	The AOGCM range is much bigger if the results from perturbed physics experiments are included. [Matthew Collins]	Taken into account. Changed to 'AR4 AOGCMs'. Perturbed physics ensembles are now mentioned.
10-1166	A	64:45		"constrained from observations" is inaccurate, should be replaced by "consistent with 20th Century observations". [Stefan Rahmstorf]	Changed as suggested.
10-1167	A	64:48		"shape of the pdf is very likely right-skewed" - this is (I apologise in advance) a nonsensical statement. It implies there is a "true pdf" and we can find out with a certain likelihood what it looks like. But in fact the true climate sensitivity is just one value, not a distribution. The pdf reflects not the climate sensitivity per se, but our lack of knowledge about its value. Hence there can be no true or likely shape of the pdf. That we have right-skewed ones just reflects the kind of data constraints and models used. You could design an experiment using some LGM data that produces a left-skewed one - no less valid than	Taken into account. Summary is rewritten completely. Replaced by 'uncertainty on the upper bound is larger than on the lower bound'.

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				the right-skewed one, simply showing a different result. [Stefan Rahmstorf]	
10-1168	A	64:53	64:56	Sorry for the boring repetition, but again you single out one subset of rather uncertain and limited estimates to highlight. I don't think this is appropriate, and in this case the text appears rather misleading as the "climate models and climate change in different periods" refers primarily to highly simplified models looking at little more than energy balance or temperature over the past century. [James Annan]	New expert judgment of the likely range is given. No papers are published so far that show how different lines of evidence can be combined formally. If published in time, they will be cited.
10-1169	A	64:54	64:54	I fully support mentionning the probability values : in this context, it is more policy relevant to explicitely state that the probability of a sensitivity above 6 could reach 33%, than to qualify it as unikely. Why not adding (< 10 %) after "1 ". [Michel Petit]	New expert judgment of the likely range is given without percentages. Comment no longer applicable.
10-1170	A	65:1	65:6	I've already objected to the averaging of different pdfs. In order for climate sensitivity to be greater than even 4.5C, every different method would have to have a significant bias in the same direction, and some approaches already assign a fairly low probability to such a high value. There is a danger of overconfidence in the analyses, but that does not justify simply forming as wide as possible a range and claiming it to be the best we can do. [James Annan]	Taken into account. New expert judgment of the likely range is given without averaging. No papers are published so far that show how different lines of evidence can be combined formally.
10-1171	A	65:1	65:6	The description of the construction of the average pdf seems appropriately cautious, but its use to characterize the previously used 1.5-4.5 range, rather than to state a new range, seems odd. That is, the pdf is considered credible enough to say that a sensitivity below 1.5 is "very unlikely", and that it is "unlikely" to be above 4.5, and precise probabilities are even given (8% and 28%). But why not give a high and a low value that both have the same likelihood -- e.g., there is a 10% chance that sensitivity is lower than 1.6 and a 10% chance that it is above 6.8 (I am estimating these figures by eye from Box 10.2 Fig. 2). The chapter assesses a large amount of work on this topic, and then uses it to describe an old range rather than generate a new one. [Brian O'Neill]	Partly taken into account. New expert judgment of the new likely range is given without averaging. No percentages are given, as there is no consensus.
10-1172	A	65:1	65:6	It seems very unsafe thing to combine a lot of pdfs that have very complicated interdependencies that are impossible to understand and account for. If as stated "there is no formal way of estimating a single PDF" then it should not be done. [David Sexton]	Partly taken into account. Expert judgment of the new likely range is given without averaging. Some synthesis statement is inevitably needed.
10-1173	A	65:2		"expert judgement can be based on the average of the nine PDFs". First note that the nine "PDFs", the technical term "PDF" is used with different meaning by the authors of the different studies. Even if it is agreed that you have nine independent objective probability density	Taken into account. Expert judgment of the new likely range is given without averaging.

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				functions, it is not clear what the aim of taking their arithmetic average is. (if one forecaster is uncertain gives a 50% chance, and a second forecaster has knowledge and gives a 0% chance, then what is meant by saying there is an average 25% probability?). Of course, if these were samples of relative frequency from sampling, the interpretation would be easier, but the the vastly different sample sizes [orders of magnitude] would come into play. when averaging two or more probability density functions like this, the resulting curve is likely to be inconsistent with the information in any (every one) of the input probability density functions. From a statistical point of view, the operation is rather odd; in any event the meaning of this average must be explained to the reader, since naïve interpretations are misleading at best. Also note that adopting equal weighting does not avoid the need to justify the weights: Some of these distributions are based on an ensemble (several) orders of magnitude larger than others. Some of the studies effectively share ensemble members, and in this case the linear approximations used in order to obtain some of the PDFs have been shown to fail explicitly (not merely in general, but by explicit calculation, see Stainforth et al 2005). [Leonard A. Smith]	
10-1174	A	65:2		The numbers in this paragraph "unlikely above 4.5 (28% probability), might be better expressed as 28% of model runs, wherever possible. [Leonard A. Smith]	No longer applicable. No percentages are given.
10-1175	A	65:6	65:6	Mentionning the probability values is policy relevant and should be kept in later versions of the chapter. [Michel Petit]	No change requested.
10-1176	A	65:19	65:19	To my ears "colloquial" would sound less condescending than "popular". [Richard Wood]	should be page 59! rectified
10-1177	A	65:44	65:46	Such a spindown would still be 'abrupt' in the Alley sense, i.e. its timescale would not be determined by the dynamics of the forcing but by internal processes. It just happens that those processes have centennial timescale. Suggest simply omit the words, "would not be abrupt, but would evolve on the timescale of the forcing, i.e.". [Richard Wood]	done
10-1178	A	65:46	65:48	This sentence seems over-confident to me. While there are no simulations showing a rapid, abrupt MOC shutdown in response to global warming, there is GCM evidence that	should be 59:42 Suggested sentence added

