IPCC Working Group I Fourth Assessment Report

Expert and Government Review Comments on the Second-Order Draft

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.

Batch AB (15 June 2006)

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-1	A	0:0	0:0	At several occasions, it is stated that precipitation decreases in midlatitudes. However, if one accepts the probably most common definition of midlatitudes as the area of the Ferrel cells at 30-60°N/S, this is not consistent with model results. For example, in Fig. 10.3.3. precipitation decreases on land only extend to about 40-45°N/S. I would call the area equatorward of 40-45°N/S "lower midlatitudes". [Govt. of Finland (Reviewer's comment ID #: 2009-117)]	Accepted Changes are made in 10.3.2.3. and page 10-4.
10-2	A	0:0	0:0	The report does not combine sea level change projections from thermal expansion with other drivers and so does not generate a single range of sea level rise through this century, as was done in the TAR; and as also the AR4 does do for temperature. The result is no range of 'engineering' numbers for SLR, an inconsistency in the SPM between projected temperature range and SLR, and an obvious inconsistency in approach between the TAR and AR4. Users, including policy and decision makers, with find this very hard to accept. It might appear as though the TAR projections were inappropriate and devalue the IPCC review. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-29)]	Taken into account. The SOD does combine components of SL rise in 10.6.5, and this is reported in the summaries. However it was evidently unclear and will be improved. Rejected request for a single range, since unlike the TAR, chapter 10 is not producing a combined scenario range for temperature or SL, but we will produce SL ranges for the marker scenarios.
10-3	A	0:0	0:0	On possible changes of the MOC and the rise of sea-level, there is considerable uncertainty and it is necessary for the WG to steer a careful path between scare-mongering and complacency. At present the balance is dangerously close to complacency, mainly because (a) too great reliance is placed on the results of coupled AOGCMs (which have to some extent been constructed to be well-behaved, and may therefore be too well-behaved c.f. the real world) compared to simpler models which have been more extensively "exercised", and (b) the possible effects of the melting of Greenland have been largely ignored in both cases (important anyway, but especially so if this occurs more rapidly than hitherto expected, c.f. Rignot & Karangaratnam, 2006). Moreover, the ability of any models (but especially AOGCMs) to simulate satisfactorily changes in the MOC has not yet been well established, and will not be until they have satisfactorily modelled past glacial/interglacial and millennial variations, for which the necessary data (e.g. Pa/Th data) is still sparse. Frankly the results given in Fig 10.3.13 are all over the place (as they were in the TAR) and suggest that one could probably get any answer one wanted, and detailed investigations with EMICs (e.g. Rahmstorf et al, GRL, 2005, Marsh et al, Climate Dynamics, DOI 10.1007/s00382-004-0474-1) show why this may be so. Therefore any statements w.r.t. the MOC should be made with considerable caution, and I really do think that saying that the MOC is "very unlikely" to undergo a large abrupt transition during the next century displays an unwarranted level of confidence. I suggest that one should say that it is "probably unlikely", accurately reflecting the fact that it can	Noted. These comments have been addressed individually later on. "probably unlikely" does not have a statistical meaning. Very unlikely means less than 10% chance.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				reasonably be judged to be unlikely, but also that we have only moderate confidence in the accuracy of the statement. Moreover it would be fair to add a comment such as "but the possibility cannot be excluded" after the statement which relates to beyond the 21st century. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-30)]	
10-4	A	0:0		This chapter was considered well structured with significant detail well weaved into the overall picture of future global projections. The Executive Summary is also well structured and clear, however, it could include a section on the possibility of abrupt change and climate sensitivities to further increase its utility for policy makers. [Govt. of Australia (Reviewer's comment ID #: 2001-372)]	noted. The mention of other changes, summarised in box 10.1. is not warranted because they are not yet considered robust results.
10-5	A	0:0		The chapter's structure distracts from its message. The authors are recommended to have a separate section on emission scenarios at the outset of the chapter. This section then also needs to make the distinction between scenarios with and without climate policy. In view of the public discussions on SRES scenarios a few years ago, their background requires better explanation. In addition, there should be one single section providing a comprehensive assessment of all information related to sea-level rise. [European Commission (Reviewer's comment ID #: 2008-28)]	First part of comment ACCEPTED: a sentence has been added in the introduction that this chapter considers scenarios without policy intervention and also draws attention to overshoot scenarios sea level section is revised. Suggestion regarding sea level rise rejected. No organization of the material is ideal. We chose this one because we want to put the long-term projections for sea level in 10.7 with the other long-term aspects of climate change, although we recognize that separates it from related material in 10.6.;
10-6	A	0:0		My overall impression is that Chapter 10 represents a very impressive and comprehensive review of the state-of-the art in future climate assessments. In general it is well balanced and unbiased [Andrey Ganopolski (Reviewer's comment ID #: 80-1)]	Thanks.
10-7	A	0:0		I have some problem with consistency between some recent findings and projections of sea level rise Unlike previous IPCC reports, 4AR does not provide a full range of a possible range of sea level rise during 21st century. The only numbers given on page 10-4 is the range 14-44 cm for scenario A1B. (Surprisingly, the only figure with future projection of sea level rise is given in Chapter 5, Question 5.1 Figure 1, and although the figure caption refers to the Chapter 10, the figure shows a very different range for sea	REVISED: ranges are now given for ECS, TCR, T2100, and sea level

No.	Batch	Page:line From	Page:line To	Comment	Notes
				level rise from that given in Chapter 10). Since no other numbers are given, I would assume that this range will be broadly used as the "official" 4AR projection. This range is notably smaller and much more optimistic than those given in SAR (13-94 cm) and TAR (9-88 cm). Moreover, the median value for the averaged sea level rise over 21st century in accordance with this prediction (2.9 mm/yr) is smaller than the rate of changes over the last 10 years and only 50% higher than over the last 50 years. My feeling is that the recent findings do not provide a support for such optimism. In particularly, the recent data show a considerable acceleration of sea level rise and rapid melting of Greenland, not mentioning repeated claims that climate sensitivity may be well above the IPCC range. All these facts cannot explain why the new projection is so low. [Andrey Ganopolski (Reviewer's comment ID #: 80-2)]	
10-8	A	0:0		Moreover, as it is discussed in Chapter 9 (pages 41-43), there is a clear problem with the explanation of observed sea level rise by climate models. The same Chapter 9 attributes only 30% of recent sea level rise to the thermal expansion and requires contribution from glaciers and ice sheets of about 1.5 mm/yr over the last 40 years, and 2 mm/yr over the last 10 years. At the same time, by extracting the estimates for thermal expansion (230+-100) from the total estimate for sea level (290+-150) given on the page 10-4, only 20 to 100 mm are left for all other components of sea level rise, i.e. only 0.2-1 mm/yr. While I understand that the numbers given in Chapter 10 are solely based on the results of model simulations and the main task of Chapter 10 is to present results of model simulations, I am afraid that these results alone, without explicit mentioning of caveats and existing problems in attribution of observed sea level rise, will be frequently misused as an argument against serious consequences of future sea level rise. [Andrey Ganopolski (Reviewer's comment ID #: 80-3)]	Accepted. The comparison with models will be clarified in chapter 9.
10-9	A	0:0		Another issue is related to the estimates of climate sensitivity range. It is written on page 10-43 (line 48) "On the other hand, there is a consensus from most studies cited above using observational constraints (see Section 10.5.4.) that the upper limit of climate sensitivity is uncertain, with a substantial probability for sensitivity above 4.5°C, and that the current AOGCMs therefore do not cover the full possible range of sensitivities". This is a very strong statement. If, indeed, there is "a substantial" probability (as it is shown on Fig. Box 10.2, Figure 2) that ALL climate models underestimate CS (and, possibly, substantially underestimate), than this should have strong implications for the future predictions, and, especially, for committed global warming and sea level rise. However, although the issue of CS is extensively discussed both in Chapter 9 and 10, I found no mentioning of such implications in the 4AR report. [Andrey Ganopolski (Reviewer's comment ID #: 80-4)]	Partly accepted. Paragraph reworded, now mentiones only that GCMs do not sample high sensitivities, no probability given. Likely range is given in box 10.2. Observations do constrain TCR and SRES uncerainty (see box 10.2 and sec. 9.6.1.3), high sensitivities will only become important on very long timescales. As there are no GCM simulations available, implications cannot be assessed.
10-10	A	0:0		The chapter sould give a comprehensiv and cross cutting risk assessment of possible	First part of comment REJECTED: risk

No.	Batch	Page:line From	Page:line To	Comment	Notes
				future developments rather than only reporting model results. Examples for inappropriate comprehensive risk assessments are the assessment of future sea level rise (The executive summary states that SLR from 2000 to 2020 is projected as 34 +- 25 mm despite chapter 5 reports the observed rate from 1993-2005 as 3.2 mm/year. If this trend simply remains constant, the sea level could rise by 64 mm from 2000 to 2020.) and the assessment of future changes in MOC. With regard to MOC it should be mentioned that most models miss important processes (the crucial oceanic convection processes, or the overflows), which is a caveat that should be mentioned wherever these model results are discussed. By this reason the models give very widely differing results on MOC. The executive summary statement that "It is very unlikely that the MOC will undergo a major abrupt transition in the 21st Century" implies a greater confidence and much smaller uncertainty than the literature actually reports. For this case the "Guidance Notes for Lead Authors of the AR4 on Adressing Uncertainties" give clear guidance for communicating these uncertainties. In accordance to these guidelines (table 2) please add to every assessment both the amount of evidence and the level of agreement or consensus among the experts. Furthermore include in the assessment the following points and the relevant literature: (a) A discussion of the uncertainty of a shutdown threshold (how much freshwater is needed for that - see e.g. the Rahmstorf et al. (GRL 2005) model intercomparison). Information from chapter 6 on amounts that caused paleoclimatic shut-downs could also be referred to here. (b) A discussion of possible current and future freshwater sources and their uncertainty. This should include estimates of the currently observed freshwater input; e.g., from melting sea ice 0.014 Sv (Lindsay and Zhang 2005), from Greenland 0.007 Sv (Rignot and Karangaratnam 2006), and from Eurasian rivers 0.005 Sv (Peterson et al. 2002) - even without precipitation over the oceans and Canadian river runoff, this is together about a quarter of what is widely considered a rough critical magnitude (0.1 Sv). It should be discussed how those input terms might evolve in future with increased warming. (c) A discussion of ongoing observed changes (with due reference to chapter 5), i.e. salinity changes (Curry and Mauritzen 2005) and possible circulation changes (Bryden et al. 2005) and what they mean for the future. (d) A more comprehensive discussion of low probability. Include observations and paleoclimate evidence in this assessment and do not rely only on reporting what SRES scenarios in models have found. [Govt. of Germany (Reviewer's comment ID #: 2011-55)]	assessment is a task is clearly beyond the remit of this chapter and also beyond the remit of the entire WG1. Re MOC changes, the literature justifies "very unlikely". This implies up to 1 in 10 cases shows such an abrupt change. Currently, no model shows such a change and hence we believe we report cautiously. a,d, b: these fluxes are too small to be relevant for MOC changes ad c: clearly not in the remit of Chapter 10. ad d: we do consider a variety of other scenarios now explicitely mentioned in the Introduction. This chapter will not assess paleoevidence. This is done in Chapter 6. Comments regarding sea level taken into account. We will clarify how the 1993-2003 rate of SL rise is caused. We note that natural and internal variability may have contributed to its high rate.
10-11	A	0:0		The SRES scenarios B1, A1B and A2 are often described as "low", "medium" and "high" emission scenarions. Regarding the impact of these emission scenarios on the disturbance of the radiative balance of the Earth neither of them can be determined as "low" or	The relative size of the radiative forcing is simply a qualitative way of orienting the reader. There is no

No.	Batch	Page:line From	Page:line To	Comment	Notes
				"medium". As such a determination incorporates a clear judgement on their possible realisation potential (which have to be avoid in any case) we propose to delete "low", "medium" and "high" in any place it is mentioned in connection with the scenarios. [Govt. of Germany (Reviewer's comment ID #: 2011-56)]	judgement meant or implied. PARTLY ACCEPTED: these qualifiers are only meant to distinguish between the associated emissions, and not to qualify the response of the climate system. added "wrt emissions" at the beginning of sentence in Intro.
10-12	A	0:0		Figures are best appropriate to illustrate complex issues. We propose to illustrate the causal chain "CO2 emission -> CO2 equivalent concentration -> anthropogenic forcing -> global mean temperature change -> sea level rise" for all SRES scenarios in only one figure. A very good example one can find in TAR SPM of WG I figure 5. Thge advantage of such a presentation is the illustration of the range of change that is caused by the different emission scenarios. [Govt. of Germany (Reviewer's comment ID #: 2011-57)]	REJECTED: Fig 10.5.2 shows this causal chain. Fig 10.1.1 is also enhanced in this respect, but this figure has been revised as well.
10-13	A	0:0		A short table (max. 2 pages) giving the robust findings of the chapter is missed (e.g. as given at the end of chapter 6). Such a table summarizing the direction and where possible the amount and confidence of projected change in specific phenomenon is very helpful especially for policymakers. [Govt. of Germany (Reviewer's comment ID #: 2011-58)]	REJECTED: This is done in the executive summary, but not in table form because of potential redundancy and space constraints
10-14	A	0:0		Whole Chapter. PRESENTATION: Some restructuring of chapter urgently needed in regard to emission scenario description. Specifically, the various bits on the emissions for the analyzed scenarios (sections 10.2.1.1; 2nd paragraph of section 10.3; parts of section 10.2.2; parts of section 10.4.3; certain paragraphs from section 10.5.3.1; including possibly parts of figure 10.5.2) have to be joined on one place; preferrably before the radiative forcing is discussed. Currently, the reader is not informed at all about the starting point of all the subsequent analyses, namely the SRES non-mitigation scenarios and their (different) emission characteristics. It seems just logical, that the starting point, the EMISSION scenarios, are dealt with at the beginning. These changes have to be reflected in the structure of the Executive Summary. [Govt. of Germany (Reviewer's comment ID #: 2011-131)]	REJECTED: Current structure is the result of extensive discussions among LAs. No review of SRES needed at this stage. This discussion has also been clarified.
10-15	A	0:0		Whole Chapter. PRESENTATION: Joining of Sea-level rise sections urgently needed. The different sections on sea-level rise and the dynamics of the various contributors need to be joined in one section. The majority of the discussion on the Greenland and Antarctic ice-sheets in the Commitment section (10.7.4 "Commitment to Sea-Level-Change") is unsatisfactorily located, as it is disjointed of the main SLR section (10.6). The	Rejected. No organization of the material is ideal. We chose this one because we want to put the long-term projections for sea level in 10.7 with the other long-term aspects of climate

No.	Batch	Page:line From	To	Comment	Notes
				"Commitment" chapter does not seem to be the appropriate place for ice-sheet discussions, as the discussed dynamics are not directly related to the "Commitment" scenario run for 21st century, B1 and A1B. The value of the Chapter could be significantly enhanced, if the various central topics (emissions, radiative forcing, temperature, sea level - see Figure 10.1.1) could be discussed one at a time - as far as possible within the broad Chapter structure guidelines. [Govt. of Germany (Reviewer's comment ID #: 2011-132)]	change, although we recognize that separates it from related material in 10.6.
10-16	A	0:0		IMPORTANT PRESENTATION ISSUE: The general description throughout the chapter of the analysed SRES emissions scenarios is unsatisfactory given that the SRES scenarios are NON-MITIGATION scenarios, which is not clearly stated throughout the report. Thus, in every case where only the implications of non-mitigation scenarios are assessed, this has to be clearly stated for clarity. Some statements are clearly misleading, e.g. on page 15, line 48-49 "This is a subset of the SRES marker scenarios used in the TAR, and they represent a "low" (B1), "medium" (A1B), and "high" (A2) scenario." Clearly the laymen will read in to this that B1 is an optimistic A1B a medium and A2 a pessimistic emission scenario, but the main fact that all three scenarios belong to only a subset of possible emissions futures, namely the non-mitigation scenarios is not mentioned. Other examples, where the NON-MITIGATION nature of the SRES scenarios is inappropriately omitted: Page 9, line 22: "representative of low, medium and high emission trajectories", which should be amended with "UNDER THE ASSUMPTION OF NO FUTURE MITIGATION ACTION"; Page 13, lines 21-28: Under a subheading "for the 21st century" it is omitted that the SRES scenarios only cover the range of non-mitigation scenario futures, this has to be amended; Page 16, lines 6: "under plausible anthropogenic forcing scenarios" has to be clarified into "under plausible anthropogenic emission scenarios, if no emission mitigation is undertaken"; etc. etc.... See e.g. Page 45, lines 33-38, which captures the importance of why the distinction between non-mitigation and mitigation futures is important: ".... It clearly follows that the SRES scenarios cannot be regarded as capturing an agreed sense of the range of future options". [Govt. of Germany (Reviewer's comment ID #: 2011-133)]	ACCEPTED: added qualifying sentence in chapeau of section 10.2.
10-17	A	0:0		CONTENT: Provide a detailed clarification of why no mitigation scenarios have been considered in this draft of chapter 10 on projections. This shortcoming has to be rectified by the next and final draft of the chapter by explicitly including an analysis of the climate impacts of a representative set of state-of the-art mitigation scenario in the literature. Alternatively, provide a reference to the section of the AR-4 WG1 report, where the climatic implications of mitigation scenarios will be appropriately addressed. [Govt. of Germany (Reviewer's comment ID #: 2011-134)]	REJECTED: there are various scenarios which implicitly require mitigation (0 emission, stabilisation, etc). This is now mentioned in the introduction.
10-18	A	0:0		IMPORTANT: Whole Chapter: Please include results for the assessment of the physical	REJECTED: these scenarios are not yet

No.	Batch	Page:line From	To	Comment	Notes
				climate impacts of new mitigation scenarios submitted by WG 3 to WG1, as agreed on the WG1 meeting in Beijing in 2005 and documented in the WG1 TSU MEMO entitled "Transfer of new Emission Scenario Information from WG3 to WG1". Since new runs with AR-4 AOGCMs seem unfeasible by now, please include results on the physical implications of these new mitigation scenarios from EMICs and extrapolated from AOGCMs by the help of tuned simple climate models. Please provide clarification, why results for these mitigation scenarios have not been included in the SOD government review draft so far. The statement on page 65, line 37, that WG1 does not have the expertise to assess such scenarios seems confusing, as WG1 seems able to assess the physical implications of SRES emission scenarios. Just to clarify: WG1 does not have to and never did assess the economic, population, or technological characteristics of any emission scenarios, neither the SRES ones or others. It seems unacceptable, if WG1 should decide to selectively assess SRES non-mitigation scenarios, but refuse to assess mitigation scenarios on the grounds that WG1 does not have any (economic, technological?) expertise, which is not required anyway to assess the physical climate system implications of emission scenarios. [Govt. of Germany (Reviewer's comment ID #: 2011-135)]	assessed by experts to the extent that an illustrative set is available to WG1. This information must come from WG3.
10-19	A	0:0		Structure of the chapter: The chapter's readability would benefit from a restructuring to orient more systematically around each of the main subject areas rather than breaking subjects up. [William Hare (Reviewer's comment ID #: 99-35)]	Rejected—the authors feel the present structure of the chapter adequately addresses the subject matter
10-20	A	0:0		Mitigation scenarios: There are no mitigation scenarios eg Post SRES or others in this chapter yet they are in the literature and could be projected using the simple models applied in the chapter. This is a major deficit and needs to be rectified in the final draft to improve the policy relevance of this report. [William Hare (Reviewer's comment ID #: 99-36)]	REJECTED: First, there are various scenarios which implicitly require mitigation (0 emission, stabilisation, etc). This is now mentioned in the introduction. Second, these scenarios are not yet assessed by experts to the extent that an illustrative set is available to WG1. This information must come from WG3.
10-21	A	0:0		Correct characterisation of mitigation scenarios: In many places in the chapter the fact that analyses are based on non-mitigation emission scenarios is not made clear and in several cases this leads to possibly inaccurate messages to the reader. A distinction [William Hare (Reviewer's comment ID #: 99-37)]	Sentence in Chapeau to 10.2 inserted.
10-22	A	0:0		Emissions and forcing: care needs to be taken to keep the distinction between emission scenarios and forcing scenarios that result from the emissions scenarios. In several places	The reviewer does not specificy where changes should be made

No.	Batch	Page:line From	Page:line To	Comment	Notes
				the way the text is written could confuse readers. [William Hare (Reviewer's comment ID #: 99-38)]	
10-23	A	0:0		Sea level rise projections section: This section of the chapter does not review the full range of uncertainties and or projections for SLR. It lacks some important content (full range of scenarios), analysis of model vs observation discrepancy. The headline finding of the AR4 on sea level rise after reading this Chapter appears to be that SLR is less than found in the TAR for the same emission scenarios and that we are more certain of this than before and that, by implication the observed acceleration of SLR and the contributing terms will effectively stop soon and is thus not scaling in any way with temperature...are we really so certain of this? [William Hare (Reviewer's comment ID #: 99-39)]	Taken into account. We will give sea-level ranges for individual scenarios, but not a range across scenarios. Models and observations are compared in chapter 9 and this will be clarified. We will clarify how the 1993-2003 rate of SL rise is caused. We note that natural and internal variability may have contributed to its high rate. We will include an allowance for ice-sheet dynamical acceleration in the projections.
10-24	A	0:0		Structure of the chapter: The chapter's readability would benefit from a restructuring to orient more systematically around each of the main subject areas rather than breaking subjects up. [William Hare (Reviewer's comment ID #: 99-104)]	Rejected—the authors feel the present structure of the chapter adequately addresses the subject matter
10-25	A	0:0		pdfs of climate sensitivity are discussed in four different places: In Chapter 9 (Section 9.6), based on historical and paleoclimatic temperature data, and in Sections 10.5.2 (based on climate model simulations), 10.5.4.2 (based on climate model simulations), and 10.5.4.5 (based in part on historical temperature variations). I would seriously consider trying to consolidate these discussions (perhaps having just on section in Chapter 10 on climate sensitivity pdfs, and focusing exclusively on that). In any case, an overview of the various approaches (like what I have given here) discussed in the various chapters needs to be given somewhere, and there should be cross-referencing to Chapter 9 here in Chapter 10. [Danny Harvey (Reviewer's comment ID #: 101-75)]	Rejected. Chapter 9 discusses constraints from observations, chapter 10 from models and present day climatology. Overview is given in box 10.2 where cross. refs. are given to each section. Each section refers to box 10.2.
10-26	A	0:0		this chapter says a lot about model estimates but there is little connection made with observations. Do models reproduce the observed changes in heat content, sea level, precip (or salinity changes) etc, and how does this influence the likelyhood of the mean model results? The assessment part of the result need to be strenghtened. [Corinne Le Quere (Reviewer's comment ID #: 143-3)]	This is a chapter on projections. Chapters 8 and 9 address various aspects of the models' capa bilities of reproducing observations.
10-27	A	0:0		Extreme events are presented also as the mean model results. It needs to be clearly spelled out what the possible extreme events are even if they are highly unlikely. It is the role of this chapter to say if there is a possibility, even tiny, of major changes in the climate in the future.	The chapter presently assesses the published literature on possible changes in extremes.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Corinne Le Quere (Reviewer's comment ID #: 143-4)]	
10-28	A	0:0		I am going to just repeat my comments from chapter 9 here, but with the addition that for purposes of prediction for the 21st century, not only should the models and climate sensitivity be corrected for the uniformly positive albedo bias, but the scenerios should be expanded to include solar variation, since Solanki, et al (2004) predicted that there is only an 8% probability that the current unusually high levels of solar activity will be maintained for another 50 years. BEGIN REPEAT OF RELEVANT ch 9 comments. The Roesch (2006) paper (see references in Ch. 8) reported that all AR4 models had a positive albedo bias. I quote: "The mean annual surface albedo of the 15 AR4 models amounts to 0.140 with a standard deviation of 0.013. All AR4 models are slightly above the mean of PINKER (0.124) and ISCCP-FD (0.121)." This average albedo error in the models of 0.016 vs the PINKER data and 0.019 vs the ISCCP-FD, diminishes solar forcing on the order of 20 watts/m^2 or more in the plane orthogonal to the Sun. Since the models have demonstrated a good fit to the 20th century data, they must have compensating errors, most probably in their climate sensitivity to greenhouse gas forcing, since this varies so much between the models and is out of line with the climate sensitivity calculated from paleo-climate data. Solanki. et al, (2004) report that solar activity for the last 65 years has been at its highest level in 8000 years. Raimund Muscheler, et al (2005) confirm the current high level of solar activity but note that two other historical periods may be comparable. Separate climate commitment studies by Meehl, et al (2005) and Wigley, et al, (2005) report that temperature may take a century to equilibrate to new levels of forcing and over a thousand years for the climate to stablize. Proper relative attribution based on models obviously requires that their albedo errors first be corrected, and then climate commitment studies performed starting from the beginning of the time period attribution is being studied for. With the unusually high and constant level of solar activity over the latter half of the 20th century, a larger portion of the warming must be committed than has been previously reported in the studies referenced in the TAR and in this draft. The relatively constant level of solar activity over this period combined with the filtering of high frequency signals by ocean thermal inertia and systematic bias of the models, probably explains why the detection studies in this chapter did not detect a stronger solar signal. When these errors are corrected, I suspect the natural forcing (solar plus volcanic) will be a net contributer to the last 50 years of warming, although anthropogenic greenhouse gasses may still account for a majority of the warming. However, the decreased climate sensitivity to CO2 should moderate predicted future warming. S.K. Solanki, I.G. Usoskin, B. Kromer, M. Schussler, J. Beer (2004). "Unusual activity of the Sun during recent decades compared to the previous 11,000 years.". Nature 431: 1084-1087. DOI:10.1038/nature02995 Raimund Muscheler, Fortunat Joos, Simon A. Müller and Ian Snowball (2005). "Climate: How unusual is	This comment mainly has to do with attributing solar activity already observed and thus pertains to and is dealt with in Ch. 9. Model errors are addressed in Ch. 8. Future climate projections are reported as anomalies relative to a base period at the end of the 20th century. Thus, it is assumed that systematic errors are subtracted out in performing those differences, and we report climate changes, not absolute climate values. The future levels of solar activity are beyond the scope of this chapter.

No.	Batch	Page:line From	To	Comment	Notes
				today's solar activity?". Nature 436: E3-E4. DOI:10.1038/nature04045 S. K. Solanki, I. G. Usoskin, B. Kromer, M. Schüssler and J. Beer (2005). "Climate: How unusual is today's solar activity? (Reply)". Nature 436: E4-E5. DOI:10.1038/nature04046 Meehl G. A., et al. Sciencexpress, 10.1126/science.1106663 (2005). Wigley T. M. L., et al. Sciencexpress, 110.1126/science.1103934 (2005) [Martin Lewitt (Reviewer's comment ID #: 146-5)]	
10-29	A	0:0		The major problems with this chapter are the multi-century and multi-millennium projections. It is absurd. Most bring CO2 up to 4 X background, and then hold it constant from 2100 on. That's like assuming, from 1700-1800 that the number of horses increases at five times the rate that they did from 1670 to 1700, and then the number is held constant through 2000. What is the chance that this is the case? What is the chance that we will find something other than fossil fuels to power our society in, say, the next 1000 years? I'll bet pretty high, but I don't know what it will be. Nor does the IPCC. So these exercises are pointless, except as vehicles to scare people, which is not what IPCC should be about. IPCC should remove all of these from AR4—I guarantee that they will be roundly attacked and serve to undermine IPCC's credibility if they stay in the final copy. [Patrick Michaels (Reviewer's comment ID #: 176-22)]	The strategy and rationale for using various idealized forcings (e.g. 1% CO2 increase) and SRES scenarios are explained in the Introduction. Since no one knows exactly what the future may bring, the idea is to assess a variety of possible outcomes. WG1 does not assess scenarios—that is done in WG3.
10-30	A	0:0		Figures 10.7.1 through 10.7.7. Delete along with all text pertaining to multimillennial studies. As noted in my general comment on this chapter, this is absurd. [Patrick Michaels (Reviewer's comment ID #: 176-47)]	See response to comment 10-29.
10-31	A	0:0		A generally well written and interesting chapter [John Mitchell (Reviewer's comment ID #: 180-1)]	Thanks
10-32	A	0:0		The only general comment I have is there should be some assessment of scaling techniques which are used extensively in impact studies to increase the number of scenarios available. The key questions are the ability of simple models (SCMs, EMICs) to mimic GCMs, and to what extent other variables (eg precipitation, extremes) , transient simulations , and non heterogeneous forcings (eg aersols) can be interpolated using this approach. There is not a lot in the literature, but there is some information- there is a need for an assessement however short of what has been shown to be valid, and what is yet to be demonstrated. [John Mitchell (Reviewer's comment ID #: 180-2)]	Taken into account. We refer now to 11.2.1.1.3.
10-33	A	0:0		[Chapt. 10 comments in this file] My compliments to the authors on bringing together a relatively coherent picture from this huge literature. Besides some technical comments, I have two overall comments. One is that the precipitation section can be strengthened and better substantiated from the literature, and I have made some suggestions below. The other is that several figures, apparently from the chapter authors own analysis rather than	Accepted. Precipitation section modified as described for 10-507 (same reviewer).

No.	Batch	Page:line From	Page:line To	Comment	Notes
				the literature, have no statistical significance tests. I hope the authors will forgive me for bringing this up, but it doesn't actually take long to apply a basic test and mask the regions that do not pass at a high level of significance. This will probably make little difference in temperature, but is likely to modify the maps of the precipitation fields, although important features will pass. I heartily applaud the authors use of stippling to indicate regions where the multi-model mean change exceeds the multi-model standard deviation (although at this small size, it might be easier to read if the stippling were on the regions that the reader should ignore, rather than on those their eye should be drawn to, i.e., reverse the stippled/not stippled convention). [J. David Neelin (Reviewer's comment ID #: 187-23)]	Simple consistency tests are applied and are described better now. The stippling convention has been modified in several figures.
10-34	A	0:0		Chapter 10 in my view in many places oscillates between (a) providing an assessment of possible future climate changes based on the full scientific literature (which I am convinced is its role), and (b) providing a review of a certain group of climate model scenario simulations. This is not a big problem in areas where climate models do well and provide the best available source of information on future climate change. But this is a big problem in other areas where climate models do not capture the full knowledge that we have. Most of the authors of this chapter are modellers, so perhaps this is why models play such a disproportionate role in the assessment of the future here. This problem is clear e.g. for sea level rise, where the executive summary states that SLR from 2000 to 2020 is projected as 34 +- 25 mm. I think this is simply what the models say, but it is not what a full assessment of all facts including the observations would say. Or does IPCC really project with high confidence (I assume that the +- 25mm are to be seen as 90% confidence limits?) that sea level rise is going to slow down in the next 15 years? Chapter 5 reports the observed rate from 1993-2005 as 3.2 mm/year - if this trend simply remains constant, it would give 64 mm from 2000 to 2020, outside the confidence range of the above projection presented in the executive summary. Even the trend 1961-2003 is larger than the projection. I am prepared to offer a $1,000 bet that SLR from 2000 to 2020 will be above 40 mm, at equal odds. If the authors really believe their best guess of 34 mm, then they must eagerly accept this bet and expect to make easy money. I am looking forward to your positive responses - I am serious! (If you are not willing to put your money to your words, then you don't really believe them, in which case they should be changed in the report.) Another area where this problem is apparent is the assessment of future changes in MOC. I have commented on this issue for the previous draft with no apparent effect, but as I think it is important I will try again to convince you of some changes. Far too much emphasis is put on models and their results are reported uncritically, without mentioning important caveats. It is not mentioned that we presently simply do not understand what	MOC part noted. We are surprised by this comment since the author was presnet at a roundtable cross-chapter meeting in Christchurch wherein we discussed and agreed upon the use of very unlikely (10% probability) for a collapse this century. We did not feel we could assess the likelihood of a collapse beyond 2100 and now note that we cannot exclude the possibility of a collapse. Sea level partly accepted. Models and observations are compared in chapter 9 and this will be clarified. We will clarify how the 1993-2003 rate of SL rise is caused. We note that natural and internal variability may have contributed to its high rate.

No.	Batch	Page:line From	To	Comment	Notes
				determines the stability of the MOC; and that models give very widely differing results on this and we do not know the reasons for that. Also, most models discussed do not include meltwater input from Greenland, which potentially could become quite large (as a rule of thumb, melting the ice sheet in 1,000 years corresponds to an average influx of 0.1 Sv). So important processes are missing, which is a caveat that should be mentioned wherever these model results are mentioned, otherwise one misleads the audience. A proper assessment in my view should include: (a) A discussion of the uncertainty we currently have in the shutdown threshold (how much freshwater is needed for that - see e.g. the Rahmstorf et al. (GRL 2005) model intercomparison). Information from chapter 6 on amounts that caused paleoclimatic shut-downs could also be referred to here. (b) A discussion of possible current and future freshwater sources and their uncertainty. This should include estimates of the currently observed freshwater input; e.g., from melting sea ice 0.014 Sv (Lindsay and Zhang 2005), from Greenland 0.007 Sv (Rignot and Karangaratnam 2006), and from Eurasian rivers 0.005 Sv (Peterson et al. 2002) - even without precipitation over the oceans and Canadian river runoff, this is together about a quarter of what is widely considered a rough critical magnitude (0.1 Sv). It should be discussed how those input terms might evolve in future with increased warming. (c) A discussion of ongoing observed changes (with due reference to chapter 5), i.e. salinity changes (Curry and Mauritzen 2005) and possible circulation changes (Bryden et al. 2005) and what they mean for the future. (d) A more comprehensive discussion of the possible or likely impacts (currently it focuses on a non-impact, i.e. stating that it cannot cause an ice age, which is fair enough but certainly not all we can and should say about impacts). The chapter so far takes the easy road, simply reporting model results rather than attempting a proper risk assessment. It does not convey to the non-specialist reader that we have much less confidence in model results with regards to MOC stability than for most other aspects of climate. No climate model resolves the crucial oceanic convection processes, or the overflows, and several groups have struggled to even get a stable MOC in their model for present climate and have deliberately introduced measures to that effect - models with a too stable MOC pass the test for present climate, models with a too unstable one don't and are modified. I strongly believe that therefore a proper assessment of the risk of future MOC changes cannot primarily rely on reporting what SRES scenarios in models have found; it has to include the full range of information, including observations and paleoclimate. As it stands, the chapter is quite selective in the evidence it discusses and overconfident in its statements. The executive summary statement that "It is very unlikely that the MOC will undergo a major abrupt transition in the 21st Century" implies a greater confidence	

No.	Batch	Page:line From	Page:line To	Comment	Notes
				and much smaller uncertainty than we actually have, and it is at odds with at least three different expert elicitations that I have seen on this subject. Based on what many experts have said, the above statement is certainly not consensus amongst most experts, as a large number of experts explicitly assign probabilities greater than 10% to such a transition within this century. I think if the statement is changed from "very unlikely" to "unlikely" it will more accurately reflect what a very large fraction of experts would support as a consensus statement. My impression in direct discussion has been that some authors feel to be on a "moral high ground" in down-playing the risk of sea level rise, ocean circulation changes, and other aspects of climate change. This seems a peculiar concept, perhaps an over- reaction to the allegations of "alarmism" by climate skeptics? Obviously there is no merit in either down-playing or exaggerating a risk; our task and the only professional approach is to come to a sober, unbiased assessment of all the available evidence. [Stefan Rahmstorf (Reviewer's comment ID #: 206-2)]	
10-35	A	0:0		There remains some inconsistency about probability intervals. The one thing that must be avoided is the phrase "confidence interval", which appears on p43 line 43, p45 line 21, p46 line 32, p72 lines 36-7. This has a precise technical meaning and is quite wrong in this context. Ch 10 uses "5-95% uncertainty range" (p43 line 37), "90% probability interval" (p45 line 18), "5-95% range" (p49, lines 29, 34; p52, line 17; p73, line 35). I would standardise on "90% CI". This should be introduced on p43 line 37 as "... the resulting 5-95% probability interval (referred to as the 90% credible interval, or 90% CI) for equilibrium climate sensitivity ..." [Jonathan Rougier (Reviewer's comment ID #: 221-1)]	Partly accepted. 'Confidence interval' removed. 5-95% uncertainty is commonly used in the underlying literature.
10-36	A	0:0		One general caveat that must be mentioned is that models fail to simulate the strong warming of the Arctic ocean 55 million years ago, in response to radiative forcing by greenhouse gases. See Moran et al., 2006, Nature. The models underestimate observed proxy temperature estimates by 15 C! Clearly there is some missing physics in the models. Possibly this could be clouds, that keep the polar night from cooling off. [Jeffrey Severinghaus (Reviewer's comment ID #: 232-6)]	Rejected. This topic falls to the paleoclimate chapter and is out of the realm of future climate projectoins.
10-37	A	0:0		An overall comment about Chapter 10 is that it reads as if it were a summary of model results, rather than a projection of the best scientific evidence available. It should be a reflection of the best of our knowledge at the present time, observational or modeling. [Jeffrey Severinghaus (Reviewer's comment ID #: 232-7)]	Ch. 10 does not assess observations. The model projections reflect the range of possible futures relevant to the scenarios which are assessed in Wg3.
10-38	A	0:0		The ordering of material in this Chapter is strange. We go from "Projected Changes in the Physical Climate System" in section 10.3 to "Quantifying Climate Change Projections" in section 10.5, only to jump back to "Sea-level Change" in section 10.6. To make matters	We appreciate the reviewer's comments regarding re-structuring the entire chapter at this late stage, however after

No.	Batch	Page:line From	Page:line To	Comment	Notes
				worse, there is overlap between 10.3.3 (Changes in Ocean/Ice and High Latitude Climate, which includes a short ice-sheet subsection) and sections 10.6 (more on ice sheets). At the very least section 10.6 should be moved to be after 10.3. But as much of what is in 10.6 can be regarded as part of the "Physical Climate System", I think it would be better if 10.3 and 10.6 were merged into one section. [Adrian Simmons (Reviewer's comment ID #: 242-136)]	two levels of review, the authors feel the present structure is adequate to present the assessment of projected future climate change. No organization of the material is ideal. We chose this one because sea level is a subject which may readers expect to find treated in a section of its own. We are aware of the relation to 10.3 and have attempted to be consistent.
10-39	A	0:0		Basing projections on a single emissions scenario may unduly narrow the range. Past efforts generally tried to ensure that each time different factors were considered to develop alternative temperature estimates (e.g. emissions, carbon cycle models), a corresponding estimate of sea level rise was also reported. [Govt. of United States of America (Reviewer's comment ID #: 2023-670)]	Taken into account. Unlike in the TAR, chapter 10 is not producing a combined scenario range for temperature or SL, but we will produce SL ranges for the marker scenarios.
10-40	A	0:0		There are serious cross-chapter differences between 10 and 4 regarding the melting (4) or accumulation (10) of Antarctic ice with regard to sea level rise. The disconnect is serious, weakens the model projections here and should be resolved with careful language (realizing that it is a bit too late to resolve the model differences.) Please reconcile the projected negative contribution to sea level rise from Antarctica, with apparent current positive contribution discussed in Chapter 4. Are there two schools of thought that each need to be reflected in both chapters? [Govt. of United States of America (Reviewer's comment ID #: 2023-655)]	Taken into account. We do not believe there are serious inconsistencies in fact between the chapters. The major difficulty is whether to make an allowance for dynamical acceleration of ice sheets in 21st century projections. We will do this.
10-41	A	0:0		Please provide results for the low and high emissions scenarios. Justification: IPCC usually tries to characterize the range of uncertainty, but basing projections on a single emissions scenario may unduly narrow the range. Past efforts generally tried to ensure that each time different factors were considered to develop alternative temperature estimates (e.g. emissions, carbon cycle models), a corresponding estimate of sea level rise was also reported. [Govt. of United States of America (Reviewer's comment ID #: 2023-656)]	Taken into account. Unlike in the TAR, chapter 10 is not producing a combined scenario range for temperature or SL, but we will produce SL ranges for the marker scenarios.
10-42	A	2:1		Section Executive summary: At the beginning there needs to be a clear statement of the different concepts in relation to commitment used in tis chapter and in describing its findings. This need to describe what is entailed in each definition [William Hare (Reviewer's comment ID #: 99-40)]	Rejected. We refer to the much more detailed definitions involving climate change commitment in section 10.7 and choose not to repeat those definitions here.
10-43	A	3:0		Executive Summary: Should some references be made to the TAR summary in this section? i.e. what has and has not changed since the TAR and what are assements of new	In the first order draft the executive summary was arranged in the way

No.	Batch	Page:line From	Page:line To	Comment	Notes
				phenomena since the TAR. [Matthew Collins (Reviewer's comment ID #: 44-22)]	suggested by this reviewer. However, several reviewers objected to that structure and insisted that the executive summary be organized relative to topics, which is the way it is now presented. We choose not to return to the previous version. Though both methods have merit, the important thing is that the executive summary collects and presents the main conclusions, which we feel it does now in its present form.
10-44	A	3:0		It is important do include findings on ocean acidification eg something like "By the end of the century is it likely that the ocean pH will become another 0.3–0.4 units lower than at present with consequential problem for marine calcifying organisms and with conditions detrimental to high-latitude ecosystems within decades, not centuries as suggested previously" [William Hare (Reviewer's comment ID #: 99-42)]	ACCEPTED: ES para added under "Carbon Cycle"
10-45	A	3:0		I am surprised to read that the chapter 10 is only based on model projections. I was under the impression that it should provide an assessment based on simulations as well as observations and the dynamical understanding of past and future climate dynamics. Not all of this is captured in even "state-of-the-art" model simulations. [Anders Levermann (Reviewer's comment ID #: 145-2)]	Rejected. Model evaluation is covered in Ch. 8, and simulations of 20th century climate relative to observations are assessed in Ch. 9. Ch. 10 present an assessment of future climtae projections.
10-46	A	3:1		The executive summary lacks a specification of emission scenarios and a conclusion on ocean acidification. [European Commission (Reviewer's comment ID #: 2008-29)]	ACCEPTED: reworded wrt scenarios, added a para re acidification
10-47	A	3:1		Executive Summary. CONTENT: This Executive Summary draft lacks a brief paragraph at the beginning in regard to which emission scenarios are analyzed, and their characteristics. A chapter on "Future Projections" needs to clearly state at the outset, WHICH possible futures are analyzed. Furthermore, there is the need for an explanation of why the emission scenario space has been restricted to NON-MITIGATION scenarios only, if this shortcoming of the current draft is not rectified. [Govt. of Germany (Reviewer's comment ID #: 2011-136)]	ACCEPTED: sentence on emissions added in chapeau to ES.
10-48	A	3:1		Executive Summary. CONTENT: This Executive Summary draft lacks the results on Ocean Acidification. Needs to be included. [Govt. of Germany (Reviewer's comment ID #: 2011-137)]	ACCEPTED: ES para added under "Carbon Cycle"

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-49	A	3:1		Executive Summary: CONTENT: A definition needs to be given of what is understood under "commitment" AND the fact that sustained positive emissions were needed to follow a "constant composition / current radiative forcing" warming commitment scenario. Some sentences in the current draft are clearly misleading, e.g. page 3, line 20, "… arises from warming we are ALREADY committed to". Or does WG1 make a judgment about what are the economically feasible emission reduction rates? The laymen gets the impression that the analyzed commitment warming merely arises due to historic emissions, which is clearly wrong, as sustained emissions at between ~20-70% seem necessary to keep radiative forcing stabilized at its current level, depending on the lifetime of the specific gas (cp. Question 10.3. Figure 1) [Govt. of Germany (Reviewer's comment ID #: 2011-138)]	We refer the reader to the full definition of commitment in section 10.7. We have added clarification that that commitment discussed here is under the condition of stabilized concentrations (which implies further emissions).
10-50	A	3:1		Executive summary. a bullet on projected ocean acidification needs to be included [Corinne Le Quere (Reviewer's comment ID #: 143-5)]	ACCEPTED: ES para added under "Carbon Cycle"
10-51	A	3:3		not "hierachy of models" but "spectrum of models". Should be changed throughout the chapter [Anders Levermann (Reviewer's comment ID #: 145-1)]	Rejected. The term "hierarchy of models" reflects standard usage in the assessed literature.
10-52	A	3:5		Add "non mitigation" to scenarios here. [William Hare (Reviewer's comment ID #: 99-41)]	Accepted.
10-53	A	3:9	3:10	It's misleading to state that "probabilistic estimates replace expert judgment" - these kinds of probabilistic estimates are a form of expert judgment, not any objective measurement of probabilitiy. [Paul Baer (Reviewer's comment ID #: 10-3)]	Accepted. Substitute "replace" with "complement"
10-54	A	3:9	3:9	"probabilistic estimates" may be better expresses as "estimates of the probability of change" to reflect the uncertainty and dabate around different methods used to construct probabilities. [Matthew Collins (Reviewer's comment ID #: 44-23)]	Accepted.
10-55	A	3:10	3:10	Add at end "It should be pointed out that since no model has been properly validated and shown to be capable of successful prediction, all these results should be regarded as speculative" [VINCENT GRAY (Reviewer's comment ID #: 88-1477)]	Rejected. Model evaluation and comparison to observations is performed in Chapters 8 and 9.
10-56	A	3:11	3:11	There needs to be a statement that all defintive statements are definitive about the model results NOT what will actually occur. This is mostly clear, but in some sections there are statements that will be quoted as implying definitive predictions. This should be avoided at all costs.In situations where the models are known to have serious defeciences or uncertainties, the summary statements need to note that. This is most obviously the case for sea level rise, ENSO variability and MOC changes – there are actually good reasons	The reviewer does not supply specific instances related to this comment. However, we have attempted to clarify that the projections are from models, and are not predictions.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				not to trust the models. [Gavin Schmidt (Reviewer's comment ID #: 227-13)]	
10-57	A	3:12	3:46	Please correct the differences in the upper assessment of future temperature increase (3.012 (?!) °C in line 25 vs. 4.0°C in line 40).Please use only one reference period (1980-99 in line 16) vs. 1980-2000 in line 40). [Govt. of Germany (Reviewer's comment ID #: 2011-59)]	These numbers have been corrected.
10-58	A	3:12	3:46	We are told here that by the end of the century we can expect a warming up to 3.012 °C (line 25) and later we are told it is up to 4.0 °C (line 40). I don't understand the difference in the numbers, and the difference in apparent accuracy (significant digits given). Neither do I understand why in one case the reference period is 1980-1999 (line 16), and in the other 1980-2000 (line 40). [Stefan Rahmstorf (Reviewer's comment ID #: 206-3)]	These numbers have been corrected.
10-59	A	3:13	3:13	Here "temperature" refers to "global-mean temperature" I think. [Matthew Collins (Reviewer's comment ID #: 44-24)]	Accepted
10-60	A	3:13	3:13	Add "surface air" before "temperature continuing". Also add "(SAT)" after temperature and use the abbreviation. [Ronald J Stouffer (Reviewer's comment ID #: 258-96)]	Accepted.
10-61	A	3:13	3:46	I was very confused by the two paragraphs starting at line 13 and at line 38. For example, in the earlier paragraph the largest late-century warming (presumably for A2) is given as 3.012 deg C (why 4 sig figs?), whereas in the later paragraph apparently the same quantity is given as 4.0 deg C. I expect there is a reason for this but it is confusing for this reader (and I expect for others). The paragraphs need to be made consistent (or combined), or bthe difference needs to be spelt out. [Richard Wood (Reviewer's comment ID #: 294-16)]	These numbers have been corrected.
10-62	A	3:13	:53	Are the ranges for projections based on the 1.5-4.5oC range, or the range of models used for the assessment? Or is this really a 95% uncertainty range? It may be safer to clarify this here, and caution the unaccounted for uncertainties (eg climate sensitivity outside range, etc) also here, even if details may be found in the chapter. Also, are the end-of the century values from models with or without interactive carbon cycle? For example,l like the sea level section further down, since it gives an idea where the additional uncertainties are in that case. [gabi hegerl (Reviewer's comment ID #: 103-18)]	Accepted. We now clarify "all models assessed here". Carbon cycle feedbacks are included to a certain extent in the SRES scenarios, and that description is in the SRES document. What we report here are results from those scenarios, and the contents of the scenarios and their assessment is outside the area of expertise of WG1.
10-63	A	3:14	3:14	".....proportional to the associated radiative forcing" - improves clarity. "Forcing" is not defined in Glossary [Govt. of Australia (Reviewer's comment ID #: 2001-373)]	Accepted.
10-64	A	3:16	3:17	Please make clear that these values refer to the multi-model mean warming.	Accepted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Govt. of Finland (Reviewer's comment ID #: 2009-118)]	
10-65	A	3:16	3:16	Add "globally averaged SAT" before "range of only". [Ronald J Stouffer (Reviewer's comment ID #: 258-97)]	Accepted.
10-66	A	3:19	3:19	"Chapter.3" should be "Chapter 3". [Chiu-Ying LAM (Reviewer's comment ID #: 139-10)]	Accepted.
10-67	A	3:19	3:19	future variations in natural forcings could expand this range somewhat - too vague - Add sentence or 2 on the impact of a very large volcanic eruption on these projections. [Ronald J Stouffer (Reviewer's comment ID #: 258-98)]	Accepted
10-68	A	3:21	3:24	It is noted that the "quarter " relates only to the upper value of 1.73°C. It is proposed to clarify this by e.g. by the including the wording: in the case of the upper range of emissions. [Govt. of Austria (Reviewer's comment ID #: 2002-50)]	Rejected. Actually the "only about a quarter" (note the qualifer "about") refers to roughly the middle of the range.
10-69	A	3:21	3:24	Define "mid-century" and "late century". [Govt. of Finland (Reviewer's comment ID #: 2009-119)]	Accepted.
10-70	A	3:21	3:26	The use of "committed" here is a ambiguous and needs to be qualified "committed if present forcing levels were to be maintained" otherwise therei is potentially misleading impression created here. [William Hare (Reviewer's comment ID #: 99-43)]	Accepted.
10-71	A	3:22	3:27	Too many significant figures (3.012C) are used over two or three sentences. Thought should be given to presenting these figures more clearly. [Govt. of Australia (Reviewer's comment ID #: 2001-374)]	This number has been corrected.
10-72	A	3:22	3:22	Add "globally averaged" before "warming with a range". [Ronald J Stouffer (Reviewer's comment ID #: 258-99)]	Accepted.
10-73	A	3:23	3:23	"a quarter" - more like "a third"? [Chris Jones (Reviewer's comment ID #: 120-16)]	It's closer to a quarter for the mid range.
10-74	A	3:24	3:26	It is noted that similar information is provided in lines 38 to 46 on the same page. In order to avoid confusion it is proposed to refer to the para below and not provide figures twice on the same page. The sentence might read as follows: By late century, their are clear consequences depending on which scenario is followed (see figures below). the wording: in the case of the upper range of emissions. [Govt. of Austria (Reviewer's comment ID #: 2002-51)]	This paragraph has been re-written.
10-75	A	3:24	3:25	consequences - a loaded word? I would delete - there are clear consequences depending on which scenario is followed. Repleace with - the scenario followed matters. [Ronald J Stouffer (Reviewer's comment ID #: 258-100)]	Accepted
10-76	A	3:25	3:25	"with a range of 1.35°C from 1.77°C to 3.012°C" is inconsistent. 3.012 should be 3.12. [Govt. of China (Reviewer's comment ID #: 2006-68)]	These numbers have been updated.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-77	A	3:25	3:25	3.012 degrees? Too much precision quoted! Or should it be 3.12? (even then is 2 decimal places justified?) [Chris Jones (Reviewer's comment ID #: 120-17)]	These numbers have been updated.
10-78	A	3:25	3:25	I am surprised that we know the limit of commitment to 3 significant figures (3.012C) when we do not know the current surface temperature to nearly that accuracy. Suggest that only 2 significant figures be shown. [Haroon Kheshgi (Reviewer's comment ID #: 125-38)]	These numbers have been updated.
10-79	A	3:25	3:25	Four significant figures? 3.012 C? I suggest rounding off to tenth's of a degree in all cases. (though this maybe a typo p17, l19 has 3.12 C). Still, I don't think that hundredths of a degree at at all relevent. [Gavin Schmidt (Reviewer's comment ID #: 227-12)]	These numbers have been updated.
10-80	A	3:25	3:25	Add "globally averaged SAT" before "range of". [Ronald J Stouffer (Reviewer's comment ID #: 258-101)]	Accepted.
10-81	A	3:25		3 digits after the dots seems a lot for a climate projection! (3.012 to 3.01) [gabi hegerl (Reviewer's comment ID #: 103-15)]	These numbers have been updated.
10-82	A	3:25		3.012 C: Too many digits. [Anders Levermann (Reviewer's comment ID #: 145-3)]	These numbers have been updated.
10-83	A	3:27	3:27	The paragraph above gives very precise ranges based on analysis of the multi-model archive which one cannot argue with. To say then that these ranges are corroborated or confirmed by a hierarchy of models and probabilistic predictions suggests that precisely the same ranges are found in those studies. Perhaps "consistent with means and ranges found" would be better than "corroborated". [Matthew Collins (Reviewer's comment ID #: 44-25)]	Accepted.
10-84	A	3:27	3:27	Add at end "It should be pointed out that the scenarios are speculative, and that some of them, notably, A@ and A1F1 are highly improbable" [VINCENT GRAY (Reviewer's comment ID #: 88-1478)]	Rejected. WG1 does not assess scenarios, and as presented by SRES, none are more probable than others.
10-85	A	3:29	3:30	"twice " applies to "overland", not to "high northern latitude". Present phrasing is ambiguous. To be replaced by "Geographical patterns of projected warming show greatest temperature increases over land (roughly twice the global average temperature increase) and even more at high northern latitudes, with less warming over the southern..." [Govt. of France (Reviewer's comment ID #: 2010-68)]	Accepted
10-86	A	3:32	3:32	"Chapter 2" should be "Chapter 3" [Govt. of Finland (Reviewer's comment ID #: 2009-120)]	Accepted.
10-87	A	3:32		"Chapter 2" should be "Chapter 3" [Adrian Simmons (Reviewer's comment ID #: 242-137)]	Accepted.
10-88	A	3:33	3:33	Change "in the stratosphere" to "throughout the stratosphere". The wording could mean	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				only the tropical stratosphere. [Ronald J Stouffer (Reviewer's comment ID #: 258-102)]	
10-89	A	3:33		The wording "becomes established" could be read as implying that cooling in the stratosphere has not happened yet. These statements need to be phrased carefully to avoid inconsistency with the observational chapters. [Martin Manning (Reviewer's comment ID #: 155-69)]	Accepted
10-90	A	3:34	3:36	first seen - Relative to present day? [Ronald J Stouffer (Reviewer's comment ID #: 258-103)]	Accepted.
10-91	A	3:36	3:36	The last part of this sentence, stating that the warming of the oceans is "most evident at high latitudes where vertical mixing is greatest" is incorrect. The Levitus et al (2005) analysis shows that most of the heat uptake is between 10N and 45N and near 40S, with very little in high latitudes (see their fig. 2). The phrase quoted above should be deleted. [Peter Stone (Reviewer's comment ID #: 257-7)]	Accepted.
10-92	A	3:38	3:41	The bais for chosing to report mean +/- 20% should be explained. It is an unusual way of presenting results with no obvious explanation for the choice. [Lenny Bernstein (Reviewer's comment ID #: 20-68)]	Accepted. Explanation is given.
10-93	A	3:38	3:46	This first mention of the SRES scenarios in Chapter 10 should be accompanied by a simple graphic showing the assumed emissions for the sample scenarios over their entire range of application from the assumed start time (year 2000?) to their assumed end time (year 2300 or 2400?), and in a separate graph, the modelled GG concentrations for the same 300 or 400 year time period). I believe that this simple graphic would go far toward explaining much of the conclusions that follow in the chapter. For example, the Chapter 10 text, as well as the SPM, make the point that regardless of scenario chosen, the modelled mean climate temperatures are very similar for all scenarios until the year 2040 or 2050. The text explains this being due, in large part, to the "previous commitment" to warming from 20th century cumulative emissions (see my comment #4 above). But it also due quite simply to the fact that cumulative emissions in the first 40 or 50 years of the 21st century vary by rather small amounts between the various SRES scenarios -- they all start in the first year at the same emission level -- AND there is a clearly observed time lag (as described in other chapters) between changes in greenhouse gas emissions and concentrations (due in part to long atmospheric lifetimes) and observed climate responses. However, neither the Chapter 10 text or the SPM and Technical Summary make clear -- as I believe they should -- that ALL scenarios will produce GG concentrations and climate changes as observed in the highest emission scenario in 2070 and 2100 (e.g, mean temperature increases of 3 degrees C), but with just a time lag of 20 to 50 years in reaching those milestones. It is only for those climate changes and impacts that are associated with GG concentrations greater than 550 PPM will there be a clear distinction	PARTLY ACCEPTED: Fig. 10.1.1 will be supplemented with illustrative time series. Scenario-independent statements will be made wrt TCR.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				between the effects of the 3 scenarios. [Chuck Hakkarinen (Reviewer's comment ID #: 96-3)]	
10-94	A	3:38	3:41	What is the reason for reporting mean +/- 20%? It tell the reader nothing about uncertainty, and appears to be an arbitrary selection. [Jeff Kueter (Reviewer's comment ID #: 137-62)]	Accepted. Explanation is given.
10-95	A	3:38	3:46	why is the range here up to 4 whereas it is up to 3.012 on line 25? the two paragraphs need clarification [Corinne Le Quere (Reviewer's comment ID #: 143-6)]	This paragraph has been re-written.
10-96	A	3:38	:46	These results would be much more easily comprehensible if they were put into a small text table. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-31)]	This paragraph has been re-written.
10-97	A	3:38	:44	The use of ranges and the definition of what uncertainty means should be clearly stated and used consistently throughout the chapter. The two examples here are confusing. How you chose to calculate the range (+-20%) is not relevant here, but what is important is what likelihood that range covers (either 2/3, +-1 sigma, +- 2sigma). Please be explicit, you can define what ranges in temperature mean once and use it throughout. [Govt. of United States of America (Reviewer's comment ID #: 2023-657)]	This paragraph has been re-written.
10-98	A	3:39	3:39	the numbers quoted here don't seem to match those quoted above (line 25) [Chris Jones (Reviewer's comment ID #: 120-18)]	This paragraph has been re-written.
10-99	A	3:39	3:39	Add "global mean SAT" before "warming". [Ronald J Stouffer (Reviewer's comment ID #: 258-104)]	This paragraph has been re-written.
10-100	A	3:41	3:41	A definition of 'harmonized' forcing is necessary. [Govt. of Australia (Reviewer's comment ID #: 2001-375)]	This paragraph has been re-written.
10-101	A	3:41	3:43	these lines should be carefully written. What is an "harmonized" forcing ? The modification in the upper end is due to include the carbon cycle feedback ? Or what uncertainties ? What is "the simple model" ? Is it one result or does it come from a consensus ? [Pascale DELECLUSE (Reviewer's comment ID #: 58-58)]	This paragraph has been re-written.
10-102	A	3:41	3:42	What is harmonized forcing? Does this mean that uncertainty in going from concentrations to forcing is not included in the reported uncertainty of projections? If so, then these uncertainty ranges should be reported as partial estimates of uncertainty. [Haroon Kheshgi (Reviewer's comment ID #: 125-39)]	This paragraph has been re-written.
10-103	A	3:41	3:42	I think the term "harmonized forcing" needs to be explained for the benefit of the average reader of the Exec Summary. [Martin Manning (Reviewer's comment ID #: 155-70)]	This paragraph has been re-written.
10-104	A	3:43	3:44	It is surprising how narrow the range of uncertainty is for any given scenario. I had	This paragraph has been re-written.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				difficulty tracing the logic for assigning a probability to such a range. Assigning a probability is a significant change from the TAR, and its basis should be discussed fully. In addition, literature that uses similar simple models to arrive at different ranges should be assessed. I note that if a similar methodology is used as in the TAR, then the reported range does not include, for example, uncertainty contributed from natural variability nor from estimating radiative forcing from concentrations, and thus would be a partial estimate of uncertainty. Reading the description on estimating probabilities on page 10-48, I do not see how a probability can be justified. Suggest that the probability be removed, and that the range be stated as a model range. [Haroon Kheshgi (Reviewer's comment ID #: 125-40)]	
10-105	A	3:43	3:43	Move "in particular" to the end of the sentence. [Ronald J Stouffer (Reviewer's comment ID #: 258-105)]	Accepted.
10-106	A	3:44	3:44	Uncertainty range should be given as +/- two standard deviations (95% range) following conventional scientific practice. [Lenny Bernstein (Reviewer's comment ID #: 20-69)]	These ranges have been revised.
10-107	A	3:44	3:44	All uncertainty ranges should be +/- two standard deviations, following conventional scientific practice. [Jeff Kueter (Reviewer's comment ID #: 137-63)]	These ranges have been revised.
10-108	A	3:45	3:46	I am not sure why scenario A1FI is reported separately from the others? Suggest that this be listed with the previous 3. And why are not all 6 illustrative scenarios reported? [Haroon Kheshgi (Reviewer's comment ID #: 125-41)]	These ranges have been revised
10-109	A	3:46	3:46	5,8 °, carbon cycle feedback included or not ? [Govt. of France (Reviewer's comment ID #: 2010-69)]	These ranges have been revised
10-110	A	3:46	3:46	Add at end "but this particular scenario is highly unlikely" [VINCENT GRAY (Reviewer's comment ID #: 88-1479)]	Rejected. WG1 does not assess scenarios, and as presented by SRES, none are more probable than others.
10-111	A	3:48	3:53	Delete this paragraph. It is redundant with the findings presented in Chapter 9. [Lenny Bernstein (Reviewer's comment ID #: 20-70)]	Rejected. Ch. 9 has passed forward observational results synthesized here with model results.
10-112	A	3:48	3:48	Replace "assessment" by "guessing exercise" [VINCENT GRAY (Reviewer's comment ID #: 88-1480)]	Rejected. Ch. 9 has passed forward observational results synthesized here with model results.
10-113	A	3:48	3:53	This material has already been presented in Chapter 9. It is redundant. [Jeff Kueter (Reviewer's comment ID #: 137-64)]	Rejected. Ch. 9 has passed forward observational results synthesized here with model results.
10-114	A	3:48	3:53	Chapter 9 (page 4, lines 23 – 28) makes essentially the same statement as this but gives a	Accepted. Ch. 9 has passed forward