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				large decadal changes in MOC and related climate variables are possible (with constant forcing) punctuating a multi-centennial timescale recovery from a perturbation (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), and of course an abrupt switching on of the MOC is seen in the GFDL 4xCO2 stabilisation runs (Manabe, S. and R.J. Stouffer 1999: the role of the thermohaline circulation in climate, Tellus, 51A-B, 91-109). Some other spontaneous, rapid climate events are discussed in section 8.7.3. Abrupt behaviour is clearly possible. How about: "There is no direct model evidence that the MOC could collapse within a few decades in response to global warming. However a few studies do show the potential for rapid changes in the MOC, and the processes concerned are poorly understood (see 8.7)". [Richard Wood]	
10-1179	A	66:0		Question 10.2 This section drifts a little too close to "impacts" territory, in my opinion. You talk about the "profound influences" that changing extremes would have, but surely the increased risk of flooding due to climate change is likely to be dominated by the effects of economic growth, development policies and the like. I see no benefit from potentially sparking a turf war with WG2 over this, and suggest that the second half of the second sentence (l5-6) could simply be deleted. Section 10.3.6 (also on extremes) does not seem to stray so far into the impacts territory, but sticks more closely to the climate science. [James Annan]	Accepted.
10-1180	A	66:1	67:4	I think structural changes are needed to the answer to this question. I think the answer should start with a more general discussion of how changes in the mean and extremes are related. Only after this discussion, should it get into specific regions and phenomena. For example, immediately discussing northern middle to high latitude changes in precipitation at the start of paragraph 2 seems to get too specific too quickly [Robert Colman]	Rejected. The answer is directed to the question as posed, so the answer must be very specific.
10-1181	A	66:1		Seems that it would make sense to explicitly include discussion of drought in the answer to this question, rather than just "dryness" - see comments below. [Jonathan Overpeck]	Accepted.
10-1182	A	66:1		It should be added that the statements of this section are limited to extreme events that are resolved by global AOGCMs. For instance the results for intense precipitation cannot be merely extrapolated to heavy rain associated to small-scale convective events. [Serge PLANTON]	We now secifically use the term "AOGCM".
10-1183	A	66:1		Question 10.1: We suggest insertion of the word "predicted" in several places (noted in our specific line comments below) to make it clear that the reference is to projections rather than observed changes.	Noted.

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				[David & David Wratt & Fahey]	
10-1184	A	66:4	66:6	This paragraph (the "headline answer") should be in italics. [David & David Wratt & Fahey]	Accepted.
10-1185	A	66:4		Question 10.1: Suggest a more substantial opening paragraph short answer. Also strongly suggest that 'impacts' not be discussed since this is a 'science' answer. [David & David Wratt & Fahey]	The intention is for the opening to be short and to the point. We have removed reference to impacts.
10-1186	A	66:12	66:14	We suggest insertion of the word "predicted" in several places, ie: "… Another aspect of these PREDICTED changes IS related to the PREDICTED changes of precipitation, with wet extremes PREDICTED to become more severe in many areas where meman precipitation IS EXPECTED TO increase, and dry extremes where the mean precipitation IS PREDICTED TO decrease. [David & David Wratt & Fahey]	Accepted, except we prefer the use of "projected" rather than "predicted"
10-1187	A	66:25	66:25	If this Holland study comes to publication in time to be kept, please keep Ch 8 in the loop as 8.7 may need to say something about processes. [Richard Wood]	Mis-labeled comment
10-1188	A	66:26	66:26	This sentence needs to be rewritten. There seems an implicit assumption that only high temperatures are associated with the extremes. It should be acknowledged upfront that extreme cold might also change. [Robert Colman]	Paragraph has been re-written.
10-1189	A	66:26	66:26	What is a "very likely risk"? This seems to me to be mixing 2 contradictory terms. [Dave Rowell]	Paragraph has been re-written.
10-1190	A	66:27	66:28	The distinction between changes in warm and cold extremes and their relationship to maximum and minimum temperatures is unclear, and I suggest this sentence is rewritten and expanded upon. [Robert Colman]	Paragraph has been re-written.
10-1191	A	66:27	66:28	Where has "It been shown…that cold extremes warm up faster than daily minimum temperatures". One reference is Knappenberger et al., Climate Research 17, 45-53. Be more explicit than "For a future warmer climate". What period, what assumptions? [Jeffrey Kueter]	Paragraph has been re-written.
10-1192	A	66:28		I do not understand the following phrase: "..but cold extremes warm up faster than daily minimum temperatures." [PATRICIO ACEITUNO]	Paragraph has been re-written.
10-1193	A	66:29	66:30	The discussion of "cold air outbreaks" is unclear here. Firstly it is not clear what a cold air outbreak is. Secondly the decline figures are too specific -- they don't mention what timescale, or scenario might be involved. Also they sound way too high: are we really expecting a 100% drop in cold outbreaks -- this sounds like no cold days.	Paragraph has been re-written.