No.	Batch	Page:line From	Page:line To	Comment	Notes
				slightly different description of the provenance of the result. The issue here is not repetition, but whether or not the two chapters are using independent evidence that just happens to produce exactly the same values of the climate sensitivity range, or using a common pool of evidence. Because the latter is intended I think some cross-chapter referencing is required. [Martin Manning (Reviewer's comment ID #: 155-71)]	observational results synthesized here with model results.
10-115	A	3:48		The paragraph on climate sensitivity starts with "an expert assessment" which is correct but puts too much emphasis on that part for my view. The most outstanding new development are available constraints from observed changes and estimates of climate model uncertainty, the experts then just decided for a way to integrate the information coming out of different lines. Could this be rephrased to change the emphasis? [gabi hegerl (Reviewer's comment ID #: 103-16)]	Accepted. Summary statement says 'based on available constraints from obs. and strengths of know feedbacks in GCMs', which makes exactly that point. Statement is identical to box 10.2, suggestions for improvement will be discussed when revising box 10.2.
10-116	A	3:48		I think it would be helpful for the reader to relate the paragraph on climate sensitivity to the previous paragraph of the models projection of the warming in 2100. That should be done in two ways: 1) What do we know about the climate sensitivities of the models that provide the projections and 2) What does a climate sensitivity of 3 degrees mean in terms of 2100 warming for the different scenarios. [Anders Levermann (Reviewer's comment ID #: 145-4)]	Accepted. We now related this paragraph to the previous one.
10-117	A	3:48	:53	Delete this paragraph. It is redundant with the findings presented in Chapter 9 and belongs in Chapter 9 alone. [Govt. of United States of America (Reviewer's comment ID #: 2023-658)]	Rejected. Ch. 9 has passed forward observational results synthesized here with model results.
10-118	A	3:49	3:49	"Equilibrium warming.......degree centigrade" relative to what time point? Note preceding paras use present levels (1980- 2000/ 1980 to 1999 - why are they different?) [Govt. of Australia (Reviewer's comment ID #: 2001-376)]	Accepted. We now note this is a doubling of CO2 from present day values.
10-119	A	3:50	3:50	Replace "is likely to " by "may" [VINCENT GRAY (Reviewer's comment ID #: 88-1481)]	Rejected. The use of "likely" is intentional to portray level of confidence in this result.
10-120	A	3:50	3:50	Replace "most likely" with "best guess" [VINCENT GRAY (Reviewer's comment ID #: 88-1482)]	Rejected. The use of "likely" is intentional to portray level of confidence in this result.
10-121	A	3:51	3:53	Please clarify "substantially higher than 4.5C" and "cannot be excluded", the latter with reference to the standard terms for the probability of assessed outcomes defined for this purpose. Eg, do you judge it to be exceptionally unlikely that S>6C, or very unlikely, or merely unlkely? I don't think you can reasonably duck this decision. "For fundamental physical reasons, as well as data limitations". I think I know what you	We do not assign a liklihood value for high values for reasons given in the body of the text. Therefore, we assess and assign liklihood values to what we can at this time, which is the likely

No.	Batch	Page:line From	Page:line To	Comment	Notes
				are trying to say, but this camel of a statement is horribly misleading. In fact it could only be true if S really was substantially higher than 4.5C! If S really is close to 3, then of course more (paleo reanalyses, as well as future) data is bound to improve our confidence in this value, and "fundamental physical reasons" can hardly prevent this. Furthermore, your (correct) comment that the observational agreement is generally worse for high values is a severe threat to your conclusion that such values cannot be reasonably be ruled out. [James Annan (Reviewer's comment ID #: 6-13)]	range, the most likely value, and the possibility of high values. Summary statment is identical to box 10.2. Review comment identical to 10-1250, see reply to 10-1250. 'Likely 2-4.5K' uses standard terms of probability. No statement is possible for values like 6K.
10-122	A	3:51	3:51	Delete "very" [VINCENT GRAY (Reviewer's comment ID #: 88-1483)]	Rejected. No reason given for proposed change.
10-123	A	3:56	3:56	Delete "very likely" [VINCENT GRAY (Reviewer's comment ID #: 88-1484)]	Rejected. No reason given for proposed change.
10-124	A	3:56	3:57	An quantitative indication of how significant these increase are likely to be compared to the present is needed here. [William Hare (Reviewer's comment ID #: 99-44)]	We state clearly "very likely"
10-125	A	3:56	4:3	CONTENT: Include specific/numeric results on the projected increase of heat waves under different emission scenarios. [Govt. of Germany (Reviewer's comment ID #: 2011-139)]	This is too detailed for the executive summary. More details are given in the body of the text.
10-126	A	3:56		change "likely" to "high" [Danny Harvey (Reviewer's comment ID #: 101-65)]	We state clearly "very likely" because that gives an assessment of certainty
10-127	A	4:1		the "decrease of diurnal temperature ranges" might merit a separate sentence, since this parameter is of particular importance to plant growth and flowering. In addition, this observation is not self-evident; if the maximum temperature increased as much as the minimum temperature, the range would stay the same. Therefore: "Almost everywhere, daily minimum temperatures increase faster than daily maximum temperatures, therefore diurnal temperature ranges decrease." [Govt. of Germany (Reviewer's comment ID #: 2011-140)]	Accepted.
10-128	A	4:2	4:2	Replace "are shown to " by "may" [VINCENT GRAY (Reviewer's comment ID #: 88-1485)]	Wording changed to "are projected to"
10-129	A	4:5	4:18	No order of magnitude of the precipitation changes is provided [Govt. of France (Reviewer's comment ID #: 2010-70)]	Rejected. This is too much detail for the executive summary. Specific values are provided in the body of the text.
10-130	A	4:6	4:9	Here it would be better to be specific about the regions that are particularly affected. [European Commission (Reviewer's comment ID #: 2008-30)]	Rejected. We already provide some general guidance here as to regions.

No.	Batch	Page:line From	Page:line To	Comment	Notes
					More details are provided in the body of the text.
10-131	A	4:6	4:9	PRESENTATION: Avoid the term "precipitation maxima" in the section on "mean precipitation" as this mixture of temporal and regional maxima is confusing to the reader. Rather name the affected regions with dryer and wetter mean conditions specifically, such as the Mediterranean region, Australia, South Africa etc. [Govt. of Germany (Reviewer's comment ID #: 2011-141)]	Rejected. We already provide some general guidance here as to regions. More details are provided in the body of the text. However, we clarify "precipitation maxima" with the qualifier "regional".
10-132	A	4:6	4:6	Rep[lace "show" by "indicate" [VINCENT GRAY (Reviewer's comment ID #: 88-1486)]	Accepted.
10-133	A	4:9	4:9	Insert after "precipitation" "are projected to" [VINCENT GRAY (Reviewer's comment ID #: 88-1487)]	Accepted.
10-134	A	4:12	4:13	Text in brackets hard to understand for a policy reader. Perhaps rephrase i.e. proportionately more of the total annual precipitation projected falls in a given precipitation event. [Govt. of Australia (Reviewer's comment ID #: 2001-377)]	Rejected. This is not the correct interpretation of precipitation intensity. This has been re-worded slightly for clarity.
10-135	A	4:12	4:12	Replacve "increase" with "is projected toincrease" [VINCENT GRAY (Reviewer's comment ID #: 88-1488)]	Accepted.
10-136	A	4:13	4:13	Insert after ."that" "may" [VINCENT GRAY (Reviewer's comment ID #: 88-1489)]	Rejected. This is a model result, and this is exactly what the models show.
10-137	A	4:15	4:15	Replacve "increase" with "is projected toincrease" [VINCENT GRAY (Reviewer's comment ID #: 88-1490)]	Accepted.
10-138	A	4:15	4:15	"… for summer drying … during summer" - drop one "summer". [Anthony Hirst (Reviewer's comment ID #: 107-4)]	Accepted.
10-139	A	4:16	4:16	Replace "is" by "may be" [VINCENT GRAY (Reviewer's comment ID #: 88-1491)]	Rejected. No reason given for proposed change.
10-140	A	4:16		Delete one of the references to summer in this line. [Adrian Simmons (Reviewer's comment ID #: 242-138)]	Accepted.
10-141	A	4:17	4:18	This sentense is not correct if it is in terms of percentage change and the definition of extreme includes given percentile values. According to Emori and Brown (2005), the percentage changes in mean and extreme precipitation are not very different in high latitudes except northern North Atlantic (compare their Figures 2a and 3a). It should be changed, for example, to "Precipitation extremes increase more than does the mean in some tropical areas." If it means to include also the areas where the mean precipitation decreases (negative increase), it would be better to state "Precipitation extremes increase more than does the	Accepted. However the level of proposed detail is too much for an executive summary. Given the geographic complexity referred to, we now change "most" to "many", and the reader can go to the main body of text for regional details.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				mean in some tropical areas, many subtropical and mid-latitude areas (where the mean decreases), and a part of high-latitude areas." [Seita Emori (Reviewer's comment ID #: 62-15)]	
10-142	A	4:17	4:17	Insert after "extremes" "could" [VINCENT GRAY (Reviewer's comment ID #: 88-1492)]	Rejected. This is a model result, and this is exactly what the models show.
10-143	A	4:21	3:21	Insert after "caps" "may" [VINCENT GRAY (Reviewer's comment ID #: 88-1495)]	Rejected. No reason given to support request.
10-144	A	4:21	4:21	Replace "As" by "If" [VINCENT GRAY (Reviewer's comment ID #: 88-1493)]	Rejected. No reason given to support request.
10-145	A	4:21	4:21	Insert after "extent" "may" [VINCENT GRAY (Reviewer's comment ID #: 88-1494)]	Rejected. No reason given to support request.
10-146	A	4:23	4:23	Insert after "Sea ice" "is projected to" [VINCENT GRAY (Reviewer's comment ID #: 88-1496)]	Accepted.
10-147	A	4:24	4:24	Insert before "reduction" "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1497)]	Accepted.
10-148	A	4:25	4:25	Specify which models. "Forcing" should be "emissions". Results only refer to summer ice cover. [European Commission (Reviewer's comment ID #: 2008-31)]	Accepted. Wording changed.
10-149	A	4:25	4:25	CONTENT: Change sentence "…some models project sea ice cover to disappear entirely in the high forcing A2 scenario in the latter part of the 21st century" to something more precise like "… some models project SUMMER sea ice cover to disappear entirely in the high EMISSION A2 scenario in latter part of the 21st century. The mean of the models under the non-mitigation SRES A2 and A1B emission scenarios projects a decrease of about 80% in summer sea ice." Reasons for change: a) The results refer only to Northern Hemispheric SUMMER (cp. figure 10.3.10 b); b) The expression "SOME models... " is vague, therefore the MEAN results are added; c) A2 is not a forcing, but an emission scenario, which is a crucial difference. [Govt. of Germany (Reviewer's comment ID #: 2011-142)]	Accepted: Should say summer. Word Emission is used. Rejected. We prefer to use our original wording without referring to the emission scenario specific mean.
10-150	A	4:25	4:24	Summer or all ice? [William Hare (Reviewer's comment ID #: 99-47)]	Accepted: Should say summer.
10-151	A	4:25		is annual or summer sea ice cover projected to disappear? [gabi hegerl (Reviewer's comment ID #: 103-17)]	Accepted: Should say summer.
10-152	A	4:26	4:27	Not clear that this refers to the current permafrost. [European Commission (Reviewer's comment ID #: 2008-32)]	Accepted: Sentence changed
10-153	A	4:26	4:27	CONTENT: Change sentence "Thawing of the upper layer of permafrost is … " to "SUMMER thawing of the upper layer of PRESENT permafrost is…"	Accepted: Sentence changed

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Govt. of Germany (Reviewer's comment ID #: 2011-143)]	
10-154	A	4:26	4:26	The statement "Thawing of the upper ..." is difficult to understand. Is the message that melting from top will take place over 90% of the perennially frozen area or will the active layer thicknes increase by 90%? Use a better term than "upper layer" (active layer?) and make clear what "90%" refers to (area, thickness?). [Wilfried Haeberli (Reviewer's comment ID #: 94-14)]	Accepted: Sentence changed
10-155	A	4:26	4:27	as much as 90% - I assume this is a spatial reduction. The wording is such that it could mean vertically. Reword. [Ronald J Stouffer (Reviewer's comment ID #: 258-106)]	Accepted: Sentence changed
10-156	A	4:27	4:27	Add at end "But, of course, this scenario is highly improbable" [VINCENT GRAY (Reviewer's comment ID #: 88-1498)]	Rejected. No reason given to support request.
10-157	A	4:30	4:30	Delete "unanimous" [VINCENT GRAY (Reviewer's comment ID #: 88-1499)]	REJECTED : no reason justifying change
10-158	A	4:31	4:31	(1) Explain "Earth System" - oceans and terrestrial. (2) ……"absorbed anthropogenic carbon dioxide from the atmosphere" [Govt. of Australia (Reviewer's comment ID #: 2001-378)]	ACCEPTED : sentence rephrased
10-159	A	4:31	4:31	Replace "would" by "could" [VINCENT GRAY (Reviewer's comment ID #: 88-1500)]	REJECTED : no reason justifying change
10-160	A	4:31	4:31	Replace "anthropogenic" by "additional" [VINCENT GRAY (Reviewer's comment ID #: 88-1501)]	REJECTED : no reason justifying change
10-161	A	4:32	4:32	"growingly large fraction" ugh [James Annan (Reviewer's comment ID #: 6-14)]	ACCEPTED : sentence rephrased
10-162	A	4:32	4:32	"growingly large" is bad English - how about "increasing"? [Paul Baer (Reviewer's comment ID #: 10-4)]	ACCEPTED : sentence rephrased
10-163	A	4:32	4:32	Replace "agrowingly large" by "an increrasing" [VINCENT GRAY (Reviewer's comment ID #: 88-1502)]	ACCEPTED : sentence rephrased
10-164	A	4:32	4:32	Replace "anthropgenic" by "human related" [VINCENT GRAY (Reviewer's comment ID #: 88-1503)]	REJECTED : no reason justifying change
10-165	A	4:32	4:35	There are two points of confusion here. First, if the positive feedback ranges from an extra 20 to an extra 200 ppmv, then the difference between the low and high feedback cases is 180 ppmv. However, you give the range experienced with feedback as 730-1020 ppmv, a difference of 290 ppmv. Second, the term "standard value" is vague. I had thought that you meant the case without feedback (as clearly implied here), in which case the 730 ppmv case represents negative feedback, but the implication is that the feedback is always positive. However, the main text (page 36, lines 45-46) makes it clear that this "standard"	ACCEPTED : section clarified

No.	Batch	Page:line From	Page:line To	Comment	Notes
				case actually includes a positive climate-carbon cycle feedback. The wording needs to be clarified and the contradictions resolved. [Danny Harvey (Reviewer's comment ID #: 101-67)]	
10-166	A	4:32	4:32	Change "growingly large fraction" to "a larger and larger fraction". [Ronald J Stouffer (Reviewer's comment ID #: 258-107)]	ACCEPTED : wording changed
10-167	A	4:32		change "a growingly large" to "an increasingly large" [Danny Harvey (Reviewer's comment ID #: 101-66)]	ACCEPTED
10-168	A	4:32		The reference to the CO2 staying "airborne" should be rewritten. CO2 is part of the air itself, not something that is airborne. "remain in the atmosphere" is better than "stay airborne". [Adrian Simmons (Reviewer's comment ID #: 242-139)]	ACCEPTED
10-169	A	4:32		replace "growingly" by "increasingly", and "airborne" by "in the atmosphere" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-32)]	ACCEPTED
10-170	A	4:33	4:33	…….additional atmospheric CO2 concentration….. [Govt. of Australia (Reviewer's comment ID #: 2001-379)]	ACCEPTED
10-171	A	4:34	4:34	730 and 1020 ppm - How can the lower end of the range be lower than 830 and be consistent with the statement that the carbon feedbacks increase the CO2 in the atmosphere. Needs more… [Ronald J Stouffer (Reviewer's comment ID #: 258-108)]	ACCEPTED : the text is clarified. The positive feedback refers to the difference between the coupled and uncoupled runs for each of the C4MIP model, not to the difference between the C4MIP models concentration and the one used by the standard AR4 AOGCMs
10-172	A	4:35	4:35	need to phrase this differently, because as it stands it implies that the carbon cycle models could be above or below the "standard" value of 830. This implies that carbon cycle feedbacks could be positive or negative - need to explain that values below 830 still represent a positive feedback, because the "uncoupled" run of that model would have been lower still. [Chris Jones (Reviewer's comment ID #: 120-19)]	ACCEPTED : the text is clarified.
10-173	A	4:37	4:37	presumably there is a cooler value to match the extra +1.2C warmer. [James Annan (Reviewer's comment ID #: 6-15)]	NOTED : the full range of warming is given
10-174	A	4:37	4:39	Suggest stating the full range of temperature enhancements from carbon-climate feedbacks, not only the upper extreme. [Haroon Kheshgi (Reviewer's comment ID #: 125-42)]	ACCEPTED: the full range of warming is given
10-175	A	4:37	4:37	upper warming estimate 1.2C higher - Awkward. Reword.	ACCEPTED : sentence clarified

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Ronald J Stouffer (Reviewer's comment ID #: 258-109)]	
10-176	A	4:38	4:38	Cannot understand phrase "where the CO2 concentration is prescribed" [Govt. of Australia (Reviewer's comment ID #: 2001-380)]	ACCEPTED : sentence clarified
10-177	A	4:39	4:40	it leads to a reduction of the emissions required to achieve a given atmospheric CO2 stabilization - this is very unclear to me. Reduction from what? Previous emission scenarios? [Ronald J Stouffer (Reviewer's comment ID #: 258-110)]	ACCEPTED : sentence clarified
10-178	A	4:40	4:40	"……..a reduction in the direct anthropogenic emissions required…." Purpose of change is clarification - since some projected changes to global carbon cycle involve emissions (eg affects on soil carbon with warmer temperature). [Govt. of Australia (Reviewer's comment ID #: 2001-381)]	ACCEPTED : sentence clarified
10-179	A	4:40	4:42	The sentence starting: "The higher the stabilization scenario .." might be misleading. It is proposed to delete the last part: ..and hence the larger the emission reduction. [Govt. of Austria (Reviewer's comment ID #: 2002-52)]	ACCEPTED : sentence clarified
10-180	A	4:40		Insert "greater" before "reduction" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-33)]	NOTED : sentence clarified
10-181	A	4:41	4:41	Suggest "stablisation scenario" should either be expressed more fully, or described in Glossary. [Govt. of Australia (Reviewer's comment ID #: 2001-382)]	NOTED : sentence clarified
10-182	A	4:42	4:42	Final clause is unclear - "larger emission reductions" to do what? Sentence needs recasting. [Govt. of Australia (Reviewer's comment ID #: 2001-383)]	NOTED : sentence clarified
10-183	A	4:42	4:42	Add at end "The highly improbable status of the A2 scenario makes all of this highly speculative"" [VINCENT GRAY (Reviewer's comment ID #: 88-1504)]	REJECTED : no reason given for suggested change
10-184	A	4:44	5:2	Sea level rise: The issues raised here reflect concerns expressed in general comments and the chapter section on sea level change. 1) The range of SLR estimates needs to be shown for the full range of scenarios deployed in the earlier section on temperature and the individual components explained. 2) A comparison of each term with recent trends would be useful in illustrating the uncertainties involved in these projections (eg Church et al estimate that if current trends continue in the observed acceleration of SLR that ca 35cm increase would result by 2100, recent glacier and small ice cap losses are around 0.8mm/yr SLRE and the projected losses are of this order over the next century, and a comparison of the ice sheet contributions from recent observations with that from models to 2100.3) More fundamentally there seems to be an uexplained disconnection between the ice sheet projections and recent observations (Chapter 4) that imply that loss rates are	Taken into account. Unlike in the TAR, chapter 10 is not producing a combined scenario range for temperature or SL, but we will produce SL ranges for the marker scenarios. Models and observations are compared in chapter 9 and this will be clarified. We will clarify how the 1993-2003 rate of SL rise is caused. We note that natural and internal variability may have contributed to its high rate. We will

No.	Batch	Page:line From	Page:line To	Comment	Notes
				likely to slow? Can this be correct? [William Hare (Reviewer's comment ID #: 99-45)]	include an allowance for ice-sheet dynamical acceleration in the projections.
10-185	A	4:44	5:2	I think it is simply unacceptable to be putting out numbers that do not include any account of the potential for ice stream acceleration, or, and as importantly, for any of the terms relating to unexplained sea level rise during the 20th century. Given that sea level has been carefully observed over the last decade and is rising at a rate of 3 mm per year, and this rate of rise looks to be a relatively smooth acceleration from values for earlier this century (e.g., see Figure 5.5.1 or TS-21), a reasonable assumption would be that there would be a continuing rise at this rate or slightly more. This would lead to at least a 60 mm rise over the period 2000 to 2020 (and over 150 mm by 2050 and over 300 by 2100 not accounting for any acceleration--and that is occurring and very likely to continue--even the results shown indicate that). Yet the estimate here for 2050 is less than 60% of the extrapolated trend value--implying that there will be some unknown mechanism leading to a 40% reduction in the rate of rise even as the world continues to warm. Even the upper limit indicated is less (by a small amount) that a continuation of the present rate without any acceleration. This is very wishful thinking--is there any physical basis for thinking there are mechanisms to slow the rate by so much? It is fine to indicate shat the model results show for various terms, but using them alone to develop the range of projections for what is likely to occur simply cannot ignore the observational record, as is done here. Not somehow including account for the effects of the real mechanism of glacial flow in the uncertainty range is, I think, unacceptable. Especially given how it is clear that the media and public focus on numbers, putting out numbers that are so low seems to me totally irresponsible and completely incompatible with the TAR results. [Michael MacCracken (Reviewer's comment ID #: 152-267)]	Accepted. Models and observations are compared in chapter 9 and this will be clarified. We will clarify how the 1993-2003 rate of SL rise is caused. We note that natural and internal variability may have contributed to its high rate. We will include an allowance for ice-sheet dynamical acceleration in the projections.
10-186	A	4:44	5:2	I'm somewhat uneasy with the discussion here. Sea Level Rise (SLR) is likely to be one of the most severe potential climate change impacts on society, yet there is no attempt here to provide a truly comprehensive assessment of the potential changes and their true uncertainties. The discussion here centers on one particular scenario and a certain set of model simulations. Projected rates are provided based on these estimates, yet is seems quite plausible that these are significant underestimates. The physics of basal lubrication and the importance of ice dynamics (as highlighted by Rignot and Kanagaratnam, 2006) are poorly understood and not fully accounted for in current climate model simulations. Until we can be relatively confident that these effects are well represented, there is a substantial probability that we may be underestimating the dynamic nature of the ice sheets, and the potential for significant increases in ice streaming and calving. It is casually noted here that "The Greenland ice sheet is projected to lose mass, because increased melting will exceed increased snowfall; it would contribute more to sea level if	Taken into account. We will include an allowance for ice-sheet dynamical acceleration in the projections.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				there are further accelerations in ice flow.". Yet Rignot and Kanagaratnam (2006) suggests that there is evidence of further such acceleration already, i.e. that our previous estimates of ice streaming have been a factor of two too small. There is some suggestion (e.g. Cook et al, Science, '05) that the same is true for Antarctica. It would seem quite possible that the models significantly underestimate the potential acceleration of ablation for both major ice sheets. This issue needs to be handled with far more circumspection than at present, for it has potentially greater societal ramifications then any other issue delt with in AR4. [Michael Mann (Reviewer's comment ID #: 156-56)]	
10-187	A	4:44		This bullet in my view needs a rewrite. Why do we not get a proper range of sea level rise like in the TAR? Why focus only on A1B? It is impossible to derive a range from the given value for A1B and the "scenario spread", as we don't know whether A1B is in the middle of the spread. Also we don't know what climate sensitivity range the spread of +-150mm for A1B covers, and what uncertainty range in ocean mixing. When giving a modeled range, please include a caveat like: "but note these models underestimate the past observed sea level rise by 40%". There is very little credibility in the IPCC projecting a slow-down in sea level rise compared to the past decades in response to an increase in temperature. [Stefan Rahmstorf (Reviewer's comment ID #: 206-5)]	Taken into account. Unlike in the TAR, chapter 10 is not producing a combined scenario range for temperature or SL, but we will produce SL ranges for the marker scenarios. Models and observations are compared in chapter 9 and this will be clarified. We will clarify how the 1993-2003 rate of SL rise is caused. We note that natural and internal variability may have contributed to its high rate. We will include an allowance for ice-sheet dynamical acceleration in the projections.
10-188	A	4:45	4:45	The projected best-estimate rate of sea-level rise requires that there be a strong deceleration over the next 14 years. The models are apparently unable to match the ongoing rate of sea-level rise. As a minimum, the authors should consider adding the unexplained contribution to the projections--assuming that the unexplained contribution will drop to zero (or become negative, actually) seems unduly optimistic. [Richard B. Alley (Reviewer's comment ID #: 4-11)]	Accepted. Models and observations are compared in chapter 9 and this will be clarified. We will clarify how the 1993-2003 rate of SL rise is caused. We note that natural and internal variability may have contributed to its high rate. We will include an allowance for ice-sheet dynamical acceleration in the projections.
10-189	A	4:45	4:47	The conclusion that sea-level projections are lower and less uncertain than in the TAR seems to be at odds with the mismatch between observations and modelling results. This requires some discussion, in particular of the limitations of current models. [European Commission (Reviewer's comment ID #: 2008-34)]	Taken into account. Models and observations are compared in chapter 9 and this will be clarified. We will clarify how the 1993-2003 rate of SL rise is caused. We note that natural and

No.	Batch	Page:line From	Page:line To	Comment	Notes
					internal variability may have contributed to its high rate. Unlike in the TAR, chapter 10 is not producing a combined scenario range for temperature or SL, but we will produce SL ranges for the marker scenarios and compare these with the TAR.
10-190	A	4:45	4:45	Are these uncertainty estimates ±1 std or ±2 std? [Govt. of Finland (Reviewer's comment ID #: 2009-121)]	Taken into account. All uncertainties will be expressed as 5-95% ranges.
10-191	A	4:45	4:48	Please give explicit estimates of sea level rise by 2100 also for the B1, A2 and AIFI scenarios. [Govt. of Finland (Reviewer's comment ID #: 2009-122)]	Accepted. SL ranges will begiven for other marker scenarios.
10-192	A	4:45	4:47	CONTENT: The stated mean SLR projection and its uncertainty range of sea level rise by 2100 above 2000 for the A1B scenario, namely 290+-150mm seems to be markedly lower compared to the TAR, where the mean projection was around 390 mm and the upper uncertainty range 700mm above 1990 (cf. Figure 5e in WG1 SPM TAR). If this implicit conclusion ('lower and less uncertain sea level rise projections compared to TAR') is indeed drawn, despite the mismatch between current observations and modeling studies of ice sheets, this conclusions needs to be thoroughly reasoned - as well here in the Executive Summary. Alternatively, remarks about the major uncertainties given the recently observed accelerating ice-sheets have to be placed upfront, instead of the possibly misleadingly - likely inadequate - model projections. [Govt. of Germany (Reviewer's comment ID #: 2011-144)]	Taken into account. We will produce SL ranges for the marker scenarios and compare these with the TAR. We will include an allowance for ice-sheet dynamical acceleration in the projections.
10-193	A	4:45	4:46	the increase in sea level in 1993-2003 was 31 mm. This statement here says that the increase in sea level will slow down in the next 2 decades. This needs to be commented. Do models reproduce the observed sea level and heat content increase in the past 50 years? [Corinne Le Quere (Reviewer's comment ID #: 143-7)]	Taken into account. Models and observations are compared in chapter 9 and this will be clarified. We will clarify how the 1993-2003 rate of SL rise is caused. We note that natural and internal variability may have contributed to its high rate.
10-194	A	4:45	4:45	The number cited (35+/-25 mm over two decades) is puzzling. It seems to predict on the average a decrease in the rate of SLR over the next decade, since the observed rate over 1993-2003 (page 10-61) is 31 mm/decade. This reinforces my concerns that the discussion in the executive summary does not reflect the potential uncertainties and in particular, the potential that SLR will be significantly greater than what is indicated. [Michael Mann (Reviewer's comment ID #: 156-57)]	Taken into account. Models and observations are compared in chapter 9 and this will be clarified. We will clarify how the 1993-2003 rate of SL rise is caused. We note that natural and internal variability may have contributed to its high rate.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-195	A	4:45	4:49	The figure of 34±25 mm of sea level rise in the 20 year period from 2000 to 2020 seems extremely unlikely to in fact be an accurate estimate. My guess is that this is model output that was uncritically reported, without noting that the models badly underestimate observed sea level rise of the past decade. Current observed sea level rise is 3.1 mm/yr. Simple extrapolation forward in time yields 62 mm over the two decade period in question. This actually exceeds the stated error limits! Instead, I recommend that the text go beyond models to incorporate all of the scientific evidence that is relevant. This includes observations of current sea level rise from Topex-Poseidon, and glacier sliding estimates that have been published recently. Glacier "ice quakes" in Greenland point to a rapid increase in glacier flow, possibly in response to anthropogenic warming. Sea levels 3 m higher than today are seen in the paleoclimate records from the last interglacial period, 129-119 kyr before present. The temperature is thought to have been about 2 C warmer, similar to what we will experience mid-century. Models do not accurately simulate this 3 m rise, implying that the models are missing some critical physics, and therefore should not be trusted on the topic of future sea level rise. [Jeffrey Severinghaus (Reviewer's comment ID #: 232-5)]	Taken into account. Models and observations are compared in chapter 9 and this will be clarified. We will clarify how the 1993-2003 rate of SL rise is caused. We note that natural and internal variability may have contributed to its high rate. We will include an allowance for ice-sheet dynamical acceleration in the projections.
10-196	A	4:45	5:2	Sea level rise projected by the models is not the same as the expected value – particularly since 100% of the models do not include any kind of dynamic ice sheet response. Since this is the biggest uncertainty in this term, I would strongly suggest simply using the models to say something about thermal expansion, but leaving the contribuiton from ice sheets to the glaciologists. Definitive statements about the negative mass balance in Antarctica are ill-advised. [Gavin Schmidt (Reviewer's comment ID #: 227-14)]	Taken into account. We will include an allowance for ice-sheet dynamical acceleration in the projections. (Note that glaciological models for this are so far rudimentary, but that surface mass balance models, projecting increasing accumulation on Antarctica, are relatively well established.)
10-197	A	4:45		Better to give spread over models than spread over scenarios. If the first is small that gives higher confidence in models, if the later is small this means: No matter what scenario is chosen, the outcome is the same. I do not think the later is the case. [Anders Levermann (Reviewer's comment ID #: 145-5)]	Accepted. Spread over scenarios will not be given. Instead, we will provide ranges for individual scenarios.
10-198	A	4:45		As I understand it, the estimate given for the sea level rise underestimates even the present rate of change. This is likely caused by problems with the models. I think such a caveat should be mentioned, such that the reader can put these estimates into place. I would be helpful as well to mention that IPCC TAR projections for the period up to 2005 where lower than what was observed during this period. This would also place the given estimates at the lower end of possibilities. [Anders Levermann (Reviewer's comment ID #: 145-6)]	Taken into account. Models and observations are compared in chapter 9 and this will be clarified. We will clarify how the 1993-2003 rate of SL rise is caused. We note that natural and internal variability may have contributed to its high rate.
10-199	A	4:45	:54	It needs to be made absolutely clear (I.e. much more so than it is at present) that these results relate solely to thermal expansion, and are therefore minimum estimates, possibly	Rejected. These projections do include all components, of which thermal

No.	Batch	Page:line From	Page:line To	Comment	Notes
				by a large margin, especially after 2100 [Govt. of United Kingdom (Reviewer's comment ID #: 2022-34)]	expansion is the largest, and is stated separately.
10-200	A	4:45		Projections on sea level rise (without full uncertainty analysis) must also be present for the B1 and A2 scenarios as high and low projections. This is critical to avoid the appearance of selection. Either give the calculated numbers or make an expert judgment. Going without looks biased. Basing projections on a single emissions scenario may unduly narrow the range. Past efforts generally tried to ensure that each time different factors were considered to develop alternative temperature estimates (e.g. emissions, carbon cycle models), a corresponding estimate of sea level rise was also reported. [Govt. of United States of America (Reviewer's comment ID #: 2023-659)]	Accepted. We will produce SL ranges for the marker scenarios.
10-201	A	4:46	4:46	It is important in this section to address all SRES scenarios, so as to be explicit about uncertainty. [European Commission (Reviewer's comment ID #: 2008-33)]	Accepted. We will produce SL ranges for the marker scenarios.
10-202	A	4:46	4:46	PRESENTATION: The sea level rise section needs to be reordered for clarity. Furthermore, in parallel to the temperature section, all IPCC SRES marker scenarios A1FI, A1B, A1T, A2, B1 and B2 need to be addressed, not only A1B. Given the apparent mismatch between model estimates and present observations for sea level rise, this uncertainty and its possible implications for the projections have to be stated upfront (possibly inadequate representation of accelerating WAIS, Greenland disintegration). [Govt. of Germany (Reviewer's comment ID #: 2011-145)]	Accepted. We will produce SL ranges for the marker scenarios. We will include an allowance for ice-sheet dynamical acceleration in the projections.
10-203	A	4:46	4:46	2100, at a rate - Add "of increase" after "rate". [Ronald J Stouffer (Reviewer's comment ID #: 258-111)]	Accepted.
10-204	A	4:46		These ranges, in particular the 14-43cm range, are not well explained and well qualified in section 10.6 See my comments to section 10.6.5 [Gerrit Burgers (Reviewer's comment ID #: 34-20)]	See 10-205.
10-205	A	4:46		These ranges, in particular the 14-43cm range, (i) cover only part of the range discussed at page 61, lines 42-56 and (ii) cover only the A1B runs. I propose to (i) extend the range in such a way that it includes the "models that agree with observations estimate" and the "correction for mismatch between total and sum of terms" estimate and (ii) includes results for B1 and A2 runs as well. [Gerrit Burgers (Reviewer's comment ID #: 34-21)]	Taken into account. Models and observations are compared in chapter 9 and this will be clarified. We will clarify how the 1993-2003 rate of SL rise is caused. We will produce SL ranges for the marker scenarios.
10-206	A	4:47	4:47	What is an "average model"? Second sentence hard to relate to first sentence in para. [Govt. of Australia (Reviewer's comment ID #: 2001-384)]	Accepted. Results for average model deleted.
10-207	A	4:49	4:54	Chapter 6.2.3. states that the Antarctic ice sheet was smaller than today in a climate warmer by 2-3 °C (Pliocene). Furthermore in chapter 5 one reads Antarctica already has been losing mass. Therefore replace the sentences "Since the Antarctic Ice sheet....under scenario A1B:" by "Models suggest that the Antarctic ice sheet might grow in a warmer	Taken into account. We will include an allowance for ice-sheet dynamical acceleration in the projections. The state of science does not permit to

No.	Batch	Page:line From	Page:line To	Comment	Notes
				climate, but these models do not capture important aspects of ice flow dynamics, and they are contradicted by recent observational data and paleoclimatic evidence. In view of all evidence, it has to be considered more likely than not that the Antarctic ice sheet will shrink rather than grow in a warming climate, but the uncertainty on this issue remains high." [Govt. of Germany (Reviewer's comment ID #: 2011-60)]	assess the probability that dynamical acceleration will exceed increased accumulation.
10-208	A	4:49	4:54	I think this statement ("Since the Antarctic ice sheet will receive increased snowfall without experiencing substantial surface melting, it will contribute negatively to sea level unless there are larger accelerations in ice flow of the kind presently taking place in some West Antarctic ice streams.-") is misleading as it is a statement only about what models are projecting and does not put this in the context of recent observations of negative mass balance, which cannot be attributed to earlier (Holocene or deglacial climate changes). Vaughan estimates that rf the current discharge doubles then an additional 5cm/century rise would result and if the discharges speeds accelerate toward those of Jakobshavn Isbrae (whose own acceleration is recent and unexpected from model assessments) would contribute 30cm/century. This would approach (at the lower end) and outweigh at the upper end projected increases in accumulation. I think might be best to say something quite frank about this situations: "Observed changes in the Antarctic ice sheet in particular the overall mass loss estimated for the most recent decade at present are not able to be quantitatively explained. This situation qualifies model based projections of the future contribution of this ice sheet to sea level rise. W whilst accumulation is very likely to increase there is a risk that a continued acceleration of ice loss from the WAIS Amundsen sea sector could outweigh. At this stage no likelihood can be placed on either outcome". [William Hare (Reviewer's comment ID #: 99-46)]	Accepted. We will include an allowance for ice-sheet dynamical acceleration in the projections.
10-209	A	4:49		The statement that Antarctica will contribute negatively to sea level contradicts the finding of chapter 5 that Antarctica already has been losing mass (see also the recent GRACE mission results). Also it contradicts paleoclimatic evidence, which points to a smaller Antarctic ice sheet in a climate warmer by 2-3 °C in the Pliocene (see section 6.2.3). Hence, a fair assessment of the evidence would read something like this: "Models suggest that the Antarctic ice sheet might grow in a warmer climate, but these models do not capture important aspects of ice flow dynamics, and they are contradicted by recent observational data and paleoclimatic evidence. In view of all evidence, it has to be considered more likely than not that the Antarctic ice sheet will shrink rather than grow in a warming climate, but the uncertainty on this issue remains high." Please do not uncritically trust models in areas where they do not capture observed changes. [Stefan Rahmstorf (Reviewer's comment ID #: 206-6)]	See 10-207.

No.	Batch	Page:line From	To	Comment	Notes
10-210	A	4:49	:56	Please reconcile the projected negative contribution to sea level rise from Antarctica, with apparent current positive contribution discussed in Chapter 4. [Govt. of United States of America (Reviewer's comment ID #: 2023-660)]	Taken into account. We will include an allowance for ice-sheet dynamical acceleration in the projections.
10-211	A	4:50	4:50	Replace "will receive" by "is projected to receive" [VINCENT GRAY (Reviewer's comment ID #: 88-1505)]	Rejected. It is obviously a projection in this context.
10-212	A	4:50	4:50	Replace "will contribute" with "may contribute" [VINCENT GRAY (Reviewer's comment ID #: 88-1506)]	Rejected. It is obviously a projection in this context.
10-213	A	4:52	4:51	Does "larger accelerations" mean more geographically spread pattern of accelerated ice streams; or further acceleration in presently identified ice streams' (or both)? [Govt. of Australia (Reviewer's comment ID #: 2001-385)]	Taken into account by clarifying the text.
10-214	A	4:53	4:53	"Dynamical imbalance" of what? [Govt. of Australia (Reviewer's comment ID #: 2001-386)]	Taken into account; term avoided when text rewritten.
10-215	A	4:54	4:54	"increased snow accumulation in Antarctica…." [Govt. of Australia (Reviewer's comment ID #: 2001-387)]	Accepted.
10-216	A	4:54	:56	Much more could and should be said here about the effect of melting of the Greenland ice-sheet. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-35)]	Include components If space permits.
10-217	A	4:55	4:55	"….. lose mass…." - by when? [Govt. of Australia (Reviewer's comment ID #: 2001-388)]	Taken into account by clarification. It is during the 21st century.
10-218	A	4:55	4:55	correct "lose" [Pascale DELECLUSE (Reviewer's comment ID #: 58-59)]	Rejected. It is correct as "lose".
10-219	A	4:55	4:55	Replace "will" by "may" [VINCENT GRAY (Reviewer's comment ID #: 88-1507)]	Rejected. Since it is part of a statement about a projection, this is not required.
10-220	A	4:56	4:56	Replace "predicted" with "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1508)]	Rejected. This is a result which holds for all SRES scenarios, for instance, not a projection for a particular scenario.
10-221	A	5:1	5:2	I assume the sea level rises in the Arctic are due to the thinning of the sea ice (less weight on the water. Correct? In the Southern Ocean, the changes are related to the poleward wind shift. I assume. [Ronald J Stouffer (Reviewer's comment ID #: 258-112)]	Rejected. In the Arctic it is probably due to freshening, in the Southern Ocean possibly due to windstress, or maybe low thermal expansivity. We don't have space for this in the Exec Summ but it is discussed in 10.6.2.
10-222	A	5:4	5:11	I am not convinced that this accurately summarises the current multi-model finding. I expand later but a more accurate summary might be "The multi-model mean picture is for a weak shift towards conditions which may be described as "El Nino-like" with sea surface temperatures in the central and east Pacific warming more than those in the west,	Accepted. Text modified.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				with an east-ward shift in mean precipitation and a weakening of the interannual ENSO-Asian Australian monsoon connection. However, the term El Nino-like does not accurately describe all the changes seen in models in the tropical Pacific region and some models show opposite changes or changes which conflict with out understanding of present-day El Nino. Based on various assessments of the current multi-model archive in which present day El Nino is now much better simulated than in the TAR, it is unlikely that there will be any changes in ENSO amplitude or period in the coming century under the scenarios considered." A caveat of the form "Projection of changes in mean climate and variability in the tropical Pacific region is still hampered by systematic biases in AOGCMs in the region." [Matthew Collins (Reviewer's comment ID #: 44-26)]	
10-223	A	5:4	5:11	First phrase of the statement is wrong and refers to only one recent publication. The following replacement statement was agreed by a gathering of El Nino in IPCC experts at a recent workshop: "The models with the closer-to-observed El Niño variability do not show, on average, significant changes in the mean state towards the current peak El Niño or La Niña patterns. The projected changes in amplitude of El Niño events for the 21st century are within the variability observed over the last 150 years". Second phrase is ok. The last phrase focus should be on "frequency" rather than "amplitude". [Eric Guilyardi (Reviewer's comment ID #: 91-1)]	Rejected for the mean change. The sentence is based on multi references and is consistent with Figs. 10.3.5 and 10.3.6 where changes in East-West SST gradient in the tropical Pacific are clear (there are differences between east of the dateline and west of it). Taken into account for the variability change.
10-224	A	5:4	5:8	This section contradicts 10.3.5.3 except its conclusion, on which it should be based. In the first paragraphs of 10.3.5.3 it is stated that the most realistic model show either no change in mean state or a slight shift to more El Niño-like conditions. Later, the subset of models that show an El Niño-like response is further discussed. Here, it is claimed that a majority of models show a mean El Niño-like response pattern. Change to "The models that simulate ENSO in the current climate most realistically show no large changes in the mean state that resemble the ENSO pattern. Equatorial SST warms more than off-equatorial SST, but there the longitude of largest heating is model-dependent." This is based on the CMIP-2 work of Collins et al. and the 4AR model work by van Oldenborgh et al., see www.knmi.nl/research/oceanography/enso/global_warming for a summary with the relevant plots. [Govt. of Netherlands (Reviewer's comment ID #: 2016-48)]	See note 10-223.
10-225	A	5:5	5:5	Replace "show" with"is projected to" [VINCENT GRAY (Reviewer's comment ID #: 88-1509)]	Rejected. Not relevant here.
10-226	A	5:8	5:11	The changes in the amplitude of ENSO in the most realistic models over 2051-2100 are all of the same magnitude as the natural variability from 1850 to 2000, so a statement can be made: "Future changes of ENSO interannual variability differ from model to model. However,	Taken into account. Text modified.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				in the most realistic models the changes in amplitude up to 2100 do not exceed the observed variability since the mid-nineteenth century." [Govt. of Netherlands (Reviewer's comment ID #: 2016-49)]	
10-227	A	5:13	5:20	The statements concerning increased rainfall in the australasian and west african monsoons are stronger than those in Ch. 11 and need to be harmonized with that Chapter. There is little agreement in the Australasian case among the AR4 models, while for Africa the conjunction of 1) the inability of most coupled models to simulate the 20th century drought, 2) empirical downscaling results that produce drying when forced by input from global models that produce increase in precip, and 3) one global model that simulates the 20th century very well that projects drying in the 21st century, has convinced the African sub-group on Ch. 11 to avoid saying that west African rainfall will likely increase [Isaac Held (Reviewer's comment ID #: 105-53)]	Taken into account. Text modified.
10-228	A	5:13	5:20	These important results on the effects on the monsoon need to be carried forward to the TS and SPM as they affect such a large proportion of the world's population. [Michael MacCracken (Reviewer's comment ID #: 152-268)]	Noted.
10-229	A	5:14	5:20	Does increase in precipitation lead to more soil water availability? If not, mention? [Ronald J Stouffer (Reviewer's comment ID #: 258-113)]	No change necessary.
10-230	A	5:23	5:23	Phrase "sea level pressure" seems inprecise. Do you mean atmospheric pressure at sea level? [Govt. of Australia (Reviewer's comment ID #: 2001-389)]	"sea level pressure" is an accepted term that is in general usage in the scientific literature.
10-231	A	5:23	5:23	Replace "increases" with"is projected to increrase" [VINCENT GRAY (Reviewer's comment ID #: 88-1510)]	Accepted.
10-232	A	5:25	5:25	Is "several degrees latitude" correct? The AR4 multimodel studies suggest that there is some poleward shift (Yin 2005) but that this is too small to be seen by visual comparison on hemispheric maps (Lambert and Fyfe 2006). [Govt. of Finland (Reviewer's comment ID #: 2009-123)]	This is a problem of resolution—if there is adequate spatial resolution, this shift can be analyzed.
10-233	A	5:27	5:27	Replace "is" by "may be" [VINCENT GRAY (Reviewer's comment ID #: 88-1511)]	"projected" is inserted here
10-234	A	5:28	5:28	Insert after "that" "possible" [VINCENT GRAY (Reviewer's comment ID #: 88-1512)]	Rejected. This is a model result, and this is exactly what the models show.
10-235	A	5:32	5:36	The global decrease in tropical cyclones frequency should be mentioned here. [Ruth McDonald (Reviewer's comment ID #: 173-10)]	Accepted.
10-236	A	5:32	5:36	The internal text (p 33-35) is much more ambiguous. Add: One prominent study shows a 6% increase in maximum wind speed and an 18% increase in precipitation by 2080, assuming a 1% increase per year in atmospheric carbon dioxide.	Rejected. Nearly all studies show increased maximum wind speeds and precipitation, though with different

No.	Batch	Page:line From	To	Comment	Notes
				[Patrick Michaels (Reviewer's comment ID #: 176-23)]	amplitudes. These details are appropriately given in the main body of the text, not in the executive summary.
10-237	A	5:32	:36	Tropical cyclone section: would it be useful to give approximate quantitative changes in intensity? [gabi hegerl (Reviewer's comment ID #: 103-19)]	Rejected. These numbers are model-dependent and are best given in the main body of the text.
10-238	A	5:33	5:36	This sentence is a little too much affirmative concerning the impact of climate change on the intensity of tropical cyclones. It only accounts for results that have been obtained with high resolution models (less or about 1° according to the text, but apparently only less than 1°) but that have generally their weaknesses compared to their large scale counterparts like the absence of ocean-atmosphere coupling or the absence of scale interaction (for+H72 some mesoscale models embedded in larger scale models). Even with such models, the conclusions are not so clear concerning the intensification of maximum wind speed and the intensification of precipitation associated to tropical cyclones under climate change conditions. For instance, the study of Knutson and Tuleya (2004) shows only a slight increase of maximum wind speed (6%) when precipitation change is more important (18%). Coupled models at about 1° resolution supposed to be covered by the summary, don't always exhibit an increase of cyclone intensity (Sugi et al, 2002; Hasegawa and Emori, 2005). [Govt. of France (Reviewer's comment ID #: 2010-71)]	Accepted. A qualifier is now added: "show a general increase of peak wind..." indicating there are some exceptions but there is more evidence for increased intensity.
10-239	A	5:33	5:36	An other recent study which is not mentioned (Chauvin et al, Climate dynamics, published on line in March 2006), analysing the results of two scenarios performed with a global climate model (uniform or variable resolution but in each case of the order of 50km over the tropical Atlantic), shows that mean hurricane dynamical characteristics are weakly changed by the warming but that precipitation core is significantly enhanced. The sentence should reflect a different degree of confidence between the two results and the results from larger scale coupled GCM should be reflected more accurately. [Govt. of France (Reviewer's comment ID #: 2010-72)]	Accepted, and reference added to main body of text.
10-240	A	5:33	5:36	Statement is too general and cumbersome as written. Suggest: Results from embedded high-resolution models and global models, ranging in grid spacing from 1 degree to 9 km, generally show increased peak wind intensities and, where analyzed, increased near-storm precipitation in future tropical cyclones. Most recent published modeling studies investigating tropical storm frequency simulate a decrease in the overall number of storms, and of the relatively weak storms, in most basins, although some models show an increase in the numbers of the most intense tropical cyclones.	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Thomas Knutson (Reviewer's comment ID #: 132-3)]	
10-241	A	5:33	5:36	This sentence is a little too more affirmative concerning the impact of climate change on the intensity of tropical cyclones. It only account for results that have been obtained with high resolution models (less or about 1° according to the text, but apparently only less than 1°) but that have generally their weaknesses compared to their large scale counterparts like the absence of ocean-atmosphere coupling or the absence of scale interaction (for some mesoscale models embedded in larger scale models). Even with such models, the conclusions are not so clear concerning the intensification of maximum wind speed as it seems to be for the intensification of precipitation associated to tropical cyclones under climate change conditions. For instance, the study of Knutson and Tuleya (2004) shows only a slight increase of maximum wind speed (6%) while precipitation change is more important (18%). Coupled models at about 1° resolution supposed to be covered by the summary, don't always exhibit an increase of cyclone intensity (Sugi et al, 2002; Hasegawa and Emori, 2005). [Serge PLANTON (Reviewer's comment ID #: 199-1)]	This has been re-written
10-242	A	5:33	5:36	An other recent study which is not mentioned (Chauvin et al, Climate dynamics, published on line in March 2006), analysing the results of two scenarios performed with a global climate model (uniform or variable resolution but in each case of the order of 50km over the tropical Atlantic), shows that mean hurricane dynamical characteristics are weakly changed by the warming but that precipitation core is significantly enhanced. The sentence should reflect a different degree of confidence between the two types of results (concerning wind and precipitation) and the results from larger scale coupled GCM should be reflected more accurately. [Serge PLANTON (Reviewer's comment ID #: 199-2)]	Accepted, and we cite the Chauvin et al paper in the main body of the text.
10-243	A	5:34	5:34	Replace "show" with "project" [VINCENT GRAY (Reviewer's comment ID #: 88-1513)]	Accepted.
10-244	A	5:39	5:39	Models projections only show fewer midlatitude storms for the Northern and Southern Hemispheres. There are no consistent regional changes. [Ruth McDonald (Reviewer's comment ID #: 173-27)]	Accepted.
10-245	A	5:39	5:41	I do not consider the evidence for increasing intensity of mid-latitude storms to be at all robust. This seems to me to be a Euro-centric view, and not particularly justified in the global context and certainly not for mid-latitude Southern Hemisphere countries. While this may seem a small point, every piece of grey literature for the next 6 years will be quoting "IPCC (2007) says fewer but more intense mid-latitude storms", when there really needs to be further qualification given.	Accepted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				There doesn't seem to be too much argument about the storm tracks shifting poleward. The main problem is that most published studies have assessed intensity in terms of central pressure, which will always be lower for a sample of cyclones weighted more to higher latitudes. [A. Brett Mullan (Reviewer's comment ID #: 182-8)]	
10-246	A	5:40	5:40	The poleward shift in cyclones is not as clear a signal in the Northern Hemisphere as in the Southern Hemisphere. [Ruth McDonald (Reviewer's comment ID #: 173-28)]	Accepted.
10-247	A	5:43	5:54	Line 50 - "very unlikely" change to "unlikely" - see comments above on TS. This paragraph makes no mention of the model deficiencies as far as being able to get the MOC changes correct goes - e.g. lack of Greenand Ice Sheet melting contributing to changes etc… Furthermore precipitation and river run-off are problematic - all of which means that the freshwater flux into the North Atlantic is probably not that well modelled. It makes no reference to threshold phenomena in models and the uncertainties there (e.g. Rahmstorf et al. 2005 Geophy. Res. Lett.), nor to that fact that possible changes may already be occuring [5.3.2, Box 5.1] which the models may be failing to capture. [Meric Srokosz (Reviewer's comment ID #: 250-12)]	Rejected: see repsonse to same comment by reviewer below.
10-248	A	5:43		I think it is necessary to mention the fact that most of these models do not include glacial melting especially from Greenland. [Anders Levermann (Reviewer's comment ID #: 145-7)]	Noted. This is done in the main body of the Chapter.
10-249	A	5:43		I am wondering, if consequences should not be mentioned at this place too. Especially with respect to the sea level rise in the north Atlantic, the shift in intertropical convergence zone and the effect on oceanic carbon and heat uptake and the effect on ENSO. All of these relate to other paragraphs of this section and would probably dominate changes in these phenomena in the case of a AMOC collapse. [Anders Levermann (Reviewer's comment ID #: 145-8)]	Noted. We decided against thiks change.
10-250	A	5:44	4:44	I think a more accurate version of the opening sentence would be "Those models reasonably consistent with present day observations project a range of responses of the Atlantic Meridional Circulation from no change to a reduction of 60% by 2100." [Matthew Collins (Reviewer's comment ID #: 44-27)]	Accepted.
10-251	A	5:46	5:46	Insert after "still "a possible" [VINCENT GRAY (Reviewer's comment ID #: 88-1514)]	Rejected. No reason given to support request.
10-252	A	5:46		"Over the North Atlantic" should be "around the North Atlantic", see Chapter 10, p70, line 10 and p24, line 41. [Gerrit Burgers (Reviewer's comment ID #: 34-14)]	Accepted
10-253	A	5:47	5:47	Delete "much larger"	Rejected. No reason given to support

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[VINCENT GRAY (Reviewer's comment ID #: 88-1515)]	request.
10-254	A	5:47	5:48	The sentence" Although the MOC weakens in most models run for the three SRES scenarios, none shows a collapse of the MOC by the year 2100" needs to have included a qualifier to the effect that this only for runs that do not include freshwater fluxed from melting of the Greenland ice sheet or from increased freshwater input from Artic rivers (see eg Schaeffer, M., F. M. Selten, J. D. Opsteegh, and H. Goosse (2002). "Intrinsic limits to predictability of abrupt regional climate change in IPCCSRES scenarios." Geophysical Research Letters 29(16)) [William Hare (Reviewer's comment ID #: 99-48)]	Rejected. This is not relevant to our analysis. We address greenland melting in main body of text.
10-255	A	5:48		Add after "… none shows a collapse of the MOC by the year 2100". : ", but it should be kept in mind that these models do not include the effect of meltwater runoff from Greenland." [Govt. of Germany (Reviewer's comment ID #: 2011-61)]	Rejected. See reply to next item
10-256	A	5:48		"… none shows a collapse of the MOC by the year 2100". Add: ", but it should be kept in mind that these models do not include the effect of meltwater runoff from Greenland." [Stefan Rahmstorf (Reviewer's comment ID #: 206-10)]	Rejected. This is not relevant to our analysis. We address greenland melting in the main body of the text.
10-257	A	5:50	5:50	Given that the coupled models largely omit contribution from melting of Greenland, and underestimate the sea-level rise generally, the assessment of "very unlikely" seems overly confident to me; "unlikely" would better reflect the uncertainties and model shortcomings. [Richard B. Alley (Reviewer's comment ID #: 4-12)]	Rejected: We agreed in Christchurch that the very unlikely assessment (representing < 10% probability) was appropriate.
10-258	A	5:50	5:52	The sentences "It is very unlikely that the MOC will undergo a large abrupt transition during the course of the 21st century. At this stage it is too early to assess the likelihood of a large abrupt change of the MOC beyond the end of the 21st century" do not appear to have a good basis for making such a strong statement "very unlikely". I would suggest a re-word that is like the following: "Whilst it appears unlikely that the MOC will undergo a large abrupt transition during the course of the 21st century at this stage it is too early to assess quantitatively the likelihood of a large abrupt change of the MOC." I agree that an abrupt change in the MOC is unlikely however "very" is not justified by the science as I understand it. In addition the range of projected freshwater losses from the GIS bring the freshwater fluxes into the range during this century that triggers abrupt changes in some models, hence delete reference to 21st century. [William Hare (Reviewer's comment ID #: 99-49)]	Rejected: Very unlikely means < 10% probability.
10-259	A	5:50		"It is very unlikely.." There is no basis for making such a high-confidence statement - see my general comment on the whole chapter. An "abrupt transition" would include not just a complete collapse but phenomena like e.g. found in the simulation of Schaeffer et al.: Schaeffer, M., F. M. Selten, J. D. Opsteegh, and H. Goosse (2002), Intrinsic limits to predictability of abrupt regional climate change in IPCC SRES scenarios, Geophys. Res.	Rejected: Very unlikely means < 10% probability.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Let., 29, art. no.-1767. If some models do show transitions like this for SRES scenarios before 2100, one cannot say they are "very unlikely". [Stefan Rahmstorf (Reviewer's comment ID #: 206-11)]	
10-260	A	5:50		replace "very" by "probably", and add "but the possibility cannot be excluded" after "the end of the 21st century" (two lines down) [Govt. of United Kingdom (Reviewer's comment ID #: 2022-36)]	Rejected: Very unlikely means < 10% probability.
10-261	A	5:51	5:54	I have similar concerns here to those expressed for the discussion of sea level rise in the chapter. Namely, that probabilities and risks of a THC collapse are determined in a narrow sense, from a specific scenario or two, and based on a particular set of modeling experiments. Yet, there should still be some caution in the reliability we attribute to the existing model predictions, given that we do not entirely understand some of the key underlying processes. For example, there are some engimatic findings in the Bryden et al (2005) Nature paper that call into question our understanding of the relationship between the MOC and gyre-scale circulations in the North Atlantic, and there are still questions about precisely which processes control mixing in the deep ocean. Therefore one should discuss the risk of ocean circulation changes based not just on a set of model simulations and scenarios, but based on a more comprehensive assessment of observed and paleo-reconstructed changes which provide a broader context for assessing our true understanding. [Michael Mann (Reviewer's comment ID #: 156-82)]	Rejected: Very unlikely means < 10% probability.
10-262	A	5:54	5:54	Add "after 2100" to end of sentence. [Ronald J Stouffer (Reviewer's comment ID #: 258-114)]	Accepted.
10-263	A	6:1	6:4	This summary of the radiative forcing section rather sticks out like a sore thumb, radiative forcing not being as much of a policy-relevant variable than other variables discussed (I also note this section uses a different definition of radiative forcing to the rest of the report). Would it be possible to link this summary to the discussion of uncertainties? [Matthew Collins (Reviewer's comment ID #: 44-28)]	ACCEPTED – the summary now links uncertainty in forcing (due to radiative parameterizations) to the range of projections from the multi-model ensemble.
10-264	A	6:2	6:2	Recall the existence of lines in the spectrum of the GHG molecules and the physical processes involved in evaluating the radiative forcing for a specific atmospheric concentration. [Govt. of France (Reviewer's comment ID #: 2010-73)]	NOTED.
10-265	A	6:3	6:4	It would be very much appreciated to inform about the consequences of such bias for the temperature projections (if any) by AOGCMs. [Govt. of Austria (Reviewer's comment ID #: 2002-53)]	See comment 10-263.
10-266	A	6:3	6:4	Explain the reasons for this mismatch and what its effects are on projections. [European Commission (Reviewer's comment ID #: 2008-35)]	TAKEN INTO ACCOUNT – the summary points now focus exclusively

No.	Batch	Page:line From	Page:line To	Comment	Notes
					on the forcing at the tropopause.
10-267	A	6:3	6:4	CONTENT: Amend the sentence "… but AOGCMs significantly overestimate the surface forcing by about 0.5W/m2 at the surface." by the possible reasons for this mismatch as well as the possible implications for what happens, if this mismatch were corrected. Does this point towards a possibly underestimated / overestimated climate sensitivity/TCR in the current AR-4 models? Is it mainly caused by the omission of shortwave methane forcing (see Chapter 2)? [Govt. of Germany (Reviewer's comment ID #: 2011-146)]	See comment 10-266.
10-268	A	6:3	6:4	Does this apply to all models? How important is the 0.5 w/m**2? What is your assessment? How does this impact the ocean heat uptake assessments? [Ronald J Stouffer (Reviewer's comment ID #: 258-115)]	See comment 10-266.
10-269	A	6:4		What does an overestimate of the surface rad forcing by models imply - could it be briefly explained what this would lead to, [gabi hegerl (Reviewer's comment ID #: 103-20)]	See comment 10-266.
10-270	A	6:10	6:10	Phrase "at any given point in time" is either too loose or confusing. Should it read ".....indicate that at either of these nominal points in time to achieve stabilisation we are committed to…." [Govt. of Australia (Reviewer's comment ID #: 2001-390)]	This phrase has been deleted.
10-271	A	6:11	6:11	Replace "occurs" with "is projected to occur" [VINCENT GRAY (Reviewer's comment ID #: 88-1516)]	This is related to what the models actually show in terms of mechanism, not a projection per se, so the wording is correct in that context.
10-272	A	6:14	6:15	A timescale is needed on the comment that "If GHG concentrations could be reduced, global temperatures would decrease." [Govt. of Australia (Reviewer's comment ID #: 2001-391)]	Accepted
10-273	A	6:14	6:15	The second draft of Chapter 10 of AR4 sounds good because "new findings about the dangerous anthropogenic interference with climate system" are described in detail, which are useful information for discussion of the long-term target of GHG emission reduction. In addition, I strongly recommend that a possibility of prosperous future should be described from the viewpoint of scientific aspect for encouragement of pessimistic people about prevention of global warming. Therefore, I recommend the following sentence enough for the pessimistic people to be encouraged; " If GHG concentrations in the atmosphere could be reduced with application of many advanced mitigation options, the induced climate change, such as temperature, precipitation, sea ice and MOC, would be mitigated except some features with hysteresis effect, such as sea level change due to thermal expansion."	Accepted. We now add information about sea level rise, and the time scales of response of temperature and sea level, but we choose to not add information on mitigation options since the experiments we are assessing assume only idealized forcing changes.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Koki Maruyama (Reviewer's comment ID #: 169-1)]	
10-274	A	6:15	6:15	Replace "would" with "might" [VINCENT GRAY (Reviewer's comment ID #: 88-1517)]	This describes a model result and is the correct wording for that result.
10-275	A	6:16	6:16	Replace "will" by "may" [VINCENT GRAY (Reviewer's comment ID #: 88-1518)]	This describes a model result and "will" is now replaced more appropriately by "would"
10-276	A	6:17	6:17	Replace "will" by "may" [VINCENT GRAY (Reviewer's comment ID #: 88-1519)]	This describes a model result and "will" is now replaced more appropriately by "would"
10-277	A	6:19	6:19	Sea level or sea-level rise? [European Commission (Reviewer's comment ID #: 2008-36)]	Accepted and clarified.
10-278	A	6:19	6:19	CONTENT: Clarify whether sea level (mm) is "greater than in the 21st century" or whether - as currently stated - sea level RISE (mm/yr) is indeed greater. [Govt. of Germany (Reviewer's comment ID #: 2011-147)]	Accepted. It meant amount rather than rate.
10-279	A	6:19	6:19	Replace "With concnetrations" by "If concentrations are" [VINCENT GRAY (Reviewer's comment ID #: 88-1520)]	Accepted.
10-280	A	6:20	6:20	Phrase "a few 0.1mm per century" is hard to understand - and read literally seems a trivial increase in sea level. [Govt. of Australia (Reviewer's comment ID #: 2001-392)]	Rejected. It is metres, not millimetres.
10-281	A	6:20	6:20	Replace "is" by "could be" [VINCENT GRAY (Reviewer's comment ID #: 88-1521)]	Rejected. This is a statement about what is projected to happen under the scenario just stated in the text.
10-282	A	6:21		replace "reducing" by "which decreases" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-37)]	Accepted and clarified.
10-283	A	6:22	6:23	The comment that "some glacier volume may persist at high altitudes" is too vague. This should probably read "some low latitude glacial volume..." or similar. [Govt. of Australia (Reviewer's comment ID #: 2001-393)]	Rejected. We can't say in which regions it would survive – it depends on both altitude and latitude as well as regional climate change.
10-284	A	6:22	6:23	"..but most could disappear over centuries." – Is it possible to be more quantitative than this rather vague statement? [Martin Manning (Reviewer's comment ID #: 155-72)]	Rejected. Unfortunately not, since not much work has been done on this yet.
10-285	A	6:22	8:22	Replace "Under" by "For a projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1522)]	Rejected. This phrase defines the temperature scenario being considered.
10-286	A	6:22		replace "could" by "would probably" or "are likely to" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-38)]	Rejected. It must depend on how much warming there is, but we don't yet have quantitative projections for this.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-287	A	6:25	6:30	Need some sense of how long it would take for Greenland ice sheet to be eliminated under a 3.1 degree C rise. First sentence is confusing in referring to 3.1 degree C in line 25 and then to 3 degree C in line 28. Could the sentence stop in line 27 after "century"? [Govt. of Australia (Reviewer's comment ID #: 2001-394)]	Accepted need for clarification of thresholds. We cannot give rates for many centuries into the future because the ice sheet evolution needs to be coupled to the climate.
10-288	A	6:25	6:25	Is the uncertainty estimate ±1 std or ±2 std? [Govt. of Finland (Reviewer's comment ID #: 2009-124)]	Taken into account. It was 2 sd but all are being changed to 5-95% ranges.
10-289	A	6:25	6:25	PRESENTATION: These sections on sea level rise seem not to have a direct link with the analyzed commitment scenarios as described at the beginning of the section (page 6, line 8). Thus, shift these two paragraphs to the sea level rise subheading, page 4, line 44. [Govt. of Germany (Reviewer's comment ID #: 2011-148)]	Rejected. The thermal expansion rates do relate to the commitment scenarios, as has now been clarified. We wish to keep the long-term consequences of climate change in one paragraph.
10-290	A	6:25	6:25	Replace "With" by "If" [VINCENT GRAY (Reviewer's comment ID #: 88-1523)]	Accepted.
10-291	A	6:25	6:25	Replace "maintained" by "were to be" [VINCENT GRAY (Reviewer's comment ID #: 88-1524)]	Accepted.
10-292	A	6:25	6:28	Add (see Figure 10.7.7) "This requires over 1000 years' sustained temperature anomaly of this magnitude". [Patrick Michaels (Reviewer's comment ID #: 176-24)]	Accepted.
10-293	A	6:25	6:25	Add "for centuries to millenia" after "warming maintained". [Ronald J Stouffer (Reviewer's comment ID #: 258-116)]	Accepted.
10-294	A	6:26	6:26	Replace"would" by "could conceivably" [VINCENT GRAY (Reviewer's comment ID #: 88-1525)]	Rejected. This is qualitatively a robust conclusion of several model studies, and the uncertainty is quantified by the range given.
10-295	A	6:27	6:27	Insert after "mountains" "possibly" [VINCENT GRAY (Reviewer's comment ID #: 88-1526)]	Rejected. If the ice sheet were removed, that is how much sea level would rise.
10-296	A	6:27	6:27	Insert before "global" "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1527)]	Rejected as unnecessary. This is obviously a projection.
10-297	A	6:27	6:27	COMMENT: This phrase gives the misunderstanding that "3°C" are the threshold of Greenland Ice Sheet melting. Additional example and/or information are required. REASON: Chapter 10.7.4.3 description is "For a global warming of 3°C relative to present, models suggest Greenland would contribute 0.2-3.9mm/yr to sea level." Keep consistency with this Chapter. RECOMMEND: replace the phrase "initially at a rate of up to 0.4m per century." with "initially at a range of rate 0.02-0.39m per century".	Taken into account by clarifying the thresholds and rates.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Govt. of Japan (Reviewer's comment ID #: 2014-41)]	
10-298	A	6:28	6:28	Replace"would" by "could conceivably" [VINCENT GRAY (Reviewer's comment ID #: 88-1528)]	Rejected. No reason given for this change. "Would" is already conditional.
10-299	A	6:29	6:29	Replace "medium" by "a remote" [VINCENT GRAY (Reviewer's comment ID #: 88-1529)]	Rejected. The works cited in the chapter give some support for this possibility, but are equivocal.
10-300	A	6:29	6:30	"…medium likelihood that it could not" seems a rather strange statement. Do you mean "… medium likelihood that it would not"? [Martin Manning (Reviewer's comment ID #: 155-73)]	Accepted.
10-301	A	6:29	6:30	Two comments on the sentence that spans these two lines. Firstly, "even if climate were subsequently returned to the pre-industrial" should be changed to "even if emissions were returned to the pre-industrial". If the Greenland Ice Sheet cannot be regenerated, climate cannot be returned to its pre-industrial state. Secondly, does the statement that there is a "medium likelihood it could not be regenerated" apply even if orbital parameters change so as to give conditions that would be expected to trigger the next ice age? [Adrian Simmons (Reviewer's comment ID #: 242-140)]	Taken into account. It means preindustrial composition. The reviewers is right that this would not give exactly the same climate, but in fact the climate outside Greenland is mostly unaffected by loss of the ice sheet. If there were another glacial (not preindustrial composition) the ice sheet presumably could be regenerated.
10-302	A	6:32	6:32	Replace" "will" by ":might" [VINCENT GRAY (Reviewer's comment ID #: 88-1530)]	Rejected. Since it speaks of GCMs, it must be a projection, and the models all agree on this.
10-303	A	6:32	6:39	This paragraph does not reflect the science very well as it does not balance the certainties of the processes discussed very well and hence the message that comes through is that it is more likely that for a 3oC global warming the AIS will grow than decrease in mass eg 1) "GCMs indicate that ... will cause a negative contribution to sea level---". This is too categorical a statement and its needs to be coupled with the other terms in the equation to avoid a biased impression of the state of the science. 2) "However,...especially if the major ice shelves were weakened". The major ice shelves seem likely to be weakened by surface melting (warming within the range of this for the Ross Ice Shelf by 2080s or earlier in some models) or basal melting (Williams et al 2002) 3) "In the absence of models for the relevant processes, there is little agreement about what dynamical changes could occur" Yes and no: loss of ice shelves does translate into faster loss of ice from discharge ice streams (eg Warner, R. C. and W. F. Budd (1998)) 4) ".... Alternatively, rapid discharge may be transient and insufficient to outweigh the increased snow accumulation." Where is the evidence for this and is it strong enough to justify inclusion here? [William Hare (Reviewer's comment ID #: 99-50)]	Taken into account. Such modeling studies as have been done and are cited in the chapter suggest that the increased flow velocities may be a transient phenomenon following removal of ice shelves.

No.	Batch	Page:line From	To	Comment	Notes
10-304	A	6:32	6:39	Suggested rewording of para. like: "The long term stability of the Antarctic ice sheet is likely to be determined by the effects of global warming on accumulation of snow, the stability of ice shelves and the dynamical effects the removal of these would have on ice streams, and last on melting over the margins of the continent and the effect this has on the dynamical behaviour of glaciers. GCMs indicate that greater warming is very likely to lead to an increase accumulation of snow on the Antarctic ice sheet relative to present-day. For a warming of a few oC above preindustrial this is likely to be counteracted by increased ice discharge into the ocean, especially if the major ice shelves were weakened. Ocean warming is projected to very likely negatively affect important ice shelves. In the absence of models for the relevant processes, there is little agreement about what dynamical changes could occur. Sustained global warming of 3oC or more has been found in model assessments to lead to a decay of the ice sheet. By analogy with past climate changes, sustained global warming of 2°C has been suggested as a threshold beyond which there will be a commitment to large sea-level contribution from the WAIS, with maximum rates of several mm yr–1." [William Hare (Reviewer's comment ID #: 99-51)]	Taken into account.
10-305	A	6:37	6:37	Replace "will" by "could" [VINCENT GRAY (Reviewer's comment ID #: 88-1531)]	Rejected. No need for this as the statement is already hypothetical.
10-306	A	7:1		See comment above. The first paragraph seems to imply that the goal of this chapter is to present projections from non-climate policy emission scenarios only. This would be misleading. [European Commission (Reviewer's comment ID #: 2008-37)]	ACCEPTED: scenario range now clearly stated.
10-307	A	7:1		Introduction. CONTENT: The first sentence states that the "this chapter addresses various aspects regarding projections of future climate change." Further down, the SRES non-mitigation scenarios B1, A1B and A2 are introduced as "low", "medium" and "high" scenarios without any mentioning that these scenarios are non-mitigation scenarios only. Clearly the fact that this Chapter is labeled "Global Climate Projections" but in fact deals in most cases only with non-mitigation scenarios is not satisfactory. Every possible effort should be undertaken to extrapolate the presented AOGCM results of A1B, A2, and B1 to plausible mitigation cases, just as these results were extrapolated to the other illustrative SRES scenarios by using, e.g., simple model tunings to AOGCMs. Here in the introduction however, a disclaimer is needed towards the beginning in order to state this important constraint in regard to the actual scope of this chapter, in case that mitigation scenarios are not properly addressed in the final version. If this shortcoming - not to properly analyze mitigation scenarios - is not rectified in the next and final draft, the reader has to be made aware in the beginning of the chapter, that this projection chapter deals primarily with non-mitigation scenarios only.	ACCEPTED: non-mitigation now mentioned. Also, we state explicitly those scenarios which imply some form of mitigation/intervention.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Govt. of Germany (Reviewer's comment ID #: 2011-149)]	
10-308	A	7:5	7:5	Add at end ""It should always be remembered that no model has ever been properly validated, in terms of its being shown capoable of reliable future predictuion, and the scenarios are all merely projections based on particular assumptions, some of which are improbable. The results given here are therefore speculative projections and not predictions" [VINCENT GRAY (Reviewer's comment ID #: 88-1532)]	REJECTED: statement wrong.
10-309	A	7:7	7:7	Indicate when undertaken, eg since the TAR [Govt. of Australia (Reviewer's comment ID #: 2001-395)]	ACCEPTED: statement added
10-310	A	7:7	7:13	It would be helpful to the reader to have illustrative diagrams of what the time paths of emissions and concentrations look like. The present description is hard to comprehend for a policymaker. [Govt. of Australia (Reviewer's comment ID #: 2001-396)]	REJECTED: space limitation
10-311	A	7:10	7:10	Insert after ":increase" "(itself a most improbable figure, as the current rate is only 0.4% a year)" [VINCENT GRAY (Reviewer's comment ID #: 88-1533)]	REJECTED: statement wrong. Current increase is 0.67%/yr. Rate is variable. p7, line 9 says "idealised".
10-312	A	7:11		Somewhere in the text, you need to add what Covey (2003) with regard to the 1%/year increase used in the CMIP studies: "It is also not a good estimate of future anthropogenic climate forcing, except perhaps as an extreme case in which the world accelerates its consumption of fissil fuels while reducing its production of anthropogenic aerosols". You don't have to quote directly, but obviously this deserves to be mentioned. [Patrick Michaels (Reviewer's comment ID #: 176-25)]	REJECTED: these are idealised scenarios used in the literature. We assess the response of climate models not scenario choices.
10-313	A	7:12	7:16	The intercomparison of AOGCM results to understand response differences among the models are why the idealized CO2 increase integrations are important. Reword. [Ronald J Stouffer (Reviewer's comment ID #: 258-117)]	ACCEPTED: wording added
10-314	A	7:14	7:14	It would help reader to begin a new para at line 14. [Govt. of Australia (Reviewer's comment ID #: 2001-397)]	ACCEPTED
10-315	A	7:14	7:14	Insert after "experiments" " (itself a most improbable figure, as the current rate is only 0.4% a year)" [VINCENT GRAY (Reviewer's comment ID #: 88-1534)]	REJECTED: statement wrong. Current increase is 0.67%/yr. Rate is variable. p7, line 9 says "idealised".
10-316	A	7:16	7:16	Para break probably needed to communicate that moving on to discussion of a second class of these experiments. [Govt. of Australia (Reviewer's comment ID #: 2001-398)]	ACCEPTED
10-317	A	7:17	7:17	Replace "anthropogenic" by "human-induced"	REJECTED

No.	Batch	Page:line From	To	Comment	Notes
				[VINCENT GRAY (Reviewer's comment ID #: 88-1535)]	
10-318	A	7:18	7:18	Add at beginning "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1536)]	ACCEPTED
10-319	A	7:19	7:20	Change order of shortcut scenarios for "(B1, A1B and A2)" instead "(A1B, B1and A2)" because B1 is "low" scenario. [Valentin Meleshko (Reviewer's comment ID #: 175-1)]	ACCEPTED
10-320	A	7:20	7:20	It needs to be emphasized that these are non mitigation scenario and hence are not absolutely low as is implied here [William Hare (Reviewer's comment ID #: 99-52)]	ACCEPTED: para added
10-321	A	7:22	7:22	Insert after "scenarios" "It might be mentioned that scenario A2 is highly improbable" [VINCENT GRAY (Reviewer's comment ID #: 88-1537)]	REJECTED: no assessment of scenarios
10-322	A	7:23	7:23	Is 'remit' the correct word? Suggest change to 'scope'. [Govt. of Australia (Reviewer's comment ID #: 2001-399)]	ACCEPTED
10-323	A	7:23		I don't understand the use of "By the same argument". What argument? Also it should be made clear that "this report" refers to the WG1 contribution to the AR4 not the full assessment report. So I suggest for the sentence beginning on line 23: "Also it is not within the remit of the Working Group I contribution to the Fourth Assessment Report to assess the plausibility or likelihood of emission scenarios." [Martin Manning (Reviewer's comment ID #: 155-74)]	ACCEPTED
10-324	A	7:23		Delete "By the same argument" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-39)]	reworded
10-325	A	7:24	7:24	Add at end "even if it is sometimes obvious" [VINCENT GRAY (Reviewer's comment ID #: 88-1538)]	REJECTED
10-326	A	7:26	7:26	Results … begin in year 2000 - Is not clear what this means. It is the start of the integrations and analysis (with the caveats noted later in the text). [Ronald J Stouffer (Reviewer's comment ID #: 258-118)]	REJECTED
10-327	A	7:29	7:29	Insert before "forcing" "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1539)]	REJECTED
10-328	A	7:32	7:32	Insert after "warming" "shown by the contaminated surface record" [VINCENT GRAY (Reviewer's comment ID #: 88-1540)]	REJECTED: not clear what contaminated means
10-329	A	7:43		I think it should be mentioned that an average over models does not have a dynamical justification, in the sense that there are no scientifically based equations that underlie the averaged outcome. [Anders Levermann (Reviewer's comment ID #: 145-9)]	REJECTED: following sentence clarifies meaning
10-330	A	7:45	7:46	note that the statement taht "individual model biases tend to cancel" is quite dangerous because not really demonstrated for this range of projection where external drivers might	REJECTED: "empirically" and "tend to" should be sufficient qualifiers of

No.	Batch	Page:line From	Page:line To	Comment	Notes
				push some divergence in this system. [Pascale DELECLUSE (Reviewer's comment ID #: 58-60)]	this statement.
10-331	A	7:48	7:49	I like "sampling protocol" as a term but I think it is not quite right to say "unable to span the full range of uncertainty" which, one may argue, is infinite. How about "the spread is not neccesarily representative of that which would be found in an ensemble desinged to sample a wide range of uncertainties". [Matthew Collins (Reviewer's comment ID #: 44-29)]	ACCEPTED: reworded
10-332	A	7:48	7:49	Is there an entry threshold that has to be met for a model's results to be included in the "ensemble of opportunity"? If not, is there not the possibility that admission of a rogue model that could be demonstrated to be unreliable would result in a span of the ensemble that exceeds the real uncertainty? [Adrian Simmons (Reviewer's comment ID #: 242-142)]	PARTLY ACCEPTED: reworded
10-333	A	7:53	7:54	The statement "Uncertainties derived from…the multi-model ensemble are…consistent with estimates from other methods" Is simply not true. Estimates based on observed 20th century changes all give 90% confidence intervals which extend to much higher sensitivities than any of the models (see table 9.6.1 and fig. 9.6.1.) In fact none of the models' sensitivities fall outside the newly adapted range of 2.0.to 4.5 C, in spite of the fact that this interval is only said to be "likely", i.e., there is a 10 to 34 % chance that the sensitivity is actually outside the range covered by the models. The present statement is also contradicted by the earlier statement in the same paragraph: "...the spread of model(s) is unable to span the full possible range of uncertainty...". The sentence starting on line 53 should be deleted. [Peter Stone (Reviewer's comment ID #: 257-8)]	ACCEPTED: sentence deleted.
10-334	A	7:53	:54	When you say estimates from ensembles of opportunity yield consistent estimates as those from "other methods" (are that the probabilistic ones?) what does this mean? The 95% range of the probabilistic estimates is probably wider, right? Can this be clarified (I know this is asking for lots, but it might avoid a bit of confusion) [gabi hegerl (Reviewer's comment ID #: 103-21)]	no longer applies: sentence deleted
10-335	A	8:5	8:5	Insert before "global" "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1541)]	REJECTED: "estimates" is stated
10-336	A	8:11	8:11	Insert before "CO2" "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1542)]	REJECTED: context clear
10-337	A	8:12	8:13	did you mean to mention "ocean" AND "marine" biospheres? Are they different? [Chris Jones (Reviewer's comment ID #: 120-20)]	REJECTED: there is an inorganic and an organic part of the ocean carbon cycle.
10-338	A	8:17	8:18	To say that "some of the probability estimates no longer rely solely on expert judgment" is technically correct but somewhat misleadling. Expert judgments are an essetential	NOTED: valid comment. Wording actually still includes "expert

No.	Batch	Page:line From	To	Comment	Notes
				component of all of the PDFs calculated using models and observations. Thus while the probability estimates from such approaches are not the same as experts drawing PDFs with their pencils, they are inseparable from a wide variety of the assumptions made in setting up the models and PDF calculations ("priors" in a broad sense). [Paul Baer (Reviewer's comment ID #: 10-5)]	judgement" -> "no longer rely solely"
10-339	A	8:18	8:18	Replace "judgement" by "guesswork" [VINCENT GRAY (Reviewer's comment ID #: 88-1543)]	REJECTED: offensive suggestion
10-340	A	8:19	8:19	"This gives us a much more complete understanding of model response uncertainties...": "Understanding" is certainly not the right word. "Assessment" might be better. [Sandrine Bony (Reviewer's comment ID #: 25-3)]	ACCEPTED: reworded
10-341	A	8:26	8:26	Add at end "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1544)]	REJECTED: "estimates" is stated
10-342	A	8:33	8:33	Insert before "results "prohected" [VINCENT GRAY (Reviewer's comment ID #: 88-1545)]	REJECTED: these are results indeed
10-343	A	8:46	8:47	Discuss the uncertainties associated with the evaluation of the radiative forcing for a specific atmospheric concentration. Expand on the benchmark quoted page 6 line 2, in a brief summary of the radiative transfert physics. Possibility of coupling between GHGs warming effect through possible absorption lines overlapping ? [Govt. of France (Reviewer's comment ID #: 2010-74)]	Covered in section 10.2. Tutorial material found in text books not appropriate here.
10-344	A	8:53	8:55	Figure 10.1.1: Given the importance of climate feedback on carbon concentrations (and in this chapter), should not Fig 10.1.1 include an arrow to represent this? [Govt. of Australia (Reviewer's comment ID #: 2001-400)]	ACCEPTED: Figure modified
10-345	A	8:56	8:57	This statement is too "negative". Replace with "the array of uncertainty suggests that specifying quantitative ranges of possible future climate change are difficult". [Govt. of Australia (Reviewer's comment ID #: 2001-401)]	ACCEPTED: statement deleted
10-346	A	8:56	8:56	Replace "suggests" by "shows" [VINCENT GRAY (Reviewer's comment ID #: 88-1546)]	REJECT: no longer applies
10-347	A	8:56	8:57	Suggest delete first sentence and "However,". This sentence is not true, and I don't think that modifying it to make it true would be helpful. [Jonathan Rougier (Reviewer's comment ID #: 221-2)]	REJECT: no longer applies
10-348	A	8:56	:57	This statement is very unhelpful and is probably a hostage to fortune. Replace it by something concrete & positive like "Despite these multiple sources of uncertainty it is still possible to…etc etc" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-40)]	REJECT: no longer applies
10-349	A	8:57	8:57	Replace "climate change" with "changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1547)]	REJECT: no longer applies

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-350	A	8:57		{NB: Here & elsewhere} The role of multi-model ensembles needs to be handled with care, and more clearly explained. They do provide some limited benefit w.r.t. model error, but they do not produce formal probability estimates, and they do nothing w.r.t. other sources of error. The chapter is in general rather self-satisfied & seems to be a bit complacent about the use of multi-model ensembles, which are certainly a step forward, and desirable & useful, but are by no means sufficient... [Govt. of United Kingdom (Reviewer's comment ID #: 2022-41)]	REJECT: we say it "improved". Formulation does not suggest final projections
10-351	A	9:3	9:3	Replace "climate change" with "changes in the climate"; Twice [VINCENT GRAY (Reviewer's comment ID #: 88-1548)]	REJECT: no reason given
10-352	A	9:9		Hear hear. This chapter should use information from models AND observations in its assessment of possible future changes - this is often not the case, e.g. when it comes to sea level rise or MOC changes. [Stefan Rahmstorf (Reviewer's comment ID #: 206-12)]	NOTED: we assess what is in the literature. Observed MOC changes are very controversial and cannot, at this stage, be used as a constraint.
10-353	A	9:12	9:12	PRESENTATION: In line with the introduction figure 10.1.1, the (uncertainties in) emission scenarios should be dealt with prior to discussing radiative forcing. Clearly, the sections as 10.2.1.1 "SRES scenarios" are not properly categorized under a "radiative forcing" section 10.2 (as the SRES scenarios are emission scenarios, not forcing scenarios) and need to be lifted into a new "emission" section. This is to increase clarity for the reader and follow the logic of figure 10.1.1 to separate the different uncertainties in regard to future emissions and radiative forcing implications. [Govt. of Germany (Reviewer's comment ID #: 2011-150)]	ACCEPTED – The section has been renamed, and the fact that the emissions and concentrationsare illustrated in Figures 10.1.1 and Figure 10.5.2. is noted in the text here.
10-354	A	9:12		Section 10.2 does not present the main SRES scenarios to the extent anticipated elsewhere. An explanation of the central components of the SRES and that they are 'non-mitigation' is necessary. In the TAR the SRES was discussed explicitly and this should also be the case for the AR4. [Govt. of Australia (Reviewer's comment ID #: 2001-402)]	See comment 10-353.
10-355	A	9:15	9:16	The simulations of the 19th and 20th centuries are based upon changes in LLGHGs that are reasonably constrained by the observational record. "So models have qualitatively similar time-evolutions of their RF time-histories (Ex. see Figure 2.26)." However, estimates of future concentrations of LLGHGs ... are clearly subject to significant uncertainties. [Yoko Tsushima (Reviewer's comment ID #: 269-5)]	ACCEPTED, but for LLGHGs only – there are substantial differences among the time histories for non-sulfate aerosol species.
10-356	A	9:19	9:23	Again, this only refers to scenarios that do not consider climate policy. [European Commission (Reviewer's comment ID #: 2008-38)]	ACCEPTED – This is now addressed in the introduction (section 10.1).
10-357	A	9:19	9:23	CONTENT: The sentence "For these reasons, a range of projections for future climate change has been conducted using coupled AOGCMs" is misleading, as only non-mitigation scenarios were assessed with AOGCMs plus some constant-radiative forcing	ACCEPTED – This is now addressed in the introduction (section 10.1).

No.	Batch	Page:line From	To	Comment	Notes
				commitment scenarios. Please clarify this issue for the reader, by mentioning explicitly that only non-mitigation scenarios were assessed in addition to a theoretical "constant composition/current radiative forcing" scenario. The same applies to the subsequent sentence, where it is implied that B1, A1B and A2 are presenting low, medium and high emission trajectories. Please specify that this statement is only valid for the subset of non-mitigation scenarios. [Govt. of Germany (Reviewer's comment ID #: 2011-151)]	
10-358	A	9:19	9:22	Many other aerosol types are used in the AR4 models. Need to mention. This is an advance since the TAR…the aerosols are more than just the sulphates as noted later in the text. [Ronald J Stouffer (Reviewer's comment ID #: 258-119)]	ACCEPTED
10-359	A	9:20	9:20	Change end of sentence to "-- coupled AOGCMs forced by non-mitigation emission scenarios" [William Hare (Reviewer's comment ID #: 99-53)]	ACCEPTED – This is now addressed in the introduction (section 10.1).
10-360	A	9:36	9:36	Replace "predicted" by "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1549)]	ACCEPTED
10-361	A	9:43	9:45	This is unclear. Would this require hypothetical scenarios without emissions or with a level of emissions that would keep radiative forcing constant? [European Commission (Reviewer's comment ID #: 2008-39)]	ACCEPTED – An additional explanatory sentence has been added noting that concentrations are held fixed.
10-362	A	9:43	9:44	CONTENT: The statement "… and a new idealized scenario designed to quantify committed climate change due to historical emissions" is incorrect, as the assessed commitment scenarios are "constant composition / current radiative forcing" scenarios, not "zero-emission" scenarios. This has to be corrected by stating "and a new idealized scenario that assumes future radiative forcing in the future to be held constant at present levels. This theoretical 'constant composition' scenario would materialize, if e.g. greenhouse gas emissions were reduced by more than 30% to ~80%, depending on atmospheric residence times of the individual greenhouse gases (cf. Question 10.3. Figure 1). The effect of historic emissions on the climate is less than what indicated by the commitment scenarios, as a complete 100% reduction of all emission would decrease radiative forcing levels below present levels in the long-term (cf. Wigley 2005 or Hare & Meinshausen 2006)." [Govt. of Germany (Reviewer's comment ID #: 2011-152)]	ACCEPTED – For the first part of the suggested revision.
10-363	A	9:43	9:43	This sentence is incorrect as it says "a new idealized scenario designed to quantify committed climate change due to historical emissions" when the scenario is to hold forcing constant at year 2000 levels, which implies emissions continue after 2000 and hence the warming committed is not due solely to historical emissions (see Hare and	SEE COMMENT 10-362.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Meinshausen, 2006 for a discussion of these issues) [William Hare (Reviewer's comment ID #: 99-54)]	
10-364	A	9:44	9:44	Replace "predicted" by "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1550)]	ACCEPTED
10-365	A	9:47	9:50	This seems a good place to note that the stabilization scenarios are un-realistic in a number of ways. One of the important that they are not realistic is that they break the connection between the emission of GHG and aerosols. [Ronald J Stouffer (Reviewer's comment ID #: 258-120)]	REJECTED – The common policy for all scenarios is to avoid characterization of their physical or socioeconomic realism.
10-366	A	9:48	9:48	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1551)]	REJECTED -- NO REASON FOR CHANGE GIVEN.
10-367	A	9:49	9:49	Add "constant forcing" before "commitment" so that this is described accurately [William Hare (Reviewer's comment ID #: 99-55)]	ACCEPTED
10-368	A	9:50	9:50	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1552)]	REJECTED -- NO REASON FOR CHANGE GIVEN.
10-369	A	9:54	9:54	CONTENT: Change "SRES scenarios" to "non-mitigation SRES emission scenarios" for clarity. [Govt. of Germany (Reviewer's comment ID #: 2011-153)]	ACCEPTED – This is now addressed in the introduction (section 10.1).
10-370	A	9:54	9:54	Add "non-mitigation" before "SRES" [William Hare (Reviewer's comment ID #: 99-56)]	ACCEPTED – This is now addressed in the introduction (section 10.1).
10-371	A	10:0		Table 10.2.1: Entries "ENSEMBLE", "BP", "MRI", "GISS", "CIESEN", "NCAR" and "COVEY" need definitions. Some may be replacable by a simple "YES". Suggest also an additional entry under the columns CH4, N2O and CFCs of the form "YES (EQUIV)" or similar for those simulations where these well-mixed greenhouse gases are included as an equivalent CO2. [Anthony Hirst (Reviewer's comment ID #: 107-1)]	ACCEPTED.
10-372	A	10:0		Table 10.2.1: This table needs completion. In the case of the CSIRO-MK3.0, the entries are CO2 - YES; CH4 - YES (as equivalent CO2); N2O - YES (as equivalent CO2); Strat O3 - YES; Trop O3 - YES; CFCs - YES (as equivalent CO2); SO4 - YES; Land Use - const; Solar - const.; all others are blank. [Anthony Hirst (Reviewer's comment ID #: 107-2)]	ACCEPTED
10-373	A	10:0		Table 10.2.1: Once completed, this table should be sent out again for review (or at least to modelling groups involved for checking). [Anthony Hirst (Reviewer's comment ID #: 107-3)]	ACCEPTED
10-374	A	10:0		Table 10.2.1: the caption does not incorporate all of the categories shown on the table and does not really explain the simulations. Fix the table. [Govt. of United States of America (Reviewer's comment ID #: 2023-661)]	See comment 10-371.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-375	A	10:4	10:10	Table 10.2.1: What are the red shaded regions representing? [Gareth S. Jones (Reviewer's comment ID #: 121-125)]	NO CHANGE NECESSARY – The red regions were missing data but now all data has been supplied.
10-376	A	10:4	10:10	Correction to row of table "UKMO-Hadcm3" (model 22 in the table?) and columns volcanic and solar. Should be "Const" [Gareth S. Jones (Reviewer's comment ID #: 121-126)]	ACCEPTED
10-377	A	10:4	10:10	Table 10.2.1 Is the volcanic column correct for GISS-EH and GISS-ER? Are the forcings from volcanic eruptions included in their climate projections? Do they assume some probability of liklihood of eruptions in the future? [Gareth S. Jones (Reviewer's comment ID #: 121-127)]	NO CHANGE NECESSARY – Yes, the GISS models include probabilistic future eruptions.
10-378	A	10:6		« Const. means the forcing agent is set to a constant or annually cyclic distribution » for the simulations of the futur should probably be added. Otherwise, the difference with « -- » is not clear. [Jean-Louis DUFRESNE (Reviewer's comment ID #: 60-9)]	ACCEPTED
10-379	A	10:8	10:8	This Table is unreadfable [VINCENT GRAY (Reviewer's comment ID #: 88-1553)]	TAKEN INTO ACCOUNT
10-380	A	10:8	10:10	The table entries are unreadable. [Andrew Lacis (Reviewer's comment ID #: 138-15)]	TAKEN INTO ACCOUNT
10-381	A	10:9	10:9	In the table 10.2.1, "Strat O3" and "Trop O3" for MIROC3.2(H) and MIROC3.2(M) should be "YES". [Seita Emori (Reviewer's comment ID #: 62-16)]	TAKEN INTO ACCOUNT
10-382	A	10:12	:15	Either include a brief reiteration (a footnote ?) of the definition of the first and second indirect effects, or refer to a glossary, or cross-refernce to an explanation elsewhere. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-42)]	ACCEPTED
10-383	A	10:17	10:17	Add at end "It should be noted that A2 and A1F1scenarios are highly improbable" [VINCENT GRAY (Reviewer's comment ID #: 88-1554)]	See comment 10-423.
10-384	A	10:20	10:20	Insert after "comparable "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1555)]	ACCEPTED
10-385	A	10:21	10:21	Insert before "declimae" "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1556)]	ACCEPTED
10-386	A	10:25	11:35	PRESENTATION: The concepts of shortwave and longwave forcing have to be properly introduced and explained for the non-expert. [Govt. of Germany (Reviewer's comment ID #: 2011-155)]	ACCCEPTED.
10-387	A	10:25	13:7	Suggest to shorten these two sections and refer to Chapter 8. [European Commission (Reviewer's comment ID #: 2008-40)]	See comment 10-388.
10-388	A	10:25	13:7	PRESENTATION: Two and a half pages on the AOGCM's radiative forcing validation	ACCEPTED. These sections have been

No.	Batch	Page:line From	Page:line To	Comment	Notes
				(sections 10.2.1.3 and 10.2.1.4) seems overproportionally long, given that both Chapter 8 on climate model validation and Chapter 2 on radiative forcing are appropriate places to deal with this matter. Not that it would not be important, but given by comparison to less than half a page on ocean acidification (section 10.4.2) this comparison seems clearly too long. For example: The paragraph on page 11, line 36, seems a prime candidate for being shortened. [Govt. of Germany (Reviewer's comment ID #: 2011-154)]	shortened. It does not make sense to move this material to chapter 2, since chapter 2 focuses on present-day, not future, forcing. It does not make sense to shift this material to chapter 8, since these sections show that a significant part of the range in model response is due to the formulation of radiative forcing.
10-389	A	10:25	13:7	SECTION 10.2.1.3 Comparison of modelled forcings to estimates in Chapter 2. Important but could be reduced very substantially and terms need to be defined better for the uninitiated. Most to an Appendix? [William Hare (Reviewer's comment ID #: 99-57)]	See comments 10-386 and 10-388.
10-390	A	11:4	11:3	Add at end "but this is only for a global average and may not be so good regionally" [VINCENT GRAY (Reviewer's comment ID #: 88-1557)]	ACCEPTED – The sentence now notes that the comparison is between global-mean values.
10-391	A	11:24	:26	Some clarification needed. I presume it means that because different models have different 2xCO2 forcing, specifying the climate sensitivity and then applying the best estimate of CO2 forcing will not reproduce the individual model results. Another approah would be to be specify sensitivity as K/Wm-2. (This raises a separate issue- even this will vary for different forcings (eg non uniformly spatial aerosols) may give a different value to increasing CO2. This relates also to using SCM models to represent GCMs) [John Mitchell (Reviewer's comment ID #: 180-3)]	ACCEPTED – Additional text has been added to clarify that the relationships between forcing and response in a given model should be derived using the radiative forcing calculated by that model.
10-392	A	11:31	11:31	"carbon dioxide" should be "carbon dioxide.". [Chiu-Ying LAM (Reviewer's comment ID #: 139-11)]	ACCEPTED
10-393	A	11:31		The source of the data should be included in the caption to the table [Danny Harvey (Reviewer's comment ID #: 101-68)]	ACCEPTED
10-394	A	11:33	11:33	In the table 10.2.2, "CCSR" should be "CCSR/NIES/FRCGC". [Seita Emori (Reviewer's comment ID #: 62-17)]	ACCEPTED
10-395	A	11:40	13:6	This seems to be the weakest point of the Report: the method of LBL (line-by-line) simulations. None of the mentioned LBL codes know the exact analytic solution of the radiative transfer equation. All of them are able to compute the present flux-components on the given monochromatic optical depths with very high acuracy, but none of them is able to produce reliable flux (and temperature) sensitivities at the surface for optical depth perturbations (different absorbent concentrations). This is because none of them is able to derive the correct sensitivity function from the greenhouse function (differentitating it by the optical depth), and none of them has the correct surface temperature boundary	REJECTED – The opinions expressed here regarding the accuracy of LBL models are not supported by the literature.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				condition (because of the lack of the exact analytic solution of the transfer equation). The widely used semi-infinite approximation leads to high inaccuracies in optically thin atmospheres (particularily over the polar regions). Details in the forthcoming TellusB article of Ferenc Miskolczi. [MIKLOS ZAGONI (Reviewer's comment ID #: 300-6)]	
10-396	A	12:11	12:13	This sentence was included in the First Order Draft but when the experiments 3a or 3b were detailed in a specific Table (10.2.2). This is no more the case and the sentence is useless. [Govt. of France (Reviewer's comment ID #: 2010-75)]	ACCEPTED – The sentence has been deleted.
10-397	A	12:11	12:11	Specify what is 3b and 3a. [Chiu-Ying LAM (Reviewer's comment ID #: 139-12)]	See comment 10-396.
10-398	A	12:11	12:13	This sentence was included in the First Order Draft but when the experiments 3a or 3b were detailed in a specific Table (10.2.2). This is no more the case and the sentence is useless. [Serge PLANTON (Reviewer's comment ID #: 199-3)]	See comment 10-396.
10-399	A	12:16		« Total forcings calculated from the AOGCM and LBL codes due to the increase in LLGHGs from 1860 to 2000 differ by less than 0.04, 0.49, and 0.08 Wm -2 at the top of model, surface, and pseudo-tropopause at 200mb, respectively. (Table 10.2.3). » In fact Table 10.2.3 gives a difference at the tropopause of 0.10 W.m-2, not 0.08W.m-2 [Jean-Louis DUFRESNE (Reviewer's comment ID #: 60-1)]	ACCEPTED
10-400	A	12:22	12:22	"RTMIP" should be "RTMIP.". [Chiu-Ying LAM (Reviewer's comment ID #: 139-13)]	ACCEPTED
10-401	A	12:31	12:31	PRESENTATION: Replace one "longwave" with "shortwave". [Govt. of Germany (Reviewer's comment ID #: 2011-156)]	ACCEPTED
10-402	A	12:40	12:40	Is it relative humidity or should it be specific humidity? [Andrew Lacis (Reviewer's comment ID #: 138-16)]	ACCEPTED – The specific, not the relative, humidity increases in most models of warmer climates.
10-403	A	13:11	13:11	I don't understand this sentence ("..due to diverse formulations of radiative transfer rather than differences in forcings"). Maybe it is because forcing is often understood as "radiative forcing". Do you mean "emission" or "sources" instead? [Sandrine Bony (Reviewer's comment ID #: 25-4)]	ACCEPTED – The sentence has been reworded and clarified.
10-404	A	13:17	13:17	PRESENTATION: State the time-frame of B1 scenario CO2 concentration changes, namely "by 2100". [Govt. of Germany (Reviewer's comment ID #: 2011-157)]	ACCEPTED
10-405	A	13:19	13:19	Title needs to be changed to "...Non Mitigation Forcing Projections for the 21st Century". [William Hare (Reviewer's comment ID #: 99-59)]	ACCEPTED – This is now discussed in the introduction (section 10.1)