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				[Robert Colman]	
10-1194	A	66:36	66:36	Change "could be a decrease" to "is likely to be a decrease", or "there is expected to be a decrease" [Robert Colman]	Accepted.
10-1195	A	66:36	66:36	First word : "will" instead of "could" ? Or "a decrease in diurnal temperature range is likely in most regions..." [Michel Petit]	Re-written.
10-1196	A	66:41	66:47	As mentioned above, an explanatory statement as to why tropical cyclones might decrease would help. [FILIPPO GIORGI]	Accepted.
10-1197	A	66:41	66:42	Quantify from Knutson and Tuleya: 6% increase in wind speed, 14% increase in central pressure fall, and 7% increase in average precipitation rate by model year 2080. Small numbers amid noisy data. [Jeffrey Kueter]	Knutson and Tuleya is not the only study that projects such changes, so it is not appropriate to single out that one study.
10-1198	A	66:41	66:47	This does not appear to be consistent with Ch 8, section 8.5.3 page 8.51 lines 33-34. Chapter 8 says that there is substantial disagreement among the models of the changes in the intensity of tropical cyclones. No mention of this disagreement in made in Q10.1 [Ruth McDonald]	Qualifying language now added, and further coordination with Ch. 8.
10-1199	A	66:41	66:41	Contradicted by the statement in chapter 8, page 52, line 4-5 "There is no agreement among the models whether global warming will make tropical cyclones more or less intense", and by the following lines chapter 8, page 52, lines 5-10 [Michel Petit]	Qualifying language now added, and further coordination with Ch. 8.
10-1200	A	66:44	66:47	this is only a single study, so I don't think it should be emphasised too much. I suggest this be shortened, and the caveat be put in that it is only one model. [Robert Colman]	This sentence has been re-written.
10-1201	A	66:51	66:51	We suggest replacing "shown" with "predicted", ie "… Several studies have PREDICTED a possible reduction …" [David & David Wratt & Fahey]	Accepted.
10-1202	A	66:51		The following phrase (dot is missing at the end…): "Several studies have shown a possible reduction of midlatitude storms but and increase in intense storms", repeat the same idea in the previous phrase (starting in line 49). [PATRICIO ACEITUNO]	This paragraph has been re-written.
10-1203	A	66:52	66:52	Should there be a period between the two words "storms" and "Regionally"? [Xiaolan L. WANG]	This paragraph has been re-written.
10-1204	A	66:53	66:54	We suggest a slight rewording, including insertion of the word "predicted", ie " … More regional aspects of these PREDICTED changes INCLUDE a more active storm track …"	This paragraph has been re-written.

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				[David & David Wratt & Fahey]	
10-1205	A	66:56	66:57	We assume this sentence refers to model studies of projected future climate, rather than to observations of past changes. If so, we suggest a wording change to: "… that have PREDICTED a poleward shift …" [David & David Wratt & Fahey]	This paragraph has been re-written.
10-1206	A	67:1		a couple things about this answer - first, chap 6 says LOTS about abrupt change, and there should be more compatibility. For example - definitions. Also, chap 6 megadrought and hydrological regime change discussion suggest that it is innappropriate to say (your lines 14-16) that abrupt climate changes are unlikely in the 21st century - perhaps for the examples you cite, but not for regional megadrought, or an abupt shift in hydrologic regime (e.g., to a regime characterized by more frequent, longer droughts - the kind of change that has happened in the past, and certainly can't be ruled out in the future, especially given the large changes in forcing and mean climate state that are likely). I propose more discussion of abrupt change between chapters, and a more rigorous treatment in the SOD of Chap 10. [Jonathan Overpeck]	The focus of this FAQ was large-scale abrupt changes with global implications. For this reason, more regional phenomena such as heat waves or megadroughts are not addressed in this FAQ. Re lines 14-16 we specify clearly which type of abrupt changes are unlikely (MOC and GIS). In particular, a definition of the temporal and spatial extent of the changes we address in this FAQ is given.
10-1207	A	67:1		55. Page 67, line 1 – likely – Much of this seems very likely to me. If sea level increases (very likely or lock) and extreme winds increase (likely) then wave height increases (likely). [Ronald Stouffer]	This paragraph has been re-written.
10-1208	A	67:4	67:25	There seem to be two alternative versions of the same paragraph here. [Richard Wood]	Mis-numbered comment
10-1209	A	68:1		I think this question is a little ill posed. "major" and "abrupt" climate changes could be two very different things. For example, a major climate change could be large decreases in rainfall. The answer nicely addresses the question of "abrupt" but ignores the "major" part of the question. I suggest that Major be defined either in the first paragraph, or in paragraph 3 to state clearly what is being addressed in the question. [Robert Colman]	notion "major" clarified
10-1210	A	68:1		Question 10.2: This generally reads well, but we think it could be improved by a tighter initial paragraph as a "headline answer". A specific suggestion is made below. [David & David Wratt & Fahey]	ok
10-1211	A	68:4	68:17	We think that the second paragraph of this answer ("Based on currently available results …") is a useful short answer to the question. We suggest that thepositions of the first and second paragraph be interchanged, and the new first paragraph ("Based on currently available results ...") be written in italics as a "headline answer". [David & David Wratt & Fahey]	ok
10-1212	A	68:11	68:12	Deciding the question whether abrupt climate change would be "dangerous" is a value	cannot forward ref to WG3. We

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				judgment [see, for example, Keller, K., M Hall, S.-R. Kim, D. F. Bradford, and M. Oppenheimer. 2005. Avoiding dangerous anthropogenic interference with the climate system. Climatic Change 73:227-238; Dessai, S., W. N. Adger, M. Hulme, J. Turnpenny, J. Kohler, and R. Warren. 2004. Defining and experiencing dangerous climate change - An editorial essay. Climatic Change 64 (1-2):11-25.Schneider, S. H. 2001. What is 'dangerous' climate change? Nature 411 (6833):17-19.]. In addition, the word "dangerous" may be seen by many as an interpretation of Article 2 of the UNFCCC. It may be useful to expand on this issue and to refer to chapter 19 of WG II, where this issue is discussed in more detail. [Klaus Keller]	reworded sentence to indicate value judgement
10-1213	A	68:14	68:17	The use of the word "unlikely" in conjunction with guidance notes on addressing uncertainty [IPCC. 2005. Guidance Notes for Lead Authors of the IPCC Fourth Assessment Report on Addressing Uncertainties. http://www.ipcc.ch/activity/uncertaintyguidancenote.pdf, accessed November 1, 2005: IPCC.] could be interpreted as a rather precise probabilistic statement (i.e., a probability larger than 10%, but less than 33%). This is a rather interesting statement, yet the reasoning underlying this assessment is somewhat unclear. What is the specific evidence used for this assessment? Is it possible to provide seperate probabilities for the discussed climate thresholds (i.e., Greenland ice sheet or large-scale ocean circulation changes)? How does this relate, for example, to the assessment of Gregory et al (2004))? [Gregory, J. M., P. Huybrechts, and S. C. B. Raper. 2004. Climatology - Threatened loss of the Greenland ice-sheet. Nature 428 (6983):616-616] How does this relate to the statement on page 69, lines 4-5 of the same chapter? [Klaus Keller]	changed to "not likely" which is not a reserved notion. There are too few studies available to make a semi-quantitative expert judgement
10-1214	A	68:15	68:15	Does unlikey mean a probability of les than33% ? If so, better mention the probability value, as, for such a dramatic event, most readers will interpret unlikely as a much lower probability, if they are not familiar with the Uncertainty Guidance Note. [Michel Petit]	see 10-1213
10-1215	A	68:19		Suggest that the definition of 'abrupt' be reconsidered. 'faster than the perturbation that is inducing the change' is very vague and will not be understood by most. Isn't the issue whether the changes are short compared to expectations based on previous changes or short compared to human scales, ie several generations? [David & David Wratt & Fahey]	specified
10-1216	A	68:33		Suggest using term other than 'shut-down'. [David & David Wratt & Fahey]	added "collapse" which is also a widely used word in the public
10-1217	A	68:34	68:35	The only mention of the large-scale meridional temperature contrast as the ultimate cause of the zonal wind system is too simple. Terrestrial rotation (Coriolis effect) is also a	included "rotating Earth"

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				strong forcing of atmospheric circulation at these latitudes. [Serge PLANTON]	
10-1218	A	68:36	66:36	Need to reword "cannot shut down", to put this a little better. Also should be a little more explicit with the "temperature contrasts" discussed, to say why the Gulfstream is such a robust feature. [Robert Colman]	reworded
10-1219	A	68:50	68:50	"intensification" is more appropriate than "acceleration" (see comment n 22). [Serge PLANTON]	ok
10-1220	A	68:51	68:52	We suggest the insertion of the words "would" and "predicted" to make it clear this is about projections rather than observations, ie: "… Both effects WOULD reduce the density of the water …This reduction IS PREDICTED TO PROCEED IN LOCKSTEP …" [David & David Wratt & Fahey]	ok
10-1221	A	69:8	69:8	Need to reword this along the lines of "No climate model has produced such an outcome" [Robert Colman]	ok - more straightforward
10-1222	A	69:8		Although perhaps appropriate in this case, it would be unfortunate if all phenomena for which "no climate model simulation exists that would produce such an outcome" would have to be classified as "mere speculation." Ignoring intense events like tornados, there are a good many large scale phenomena that are not produced by models; the sentence might be more effective and less harmful if rephrased. [Leonard A. Smith]	agree in principle. However, discussion is clearly focused on MOC and ice age triggering.
10-1223	A	69:10	69:10	Suggest either "…concerning the magnitude of climate sensitivity" or " …concerning how large or small climate sensitivity could be". Present text could suggest a bias toward worrying about the high end. [Richard Wood]	should be 63:9 sentence adjusted
10-1224	A	69:19	69:20	…increased meridional transport of moisture is unable to compensate for this." is unclear. I assume that this should say that increased precipitation (by snowfall) cannot compensate for the melting. [Robert Colman]	ok - changed to intensified, used before.
10-1225	A	69:20	69:20	Saying that the possibility exists that the Greenland ice sheet may reduce its size substantially is too weak. Considering that there is evidence that there is already melting taking place, and that some studies suggest that with reasonably modest warming total melting is likely to occur, the sentence should indicate a much higher likelihood of occurrence than it simply being a possibility. [Robert Colman]	delete "the possibility exists"

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10-1226	A	69:20	69:21	It is more than a possibility that the issue will decay with sustained warming. [William Hare]	ok see 10-1225
10-1227	A	69:23	69:24	This sentence is a description of model results and also depends on what is meant by "slow" hence it needs to be qualified as the question in this section is about the risks of abrupt change: 1) 0.5m/century SLR from the GIS decay could be expected if the polar amplification is larger than estimated by the models and similar to observations 2) there is evidence from the Eemian of a metre scale contribution from the GIS contributing to an SL high stand at at ca125 kyrs BP within a few centuries Stirling, C. H., T. M. Esat, et al. (1998). "Timing and duration of the Last Interglacial: evidence for a restricted interval of widespread coral reef growth." Earth and Planetary Science Letters 160(3-4): 745-762. [William Hare]	no action - slow is clearly quantified
10-1228	A	69:23	69:24	a slow process taking many hundreds of years is formulated to vague. This might be interpreted as a collapse or total disappearance within 300 years implying a sea level rise of say 15 mm/yr. I don't think the model experiment done so far justify this so the formulation should be rephrased more clearly so that there can be no misunderstanding about the upper limit of sea level rise due to a collapse of Greenland [Roderik S.W. Van de Wal]	clear as is. 300 years is not "many hundred years" but rather "a few hundred years".
10-1229	A	69:26	:33	Suggest this material should be brought into line with section 10.6.7. [John Church]	changed wording
10-1230	A	69:30	69:33	"no quantitative information…" is wrong. Perhaps you mean no information from the current generation of ice sheet models. Other approaches (Oppenheimer 1998; Oppenheimer and Alley 2004, 2005) provide plenty of quantitative information on this point, as noted above. These ought to be referenced here as discussing scenarios for deglaciation of WAIS on multi-century timescales, which is indeed relatively abrupt. Similarly, Hansen's (Climatic Change, 2005) discussion of abrupt deglaciation, most likely applciable to Greenland, ought to be mentioned. [Michael Oppenheimer]	added specifier. No ref. in FAQs.
10-1231	A	69:33		I consider that it is pertinent to add the idea that the net contribution of Antarctica to sea level rise during the 21st century will be negative, as expressed in page 56, lines 9-11. [PATRICIO ACEITUNO]	This FAQ is on abrupt change and not about the slow changes during the 21st cty.
10-1232	A	70:13	70:13	Suggest "…against various sets of observations…" [Richard Wood]	Mis-numbered comment
10-1233	A	70:17	70:17	The impact of choice of observational constarint is poorly understood. Suggest "…with different models and different observational constraints, to estimate the contribution of structural uncertainties and choice of observations to the results." [Richard Wood]	Mis-numbered comment