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-406	A	13:19		The authors need to make a clear distinction between emission and forcing projections. [European Commission (Reviewer's comment ID #: 2008-41)]	ACCEPTED
10-407	A	13:19		Section 10.2.2.: PRESENTATION: The majority of this section "Recent Developments in Forcing Projections for the 21st Century" deals in fact with emissions projections, not forcing projections. Thus, clarify the structure of the report by adding a new section on emission projections before the current section 10.2 and move the paragraphs that discuss emissions to there. [Govt. of Germany (Reviewer's comment ID #: 2011-158)]	ACCEPTED – The section has been renamed to acknowledge that it covers both emissions and radiative forcing.
10-408	A	13:21	13:47	These three paragraphs are completely garbled and display an ignorance of the actual projections of the SRES scenarios. They should be replaced by a Table showing the scenario projections, as compared with the measured climate parameters [VINCENT GRAY (Reviewer's comment ID #: 88-1567)]	TAKEN INTO ACCOUNT – The material in section 10.2.2.1 has been largely eliminated into order to avoid duplication with the WG III report.
10-409	A	13:22	13:22	Replace "predictions" with " projections" [VINCENT GRAY (Reviewer's comment ID #: 88-1558)]	ACCEPTED
10-410	A	13:30	13:30	SECTION 10.2.2.1 Projections for radiative species considered in SRES: This subsection is good as far is it goes but it needs to show the implications of the reduced SO2 emissions on the radiative forcing projections at the end. [William Hare (Reviewer's comment ID #: 99-60)]	See comment 10-408.
10-411	A	13:30	14:18	Does this discussion belong in this chapter/report? It seems a reference to the WGIII report is all that is needed. [Ronald J Stouffer (Reviewer's comment ID #: 258-121)]	See comment 10-408.
10-412	A	13:30		Again, clarify that the SRES scenarios do not consider mitigation. [European Commission (Reviewer's comment ID #: 2008-42)]	ACCEPTED – This is now discussed in the introduction (section 10.1).
10-413	A	13:30		Section 10.2.2.1: This section deals primarily with a comparison of actual present day emissions and the SRES non-mitigation scenarios. Under a section heading, "for the 21st Century" it seems required that the SRES scenarios are as well discussed in a longer term context, namely the fact that these SRES emissions are non-mitigation scenarios. [Govt. of Germany (Reviewer's comment ID #: 2011-159)]	ACCEPTED – This is now discussed in the introduction (section 10.1).
10-414	A	13:30		Section 10.2.2.1: Clarify that the SRES scenarios are non-mitigation scenarios and are here compared to the - partially already mitigating - real world developments. [Govt. of Germany (Reviewer's comment ID #: 2011-160)]	ACCEPTED – This is now discussed in the introduction (section 10.1).
10-415	A	13:31	13:34	Replace from "Recent trends" on line 31 to "agreement" on line 34 with "All the scenarios were 5% too high in 2000, and , so far, up to the last official figure in 2002, they remain so" [VINCENT GRAY (Reviewer's comment ID #: 88-1559)]	REJECTED – This would eliminate the discussion of an important recent analysis of trends in LLGHGs and the correspondence with the SRES scenarios.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-416	A	13:32	13:33	Second sentence is hard to follow. What does "current" refer to? What does "total" refer to? [Govt. of Australia (Reviewer's comment ID #: 2001-403)]	ACCEPTED – The sentence has been clarified.
10-417	A	13:34	13:34	Delete "Perhaps the" [VINCENT GRAY (Reviewer's comment ID #: 88-1560)]	ACCEPTED
10-418	A	13:34	13:34	.Delete "related to the decline in SO2 emissions" [VINCENT GRAY (Reviewer's comment ID #: 88-1562)]	REJECTED – The sentence would not make sense with the suggested change.
10-419	A	13:34	13:34	Rerplace "predicted" with "in SO2 emissions projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1563)]	ACCEPTED
10-420	A	13:35	13:35	Insert after "are" " that all of the scenarios project large increases in methane concentration, when in actuality the concentration has stabilised. Also" [VINCENT GRAY (Reviewer's comment ID #: 88-1561)]	TAKEN INTO ACCOUNT – Please see comment 10-422.
10-421	A	13:37	13:37	The correct term for "Reforming Economies" is "Economies in Transition" [William Hare (Reviewer's comment ID #: 99-58)]	REJECTED – The term "Reforming Economies" is the one used in the studies by Van Vuuren and O'Neill discussed in this section.
10-422	A	13:41	13:47	Add somewhere: "It is noteworthy that methane growth in the atmosphere has declined to near zero, and, in two recent years was actually negative". Source is Figure 2.5 from this draft. [Patrick Michaels (Reviewer's comment ID #: 176-26)]	ACCEPTED
10-423	A	13:42	13:42	Insert after "scenarios" "such as A2 and A1F1, which are, therefore, highly improbable" [VINCENT GRAY (Reviewer's comment ID #: 88-1564)]	REJECTED – The suggested addition is an editorial remark. This section already includes references to recent literature on the correspondence between current trends and the SERES scenarios and the implications for the likelihood of these scenarios.
10-424	A	13:44	13:44	Insert after "2005)" "but since it projects a 70% increase by 2100, when the concentration is currently stable, this is difficult to believe" [VINCENT GRAY (Reviewer's comment ID #: 88-1565)]	See Comment 10-422.
10-425	A	13:44	13:47	Replace from ."Recent Trends": to end of paragraph with "Since the trend in methane comcentration is a decreasing rate of increase, the actual concentration is most likely to decrease rather than increase.. All the scenarios project a current increase which is not happening. However, the scenario B1 projects a slight decrease by 2100" [VINCENT GRAY (Reviewer's comment ID #: 88-1566)]	REJECTED – This is not consistent with the conclusions of chapter 7.
10-426	A	13:46		Not clear- is this the change in forcing due to methane?, is it an increase or decrease?	See Comment 10-427. The value

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[John Mitchell (Reviewer's comment ID #: 180-4)]	quoted is the change in CH4 forcing from the 1990s to the 2020s.
10-427	A	13:46		I believe "radiative forcing by methane between 2000 and 2030 is less than 0.004Wm-2" should be replaced by "the change in radiative forcing by methane from 2000 to 2030 is less than 0.004Wm-2". The current radiative forcing by methane is stated to be about 0.5Wm-2 in the Summary for Policymakers. [Adrian Simmons (Reviewer's comment ID #: 242-143)]	ACCEPTED
10-428	A	14:4	14:4	Replace "anthropogenic" by "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1568)]	REJECTED -- NO REASON FOR CHANGE GIVEN..
10-429	A	14:4	14:18	Add in somewhere: "This behavior would argue that the emissions trajectory for the future is somewhere beneath A1B". [Patrick Michaels (Reviewer's comment ID #: 176-27)]	REJECTED – The time behavior of the recent past is not necessarily a good predictor of the end of the 21st century and therefore cannot be used to predict the long-term emissions trajectory.
10-430	A	14:6	14:6	Does phrase "grow much less rapidly" apply to all of the most significant rediative forcing species, or only to some? [Govt. of Australia (Reviewer's comment ID #: 2001-404)]	ACCEPTED – Streets (2001) concerned CO_2 and CH_4, and this has now been noted in the text.
10-431	A	14:7	14:7	Replace "will" by "may" [VINCENT GRAY (Reviewer's comment ID #: 88-1569)]	ACCEPTED
10-432	A	14:20		Ozone and nitrate were covered in the SRES report. [European Commission (Reviewer's comment ID #: 2008-43)]	ACCEPTED – The title of the section has been changed.
10-433	A	14:20		Section 10.2.2.2: This section is titled "Projections for radiative species: Extensions beyond SRES", where SRES stands for "Special Report on Emission Scenarios". The first two paragraphs on ozone precursor emissions and nitrate aerosols need to be placed under a different subheading as these emissions were included in the scenarios of the Special Report on Emission scenarios. Thus, the sections on tropospheric ozone forcing and nitrate aerosols deal with advances in the CLIMATE MODELING field, not the SRES, i.e. the EMISSION modeling field. [Govt. of Germany (Reviewer's comment ID #: 2011-161)]	ACCEPTED – The material has been reorganized and relabelled as suggested.
10-434	A	14:23	14:23	Replace "anthropogenic" by "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1570)]	REJECTED -- NO REASON FOR CHANGE GIVEN.
10-435	A	14:26	14:26	Delete "well-mixed" They are not well-mixed [VINCENT GRAY (Reviewer's comment ID #: 88-1571)]	ACCEPTED – The term well-mixed has been replaced with long-lived for consistency with Chapter 2.
10-436	A	14:32	14:34	Please clarify. [European Commission (Reviewer's comment ID #: 2008-44)]	TAKEN INTO ACCOUNT – Comparisons of recent trends against

No.	Batch	Page:line From	Page:line To	Comment	Notes
					the SRES scenarios have been removed since these topics fall under the mandate of WG III.
10-437	A	14:32	14:34	This sentence "The growth rates of atmospheric methane …" is misplaced and seems to belong to the above section 10.2.2.1, where SRES scenarios are compared to recent emission observations. Furthermore the sentence should clarify that the B2 SRES scenario is an EMISSION scenario, thus the recent concentrations are "comparable to, or less than, concentrations that are estimated, if a B2 SRES emissions scenario is assumed." [Govt. of Germany (Reviewer's comment ID #: 2011-162)]	See comment 10-436.
10-438	A	14:32	14:32	Insert after "fallen" "to zero, with the possibility of negative growth in the future" [VINCENT GRAY (Reviewer's comment ID #: 88-1572)]	See comment 10-436.
10-439	A	14:32	14:32	Specify which "recent projections" [William Hare (Reviewer's comment ID #: 99-61)]	See comment 10-436.
10-440	A	14:33	14:34	Replace from "comparable" on line 33 to end on line 34 with "all project an unrealistic increase" [VINCENT GRAY (Reviewer's comment ID #: 88-1573)]	REJECTED – Other parts of the assessment suggest that the decline in methane growth rates may not be a permanent feature.
10-441	A	14:33	14:33	Result from the B2 SRES emission scenario [William Hare (Reviewer's comment ID #: 99-62)]	ACCEPTED
10-442	A	14:43		Add "in magnitude" after "increases" [Martin Manning (Reviewer's comment ID #: 155-75)]	ACCEPTED
10-443	A	14:44	14:44	Add at end "The A2 scenario projects such a huge groeth (nine times) in coal production that this result is hardly surprising" [VINCENT GRAY (Reviewer's comment ID #: 88-1574)]	ACCEPTED – The wording of the new sentence is not identical to that suggested..
10-444	A	15:5	15:5	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1575)]	REJECTED -- NO REASON FOR CHANGE GIVEN.
10-445	A	15:5	:7	The review of current/past stratospheric changes (sulfate, water vapor, etc) should not be done here as it may be in conflict with the more thorough and up to date coverage in Chapter 2 (where it belongs). Shorten this paragraph and just note the range of possible error in your future scenarios due to the lack of these forcings. Also see details on next comment. [Govt. of United States of America (Reviewer's comment ID #: 2023-662)]	See comment 10-388.
10-446	A	15:8	15:11	Re: Marquart et al. (2003) estimate that the radiative forcing by contrails will increase from 0.035 W m–2 in 1992, to 0.094 W m–2 in 2015, and to 0.148 W m–2 in 2050. The rise in forcing is due to an increase in subsonic aircraft traffic following estimates of future fuel consumption (Penner et al., 1999). The projections cited for the year 2050 are	TAKEN INTO ACCOUNT – To the best of this LA's knowledge, the issues raised by this reviewer have not been addressed in the peer-reviewed

No.	Batch	Page:line From	Page:line To	Comment	Notes
				based on static scenarios that only consider fuel burn from aviation changes. What would be the estimates of other ongoing dynamic changes (e.g., loading of GHG, circulation changes, etc.) are taken into account? [Govt. of United States of America (Reviewer's comment ID #: 2023-663)]	literature.
10-447	A	15:13	15:17	Sulphur was also covered in the SRES report. [European Commission (Reviewer's comment ID #: 2008-45)]	ACCEPTED – This section has been relabelled.
10-448	A	15:13	15:17	The current section "Pitari et al. (2002) examine the effect of future emissions under … to 2030 is -0.06 W/m2." does not belong under a heading "Extensions beyond SRES", as sulphate aerosol emissions are part of the SRES non-mitigation scenarios. Please reorganize. [Govt. of Germany (Reviewer's comment ID #: 2011-163)]	See comment 10-447.
10-449	A	15:19	15:20	I am suprised by this statement because I expected deserts to grow with climate change. [Pascale DELECLUSE (Reviewer's comment ID #: 58-61)]	NO CHANGE NECESSARY.
10-450	A	15:35	15:35	It would be convenient if Table 10.3.1 included the model numbers, matching Table 8.2.1. [Govt. of Australia (Reviewer's comment ID #: 2001-405)]	Accepted. Numbers are added.
10-451	A	15:39	15:39	Replace "climate change" with "changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1576)]	Accepted.
10-452	A	15:43	15:43	Add "10.1 and" before "table 10.3.1". [Ronald J Stouffer (Reviewer's comment ID #: 258-122)]	Accepted. Sentence is modified.
10-453	A	15:46	15:46	Replace "climate change" with "changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1577)]	Rejected. as brevity is needed.
10-454	A	15:47	15:47	PRESENTATION: The current statement "(see Section 10.2 for details regarding the scenarios, and also Figure 10.3.1)." points to the fact that there is an urgent need for a consolidated section that deals with the emission scenarios - to be inserted before current section 10.2, which deals with "Projected Changes in Radiative Forcing". Figure 10.3.1. should be amended with emission trajectories for the discussed SRES non-mitigation emission scenarios, as currently only the temperature implications, not the actual emissions are plotted. [Govt. of Germany (Reviewer's comment ID #: 2011-164)]	Taken into account. The scenarios are described earlier, and also depicted elsewhere
10-455	A	15:48	15:49	The sentence "This is a subset of the SRES marker scenarios used in the TAR and they represent a "low" (B1), "medium" (A1B) and "high" (A2) scenario." is misleading as it omits the fact that all these three scenarios are non-mitigation scenarios. Apparently, no mitigation scenarios seem to have been assessed in this Chapter draft so far, which has to be rectified for the next and final version. [Govt. of Germany (Reviewer's comment ID #: 2011-165)]	Taken into account. Consistent with previous discussion, 'relatively' is added.
10-456	A	15:49	15:49	low, medium, high - In terms of what? Radiative forcing?	Taken into account, in previous

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Ronald J Stouffer (Reviewer's comment ID #: 258-123)]	discussions.
10-457	A	15:53	15:53	Replace "climate change" with "changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1578)]	Rejected. As the phrase cannot be changed
10-458	A	16:5	16:6	The reference to "plausible anthropogenic forcing scenarios" is not really right here: Only non-mitigation scenarios have been used and arguable these may even be less plausible than policy scenarios! Replace with something like "This section considers the basic changes in climate over the next hundred years simulated by current climate models using anthrpogenic forcing scenarios without mitigation" [William Hare (Reviewer's comment ID #: 99-63)]	Taken into account. 'Non-mitigation' added.
10-459	A	16:6	16:6	No, these are emission scenarios, and it is important to emphasise that they do not consider mitigation. [European Commission (Reviewer's comment ID #: 2008-46)]	Taken into account. In the context of modelling, the forcing is what is relevant. 'Non-mitigation' now added.
10-460	A	16:6	16:6	The first sentence of this paragraph has to be corrected, since the assessed scenarios are not "plausible anthropogenic forcing scenarios", but "NON-MITIGATION anthropogenic EMISSION scenarios". [Govt. of Germany (Reviewer's comment ID #: 2011-166)]	Taken into account. In the context of modelling, the forcing is what is relevant. 'Non-mitigation' now added.
10-461	A	16:6	16:6	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1579)]	Why?
10-462	A	16:6		Replace "plausible" with "non-mitigation". I don't think it befits the IPCC to call non-mitigation scenarios plausible, given the Kyoto protocol is in force now. This otherwise sounds like a political judgement, i.e. IPCC finds it plausible that no mitigation occurs despite Kyoto. This needs to be fixed throughout the chapter - wherever these scenarios are discussed, call them "non-mitigation scenarios". Why don't mitigation scenarios get discussed in the report? There are publications on those that should be discussed and cited! [Stefan Rahmstorf (Reviewer's comment ID #: 206-13)]	Taken into account. Non-mitigation added, in initial use. Other idealised scenarios are mentioned elsewhere, but not used for the major experiment.
10-463	A	16:9		Same comment as for chapter 10 page 7 line43 [Anders Levermann (Reviewer's comment ID #: 145-10)]	Answered previously.
10-464	A	16:10	16:11	To say that "it's anticipated that this might be true for climate changes" gives no sense of how likely it is believed that it is that it's true. "might be true" can be anything from "imaginably but barely possible" to "more likely than not". [Paul Baer (Reviewer's comment ID #: 10-7)]	Accepted.
10-465	A	16:10	16:11	"it is anticpated that this might be true" could be replaced by either "this might be true" or "it is anticpated that this is true". [Adrian Simmons (Reviewer's comment ID #: 242-145)]	Accepted.
10-466	A	16:11	16:11	Replace "climate change" with "changes in the climate"	Accepted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[VINCENT GRAY (Reviewer's comment ID #: 88-1580)]	Changed.
10-467	A	16:22	16:22	Add at end "these experiments are completeluy unrealistic since the currect rate of carbon dioxide increase is only 0.4% a year" [VINCENT GRAY (Reviewer's comment ID #: 88-1581)]	Rejected. The equivalent forcing is larger than this. The experiment is described as idealized.
10-468	A	16:43	16:43	"presented in 10.2," should be "presented in Section 10.2,". [Chiu-Ying LAM (Reviewer's comment ID #: 139-14)]	Accepted.
10-469	A	16:44	16:44	After "in each scenario" add - with ocean heat uptake playing a list on determining the response. [Ronald J Stouffer (Reviewer's comment ID #: 258-124)]	Rejected. Unnecessary detail, here.
10-470	A	16:45	16:45	Insert afetr "have" ":projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1582)]	Taken into account. We prefer to use simulated.
10-471	A	16:46	16:46	Replace "increases" with "is projected to increase" [VINCENT GRAY (Reviewer's comment ID #: 88-1583)]	Rejected. Unnecessary detail, here.
10-472	A	16:47	16:48	It is important to point out here that the mean precipitation is limited rather by the total available energy, rather than the water vapour holding capacity of the atmosphere. It is the balance between radiative cooling and latent heating in the troposphere that controls mean precipitation rate (Allen and Ingram, 2002), and the paragraph should refer to 9.5.4.2, where this is discussed. [Govt. of Australia (Reviewer's comment ID #: 2001-406)]	Accepted. Text is modified.
10-473	A	16:54	16:54	Emissions, not forcings [European Commission (Reviewer's comment ID #: 2008-47)]	Rejected. Models do not use emission directly.
10-474	A	16:54	16:54	10.3.2 should be 10.3.1. [Govt. of Finland (Reviewer's comment ID #: 2009-125)]	Accepted. Order is revised.
10-475	A	16:54	16:54	Replace "varying forcings" with "varying emissions" in order to add clarity as the main reason are the different emissions of the EMISSION scenarios. [Govt. of Germany (Reviewer's comment ID #: 2011-167)]	Rejected. Models do not use emission directly.
10-476	A	17:0		Table 10.3.2: This table is confusing. [European Commission (Reviewer's comment ID #: 2008-50)]	Accepted.
10-477	A	17:0		Table 10.3.2: Split this table into two with separated mean warmings and pattern indicators. The current table format is utterly confusing given the multiple numbers in each column entry. [Govt. of Germany (Reviewer's comment ID #: 2011-170)]	Accepted.
10-478	A	17:11	17:12	following values given the second column of Table 10.3.2 change for "from 0.64 to 0.69°C" instead of wording "from 0.64°C to 0.769°C" [Valentin Meleshko (Reviewer's comment ID #: 175-2)]	Accepted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-479	A	17:12	17:13	This conclusion is only valid for non-mitigation scenarios. [European Commission (Reviewer's comment ID #: 2008-48)]	Accepted. Wording is modified.
10-480	A	17:12	17:13	Insert "non-mitigation" before "scenario" so that the sentence reads: "… shows that no matter which non-mitigation scenario is followed, the warming is similar on the timescale of the next decade or two". Reason for change: No non-mitigation scenarios have been assessed in this draft, it seems. Alternatively, assess non-mitigation scenarios in order to substantiate this statement. [Govt. of Germany (Reviewer's comment ID #: 2011-168)]	Taken into account. Wording is modified.
10-481	A	17:12	17:12	Should refer to "non-mitigation" scenario to correct [William Hare (Reviewer's comment ID #: 99-64)]	Taken into account.
10-482	A	17:14	17:16	This sentence "It is also worth noting that nearly half of the early century climate change arises from warming we are already committed to (0.31°C for early century)." is not really correct as it implies that "committed" is due to past emissions. The story is more complex: to capture this I suggest wording like: "It is also worth noting that a significant amount of climate change will occur irrespective of policy interventions and on this time scale (early century) a lower bound on this can estimated from the warming resulting from constant forcing from 2000". [William Hare (Reviewer's comment ID #: 99-65)]	Taken into account. 'Constant composition' commitment now stated, and reference to 10.7.1 made.
10-483	A	17:14	17:14	"discussed in 10.5," should be "discussed in Section 10.5,". [Chiu-Ying LAM (Reviewer's comment ID #: 139-15)]	Accepted.
10-484	A	17:15	17:15	Insert after "are" "thought to be" [VINCENT GRAY (Reviewer's comment ID #: 88-1584)]	Taken into account. 'From these results' added.
10-485	A	17:17	17:18	As per above, zero emissions or constant forcing? [European Commission (Reviewer's comment ID #: 2008-49)]	Taken into account. 'From these results' added.
10-486	A	17:17	17:18	Replace current text "… and with only about a quarter of that warming due to climate change we are already committed to (0.46°C)" with " … and with only about a quarter of that warming (0.46°C) due to climate change that would happen, if we held current radiative forcing levels constant by reducing greenhouse gas emissions by 30% to ~80% percent." The current formulation is incorrect, given that a zero emission scenario is different from the here analyzed "constant composition / current radiative forcing" scenario. [Govt. of Germany (Reviewer's comment ID #: 2011-169)]	Taken into account. 'From these results' added.
10-487	A	17:18	17:18	Replace "are" by "we think we are" [VINCENT GRAY (Reviewer's comment ID #: 88-1585)]	Taken into account. 'From these results' added previously.
10-488	A	17:18	17:18	Change "we are already committed to" to "that would results from constant forcing from 2000" to accurately reflect the calculation.	Taken into account. 'Commitment' qualified previously.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[William Hare (Reviewer's comment ID #: 99-66)]	
10-489	A	17:20	17:20	Replace "climate change" with "changes in the climate we think" [VINCENT GRAY (Reviewer's comment ID #: 88-1586)]	Rejected.
10-490	A	17:20	17:20	Change "we are already committed to" to "that would results from constant forcing from 2000" to accurately reflect the calculation. [William Hare (Reviewer's comment ID #: 99-67)]	Taken into account. 'Commitment' qualified previously.
10-491	A	17:22	17:31	Clarity of table could be improved and change "Commit" to "Constant forcing commitment" [William Hare (Reviewer's comment ID #: 99-68)]	Accepted. Phrase added to caption.
10-492	A	17:26	17:29	I think M is the Arcsin-Mielke statistic. This should be referenced to Watterson (1996) so that the intrepid can investigate why it is used. [Gavin Schmidt (Reviewer's comment ID #: 227-15)]	Taken into account. 'see text'
10-493	A	18:1	18:2	Figure 10.3.5: An explanation of this figure should provide discussion of the accuracy of the ocean models that are used, even if this is a reference to a section in Chapter 8. [Govt. of Australia (Reviewer's comment ID #: 2001-407)]	Accepted. Discussion of variation among models now refers to Chapter 8.
10-494	A	18:1	18:2	Figure 10.3.5: This figure seems to imply that an El-Nino like pattern exists, however, no text in the report describes this. The text around the figure needs to more clearly explain this discrepancy. [Govt. of Australia (Reviewer's comment ID #: 2001-408)]	Taken into account. Section 10.3.5.3 referred to, now.
10-495	A	18:10	18:11	Only valid for non-mitigation scenarios. [European Commission (Reviewer's comment ID #: 2008-51)]	Taken into account, with 'under these scenarios' added.
10-496	A	18:10	18:11	Add "under non-mitigation scenarios" behind "… but now we also see its evolution during the 21st century", as only SRES non-mitigation scenarios are analyzed in this draft section. Alternatively provide some insight into possible changes under mitigation scenarios in order to draw wider encompassing conclusions for the 21st century. [Govt. of Germany (Reviewer's comment ID #: 2011-171)]	Taken into account, with 'under these scenarios' added.
10-497	A	18:38	18:40	This sentence gives the misleading impression that the warming would be smaller in the Arctic than over the equatorial Pacific. [Govt. of Finland (Reviewer's comment ID #: 2009-126)]	Accepted. Sentence is modified.
10-498	A	18:45	18:48	I now understand the M measure better, but wouldn't a simple pattern correlation be easier to understand? I guess there maybe good reasons why it isn't. [Matthew Collins (Reviewer's comment ID #: 44-30)]	Taken into account. Correlation is a different, less precise, statistic, as now noted.
10-499	A	19:7	19:7	correct ".." [Pascale DELECLUSE (Reviewer's comment ID #: 58-62)]	Accepted.
10-500	A	19:7	19:7	"consideration here.." should be "consideration here.". [Chiu-Ying LAM (Reviewer's comment ID #: 139-16)]	Accepted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-501	A	19:14	19:14	Change "low" to "small"? [Ronald J Stouffer (Reviewer's comment ID #: 258-125)]	Accepted.
10-502	A	19:17		figure 10.3.6. could the colour scheme for precip be the other way round - i.e. more intuitive if red implies dry and blue implies wet (as in figure 10.3.9) [Chris Jones (Reviewer's comment ID #: 120-37)]	Accepted.
10-503	A	19:46		Fig 10.3.7 Should point out changes in cloud in subtropics are large in % terms, but small in absolute terms. Does not give good indication of strength of feedback. [John Mitchell (Reviewer's comment ID #: 180-5)]	Taken into account. Changes are absolute (of percent cover), as noted now in caption.
10-504	A	19:48	19:48	It is not clear to which section of the report the following sentence makes reference. "The larger changes relate well to changes in precipitation depicted earlier." Please indicate specifically. [Valentin Meleshko (Reviewer's comment ID #: 175-3)]	Accepted. Text is modified.
10-505	A	19:54	19:57	This usage of the term "cloud radiative forcing diagnostic" seems incompatible with the standard IPCC definition of radiative forcing (see http://www.grida.no/climate/ipcc_tar/wg1/214.htm), which is defined as an ".... externally imposed perturbation in the radiative energy budget....". Surely, the "externally imposed perturbation" via indirect effects on clouds is not -22.3W/m2. Please rephrase and use consistent terminology. [Govt. of Germany (Reviewer's comment ID #: 2011-172)]	Taken in account. Term is used elsewhere but should be added in the glossary.
10-506	A	20:28		"project" or "predict" would be better than "simulate". [Adrian Simmons (Reviewer's comment ID #: 242-147)]	Why?
10-507	A	20:36		after "than the increases at high latitudes" add "Despite inter-model differences in the precise spatial locations of the increases and droughts, Neelin et al (2006) show that the amplitudes of the precipitation increase and decreases in the tropics and subtropics agree reasonably well among the models. These amplitudes increase approximately linearly with the tropical mean warming and imply substantial precipitation changes may be expected in the tropics." [J. David Neelin (Reviewer's comment ID #: 187-27)]	Accepted. Reference is added.
10-508	A	20:36		If possible, I would like to suggest the addition of a figure (inserted pg 10-21, line 2, after Fig. 10.3.9. The figure would be referred to in the sentences added pg 10-20, line 36). This would be the amplitude of the precipitation increase and decrease regions as a function of time, similar to figure 2 of Neelin et al (2006, PNAS). We would be happy to produce any variant of this figure, as needed (e.g. annual average). We can also put an inset of the scaling of precipitation amplitude with temperature into this figure if desired. I will attempt to upload an eps of this figure into the review comment data base, but if this	Taken into account. Stippling is modified in Fig. 10.3.6. A further figure is not possible.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				doesn't work, it is published in PNAS, April 18 2006, vol 103, no 16, pg 6111. Please email neelin@ucla.edu if a modified version is desired for consideration. The gap that I feel the figure would fill is that left by the vast unstippled area in Fig. 10.3.9, where precipitation changes are large but the models agree poorly on a point-by-point basis. This figure indicates that the models in fact are doing processes that agree fairly well under a suitable measure. Chapter 10 is the appropriate location for addressing this properly because chapter 11 is structured by specific regions, thus a general view of this sort fits better in 10. [J. David Neelin (Reviewer's comment ID #: 187-28)]	
10-509	A	20:36		Draft Figure caption for the suggested added figure after Fig. 10.3.9. "Amplitude of dry and wet precipitation changes (30-year running mean, relative to 1901-1960) for June-Aug. (JJA) from ten models for the A2 scenario. The anomaly at each year is projected onto the normalized spatial pattern of late 21st century (2070-2099 average) negative and positive precipitation change, respectively. Units mm/day. The error bars are 2 standard deviation values of the same projection quantity evaluated for internal variability in the control runs for each model. Inset shows the relationship to tropical average surface air temperature. [J. David Neelin (Reviewer's comment ID #: 187-29)]	see above
10-510	A	20:39		The statement "With annual mean precipitation being of particular importance" should be clarified. Importance for what? For some, precipitation during the crop-growing season may be of most importance. For others it might indeed be the annual mean. [Adrian Simmons (Reviewer's comment ID #: 242-148)]	Accepted. Text is modified.
10-511	A	20:44	20:45	Explain the physical meaning of the absence of stippling in the dryer areas both in figures 10.3.6 and 9. Is the drying less certain than the precipitation increase in northern latitudes ? The answer is given page 31, line 20-21 and should be recalled here or in the relevant figure captions.An alternative would be to use the same stippling as in figure SPM-6 where stippling indicates regions where 75% or more of the models used agree in the sign of precipitation changes. [Govt. of France (Reviewer's comment ID #: 2010-76)]	Accepted. Stippling is modified and discussed more fully.
10-512	A	20:44		« global mean seen in Figure 10.3.1. » It should be Figure 10.3.2 [Jean-Louis DUFRESNE (Reviewer's comment ID #: 60-2)]	Accepted.
10-513	A	20:45	20:47	It would be more illustrative to give per cent changes relative to the present-day mean for land and ocean separately, rather than per cent contributions to the global mean increase. [Govt. of Finland (Reviewer's comment ID #: 2009-127)]	Accepted. This is added.
10-514	A	20:45	48:18	References that better substantiate this sentence: after "Mediterranean region" add "(Rowell and Jones 2006)"; after "Caribbean region" add "(Neelin et al 2006)". [Both refs are already in the IPCC data base and meet IPCC citation criteria; the latter has appeared	Accepted. References are added.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				in PNAS with ref given below; the first is also cited elsewhere inthe chpt.] [J. David Neelin (Reviewer's comment ID #: 187-25)]	
10-515	A	21:1	21:2	Figure 10.3.9: This figure provides useful information, however, panel (b) is considerably misleading as it conveys the impression that soil moisture can be compared and averaged across models. Either the title of the panel needs to be altered, or the authors must provide more caveats and state that the panel is indicative only of trends. [Govt. of Australia (Reviewer's comment ID #: 2001-410)]	Taken into account. Stippling now shows agreement in sign of change. Text modified.
10-516	A	21:5		After "transport of water vapor." add: "An important factor in the precipitation response is the differential increase in moisture between strongly convecting regions and subsidence regions (Chou et al 2006). While the Clausius-Clapeyron equation constrains moisture increases as temperatuer increases, convective regions maintain a deeper layer of high relative humidity (Bretherton et al 2004). The resulting moisture gradient between convective regions and descent regions is affects the moisture transports, tending to increase the precipitation within convective regions and reduce precipitation in surrounding descent regions (Chou and Neelin 2004). Precipitation reductions at the margins of the convection zones can be associated with inflow of less moistened air from the descent regions (Neelin et al 2003). [J. David Neelin (Reviewer's comment ID #: 187-30)]	Taken into account. Extended argument is not possible but references are added.
10-517	A	21:17	21:22	It would be even more useful to maps of the change in summer soil moisture, so I highly recommend adding such maps. Wang (2005, Climate Dynamics) produced such maps for the IPCC AR4, so why don't we see them? I particularly recommend including his Fig 4d. Also: no source is given, here or in the figure caption, for the maps that you do show. [Danny Harvey (Reviewer's comment ID #: 101-69)]	Additional figure is not possible, but we are pleased to include the reference. As noted earlier, figures are made by the authors, but are comparable to results in the literature.
10-518	A	21:27	21:27	It is "Wang and Swail, 2006a", not "Wang and Swail, 2006b". So, please replace the "2006b" in this line with "2006a". [Xiaolan L. WANG (Reviewer's comment ID #: 282-21)]	Accepted.
10-519	A	21:29	21:29	It seems very surprising that the mean pressure decrease in high latitudes would not exceed the intermodel standard deviation. Please check your calculations. [Govt. of Finland (Reviewer's comment ID #: 2009-128)]	Taken into account. Text now explains result.
10-520	A	21:42	21:43	In the comment that warming is amplified at high latitudes because of positive feedbacks involving snow and sea ice, it should be added that there are other processes which also contribute to this amplification (such as increased poleward energy transport and cloud feedbacks), and reference made to 8.6.3.3 with this point is discussed. [Govt. of Australia (Reviewer's comment ID #: 2001-409)]	Accepted. Reference to 8.6.3.3 + other processes added.
10-521	A	21:55	22:1	Add "Holloway and Sou (2002) argue that, because of sampling error, Rothrock et. al overestimated arctic ice loss by a factor of between two and three". [Patrick Michaels (Reviewer's comment ID #: 176-28)]	Reject. Not relevant in present context.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-522	A	22:4	22:6	I am not sure if it is justified to say that the changes are small, when the snow retreats northward by several degrees of latitude. Also, Fig. 10.3.12 only represents the mid-winter (DJF) conditions. In high laitudes, changes in autumn and spring snow cover might be larger. [Govt. of Finland (Reviewer's comment ID #: 2009-130)]	Accepted. This actially refers to page 23. The text has been changed (see 10-559)
10-523	A	22:7	22:7	Replace "decreases" with "is projected to decrease" [VINCENT GRAY (Reviewer's comment ID #: 88-1587)]	Accepted
10-524	A	22:24	:26	Basic state' is a bit vague - perhaps 'basic state of ice thickness'? [Steve Harangozo (Reviewer's comment ID #: 98-25)]	Accepted. Changed to basic state of ice thickness and extent.
10-525	A	22:28	22:39	mention that vegetation-snow albedo feedbacks will likely amplify high latitude changes further. Could cite Chapin et al, Science, 2005. [Chris Jones (Reviewer's comment ID #: 120-21)]	Rejected. Dont see how this fits within section 10.3.3.2
10-526	A	22:33	:34	Should 'mean' be the 'ensemble mean'? - mean appears twice in same sentence. Otherwise change 'mean' in line 34 to 'average' [Steve Harangozo (Reviewer's comment ID #: 98-26)]	Accepted. Sentence corrected.
10-527	A	22:43	22:53	"The statements made in this section regarding the loss of permafrost are misleading and the models on which the conclusions are based have some rather important limitations. Given that these statements have been carried forward to the summary for policy makers and the Technical Summary (as robust findings) it is important that the editors have a better understanding of the limitations of the models and the proper interpretation of the results. From my understanding, all papers referenced in this section have been written by scientists that would most likely be considered climate modellers rather than permafrost scientists. The terminology that is used is not what would be used by the permafrost research community. A number of individual comments are offered below." [Govt. of Canada (Reviewer's comment ID #: 2004-181)]	Paragraph has been rewritten.
10-528	A	22:43	22:53	"The models of Lawrence and Slater (2005), Yamaguchi et al (2005) and Kitabata et al. (2006) do not simulate permafrost distribution as they only consider the upper few metres of the soil (3.43 m depth for Lawrence and Kitabata models and 10 m for Yamaguchi). Because the models do not treat deeper levels, they can not adequately simulate permafrost extent and the maps produced in these papers have led to misinterpretations in the media. What these authors are simulating is thaw depth." [Govt. of Canada (Reviewer's comment ID #: 2004-182)]	Paragraph has been rewritten.
10-529	A	22:43	22:53	"It would be more appropriate to say that there may be an increase in thaw depth over much of the permafrost region in response to climate warming rather than say that over half of the area covered by the top most layer of permafrost could thaw by 2050 and 90%	Paragraph has been rewritten.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				by 2100. As mentioned in the comment above, the model simulates only very shallow ground temperature and thaw depth, not permafrost extent. Permafrost scientists would not use the terminology that is used in this section and do not refer to thawing of the upper layer of permafrost but rather deepening of the active layer or increases in thaw depth. The terminology used here again is misleading and the statement does not make sense." [Govt. of Canada (Reviewer's comment ID #: 2004-183)]	
10-530	A	22:43	22:53	"Note that the maps presented in papers such as that by Lawrence and Slater (2005) appear to indicate loss of large areas of permafrost. Again the map is misleading and what it really shows is the thaw depth. Areas where permafrost is indicated as absent are areas where the thaw depth has been predicted to be greater than 3.43 m. Since the deeper thermal regime is not simulated these authors can not say anything about the change in permafrost extent." [Govt. of Canada (Reviewer's comment ID #: 2004-184)]	Paragraph has been rewritten.
10-531	A	22:43	22:53	"A more important concern with the papers cited in this section is the fact they only simulate the ground thermal regime in the upper few metres (as shallow as 3.43 m in the case of the Lawrence and Kitabata papers). Heat flow downward into the frozen soil below is not considered (there is a zero flux boundary at shallow depth), i.e heat flow is restricted to the upper few metres of the soil. This is simply not the case and setting a lower boundary at depths of 3.43 m or even 10 m (as in Yamaguchi paper) is not approriate for simulations of 50 to 100 years." [Govt. of Canada (Reviewer's comment ID #: 2004-185)]	Paragraph has been rewritten.
10-532	A	22:43	22:53	"Heat is conducted into the soil well below depths of 3 to 10 m over time periods of 100 years. Ignoring the rather significant heat sink that is provided at depth by the frozen ground leads to a greater warming of the near surface soil than would actually be the case, and therefore greater thawing than would actually occur (i.e. there is an artificial warming of the upper part of the ground). This lag that occurs between changes in surface temperature and those at depth is discussed in Lachenbruch et al (1982)." [Govt. of Canada (Reviewer's comment ID #: 2004-186)]	Paragraph has been rewritten.
10-533	A	22:43	22:53	"There may also be considerable ice in the upper part of the soil and a large amount of latent heat would be required to melt the ice. The cited papers do not adequately acknowledge the role of excess ice and the latent heat requirements in their models and ignore the buffer provided by the ice-rich permafrost. This again leads to projections of thaw that are overestimates as permafrost degradation will be retarded in ice rich soils	Paragraph has been rewritten.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				(see for eg. Riseborough 1990). Observations of recent trends in temperatures in warm ice-rich permafrost at the southern fringe of the Canadian permafrost zone (Smith et al. 2005) indicate there is little change in permafrost temperature as as it approaches 0°C further illustrating the lag that occurs between increases in air temperature and disappearance of shallow ice-rich permafrost. " [Govt. of Canada (Reviewer's comment ID #: 2004-187)]	
10-534	A	22:43	22:53	"Another illustration of this lag effect can be found in Halsey et al. (1995) and Vitt et al. (1994). Thin permafrost is found in peatlands at the southern margin of the Canadian discontinuous permafrost zone (eg. northern Alberta) that likely formed during colder conditions of the Little Ice Age. Permafrost still exists in peatlands today, even though this region has experienced more than a century of warming, due to the insulating properties of the thick peat and the buffering effect provided by the ice-rich soil. Note that field observations (see for eg. Burgess and Smith 2003) indicate that at present thaw depth is less than 2 m in this region even though the ground has been warming for more than a century." [Govt. of Canada (Reviewer's comment ID #: 2004-188)]	Paragraph has been rewritten.
10-535	A	22:43	22:53	"It is also important to note that results of other modelling studies that do consider the heat sink provided by the underlying permafrost do not support the results cited in this section. Work by Burgess et al. (2000) for example shows that predicted thaw depths at Tuktoyaktuk (where permafrost is about 600 m thick), are less than 0.5 m over a 50 year period, increasing to depths of about 1.2 m where soils are ice poor (less where soil is ice-rich). This is considerably less than that predicted by Lawrence and Slater (2005) where thaw depths greater than 3.5 m are predicted in 100 years." [Govt. of Canada (Reviewer's comment ID #: 2004-189)]	Paragraph has been rewritten.
10-536	A	22:43	22:53	"Note that Lawrence and Slater (2005) acknowledge that a drawback representation of permafrost in their model is that the soil column is only 3.43 m thick and that thermal inertia related to deep frozen soils may mitigate the actual rate of deepening of the active layer, yet they still conclude that permafrost is reasonably simulated. Several permafrost scientists would disagree with their conclusions and those stated in this chapter and in fact, these concerns about the limitations of the model were raised by a number of permafrost scientists in the media following the publication of Lawrence and Slater (2005) last December." [Govt. of Canada (Reviewer's comment ID #: 2004-190)]	Paragraph has been rewritten.
10-537	A	22:43	22:53	"Given the rather significant limitations (some of which the authors themselves	Paragraph has been rewritten.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				acknowledge) of the models on which statements in this section are based, the limited validation of the model (limited comparison with field data) and the fact that field observations and results of other modelling studies do not support these results, caution should be used in the interpretation of the results and presentation in the IPCC. This also leads one to question other statements in this section regarding runoff since permafrost degradation is not adequately simulated. Has the fact that there will be subsidence of the ground (as ground ice melts) and ponding of water been considered in the runoff estimates? The impermeable nature of permafrost that remains below depths of a few metres does not appear to be considered as Lawrence and Slater (2005) assume that all ice is converted to water in 50 years, which will not be the case." [Govt. of Canada (Reviewer's comment ID #: 2004-191)]	
10-538	A	22:43	22:53	"References cited above: Lachenburch, A.H. Sass, , J.H., Marshall, B.B.and Moses, T.H. 1982. Permafrost, heat flow and the geothermal regime at Prudhoe Bay Alaska, J. Geophys. Res., 87:9301-9316. Riseborough, D.W. 1990. Soil latent heat as a filter of the climate signal in permafrost. Proceedings of the Fifth Canadian Permafrost Conference, Université Laval, Quebec, Collection Nordicana No. 54, p. 199-205. Smith, S.L., Burgess, M.M., Riseborough, D. and Nixon, F.M. 2005. Recent trends from Canadian permafrost thermal monitoring network sites. Permafrost and Periglacial Processes 16: 19-30. Halsey, L.A., Vitt, D.H. and Zoltai, S.C. 1995. Disequilibrium response of permafrost in boreal continental western Canada to climate change. Climate Change, v. 30 p. 57-73. Vitt, D.H., Halsey, L.A. and Zoltai, S.C. 1994. The bog landforms of continental western Canada in relation to climate and permafrost patterns. Arctic and Alpine Research, v. 26, p. 1-13. Burgess, M.M. and Smith, S.L. 2003. 17 years of thaw penetration and surface settlement observations in permafrost terrain along the Norman Wells pipeline, Northwest Territories, Canada. Proceedings of 8th International Conference on Permafrost, July 2003, Zurich Switzerland, M. Phillips, S.M. Springman and L.U. Arenson (eds.), A.A. Balkema, Lisse, the Netherlands, p. 107-112. Burgess M.M., Desrochers, D.T., and Saunders, R.2000. Potential changes in thaw depth and thaw settlement for three locations in the Mackenzie Valley; in The Physical Environment of the Mackenzie Valley, Northwest Territories: a Base Line for the Assessment of Environmental Change, (ed.) L.D. Dyke and G.R. Brooks; Geological Survey of Canada Bulletin 547, p. 187-195." [Govt. of Canada (Reviewer's comment ID #: 2004-194)]	Paragraph has been rewritten.
10-539	A	22:43	22:53	I recommend that this chapter delete its coverage of a paper by Lawrence and Slater (2005). This paper appears to have been inadequately reviewed, if at all. It suffers from extreme miscalibration (the extent of all Arctic permafrost is compared with the	Paragraph has been rewritten.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				distribution of empirical continuous permafrost and judged to be adequate). The zero-flux boundary at 3.43 m in the model ensures that conclusions about "melting" (sic) of permafrost are drastically overstated. There is thus no basis for the quantitative estimates of runoff made in the paper. [Frederick Nelson (Reviewer's comment ID #: 188-7)]	
10-540	A	22:43	22:53	The statements made in this section regarding the loss of permafrost are misleading and the models on which the conclusions are based have some rather important limitations. Given that these statements have been carried forward to the summary for policy makers and the Technical Summary (as robust findings) it is important that the editors have a better understanding of the limitations of the models and the proper interpretation of the results. From my understanding, all papers referenced in this section have been written by scientists that would most likely be considered climate modellers rather than permafrost scientists. The terminology that is used is not what would be used by the permafrost research community. A number of individual comments are offered below. [Sharon Smith (Reviewer's comment ID #: 244-58)]	Paragraph has been rewritten.
10-541	A	22:43	22:53	It should be noted that the conclusion regarding future permafrost extent relies heavily on a recent paper by Lawrence and Slater (2005) and further specific comments are offered below regarding this work. One must question how rigorous the review of this paper was when it was submitted to the journal October 31 2005, revised November 6 2005 and accepted November 15 2005. The review period was less than one week (note this one week, Oct 31-Nov 6 period included dissemination of the paper to reviewers by the editors, the review, return of comments to the editors and then revisions by the authors). [Sharon Smith (Reviewer's comment ID #: 244-59)]	Paragraph has been rewritten.
10-542	A	22:43	22:53	The models of Lawrence and Slater (2005), Yamaguchi et al (2005) and Kitabata et al. (2006) do not simulate permafrost distribution as they only consider the upper few metres of the soil (3.43 m depth for Lawrence and Kitabata models and 10 m for Yamaguchi). Because the models do not treat deeper levels, they can not adequately simulate permafrost extent and the maps produced in these papers have led to misinterpretations in the media. At best the results from these models provide information on changes in thaw depth, however there are still issues with the model which lead one to question any conclusions made about changes in thaw depth. [Sharon Smith (Reviewer's comment ID #: 244-60)]	Paragraph has been rewritten.
10-543	A	22:43	22:53	It would be more appropriate to say that there may be an increase in thaw depth over much of the permafrost region in response to climate warming rather than say that over half of the area covered by the top most layer of permafrost could thaw by 2050 and 90% by 2100. As mentioned in the comment above, the model simulates only very shallow ground temperature and thaw depth, not permafrost extent. Permafrost scientists would	Paragraph has been rewritten.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				not use the terminology that is used in this section and do not refer to thawing of the upper layer of permafrost (how is this defined?) but rather deepening of the active layer or increases in thaw depth. The terminology used here again is misleading and the statement does not make sense. [Sharon Smith (Reviewer's comment ID #: 244-61)]	
10-544	A	22:43	22:53	Note that the maps presented in papers such as that by Lawrence and Slater (2005) appear to indicate loss of large areas of permafrost. Again the map is misleading and what it really shows is the thaw depth. Areas where permafrost is indicated as absent are areas where the thaw depth has been predicted to be greater than 3.43 m. Since the deeper thermal regime is not simulated these authors can not say anything about the change in permafrost extent. [Sharon Smith (Reviewer's comment ID #: 244-62)]	Paragraph has been rewritten.
10-545	A	22:43	22:53	A more important concern with the papers cited in this section is the fact they only simulate the ground thermal regime in the upper few metres (as shallow as 3.43 m in the case of the Lawrence and Kitabata papers). Heat flow downward into the frozen soil below is not considered (there is a zero flux boundary at shallow depth), i.e heat flow is restricted to the upper few metres of the soil. This is simply not the case and setting a lower boundary at depths of 3.43 m or even 10 m (as in Yamaguchi paper) is not approriate for simulations of 50 to 100 years. [Sharon Smith (Reviewer's comment ID #: 244-63)]	Paragraph has been rewritten.
10-546	A	22:43	22:53	Heat is conducted into the soil well below depths of 3 to 10 m over time periods of 100 years. Ignoring the rather significant heat sink that is provided at depth by the frozen ground leads to a greater warming of the near surface soil than would actually be the case, and therefore greater thawing than would actually occur (i.e. there is an artificial warming of the upper part of the ground). This lag that occurs between changes in surface temperature and those at depth is discussed in Lachenbruch et al (1982). [Sharon Smith (Reviewer's comment ID #: 244-64)]	Paragraph has been rewritten.
10-547	A	22:43	22:53	There may also be considerable ice in the upper part of the soil and a large amount of latent heat would be required to melt the ice. The cited papers do not adequately acknowledge the role of excess ice and the latent heat requirements in their models and ignore the buffer provided by the ice-rich permafrost. This again leads to projections of thaw that are overestimates as permafrost degradation will be retarded in ice rich soils (see for eg. Riseborough 1990). Observations of recent trends in temperatures in warm ice-rich permafrost at the southern fringe of the Canadian permafrost zone (Smith et al. 2005) indicate there is little change in permafrost temperature as as it approaches 0°C further illustrating the lag that occurs between increases in air temperature and disappearance of shallow ice-rich permafrost.	Paragraph has been rewritten.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Sharon Smith (Reviewer's comment ID #: 244-65)]	
10-548	A	22:43	22:53	Another illustration of this lag effect can be found in Halsey et al. (1995) and Vitt et al. (1994). Thin permafrost is found in peatlands at the southern margin of the Canadian discontinuous permafrost zone (eg. northern Alberta) that likely formed during colder conditions of the Little Ice Age. Permafrost still exists in peatlands today, even though this region has experienced more than a century of warming, due to the insulating properties of the thick peat and the buffering effect provided by the ice-rich soil. Note that field observations (see for eg. Burgess and Smith 2003) indicate that at present thaw depth is less than 2 m in this region even though the ground has been warming for more than a century. [Sharon Smith (Reviewer's comment ID #: 244-66)]	Paragraph has been rewritten.
10-549	A	22:43	22:53	It is also important to note that results of other modelling studies that do consider the heat sink provided by the underlying permafrost do not support the results cited in this section. Work by Burgess et al. (2000) for example shows that predicted increases in thaw depths at Tuktoyaktuk (where permafrost is about 600 m thick), are less than 0.5 m over a 50 year period. For ice poor soils the thaw depth would increase to about 1.2 m over the 50 years (thaw depth would be less where soil is ice-rich). This is considerably less than that predicted by Lawrence and Slater (2005) where thaw depths greater than 3.5 m are predicted in 100 years. [Sharon Smith (Reviewer's comment ID #: 244-67)]	Paragraph has been rewritten.
10-550	A	22:43	22:53	Note that Lawrence and Slater (2005) acknowledge that a drawback of representation of permafrost in their model is that the soil column is only 3.43 m thick and that thermal inertia related to deep frozen soils may mitigate the actual rate of deepening of the active layer, yet they still conclude that permafrost is reasonably simulated. Several permafrost scientists would disagree with their conclusions and those stated in this chapter and in fact, these concerns about the limitations of the model were raised by a number of permafrost scientists in the media following the publication of Lawrence and Slater (2005) last December. [Sharon Smith (Reviewer's comment ID #: 244-68)]	Paragraph has been rewritten.
10-551	A	22:43	22:53	Given the rather significant limitations (some of which the authors themselves acknowledge) of the models on which statements in this section are based, the limited validation of the model (limited comparison with field data) and the fact that field observations and results of other modelling studies do not support these results, caution should be used in the interpretation of the results and presentation in the IPCC. This also leads one to question other statements in this section regarding runoff since permafrost degradation is not adequately simulated. Has the fact that there will be subsidence of the ground (as ground ice melts) and ponding of water been considered in the runoff	Paragraph has been rewritten.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				estimates? The impermeable nature of permafrost that remains below depths of a few metres does not appear to be considered as Lawrence and Slater (2005) assume that all ice is converted to water in 50 years, which will not be the case. [Sharon Smith (Reviewer's comment ID #: 244-69)]	
10-552	A	22:43	22:53	References cited above in comments on chapter 10: Lachenburch, A.H. Sass, , J.H., Marshall, B.B.and Moses, T.H. 1982. Permafrost, heat flow and the geothermal regime at Prudhoe Bay Alaska, J. Geophys. Res., 87:9301-9316. Riseborough, D.W. 1990. Soil latent heat as a filter of the climate signal in permafrost. Proceedings of the Fifth Canadian Permafrost Conference, Université Laval, Quebec, Collection Nordicana No. 54, p. 199-205. Smith, S.L., Burgess, M.M., Riseborough, D. and Nixon, F.M. 2005. Recent trends from Canadian permafrost thermal monitoring network sites. Permafrost and Periglacial Processes 16: 19-30. Halsey, L.A., Vitt, D.H. and Zoltai, S.C. 1995. Disequilibrium response of permafrost in boreal continental western Canada to climate change. Climate Change, v. 30 p. 57-73. Vitt, D.H., Halsey, L.A. and Zoltai, S.C. 1994. The bog landforms of continental western Canada in relation to climate and permafrost patterns. Arctic and Alpine Research, v. 26, p. 1-13. Burgess, M.M. and Smith, S.L. 2003. 17 years of thaw penetration and surface settlement observations in permafrost terrain along the Norman Wells pipeline, Northwest Territories, Canada. Proceedings of 8th International Conference on Permafrost, July 2003, Zurich Switzerland, M. Phillips, S.M. Springman and L.U. Arenson (eds.), A.A. Balkema, Lisse, the Netherlands, p. 107-112. Burgess M.M., Desrochers, D.T., and Saunders, R.2000. Potential changes in thaw depth and thaw settlement for three locations in the Mackenzie Valley; in The Physical Environment of the Mackenzie Valley, Northwest Territories: a Base Line for the Assessment of Environmental Change, (ed.) L.D. Dyke and G.R. Brooks; Geological Survey of Canada Bulletin 547, p. 187-195. [Sharon Smith (Reviewer's comment ID #: 244-72)]	Paragraph has been rewritten.
10-553	A	22:51	22:53	"It is important to note that the permafrost distribution that is simulated by Stendel and Christensen (2002) is the equilibrium distribution resulting from increases in air temperature over the next century. This permafrost distribution however will not be realized within this time period and there will be a considerable lag between changes in air temperature and changes in permafrost distribution (as discussed in detail above) - it will take much longer than a century for these changes to be realized. Stendel and Christensen (2002) acknowledge that they have simulated the potential permafrost distribution as they do not consider the heat transfer into the deeper ground which will occur slowly. It would therefore be more correct to say that over time, there may be a poleward movement of the extent of permafrost. " [Govt. of Canada (Reviewer's comment ID #: 2004-192)]	Paragraph has been rewritten.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-554	A	22:51	22:53	There is no time period given for the 30 to 40 % increase in active-layer thickness predicted by Stendel and Christensen (2002). There are also issues with these results as only the upper 5.7 m of the ground is modelled so that increases in active layer thickness may be overestimated (for reasons already discussed above). [Govt. of Canada (Reviewer's comment ID #: 2004-193)]	Paragraph has been rewritten.
10-555	A	22:51	22:51	Replace "movement" with "retreat" [Govt. of Finland (Reviewer's comment ID #: 2009-129)]	Paragraph has been rewritten.
10-556	A	22:51	22:53	It is important to note that the permafrost distribution that is simulated by Stendel and Christensen (2002) is the equilibrium distribution resulting from increases in air temperature over the next century. This permafrost distribution however will not be realized within this time period and there will be a considerable lag between changes in air temperature and changes in permafrost distribution (as discussed in detail above) - it will take much longer than a century for these changes to be realized. Stendel and Christensen (2002) acknowledge that they have simulated the potential permafrost distribution as they do not consider the heat transfer into the deeper ground which will occur slowly. It would therefore be more correct to say that over time, there may be a poleward movement of the extent of permafrost. [Sharon Smith (Reviewer's comment ID #: 244-70)]	Paragraph has been rewritten.
10-557	A	22:51	22:53	There is no time period given for the 30 to 40 % increase in active-layer thickness predicted by Stendel and Christensen (2002). There are also issues with these results as only the upper 5.7 m of the ground is modelled so that increases in active layer thickness may be overestimated (for reasons already discussed above). [Sharon Smith (Reviewer's comment ID #: 244-71)]	Paragraph has been rewritten..
10-558	A	23:2		rewrite as "some areas are projected to see increased snow extent during …" [Danny Harvey (Reviewer's comment ID #: 101-70)]	Accepted: sentence chang.
10-559	A	23:4	23:4	I suggest to add the following two new sentences after ending of sentence at line 4. "Multimodel simulation (7 members) indicates that considerable increase of snow amount is expected over Siberia in winter by the mid of 21st century and that is statistically significant. Furthermore, due to melting of the abundant snow amount in early spring, it can contribute to development of extensive flooding over major river basins of the Northern Eurasia (Meleshko et al., 2004)." Appropriate reference is the following: Meleshko V.P., V.M.Kattsov, V.A.Govorkova, S.P.Malevsky-Malevich, E.D.Nadjozhina, P.V.Sporyshev. 2004: Anthropogenic climate change in 21st century over Northern Eurasia. Meteorology and Hydrology, No.7, 5-26. Please also note that the next sentence in line 4 gives an impression that snow increase is not significant in northern regions. I disagree. In some regions do! Anyhow I think eccent must be a bit modified in the sentence begining from "However, the projected snow coverage changes. .". If you have	Accepted. Meleshko reference is added but we chose alternate wording to reflect comment.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				any questions on this proposal, please do not hasitate to communicate with me. [Valentin Meleshko (Reviewer's comment ID #: 175-4)]	
10-560	A	23:5	23:5	"are small" - This seems to contradict what is one page 22 Lines 30-39. [Ronald J Stouffer (Reviewer's comment ID #: 258-126)]	Accept: sentence removed (along with subsequent sentence)
10-561	A	23:10	23:24	Why changes in Greenland ice sheet are considered here and not those of the Antarctic ice sheet? Moreover, it could be more consistent to include this section into section 10.6.4 [Jean-Louis DUFRESNE (Reviewer's comment ID #: 60-3)]	Noted: Because of relationship with Atlantic MOC only cursory attention given here.
10-562	A	23:11	23:24	I presume that the "slightly negative Greenland ice sheet mass balance" referenced on line 13 is supposed to be modern (or averaged over some unspecified interval). There is much information on the mass balance of Greenland included in Chapter 4, including much more up-to-date observations, fairly strong evidence of accelerating mass loss recently, and indications that the imbalance over the last few years is sufficient to matter in assessments of sea-level rise and is not small compared to total snowfall. The text here is not consistent with chapter 4. [Richard B. Alley (Reviewer's comment ID #: 4-13)]	Accepted. Text now made consistent with Chapter 4
10-563	A	23:14	23:14	Insert after "margins" "but Zwally et al (2005) find an overall increase in Greenlamd mas between 1992 and 2002 [VINCENT GRAY (Reviewer's comment ID #: 88-1588)]	Rejected: No reason given for suggested change. Zwally now referenced.
10-564	A	23:15	23:16	Replace "a continuation of" with "in contrast to the evidence of Zwally el 2005, a" [VINCENT GRAY (Reviewer's comment ID #: 88-1589)]	Rejected: No reason given for suggested change. Zwally now referenced.
10-565	A	23:15	23:16	"continuation of the melting of Greenland" - Is this established? The net change is melting? Reference? [Ronald J Stouffer (Reviewer's comment ID #: 258-127)]	Reference in previous sentence
10-566	A	23:19	23:19	"see Figure 10.6.4 below" but no Figure 10.6.4 was found. [Chiu-Ying LAM (Reviewer's comment ID #: 139-17)]	Changed to Fig. 10.7.7
10-567	A	23:19	23:20	This is a general comment. I think all references to Greenland's behavior 1000 (or more) years from now are absurd. They all assume a world powered by fossil fuels (or some other carbon-emitting source) for the next millennium. History clearly demonstrates that technological changes are sufficient as to make this exceedingly unlikely. [Patrick Michaels (Reviewer's comment ID #: 176-29)]	Reject. There are thresholds beyond which Greenland may destabilise.
10-568	A	23:20		Would be useful to add that 0.1 Sv would contribute about 10 mm /yr in sea level rise. [Martin Manning (Reviewer's comment ID #: 155-76)]	Accepted.
10-569	A	23:23	23:24	What happens to the Arctic winter circulation after the melting of Greeland is (for the time being) an academic issue. Consider deleting.	Accepted. Sentence removed.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Govt. of Finland (Reviewer's comment ID #: 2009-131)]	
10-570	A	23:26	25:52	The whole discussion of the MOC mixes up collapse, abrupt changes, abrupt reduction, abrupt transitions, and so on without defining abrupt very clearly and without distinguishing between the various possible outcomes clearly. There is also no discussion of the fact that the model results differ widely in terms of the variability that they have for the MOC [Figure 10.3.13]. This suggests that MOC predictions have a long way to go and any conclusions drawn should tempered with caution. The whole section is a bit suspect in concluding that it is "very unlikely" that the MOC will undergo an abrupt transition (see comments below). [Meric Srokosz (Reviewer's comment ID #: 250-16)]	Reject. Abrupt change is clearly defined in box 10.1
10-571	A	23:34		I thought the revised MOC section read very well. It's a shame that several models are missing in Fig. 10.3.13. Can the missing models be added? (Let me know if you need help with the Hadley models) [Richard Wood (Reviewer's comment ID #: 294-17)]	We are trying to get more models included in the figure.
10-572	A	23:35		Fig 10.5.1b, c Doesn't look as if precip scales simply with temeprature- you should note the relvance of this fact to scaluing approaches for impacts. [John Mitchell (Reviewer's comment ID #: 180-11)]	
10-573	A	23:38	23:42	Here we see a departure from the statement (although refering to ensemble mean changes) on page 7, line 47 that all models are weighted equally. Two models are rejected on the basis of having a present day MOC strength "inconsistent" with estimates of the present day value and, although not stated in the text but rather in the figure caption, two models are also rejected for having large drifts. The latter seems appropriate (perhaps their curves should be removed from the figure to avoid confusion), but I am not sure the former is. Especially as these models are presumably used in the the multi-model mean figures. Is the conclusion altered considerably if these models are included? It rather begs the question of why are they not excluded from the multi-model ensemble mean plots and/or why aren't other metrics also used to accept or reject models. Thus opening up a huge can of worms. [Matthew Collins (Reviewer's comment ID #: 44-31)]	Rejected: We cannot selevtively remove models from our figures.
10-574	A	23:41	23:41	"cannot be" - Too strong? Is "will not be" better? [Ronald J Stouffer (Reviewer's comment ID #: 258-128)]	Accepted.
10-575	A	23:44	23:44	Figure 10.3.13 is very poor - as I noted in my earlier review. It is difficult to distinguish the various curves and impossible to compare with the equivalent figure in TAR. Some of the individual model results exhibit quite abrupt changes in the MOC but this is difficult to discern in the many overlaid curves. It needs to be improved. [Meric Srokosz (Reviewer's comment ID #: 250-13)]	Rejected: This figure appeared in similar form in peer-reviewed literature.
10-576	A	24:5	24:6	To say "no modelssimulates an abrupt reduction of the MOC within the 21st century" is	Sentence has been changed to refer to