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10-1234	A	70:20	70:21	I don't expect this will be published in time. I've mentioned possible alternative references at the appropriate place in the main text [James Annan]	References have been revised and updated accordingly.
10-1235	A	70:24	70:25	Full reference now available (although it's not actually appeared): Sola Vol 1 pp 181-184, 2005. [James Annan]	References have been revised and updated accordingly.
10-1236	A	70:42	70:42	This box does a superb job of drawing together the various pdfs. However I feel it is incomplete without a short summary of the analysis of the component climate sensitivity feedbacks (section 8.6). This analysis provides a complementary approach and contributes to understanding and quantitative confidence in the ranges given – see Ch 8 p 3 ll 29-34, and p 5 l 55 to p 6 l 30. I suggest the most appropriate way to do this would be to ask the LAs responsible for 8.6 to draft a short paragraph. [Richard Wood]	This text has been re-written.
10-1237	A	70:53	86:15	In the list of references, page numbers are missing for those references on line 53 of page 70; line 22 of page 71; lines 7, 20, 40, 54 of page 73; lines 25, 27,35 of page 74; lines 8, 40 of page 75; lines 21, 57 of page 77; lines 20, 53 of page 78; lines 46, 56 of page 79; line 23 of page 81; line 46 of page 82; lines 29, 50 of page 84; and lines 8, 15 of page 86. [Chiu-Ying LAM]	References have been revised and updated accordingly.
10-1238	A	71:16	71:16	Suggest that the caption to Fig. B10.2.1(b) makes it clear that the Annan and Schneider lines are added there for convenience of display but are based on a different method (LGM). [Richard Wood]	Revision made.
10-1239	A	71:37	71:38	Add the following reference between lines 37 and 38: "Caires, S., V. R. Swail, and X. L. Wang, 2005: Projection and analysis of extreme wave climate. J. Climate, accepted subject to revision." (see Comment #29 above). See file "CairesSwailWang_GEV_GPD.pdf" on the anonymous ftp site given in Comment 26 above. [Xiaolan L. WANG]	References have been revised and updated accordingly.
10-1240	A	72:44	72:44	Replace "in press" with "8, 2990–3013". [Aiguo Dai]	References have been revised and updated accordingly.
10-1241	A	75:23	75:24	The complete reference to Haarsma et al is given in remark 3. [Reindert Haarsma]	References have been revised and updated accordingly.
10-1242	A	75:39	73:40	A better reference is: J. C. Hargreaves and J. D. Annan, 2006, Using ensemble prediction methods to examine regional climate variation under global warming scenarios. Ocean Modelling Vol 11 Nos 1-2 p174-192 (mentioned where referenced above) [James Annan]	References have been revised and updated accordingly.

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10-1243	A	79:11	79:12	Full reference for McDonald et al. 2005. McDonald RE, Bleaken DG, Cresswell DR, Pope VD and Senior CA (2005) Tropical storms: representation and diagnosis in climate models and the impacts of climate change. Climate Dynamics 25: 19-3 [Ruth McDonald]	References have been revised and updated accordingly.
10-1244	A	80:3	80:3	Nature vol 429 should be changed to 430 [Andrew Lacis]	References have been revised and updated accordingly.
10-1245	A	81:50	81:53	The Rauthe et al 2004 reference is listed twice. [Ron Miller]	References have been revised and updated accordingly.
10-1246	A	85:15	85:16	Vellinga and Wood 2005 is currently under review by Climatic Change, and on target to be accepted by January 2006. [Michael Vellinga]	References have been revised and updated accordingly.
10-1247	A	85:27	85:29	Update this reference to "Wang, X. L., and V. R. Swail, 2005a: Historical and possible future changes of wave heights in northern hemisphere oceans. In: Atmosphere-Ocean Interactions - Vol. 2 [Perrie, W. (ed.)]. Advances in Fluid Mechanics Series Vol 39. Wessex Institute of Technology Press, Southampton, UK. ISBN: 1-85312-929-1, apx 300 pp." (see file "AtmosphereOceanInteractions-Vol2-Jan20.pdf" on the anonymous ftp site given in Comment 26 above) [Xiaolan L. WANG]	References have been revised and updated accordingly.
10-1248	A	85:32	85:32	Update "2005" to "2005b". [Xiaolan L. WANG]	References have been revised and updated accordingly.
10-1249	A	85:33	85:33	Update "submitted" to "in press". [Xiaolan L. WANG]	References have been revised and updated accordingly.
10-1250	A	86:43	86:43	In association with comment # 14, insert a new reference "Wu., M.C., and J.C.L. Chan, 2005 : Observational relationships between summer and winter monsoons over East Asia. Part II : Results. Int. J. Climatology, 25, 453-468". [Chiu-Ying LAM]	References have been revised and updated accordingly.
10-1251	A	88:6	88:7	In Table 10.3.1 a), insted of "CCSM3, USA ", " CCSM3, USA and Japan" is strongly recommended and NCAR hase already agreed it through the formal MOU between NCAR and CRIEPI. <Note> CRIEPI completed IPCC runs with CCSM3 using the Earth Simulator before NCAR did. CRIEPI sent the data set to NCAR and NCAR merged the CRIEPI data set and the NCAR data set and sent the aggregated data set to PCMDI according to the official MOU of collaboration between NCAR and CRIEPI. Many scientists in NCAR and other research organizations in the world used the data set provided by CRIEPI and they made many excellent paperes already referred in AR4. The internationl collaboration between NCAR and CRIEPI greatly contributed for IPCC AR4.	Added "run in US and Japan".