No.	Batch	Page:line From	To	Comment	Notes
				misleading in the light of Figure 10.3.13 where some curves show abrupt changes - I can only assume that these have been dismissed as variability. Since the individula curves in the figure are difficult to see such a morrass is difficult to interpret (see comments on figure above). [Meric Srokosz (Reviewer's comment ID #: 250-14)]	abrupt shutdown.
10-577	A	24:13	24:15	Discussion of Gregory et al. resulst fails to note that the models considered have probably got the freshwater flux into the North Atlantic wrong (no Greenland Ice Sheet melting, and debatable how well they do precipitation and river run-off). Something should be said about this. [Meric Srokosz (Reviewer's comment ID #: 250-15)]	Rejected: This is no relevant to the discussion of Gregory et al.
10-578	A	24:14		This sentence should read: "Gregory et al. found that in models that do not include meltwater runoff from Greenland, the MOC reduction was caused more by changes in surface heat flux…" Otherwise it is misleading. [Stefan Rahmstorf (Reviewer's comment ID #: 206-14)]	Rejected: This is not relevant to the discussion of Gregory et al.
10-579	A	24:23	24:23	Insert after "1% year" "in contrast to the actual current rate of 0.4% a year" [VINCENT GRAY (Reviewer's comment ID #: 88-1590)]	Rejected: Not relevant to discussion
10-580	A	24:25	24:25	Change "high resolution" to "higher resolution". What is high is vague. [Ronald J Stouffer (Reviewer's comment ID #: 258-129)]	Accepted
10-581	A	24:33	24:35	I had difficulty understanding this sentence. [Richard B. Alley (Reviewer's comment ID #: 4-14)]	Accepted: sentence has been modified.
10-582	A	24:33	24:33	"these simulations" To what does "these refer? The refeence contains models that are not EMICs. [Ronald J Stouffer (Reviewer's comment ID #: 258-130)]	Accepted: sentence has been modified.
10-583	A	24:40	24:42	This statement is not fully consistent with the one in chapter 11, page 32, lines 23-24 which mentions "a small possibility of cooling" [Govt. of France (Reviewer's comment ID #: 2010-77)]	Chapter 11 will change their text.
10-584	A	24:44		"acts as a positive feedback" is wrong - it reinforces this change, but not as a positive feedback (which would be stronger the more warming there is), but simply as a superimposed delayed response to earlier changes. [Stefan Rahmstorf (Reviewer's comment ID #: 206-15)]	Accepted. Sentence changed.
10-585	A	24:45	24:49	Another physical reasoning for the increased Arctic heat transport is thatr the sea ice edge and associated deep water formation areas tend to move poleward in the warming world. [Ronald J Stouffer (Reviewer's comment ID #: 258-131)]	Unclear. No action taken.
10-586	A	24:52	24:52	Delete "robust" [VINCENT GRAY (Reviewer's comment ID #: 88-1591)]	Rejected: No reason given for why should be deleted
10-587	A	24:54	:56	The statement is formally correct, but appears to suggest that increased mixing is	Rejected. The statement is correct.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				somehow likely and may therefore stabilise the MOC, which is incorrect (the opposite is actually more probable). Something needs to be added to the effect that we do not actually know how stable the MOC is in the real world, or how close the present state may be to a possible bifurcation (see Marsh et al, loc cit, and references provided by Rahmstorf who makes the same point) [Govt. of United Kingdom (Reviewer's comment ID #: 2022-43)]	Question? How does the UK Government know about review comments provided by Rahmstorf?
10-588	A	25:7	25:7	The convection in the Lab Sea stopping in a 1% CO2 increase run is also discussed in Stouffer et al. 2006. Stouffer, R. J., T. L. Delworth, K. W. Dixon, R. Gudgel, I. Held, R. Hemler, T. Knutson, M. D. Schwarzkopf, M. J. Spelman, M. W. Winton, A. J. Broccoli, H-C. Lee, F. Zeng, and B. Soden, 2006: GFDL's CM2 global coupled climate models. Part IV: Idealized climate response. Journal of Climate, 19(5), 723-740. [Ronald J Stouffer (Reviewer's comment ID #: 258-132)]	Accepted. Reference addaed
10-589	A	25:23	25:32	This issue should be dealt with in the "common questions" (and in fact it is), as it is a simple tutorial on orbital changes causing ice ages, solely in response to a "popular notion" in parts of the general public. It is mispaced here as it is not part of the summary of the scientific literature on future projections, as no scientific paper has ever suggested a coming ice age due to MOC collapse; this simply is not a serious topic discussed in the scientific community. Since the chapter is far too long, and the issue is appropriately covered elsewhere, I propose to cut this entire paragraph. [Stefan Rahmstorf (Reviewer's comment ID #: 206-16)]	Accepted: This paragraph has been shortened and moved to Box 10.1
10-590	A	25:23	25:32	A cross-reference to Chapter 6, where glaciation is discussed, could be given here. [Adrian Simmons (Reviewer's comment ID #: 242-149)]	No longer apples.
10-591	A	25:23	25:32	This whole paragraph seems genuinely irrelvant to the discussion. No one seriously belives that the shutdown of the MOC will lead to an ice age. The space could better be used to note the many uncertainties assiciated with trying to predict the MOC - not least model resolution and failure to correctly represent the North Atlantic Current and NADW [8.3.2] - both important to the MOC. Plus the poor representation of freshwater flux into the ocean - again critical for the MOC. Rahmstorf et al.(2005, Geophys. Res. Lett.) results suggest that there may be thresholds and that AOGCMs may be too stable to allow the MOC to abrupt shutdown slowdown. [Meric Srokosz (Reviewer's comment ID #: 250-17)]	We have shortened the paragraph and moved it to Box 10.1
10-592	A	25:24		relatively solid is a substantial exaggeration: it would be more accurate just to say "plausible" or "reasonable" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-44)]	No longer applies
10-593	A	25:31	:32	This is a heroic statement ! Surely you should replace "it is not possible" by "there is no known mechanism", possibly adding "and it is not considered to be at all likely". [Govt. of United Kingdom (Reviewer's comment ID #: 2022-45)]	Statement has been modified.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-594	A	25:35	25:35	Insert after "Greenland" "(not confirmed by Zwally et al 2005, whi found a mass increase)" [VINCENT GRAY (Reviewer's comment ID #: 88-1592)]	Rejected: Not relevant in this context
10-595	A	25:47	25:52	The models for Greenland melt, just above, clearly do not capture the full dynamics, and so are likely (dare I say highly likely) to be in error, with the sign of the error such that Greenland is expected to lose mass faster than projected in those models, as explained in chapter 4. Thus, taking the model results as sufficiently definitive on rate of sea-level rise from Greenland that one can call the possibility of a major MOC change "highly unlikely" is overly confident, in my opinion. [Richard B. Alley (Reviewer's comment ID #: 4-15)]	We have added the words: "based on currently available simulations"
10-596	A	25:47		Please explain LSW [Jean-Louis DUFRESNE (Reviewer's comment ID #: 60-4)]	Accepted.
10-597	A	25:47		change "will reduce" to "will decrease" [Danny Harvey (Reviewer's comment ID #: 101-71)]	Accepted
10-598	A	25:48	25:48	For reasons given above change "very unlikely" to "unlikely" [Meric Srokosz (Reviewer's comment ID #: 250-18)]	Rejected: Very unlikely means < 10%.
10-599	A	25:48		change "very unlikely" to "probably unlikely", otherwise a confidence is suggested that is not based on the state of the science [Stefan Rahmstorf (Reviewer's comment ID #: 206-17)]	Rejected: Very unlikely means < 10%.
10-600	A	25:48		replace "very" by "probably" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-46)]	Rejeted: Very unlikely means < 10%.
10-601	A	25:50		add "but the possibility cannot be excluded" after "the end of the 21st century" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-47)]	Accepted
10-602	A	26:1	26:14	I wonder if it is worth discussing the following generic issues about the impact of climate change on variability after this paragraph. "Many of the studies reviewed below use long time-series from stable control or quasi-stable stabilisation experiments in order to find statistically significant changes in variability (the length and level of forcing are numerous). Changes in variability, if they occur, will happen against a background of relatively slow athropogenic changes in mean climate, but also in the presence of continuing natural variations in climate and possibly shorter-time scale forced variations brought about by, for example, large volcanic eruptions. Many variability phenomena, such as ENSO, already exhibit significant low-frequency fluctuations in amplitude and frequency of occurance so the ability to detect in future decades, with some high probability, anthropogenically-forced changes in variability occured may be limited. Nevertheless, as the background signal of climate change does strengthen, the impacts of climate change are likely to most strong during extremes of variable." Just some rambling thoughts really so please ignore if you wish.	Noted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Matthew Collins (Reviewer's comment ID #: 44-32)]	
10-603	A	26:2	26:2	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1593)]	Rejected: "anthropogenic" is widely used
10-604	A	26:8		« Räisänen (2002) also found an increase in monthly mean precipitation » variability [Jean-Louis DUFRESNE (Reviewer's comment ID #: 60-5)]	Accepted
10-605	A	26:10	26:10	note that this result is model dependent and that there is no consensus about increased variability in the tropical Pacific [Pascale DELECLUSE (Reviewer's comment ID #: 58-63)]	Accepted. Sentence deleted.
10-606	A	26:14	26:14	Add at end "But it should be remembered that the A2 scenario is highly improbable" [VINCENT GRAY (Reviewer's comment ID #: 88-1594)]	Rejected
10-607	A	26:17		10.3.5.2: Most of this section is simply about the mean monsoons, not their variability, so locating it in the section on changes in variability is confusing -- in the rest of the section, the seasonal cycle is not considered part of "variability". [Isaac Held (Reviewer's comment ID #: 105-57)]	Rejected. It is confusing trying to separate var and changes in variability.
10-608	A	26:18	26:51	The change in humidity is about 6%/K , assuming constant relative humidity which is approximately the case, and in the absence of structure and circualtions changes, should lead to a similar change in precipitation. Precipitation increases only 1 to 2%/K in the global mean. A weakening of circulation is one way this of helping reduce the efficiency of precipitation. [John Mitchell (Reviewer's comment ID #: 180-6)]	Noted.
10-609	A	26:18		« In the tropics, an increase of precipitation is projected in the Asian monsoon and African monsoon in JJA, » figure 10.3.6 shows this, except along the coast of Africa [Jean-Louis DUFRESNE (Reviewer's comment ID #: 60-6)]	Accepted. Text added indicating East African vs West African changes.
10-610	A	26:24	26:24	Insert after "As" "a projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1595)]	Accepted "As a projected warming will be faster over land than over the oceans"
10-611	A	26:24		The opener "As global warming will lead to faster warming over land than over the oceans" should be changed to "As GHG emissions are projected to lead to faster warming over land than over the oceans". Global warming is an effect, not a cause, and it comprises the contributions of warmings over land and oceans. [Adrian Simmons (Reviewer's comment ID #: 242-150)]	Taken into account. "As a projected warming will be faster over land than over the oceans"
10-612	A	26:30	26:33	I doubt that the weakening of the monsoonal circulations can be thought of usefully as caused by reductions in the middle to upper troposphere thermal gradients. A more useful causal progression would be (convection, divergence) => (rotational flow -- a la Gill) => (thermal gradients needed to balance rotational flow). The weakening of convection is most easily understood as simply due to the increased dry stability, resulting in less	Noted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				vertical motion for the same latent heat release. [Isaac Held (Reviewer's comment ID #: 105-54)]	
10-613	A	26:35	26:43	This paragraph could make reference to figure 10.3.6. [Matthew Collins (Reviewer's comment ID #: 44-33)]	Accepted. Will refer Figs. 10.3.6 and 10.3.9
10-614	A	26:40	26:40	Replace "shows" by "projects" [VINCENT GRAY (Reviewer's comment ID #: 88-1596)]	Rejected.
10-615	A	26:40	26:41	The statement that the AR4/PCMDI ensemble indicates increase in rainfall in the tropics and decrease in the subtropics is a too vague. The tropical increase is only robust in east Africa. The statement about the subtropics is immediately contradicted by the sentence to follow about moistening the Sahara. [Isaac Held (Reviewer's comment ID #: 105-55)]	Accepted. Text modified.
10-616	A	26:41	26:43	I have a serious problem with this sentence. The two studies cited are both with the same model (CCSM) as I understand it, and this model is an outlier among the AR4 models in its extent of moistening of the Sahara (see Ch. 11 supplementary material figures on Africa). The discussion of Haarsma et. al. in Ch. 11 in the SOD is confusing at present -- but I do not think that a simple correlation between Saharan SLP (or temperature) and Saharan or Sahel rainfall exists across the ensemble. I am sure that there is a "monsoonal - heat low" effect like this operating in the models, but I doubt that it is the predominant mechanism. For example, if one just increases CO2 in the GFDL model, holding SSTs fixed, Sahel and Saharan rainfall increase, bujt the opposite occurs if one increases SSTs simultaneously. In any case this should be harmonized with the Ch. 11 text. [Isaac Held (Reviewer's comment ID #: 105-56)]	Taken into account. Text modified.
10-617	A	26:41	26:43	This contradicts the view of Ch.11, which (rightly in my view) concludes that it is uncertain whether the Sahel will receive more or less rainfall in the future. [Dave Rowell (Reviewer's comment ID #: 222-48)]	Accepted. Text modified.
10-618	A	26:47	26:47	same remark as above [Pascale DELECLUSE (Reviewer's comment ID #: 58-64)]	Model dependent; noted.
10-619	A	26:57	26:57	Replace "show" with "projects" [VINCENT GRAY (Reviewer's comment ID #: 88-1597)]	Accepted
10-620	A	27:3	27:3	Replace "in the" with "in a projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1598)]	Rejected
10-621	A	27:6	27:6	Replace "cools" with "might cool" [VINCENT GRAY (Reviewer's comment ID #: 88-1599)]	Rejected
10-622	A	27:7	27:7	Replace "decreases" with "may decrease" [VINCENT GRAY (Reviewer's comment ID #: 88-1600)]	Rejected
10-623	A	27:15	27:37	Recent studies with IPCC AR4 coupled GCMs suggest that an important tropical SST	Accepted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				fingerprint to global warming is an enhanced equatorial warming relative to the subtropics. (Zhengyu Liu, Steve Vavrus, Feng He, Na Wen and Yafang Zhong. 2005: Rethinking Tropical Ocean Response to Global Warming: The Enhanced Equatorial Warming. Journal of Climate: Vol. 18, No. 22, pp. 4684–4700) [Govt. of China (Reviewer's comment ID #: 2006-69)]	
10-624	A	27:16	27:37	This paragraph is slighty self-contradictory. For example, (lines 23-24) "most realistic models show either no change in the mean state or a slight shift towards El Nino-like mean conditions" contradicts (lines 27-28) "many models with mostly an El Nino-like change". The issue is in how one defines El Nino-like changes. There are two approaches, one which looks at simple east-west gradients and the other which projects trends onto patterns of present day variability. The confusion has probably arisen because of the term El Nino-like (which I am guilty of using) which, I think, fails to capture many of the complex and regional aspects of the climate change patterns seen in AOGCMs in the tropical Pacific. For example, models can warm more in the east but also have a thermocline which shoals there. The whole issue is hampered by systematic biases in models in the region. Perhaps this paragraph could be reorganised to highlight the differences in approach in assessing trends and to add the caveat about the term "El Nino-like climate change" not capturing the whole story. [Matthew Collins (Reviewer's comment ID #: 44-34)]	Taken into account. Text modified.
10-625	A	27:16	27:16	Insert after "concentrations" "are supposed to" [VINCENT GRAY (Reviewer's comment ID #: 88-1601)]	Rejected.
10-626	A	27:16	27:22	From figures 10.3.5 and 10.3.6 it is almost impossible to distinguish any significant change in East-West SST gradient in the Pacific. Hence the phrase that states an 'El Niño-like change" of the mean state is misleading. If the models that best represent El Niño in current climate are kept, the diversity of their mean state zonal reponse in the tropical Pacific precludes any such statement. The next points discussed (lines 22-25) are sufficient here the make the point. [Eric Guilyardi (Reviewer's comment ID #: 91-2)]	Rejected. Changes in East-West SST gradient in the tropical Pacific is clear from the figures (see the difference between east of the dateline and west of it)
10-627	A	27:25	27:39	The discussion of Yamaguchi and Noda results is misleading and the term "El Niño-like" is both used for mean state and El Nino variability which confuses things (it is quite unfortunate this term is used at all to decribe a mean state change!). I suggest to reduce the emphasis put on this particular study which did not discriminate between model with correct El Niño variability and others. In consequence, I would remove figure 10.3.14 and suggest to find a figure more relevant to the executive summary statement (diversity of El Niño amplitude change for instance, cf van Oldenborgh et al. 2005 or Guilyardi 2006) [Eric Guilyardi (Reviewer's comment ID #: 91-3)]	Accepted. Figure 10.3.14 will be replaced to a new one.
10-628	A	27:26	27:33	This paragraph is concerned by some statement about the mean state in the equatorial	Rejected. See 10-626.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Pacific. Note that an evolution toward an "ENSO" state would mean a warming in the east stronger than a warming in the west. Some models may present such a pattern. However looking at figure 10.3.3, there is no obvious gradient in the equatorial Pacific in the model mean. I would suggest to be more careful about the evolution of the Pacific mean state. [Pascale DELECLUSE (Reviewer's comment ID #: 58-65)]	
10-629	A	27:27	27:28	the phrase "an ENSO-like warming global warming pattern is simulated by models with an El Nino-like change" does not say anything! [Danny Harvey (Reviewer's comment ID #: 101-72)]	Taken into account. Text modified. El Nino-like change indicates positive polarity of ENSO-like warming.
10-630	A	27:28	27:29	I presume you mean dry static stability. Moist static stability doesn't change much- hence the amplification of temperature increase with height in the tropic. Also, evaporative damping is less effective in the cooler east Pacific and contributes to faster warming in the east Pacific compared to the west [John Mitchell (Reviewer's comment ID #: 180-7)]	Taken into account. Changed to dry static stability.
10-631	A	27:32		Might it be worth mentioning that dynamical cold tongue effects envisioned by, e.g,Clement et al 1996, seem not to be crucial? (Or is that argument already sufficiently settled?) ref: Clement, A., R. Seager, M. A. Cane and S. E. Zebiak, 1996: An ocean dynamical thermostat, {\jclim} {\bf 9}, 2190--2196. [J. David Neelin (Reviewer's comment ID #: 187-34)]	Noted.
10-632	A	27:39		figure 10.3.14 refers to "HadGEM3" - should be either HadCM3 or HadGEM1? [Chris Jones (Reviewer's comment ID #: 120-38)]	Accepted. It is HadGEM1. Figure totally modified.
10-633	A	27:41	28:14	Again there are some contradictory statements coming from the literature. E.g. (line 48, page 27) "not statistically significant changes in amplitude of ENSO" and (line 6, page 28) "an increasing likelihood of increasing El Nino amplitude in the future". The bottom line being it depends on how you define El Nino, how you do your statistical test, what scenario you look at and what models you consider best (see also comment 31). The thing that jumps out too me when I look at all these studies is that no models produce really big changes in ENSO even at high CO2 forcing. The different conclusions about statistically significant changes show that the significance is marginal. I think the slight stronger conclusion that "it is unlikely [10-33% chance] that there will be any changes in ENSO amplitude or period in the coming century under the scenarios considered" is justified and does alow us to move along from the TAR position of not knowing anything. It also recognises that models have improved considerably in their ability to simulate ENSO. [Matthew Collins (Reviewer's comment ID #: 44-35)]	Accepted. Actually different literatures have different conclusions on this aspect. Our assessment is no significant changes in ENSO amplitude.
10-634	A	27:52	27:54	Increase in ENSO amplitude associated with stronger uppermost ocean stratification (due to dominance of surface warming) is reported by Chen et al. (2005), which is opposite, but the mechanism appears to be similar, to Meehl et al. (2006a) study.	Taken into account. Text deleted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Chen, X., M. Kimoto, and M. Takahashi, 2005: Changes in ENSO in response to greenhouse warming as simulated by the CCSR/NIES/FRCGC coupled GCM. SOLA, 1,149-152, doi: 10.2151/aola. 2005-039. [Masahide Kimoto (Reviewer's comment ID #: 127-7)]	
10-635	A	28:4	28:9	it is very difficult to reach a consensus from the results of different models. I agree with the conclusion of this paragraph which states that "there is no clear indication regarding future changes…." and I suggest to write more carefully the above lines which all tend to say that the amplitude of El Nino increases. [Pascale DELECLUSE (Reviewer's comment ID #: 58-67)]	Noted.
10-636	A	28:4	28:6	The statement made here by Guilyardi 2006 must be cautionned by the fact that there is considerable spread of El Niño behavior among the models and the changes in the subsurface thermocline properties that may be important for El Nino change could not be assessed. [Eric Guilyardi (Reviewer's comment ID #: 91-4)]	Accepted. Text deleted.
10-637	A	28:6		Is "an inceasing" the right word here. "a strong", "a high" or "a significant" would seem to fit in better with the words that appear in the preceding lines. If "increasing" remains, then it should be said previously that the models are getting better. [Adrian Simmons (Reviewer's comment ID #: 242-151)]	Text deleted.
10-638	A	28:16	28:20	This paragraph about ocean waves is a bit isolated. There are many factors that affect ENSO period and many mechanisms to explain ENSO variability that do not explicitly involve ocean waves. I think it could be omitted. [Matthew Collins (Reviewer's comment ID #: 44-36)]	Accepted. These two sentences are deleted.
10-639	A	28:30	28:31	following the above statement, I suggest to withdraw these two lines [Pascale DELECLUSE (Reviewer's comment ID #: 58-66)]	Rejected.
10-640	A	28:30	28:34	This summary needs to be corrected with the previous comments - last phrase is ok, can be update with summary statement above [Eric Guilyardi (Reviewer's comment ID #: 91-5)]	Taken into account. First sentence is modified.
10-641	A	28:30	28:34	Same comments as for 10-5, l.8-11: The changes in the amplitude of ENSO in the most realistic models over 2051-2100 are all of the same magnitude as the natural variability from 1850 to 2000, so a statement can be made: "Future changes of ENSO interannual variability differ from model to model. However, in the most realistic models the changes in amplitude up to 2100 do not exceed the observed variability since the mid-nineteenth century." [Govt. of Netherlands (Reviewer's comment ID #: 2016-50)]	See 10-226.
10-642	A	28:33	28:34	To first approximation, I don't think that the conclusion from the AR4 models is that there is wide disagreement about the future of ENSO -- to first order, the models that have relatively realistic ENSOs do not predict large changes in ENSO characteristics. This	Noted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				seems like an important conclusion. Focusing on the fact that they disagree about the character of these relatively modest changes seems besides the point. In none of these models does ENSO disappear or change period dramatically. [Isaac Held (Reviewer's comment ID #: 105-58)]	
10-643	A	28:46		Replace "global warming" by "GHG emissions" - see coment #150. [Adrian Simmons (Reviewer's comment ID #: 242-152)]	Taken into account. Text modified.
10-644	A	29:1	29:1	"This eastward shift is the expected response of an El Niño-like climate change" This holds true in the MPI model, but not in other models with a realistic ENSO behaviour. The MRI model has unrealistic ENSO feedback strengths, possbily related to the flux corrections. [Govt. of Netherlands (Reviewer's comment ID #: 2016-51)]	Taken into account. Text deleted.
10-645	A	29:2	29:2	Please also cite Oldenborgh, G.J. van and G. Burgers, Searching for decadal variations in ENSO precipitation teleconnections, Geophys. Res. Lett., 2005, 32, 15, L15701, doi: 10.1029/2005GL023110, who reached the same conclusions. [Govt. of Netherlands (Reviewer's comment ID #: 2016-52)]	Accepted.
10-646	A	29:9	29:9	Replace "can" by "is projected to" [VINCENT GRAY (Reviewer's comment ID #: 88-1602)]	Rejected
10-647	A	29:9	29:11	Change the last sentence to be consistent with the previous text. Change to: Some models predict no change in the ENSO-monsoon relationship, while others do". [Patrick Michaels (Reviewer's comment ID #: 176-30)]	Accepted. Text modified.
10-648	A	29:9	29:11	Decadal variability of the correlation between timeseries computed over a moving window can also occur because of 'sampling error', ie. the degree of in-phase/out-of-phase relationships varying purely by chance between particular periods. I think this is a different point to the 'internal variability' cause that is cited, and should not be neglected unless it is proved that the decadal changes in correlation are statistically significant. [Dave Rowell (Reviewer's comment ID #: 222-49)]	Taken into account. Text modified.
10-649	A	29:15	29:15	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1603)]	Rejected: "anthropogenic" is widely used
10-650	A	29:18	29:22	Add:"In the figure 10.3.6, it can be seen that the enhanced rainfall is also projected in East Asian monsoon." [Govt. of China (Reviewer's comment ID #: 2006-70)]	Rejected. Not relevant here.
10-651	A	29:29	29:30	The results of McHugh and Rogers (2005) also show an increase in NAO in most models (Fig. 3a in the final version of the paper), in good agreement with other studies. [Govt. of Finland (Reviewer's comment ID #: 2009-132)]	Accepted. Text deleted.
10-652	A	29:38	29:46	This paragraph on 20th-century changes probably belongs in chapter 9 [Govt. of Australia (Reviewer's comment ID #: 2001-411)]	Accepted. Paragraph deleted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-653	A	29:51	29:51	It seems very surprising that the mean pressure decrease in high latitudes would not exceed the intermodel standard deviation. Please check your calculations. [Govt. of Finland (Reviewer's comment ID #: 2009-133)]	Noted.
10-654	A	30:25	30:26	Is there really a current increasing trend in the NAO/AO? Didn't it broke down despite the continued Arctic warming? [Govt. of Sweden (Reviewer's comment ID #: 2020-17)]	Accepted. Text deleted.
10-655	A	30:28		In this paragraph some particular aspects of changes in the Southern Hemisphere, midlatitude circulation are discussed. Would it be possible to include something on the physical reasons behind changes in wave patterns and changes in the zonal mean circulation. One example of such a study is the paper: Brandefelt, J. and Källén, E., 2004: The response of the Southern Hemisphere atmospheric circulation to an enhanced greenhouse gas forcing. J. Climate, 17, 4425-4442. [Govt. of Sweden (Reviewer's comment ID #: 2020-16)]	Taken into account.
10-656	A	30:38	:40	By accounting for' is very vague - please explain [Steve Harangozo (Reviewer's comment ID #: 98-27)]	Taken into account. Text modified.
10-657	A	30:44	30:52	It should perhaps be noted here that these impacts of changes in annular modes are already included in the multi-model ensemble mean fields presented previously and should not be considered in addition to those changes. [Matthew Collins (Reviewer's comment ID #: 44-37)]	Rejected. This paragraph was added based on review comment on FOD.
10-658	A	30:54	30:54	Replace "likely" with "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1604)]	Rejected. A word of uncertainty.
10-659	A	30:57	30:57	This wording: i.e. that the SAM change would appear "more remarkably" than the NAM, is unclear and requires revision. [Govt. of Australia (Reviewer's comment ID #: 2001-412)]	Accepted. Text deleted.
10-660	A	30:57	:57	appear earlier and more remarkably' is very vague. More remarkable than what and in what way? [Steve Harangozo (Reviewer's comment ID #: 98-28)]	Accepted. Text deleted.
10-661	A	31:8	31:13	It is necessary to give clear definition of weather and climate extreme, respectively. [Govt. of China (Reviewer's comment ID #: 2006-71)]	This definition is given in Ch. 3—we now refer the reader to this point
10-662	A	31:16	31:16	Replace "an increased" with "a projected increased" [VINCENT GRAY (Reviewer's comment ID #: 88-1605)]	Accepted
10-663	A	31:28	31:28	Replace "an incresed" with "a projected increased" [VINCENT GRAY (Reviewer's comment ID #: 88-1606)]	Accepted
10-664	A	31:31	31:31	Replace "an incresed" with "a projected increased" [VINCENT GRAY (Reviewer's comment ID #: 88-1607)]	Accepted
10-665	A	31:31		Why necessarily "with increased evapotranspiration"?. The heavy episodic rainfall could	Accepted—clarify heavy runoff could

No.	Batch	Page:line From	Page:line To	Comment	Notes
				tend to run off, and plants could shut down their transpiration during extended dry spells. Some expansion of the remark could be given. [Adrian Simmons (Reviewer's comment ID #: 242-153)]	accompany heavy rainfall
10-666	A	31:46	31:46	Should state what "precipitation intensity" refers to: a definition of this would be useful. [Govt. of Australia (Reviewer's comment ID #: 2001-413)]	Accepted—definition given
10-667	A	31:46	31:46	Replace "increses" with " is projected to increase" [VINCENT GRAY (Reviewer's comment ID #: 88-1608)]	Accepted
10-668	A	31:47	31:47	Replace "increses" with " projection" [VINCENT GRAY (Reviewer's comment ID #: 88-1609)]	Rejected—projection clarification already given earlier in this sentence
10-669	A	31:48	31:52	I have also commented on this argument in ch. 9. I do not understand the claim that energy considerations limit the mean precip response. This is true in the global (and tropical) mean but not locally. [Isaac Held (Reviewer's comment ID #: 105-59)]	Accepted—"large scale" qualifier now added
10-670	A	31:52	31:54	Sentence starting "additionally, increases in the frequency…" is unclear, and could also do with some physical interpretation if it is considered a robust finding. [Govt. of Australia (Reviewer's comment ID #: 2001-414)]	Accepted—qualifier regarding timescale is included, though this is not a robust finding and denoted by the addition of "can"
10-671	A	31:54	31:57	The citation of Emori and Brown (2005) is not to the point. They partially showed the mechanism of the greater increase in extreme precipitation than that in the mean, which seems to be one of the important points in this paragraph, but it is not stated here. A better citation would be, for example, "The increase of mean and extreme precipitation in various regions has been attributed to contributions from both dynamic and thermodynamic processes associated with global warming (Emori and Brown, 2005). The greater increase in extreme precipitation than in the mean is attributed to the greater thermodynamic effect for the extremes due to increases of water vapour, in areas mainly over subtropics." [Seita Emori (Reviewer's comment ID #: 62-18)]	Accepted
10-672	A	32:2	32:6	"Recent studies ... better quantify this result using a gamma distribution to study precipitation extremes. For example, Kharin and Zwiers (2005)...Watterson and Dix (2003) showed...". Kharin and Zwiers did not use a gamma distribution (as mentioned) Watterson and Dix did (as mentioned) so the first intro sentence is a bit missleading as it suggests that they bot used a gamma distribution. Both distributions have a scale parameter but they are not the same. [Simon Brown (Reviewer's comment ID #: 32-1)]	Accepted
10-673	A	32:14	32:27	This paragraph is about flooding changes, and should state this clearly upfront. The paragraph should also be restructured to make particular points, then illustrate with references, rather than list findings in various studies. One example is the references of	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Watterson (2005) and McCabe et al (2001), which are discussed separately at the beginning and end of the paragraph, but both say the same thing. [Govt. of Australia (Reviewer's comment ID #: 2001-415)]	
10-674	A	32:15	32:15	Replace "showed" with "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1610)]	Rejected—no reason given for suggested change
10-675	A	32:16	32:16	Replace "an increase" with "a projected increase" [VINCENT GRAY (Reviewer's comment ID #: 88-1611)]	Rejected—no reason given for suggested change
10-676	A	32:18	32:25	The regional examples for Europe and Asia overlap with Section 11.3. Consider deleting. [Govt. of Finland (Reviewer's comment ID #: 2009-134)]	Ch. 11 now referred to
10-677	A	32:21	32:21	Replace "found" by "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1612)]	Rejected—no reason given for suggested change
10-678	A	32:22	32:25	The physics of the increase in river runoff seem very different than what is discussed above. This difference should be noted. The increase spring river flow is related to the increase wintertime precipitation and snow depth in high latitudes. It also seems related to the faster melt period. [Ronald J Stouffer (Reviewer's comment ID #: 258-133)]	Accepted
10-679	A	32:26	32:26	It is uncertain what "surface" cyclones refers to, this needs to be explained. [Govt. of Australia (Reviewer's comment ID #: 2001-416)]	Accepted—phrase now deleted
10-680	A	32:27	32:7	Add: "In contrast, Lins and Slack (1999) found no secular trend in maximum streamflow in the U.S." [Patrick Michaels (Reviewer's comment ID #: 176-31)]	Rejected—no complete reference given, and most regional changes are discussed in Ch. 11
10-681	A	32:29	32:44	Kamiguchi et al. (2006) analyzed changes in Frich's five precipitation-based extremes indices over land due to global warming using a global 20-km-mesh atmospheric model. The model projects that, at the end of the 21st Century under the IPCC SRES A1B scenario, heavy precipitation increases enormously in South Asia, the Amazon, and West Africa, while a dry spell increases in South Africa, south Australia, and the Amazon, suggesting that the risk of water-related disasters will be higher in these regions. In the Asian monsoon region, heavy precipitation increases notably in Bangladesh and in the Yangtze River basin due to the intensified convergence of water vapor flux in summer. In the Amazon, a dry spell greatly increases due to a reduction in the Walker circulation caused by an El Nino-like change in SST. Kamiguchi, K., A. Kitoh, T. Uchiyama, R. Mizuta and A. Noda, 2006: Changes in Precipitation-based Extremes Indices Due to Global Warming Projected by a Global 20-km-mesh Atmospheric Model. SOLA, 2, 64-67, doi:10.2151/sola.2006-017. [Masato Sugi (Reviewer's comment ID #: 259-4)]	Accepted
10-682	A	32:32	32:32	Reference should read figure 10.3.16 rather than 10.3.17	Accepted--Some editing has been

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Govt. of Australia (Reviewer's comment ID #: 2001-417)]	performed.
10-683	A	32:32	32:32	Reference to figure (10.3.17) wrong. Dry periods relates to fig 10.3.16 not 10.3.17 and elsewhere in this paragraph [Simon Brown (Reviewer's comment ID #: 32-2)]	Accepted
10-684	A	32:35	32:35	Reference should read figure 10.3.16 rather than 10.3.17 [Govt. of Australia (Reviewer's comment ID #: 2001-418)]	Accepted
10-685	A	32:42	32:44	It would be useful if this section would note that this analysis points towards the need to better understand the extent to which variables other than temperature scale with radiative forcing, or not. Figures 10.3.16 and 10.3.17 certainly suggest that scaling would not represent these extremes very well in the mean. [Susan Solomon (NOAA) (Reviewer's comment ID #: 247-4)]	Accepted
10-686	A	32:50	33:48	Uchiyama et al. (2006) analyzed changes in Frich's five temperature-based extremes indices over land due to global warming using a global 20-km-mesh atmospheric model. The model projects that, at the end of the 21st Century under the IPCC SRES A1B scenario, the total number of frost days (Fd) decreases by more than 20 days per year and the length of the growing season (GSL) increases by about 14-34 days in northern mid- and high latitudes. The heat wave duration index (HWDI) and the percentage of time with a daily minimum temperature above the present-day 90th percentile (Tn90) increase worldwide. The intra-annual extreme temperature range (ETR) decreases in northern high latitudes, east Asia, and eastern North America by 1.3-3.9°C, but it increases by 1.0°C in the Amazon. Uchiyama, T., R. Mizuta, K. Kamiguchi, A. Kitoh and A. Noda, 2006: Changes in temperature-based extremes indices due to global warming projected by a global 20-km-mesh atmospheric model, SOLA, 2006, Vol. 2, 68-71, doi:10.2151/sola.2006-018. [Masato Sugi (Reviewer's comment ID #: 259-5)]	Accepted
10-687	A	32:50		Generally section 10.3.6.2 on Temperature Extremes needs significant tightening to ensure it is more focussed and that a more coherent storyline emerges. [Govt. of Australia (Reviewer's comment ID #: 2001-419)]	Accepted—some editing has been performed
10-688	A	32:51	32:52	The first sentence refers only to increased risk of High temperature extremes, and therefore should state this as the risk of low-temperature extremes is actually reduced as also concluded by the TAR. [Govt. of Australia (Reviewer's comment ID #: 2001-420)]	Accepted
10-689	A	32:51	32:51	Delete "very likely" [VINCENT GRAY (Reviewer's comment ID #: 88-1613)]	Rejected—no reason given for suggested change
10-690	A	32:52	32:52	Replace "confirmed" by "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1614)]	Rejected—no reason given for suggested change

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-691	A	33:4	33:9	This paragraph is unclear. The definition of extremes is stated to be exceeding the 95th percentile - by definition this will not change in a warmer climate, so the nature of the changes discussed needs to be made explicit. This problem could be avoided by defining upfront what they mean by "extremes", and how changes in these are defined. [Govt. of Australia (Reviewer's comment ID #: 2001-421)]	Accepted—clarification added, and earlier in this section reference given to definition of extremes in Ch. 3
10-692	A	33:4	33:7	The 40% value is much lower than those reported in Ch. 11, where Table 11.2 indicates a large majority of the individual DJF and JJA seasons in late 21st century to be warmer than the single warmest DJF and JJA seasons in 1980-1999. The low values in Weisheimer and Palmer (2005) are associated with a peculiarity in their methodology. Please consult Ch. 11 authors for more information. [Govt. of Finland (Reviewer's comment ID #: 2009-135)]	Accepted. This has been revised.
10-693	A	33:6	33:6	Replace "rises" with "projected to rise" [VINCENT GRAY (Reviewer's comment ID #: 88-1615)]	Accepted
10-694	A	33:7	33:9	Shouldn't this very robust result be reflected somewhere in the Exec Summary? The current statement there on temperature extremes is purely qualitative and much less specific. [Martin Manning (Reviewer's comment ID #: 155-77)]	Executive summary reflects the result ofa very likely risk of increased temperature extremes
10-695	A	33:8	33:9	The claim that extreme temperatures are 20 to 100 times more frequent is a strong one, and it needs to be clarified exactly what this means. At present it is unclear. [Govt. of Australia (Reviewer's comment ID #: 2001-422)]	Accepted—clarification added
10-696	A	33:9	33:9	Delete from "and thus" to "result" [VINCENT GRAY (Reviewer's comment ID #: 88-1616)]	Rejected—no reason given for suggested change
10-697	A	33:11	33:11	Insert after "possible" "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1617)]	Phrase re-worded
10-698	A	33:13	33:16	Wording. Did the value of the wintertime mean change in the warmer climate? [Ronald J Stouffer (Reviewer's comment ID #: 258-134)]	Accepted—wording clarified
10-699	A	33:14	33:14	Insert after "future" "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1618)]	Rejected—no reason given for suggested change
10-700	A	33:18	33:18	Insert after "documented" "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1619)]	Rejected—no reason given for suggested change
10-701	A	33:20	33:20	Define "heat waves". [Govt. of Australia (Reviewer's comment ID #: 2001-423)]	Accepted
10-702	A	33:35	33:35	Insert after "future" "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1620)]	Rejected—no reason given for suggested change
10-703	A	33:38	33:38	Insert after "future" "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1621)]	Rejected—no reason given for suggested change

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-704	A	33:40	33:40	Replace "shown" by "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1622)]	Wording clarified
10-705	A	33:43	33:43	Insert before "results" "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1623)]	Rejected—no reason given for suggested change
10-706	A	33:44	33:45	The discussion of "growing season" as defined by frost days is relevant to only certain areas of the globe, and is most relevant to northern hemisphere mid-to high latitudes. This should be made clear, and also that in other regions growing season is likely to be controlled by other factors (such as precipitation amounts or soil moisture). [Govt. of Australia (Reviewer's comment ID #: 2001-424)]	Accepted
10-707	A	33:50		Section 10.3.6.3 Tropical cyclones (hurricanes) : no mention of the tracks changes, while chapter 8, page 40, line 53 to page 41, line2 put a strong amphasis on it [Govt. of France (Reviewer's comment ID #: 2010-78)]	Ch. 8 has now re-written that section, and does not discuss projections in any case
10-708	A	33:50		Section 10.3.6.3. Correction: Walsh et al. (2004) found a "56% increase in the number of storms of maximum wind speed greater than 30 ms-1", not a 56% increase in maximum wind speed. Otherwise a good section. [Kevin Walsh (Reviewer's comment ID #: 280-1)]	Accepted
10-709	A	34:1	23:1	Replace "climate change" with "change of climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1624)]	Rejected—reviewer has given no reason for suggested change
10-710	A	34:1	35:5	This section is too long. I am sure it could be reduced to half a page without loss of any significant points. [Martin Manning (Reviewer's comment ID #: 155-78)]	Rejected—reviewer gives no suggestions for how to make change
10-711	A	34:3	34:3	Replace word "higher" for "lower" in expression "..100 km resolution or higher cannot simulate observed tropical intensity…" [Valentin Meleshko (Reviewer's comment ID #: 175-5)]	Accepted
10-712	A	34:4	34:4	reference should be Yoshimura et al. 2006 [Ruth McDonald (Reviewer's comment ID #: 173-1)]	Accepted
10-713	A	34:4	34:6	There is a regional decrease in the North Pacific in Sugi et al. (2002) [Ruth McDonald (Reviewer's comment ID #: 173-3)]	Accepted
10-714	A	34:4	34:4	Citation "Yoshimura and Sugi, 2006" should be corrected as "Yoshimura et al., 2006". [Masato Sugi (Reviewer's comment ID #: 259-6)]	Accepted
10-715	A	34:4	34:22	The order of citations should be changed. The order should be Sugi et al.(2002), Yoshimura et al. (2006), McDonald et al.(2005), Hasegawa and Emori (2005), and then lower resolution models: Bengtsson et al.(2006), Tsutsui(2002). The citation of Yoshimura and Sugi (2005) should be moved (and merged) to Chapter10 page 35 lines 7-14. Following changes to lines 4-22 are suggested: A study with roughly 100 km grid spacing (T106) showed a decrease in tropical cyclone	Accepted