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				[Koki Maruyama]	
10-1252	A	88:8	88:9	In Table 10.3.1 b) , insted of "CCSM3, USA ", " CCSM3, USA and Japan" is strongly recommended and NCAR hase already agreed it through the formal MOU between NCAR and CRIEPI. <Note> CRIEPI completed IPCC runs with CCSM3 using the Earth Simulator before NCAR did. CRIEPI sent the data set to NCAR and NCAR merged the CRIEPI data set and the NCAR data set and sent the aggregated data set to PCMDI according to the official MOU of collaboration between NCAR and CRIEPI. Many scientists in NCAR and other research organizations in USA used the data set provided by CRIEPI and they made many excellent paperes already referred in AR4. The internationl collaboration between NCAR and CRIEPI greatly contributed for IPCC AR4. [Koki Maruyama]	Added "run in US and Japan"
10-1253	A	90:0		There should be a dashed arrow linking climate model response to concentrations as the C4MIP models include this link [Fortunat Joos]	We appreciate this connection, but this figure is simply illustrative of contributions to uncertainty with four parts
10-1254	A	91:0		Fig. 10.2.1 Move X-axis label to outside of graph. [Melinda Marquis]	Accepted
10-1255	A	91:0		fig 10.2.1; the device showing the interpretation of the box whisker graph should be rotated 90 degrees counterclockwise (but the text inverted) to conform with the actual bars in the figure; ditto fig 10.2.2. [Stephen E Schwartz]	Accepted
10-1256	A	91:6	91:7	Explain in a foot note what means "box-and whisker diagram representing percentiles". This concept is not part of the background of any European policy maker. [Michel Petit]	Accepted.
10-1257	A	92:0		Figure 10.2.2 Use "TAR" on legend in Figure rather than ambiguous "IPCC" [Melanie Fitzpatrick]	Accepted
10-1258	A	92:0		Fig. 10.2.2Move X-axis label to outside of graph. [Melinda Marquis]	Accepted.
10-1259	A	92:6	92:7	Explain in a foot note what means "box-and whisker diagram representing percentiles". This concept is not part of the background of any European policy maker. [Michel Petit]	Accepted
10-1260	A	93:0		Figure 10.2.3 The caption and/or the associated text must explain what is meant by these forcings as a function of altitude. It does not appear that forcings at different altitudes would be consistent with the definition of forcing given in Section 2.	ACCEPTED -- the caption and the text now explain that the forcings plotted in figure 10.2.3 are

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				The definition of radiative forcing from the TAR and earlier IPCC climate assessment reports is retained. Ramaswamy et al. (2001) define it as "the change in net (down minus up) irradiance (solar plus long-wave; in W m –2) at the tropopause AFTER allowing for stratospheric temperatures to readjust to radiative equilibrium, but with surface and tropospheric temperatures and state held fixed at the unperturbed values". [Stephen E Schwartz]	instantaneous changes in net fluxes without stratospheric adjustment
10-1261	A	93:0		Figure 10.2.3 Figure 10.2.3. Comparison of shortwave and longwave radiative forcings for doubling CO2 from its concentration in 1860 for AOGCMs and line-by-line (LBL) radiative transfer codes (Collins et al., 2005b). The figure caption should specify the substance or process responsible for the shortwave forcing. The implication is that the (negative) shortwave forcing is due to doubling of CO2. [Stephen E Schwartz]	ACCEPTED -- the caption now notes that the forcing in the shortwave is due to absorption bands of CO2 in the near infrared
10-1262	A	96:0		Figure 10.3.2. The reference period is missing. A comment on the discontinuity at 2100 due to the change of ensemble size, might be added. [Serge PLANTON]	Accepted.
10-1263	A	96:0		Please, explain the meaning of the shaded areas around the lines. [Ilkka Savolainen]	Accepted.
10-1264	A	97:0		Figure # 10.3.3: It would help the figure if a thin dotted line at y=1.0 could be added. [David Sexton]	This figure has been revised.
10-1265	A	97:1		As I said, too many figures ! [FILIPPO GIORGI]	Number of figures has been reduced.
10-1266	A	98:0		Fig.10.3.4 Add label to X axis and to color bar. [Melinda Marquis]	X axis is latitude, color bar is temperature
10-1267	A	100:0		Fig.10.3.6 Add label and units to color bar. [Melinda Marquis]	Color bar corresponds to respective figure titles
10-1268	A	100:1		I much appreciate the inclusion of seasonal plots. [FILIPPO GIORGI]	Noted. Thank you.
10-1269	A	101:0	102:	In Fig. 10.3.7 b and 10.3.8b, the stippling is nearly invisible due to the fact that the authors use the same color as for the shading. The meaning of the stippling should be explained in the corresponding figure caption. [Christoph, C. Raible]	Accepted.
10-1270	A	101:0		Fig. 10.3.7 Add label to color bar. [Melinda Marquis]	Color bar label is at right (%)
10-1271	A	102:0		Fig.10.3.8 Add label to color bar. [Melinda Marquis]	Color bar label is at right