No.	Batch	Page:line From	To	Comment	Notes
				frequency globally but a regional increase over the North Atlantic and no significant changes in intensity (Sugi et al., 2002). Yoshimura et al. (2006) conducted an experiment using the same model but different SST patterns and two different convection schemes, and showed a decrease in global frequency of relatively week tropical cyclones but not significant change in the frequency of intense storms. They also showed that the regional changes were dependent on the SST pattern, and precipitation near the storm centers might increase in the future. In another global modelling study with roughly a 100 km grid spacing, there was a 6% decrease in tropical storms globally and a slight increase in intensity, with both increases and decreases regionally related to the El Niño-like base state response in the tropical Pacific to increased GHGs (McDonald et al., 2005). Another study with the same resolution model indicated decreases in tropical cyclone frequency and intensity but more mean and extreme precipitation from the tropical cyclones simulated in the future in the western north Pacific (Hasegawa and Emori, 2005). An AOGCM analysis with a more coarse resolution atmospheric model (T63, or about 200 km grid spacing) showed little change in overall numbers of the representations of tropical storms in that model, but a slight decrease in medium intensity storms in a warmer climate (Bengtsson et al., 2006). In a global warming simulation with a coarse resolution atmospheric model (T42, or about 300 km grid spacing), the frequency of global tropical cyclone occurrence does not show significant changes (Tsutsui, 2002). [Masato Sugi (Reviewer's comment ID #: 259-10)]	
10-716	A	34:5	34:5	Replace "showed" with "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1625)]	Rejected—reviewer has given no reason for suggested change
10-717	A	34:6	34:6	There are no significant changes in MAXIMUM intensity in Sugi et al. (2002) [Ruth McDonald (Reviewer's comment ID #: 173-2)]	Accepted
10-718	A	34:6	34:6	"that same resolution" should be "the same resolution" [Masato Sugi (Reviewer's comment ID #: 259-7)]	This section has been re-written
10-719	A	34:9	34:12	The text needs to point out that the study of Yoshimura and Sugi (2006) considers changes with fixed SSTs (as well as increased CO2 concentrations). The decreasing precipitation associated with the increasing CO2 (which they note) is not seen if SSTs are allowed to change, and in fact an increase in precipitation is usually found. This should be made clear, since it makes the results less relevant to the real world increased CO2 situation. [Govt. of Australia (Reviewer's comment ID #: 2001-425)]	Accepted
10-720	A	34:9	34:9	Replace "demonstrated" with "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1626)]	Rejected—reviewer has given no reason for suggested change
10-721	A	34:9	34:9	Citation "Yoshimura and Sugi, 2006" should be corrected as "Yoshimura and Sugi, 2005".	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Masato Sugi (Reviewer's comment ID #: 259-8)]	
10-722	A	34:10	34:11	See comment above- dry static stability increase, but I would thought moist static stability was more relevant in these regions. [John Mitchell (Reviewer's comment ID #: 180-8)]	We can only assess the available literature at this time.
10-723	A	34:11	34:14	This discussion (Yoshimura and Sugi (2006)) duplicates much of the discussion of this paper on the previous page, lines 9 to 12, and should be rationalised. [Govt. of Australia (Reviewer's comment ID #: 2001-427)]	Accepted—text has been revised
10-724	A	34:11	34:11	Insert before "decrease" "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1627)]	Rejected—reviewer has given no reason for suggested change
10-725	A	34:12	34:14	Yoshimura et al. (2006) also suggested that the global frequency of weak TCs may decrease but the frequency of intense TCs may either decrease or increase. This is an important point as it illustrates some of the uncertainty on model predictions of future changes. [Ruth McDonald (Reviewer's comment ID #: 173-4)]	Accepted
10-726	A	34:12	34:14	One of the most important conclusions of Yoshimura et al (2006) was a decrease in global frequency of "relatively week" tropical cyclones but not significant change in the frequency of "intense storms". Therefore, "Yoshimura et al.(2006) … in the future" should be changed as "Yoshimura et al. (2006) conducted an experiment using the same model but different SST patterns and two different convection schemes, and showed a decrease in global frequency of relatively week tropical cyclones but not significant change in the frequency of intense storms. They also showed that the regional changes were dependent on the SST pattern, and precipitation near the storm centers might increase in the future", and these sentences should be moved to line 6 after (Sugi et al., 2002). [Masato Sugi (Reviewer's comment ID #: 259-9)]	Accepted--Text has been re-written
10-727	A	34:13	34:13	Replace "showed" with "projected that" [VINCENT GRAY (Reviewer's comment ID #: 88-1628)]	Rejected—reviewer has given no reason for suggested change
10-728	A	34:18	34:18	Insert after "was" "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1629)]	Rejected—reviewer has given no reason for suggested change
10-729	A	34:20	34:22	Tsutsui (2002) found a that the mean intensity of the global TCs increased significantly in their model. [Ruth McDonald (Reviewer's comment ID #: 173-5)]	Accepted
10-730	A	34:24	34:24	Delete "credibly" [VINCENT GRAY (Reviewer's comment ID #: 88-1630)]	Rejected—reviewer has given no reason for suggested change
10-731	A	34:24	34:30	Maybe some mention should be on Knutson and Tuleya (2004) made here of the comments made by Michaels et al. (2005) and also the reply by Knutson and Tuleya	Rejected. This exchange did not change the main conclusions of the

No.	Batch	Page:line From	Page:line To	Comment	Notes
				(2005). Michaels, Patrick J., Knappenberger, Paul C., Landsea, Christopher Comments on CO2 }Impacts of CO2-Induced Warming on Simulated Hurricane Intensity and Precipitation: Sensitivity to the Choice of Climate Model and Convective Scheme Journal of Climate 2005 18: 5179-5182 Knutson, Thomas R., Tuleya, Robert E. Reply Journal of Climate 2005 18: 5183-5187 [Ruth McDonald (Reviewer's comment ID #: 173-11)]	original paper.
10-732	A	34:24	34:43	The study on changes in tropical cyclone tracks by Wu and Wang (2004) could be added to this section. They found changes in tracks in the N Atlantic. Wu, Liguang, Wang, Bin Assessing Impacts of Global Warming on Tropical Cyclone Tracks Journal of Climate 2004 17: 1686-1698 [Ruth McDonald (Reviewer's comment ID #: 173-12)]	Accepted.
10-733	A	34:24	34:30	Add after "in precipitation": "by the year 2080, using a 1%/year annual increase in atmospheric carbon dioxide". (Observed increases in recent decades have averaged less than half this value). While this study used simulated tropical cyclones and found that an average of 55% of model variance in these storms was related to SST, a real-world study of the Atlantic Basin (Michaels et al., JClim., 2005) found an explained variance of 11%. Knutson and Tuleya (2004) further noted that CO2- induced tropical cyclone changes are unlikely to be statistically detectable for several decades. [Patrick Michaels (Reviewer's comment ID #: 176-32)]	Clarification added, though Michaels reference is assessed elsewhere in the report, and other results are given in this section from other modeling studies in addition to Knutson and Tuleya
10-734	A	34:25	34:28	Need to say for what level (global) warming these percentage changes are found. [Govt. of Australia (Reviewer's comment ID #: 2001-426)]	Clarification added that this is at the time of TCR, and these numbers are given in the table in Ch. 8
10-735	A	34:25	34:28	replace Knutson and Tuleya sentence with this: For example, Knutson and Tuleya (2004) used a high resolution (down to 9km) mesoscale hurricane model to simulate hurricanes with intensities reaching about 60-70 m/sec, depending on the treatment of moist convection in the model. They used mean tropical conditions from nine global climate models with increased CO2 to simulate tropical cyclones with 14% more intense central pressure falls, 6% higher maximum surface wind speeds, and about 20% greater near storm rainfall after an 80-year build-up of CO2 at 1%/yr compounded. [Thomas Knutson (Reviewer's comment ID #: 132-1)]	Accepted
10-736	A	34:26	34:26	Replace "show" with "project that" [VINCENT GRAY (Reviewer's comment ID #: 88-1631)]	Rejected—reviewer has given no reason for suggested change
10-737	A	34:27	34:27	14% more intense central pressures needs clarification (alternatively, give the decrease in central pressure in hPa). [Govt. of Finland (Reviewer's comment ID #: 2009-136)]	This sentence has been re-written
10-738	A	34:38	34:38	56% increase in frequency of cyclones, not in windspeed [Govt. of Finland (Reviewer's comment ID #: 2009-137)]	This sentence has been revised

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-739	A	34:38	34:38	The 56% increase is in the number of stomrs with maximum windspeed greater than 30 m/s. This is not quite how this result is presented in the text. [Ruth McDonald (Reviewer's comment ID #: 173-6)]	Accepted
10-740	A	34:42	:42	Word missing after 'respect' [Steve Harangozo (Reviewer's comment ID #: 98-29)]	Accepted
10-741	A	34:45	35:5	This paragraph could be shortened. More detail information has been given for this study than for the lower resolution GCMs. I am not sure this is necessary. [Ruth McDonald (Reviewer's comment ID #: 173-7)]	Rejected—this study is more credible because the model is able to resolver relatively realistic tropical cyclones at high resolution
10-742	A	34:49	34:49	Should be corrected as " … added to the observed SSTs, … " [Masato Sugi (Reviewer's comment ID #: 259-11)]	Accepted
10-743	A	34:51	34:51	I suggest that windspeed is inserted before intensities at the end of the line, to make it clear that this is the measure of intensity being refered to, rather than precipitation of pressure. [Ruth McDonald (Reviewer's comment ID #: 173-8)]	Accepted
10-744	A	34:56	34:56	"simulated tropical cyclones in that model, although statistically significant increases were not found in all basins." [Thomas Knutson (Reviewer's comment ID #: 132-2)]	Accepted
10-745	A	35:7	35:7	Replace "showed" with "projected [VINCENT GRAY (Reviewer's comment ID #: 88-1632)]	Rejected—no reason given for suggested change
10-746	A	35:11	35:14	Chapter 10 page 34 Lines 9-12 should be moved here and merged. Some explanation on the regional changes should be added. Suggested changes to the lines 7-14 are: Sugi and Yoshimura (2004) and Yoshimura and Sugi (2005) further investigated a mechanism of the tropical precipitation change and the tropical cyclone frequency change. They showed that the effect of CO2 enhancement (without changing SST conditions) was a decrease in mean precipitation and a significant decrease in the number of tropical cyclones as simulated in a T106 atmospheric model, while the effect of SST increase (without CO2 enhancement) was an increase in mean precipitation with increased dry static stability and no significant change in the tropical cyclone frequency. Regarding the regional scale tropical cyclone frequency changes, Sugi et al (2002) and McDonald et al (2005) indicated that the changes in dynamical factors such as low level vorticity and vertical wind shear were responsible rather than thermo-dynamical factors such as SST and moist instability. [Masato Sugi (Reviewer's comment ID #: 259-13)]	Accpeted—text has been revised
10-747	A	35:12	35:12	Replace "showed" with "projected [VINCENT GRAY (Reviewer's comment ID #: 88-1633)]	Rejected—no reason given for suggested change

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-748	A	35:12		What does "without changing SST conditions" mean - that this was an atmosphere-only model with prescribed fixed SST? If that is the case, I would strongly suggest to not cite and discuss this paper - if you warm the atmosphere but keep SST fixed, what do you expect for effect on precipitation? You don't need a model to see it will lead to reduced evaporation and precipitation, but clearly this is nothing physical - likely even the sign of the effect will be wrong, compared to the coupled system. It is not the role of the IPCC to discuss model sensitivity studies in highly unrealistic settings if no robust conclusions about the real system can be drawn from them. [Stefan Rahmstorf (Reviewer's comment ID #: 206-18)]	This text has been revised
10-749	A	35:14	35:14	Citation "Yoshimura and Sugi, 2006" should be corrected as "Yoshimura and Sugi, 2005". [Masato Sugi (Reviewer's comment ID #: 259-12)]	Accepted
10-750	A	35:16	35:19	Tropical cyclone sythesis paragraph does not even mention uncertanty or model dependance so the impression is there there is none. Surely one of the main points to take away is that there is high uncertainty and results are model dependant. [Simon Brown (Reviewer's comment ID #: 32-3)]	Accepted—paragraph has been re-written
10-751	A	35:16	35:19	This sentence is too much affirmative concerning the impact of climate change on the intensity of tropical cyclones.. In addition, there is no mention of the specific studies which are synthetized. It cannot be the ensemble of studies mentioned in the sub-section as their results are to some extent contradictory as far as the intensity of tropical cyclones is concerned. [Govt. of France (Reviewer's comment ID #: 2010-79)]	
10-752	A	35:16	35:19	The summary makes no mention of the large uncertainties in future predictions of tropical cyclones. Most of the studies described are single simulations of relatively short length. It is hard to assess if the changes predicted by these models are outside natural variability. There is little agreement on the regional changes. [Ruth McDonald (Reviewer's comment ID #: 173-9)]	Accepted—paragraph has been re-written
10-753	A	35:16	35:19	Add that "These changes are not likely to be statistically detectable for several decades, or even longer, given the range of modeled or observed dependence of hurricane intensity on SST." That follows from the previous suggestion. [Patrick Michaels (Reviewer's comment ID #: 176-33)]	Paragraph has been re-written
10-754	A	35:16	35:19	The degree of confidence for an increased peak wind intensity is much lower than for an increased mean and peak precipitation intensities. In addition, there is no mention of the specific studies which are synthetized. It cannot be the ensemble of studies mentioned in the sub-section as their results are to some extent contradictory as far as the intensity of tropical cyclones is concerned. [Serge PLANTON (Reviewer's comment ID #: 199-4)]	Accepted—text has been re-written
10-755	A	35:18	35:18	The word "hurricanes" should be replaced by "tropical cyclones".	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Masato Sugi (Reviewer's comment ID #: 259-14)]	
10-756	A	35:18	35:18	The word "hurricanes" should be replaced by "tropical cyclones". [Masato Sugi (Reviewer's comment ID #: 259-15)]	Accepted
10-757	A	35:19	35:19	I would add the following caveat here: "However, it should be noted that almost all these experiments are done with atmosphere-only models. Coupling with ocean may alter the conclusion as it is known that SST cooling due to the passage of tropical cyclones affects the tropical cyclone intensity (Knutson et al., 2001)." Knutson, T. R., R. E. Tuleya, W. Shen, and I. Ginis, 2001: Impact of CO2-induced warming on hurricane intensities as simulated in a hurricane model with ocean coupling. Journal of Climate, 14(11), 2458-2468. (They found that the ocean effect was minor, but it may not generally be the case.) [Seita Emori (Reviewer's comment ID #: 62-19)]	Noted, but this is a summary assessment and individual studies are assessed in detail earlier, and, as noted, hte SST effect was noted to be minor
10-758	A	35:21		Section 10.3.6.4 This is the only section of the IPCC WG1 FAR that provides literature support for the SPM summary statement of "fewer but more intense mid-latitude storms", and the evidence is far from robust I my opinion, with section 10.3.6.4 poorly laid out. What evidence there is applies more to high latitudes (poleward of 60 degrees, say) rather than middle latitudes (around 40 degrees). There does indeed seem to be good evidence for a poleward shift of storm tracks in both hemispheres (Geng and Sugi, 2003; FischerBruns et al., 2005; etc) and the section should start with this. A number of regional studies are referenced (Leckebusch and Ulbrich, 2004; etc, including Cassano et al., 2006, and Lynch et al, 2006, which are very focussed on north and south polar regions, respectively). The regional studies should be clearly separated from the global studies. It is only the global studies that can be used to support the "fewer but more intense mid-latitude storms" statement, and here the evidence is weak or mixed. My interpretation is that the mid-latitude storms shift poleward and so there is less storminess in mid-latitudes and more in high latitudes; for the Southern Hemisphere, the decrease occurs around 40°S and the increase around 60°S, as clearly shown in maps produced by Fischer-Bruns et al. (2005) and Wang and Swail (2006b). The papers that claim increased intensity either have a flawed methodology for assessing intensity or do not partition the results by latitude. When cyclones move poleward, their central pressure drops, but this does NOT mean they are necessarily more intense. If the central pressure drops for cyclones in a narrowly-specified latitude band, then you could indeed claim increased intensity there, but no-one has done this sort of analysis as far as I am aware. The correct measure of cyclone intensity is vorticity, but even here you have to	Accepted—text has been revised

No.	Batch	Page:line From	Page:line To	Comment	Notes
				be careful in respect to spatial smoothing, as noted by Sinclair (1997: "Objective identification of cyclones and their circulation intensity, and climatology", Weather and Forecasting, 12: 595-612). Lambert and Fyfe (2006) and Fyfe (2003) use a flawed methodology which identifies cyclones solely by low central pressure. For the Southern Hemisphere, what they find is just the sub-Antarctic trough (ie, semi-permanent low centres), and for the Northern Hemisphere too their maps show maximum cyclone density well poleward of the true cyclone tracks. So the results of Lambert and Fyfe apply perhaps to the sub-Antarctic trough (in the Southern Hemisphere) but not to middle latitudes. Geng and Sugi (2003) use a realistic approach for identifying the cyclone tracks, but then stratify them into intensity classes according to the central pressure gradient. Thus, if there is a general tendency for poleward movement in a sample of cyclones, the central pressure will be lower (or conversely, the pressure gradient higher), but again this does NOT mean the cyclones are necessarily more intense in terms of circulation (ie, vorticity) or winds. The high latitude cyclones may be more intense, but the analysis is not very clear on this point – are the larger wave heights found by Wang and Swail (2006b), for example, due to stronger winds on average per cyclone, or by more cyclones at these latitudes? So I remain unconvinced that the evidence provided in the FAR supports the contention of increased cyclone intensity at middle latitudes. [A. Brett Mullan (Reviewer's comment ID #: 182-9)]	
10-759	A	35:23	35:23	Replace "shown" by "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1634)]	Rejected—reviewer has given no reason for suggested change
10-760	A	35:28	35:32	The density of strong cyclones only increased in the NH in JJA in Gen and Sugi (2003). [Ruth McDonald (Reviewer's comment ID #: 173-19)]	
10-761	A	35:29	35:29	Insert before "decrease" "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1635)]	Rejected—reviewer has given no reason for suggested change
10-762	A	35:31	35:31	Other earlier studies also showed this result e.g Carnell and Senior Clim Dyn 1998. [Ruth McDonald (Reviewer's comment ID #: 173-18)]	Accepted—text revised but older reference not included
10-763	A	35:31	35:31	Type-o…I think. Density should be intensity. Correct? If not, what is meant by density? [Ronald J Stouffer (Reviewer's comment ID #: 258-135)]	Accepted—density is the correct term and we add the definition in the text
10-764	A	35:32	35:32	"consistent": again the consistency is less obvious, as Geng & Sugi show mainly increased intensity of summer storms, whereas Lambert and Fyfe address winter storm mainly. [Bart Van den Hurk (Reviewer's comment ID #: 274-130)]	Accepted—phrase deleted
10-765	A	35:33	35:33	Replace "shown" by "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1636)]	Rejected—reviewer has given no reason for suggested change

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-766	A	35:33	35:35	Lambert 2004 could also be referenced here [Ruth McDonald (Reviewer's comment ID #: 173-20)]	Rejected—incomplete reference given
10-767	A	35:33	35:35	Bengtsson et al (2006) found fewer cyclones in NH and SH but no increase in the intense cyclones in the NH in a 3 member ensemble of ECHAM5-OM. This illustrates some of the uncertainty in the future changes. [Ruth McDonald (Reviewer's comment ID #: 173-21)]	The Bengtsson et al. Reference is cited for its main conclusion of a poleward shift of storm tracks, and now mentioned for no change in intensity.
10-768	A	35:37	35:37	Replace "showed" with "projected [VINCENT GRAY (Reviewer's comment ID #: 88-1637)]	Rejected—reviewer has given no reason for suggested change
10-769	A	35:41	35:43	Lambert and Fyfe (2006) did not find any obvious shift in storm stracks in AR4 climate change simulations. This could be mentionned here. [Sandrine Bony (Reviewer's comment ID #: 25-5)]	Accepted—but qualified by their methology that identified only central pressures that may not adequately show actual storm tracks
10-770	A	35:41	35:41	The signal is not as clear in Shubert et al 1998 as this statement seems to imply. Only the cyclone density shifted north in their model. The storm tracks shifted eastward or southeastward. [Ruth McDonald (Reviewer's comment ID #: 173-22)]	Accepted
10-771	A	35:41	35:46	There appears to be no poleward shifts in the cyclones in Watterson (2005), Inatsu and Kimoto (2005) or Lambert and Fyfe (2006). This suggests that there is some uncertainty in the poleward shift of the cyclones. [Ruth McDonald (Reviewer's comment ID #: 173-25)]	Accepted—poleward shift qualified
10-772	A	35:41	:46	It is said that « number of more recent studies have documented a poleward shift of several degrees longitude in midlatitude storm tracks ». Nevertheless, in their paper Lambert and Fyfe 2006 (referenced line 34 of the same page) said that «Even though the numbers of events change,there is no apparent change seen in the geographical distribution of the events,i.e. there is no obvious change in the positions of the storm tracks seen on hemispheric charts. This was also evident in the results for the filtered variance of the meridional wind which was used as a proxy for cyclone activity. In spite of this,it is possible that small shifts in the storm tracks,which are difficult to resolve with the relatively coarse grid used for analysis,could occur. » This point should be mentioned and/or clarify. [Jean-Louis DUFRESNE (Reviewer's comment ID #: 60-10)]	Accepted—language qualified
10-773	A	35:42	35:42	May be, my English is too poor:I don't understand the reason for including the word longitude [Govt. of France (Reviewer's comment ID #: 2010-80)]	Accepted—should be latitude
10-774	A	35:42	35:42	Replace "documented" by "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1638)]	Rejected—no reason given for suggested change

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-775	A	35:43	35:43	The poleward shift is not clear in the NH in Bengtsson et al. (2006). [Ruth McDonald (Reviewer's comment ID #: 173-23)]	Accepted—language qualified
10-776	A	35:43	35:43	Add in reference to Fyfe (2003) . [Ruth McDonald (Reviewer's comment ID #: 173-24)]	Rejected—further discussion of methodology of Fyfe indicates issues with using only central pressures to detect poleward shifts now given in text
10-777	A	35:44	35:44	Replace "showed" with "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1639)]	Rejected—no reason given for suggested change
10-778	A	35:53	35:53	Replace "showed" with "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1640)]	Rejected—no reason given for suggested change
10-779	A	36:1	36:1	Replace "shows" by "projects" [VINCENT GRAY (Reviewer's comment ID #: 88-1641)]	Rejected—no reason given for suggested change
10-780	A	36:1	36:1	The poleward shift in cyclones is not as clear a signal in the Northern Hemisphere as in the Southern Hemisphere. [Ruth McDonald (Reviewer's comment ID #: 173-26)]	Accepted—language qualified
10-781	A	36:1	:1	shows' should be 'show' [Steve Harangozo (Reviewer's comment ID #: 98-30)]	Accepted
10-782	A	36:6	36:6	Replace "shown" with "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1642)]	Rejected—no reason given for suggested change
10-783	A	36:6	36:7	Please replace the phrase "for most regions of the midlatitude oceans" with "for many regions of the middle and high-latitude oceans", because it is much more precise to say "… have shown that for many regions of the middle and high-latitude oceans, an increase of extreme wave height is likely to occur in a future warmer climate." [Xiaolan L. WANG (Reviewer's comment ID #: 282-22)]	Accepted
10-784	A	36:9	36:6	Replace "showed" with "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1643)]	Rejected—no reason given for suggested change
10-785	A	36:12	36:12	The title of this section is confusing, suggest revision to make the subject more clear. [Govt. of Australia (Reviewer's comment ID #: 2001-428)]	ACCEPTED : title changed
10-786	A	36:17	36:17	Surprised to see C4MIP was "supported" - how exactly? [Chris Jones (Reviewer's comment ID #: 120-22)]	NOTED : unnecessary information removed
10-787	A	36:18	36:18	"Ten" models? Should be 11. [Chris Jones (Reviewer's comment ID #: 120-23)]	ACCEPTED
10-788	A	36:23	36:23	the phrase "CO2 was radiatively inactive" is not quite accurate - that would result in a global cooling. Rather it's radiative forcing was held fixed at pre-industrial levels. [Chris Jones (Reviewer's comment ID #: 120-24)]	ACCEPTED : sentence clarified
10-789	A	36:24	36:26	Comment: This makes the C4MIP studies very questionable! As noted above, text on	REJECTED

No.	Batch	Page:line From	Page:line To	Comment	Notes
				page 14 makes it clear that the future pathway is likely to be somewhere below A1B because of observed emissions. So, coupling the observed value to A2 is about as inconsistent a scenario as could be imagined. The text needs to be altered to note this. [Patrick Michaels (Reviewer's comment ID #: 176-34)]	
10-790	A	36:30	36:30	Replace "will" by "might" [VINCENT GRAY (Reviewer's comment ID #: 88-1644)]	REJECTED : no reason given for suggested change
10-791	A	36:33	36:37	PRESENTATION: Merge this last section with the results presented from page 37, line 12 onwards in order to avoid redundancy. [Govt. of Germany (Reviewer's comment ID #: 2011-173)]	ACCEPTED
10-792	A	36:33	36:33	Add, "However, Chen et al. (2006) found that boreal photosynthesis increases more than respiration in warmer years". [Patrick Michaels (Reviewer's comment ID #: 176-35)]	REJECTED : these detailled processes are described in Chapter 7.
10-793	A	36:34	36:34	Replace "will" by "might" [VINCENT GRAY (Reviewer's comment ID #: 88-1645)]	REJECTED : no reason given for suggested change
10-794	A	36:34	36:34	better to say "affects" than "controls" - no one thing controls the carbon cycle. [Chris Jones (Reviewer's comment ID #: 120-25)]	ACCEPTED : sentence rephrased
10-795	A	36:36	36:36	Replace "leads" with "might lead" [VINCENT GRAY (Reviewer's comment ID #: 88-1646)]	REJECTED : no reason given for suggested change
10-796	A	36:36		Figure 10.4.3 COMMENT Full range of S scenarios (S350 upwards) need to be shown here to fully assess the literature and to deal with the mandate to be policy relevant (EU 2oC target for example imlplies S levels lower than S450 in the long term). [William Hare (Reviewer's comment ID #: 99-69)]	NOTED : Scenario SP450 has been added in figure 10.4.2. For figure 10.4.3 we use the same scenarios as the ones used in the TAR (450, 550, 650, 750, 1000).
10-797	A	36:37	36:37	Warming by when? Which climate sensitivity? [European Commission (Reviewer's comment ID #: 2008-52)]	NOTED : time frame clarified in the text, note that climate sensitivities vary amongst the C4MIP models
10-798	A	36:37	36:37	PRESENTATION: Amend the current statement "… and hence an additional warming ranging between 0.1 and 1.5°C..." with the time frame (warming by when? - supposedly by 2100) and the assumed climate sensitivity range implied by the C4MIP models. [Govt. of Germany (Reviewer's comment ID #: 2011-174)]	NOTED : time frame clarified in the text, note that climate sensitivities vary amongst the C4MIP models
10-799	A	36:37	36:37	Replace "an" by "a projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1647)]	REJECTED : no reason given for suggested change
10-800	A	36:37	36:37	Add "globally averaged SAT" before "ranging between". What scenario is used? Is this important? [Ronald J Stouffer (Reviewer's comment ID #: 258-136)]	NOTED : sentence modified, and ref to SRESA2 added
10-801	A	36:45	36:48	the bern model includes scenarios with negative CO2 feedback. Are these realistic?	REJECTED : the BERN model

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Corinne Le Quere (Reviewer's comment ID #: 143-8)]	configurations all have a positive feedback
10-802	A	36:55	36:55	Add at end "As before, because the A2 scenario is so improbable, the calculations must also be suspect" [VINCENT GRAY (Reviewer's comment ID #: 88-1648)]	REJECTED : no reason given for suggested change
10-803	A	36:55	36:55	Add this sentence. "It is important to note, that despite the use of the A2 scenario, Hansen and Sato (2004) have stated that the growth rates of climate forcings in recent years have been below all IPCC scenarios". [Patrick Michaels (Reviewer's comment ID #: 176-36)]	REJECTED : no reason given for suggested change
10-804	A	36:57		figure 10.4.1. be clearer about what the black lines in panel (b) are? Are they from the C4MIP GCMs with no feedbacks? Or are they "standard" GCMs with "standard" CO2 scenarios? Are they all forced with the black line from panel (a)? [Chris Jones (Reviewer's comment ID #: 120-39)]	ACCEPTED : legend clarified
10-805	A	37:7	37:15	The additional warming from the non-CO2 greenhouse gases would have increased the CO2 concentrations in the C4MIP simulations. Was this positive feedback taken into account in Fig. 10.4.1b? [Govt. of Finland (Reviewer's comment ID #: 2009-138)]	NOTED : Yes it was accounted for.
10-806	A	37:12	37:13	Why does the lower limit of the temperature range decrease in the C4MIP cases relative to the standard IPCC-AR4 models. This appears to contradict the assertion made elsewhere that the carbon cycle feedbacks are always calculated to the positive. Or is it that you are not actually comparing apples with apples here. [Martin Manning (Reviewer's comment ID #: 155-79)]	NOTED : , the lowest temperature change from the C4MIP models comes from the low TCR (1.4°C) of CSM1-climate-carbon cycle model(Fung et al, PNAS 2005).
10-807	A	37:15	37:15	Not adequtely reported in SPM, page 15, line 4 [Govt. of France (Reviewer's comment ID #: 2010-81)]	NOTED
10-808	A	37:15	37:15	Insert after "is" "projected to be" [VINCENT GRAY (Reviewer's comment ID #: 88-1649)]	REJECTED : no reason given for suggested change
10-809	A	37:15	37:15	Add at end "but all these calculations are affected by the extreme implausibility of the A2 scenario" [VINCENT GRAY (Reviewer's comment ID #: 88-1650)]	REJECTED : no reason given for suggested change
10-810	A	37:21	37:21	Replace "will" by "is projected to" [VINCENT GRAY (Reviewer's comment ID #: 88-1651)]	REJECTED : no reason given for suggested change
10-811	A	37:21	37:36	Full range of S scenarios (S350 upwards) need to be shown here to fully assess the literature and to deal with the mandate to be policy relevant (EU 2oC target for example imlplies S levels lower than S450 in the long term). [William Hare (Reviewer's comment ID #: 99-70)]	NOTED : Scenario SP450 has been added in figure 10.4.2. For figure 10.4.3 we use the same scenarios as the ones used in the TAR (450, 550, 650, 750, 1000).

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-812	A	37:26	37:26	Replace "climate change" with "changes in the claimate" [VINCENT GRAY (Reviewer's comment ID #: 88-1652)]	REJECTED : no reason given for suggested change
10-813	A	37:31	37:32	Replace "climate change" with "changes in the claimate" [VINCENT GRAY (Reviewer's comment ID #: 88-1653)]	REJECTED : no reason given for suggested change
10-814	A	37:32		after "reduction", I would add "relative to the case without climate-carbon cycle feedback" [Danny Harvey (Reviewer's comment ID #: 101-73)]	ACCEPTED
10-815	A	37:34	37:36	Here and in Figure 10.4.3.: Why not show results for lower SP scenarios as well? [European Commission (Reviewer's comment ID #: 2008-53)]	NOTED : , Scenario SP450 has been added in figure 10.4.2. For figure 10.4.3 we use the same scenarios as the ones used in the TAR (450, 550, 650, 750, 1000).
10-816	A	37:34	37:36	Next to SP1000 related results, state as well results for the lower-bound emissions scenarios, namely SP450 and SP350. [Govt. of Germany (Reviewer's comment ID #: 2011-175)]	NOTED : , Scenario SP450 has been added in figure 10.4.2. For figure 10.4.3 we use the same scenarios as the ones used in the TAR (450, 550, 650, 750, 1000).
10-817	A	37:38		figure 10.4.2 caption. Better reference for the Hadley simple model is Jones et al, 2006 book chapter which describes the stabilisation runs with it. Jones, C. D., Cox, P. M. and Huntingford, C. "Impact of Climate-CarbonCycle Feedbacks on Emission Scenarios to Achieve Stabilisation", chapter34 in "Avoiding Dangerous Climate Change". Eds: Schellnhuber, H. J.,Cramer, W., Nakicenovic, N., Wigley, T. and Yohe, G. CambridgeUniversity Press, 2006. [Chris Jones (Reviewer's comment ID #: 120-40)]	ACCEPTED : new caption
10-818	A	37:43	37:43	Give results for a 350 ppm stabilisation scenario instead. [European Commission (Reviewer's comment ID #: 2008-54)]	NOTED : Scenario SP450 has been added in figure 10.4.2. For figure 10.4.3 we use the same scenarios as the ones used in the TAR (450, 550, 650, 750, 1000).
10-819	A	37:43	37:43	Replace "S450" with "SP350" and include in rest of the text the results for a 350 ppm CO2 stabilization scenario. [Govt. of Germany (Reviewer's comment ID #: 2011-176)]	NOTED : Scenario SP450 has been added in figure 10.4.2. For figure 10.4.3 we use the same scenarios as the ones used in the TAR (450, 550, 650, 750, 1000).
10-820	A	37:56	38:5	Include results for an SP350 stabilization scenario. [Govt. of Germany (Reviewer's comment ID #: 2011-177)]	NOTED : Scenario SP450 has been added in figure 10.4.2. For figure 10.4.3 we use the same scenarios as the

No.	Batch	Page:line From	Page:line To	Comment	Notes
					ones used in the TAR (450, 550, 650, 750, 1000).
10-821	A	37:56	38:5	Full range of S scenarios (S350 upwards) need to be shown here to fully assess the literature and to deal with the mandate to be policy relevant (EU 2oC target for example imlplies S levels lower than S450 in the long term). [William Hare (Reviewer's comment ID #: 99-71)]	NOTED : Scenario SP450 has been added in figure 10.4.2. For figure 10.4.3 we use the same scenarios as the ones used in the TAR (450, 550, 650, 750, 1000).
10-822	A	38:4	38:4	1.5 to 4.5 C - Reference box 10.2 and dicussion in 10.5.2. [Ronald J Stouffer (Reviewer's comment ID #: 258-137)]	ACCEPTED
10-823	A	38:7	37:7	Add "in future projections" after "effect of land cover change". Some models had changing land cover in the historical runs. [Ronald J Stouffer (Reviewer's comment ID #: 258-138)]	ACCEPTED
10-824	A	38:7	38:26	add discussion here that climate affects land-cover through changes to vegetation (e.g. Cox et al., 2004, Theoretical and Applied Climatology, 78) and such vegetation changes have positive feedbacks on regional climate - especially precip (Betts et al., 2004, Theoretical and Applied Climatology, 78) [Chris Jones (Reviewer's comment ID #: 120-26)]	REJECTED : a detailled description of land cover interaction with climate is given in Chapter 7
10-825	A	38:23	38:23	Replace "found" by "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1654)]	REJECTED : no reason given for suggested change
10-826	A	38:28	38:39	When investigate the impacts of deforestation in the tropical region on the atmospheric GHG concentration, it is necessary to consider whether the original vegetation type is the tropical rain forest or the tropical seasonal forest. It is also necessary to consider whether the alternative vegetation is C3 type grass or C4 type grass. [Masato Sugi (Reviewer's comment ID #: 259-16)]	REJECTED : this is not the scope of this section
10-827	A	38:31	38:31	Replace "show" by "project" [VINCENT GRAY (Reviewer's comment ID #: 88-1655)]	REJECTED : no reason given for suggested change
10-828	A	38:39	38:39	Replace "dominates" by "is projected to dominate" [VINCENT GRAY (Reviewer's comment ID #: 88-1656)]	REJECTED : no reason given for suggested change
10-829	A	38:41	39:6	Ocean acidification. Ocean acidification due to CO2 will also likely lead to an increase in the base ammonia transferring from the atmosphere to the ocean and a lesser reduction in the removal of atmospheric acids (e.g., nitric acid, hydrochloric acid) from the atmosphere to the ocean: Jacobson, M.Z., Studying ocean acidification with conservative, stable numerical schemes for nonequilibrium air-ocean exchange and ocean equilibrium chemistry, J. Geophys. Res., 110, D07302, doi:10.1029/2004JD005220, 2005 This paper also calculated the potential future increase in ocean H+ by a factor of 2.5	Goes beyond scope of chapter.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				(decrease of pH from 8.14 to 7.85) by 2100 under the SRES A1B emission scenario and was able to account for the historical trend of CO2 in the atmosphere by accounting for emissions, ocean dissolution and transfer, and biomass burning/uptake. [Mark Jacobson (Reviewer's comment ID #: 116-33)]	
10-830	A	38:41		This section should include a paragraph describing the effect on acidification dur to ocean/atmosphere flux. [Govt. of Australia (Reviewer's comment ID #: 2001-429)]	REJECT: clearly stated at the beginning
10-831	A	38:41		It is important to provide a complete picture of the physical changes that can be expected in the oceans to allow WGII to provide a comprehensive discussion of the possible ecosystem response. As such, this section should include a paragraph explaining uncertainty in the models and, importantly, whether there will be differences in pH regionally and/or latitudinally. [Govt. of Australia (Reviewer's comment ID #: 2001-430)]	NOTED: section will be expandend.
10-832	A	38:41		Given the novelty and potential importance of research on ocean acidification, this section should be expanded. [European Commission (Reviewer's comment ID #: 2008-55)]	NOTED: section will be expandend.
10-833	A	38:41		Section 10.4.2: Please provide more specifics in this section. Two paragraphs on ocean acidification seems inappropriately constrained given the potential future implications on oceanic food chains. For example, discuss (the uncertainty of) the values presented in figures 10.4.4 and 10.4.5. Furthermore, provide the non-expert reader with more explanations on the importance of the aragonite and calcite saturation points, which are crossed at 500ppm (?) and 1000ppm CO2, respectively. [Govt. of Germany (Reviewer's comment ID #: 2011-178)]	NOTED: section will be expandend.
10-834	A	38:41		SECTION 10.4.2 Ocean Acidification due to Increasing Atmospheric Carbon Dioxide. This needs to be expanded to properly develop the assessment and to ensure completenes. Discussion should include refernce to pH, aragonite and calcite saturation levels vs pCO2 levels [William Hare (Reviewer's comment ID #: 99-72)]	NOTED: section will be expandend.
10-835	A	38:44	38:46	need to say that the pH decrease depends almost only on the concentration of atmospheric CO2 [Corinne Le Quere (Reviewer's comment ID #: 143-9)]	NOTED: section will be expandend.
10-836	A	38:45	38:45	Replace "already" by "calculated to be" [VINCENT GRAY (Reviewer's comment ID #: 88-1657)]	REJECT: statement based on measurements
10-837	A	38:45	38:45	Replace "will" by "could possibly" [VINCENT GRAY (Reviewer's comment ID #: 88-1658)]	ACCEPT: reworded

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-838	A	38:45	38:45	"will become" - How likely is this statement? Or change words to "could reduce". [Ronald J Stouffer (Reviewer's comment ID #: 258-139)]	ACCEPT: reworded
10-839	A	38:46	38:46	Does the IS92a scenario need a reference? Is it worth noting that the CO2 concentration is similar to the A2? [Ronald J Stouffer (Reviewer's comment ID #: 258-140)]	REJECT: differences between A2 and IS92a are significant.
10-840	A	38:47	38:37	Replace "will" by "could possibly" [VINCENT GRAY (Reviewer's comment ID #: 88-1659)]	ACCEPT: reworded
10-841	A	38:48	38:51	Need to give some indication over wht timescale the experimental conclusion would result' or link to next para. Better clarity in drafting would help the reader. [Govt. of Australia (Reviewer's comment ID #: 2001-431)]	NOTED: section will be expandend.
10-842	A	38:48	38:48	Insert after "carbinate " "unless evolution comes to thje rescue" [VINCENT GRAY (Reviewer's comment ID #: 88-1660)]	REJECT: unclear what is meant
10-843	A	38:48	38:51	these comments belong to chapter 7 [Corinne Le Quere (Reviewer's comment ID #: 143-10)]	OK
10-844	A	38:50	38:50	Insert after "skeletons" "without evolutionary development" [VINCENT GRAY (Reviewer's comment ID #: 88-1661)]	REJECT: not in scope of WG1
10-845	A	38:53	38:53	Replace "will" by "could conceivably" [VINCENT GRAY (Reviewer's comment ID #: 88-1662)]	NOTED: reworded
10-846	A	38:53	38:55	these are important comments and could be expanded to provide more information, especially regarding the timing of the projected undersaturation in the Southern Ocean. [Corinne Le Quere (Reviewer's comment ID #: 143-11)]	NOTED: section will be expandend.
10-847	A	38:53	38:55	need to say that the undersaturation depends not only on atmospheric CO2, but also on the local conditions (mainly temperature and alkalinity). [Corinne Le Quere (Reviewer's comment ID #: 143-13)]	NOTED: section will be expandend.
10-848	A	38:55	38:55	Replace "will" by "might" [VINCENT GRAY (Reviewer's comment ID #: 88-1663)]	REJECT: correct usage of will. Chemical consequence with little uncertainty.
10-849	A	38:57	38:57	Replace "will" by "might" [VINCENT GRAY (Reviewer's comment ID #: 88-1664)]	REJECT: correct usage of will. Chemical consequence with little uncertainty.
10-850	A	38:57	39:2	these comments belong to chapter 7 [Corinne Le Quere (Reviewer's comment ID #: 143-12)]	OK
10-851	A	39:1	39:1	Insert after "threaten" "some" [VINCENT GRAY (Reviewer's comment ID #: 88-1665)]	REJECT: some not needed as marine organisms are specified, ie those which calcify.
10-852	A	39:2		It would be helpful if the chapter would cite a numeric range for the pH decrease by 2100	ACCEPT: ES modified.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				shown in Figure 10.4.5 as we are carrying that numeric range into the SPM. [Martin Manning (Reviewer's comment ID #: 155-80)]	
10-853	A	39:8		The text in this section should move up and be combined with the previous text on emissions. Here it is confusing and somewhat redundant. [European Commission (Reviewer's comment ID #: 2008-56)]	TAKEN INTO ACCOUNT – The point of this section is to point out how chemistry responds to climate change, not to document future changes in emissions.
10-854	A	39:8		The discussion on the emission scenarios in this section 10.4.3 belongs to a newly-to-be created emission section, which is to be inserted before the current section 10.2 for clarity, transparency and to avoid redundancy. In terms of the latter just one example: page 14, line 32-34 deals with the recent methane abundances observations, as well as page 40, line 37. This redundancy and others can be avoided by providing a slightly improved structure for this chapter 10 along Figure 10.1.1 "Emissions->concentrations->radiative forcing->temperature & precipitation -> sea level"... [Govt. of Germany (Reviewer's comment ID #: 2011-179)]	See comment 10-853.
10-855	A	39:18	39:18	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1666)]	REJECTED -- NO REASON FOR CHANGE GIVEN.
10-856	A	39:23	39:23	Replace "will" by "might" [VINCENT GRAY (Reviewer's comment ID #: 88-1667)]	ACCEPTED
10-857	A	39:25	39:25	Replace "find" with "project" [VINCENT GRAY (Reviewer's comment ID #: 88-1668)]	ACCEPTED
10-858	A	39:32	39:32	Insert after "emissions" "may" [VINCENT GRAY (Reviewer's comment ID #: 88-1669)]	ACCEPTED
10-859	A	39:33	39:33	Insert before "leading" "possibly" [VINCENT GRAY (Reviewer's comment ID #: 88-1670)]	ACCEPTED
10-860	A	39:36	39:42	Future ozone levels will also depend on the future behaviour of vegetation - e.g. Sanderson et al (2003, GRL) showed that future changes in vegetation in response to climate change lead to lower growth of surface ozone due to reduced VOC emissions from vegetation. [Chris Jones (Reviewer's comment ID #: 120-27)]	ACCEPTED
10-861	A	39:41	39:41	Replace "grows" with "may grow" [VINCENT GRAY (Reviewer's comment ID #: 88-1671)]	ACCEPTED
10-862	A	39:41	39:41	Replace "is a;lso" by "may also be" [VINCENT GRAY (Reviewer's comment ID #: 88-1672)]	ACCEPTED
10-863	A	40:5	40:5	Replace "results" with "projections" [VINCENT GRAY (Reviewer's comment ID #: 88-1673)]	ACCEPTED

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-864	A	40:14	40:14	Replace "climate change" with "chamge of climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1674)]	REJECTED -- NO REASON FOR CHANGE GIVEN.
10-865	A	40:14	40:14	Replace "increases" with "projects an increase" [VINCENT GRAY (Reviewer's comment ID #: 88-1675)]	ACCEPTED
10-866	A	40:18	40:18	Replace "find" with "project" [VINCENT GRAY (Reviewer's comment ID #: 88-1676)]	ACCEPTED
10-867	A	40:21	:22	Is this because of the decrease in dry static stability at the tropopause?- warmer troposphere, colder strat- easier for descending motion. [John Mitchell (Reviewer's comment ID #: 180-9)]	ACCEPTED – The reasons for the change in strat-trop exchange are now given.
10-868	A	40:27	40:27	Replace "climate change" with "chamge of climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1677)]	REJECTED -- NO REASON FOR CHANGE GIVEN.
10-869	A	40:27	40:35	the paragraph opens with a reference to increasing water vapour and temperature impacting the tropospheric O3 concentration, but then goes on to explain only how increasing water vapour impacts tropospheric ozone. A coupled of lines on the direct effect of warmer temperatures on tropospheric O3 is also warranted. [Danny Harvey (Reviewer's comment ID #: 101-74)]	ACCEPTED – The effects of temperature are now also discussed.
10-870	A	40:27	40:45	Johnson et al (2001, GRL) found a 27% increase in CH4 levels in a control run compared with a climate change run where OH was increased. [Chris Jones (Reviewer's comment ID #: 120-28)]	ACCEPTED – This study is now referenced.
10-871	A	40:28	40:28	Replace "find" with "project" [VINCENT GRAY (Reviewer's comment ID #: 88-1678)]	ACCEPTED
10-872	A	40:37	40:37	Replace "indicate" with "show" [VINCENT GRAY (Reviewer's comment ID #: 88-1679)]	ACCEPTED
10-873	A	40:37	40:37	Insert after "declone" "to zero" [VINCENT GRAY (Reviewer's comment ID #: 88-1680)]	SEE COMMENT 10-874.
10-874	A	40:37	40:37	Add: "and were negative for several years in the early 21st century". Source is figure 2.5 from the current draft. [Patrick Michaels (Reviewer's comment ID #: 176-37)]	ACCEPTED
10-875	A	40:49	41:2	Gedney et al (2004, GRL) found an increase of about 5% of total global radiative forcing due to increased CH4 emissions from wetlands under climate change [Chris Jones (Reviewer's comment ID #: 120-29)]	ACCEPTED – This study is now referenced.
10-876	A	40:53	40:54	SRES emission scenarions only considered direct anthropogenic emissions. [European Commission (Reviewer's comment ID #: 2008-57)]	ACCEPTED
10-877	A	40:53	40:54	The current sentence "The original SRES scenarios for gases other than CO2 only considered changes in direct anthropogenic emissions" seems unclear, if not wrong. SRES emissions scenarios were only about direct anthropogenic emissions. Changes in natural	ACCEPTED

No.	Batch	Page:line From	Page:line To	Comment	Notes
				CO2 emissions and sinks (terrestrial or oceanic fertilization and feedback effects) are not considered within the SRES scenarios. Please correct. [Govt. of Germany (Reviewer's comment ID #: 2011-180)]	
10-878	A	41:4	41:44	It could be mentionned in this section that the projected temperature change for the 21st century will be remarkably independent of the effects of anthropogenic sulfate aerosols for the SRES A2 scenario (Dufresne et al., GRL, 2005). [Sandrine Bony (Reviewer's comment ID #: 25-7)]	REJECTED – This section just deals with the changes in chemistry due to anthropogenic climate change.1
10-879	A	41:47	41:47	I think you should add a other subsection in 10.5 that discusses possible missing future forcings. The most important new point is that of the impact of a large volcano on the projections. Solar, land use, abrupt releases of CH4 from wetlands/permafrost/ocean hydrates and etc. could also be discussed. Referencing section 8.7. [Ronald J Stouffer (Reviewer's comment ID #: 258-141)]	A sentence has been added to 10.5.1, noting the potential for these forcings or feedbacks to increase uncertainty in future changes, and referring to section 8.7 (section 10.4.4 has also been revised to note studies including the estimated impact of future natural forcings). However we do not think it is necessary to add an entirely new sub-section in 10.5 devoted to these issues, as they are already covered in Chapter 8.
10-880	A	41:50	41:50	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1681)]	Rejected as no reason given for change.
10-881	A	41:50		Replace "is injected" with "arises" [Martin Manning (Reviewer's comment ID #: 155-81)]	Changed as suggested.
10-882	A	41:51	41:52	This uncertainty does not consider non-mitigation scenarios, and is therefore smaller than in reality. [European Commission (Reviewer's comment ID #: 2008-58)]	Noted, but the intention here is just to provide a reference which supports the general point that emissions are uncertain, so no text changes are needed.
10-883	A	41:51	41:52	Please clarify that future emission uncertainties are in reality wider than implied by the referenced SRES report, which only had a mandate to deal with non-mitigation scenarios. Thus clarify for the reader, that only a part of the future emission uncertainty is dealt with in this chapter, namely the uncertainty among non-mitigation scenarios. [Govt. of Germany (Reviewer's comment ID #: 2011-181)]	The Introduction has been revised to emphasise this point.
10-884	A	41:51	41:52	This sentence "The specification of future emissions of greenhouse gases, aerosols and their precursors is uncertain" does not capture the fact that this section does not deal with the mitigation scenarios. [William Hare (Reviewer's comment ID #: 99-73)]	The Introduction has been revised to emphasise this point.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-885	A	41:52	42:14	I find this text rather repetitive, laboured and pedantic. Surely it can be shortened significantly. [Martin Manning (Reviewer's comment ID #: 155-82)]	The text emphasising the conditional nature of uncertainty information obtained from ensembles of models, and the potential impact of structural model errors, has been shortened where possible. However it is important to state such caveats carefully and completely, as noted in the IPCC Uncertainty Guidance notes, so a somewhat pedantic list of issues is, unfortunately, unavoidable, given that reviewers have insisted on their inclusion in earlier drafts.
10-886	A	41:55	41:56	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1682)]	Rejected as no reason given for change
10-887	A	41:56	41:56	errors in processes, or missing processes [Chris Jones (Reviewer's comment ID #: 120-30)]	Missing processes do indeed introduce additional uncertainty, however this point is already made in the discussion of structural uncertainty later in the same paragraph, so there is no need to change the text on this line.
10-888	A	41:57	41:57	Replace "unforced internal climate variability" by "miscellaneous clmate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1683)]	Rejected, as the need in this sentence is for a term that isolates the impact of variability generated by the climate system in the absence of changes in external forcing. The phrase "unforced internal climate variability" captures this meaning more precisely than "miscellaneous climate influences".
10-889	A	42:1	42:1	Replace "internal variability" by "miscellaneous clmate influences" [VINCENT GRAY (Reviewer's comment ID #: 88-1684)]	See response to 10-888.
10-890	A	42:1	42:1	Insert after "can be" "vaguely" [VINCENT GRAY (Reviewer's comment ID #: 88-1685)]	Rejected. There is indeed a caveat that one needs to assume that simulated internal variability is consistent with the real world, but this caveat is already included in the sentence.
10-891	A	42:2	42:1	Replace "internal variability is " by "miscellaneous clmate influences are"	See response to 10-888

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[VINCENT GRAY (Reviewer's comment ID #: 88-1686)]	
10-892	A	42:12	42:12	Should read Appendix 9A. [David Sexton (Reviewer's comment ID #: 233-3)]	In fact, Appendices 9A and 9B both mention Bayesian methods. However Appendix 9B lays out the steps involved in Bayesian inference in more detail, so we prefer to retain the existing reference as the better pointer
10-893	A	42:18	42:57	If it is not too complicated, it would be better to avoid the acronym "SCMs" because in the community it is used both for "Simple Climate Modesl" and for "Single-Column Models". [Sandrine Bony (Reviewer's comment ID #: 25-6)]	The text in Chapter 8 does not use the acronym SCM, and does not discuss single column models, so our use of SCM in Chapter 10 does not seem to cause any internal inconsistency in the AR4 report. Further, readers of this section would more readily identify simple climate models by the acronym SCM, rather than some other alternative, so we believe it is better to stick to this usage, given that the term is clearly defined, and the context is clear.
10-894	A	42:43	42:46	The absolute statement that only AOGCMs are the only tool is overstated. For example, simpler and more limited models can better simulate variability such as ENSO and abrupt change. While in principle, an AOGCM including everything with high resolution must be best, this is not the current situation. Suggest deleting this paragraph. [Haroon Kheshgi (Reviewer's comment ID #: 125-43)]	This paragraph does not seek to imply that GCMs are the only tool, and we think it would be quite wrong to delete the paragraph explaining the role of AOGCMs, while retaining the (longer) paragraphs above explaining the roles of SCMs and EMICs. We have changed the text to emphasise better the intended point, which is that only AOGCMs can, in principle, simulate comprehensively the full range of processes giving rise to climate change, variability and extremes. We think this is reasonable, while agreeing with the reviewer that simpler, more highly parameterised models can simulate some aspects of variability and change,

No.	Batch	Page:line From	Page:line To	Comment	Notes
					particularly at larger scales, realistically.
10-895	A	42:46	42:46	Worth noting the expensive nature of running AOGCMs? [Ronald J Stouffer (Reviewer's comment ID #: 258-142)]	This point is made in the previous paragraph, where we note that it is not yet possible to run very long simulations or large ensembles with AOGCMs. This text has been changed to refer specifically to computational limitations.
10-896	A	43:4		Why are simple climate models not considered? [European Commission (Reviewer's comment ID #: 2008-60)]	No change on the text. Section 10.5.2 discusses model responses to forcing based on the physics and processes underlying the model. Simple models are usually tuned towards more complex models, so their response is determined more by the complex to which they are tuned rather than by the underlying physics, so a discussion of model response is not meaningful here for simple models. The purpose of simple models is discussed in section 10.5.1, and applications are given in section 10.5.3.
10-897	A	43:4		Section 10.5.2 "Range of Responses from Different Models" is missing a subsection on the third category of models next to AOGCMs and EMICs, namely Simple Climate Models. Please provide a section on Simple Climate Models, and their specific purpose. [Govt. of Germany (Reviewer's comment ID #: 2011-182)]	No change on the text. Section 10.5.2 discusses model responses to forcing based on the physics and processes underlying the model. Simple models are usually tuned towards more complex models, so their response is determined more by the complex to which they are tuned rather than by the underlying physics, so a discussion of model response is not meaningful here for simple models. The purpose of simple models is discussed in section 10.5.1, and applications are given in section 10.5.3.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-898	A	43:7	43:16	You could mention that equilibrium climate sensitivity and TCR estimates for each AR4 model are given in Table 8.8.1. [Sandrine Bony (Reviewer's comment ID #: 25-8)]	Accepted.
10-899	A	43:7	43:7	Replace "anthropogenic" with "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-1687)]	Rejected.
10-900	A	43:10	43:11	Add "The observed rate of increase in the last three decades has been slightly less than one-half of this value". [Patrick Michaels (Reviewer's comment ID #: 176-38)]	Rejected. WG1 does not assess scenarios.1%/yr is an academic scenario widely used in the literature to compare model responses, like climate sensitivity and TCR. Academic scenarios are not intended to mimic the observed CO2 increase.
10-901	A	43:11	43:11	Insert after "CO20" "(as opoosed to the measred value of 0.4%) [VINCENT GRAY (Reviewer's comment ID #: 88-1688)]	Rejected. WG1 does not assess scenarios.1%/yr is an academic scenario widely used in the literature to compare model responses, like climate sensitivity and TCR. Academic scenarios are not intended to mimic the observed CO2 increase.
10-902	A	43:14		« These two measures have become standard metrics... » these measures are quantities, they are not metrics, therefore I would propose « These two measures have become standard to quantify how an AOGCM... » [Jean-Louis DUFRESNE (Reviewer's comment ID #: 60-7)]	Accepted.
10-903	A	43:25	:26	Fig 10.5.1a Doesn't look so good at lower sensitivities [John Mitchell (Reviewer's comment ID #: 180-10)]	Noted, no changes on the text. The agreement is worse for low sensitivities from other EMICs, but reasonably good for the AOGCMs
10-904	A	43:26	43:26	should read "Bern2.5D EMIC covers" instead of "EMIC covers" to avoid confusion with other EMICs in fig10.5.1a. [David Sexton (Reviewer's comment ID #: 233-4)]	Accepted.
10-905	A	43:33	43:33	It would be helpful to note that this graph is for global mean at equilibrium and for doubled CO2, and that the apparent linearity between temperature and precipitation responses may not hold for other forcings such as e.g., aerosols, or on smaller scales. [Susan Solomon (NOAA) (Reviewer's comment ID #: 247-5)]	Accepted. Added sentence as suggested.
10-906	A	43:37	43:51	Much of this is very repetitive with Box 10.2. Why not locate the box about here and significantly shorten the main text. [Martin Manning (Reviewer's comment ID #: 155-83)]	Party accepted. Text is significantly shortened as suggested. The location of the box needs to be decided on.