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10-1272	A	103:0	103:	The color bar (red for negative and blue for positive changes) used for all panels is confusing. In particular, it is inverse to the color bar used in Figure 10.3.6. So my suggestion is to change the color bar for all precipitation figures to green for positive and yellow to brown for negative changes. Note that in weather forecasting offices green is used to indicate precipitation in weather maps. [Christoph, C. Raible]	A matter of style.
10-1273	A	103:0		Fig.10.3.9 Label graphs: a, b, c and d. Put graphs in order, e.g., a and b on top, then c and d on bottom. They're currently out of order. Add labels to color bars. [Melinda Marquis]	Panel labels added and ordered. Color bar labeled at right.
10-1274	A	104:0	104:	In Fig. 10.3.10 a, the time series denoted by "+" is not explained in the legend or the caption. [Christoph, C. Raible]	Rejected. Caption points out it is thr COMMIT run for CNRM-CM3
10-1275	A	104:0		Figure 10.3.10(a). Showing all models and three scenarios, as well as the commitment "scenario" produces a plot that is way too busy. It is impossible to tell one scenario from another and also what the spread in models contributes compared with the spread in scenarios. I suggest this plot be redrafted with dramatically fewer lines. A possible alternative would be to show a mean model result from the different scenarios, along with error bars indicating standard deviation (or some other measure of spread). Another possibility would be to show 'envelopes' of projected changes for each scenario in a different colour. [Robert Colman]	Rejected. Figure taken from published literature.
10-1276	A	104:0		Figure 10.3.10(a). There is an obvious outlier showing a roughly constant (and anomalously large) sea ice extent. If this is in error (as it appears to be) it should be removed. [Robert Colman]	Rejected. Figure taken from published literature.
10-1277	A	104:0		Fig. 10.3.10 Add labels to X and Y axes. [Melinda Marquis]	Rejected. Standard notation used.
10-1278	A	104:0		Figure 10.3.10.a. Intermodel differences should be presented in an other form (rather with a coloured area for only one scenario) since each individual curve cannot be distinguished. [Serge PLANTON]	Rejected. Figure taken from published literature.
10-1279	A	105:0		Fig. 10.3.11 Add label and units to color bar. [Melinda Marquis]	Rejected. Standard notation used.
10-1280	A	106:0		Fig.10.3.12 Add label and units to color bar. [Melinda Marquis]	Taken into account: Figure deleted
10-1281	A	107:0	107:	Figure should have clearer caption and colours	Rejected. Standard notation used.

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				[Axel Michaelowa]	
10-1282	A	107:0		Fig.10.3.13 Add label to color bar. [Melinda Marquis]	Rejected. Standard notation used.
10-1283	A	108:1	108:17	Figure 10.3.14 is very difficult to read and is presented in a very different way to the equivalent figure in the TAR, which makes comparing changes in results between the present and previous assessment problematic. I think that the way of presenting the results used here is better. Two suggestions: a) add an extra figure showing the TAR results in the same format; b) do a further less cluttered figure showing the results from only those models that are consistent with late 20th century observations. [Meric Srokosz]	Noted.
10-1284	A	109:0		Figure 10.3.15 [cited in section 10.3.5 on p24]. I suggest to replace this Figure by a more recent one from (van Oldenborgh, Philip and Collins 2005) [already included in references of Chapter 10] available on http://www.knmi.nl/~oldenbor/mm_enso_changes.gif . The darker colours indicate models with a more reliable ENSO cycle, in particular a reasonable balance between surface and thermocline modes and a spectrum that resembles the observed one. [Gerrit Burgers]	Thanks for the suggestion, but we have replaced the existing figure with another from a similar comparison of 18 AOGCMs that shows roughly the same thing.
10-1285	A	110:0	111:0	These are wonderful figures. Can they be composited with an image as in the work of Wallace and colleagues, showing the regression on the NAM and SAM changes of the temperature and precipitation patterns to be expected at some time (say 2050) ? Images of that type communicate to the non-expert how much and where of a change in temperature or precip to expect in a way that an index cannot. [Susan Solomon]	Nice idea, but no figure like this exists presently that we are aware of. We are now combining Fig. 10.3.16 with 10.3.17 into a single two-panel figure. Additionally, in Fig. 10.3.9 we show changes in precipitation and temperature that include the chagnes in SAM and NAM.
10-1286	A	110:0		Fig. 10.3.16 Add units and labels to X and Y axes. [Melinda Marquis]	Accepted
10-1287	A	111:0		Fig. 10.3.17 Add units and labels to X and Y axes. [Melinda Marquis]	Accepted
10-1288	A	112:0	115:0	These are interesting figures but units of standard deviations are hard for the non-expert to interpret. Please consider other ways of presenting this data, or graphical ways to explain what a change of this type would represent. For example, changes in frost days and growing season length could be clearer in units of days. [Susan Solomon]	This is a good suggestion, but such a depiction in non-normalized units across the models is very noisy. The main message here is qualitative changes in these indices
10-1289	A	112:0		Fig. 10.3.18 Add units and labels to X and Y axes. [Melinda Marquis]	Accepted

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10-1290	A	113:0		Fig. 10.3.19 Add units and labels to X and Y axes and to color bars. [Melinda Marquis]	Caption gives units
10-1291	A	114:0		Fig. 10.3.20 Add units and labels to X and Y axes. [Melinda Marquis]	Caption gives units
10-1292	A	115:0		Fig. 10.3.21 Add units and labels to X and Y axes and to color bars. [Melinda Marquis]	Caption gives units
10-1293	A	116:0		Please indicate also the TAR error bar. For example, an arrow could be shown for year 2100 [Fortunat Joos]	Comment taken into account
10-1294	A	117:0	117:	Can one add axis labels to the figure? [Klaus Keller]	Accepted
10-1295	A	117:0		Fig. 10.4.2 Add units and labels to X and Y axes. [Melinda Marquis]	Accepted
10-1296	A	118:0	118:	Can one add axis labels to the figure? [Klaus Keller]	Accepted
10-1297	A	118:0		Fig. 10.4.3 Add units and labels to X and Y axes. [Melinda Marquis]	Accepted
10-1298	A	119:0		Figure # 10.5.1: What the black circles in b) and c)? [David Sexton]	They are the range from the TAR. Change made.
10-1299	A	121:6		change "fourfould" for "fourfold" [PATRICIO ACEITUNO]	Figure has been deleted
10-1300	A	123:0		Please delete figure 10.5.5. There is no need to spend a figure on a single study. The material of this figure is already in Fig 10.15.7. [Fortunat Joos]	Accepted. Figure a candidate for supplementary material.
10-1301	A	123:0		Fig. 10.5.5 Add labels a, b, c, d, e and f to graphs. [Melinda Marquis]	Figure a candidate for supplementary material
10-1302	A	124:0	124:	What is the source of this figure? What is the source for the observations and the associated uncertainty? [Klaus Keller]	Figure has been deleted
10-1303	A	124:0		Fig. 10.5.6 The label "observations *with uncertainty* makes me wonder why the "uncertainty" or how much "uncertainty. Are these just observations? Clarify uncertainty of observations. [Melinda Marquis]	Figure has been deleted
10-1304	A	124:13		change "Terraton" for "Teraton" [PATRICIO ACEITUNO]	Figure has been deleted

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10-1305	A	124:14		change "Petagramm" for "Petagram" [PATRICIO ACEITUNO]	Figure has been deleted
10-1306	A	125:0	125:	What is the source of this figure? [Klaus Keller]	Figure has been deleted
10-1307	A	126:0	126:	What is the source of this figure? [Klaus Keller]	Figure has been deleted
10-1308	A	126:0		Figure#10.5.8: This looks a very nice figure but the fact that red is used for both the historic forcing and the A2 SRES scenario, gives the impression that this scenario look like it is somehow better and more preferable than the others. Is it possible to change the colour of the historic part of this time series? Same can be said for figures # 10.5.6 and # 10.5.7. [David Sexton]	Figure has been deleted
10-1309	A	128:0		Fig. 10.5.10 Add labels a and b to graphs. [Melinda Marquis]	Figure has been deleted
10-1310	A	129:0		Fig. 10.5.11 Remove labels a and b from graphs, because they're in wrong graphs (caption doesn't match graphs currently). Add labels a, b and c to graphs -- each label on correct graph. [Melinda Marquis]	Accepted
10-1311	A	129:6		This is what you get when you use untested models with the discredited SRES Scenarios [Vincent Gray]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-1312	A	130:0		Figure # 10.5.12: X-axis title should read "surface warming threshold". [David Sexton]	Accepted
10-1313	A	132:0		Fig. 10.5.14 Add labels a and b to graphs. [Melinda Marquis]	Figure has been deleted
10-1314	A	133:0		Fig. 10.5.15 Edit caption to refer to parts a and b, rather than to :first panel: and "second." Add labels and units to Y axes. [Melinda Marquis]	Figure has changed
10-1315	A	134:5		This diagram is nonsense because CO2 can never increaseby 1% a tear [Vincent Gray]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-1316	A	135:0	135:	Which pdfs are fitted with normal distributions? Just the "multi-model ensemble"? Can	Accepted

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				one link this statement to the figure legend? [Klaus Keller]	
10-1317	A	135:0	135:	The color coding between Knutti vs AR4 AOGCMs is very difficult to distinquish. [Klaus Keller]	Style question
10-1318	A	135:0		Fig. 10.5.17 Explain dashed versus solid lines. Is it simply that the former refer to 2020-2030 and the latter refer to 2090-2100? [Melinda Marquis]	Accepted.
10-1319	A	135:6		The SRES Scenarios are discredited [Vincent Gray]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-1320	A	138:0		Fig. 10.6.1 Add label and units to Y axis (Time, in years). [Melinda Marquis]	Caption specifically says "during the 21st century"
10-1321	A	138:5		Again, the SRES Scenarios are dubious [Vincent Gray]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-1322	A	139:0		Fig. 10.6.2. Add label and units to X and Y axes and to color bars. [Melinda Marquis]	Latitude and longitude labels (N-S, E-W) accompany each number, and units are given in caption
10-1323	A	140:0	140:0	This is an important concept, but this figure will be very difficult for the non-expert to get much out of. Please consider presenting something that better communcates the likelihood of a major change in Greenland ice, on what time scale, for what stabilization level. [Susan Solomon]	Legend in figure and caption state the SRES scenarios and sabilization at 2100
10-1324	A	140:0		Fig. 10.6.3 Add labels to X and Y axes. [Melinda Marquis]	Y axis is already labeled, x axis is years
10-1325	A	141:7		How unrealistic can you get? [Vincent Gray]	A clarifying paragraph elaborating on the use of idealized and SRES emission scenarios has been added to the Introduction
10-1326	A	143:2		The dashed curve in the legend does not match the color of the dashed curve in the figure (weird)	Figure has been modified

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				[David Rind]	
10-1327	A	146:0		Box 10.1, Fig. 1. The concept of thresholds/bifurcations is important but I am not certain that this figure conveys the point very strongly. Perhaps it might if it were combined with a fold/cusp catastrophe plot -- even showing state trajectories for rapid and gradual state transitions. Also: the arrow indicating the "bifurcation point" is pointing at the one curve (solid line) that has no bifurcation. If you plan to stick with this figure then the arrow should point to the long-dashed curve. [Garry CLARKE]	Style question under consideration.
10-1328	A	147:0		Box 10.2 Fig 1 In the Annan et al results, there should be a triangle (max likelihood estimate) at 4.5C. The lower limit is undefined, and should not terminate at 4C. It would be incorrect to represent our results as implying a high confidence that climate sensitivity is greater than 4C. It is not clear how to best show this on the figure, though (extend the left with dots: ....--*------|). Strictly speaking it is not a pdf at all, although the top end seems likely to be robust. [James Annan]	Point taken, and will try to accommodate.
10-1329	A	147:0		Box 10.2, Fig. 1 Change "c/d" to "c and d" analogous to "a and b above, but using …" Chane "e/f" analogously. [Melinda Marquis]	Accepted
10-1330	A	147:0		Box 10.2 Figure # 1e) The Stainforth et al estimate is not a PDFs, therefore the y-axis title should not be "PDF" or a caveat should be placed in figure caption. [David Sexton]	Accepted
10-1331	A	148:0	148:	Are these cdfs based on the truncated pdfs? If this is the case, then it should be clearly stated in the figure caption. In general, it would be very useful to discuss whether the main conclusions would change for a different methodological choice of truncation method. [Klaus Keller]	Revised