No.	Batch	Page:line From	Page:line To	Comment	Notes
					However, the fact that PDFs of sensitivity based on climatology (which are referred to in the box) are discussed in 10.5.4.2 suggests that the box should be placed after that.
10-907	A	43:38	43:38	The TCR values quoted here and those shown in figures 10.5.1 and 10.5.6 should probably be checked against each other to ensure they are equivalent. My multi-model TCR calculations are shown in the legend in 10.5.6 [Matthew Collins (Reviewer's comment ID #: 44-38)]	Accepted. TCR values are taken from table 8.8.1 and are the ones supplied by the individual modelling groups. They might differ slightly depending on how model control drift is accounted for. The SOD Fig. 10.5.6 is not in the final draft, and the numbers of table 8.8.1 will be used consistently across all chapters.
10-908	A	43:38		Box 10.2 cites the range as 2.0 to 4.5 (page 73, line 35), here it is 2.0 to 4.4. The ranges need to be consistent. [Martin Manning (Reviewer's comment ID #: 155-84)]	Accepted. Fixed to be consistent.
10-909	A	43:45		" most studies aiming to constrain climate sensitivity by observations do indeed indicate a log-normal distribution of climate sensitivity". Climate sensitivity is just an unknown single number and it cannot have any distribution. At least the word "probability" should be mentioned here. [Andrey Ganopolski (Reviewer's comment ID #: 80-5)]	Accepted. Added the word probability.
10-910	A	43:48	43:51	This is pretty ugly stuff. What exactly is "the upper limit of climate sensitivity"? I'm sure you realise that "the pdf of climate sensitivity" is not actually an object with any physical reality which is to be estimated, but merely a representation of your beliefs concerning the single-valued parameter of climate sensitivity. Nevertheless, such clumsy language does seem to have played a part in shaping the views which are commonly to be found in this document and the literature. [James Annan (Reviewer's comment ID #: 6-16)]	Paragraph reworded and shortened, comment no longer applies. Synthesis on climate sensitivity is now exclusively in box 10.2.
10-911	A	43:48		« On the other hand, there is a consensus from most studies cited above using observational constraints (see Section 10.5.4.) that the upper limit of climate sensitivity is uncertain, with a substantial probability for sensitivity above 4.5°C, and that the current AOGCMs therefore do not cover the full possible range of sensitivities. » This is significantly different from the statement of chapter 9, page 9-62, line 23-26 « Constraints based on observed climate change support the assessment (see Chapter 10, Box 10.2) that equilibrium climate sensitivity is likely in the range of 2.0 to 4.5°C with a most likely value around 3.0°C. Values substantially higher than 4.5°C cannot be excluded, but	Paragraph reworded and shortened, comment no longer applies. Synthesis on climate sensitivity is now exclusively in box 10.2., and consistent with chapter 9.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				agreement with observations and proxy data is generally worse for those high values than for values in the 2 4.5°C range. » One key point of chapter 9 statement is that « agreement with observations and proxy data is generally worse for those high values... ». This is probably the reason why AOGCMs dont' have very high sensitivity values. This should be specify here. [Jean-Louis DUFRESNE (Reviewer's comment ID #: 60-8)]	
10-912	A	43:48		"there is a consensus… that the upper limit of climate sensitivity is uncertain". I don't think this is accurate. More accurate is: "there is a consensus…… that it is difficult to constrain the upper limit of climate sensitivity with 20th century data, due to the uncertainty in aerosol forcing". That is quite a different statement, and it is what these studies actually show. None of these studies have made the upper limit more uncertain than it was in the TAR - they simply failed to make it less uncertain with this particular data constraint, because it is an ineffective data constraint. [Stefan Rahmstorf (Reviewer's comment ID #: 206-19)]	Comment mostly correct. Paragraph reworded and shortened, comment no longer applies. Synthesis on climate sensitivity is now exclusively in box 10.2
10-913	A	44:3	44:4	Again, this confuses zero emissions with constant radiative forcing. [European Commission (Reviewer's comment ID #: 2008-59)]	Accepted. Sentence reworded to "about half of the projected warming would occur as a result of radiative being held constant at year 2000 levels (constant composition commitment)"
10-914	A	44:3	44:4	The current sentence "… and over the next few decades, about half of the projected warming is the commitment warming already caused by well known changes in radiative forcing in the past" is wrong for the simple reason that the analyzed commitment is a "constant composition / current radiative forcing" commitment, not a zero-emission commitment (cf. Wigley 2005; Hare and Meinshausen, 2006). Thus rephrase to "... and over the next few decades, about half of the projected warming is the warming that would result, if radiative forcing were held constant at 2000 levels, implying significant emission reductions of 30 to ~80% for greenhouse gases (cf. Question 10.3. Figure 1)." [Govt. of Germany (Reviewer's comment ID #: 2011-183)]	Accepted. Sentence reworded to "about half of the projected warming would occur as a result of radiative being held constant at year 2000 levels (constant composition commitment)"
10-915	A	44:3	44:4	This sentence is not quite right and would be made more precise by rewording to "about half of the projected warming would occur as a result of radiative being held constant at year 2000 levels (constant forcing commitment)". The "radiative forcing in the past" is not committed to in the future... [William Hare (Reviewer's comment ID #: 99-74)]	Accepted. Sentence reworded to "about half of the projected warming would occur as a result of radiative being held constant at year 2000 levels (constant composition commitment)"
10-916	A	44:6	44:7	These two lines seem to have become separated from their context. What is the antecedent of "these metrics" on line 6? [Martin Manning (Reviewer's comment ID #: 155-85)]	Paragraph deleted.
10-917	A	44:6	44:7	This short paragraph appears isolated and does not read well. It would be better attached	Paragraph deleted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				to the end of the previous paragraph. [Keith Williams (Reviewer's comment ID #: 290-8)]	
10-918	A	44:9	44:19	These two paragraphs seem a bit misplaced in this section. I would suggest rather to refer to chapter 8, and more specifically to section 8.6.2.3 (entitled "What explains the current spread in models' climate sensitivity estimates?" and to section 8.6.3.2.2 (entitled "Interpretation of the range of cloud feedbacks among climate models"). In case you choose to keep the first paragraph, please note that Bony and Dufresne (2005) have not analyzed CFMIP slab models but AR4 coupled models. I would also suggest to remove the last sentence of the paragraph ("Improvements in simulation of low stratus for the current climate in some models have dramatically altered the contribution of changes in low cloud amounts to the climate sensitivity (Webb et al. 2006)") because we don't have any evidence to support this statement (and actually I don't think it was a conclusion from Webb et al. (2006)!). [Sandrine Bony (Reviewer's comment ID #: 25-9)]	Paragraph deleted and reference provided to section 8.6, which discusses this in detail.
10-919	A	44:9	44:15	Bony and Dufresne only look at the tropics so Webb et al 2006 should be cited for sea ice region and BOTH Bony and Dufresne and Webb et al should be cited for low stratus clouds. Final sentence should be deleted as I am not aware that Webb et al made this point, though it was pointed out in the paper that prognostic cloud water schemes seem to have lower sensitivities. But there was no general statement about improvements leading to dramatic alterations. [David Sexton (Reviewer's comment ID #: 233-5)]	Paragraph deleted and reference provided to section 8.6, which discusses this in detail.
10-920	A	44:9	44:15	This paragraph appears to be an (inaccurate) repetition of part of Chapter 8. It does not fit well with the rest of the discussion in this sub-section and I suggest it is completely removed. My particular concerns are in the next 3 comment entries. [Keith Williams (Reviewer's comment ID #: 290-9)]	Paragraph deleted and reference provided to section 8.6, which discusses this in detail.
10-921	A	44:9	44:10	Whilst the first sentence is not inaccurate, just discussing boundary-layer cloud here may suggest to the reader that ALL of the uncertainty in climate sensitivity is due to this cloud type. Whilst much of the variation is associated with low cloud, there is some contribution to the range of climate sensitivity from, for example, the response of mid-latitude frontal cloud. The discussion in Chapter 8 has been carefully worded to account for this - I suggest this paragraph in Chapter 10 is just replaced by a reference to section 8.6 [Keith Williams (Reviewer's comment ID #: 290-12)]	Paragraph deleted and reference provided to section 8.6, which discusses this in detail.
10-922	A	44:13	44:15	There is no basis for the final sentence in Webb et al 2006. Indeed, there is no evidence presented anywhere in Chapter 8 for an improvement in low cloud (see comment 3). [Keith Williams (Reviewer's comment ID #: 290-10)]	Paragraph deleted and reference provided to section 8.6, which discusses this in detail.
10-923	A	44:13	44:13	The CFMIP results should be referenced to Webb et al. 2006 rather than Bony and Dufresne and "sea ice region" should be "sea ice and snow covered regions".	Paragraph deleted and reference provided to section 8.6, which

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Keith Williams (Reviewer's comment ID #: 290-11)]	discusses this in detail.
10-924	A	44:21	44:49	This section should reference the discussion in 8.8. [Ronald J Stouffer (Reviewer's comment ID #: 258-143)]	Paragraph deleted and reference provided to section 8.6, which discusses this in detail.
10-925	A	44:34	44:34	"resolution" is probably a more suitable word than "complexity". [James Annan (Reviewer's comment ID #: 6-17)]	Accepted.
10-926	A	44:55	44:55	Insert after "assume" "unrealistically" [VINCENT GRAY (Reviewer's comment ID #: 88-1689)]	Reject: they are hypothetical scenarios
10-927	A	45:7		I thought we had agreed not to cite ranges that span different scenarios – so why insert such a range for the TAR which seems a bit gratuitous as there is nothing corresponding in the AR4. [Martin Manning (Reviewer's comment ID #: 155-86)]	This summary tells what was done for TAR to contrast with what follows in the text
10-928	A	45:12	45:12	seems to be contradicted, not confirmed, by your new forecast which (unless I have misunderstood something) predicts a rise of clearly >0.2C/decade. Arguably, a result of about 0.2C/decade would satisfy both forecasts acceptably, but the probability distributions has certainly changed! [James Annan (Reviewer's comment ID #: 6-18)]	Accept: this detail is now omitted
10-929	A	45:12	45:12	Does the IS92a scenario need a reference? Is it worth noting that the CO2 concentration is similar to the A2? [Ronald J Stouffer (Reviewer's comment ID #: 258-144)]	The reference to IS92a is now omitted
10-930	A	45:15	45:15	Delete "potentially" [VINCENT GRAY (Reviewer's comment ID #: 88-1690)]	Rejected it depends on the context
10-931	A	45:21	45:21	"1.5° to 4.5°" should be "1.5° to 4.5°C". [Chiu-Ying LAM (Reviewer's comment ID #: 139-18)]	Accepted
10-932	A	45:27	45:27	Insert after "contriversial" "and "quite irreponsible" [VINCENT GRAY (Reviewer's comment ID #: 88-1691)]	Reject: not appropriate
10-933	A	45:27	45:38	The discussion of issues around the SRES scenerarios is somewhat incorrect: The statement "Furthermore, the source of an overriding objection to assigning probabilities maybe found in the following quote from the SRES report..." was not at all the reason for SRES writing team did not attempt to estimate the probability of different non-mitigation scenarios. In addition the statement that "It clearly follows that the SRES scenarios cannot be regarded as capturing an agreed sense of the range of future options" is logically incorrect as there is no "agreed" range of future options. The SRES marker scenarios were selected to span the range of non-mitigation scenarios in the literature. I would suggest that this whole paragraph be deleted or corrected. Lines 8-9 on page 46 may say all that needs to be said here.	The second half of the paragraph is deleted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[William Hare (Reviewer's comment ID #: 99-75)]	
10-934	A	45:32	45:33	I do not understand the statement that "individual scenarios are clearly not independent." Each (emission) scenario comprises a consistent set of assumptions and can surely be treated independently when used in a climate model. [Martin Manning (Reviewer's comment ID #: 155-87)]	This statement is deleted
10-935	A	45:33	45:38	This citation "As required by the Terms of Reference [of the SRES report], the scenarios in this report do not include additional climate initiatives, which means that no scenarios are included that explicitly assume implementation of the United Nations Framework Convention *ON* Climate Change (UNFCCC) or the emission scenarios of the Kyoto Protocol" is important as well in the context of the emission scenarios analyzed in this chapter 10. Please lift this citation to a newly-to-be-created section that discusses the analyzed SRES emission scenarios. Furthermore, please provide a thorough explanation of why certain emissions scenarios, namely non-mitigation scenarios, seem to be left out from this draft and include them in the next and final draft. [Govt. of Germany (Reviewer's comment ID #: 2011-184)]	The citation is deleted from this section. The illustrative scenarios are sufficient for the science questions addressed. The plots are too crowded with all SRES
10-936	A	45:49		The term "scenario uncertainty" is unfortunate despite the fact that it is used in some of the literature. The policy messages that the physical climate science community can provide should clearly distinguish between uncertainties in our area of science and the differences in projections that arise from different assumptions about emissions. In my experience, using the common term "uncertainty" for both factors led to significant misinterpretations of the TAR results because the ranges due to science uncertainties and due to scenario assumptions were conflated and the larger range then used by some to argue that the science uncertainties were too large to merit taking any action. We should learn from this and adopt a language that is less likely to be misinterpreted and that clearly separates uncertainty due to incomplete knowledge from differences in projections that would arise from alternative future policy choices. In this case I would urge use of the term "scenario range" rather than "scenario uncertainty". [Martin Manning (Reviewer's comment ID #: 155-88)]	Accepted: will try to rephrase though 'scenario range' is also problematic – see your comment 10.927
10-937	A	45:51	45:51	socioeconomic progress and political decisions may not be in the domain of climate scientists, but I'm surprised to see your assertion that they are "hardly predictable". It seems like an unnecessarily bold and sweeping claim. [James Annan (Reviewer's comment ID #: 6-19)]	Accepted: this phrase is deleted
10-938	A	46:4	:6	How was the equilibrium sensivity derived from the transient coupled runs ? Jonathan Gregory's flux method? Fitting SCMs? [John Mitchell (Reviewer's comment ID #: 180-12)]	Fitting SCMs – see Chapter 8
10-939	A	46:9	46:12	This chapter should also span the range of mitigation scenarios in the literature. The low range of the SRES scenarios do not capture the the full range of mitigation scenarios	This was not possible due to time constraints.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[William Hare (Reviewer's comment ID #: 99-76)]	
10-940	A	46:14		Figure 10.5.2: It would be helpful to show the non-marker SRES scenarios for comparison. In addition, it would be good to have a similar figure for climate policy scenarios. Third, please include sea-level projections. [European Commission (Reviewer's comment ID #: 2008-61)]	The 6 illustrative scenarios are sufficient for the science questions addressed. See 10.939. Sea level is being addressed
10-941	A	46:14		Figure 10.5.2: What is the reason that the non-marker SRES scenarios are left out from this figure? They were included in the respective First-Order-Draft figures as background and could be included in this figure for clarity in the background in each of the panels - thereby providing a more complete picture of possible non-mitigation emission futures. Alternatively, please provide the reason of why WG1 does not discuss any more the full range of SRES scenarios - given that the reasoning of limited computer resources does not affect AOGCM tuned simple climate model results. [Govt. of Germany (Reviewer's comment ID #: 2011-185)]	The illustrative scenarios are sufficient for the science questions addressed. The plots are too crowded with all SRES
10-942	A	46:14		Figure 10.5.2: Please provide a corresponding figure for mitigation scenarios, either idealized pathways for stabilization between 350ppm CO2 to 550ppm CO2 or the existing post-SRES mitigation scenarios (Swart et al. 2002, EMF-21 and many other references). One advantage of simple models is - as proven by this and the following figure - that they provide an sophisticated and relatively easy method to extrapolate from the existing AOGCM runs, which are necessarily restricted to a few scenarios only - here A1B, B1 and A2. Thus, why does Working Group 1 choose not to show mitigation scenarios for lower greenhouse gas concentrations? Please include an analysis of a subset of the existing mitigation scenarios in the final draft. [Govt. of Germany (Reviewer's comment ID #: 2011-186)]	Stabilization scenarios are treated elsewhere. Time constraints did not allow mitigation scenarios to be addressed with the nexessary heirarchy of models.
10-943	A	46:14		Please provide an extra row for sea level rise projections - in line with figure 5 e) of the TAR WG1 SPM. [Govt. of Germany (Reviewer's comment ID #: 2011-187)]	Will be considered in relation to the SPM
10-944	A	46:14		FIGURE 10.5.2 COMMENT: As is standard this should show the full range of the SRES non mitigation scenarios as well as the marker scenarios. The marker scenarios do not constrain the SRES. [William Hare (Reviewer's comment ID #: 99-77)]	See 10-941. TAR figure 10.14 gives full SRES envelope and does not need to be repeated.
10-945	A	46:16	46:35	The results from this SCM with a carbon cycle are very prominent in this chapter and in the SPM/TS. Therefore the modelling needs more explanation here. How is the carbon cycle represented in MAGICC? What processes are varied to get "high" nad "low" carbon cycle feedbacks? CO2 fertilisation? soil decomposition? ocean uptake? This area really needs more detailed explanation of what is being done [Chris Jones (Reviewer's comment ID #: 120-31)]	Wigley c-cycle model is no longer used. We use input from Bern model.
10-946	A	46:37	46:37	Replace "anthropogenic" with "human-induced"	Rejected these words are

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[VINCENT GRAY (Reviewer's comment ID #: 88-1692)]	interchangeable
10-947	A	46:37	46:53	What also should be mentioned is the 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), C. Bertrand, JP Van Ypersele, A. Berger, "Are natural climate forcings able to counteract the projected global warming?", Climatic Change, 55, 413-427, 2002. The text in this section just mentions this issue by refering to "assumptions" about forcing used. [Gareth S. Jones (Reviewer's comment ID #: 121-129)]	Accepted: reference added
10-948	A	46:37	46:44	Line 37 states that "radiative forcing projections" include "volcanic forcing", but line 44 states that the future volcanic forcing is set to zero. Only the hindcast, not the projection, includes volcanic forcing. [Adrian Simmons (Reviewer's comment ID #: 242-154)]	Zero anomaly is used in the future relative to the mean of the last 100 years
10-949	A	46:37	46:53	Much of this discussion could be moved into my new proposed subsection. [Ronald J Stouffer (Reviewer's comment ID #: 258-145)]	OK
10-950	A	46:46	46:46	Delete "this is that" [Govt. of Germany (Reviewer's comment ID #: 2011-188)]	done
10-951	A	46:46	46:46	delete "that this it" [David Sexton (Reviewer's comment ID #: 233-6)]	done
10-952	A	46:52	46:53	Suggest adding to end of sentence "(See Chapter 2.7 for discussion about uncertainty in natural forcings)." [Gareth S. Jones (Reviewer's comment ID #: 121-128)]	done
10-953	A	47:0		Fig 10.5.3 - the caption should point out that different scales are used on the left hand and right hand Y axes. Given the very wide graph, I think it would be easier to understand if only one axis were used, and if horizontal gridlines were included in the figure. [Paul Baer (Reviewer's comment ID #: 10-12)]	Figure is being redrawn
10-954	A	47:4		This text seems inconsistent with Figure 10.5.3 which shows for some cases that carbon cycle coupling leads to a lower warming result than without the coupling. Either the way in which the bars for the figure are constructed could be better explained or there is something else going on which needs to be explained in the text. [Martin Manning (Reviewer's comment ID #: 155-89)]	Method is being changed and text rewritten
10-955	A	47:6	47:16	Please explain why the mean of the AOGCMs does not match the SCM mean, and why you show ±20% for the AOGCMs but 1 std for the SCM. [Susan Solomon (co-chair WG1) (Reviewer's comment ID #: 246-11)]	This matter is being addressed
10-956	A	47:6	47:16	We have had some discussion on this graph which I appreciate but I am afraid that more	X axis will be omitted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				work is needed. I am concerned about the physical significance of the x-axis used in figure 10.5.3 and the lack of discussion of that in the chapter at present. Is there any reference for making a plot in this form? do we expect this to be linear and if so why or why not? What do the authors believe the x-axis represents, and why? Are there any concerns regarding what this scale represents for short-lived gases, which are being rapidly removed as they are emitted, versus longer-lived ones? See e.g., Smith and Wigley, Climatic Change, 2000 and Shine et al., Climatic Change, 2005, who describe some of the problems with interpretation of such quantities. If this plot is kept in its present form, it should have sufficient text associated with it for its meaning, uncertainties, and limitations to be clear. If the authors cannot explain and substantiate what the scale means in physical terms, they should consider using another format to present the information. [Susan Solomon (co-chair WG1) (Reviewer's comment ID #: 246-12)]	
10-957	A	47:7		Would be much clearer for policy audiences to use the language "emission range and response uncertainty" - See my comment on line 49 of page 45. [Martin Manning (Reviewer's comment ID #: 155-90)]	See response 10.936
10-958	A	47:9	47:12	The WG1 report needs to be internally consistent. Consistency with the IPCC inventory guidelines is very much a secondary consideration and there are several reasons why that would now be difficult to achieve due to very recent changes in the philosophy underlying the 2006 inventory guidelines and the interaction of that with the definitions of GWPs. If the GWP weighted construct is going to be used for the X-axis of Figure 10.5.3 then the GWPs really have to be the ones defined in this report. [Martin Manning (Reviewer's comment ID #: 155-91)]	No longer use GWPs
10-959	A	47:9	47:11	Please ensure consistency with the precedent and practice in IPCC reports: use the GWPs provided in this assessment which are based on current scientific information, not the values used in the UNFCCC (which are outdated). [Susan Solomon (co-chair WG1) (Reviewer's comment ID #: 246-9)]	No longer use GWPs
10-960	A	47:9	47:11	Please avoid reference to UNFCCC or national communications in this paragraph; it makes the document sound like it is trying to lay out a framework for policy based on this approach - and that is not appropriate in an IPCC report. [Susan Solomon (co-chair WG1) (Reviewer's comment ID #: 246-10)]	accept
10-961	A	47:18		Where are the non-marker SRES scenarios? [European Commission (Reviewer's comment ID #: 2008-62)]	See response 10.941
10-962	A	47:18		Figure 10.5.3: This figure is important and needs to be lifted in the SPM as it provides a useful synthesis of policy-relevant results. [Govt. of Germany (Reviewer's comment ID #: 2011-189)]	This is a decision of the SPM writing team
10-963	A	47:18		Figure 10.5.3 - The previous corresponding First Order Draft Figure (FOD 10.5.6.e)	We no longer show cumulative

No.	Batch	Page:line From	Page:line To	Comment	Notes
				showed mean results as well for the 40 non-marker SRES scenarios. Why were they deleted? They provide useful information in regard to the spread and uncertainty in the relationship between cumulative emissions and 2100 temperatures. Please include mean results for all SRES scenarios in final draft figure as shown in FOD 10.5.6.e. [Govt. of Germany (Reviewer's comment ID #: 2011-190)]	emissions. The six illustrative scenarios are sufficient for the science.
10-964	A	47:18		Figure 10.5.3 - Please include extra panels with radiative forcing and CO2 concentrations (vertical axis) versus cumulative emissions (horizontal axis) as they were provided in the first order draft (Figures FOD 10.5.7.e and 10.5.8.e). [Govt. of Germany (Reviewer's comment ID #: 2011-191)]	Rejected: not appropriate for presentation reasons. Cumulative emissions axis is also no longer used.
10-965	A	47:18		figure 10.5.3. I'm still not completely sure what the SCM results represent - are they showing a simple model which is first calibrated against the AOGCMs, and then it has a carbon cycle included in it and so the results differ? Would the SCM recreate the GCMs if it was run without a carbon cycle? I think the differences in what the two sets of results imply should be discussed more [Chris Jones (Reviewer's comment ID #: 120-41)]	This section is being reworked
10-966	A	47:18		Figure 10.5.3. Looks like the simple model is biased low for B1, A1B (I note the explanation for bias in A2) [John Mitchell (Reviewer's comment ID #: 180-13)]	The SCM will be used to scale AOGCM results
10-967	A	47:26	47:27	It should be pointed out that this is relative to the 1980-2000 mean, not pre-industrial. [Paul Baer (Reviewer's comment ID #: 10-13)]	Accepted
10-968	A	47:29	47:41	another major reason why the SCM is warmer than the AOGCMs is because it includes a carbon cycle. Even the mid-range carbon cycle settings will have a positive feedback and amplify the warming by, maybe, 0.5 degrees. Easily enough to explain most of the difference between the SCM and AOGCM numbers for A2. [Chris Jones (Reviewer's comment ID #: 120-32)]	This section is being reworked
10-969	A	47:43	48:2	explain what changes you made to the carbon cycle "settings" here - and how did you arrive at high and low values? Are they extremes of the C4MIP spread? Or based on estimates of realistic ranges of parameters? [Chris Jones (Reviewer's comment ID #: 120-33)]	This section is being reworked
10-970	A	47:43		Replace "medium" with "mid-range" for clarity [Martin Manning (Reviewer's comment ID #: 155-92)]	Accepted
10-971	A	47:48		For consistency with the text below replace "defined as the range in…" with "defined as the standard deviation of different …". [Martin Manning (Reviewer's comment ID #: 155-93)]	This will be rewritten
10-972	A	48:0		Section 10.5.4. Is there a danger that uncertainty would be overestimated by using an ensemble of different models, each with an ensemble of varying paramterization	Indeed it is possible that some redundancy could be introduced if, for

No.	Batch	Page:line From	Page:line To	Comment	Notes
				coefficients and each with a representation of stochastic physics? Could this introduce double counting? [Adrian Simmons (Reviewer's comment ID #: 242-155)]	example, several alternative methods of perturbing parameterisations were found to have more or less the same effects. This is a largely theoretical concern at present, as the community has yet to contemplate the task of producing an ensemble comprehensive enough to possess such a problem. The key would probably be to think in terms of physical processes, i.e. select for each process, or set of processes, a subset of possible parameterisation changes which fully sample the range of behaviour consistent with our current understanding. The text in section 10.5.4.2 has been revised accordingly, though we don't think an extended discussion of possible redundancy is justified at this stage.
10-973	A	48:12	54:42	Generally speaking, this section is to long and very confusing for the reader. See details below. [Jean-Louis DUFRESNE (Reviewer's comment ID #: 60-11)]	The text has been clarified, and shortened where possible, including some of the suggestions made by the reviewer. However, much of the material in this section reports new techniques which need to be described in sufficient depth to allow readers to appreciate the different assumptions and choices underlying the new information on uncertainty ranges and probabilities. While it is unfortunate that this reviewer finds the section too long and confusing, his seems to be the only comment to this effect. So we do not believe that large structural changes should be made to the section at this point, while accepting the need to shorten and clarify in specific areas.
10-974	A	48:17	48:17	"errors in the parameterisation" sounds like bugs in the computer code. "errors due to the	Accepted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				parameterisation" would be better in a similar way to the line above where it says "errors introduced by discretization" [David Sexton (Reviewer's comment ID #: 233-7)]	
10-975	A	48:40		section # 10.5.4.1 Another uncertainty in the tuned multimodel runs is that we are not certain that the tuned model is the best one that could have been obtained even though it is treated as though it is. Perturbed physics ensembles show that several disparate points in parameter space could be equally chosen to be the tuned model but with varying predictions. So this extra uncertainty should be mentioned in this section. [David Sexton (Reviewer's comment ID #: 233-8)]	A comment has been added to point out that the process of tuning a GCM does not necessarily identify the optimum location in parameter space.
10-976	A	48:50	48:50	Suggest delete "Probabilistic" at the start of the sentence. The problem is not that they are probabilistic, but that they are in the future, and moreover in a future that will not eventuate. I would add that formally these probabilities can never be "verified" since, from an operational POV, they are expressions of personal preferences. But there's no need to go into that here! [Jonathan Rougier (Reviewer's comment ID #: 221-4)]	Accepted.
10-977	A	49:0		Section 10.5.4.2. An additional sentence could be added, on line 45 after reference to Hargreaves and Annan (2006), to acknowledge a parallel study: "Challenor et al. (2006) used a statistical emulator based on similar experiments with the same EMIC to quantify the probability of substantial decline in the Atlantic overturning rate by 2100, establishing that this probability is more sensitive to SRES scenario than model parameter uncertainty." Reference: Challenor, P. G., Hankin, R. K. S., and R. Marsh (2006). Towards the Probability of Rapid Climate Change. In "Avoiding Dangerous Climate Change", Schellnhuber, H J., Cramer, W., Nakicenovic, N., Wigley, T. and Yohe, G (Eds). Cambridge University Press. [Robert Marsh (Reviewer's comment ID #: 164-2)]	Accepted.
10-978	A	49:37	49:39	The sentence saying about further work should be excluded. [Valentin Meleshko (Reviewer's comment ID #: 175-6)]	The point about accounting for structural model errors is an important one, so needs to be retained. However, the sentence has been reworded to say that studies to date do not yet account for structural model errors, rather than to call for further work.
10-979	A	49:39	49:39	Apologies: the date here is wrong, it should be "Goldstein and Rougier (2004)." Also applies p51 line 24 and p79 line 32. [Jonathan Rougier (Reviewer's comment ID #: 221-3)]	References changed as indicated.
10-980	A	49:50	50:49	I don't understand the aime of this section 10.4.5.3. The beginning of this section (until line 23 page 10-50) adresse the spread due to internal variability, and I would suggest that	The point of this section is to compare briefly different causes of spread in

No.	Batch	Page:line From	Page:line To	Comment	Notes
				this section consider only this point. The following paragraph (line 25-35) should be suppressed or included in previous section (10.5.4.2) as it consider only perturbed physics ensembles. Last paragraph (line 37-49) is still considered in chapter 8. [Jean-Louis DUFRESNE (Reviewer's comment ID #: 60-12)]	ensemble projections (e.g. internal variability cf model physics cf emissions, altering model parameters cf altering model structure, the importance of different feedbacks, etc). We have revised the first paragraph of 10.5.4 to clarify this. This comparison is an essential element of the assessment of the ensemble results, as it gives readers some understanding of the factors influencing the probabilistic results presented elsewhere. This is important, as (for example) spread arising from internal variability cannot be reduced in future, whereas spread arising from modelling or emissions uncertainty potentially can be reduced. We do not, therefore, believe it would be appropriate to discuss only internal variability at this point. We also believe it is appropriate to retain a brief discussion highlighting the importance of cloud feedback (we have now added a reference to the more comprehensive discussion in chapter 8), and comparing the role of cloud feedback in perturbed parameter and multimodel ensembles (which is not discussed in chapter 8).
10-981	A	49:53	:55	Fig 10.5.5a Why does SLP agree less at large scales? Do some models loose mass? [John Mitchell (Reviewer's comment ID #: 180-14)]	The low agreement on global mean SLP changes occurs because different models simulate different (albeit small) changes (multimodel mean change=-0.11 hPa, with a standard deviation of 0.31 hPa, and a range from -0.91hPa to +0.44 hPa). This range occurs partly because at least one model loses mass, and also because increases in atmospheric water vapour are allowed

No.	Batch	Page:line From	Page:line To	Comment	Notes
					to affect total mass in some models, but not in others. These factors also explain the low agreement at the hemispheric scale, since the inter-model variance of hemispheric changes is dominated by the global mean changes. However, this low agreement is not a cause for concern, because SLP changes on these scales are extremely small compared to the regional changes associated with changes in atmospheric circulation. A sentence to this effect has been added to the relevant figure caption.
10-982	A	50:40	50:44	Whilst this sentence is literally correct, it may be misleading due to the cancellation of longwave and shortwave cloud forcing in deep convective regions. A more accurate summary of the findings of Webb et al. (2006) would be: "They find that the ranges of climate sensitivities in both ensembles are explained mainly in terms of differences in the response of shortwave cloud forcing in areas where changes in low level clouds predominate." It would also be appropriate to add a reference to Bony and Dufresne (2005) e.g. "Bony and Dufresne (2005) find that tropical cloud feedbacks in the AR4 ensemble differ most in areas of large scale subsidence where low clouds predominate." [Keith Williams (Reviewer's comment ID #: 290-13)]	Accepted – text revised as suggested.
10-983	A	50:44	50:48	I would suggest to remove or to rewrite the sentence starting with "Narrowing uncertainties… Tsuhima et al. 2006)" because cloud microphysical properties are only one aspect of the problem. Cloud macrophysical properties are likely to be as important as cloud microphysical properties, and it is not even clear whether uncertainties in cloud feedbacks are related to cloud parameterizations themselves or to other parameterizations (e.g. PBL and convection schemes). The citations Johns et al. (2006) and Tsushima et al. (2006) do not seem particularly adequate to support this statement. [Sandrine Bony (Reviewer's comment ID #: 25-10)]	Reworded to emphasise the importance of both microphysical and macrophysical properties of cloud, and the likely need (therefore) for both improved parameterisations and higher resolution in order to reduce cloud feedback uncertainties.
10-984	A	50:48	50:49	Instead of insisting on the necessity of ensemble approaches, I would rather insist on the necessity to understand, from a physical point of view, the reasons why cloud feedbacks differ among models! [Sandrine Bony (Reviewer's comment ID #: 25-11)]	This sentence has now been removed.
10-985	A	50:56	51:9	I am a bit concerned by the huge "melting pot "of studies included in this paragraph. For instance, Bony et al. (2004), Bony and Dufresne (2005) and Williams et al (2005 -which should probably be 2006) have not provided probabilistic estimates of climate sensitivity	Agreed. Text reworded to distinguish between observables which have been used to obtain formal probabilistic

No.	Batch	Page:line From	Page:line To	Comment	Notes
				using measures of the current climate variability. They have investigated whether and how observations of current climate variability might be used to constrain some components of climate change cloud feedbacks. Similarly, Tsushima et al. (2005) proposed an observational test to evaluate the model cloud feedbacks at the seasonal timescale, but have not provided any probabilistic estimate of climate sensitivity. [Sandrine Bony (Reviewer's comment ID #: 25-12)]	estimates, and observables which have been suggested as potential constraints, but not yet used to provide pdfs.
10-986	A	50:56	51:9	It is not correct to say that the referenced studies constrain the probability estimate of the climate sensitivity. I am sure that it is the case for our studies (Bony et al. 2004, Bony and Dufresne 2005) and I claim it is also the case for other studies. It is very different to constrain curent climate (including seasonal cycle and inter-annual variability) or to constrain climate sensitivity. With our current knowledge, only the studies that address historical transient evolution of surface temperature, upper air temperature,... or the response to paleoclimatic forcings should be considered here. Moreover, most of these studies do not address probabilities estimate of the climate sensitivity but they estimate the possible range of climate sensitivity, which is quite different. [Jean-Louis DUFRESNE (Reviewer's comment ID #: 60-13)]	See response to 10-985 above. However, we disagree with the comment that only observables of transient change are currently allowable for use in constrained probabilistic estimates. The key is to demonstrate a link between the observable(s) (regardless of whether they are measures derived from present climate or historical changes) and the future variables we are trying to predict. Studies using present day measures, such as Piani et al (2005), Knutti et al (2006) and Murphy et al (2004) all find such links (to varying degrees), as do the studies based on historical changes.
10-987	A	51:17	51:17	You may need to suitably denigrate our work to justify your conclusion, but you could mention that at least some people strongly disagree with your claims! [James Annan (Reviewer's comment ID #: 6-20)]	Text revised to refer to Annan and Hargreaves (2006), who rightly point out the potential for narrowing uncertainty by combining multiple lines of evidence. We also believe, however, that further work is needed to reach a point where robust, fully objective calculations of this nature can be performed, given the need to quantify reliably all the individual constraints, the degree of interdependence between them, and to account for structural modelling errors in the results in a comprehensive probabilistic prediction framework.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-988	A	51:26	53:21	This long paragraph should be shortened and clarified. Line 35-57 of page 51 can be replace by a reference to section 10.5.4.1, 10.5.4.2 and 10.5.4.4. Lines 2-22 page 10-52 are almost impossible to understand. Please suppress or clarify. [Jean-Louis DUFRESNE (Reviewer's comment ID #: 60-14)]	This section was originally intended to be an end-to-end summary of probabilistic methods, but in practice some of this integrity has been lost by splitting the material into separate global and regional sections. The material remaining in section 10.5.4.5 largely concerns methods designed to be consistent with observational uncertainties. The first two paragraphs of the section have therefore been shortened, to avoid giving the potentially confusing impression that this section is a review of all methods. The reviewer finds lines 2-22 of page 10-52 difficult to understand, but does not make specific suggestions for improvement. Moreover, his comments are not supported by other reviewers. We have therefore made minor revisions to improve clarity where possible, but do not see a need for major revision. The brief description of the scaling method used by Stott and Kettleborough seems straightforward, and is consistent with the longer decription given in Chapter 9, to which the reader is referred. The extension to continental scale predictions is not discussed in Chapter 9, so this cannot be suppressed as it is an important contribution to probabilistic prediction.
10-989	A	51:39	51:39	A compilation (and comparison) of the different global radiative feedbacks (water vapor, clouds, lapse rate, surface albedo) of the AR4 models derived from different methodologies is given in Bony et al. (2006, J. Climate, in press, Figure 1). [Sandrine Bony (Reviewer's comment ID #: 25-13)]	Reference added.
10-990	A	52:8	52:9	Emphasise that this is about non-mitigation scenarios. [European Commission (Reviewer's comment ID #: 2008-63)]	The Introduction has been revised to emphasise that the SRES scenarios

No.	Batch	Page:line From	Page:line To	Comment	Notes
					considered here are not mitigation scenarios.
10-991	A	52:8	52:9	Please include "non-mitigation", so that the section reads "… between four representative SRES non-mitigation emissions scenarios… " [Govt. of Germany (Reviewer's comment ID #: 2011-192)]	The Introduction has been revised to emphasise that the SRES scenarios considered here are not mitigation scenarios.
10-992	A	52:12	52:14	I think more care needs to be taken over what is reported on Stott el al (2006b). First, "results are relatively model-independent" needs to be more explicitly defined. Indeed the forecasts do look quite robust. But the other results from Stott et al (2006b) are the scalings for the G, SO and NAT as these seem to be model dependent. Indeed, the scaling for SO and G for the PCM model does not overlap with 1, which means that with respect to that model, we do not understand what contributed to historical warming; the detection analysis for PCM is saying we need a cooling effect which is 2.5* size of the aerosol effect as modelled by PCM. So a cautionary note needs to be added to say this. Because of this, it is not clear that this model should have been used in the forecast. [David Sexton (Reviewer's comment ID #: 233-9)]	Although the results of Stott et al show that applying the scaling factors brings the results of the different models into better agreement, there are still significant differences between the pdfs of future change, so the "results are relatively model independent" text has now been deleted. The point at which the magnitude of the scaling factors become large enough to invalidate the technique is an arguable point, depending on whether (for example) we believe that a GCM can simulate the correct pattern of aerosol response while predicting an amplitude of response inconsistent with observations. Given that we now make a weaker statement about the results of this study, and that the scaling factors are shown and discussed in Chapter 9, we believe it is not necessary to include a detailed discussion of the assumptions here.
10-993	A	52:24	52:36	your conclusion that pdfs constrained by observations are likely to be robust is not at all justified. The work by Frame et al. (2005) shows that such pdfs are highly dependend on prior assumptions, and that the pdfs produced with different prior assumptions are quite different. [Danny Harvey (Reviewer's comment ID #: 101-76)]	The intention here was to make the point that predictions are robust in the sense that they are likely to change only slowly, as the observed signal of climate change emerges from the noise of internal variability. It was not intended to imply that the methods were robust in the sense that they are

No.	Batch	Page:line From	Page:line To	Comment	Notes
					somehow better than other methods, or insensitive to assumptions and choices made in the methodology, such as what prior to use. The text has been changed to avoid giving this impression.
10-994	A	52:33	52:36	It seems strange to say that the methods are likely to be robust provided something that has not been tested yet proves to be favourable. We cannot say what the likelihood of these relationships are based on one model; no statement should be made about such robustness as the relationships have not been tested on other models yet. This sentence should read more like "The robustness of these methods needs to be tested by seeing if the emergent constraints apply to climate models other than the one used to derive the inter-variable relationship." [David Sexton (Reviewer's comment ID #: 233-10)]	In fact these relationships have all been tested using ensembles of models, though in the cases of Piani et al and Knutti et al the ensembles are of the perturbed parameter variety, so it is probably reasonable to question the validity of the emergent constraints in these studies more than in the case of Allen and Ingram, who used a multi-model ensemble sampling variations in model structure. The text has been modified to make this distinction.
10-995	A	52:34	52:34	I'm surprised to see a citation of a non peer-reviewed conference proceedings. Of course, peer review is no guarantee of quality, but you usefully ponder over why they have not yet attempted to publish this stuff anywhere. [James Annan (Reviewer's comment ID #: 6-21)]	In fact, this work has been peer-reviewed, and is now published as a chapter of a book on "Predictability of Weather and Climate", 2006, Eds. T Palmer and R. Hagedorn, Cambridge University Press. The reference has been updated accordingly.
10-996	A	52:38	52:38	Emphasise that this is about non-mitigation scenarios. [European Commission (Reviewer's comment ID #: 2008-64)]	The Introduction has been revised to emphasise that the scenarios considered here are not mitigation scenarios.
10-997	A	52:38	52:38	Please include "non-mitigation", so that the section reads "…global mean projections for the SRES non-mitigation scenarios B1, A1B, and A2 … " [Govt. of Germany (Reviewer's comment ID #: 2011-193)]	The Introduction has been revised to emphasise that the scenarios considered here are not mitigation scenarios.
10-998	A	53:0		Section 10.5.4.6 refers to regional climate prediction, and would perhaps be best transferred to Chapter 11. [Adrian Simmons (Reviewer's comment ID #: 242-156)]	Although this section does refer to regional prediction, we believe it is better to keep it in chapter 10, for the following reasons: (1) the uncertainty information presented in chapter 11 considers variables spatially averaged over regions, whereas chapter 10

No.	Batch	Page:line From	Page:line To	Comment	Notes
					contains more information in the form of maps of grid point changes, so the information presented as maps is easier to integrate into Chapter 10; (2) chapter 11 contains only brief descriptions of the methods for producing probabilistic regional estimates, and relies on the fuller discussion in section 10.5.4..6 to emphasise the assumptions and caveats associated with the techniques. Siting that material in 10.5.4.6 allows these issues to be discussed alongside the assessment of methods applied to global or continental scale predictions, facilitating a coherent assessment of probabilistic methods in general. The "regional" methods can be, and are, applied to global variables too, so it makes sense to assess the methodological differences among all the techniques in one place. A good example is Lopez et al (2006), which assesses a regional method by applying it at the global scale, thus allowing comparison against other methods. The comparison of pdfs for 21st century global temperature in figure 10.5.7 is another example.
10-999	A	53:24	23:24	This description of the Tebaldi et al paper is unnecessary, as the paper is described and used in Chapter 11. Further coordination maybe needed between Chapter 10 and 11 authors on the treatment of this point. [Govt. of Australia (Reviewer's comment ID #: 2001-432)]	Following further discussions with Chapter 11, we confirm our belief that the description of Tebaldi et al given in Chapter 10 should be retained. This is because the paper is described more briefly in Chapter 11, and relies on the fuller description given here to emphasises their methodological assumptions, and the effects these have on the predicted changes. This is done

No.	Batch	Page:line From	Page:line To	Comment	Notes
					by Lopez et al (2006), through a comparison with another technique done at the global scale: siting the Tebaldi et al material here enables that comparison to be reported as part of a coherent assessment of global and regional techniques considered together (see also the response to 10-998 above).
10-1000	A	53:29	:30	How is the "best" estimate of climate change determined? I thought that was what we were trying to determine- seems circular logic [John Mitchell (Reviewer's comment ID #: 180-15)]	Changed "best estimate of" to "weighted ensemble mean of", as the convergence criterion actually weights ensemble members more highly according to how close they are to the ensemble mean, rather than the (a priori) unknown best estimate of change. In practice, though, this does result in the weighted ensemble mean having a stronf influence on the posterior pdfs, including the best estimate changes indicated by the modes of the pdfs.
10-1001	A	53:31	53:31	Add "The observed rate of increase in the last three decades has been slightly less than one-half of this value". [Patrick Michaels (Reviewer's comment ID #: 176-39)]	As discussed in the Introduction, 1% per year is an idealised forcing scenario used to investigate the response to increases in CO_2, not to provide predictions of the future, so it is not relevant to compare this scenario to recent historical changes in this paragraph.
10-1002	A	53:33	53:34	The main reason the 'Bayesian' approaches (Tebaldi et al, and Furrer et al) produce a smaller range than others (like simple fitting to model values) is that like standard statistical theory, the range decreases with the number of models. Is this not the case? if so, the authors should make that clear in the paragraph. [Govt. of Australia (Reviewer's comment ID #: 2001-433)]	This is not true for Furrer et al, as can be seen from Figure 10.5.7, which shows ranges for global temperature change similar to those obtained by simple fitting to the AR4 AOGCMs. On the other hand, Tebaldi et al assume that each model is an independent estimate of the true climate, so their

No.	Batch	Page:line From	Page:line To	Comment	Notes
					ranges do depend on sample size (n). However Lopez et al (2006) show that for any reasonable ensemble size (say n>5), the systematic dependence on n is small compared with random sampling effects, and with other choices in their methodology, particularly their convergence criterion. We do not therefore believe that a specific comment on sample size is necessary
10-1003	A	53:40	53:55	The Furrer et al paper is included in Table 11.1. As this provides basically a long description it should be reduced to brief assessment at this point. [Govt. of Australia (Reviewer's comment ID #: 2001-434)]	The description has been shortened where possible, but it is necessary to retain sufficient detail to give readers a clear understanding of the main features of the technique, and the assumptions it is based on. This is because the description given in Table 11.1 is very brief, and is not intended to be sufficient for this purpose. Since the Furrer et al method is used to provide probabilistic maps in Chapter 10 (not Chapter 11), it is necessary to include the description of the method here.
10-1004	A	54:0		SECTION 10.6 Sea-Level Change. Sea level rise projections I: The headline finding of the AR4 on sea level rise after reading this Chapter appears to be that SLR is less than found in the TAR for the same emission scenarios and that we are more certain of this than before. Several lines of evidence would indicate this is not consistent with the underlying science: 1) Improvements in observations of steric sea level rise, SGIC mass loss, ice sheet mass balance show that each of the terms appear to be larger than was assessed in the TAR and also that each is accelerating in a way that is associated with increases in temperature 2) Model vs observation discrepancies are not resolved and on the face of it appear to be larger than previously thought 3) Uncertainties appear to have expanded (eg very different estimates of SGIC mass/volume and sensitivity, ice sheet/ice stream motion not explained by ice sheet models etc). [William Hare (Reviewer's comment ID #: 99-78)]	Taken into account. The larger observed terms referred to in the comment are those for recent years in particular. We will clarify how the 1993-2003 rate of SL rise is caused. We note that natural and internal variability may have contributed to its high rate. Larger uncertainties do not necessarily mean the SLR should be higher, as implied by the comment. We will compare the results with the TAR.
10-1005	A	54:0		Sea level rise projections II (ctd). The SLR section does not show the range of estimates	Accepted. Unlike the TAR, chapter 10

No.	Batch	Page:line From	Page:line To	Comment	Notes
				from the full range used in the temperature scenarios and focusses on only one scenario. The full range needs to be shown [William Hare (Reviewer's comment ID #: 99-79)]	is not producing a combined scenario range for temperature or SL, but we will produce SL ranges for the marker scenarios.
10-1006	A	54:3	:4	Any information on how well scaling works? [John Mitchell (Reviewer's comment ID #: 180-16)]	Scaling works well for surface temperature everywhere, and works reasonably well for precipitation, though less well in some regions than others. A comment on scaling is now included in the text.
10-1007	A	54:7	54:8	"differences betweeen…emulated result." could be better if it described what the correction field did. So how about "adding a correction field (estimated from the difference between the scaled equilibrium patterns and transient response) to account for the mean effect of the ocean on the transient response, and allowing for uncertainty in the emulated result." [David Sexton (Reviewer's comment ID #: 233-11)]	Clarified that the correction field is an ensemble-mean field averaged over all model versions. However a longer explanation than that suggested would be needed to explain the effects of the different types of ocean model included in slab and transient GCM simulations. We don't think that space can justifiably be allocated to basic didactic material such as this, so this change is rejected.
10-1008	A	54:16	54:17	Please delete one of the two "2°C" references and include "non-mitigation", so that the sentence reads "This type of information can be displayed in a map of, for example, values of probability for temperature change larger than 2°C by the end of the 21st century under the A1B non-mitigation scenario." [Govt. of Germany (Reviewer's comment ID #: 2011-194)]	The first reference to 2°C deleted, as suggested. The Introduction has been revised to emphasise that the scenarios considered here are not mitigation scenarios.
10-1009	A	54:41	54:42	In my opinion, such a statement ("Work in this area…") should not be written in an IPCC assessment report. [Sandrine Bony (Reviewer's comment ID #: 25-14)]	Sentence deleted.
10-1010	A	54:44	54:44	While the results presented for the various terms are interesting, I would think that there also needs to be a summary to start this section of the terms contributing to the rise during the 20th century and the fraction of 20th century change that the models were able to explain--some context is needed here. [Michael MacCracken (Reviewer's comment ID #: 152-269)]	Rejected for reasons for space. The summary of totals is in the Exec Summ and it would be repetition to put it here as well. The detail is in 10.6.5.
10-1011	A	54:44	54:44	It is surprising how little connection there is in this section to what was done in the earlier assessments--I think it very important that connections be made and explanations for difference be provided, especially given the very large change from previous assessments.	Taken into account by comparison in 10.6.5. The reviewer may have the impression of large difference from the

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Michael MacCracken (Reviewer's comment ID #: 152-270)]	TAR because only A1B is given in the SOD; we will include other scenarios.
10-1012	A	54:44	54:44	For the TAR, the US Government was urged to reject the chapter on sea level rise by 7 prominent US glaciologists. Only with careful negotiation and an increase in the indication of uncertainties did the chapter survive. The AR4 analysis seems to go in directly the opposing direction, by a very large amount. Were I advising the USG on this chapter, or at least this section, I would urge its rejection as not providing a comprehensive portrayal of the potential for changes in sea level. [Michael MacCracken (Reviewer's comment ID #: 152-271)]	Noted. There are more glaciologists as LAs of the AR4 than of the TAR, some of them from the US, and we have attempted to assess the current state of knowledge comprehensively, despite the reviewer's impression to the contrary.
10-1013	A	54:44		This section requires some more work, both on substance and on presentation. It does not read well and it is difficult to extract the main conclusions from the text. Provide more figures and tables that allow for comparisons between scenarios, between model projections and between current and previous projections, as well as between the individual processes causing sea-level rise. [European Commission (Reviewer's comment ID #: 2008-65)]	Accepted within constraints of space.We will produce ranges for the marker scenarios and show them in a figure. It is hard to compare processes in a clear and useful way except by stating ranges, since the uncertainties are large.
10-1014	A	54:44		Section 10.6 "Sea-Level Change". This section is of utmost importance, but needs to be overhauled for clarity and comprehensiveness and to provide the key results in both a tabular and graphical format. It seems inappropriate for this key topic section to provide only one figure (10.6.1) on thermal expansion and one on relative regional sea level change (10.6.2). Please build on the clarity of language and easiness to read, as shown in the ice-sheet section presented in Box 10.1. [Govt. of Germany (Reviewer's comment ID #: 2011-195)]	Accepted within constraints of space. This is a complex subject. There is more to say about projections of SL in the AR4 than the TAR, but we have less space available to describe it. A figure showing other scenarios is being added.
10-1015	A	54:44		Section 10.6. Please provide a summary figure for different emission scenarios according to the style of figure Question 5.1, Figure 1. The major advancement of the temperature projections since the TAR, namely that the 'response' and 'emission' uncertainty are now more clearly separated has to be applied to the Sea Level section as well. Thus, please provide time-series projections (including uncertainties) for the combined sea level rise due to all important contribution processes (ice-sheets, expansion etc.) for at least the six SRES marker scenarios. [Govt. of Germany (Reviewer's comment ID #: 2011-196)]	Partly accepted. We will do as for temperature i.e. provide ranges at 2100 for the marker scenarios.
10-1016	A	54:44		Section 10.6. In addition to a graphical summary figure for sea level rise, please provide a matrix table with the different emission scenarios on one axis and the contributions to sea level rise by 2100/2200/2400 (including their uncertainties) on the other. [Govt. of Germany (Reviewer's comment ID #: 2011-197)]	Table of components will be considered – not all information available though.
10-1017	A	54:44		Please structure the subsection of the sea-level rise chapter so that first the uncertainties and possible model-observation mismatches are discussed before the uncertainties and	Rejected, because the comparison with observations is discussed in chapter 9.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				best guess slr contributions are stated. Currently, as e.g. in the case of ice-sheet contribution section, model results are presented, which are later qualified as not being in line with current (e.g. faster ice-stream) observations. [Govt. of Germany (Reviewer's comment ID #: 2011-198)]	It will be clarified there.
10-1018	A	54:48	54:49	The wording needs changed. Most folks will not understand the relationship be density and volume. I would delete the first sentence and change the second sentence to begin - As sea water warms, the thermal expansion … [Ronald J Stouffer (Reviewer's comment ID #: 258-146)]	Accepted.
10-1019	A	54:48		For clarity replace "This thermal expansion …" with "The consequent thermal expansion …" [Martin Manning (Reviewer's comment ID #: 155-94)]	Taken into account by rewording for clarity.
10-1020	A	55:4	55:8	Only A1B results are mentioned, I want to see the results from B1 and A1B runs as well. Perhaps even more interesting would be a classification according to temperature rise (see also comment to page 55, lines 12-16), because many results for glaciers and ice sheets are formulated in term of a change per degree warming. [Gerrit Burgers (Reviewer's comment ID #: 34-23)]	Will present all available results for AOGCMs
10-1021	A	55:4	55:4	Are the uncertainty estimates in this section ±1 std or ±2 std? [Govt. of Finland (Reviewer's comment ID #: 2009-139)]	Taken into account. They will be changed to 5-95% ranges as elsewhere in the report.
10-1022	A	55:4	55:8	Thermal expansion projections for 2000-2020 are based on results in Figure 10.6.1. Is the uncertainty (e.g. ± 0.8) the full range among models or a standard deviation, standard error, or other measure? Given that most of the models in Figure 10.6.1 do not rise above zero until well into this 20 year interval, it is unclear how the value is obtained (I presume by including parts of the curves below zero and not shown). See comment on figure (p. 136 below). [Donald L. Forbes (Reviewer's comment ID #: 72-15)]	Taken into account. They will be changed to 5-95% ranges as elsewhere in the report.
10-1023	A	55:4	55:4	Does the scenario used matter? If not change "scenario SRES A1B" to "all scenarios". [Ronald J Stouffer (Reviewer's comment ID #: 258-147)]	Will present all available results for AOGCMs
10-1024	A	55:10	55:12	The authors should provide more detailed and more specific data, [European Commission (Reviewer's comment ID #: 2008-66)]	Rejected, as no specific suggestion made.
10-1025	A	55:10	55:12	Please provide numeric sea level rise results for all emission scenarios separately, preferably in a table together with split ups for the individual contributions (incl. uncertainties). In this paragraph, not even the assumed emission scenario is specified. Please correct. [Govt. of Germany (Reviewer's comment ID #: 2011-199)]	Will present all available results for AOGCMs
10-1026	A	55:10	55:10	Before "more than twice" add in the multimodel ensemble, using the SRES A1B	Accepted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				scenario". [Ronald J Stouffer (Reviewer's comment ID #: 258-148)]	
10-1027	A	55:10		Please specify which scenario the following projections apply to (one should not have to guess that it is still talking about A1B). [Martin Manning (Reviewer's comment ID #: 155-95)]	Accepted.
10-1028	A	55:12	55:16	We have found a relation between scenario/warming in 2100 on the one hand and sea-level rise on the other hand. See KNMI Climate Change Scenarios 2006, p53, Figure 7-3, available on http://www.knmi.nl/klimaatscenarios/knmi06/achtergrond/WR23mei2006.pdf . Although it is a weak relation, it leads to a difference of about 12cm between a termperature rise of 1 K and of 5K. Please consider reformulating the sentence in a form as "To some extent, thermal expansion is correlated with global average temperature change, although the spread ....". [Gerrit Burgers (Reviewer's comment ID #: 34-24)]	Taken into account by qualifying that there is no correlation in this ensemble of models.
10-1029	A	55:18	56:20	With respect to the ordering of the text, it would seem that sections 10.6.3 and 10.6.4 (and maybe even 10.6.5) should come before section 10.6.2, or make 10.6.2 part of 10.6.1. [Michael MacCracken (Reviewer's comment ID #: 152-272)]	Rejected. There is no ideal organization. To subsume 10.6.2 in 10.6.1 would underrate its importance, and to move it elsewhere would break its connection with thermal expansion.
10-1030	A	55:22	22:24	note that in chapter 5.5, a detailed discussion shows that other contributions are needed to explain the full signal (run-offf, ice melting…). And there is still a significant residual difference between the observed sea-level and the steric contribution to sea level. [Pascale DELECLUSE (Reviewer's comment ID #: 58-68)]	Noted. This is discussed more in chapter 9 as well and mentioned in 10.6.5.
10-1031	A	55:33	55:34	What about the other SRES scenarios? [European Commission (Reviewer's comment ID #: 2008-67)]	Taken into account by inserting a comment that other scenarios have similar patterns.
10-1032	A	55:33	55:34	In regard to the sentence "Results are available… following SRES scenario A1B". Why are the B1, and A2 results not available for the AOGCMs? Why can other scenario's implications not be extrapolated from the available AOGCM results? Please attempt to provide more than a single scenario result for local sea level changes. [Govt. of Germany (Reviewer's comment ID #: 2011-200)]	Taken into account by inserting a comment that other scenarios have similar patterns.
10-1033	A	55:38	55:41	Can the end of this paragraph be clarified? I had to read it about 3 times to figure out what I think you are doing here. [Martin Manning (Reviewer's comment ID #: 155-96)]	Taken into account by rewriting for clarity.
10-1034	A	55:40	55:41	Unclear to me what this means. "It shows" - What is "it"? [Ronald J Stouffer (Reviewer's comment ID #: 258-149)]	Taken into account by rewriting for clarity.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-1035	A	55:48		For clarity add "projected to be" in front of 2.9 +/- 1.9 mm /yr [Martin Manning (Reviewer's comment ID #: 155-97)]	Accepted, presuming the comment refers to line 10.
10-1036	A	56:15	56:15	After "scenario runs" add "evaluated here". [Ronald J Stouffer (Reviewer's comment ID #: 258-150)]	Accepted.
10-1037	A	56:15		"… collapse in the SRES scenario runs if Greenland meltwater runoff is not included". [Stefan Rahmstorf (Reviewer's comment ID #: 206-20)]	Taken into account by suggestion of 10-1036.
10-1038	A	56:22	56:28	It would help to make the explicit distinction between G&IC and ice sheets at the outset (first sentence) rather than parenthetically (after the second). [Donald L. Forbes (Reviewer's comment ID #: 72-16)]	Accepted.
10-1039	A	56:22		Section 10.6.3 Glaciers and Ice Caps: This section would benefit from a few enhancements 1) A fuller explanation and reasoning in relation to the treatment of glaciers and ice caps surrounding Greenland and Antarctic ice sheets (currently addressed using a multiplier of 1.2) given the large volume of ice estimate to be in these 2) A discussion and assessment of the implications of different glacier volume assessments (see table Table 4.5.1. Extents of glaciers and ice caps) in relation to SLR projections and a basis for choosing one over another if this is to be done 3) A table for parameters discussed would make it easier for the reader to see what is going on. [William Hare (Reviewer's comment ID #: 99-80)]	Will be covered in an appendix
10-1040	A	56:24	56:24	Change "may" to "very likely" (or even 'virtually certain' or "will")--this has certainly always happened in the past as a result of climate change, so there is every reason for it to happen in the future; "may" is much too vague a word and conclusion. [Michael MacCracken (Reviewer's comment ID #: 152-273)]	Taken into account by changing to "can" since this is a general statement rather than a prediction.
10-1041	A	56:29	:29	GCM' what? 'Fields'? [Steve Harangozo (Reviewer's comment ID #: 98-31)]	Taken into account by rephrasing for clarity.
10-1042	A	56:30	56:31	This approach has several shortcomings which must be mentionned: the small glaciers with adequate observational data used for calibration cannot be directly compared with the large glaciers which provide by far the most important part of sea-level rise, because (a) the mass-balance/altitude feedback strongly affects large glaciers (size effect; cf. comment 18), (b) large firn areas at high latitudes are not temperate but cold and provide meltwater only after having warmed up to melting temperature, (and (c) large parts of large glaciers (the main meltwater contributors to sea level) are below sea level or local lake level (melting of this ice will lower sea level). Reference (which also guides to the internationally collected data sources): Haeberli, W., Maisch, M. and Paul, F. (2002): Mountain glaciers in global climate-related observation networks. WMO Bulletin, 51/1, 18-25. [Wilfried Haeberli (Reviewer's comment ID #: 94-15)]	Rejected, since (a) is covered in 10.6.3.3, (b) is taken into account, as already stated, and (c) is not a large effect. However we have rearranged somewhat in order to point the reader to the right section.
10-1043	A	57:11	57:18	These numbers might be just fine if glaciers are not having meltwater drained from them,	Rejected. Most meltwater runs off,

No.	Batch	Page:line From	Page:line To	Comment	Notes
				but once this happens, much greater melting can occur because no energy has to be devoted to heat of vaporization (or more properly sublimation) for glaciers to lose mass--one just needs the heat of fusion, and since that is much less than for vaporization, the energy saved from not having to vaporize moisture can go toward melting (and more runoff)--and this can significantly increase the loss rate. [Michael MacCracken (Reviewer's comment ID #: 152-274)]	apart from that which percolates and refreezes in the firn. The mass-balance schemes do not assume that meltwater mostly evaporates, as the reviewer implies.
10-1044	A	57:16	57:16	Accumulation rate at high latitude is not of interest here; sensitivity of accumulation rate to temperature is; I presume "sensitivity" should be added after "precipitation". [Richard B. Alley (Reviewer's comment ID #: 4-16)]	Accepted.
10-1045	A	57:16	57:16	Add "increase" after "precipitation". [Govt. of Finland (Reviewer's comment ID #: 2009-140)]	Accepted.
10-1046	A	57:21	57:21	This finding is based on other conditions staying the same, but his is not necessarily the case--e.g.., one can start getting more runoff of meltwater at higher, steeper levels, and thus, by not needing to vaporize the melt water to get loss, melting rates can go up. [Michael MacCracken (Reviewer's comment ID #: 152-275)]	Rejected. See 10-1043.
10-1047	A	57:30	57:38	Is the earlier in the season change in surface albedo as snow warms accounted for as well--and also the increased exposure to the Sun as the snow is less likely in deep valleys? It seems to me that experience is indicating that glaciers pull back quite quickly and retreat up mountains is quite fast. [Michael MacCracken (Reviewer's comment ID #: 152-276)]	Noted. Yes, snow-albedo effects are included in energy-balance modeling. Results from glacier modelling and observations are compared in chapter 9.
10-1048	A	57:30		what is bt? (Did I miss a definition?) [John Mitchell (Reviewer's comment ID #: 180-17)]	It is defined in 10.6.3.1. Taken into account by making a cross-reference.
10-1049	A	57:33	57:36	This reasoning is at least misleading if not wrong: the mass balance/altitude (b-H; b = balance, H = altitude) feedback is fundamentally important and must be treated correctly. Total surface lowering (integrated over time) must be compared with the mass balance gradient (db/dH). Due to glacier dynamics, surface lowering (dh/dt; h = ice thickness, t = time) in the ablation area can be much more rapid than 1m/y and represents a cumulative phenomenon which can lead to irreversible runaway effects ("downwasting" or even "collapse" rather than "retreat") of b(t) with large and flat glaciers (the main meltwater contributors to the sea). Such large/flat glaciers are thick (h often >1000 meters), have long response times (> 1 century) and cannot quickly "loose their ablation area" through disappearance of low-altitude ice. Already today, the b-H feedback probably enhances the directly climate-related balances on medium-size to large glaciers (> 10 km2) by about a factor of two or so. Observed and reconstructed mass losses are, therefore, also a function of glacier size (reference: Hoelzle, M., Haeberli, W., Dischl, M. and Peschke, W., (2003): Secular glacier mass balances derived from cumulative glacier length changes. Global and Planetary Change. 36, 295-306.). This is clearly confirmed by the observations and	Taken into account by adding more discussion of this point, but modelling results and scaling arguments indicate that the loss of area is dominant on multidecadal timescales. Accepted that local thinning rates can be much greater than 1 m yr-1, as now stated.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				analyses at Tyndall Glacier (Patagonia), where the b-H feedback is by far the most important influence on current developments (accelerated shrinkage). Reference: Raymond, Ch., Neumann, T.A., Rignot, E., Echelmeyer, K., Rivera, A. and Casassa, G. (2005): Retreat of Glaciar Tyndall, Patagonia, over the last half-century. Journal of Glaciology 51 (173), 239-247. [Wilfried Haeberli (Reviewer's comment ID #: 94-16)]	
10-1050	A	57:48		Please indicate the agreement between estimates of these models and observations of the present contribution to the rate of sea-level rise. E.g. from Fig. 4 of Raper and Braithwaite I get the impression that their estimate for the present rate is negative, unlike observed. Perhaps in this case my interpretation is wrong, but I could the authors discuss the agreement between their models and observations over the recent past? [Gerrit Burgers (Reviewer's comment ID #: 34-25)]	Rejected for this chapter. The comparison is done in chapter 9.
10-1051	A	57:57	57:57	A dash is missing in 2550% I am also unclear here whether the larger temperature increases as well as precipitation increases expected in glaciated regions have been incorporated (I believe they have been, but wording is unclear…) [Richard B. Alley (Reviewer's comment ID #: 4-17)]	Accepted and clarified.
10-1052	A	57:57	57:57	Typo? [European Commission (Reviewer's comment ID #: 2008-68)]	Accepted.
10-1053	A	57:57	57:57	Is it really 2550% (25x)? [Donald L. Forbes (Reviewer's comment ID #: 72-17)]	Accepted.
10-1054	A	57:57	57:57	The current text reads ".... which in this scheme offsets 2550% of the melting". Please correct. [Govt. of Germany (Reviewer's comment ID #: 2011-201)]	Accepted.
10-1055	A	57:57		should 2550 be 25-50? [John Mitchell (Reviewer's comment ID #: 180-18)]	Accepted.
10-1056	A	57:57		2550% ?? [Stefan Rahmstorf (Reviewer's comment ID #: 206-21)]	Accepted.
10-1057	A	57:57		"2550%" should presumably be "25-50%" [Adrian Simmons (Reviewer's comment ID #: 242-157)]	Accepted.
10-1058	A	58:1	58:1	But generally the situation is that the increase comes mainly as rain, not snow--so has the potential to wash away existing snow. [Michael MacCracken (Reviewer's comment ID #: 152-277)]	Rejected. On the whole, rain does not wash away snow on glaciers. It may percolate and freeze, or it may run off. However it is true that precipitation increase does not necessarily mean accumulation increase.
10-1059	A	58:8	:8	Change 'considering also for the' to 'also allowing for the'	Accepted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Steve Harangozo (Reviewer's comment ID #: 98-32)]	
10-1060	A	58:12	58:21	Confusing paragraph. The reference to G&IC on Greenland and Antarctica adding 20% leaves unclear '[20%] of what' and '[contributions] to what'. [Donald L. Forbes (Reviewer's comment ID #: 72-18)]	Accepted and clarified.
10-1061	A	58:13	58:14	But since 1998 a lot has happened, and ice streams are now flowing quite rapidly. [Michael MacCracken (Reviewer's comment ID #: 152-278)]	Taken into account by adding a remark at the end of the paragraph.
10-1062	A	58:16	:17	No mention made of when (50 years time, 100 years or some temperature threshod) ablation will start to exceed accumulation giving a positive contribution to sea level rise [Steve Harangozo (Reviewer's comment ID #: 98-33)]	Available information will be summarized
10-1063	A	58:19	58:20	This is completely wrong. Page 67, lines 9-10 correctly state, "The present SMB of Greenland is a net accumulation estimated as 0.6mm…" On page 58, we read that indeed even the Antarctic Peninsula was accumulating 1972-98, and Davis (2005) showed a net accumulation averaged over the entire continent. So the statement, "In recent years the G&IC on Greenland have together added about 20%" is dead wrong; this means that the sea-level rises which were adjusted by a factor of 1.2 are also wrong, which means the mean sea level rise should revert to what it was in the first order draft! This is very important because I suspect IPCC will get picked apart if they allow this inconsistency to stand. [Patrick Michaels (Reviewer's comment ID #: 176-40)]	Rejected. There has also been dynamical acceleration on the Antarctic Peninsula. The 20% is an observational estimate based on Dyurgerov and Meier, discussed in chapter 4, but is reasonably consistent with model expectations, as shown here.
10-1064	A	58:20		The 20% merits a separate discussion of at least a paragraph on G&IC in Greenland and Antarctica in view of recent publications of accelerated melting [same comment as to page 61, line 20-38]. [Gerrit Burgers (Reviewer's comment ID #: 34-26)]	Taken into account by trying to make clearer that this paragraph is indeed all about the G&IC on G and A, and adding a sentence about dynamical acceleration.
10-1065	A	58:20	:21	Last sentence in para. is not clear, e.g. 'to allow for them'. [Steve Harangozo (Reviewer's comment ID #: 98-34)]	Accepted and clarified.
10-1066	A	58:23	58:29	(i) The method that is followed here is not quite clear, in particular the relation to the numbers quoted previously. Can you describe the method in such detail that an outsider could check the number that you obtain, at least in principle? What is the estimate of these models of the G&IC contribution to the present rate of sea-level rise? (ii) Please mention also results for B1 and A2 scenario's. [Gerrit Burgers (Reviewer's comment ID #: 34-27)]	Taken into account by putting further methodological information into an appendix. Comparison with observations is made in chapter 9. All available AOGCM reults will be given.
10-1067	A	58:23		Please add some text to indicate whether the 0.15 C warming amount refers to regional warming over Greenland or to global mean warming. [Martin Manning (Reviewer's comment ID #: 155-98)]	Accepted and clarified.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-1068	A	58:27	58:29	What about the other SRES scenarios? [European Commission (Reviewer's comment ID #: 2008-69)]	All available AOGCM results will be given
10-1069	A	58:27	58:29	Again, here are only the A1B results given. Please provide results for - at the minimum - all 6 SRES marker scenarios, preferably in tabular format. [Govt. of Germany (Reviewer's comment ID #: 2011-202)]	All available AOGCM results will be given
10-1070	A	58:28	58:28	Firsts, it would be helpful to give this number also as a percentage of the base amount. Second, this total seems surprisingly low compared to estimates in earlier IPCC assessments, and I would think that some connection to those numbers is needed. [Michael MacCracken (Reviewer's comment ID #: 152-279)]	Rejected idea of comparison with base amount, because this is itself uncertain so it would not be clear. We think this is really only of academic interest here, although important on the long term and mentioned in 10.7. All available AOGCM results will be given
10-1071	A	58:33	58:33	Change "could" to "is very likely"--it is simply not possible that changes will not occur--for Greenland, changes have occurred since the LIA so we know it changes; similarly we know both ice sheets were different during the Eemain. Saying "could" really underplays the challenge being faced. [Michael MacCracken (Reviewer's comment ID #: 152-280)]	Taken into account by changing to "can" since this is a general statement rather than a prediction.
10-1072	A	58:39		Spell out Surface Mass Balance the first time the acronym is used – and don't abbreviate in the subheadings [Martin Manning (Reviewer's comment ID #: 155-99)]	Taken into account. It was spelled out in the previous paragraph, but we have moved it to here. I can't see any occurrences in subheadings.
10-1073	A	58:39		"SMB is immediately influenced by climate change". Is not change in the SMB part of climate change itself? [Adrian Simmons (Reviewer's comment ID #: 242-158)]	Rejected, because this feels like a matter of definition. On decadal timescales, it is more usual to regard land ice change as a consequence of climate change rather than an aspect of climate change, although they are linked.
10-1074	A	58:42	58:42	Do you really mean "underestimated"--this seems contrary to the point made in the sentence which implies, and I would agree, is that smoothed orography leads to underestimation of snowfall at the mountains, and allowance for precipitation to spread too far inland (e.g., inland onto Antarctica and Greenland--so perhaps over-estimating inland snow buildup).. [Michael MacCracken (Reviewer's comment ID #: 152-281)]	The reviewer is correct in explaining why "overestimated" is correct! However it may be clearer without "orographically forced", so this has been deleted.
10-1075	A	58:45	58:47	Given these uncertainties (and I though GCMs did refreeze meltponds rather than let them runoff), one should be quite cautious of their projections. Another factor that should be mentioned about the edges of the ice sheets is that being more perpendicular, they expose	Noted. No change made. This sentence is an assessment of the literature on SMB simulated by AGCMs. We do not

No.	Batch	Page:line From	Page:line To	Comment	Notes
				a greater face to the low angled solar radiation, tending, I would suggest, to enhancing ablation. Also, at these angles, reflected solar radiation can be reflected onto other snow areas and not just out to space. My guess is that we could name as many feedbacks going one way as the other. [Michael MacCracken (Reviewer's comment ID #: 152-282)]	use AGCM SMB directly, as stated in the next paragraph.
10-1076	A	58:48	58:52	The references are already somewhat out of date; reference to those in chapter 4 may be helpfu. [Richard B. Alley (Reviewer's comment ID #: 4-18)]	Reference to 4.6.3.1 inserted. The reason for the references given is that they are climate change studies referred to in this section.
10-1077	A	58:51	58:52	I do not at all understand how a degree day system would work. It is not the warmth of the air (i.e., conduction of heat) that does most of the melting--it is the flux of radiation to the ice (often in the IR). This is clear from the fact that snow and ice can disappear even with temperatures below freezing (via sublimation) and that once one gets to the freezing point, temperatures are more or less hooked there while melting occurs. Thus, if models are using degree day systems, no wonder they get such low estimates of loss. I would think one would really need to be working on verification studies to see if a degree day system worked. Nor should just perturbing temperature be considered adequate--one has to perturb the downward IR due to the enhanced CO_2. [Michael MacCracken (Reviewer's comment ID #: 152-283)]	Noted. No change made. Such schemes are widely used and calibrated against observed SMB changes. Section 4.6.3.1 cites the most recent examples e.g. Hanna et al for Greenland.
10-1078	A	59:6	59:7	This rate of increase, even if valid given how models underestimate the removal of moisture by sharp orographic features, cannot be indefinite--eventually one gets above freezing, or even so some of the precipitation comes as rain, so one really needs to give bounds on this value--over what temperature range. As indicated in the letter that Richard Alley sent in in regard to the TAR, I believe his analysis of observations gives a negative correlation of snowfall on Greenland and NH temperature anomaly. I do hope observations are being given weight in such unusual conditions. [Michael MacCracken (Reviewer's comment ID #: 152-284)]	Noted. The change of phase is mentioned in the previous line and included in the models used here. The sign of the relation between precipitation and temperature depends on which timescale in the past is being considered, as discussed at lines 10-12. Richard Alley is an LA of the AR4 and has reviewed this section.
10-1079	A	59:10	:10	In the HadCM3 AOGCM relationship is' does not make sense. [Steve Harangozo (Reviewer's comment ID #: 98-35)]	Accepted and corrected.
10-1080	A	59:11	59:12	So, in this value derived for Antarctica, is any account taken of the tendency of models with smoothed orography to spread precipitation inland? This could be a very important factor. [Michael MacCracken (Reviewer's comment ID #: 152-285)]	Rejected. It is because of the model bias that AGCMs are not used directly to calculate SMB change. Instead, they are used to simulate perturbations, as explained in the first two paragraphs of this section, now stated more explicitly.
10-1081	A	59:14	59:15	The statement "Since there will not be substantial ablation in Antarctica" seems too strict.	Taken into account by deleting the

No.	Batch	Page:line From	Page:line To	Comment	Notes
				It is correct that low resolution GCMs do not indicate ablation zones developing but it also correct that large areas of snow melt are presently observed (Liu, H., L. Wang, and K. C. Jezek (2006). "Spatiotemporal variations of snowmelt in Antarctica derived from satellite scanning multichannel microwave radiometer and Special Sensor Microwave Imager data (1978–2004)." J. Geophys. Res. 111(F1): 1-20) and that warming rates around the continent are not well described by GCMs at present, for a variety of reasons. I would suggest rewording to add befire "there will" something like "it is likely that for the next 7 to 8 decades" [William Hare (Reviewer's comment ID #: 99-81)]	phrase, but we reject the suggestion because this is what the projections show and, as explained earlier, it is not directly from low-resolution GCMs but from use of high-resolution simulations and observations.
10-1082	A	59:14	59:14	This seems a rather cavalier statement, given how some ice streams are starting to move and the potential for various ice streams to become unpinned as ice shelves are lost due to oceanic warming. In addition, the increase in downward IR from higher CO2 and water vapor concentrations needs to be considered. [Michael MacCracken (Reviewer's comment ID #: 152-286)]	Taken into account by deleting the phrase, but we note that this section is concerned with SMB changes, not dynamical changes.
10-1083	A	59:14	59:16	Consider the following comment regarding Chapter 4-10 cross-issues and see if a rewrite would help. Change "all studies for the 21st century find that Antarctic SMB changes contribute negatively to sea level, owing to increasing accumulation (see Chapter 4, Section 4.6.3 for comparison with changes in the last decade)" to "Although Chapter 4 reports that Antarctica has been contributing to sea level during the last decade, all studies for the 21st century find that Antarctic SMB changes contribute negatively to sea level, owing to increasing accumulation." And then explain this apparent contradiction. Justification: The reference to Chapter 4 is so subtle that many readers would not realize that the Chapter 10 authors expect Antarctica to be much less sensitive to warming than the Chapter 4 authors suggest that it was in the past. [Govt. of United States of America (Reviewer's comment ID #: 2023-664)]	Accepted. Good suggestion.
10-1084	A	59:14		All studies? "NASA Mission Detects Significant Antarctic Ice Mass Loss", see http://www.nasa.gov/home/hqnews/2006/mar/HQ_06085_arctic_ice.html [Stefan Rahmstorf (Reviewer's comment ID #: 206-22)]	Rejected. That is not a study of SMB trends in the 21st century. It is about the mass balance including dynamics, in recent years.
10-1085	A	59:19	59:28	Include results of Kiilsholm etal in the GIS discussion and Table 10.6.1.(Kiilsholm, S., J. H. Christensen, K. Dethloff, and A. Rinke (2003). "Net accumulation of the Greenland ice sheet: High resolution modeling of climate changes." Geophysical Research Letters 30(9): art. no.-1485.) [William Hare (Reviewer's comment ID #: 99-82)]	These authors contacted for relevant data
10-1086	A	59:26	59:26	Have any of these models been tested for their ability to retrodict the recent changes? [Richard B. Alley (Reviewer's comment ID #: 4-19)]	No

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-1087	A	59:28		Table 10.6.1 - Good table as it provides some clarity. Please include the non-linear ablation rate terms for Antarctica as well, e.g. in terms of power-function coefficients. Furthermore, please apply these sensitivity results to - at least - the 6 SRES marker scenarios so that overall 2100 century sea level rise (and its uncertainty) is projected. [Govt. of Germany (Reviewer's comment ID #: 2011-203)]	Approval noted. Rejected suggestion to include non-linear fit as the literature does not provide the information for this. We will produce SL ranges for the marker scenarios.
10-1088	A	59:32	59:34	What are DP/DT if not averaged? Where do the 206 and 26 come from and why are they required? Not clear. [Donald L. Forbes (Reviewer's comment ID #: 72-19)]	Taken into account by clarifying the caption.
10-1089	A	59:39		There is no discussion in this section of the known shortcomings of glacier models (esp lack of surface melt penetrating through crevices to the glacier bed and lubricating the base) and the fact that these shortcomings likely cause the ice cap models to be more resistant to warming than could be the case in reality (i.e, that the projected sea level rise might be too little and too slow compared to reality). [Danny Harvey (Reviewer's comment ID #: 101-77)]	Rejected because there is exactly this discussion (now improved) at the end of the subsection.
10-1090	A	59:43	59:43	Change "Such dynamical responses are relatively minor" to, "In the present ice sheet models such dynamical responses are relatively minor". [William Hare (Reviewer's comment ID #: 99-83)]	Taken into account by deleting the sentence, which wasn't really useful.
10-1091	A	60:1	60:1	Are we sure that these changes are "future centuries"? I think this is unclear and in part depends on what is meant by large. I would suggest deleting "in future centuries" from this sentence. [William Hare (Reviewer's comment ID #: 99-84)]	Taken into account.
10-1092	A	60:2	60:2	Delete "simulations" and replace by "models" as it is the model that do not include the processes (they were not just left out of the simulation runs.) [William Hare (Reviewer's comment ID #: 99-85)]	Accepted.
10-1093	A	60:6	60:8	The Hindmarsh and Van der Veen & Whillans papers are real, were refereed, are old, and have been largely supplanted by the subsequent modeling showing the importance of ice shelves and by the clear observations showing this importance. Leading with these old papers does not really reflect the state of knowledge, as given in chapter 4. [Richard B. Alley (Reviewer's comment ID #: 4-20)]	Accepted.
10-1094	A	60:13	60:28	This needs a little restructuring as very unlikely as part of this part of the paragraph deals with the question of at what point and with what probability an ice shelf thermal limit might be reached (for the formation of meltwater ponds that threatened the integrity of the ice shelves) and another part deals with the observed responses. I suggest a new paragraph to deal with the ice shelf thermal viability limit issue directly. [William Hare (Reviewer's comment ID #: 99-86)]	Taken into account by rephrasing for clarity.
10-1095	A	60:13	60:28	NEW PARA ON ICE SHELF RESPONSE ATMOSPHERIC AND ON OCEANIC WARMING (BASAL MELTING): In addition to material on lines 13-28 this needs a	Rejected. We do not have space for a much larger discussion. Issues such as

No.	Batch	Page:line From	Page:line To	Comment	Notes
				brief mention of the surface temperature viability limit observed (around or a bit below 0oC in January). The line of argument as to the "very unlikely" probability of a 5oC summer warming occurring over eg the Ross Ice Shelf occurring within the 21st needs further back up (range of scenarios, etc) as it is quite flimsy at present. The chances of this level of warming (or some other level needed to increase surface temperatures to a threshold level in summer) is of course also related to circulation changes, which are also somewhat uncertain but which fundamentally affect the Ross Sea environment. In addition to surface warming projections for the effects of basal melting need to be included (eg Williams, M. J. M., R. C. Warner, and W. F. Budd (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 (Reviewer's comment ID #: 99-87)]	circulation change are included in the AOGCM SRES simulations. Basal melting is discussed in later paragraphs.
10-1096	A	60:14	60:28	I would note that one has to be very careful about changes in temperature--when sea ice disappears the temperature of a region can decrease because the freezing point of ocean water is below the freezing point of fresh water--in the Arctic study this led to the apparent paradox of sea ice disappearing but with coastal air temperatures dropping--the models had the physics right, it was those thinking about it that needed to change. In my view, one does not need the surface temperature of ice shelves to rise to start to get destruction--all one needs is more energy coming to them as part of the downward IR fluxes. That heat gets absorbed and the ice sheets stay at the freezing point, but meanwhile deteriorate. Were Feynman exposed to this analysis, he would, as he did in the analysis of the shuttle failure, get out a glass of ice water and explain that melting can occur with the temperature not rising. I just do not find this analysis at all physically compelling--it rather seems to me flawed. Just as the bottom of the ice can melt without the ocean temperature rising--just brining in more heat, so can the top of the ice--and its interior. [Michael MacCracken (Reviewer's comment ID #: 152-287)]	Rejected. Although temperature is not the direct cause of melting, there is plenty of empirical support (e.g. from the Antarctic Peninsula and from degree-day schemes) for the possibility of diagnosing melting from temperature.
10-1097	A	60:15	60:16	"global warming is" --will be? Has been? Unclear. [Richard B. Alley (Reviewer's comment ID #: 4-21)]	Accepted.
10-1098	A	60:15	60:20	This text is pretty hard to follow. The paragraph is mainly about Antarctica but did just mention Greenland above, so what are "these regions" on line 16? Why is there a switch from talking in terms of % increase to talking of an increase factor? Are the ranges given 1-sigma, 2-sigma or the full range of model results? [Martin Manning (Reviewer's comment ID #: 155-100)]	Taken into account. Regions clarified, factor used instead of %, ranges will be 5-95% (based on model distributions).
10-1099	A	60:19	60:19	"very unlikely" reflects a very high degree of confidence in the GCMs; I am very doubtful that the GCMs have been validated over ice to such a high level of confidence.	Accepted. Changed to "likely".

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Richard B. Alley (Reviewer's comment ID #: 4-22)]	
10-1100	A	60:20		There is a serious problem here. The use of very unlikely is not justified (see below). Fix this with a change in global range or the certainty description. Also, please do not mix units (absolute C for one shelf, relative C for another; % for one range, factor for another). Change "uncertainties are not well characterized but these figures suggest that a local summer warming of 5°C is very unlikely for a global warming of less than 5°C" to "….global warming of less than 3-4°C". Justification: The assertion is based on a previous sentence that says that summer warming will be 20-100% of the annual warming, which will be 70-150% of global warming. Combining those two factors implies that Antarctic summer warming will be 14-150% of global average warming. Given that range, without further analysis, it is reasonable to state that Antarctic warming is very unlikely to be more than 150% of global warming. However, this information provides no basis for asserting that it is very unlikely (or even unlikely) that summer warming will be more than 100% of global warming—100% seems to be well within the range of possibility. [Govt. of United States of America (Reviewer's comment ID #: 2023-665)]	Accepted. Change to "likely" and used factors rather than %.
10-1101	A	60:20	:23	The logic of this reasoning was difficult to follow (i.e., why the entire summer has to be above freezing for significant melting). If this is intuitive and can be explained as a modeling result, then try to help the reader here. Please consult Chapter 4 authors to determine whether Chapter 4 will be revised to provide some support. One obvious issue is whether the interannual and monthly variabilities are small enough so that high temperature periods (above the 0 C mean) are barely warmer than the seasonal average and hence do not lead to much melting. [Govt. of United States of America (Reviewer's comment ID #: 2023-666)]	Taken into account.
10-1102	A	60:27	60:28	How definite is this statement? Is this confirmed by the literature? [European Commission (Reviewer's comment ID #: 2008-70)]	Will be discussed with Ch 3
10-1103	A	60:27	60:28	Rephrase current sentence "but this has not been attributed to anthropogenic climate change and could be connected with variability in the Southern Annual Mode. " Unless this statement is based on studies, that rule out a anthropogenic causation, this sentence is misleading. Rephrase to something like "..., and this might be either attributed to anthropogenic climate change or to Southern Annual Mode variability." [Govt. of Germany (Reviewer's comment ID #: 2011-204)]	Will be discussed with Ch 3
10-1104	A	60:27	:28	their cause lies in the area's ice shelves caused by oceanographic changes' is not clear. [Steve Harangozo (Reviewer's comment ID #: 98-36)]	Accepted and amended.
10-1105	A	60:34	60:35	I believe this discussion of the modeling is misleading. In the Payne et al model, a forcing was applied, but then "turned off" (so, if one wishes to interpret the grounding-line retreat as arising from warming of the ocean waters, there existed an instantaneous warming that forced the grounding-line retreat, and then no further warming and no	Accepted. We have put "one-off" in this paragraph too.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				further forcing occurred). Other positive feedbacks were omitted, as noted in the discussion. In the Dupont paper, in addition to single-event forcing, the model did not allow the perturbation to extend out of the ice stream into the ice sheet. (Payne et al did not allow thinning of the ice stream to cause flow into it from the sides.) Noting the approach to new staeady-state without noting these caveats gives an odd view of the results. (And yes, "one-off" forcing is referred to in the next paragraph, but the referent is not made specific. [Richard B. Alley (Reviewer's comment ID #: 4-23)]	
10-1106	A	60:40	:40	use 'could float', not 'could be floated' [Steve Harangozo (Reviewer's comment ID #: 98-37)]	Accepted.
10-1107	A	60:41	60:42	"Most of inland ice of West Antarctica is grounded below sea level so could be floated if it thinned sufficiently" doesn't scan properly - "and so it could be"? [Richard Hindmarsh (Reviewer's comment ID #: 106-13)]	Accepted.
10-1108	A	60:41	60:47	With Greenland (and other factors) likely to contribute to sea level rise, has this effect been considered with regard to its impact on the Antarctic Ice Sheet. During the Eemian, perhaps it was the melting of the GIS that led to sea level rise that in turn affected the WAIS as the Antarctic region was likely anomalously cold given the orbital element alignment. [Michael MacCracken (Reviewer's comment ID #: 152-288)]	Rejected as there is no literature support for this. Note that even the entire removal of the Greenland ice sheet (7 m SLE) would be a relatively small sea-level forcing of the WAIS compared with the last deglaciation (120 m SLE) and hence there is no evidence it would have a large effect. This is even more true of the <1 m SL rise from thermal expansion etc. in the 21st century.
10-1109	A	60:46	60:47	"but is difficult to model (Vieli and Payne, 2005)." is a bit empty, perhaps "but is difficult to model, (Vieli and Payne, 2005) mainly because of its poorly understood dynamics ." [Richard Hindmarsh (Reviewer's comment ID #: 106-14)]	Taken into account. It is the numerics which present difficulties.
10-1110	A	60:47		Please explain whether sea level rise caused by other factors can also cause a dynamic retreat of the grounding line for the same reasons as thinning of the ice. This is a projections chapter and sea level is indeed projected to rise. [Govt. of United States of America (Reviewer's comment ID #: 2023-667)]	See 10-1108.
10-1111	A	60:49	60:54	Yes, one cannot absolutely prove yet that the water is going to the bed, but this also seems to misstate the strength of observations. The data of Zwally are very hard to get around, and, in many places, lots of clean water goes into holes in the ice, and lots of dirty water comes out from the front. Access to the bed is highly likely. The lack of variability in someplaces is a fascinating topic, and deserves mention as given. [Richard B. Alley (Reviewer's comment ID #: 4-24)]	Accepted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-1112	A	60:49	60:54	An additional effect of the runoff, as mentioned elsewhere, is that energy need not be devoted to vaporization, but can be used to melt more ice if runoff is really removing the meltwater. (Lonnie Thompson mentions this as a key amplifying factor in melting rate). [Michael MacCracken (Reviewer's comment ID #: 152-289)]	Rejected. See 10-1043.
10-1113	A	60:49	60:54	It is well-known that much of Greenland was warmer than it is now for several decades in the early 20th century, and that the low point in smoothed temperatures was very recent. So, if 3-5 years of warming (to temperatures still below those measured early in the 20th century) produces some type of acceleration from below, then that should have been obvious in the early 20th century! You need to add something on this in here in order to prevent criticism from outside. [Patrick Michaels (Reviewer's comment ID #: 176-41)]	Covered in Ch 3 / Ch 4
10-1114	A	61:13	61:18	The Antarctic ice sheet projection is at variance with recent observations: is it projected that the current imbalance will reverse? If not then something has to be done about the way in which these model projections are discussed and used. It seems wrong to me that such a big gap is unexplained anywhere here. The present loss rate (mid estimate) if continued until 2100 would add 20mm to SLR or about 75mm more the estimate presented here (-55mm). The GIS projections are not at such a great variance with observations - if the present decadal loss rate continues around 21mm of SLR will come from this ice sheet by 2100. If the acceleration observed over recent decades continues for one more decade the SLR would be around 29mm and so on. In this context it is worth noting that Chapter 9 page 42 lines 36/47 notes that the dynamic ice discharge from both ice sheets not included in the models is sufficient to explain the difference between observations and models eg "These dynamic terms are sufficient to explain the difference between model and observations" [William Hare (Reviewer's comment ID #: 99-88)]	Accepted. We will clarify how the 1993-2003 rate of SL rise is caused. We note that natural and internal variability may have contributed to its high rate.
10-1115	A	61:13	61:18	In my view these uncertainty estimates are far too narrow, as they seem to ignore the potential for ice streams to move, the potential for enhanced melting as a result of runoff of meltwater, and possibly the increased downward IR from the added CO2 and water vapor--at least in the continental ice sheet model. In addition, given that paleoclimatic evidence seems to indicate that a few degree warming led to a relatively rapid loss of about half of the Greenland ice sheet, these results seem at odds with paleoclimatic evidence. In my view, much larger uncertainty limits are needed (especially on the melting side). And simply mentioning some of these possibilities in the next paragraph and not including them in the numerical estimates seems unacceptable to me. [Michael MacCracken (Reviewer's comment ID #: 152-290)]	Taken into account. Dynamics is discussed further in the next paragraph. We will include an allowance for ice-sheet dynamical acceleration in the projections. Rejected remark about runoff; see 10-1043. Rejected remark about CO2 and water vapour, since these effects are included in the AOGCMs on which projections are based.
10-1116	A	61:14	61:15	Looks like inconsistent treatment for Antarctica and Greenland (-40 ± 20% and ± 20% respectively). Is this -40[%] ± 20% for Antarctica? Is it just ± 20% for Greenland (no	Taken into account by clarifying.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				mean value)? [Donald L. Forbes (Reviewer's comment ID #: 72-20)]	
10-1117	A	61:18	61:18	I believe it is important to note here whether these models are able to retrodict the changes in the ice sheets as summarized in Chapter 4, for surface changes (Greenland) and total changes (Greenland and Antarctica). [Richard B. Alley (Reviewer's comment ID #: 4-25)]	See 10-1086.
10-1118	A	61:18		(i) Is there any correlation between the Greenland and Antarctica contributions in these runs? (ii) What are the numbers obtained for the B1 and A2 scenario's? One would expect that the 0.2-3.9mm/yr of page 67, line 37 is mentioned here too. (iii) How do these model estimates compare to recent observational estimates for past and present? [Gerrit Burgers (Reviewer's comment ID #: 34-28)]	(i) Rejected, as no specific action is called for. (ii) Accepted; we will produce ranges for the other marker scenarios. (iii) Rejected; this is dealt with in 9.5.2.
10-1119	A	61:20	61:38	I would appreciate very much if the authors in their estimate the ice sheet estimate can account for the dynamical contribution of ice-sheets to sea-level rise. What I mean is that the final numerical range should cover with "very high confidence" (9 out of 10, see TS) the sea-level rise in 2100, and that a larger sea-level rise due to dynamical effects falls in the "very unlikely" class. This is different from the approach taken now, where one simply discards dynamical effects in the calculations of the numerical range. E.g. the authors could decice that a dynamical contribution of more than 5cm is "very unlikely" , and between 2cm and 5cm it is unlikely, and so on, and arrive finally at a conclusion - I would expect they use e.g the results presented in section 10.7.4.3 and 10.7.4.4. I realize this is very very difficult, but I do urge the authors to give the "best estimates they can make on the basis of our knowledge so far". [Gerrit Burgers (Reviewer's comment ID #: 34-29)]	Accepted. We will include an allowance for ice-sheet dynamical acceleration in the projections.
10-1120	A	61:20	61:38	I expect at least a paragraph on G&IC in Greenland and Antarctica in view of recent publications of accelerated melting [same comment to page 58, line 20]. [Gerrit Burgers (Reviewer's comment ID #: 34-30)]	See 10-1064. There is not enough space to repeat the material here. These G&IC are included in the projections.
10-1121	A	61:20	61:21	This does not seem to be in line with recent observations. How do the authors explain the apparent mismatch between the models and the observations? [European Commission (Reviewer's comment ID #: 2008-71)]	Rejected. The present imbalance is still quite a small SL effect, so there is no conflict with present-day obs. However, we will clarify how the 1993-2003 rate of SL rise is caused.
10-1122	A	61:20	61:21	The statement "The net ice sheet contribution is thus likely to be relatively small, unless larger dynamic changes occur of the kind currently observed …." seems to point to one fundamental problem of the SLR chapter and possibly the ice-sheet model community in general. Recent observations suggest large gaps in our knowledge of ice-sheet dynamics. Thus, rather than starting out by specifying numbers based on current ice-sheet model best guesses, the chapter might want to start out with these observations, the mismatch to	Accepted. We will include an allowance for ice-sheet dynamical acceleration in the projections.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				current ice-sheet models and provide the reader with a couple of well distinguished sensitivity cases to discuss potential implications for future SLR rise (e.g. by assuming different categories of possibly dynamics,e.g. temporary feature, constant rate, rate linear in temperature, rate overproportionally increasing with temperature etc..). Thus, this would help the reader to understand possible best- and worst-case estimates, which would properly reflect the substantial uncertainty in future ice-sheet SLR contributions. Alternatively, the present understanding should be clearly flagged as insufficient for prediction of possible speed and extent of these newly observed dynamics. [Govt. of Germany (Reviewer's comment ID #: 2011-205)]	
10-1123	A	61:20	61:38	Qualitative scenarios [William Hare (Reviewer's comment ID #: 99-89)]	Rejected. No change suggested.
10-1124	A	61:33	61:33	"indicative upper limt " - is this the right word. Perhaps "observationally bounded upper limit" which allows revision if the 2m/year proves atypical. [Richard Hindmarsh (Reviewer's comment ID #: 106-15)]	Rejected. Others argue that 2 m/yr is a lower rather than an upper limit, so "indicative" seems right.
10-1125	A	61:40	61:56	Many readers will be looking for a figure showing the overall SLR projections, as in the TAR. In Chapter 5, Question 5.1, Figure 1 goes part way by showing a range of projected SLR for the 21st century, but it is not clear what models are used and what is the meaning of the limits. Contrasting the results for the subset of models with results falling within the observational range could also be shown in a figure. [Donald L. Forbes (Reviewer's comment ID #: 72-22)]	Accepted. We will provide such a figure.
10-1126	A	61:40	62:14	These estimates seem totally unacceptable as indicative of what could happen. It might well be that the satellite observed rate is not a natural variation--after all, the earlier estimates of a rate that high are limited by being tide gage stations and being unsupported by a summation of well-defined terms as is the current rate. Thus, it seems to me that for an upper limit for the next 20 years, one likely should use the upper limit of the observed rate and perhaps a hint of acceleration--but even the mean of the current rate is well above the 20 year projection, which seems a terribly biased way to look at the data (I admit I may be taking the other view, but having the projected mid range rate for the next 20 years be 40% less than the current observed rate seems unjustifiable to me, given we have no real basis for assuming the current rate is unduly affected by natural variability and warming will be accelerating). It seems to me the authors need to really, including ice stream effects, come up with plausible upper and lower estimates that are consistent with past data and incorporate, to the extent possible, the model results, including perhaps by scaling the terms up to match observations during the 20th century, etc. I also think it essential for there to be a full discussion that compares and reconciles these results to those of the TAR. And I think these estimates need to consider the full range of emissions scenarios the IPCC is considering--not just picking a few.	Accepted. We will take the present rate as the central estimate of the dynamical imbalance. We will include an allowance for ice-sheet dynamical acceleration in the projections. We will produce SL ranges for the other marker scenarios.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Michael MacCracken (Reviewer's comment ID #: 152-291)]	
10-1127	A	61:40	62:14	I do not like the way this section is currently treated as it is not linked to the earlier IPCC assessments. This is a very important section which will be straight where coastal impacts people will first look -- many may not look elsewhere. The take home message that I get is that 21st century global-mean sea-level rise is 140 to 440 mm above 2000 levels, or 210 to 430mm. Of course I could go 2 standard deviations but this is still about 100 to 540 mm using the second estimate. The reader needs to know why these numbers are so much smaller than the TAR range of 9 to 88 cm rise? If the uncertainty has been reduced so dramatically compared to earlier IPCC assessments this deserves some discussion -- how confident are the review team in these new estimates? Addition of a discussion of these points is essential. [Robert Nicholls (Reviewer's comment ID #: 191-6)]	Taken into account. Unlike the TAR, chapter 10 is not producing a combined scenario range for temperature or SL, but we will produce SL ranges for the marker scenarios. We will compare with the TAR for individual scenarios.
10-1128	A	61:40		Section 10.6.5. I miss in this section: (i) A table that summarizes estimates for various contributions and their uncertainties. It would help enormously if this table would have the identical format as table 5.5.2 (Chapter 5, page 34). (ii) A discussion of sea-level rise in different scenario's (B1, A1B and A2) or in different classes of global temperature rise (e.g. below 2.5K, 2.5-3.5K and above 3.5K in 2100). (iii) An explicit indication using the terminology of the Technical Summary (page 4) how likely it is that "larger dynamic changes occur" (iii) a discussion of how the following sources of information are combined: (a) model estimates for separate terms in past and future (b) mismatch between models and observations for separate terms in present and past (c) mismatch between estimates for total sea level rise and contributions of separate terms in present and past. In particular, I expect a thorough explanation how the lower bound on sea-level rise is lower than observed 20th sea-level rise while generally higher temperatures go hand in hand with higher sea level. [Gerrit Burgers (Reviewer's comment ID #: 34-22)]	Taken into account. (i) If space permits. (ii) We will produce SL ranges for the marker scenarios. (iii) We will include an allowance for ice-sheet dynamical acceleration in the projections but without uncertainty estimates since there is no basis for these, given the current state of understanding. (iv) We will clarify how the 1993-2003 rate of SL rise is caused and how this relates to the projections.
10-1129	A	61:40		Section 10.6.5. I would like to see more space devoted to this section and more detail. [Donald L. Forbes (Reviewer's comment ID #: 72-21)]	Accepted.
10-1130	A	61:40		10.6.5 This section is misleading. What is the logic behind the assumption that the missing 0.7 mm/yr are constant and independent from temperature? Chapter 6 makes it clear that SLR is practically all modern, there is no paleoclimatic background rate that could reasonably be assumed to be constant. Thus these 0.7 mm/yr are most likely caused by global warming, and they indicate that the models are underestimating the sea level response to global warming. Since the observed rate 1961-2003 is given as 1.8 mm/yr, I assume models give a rate of 1.1 mm/yr, which means that models are underestimating past sea level rise by about 40%! How much trust should we put in models like that in projecting the future? I think it is absolutely vital for the honesty of the report to state this	Taken into account. We will clarify how the 1993-2003 rate of SL rise is caused and how this relates to the projections.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				underestimation very clearly - the way this section is currently written, it obscures rather than clarifies this fact. This to me seems one of the most important results of the AR4 compared to the TAR: the observed rate of sea level rise is larger than what models projected in the TAR and what current models give. Hence we have to take seriously the possibility that sea level rise will proceed faster than previously thought. Currently, chapter 10 gives the opposite impression: the message of this chapter now seems to be that IPCC is correcting its sea level projections downward as compared to the TAR. That surely can't be right, and is in no way justified by the published science. [Stefan Rahmstorf (Reviewer's comment ID #: 206-23)]	
10-1131	A	61:42		SECTION 10.6.5 Projections: I think this section needs to 1) include all of the scenarios used in the warming projections 2) clearly separate projections by term (thermal, glaciers, ice sheets) and discuss each of these and compare with observations and discuss the implications for the overall projections. [William Hare (Reviewer's comment ID #: 99-90)]	Taken into account. (1) We will produce SL ranges for the other marker scenarios. (2) This is done in previous subsections.
10-1132	A	61:42	:47	Please add a table with the models used for sea-level rise projections, and the primary contributors to the sea-level rise (Antarctica, Greenland, ...) for the year 2100 and possibly an intermediate year such as 2050. Coastal planners and engineers preparing for sea level and other users of IPCC sea-level rise projections have often needed to cite the contributions from different sources, which has been supported in past assessments. In addition, please report either the historic contribution that the models estimate, or report projections both as an absolute rise over a base year, and as an acceleration over current or historic trends. [Govt. of United States of America (Reviewer's comment ID #: 2023-668)]	Taken into account. We will clarify how the 1993-2003 rate of SL rise is caused and how this relates to the projections. We will show a diagram of the projections for various scenarios at 2100. Components are given in previous subsections.
10-1133	A	61:42	:49	Results (without full uncertainty analysis) must also be present for the B1 and A2 scenarios as high and low projections. This is critical to avoid the appearance of selection. Either give the calculated numbers or make an expert judgment. Going without looks biased. [Govt. of United States of America (Reviewer's comment ID #: 2023-669)]	Accepted. We will produce SL ranges for the other marker scenarios.
10-1134	A	61:48	61:48	How do the authors explain the reduced uncertainty in the light of recent observations of more rapid ice flow on Greenland? What do the results look like for each individual SRES scenario? [European Commission (Reviewer's comment ID #: 2008-73)]	First point taken into account by changing the way we relate the projections to the 1993-2003 rate of SL change. Second point accepted; we will provide SL ranges for the other marker scenarios.
10-1135	A	61:48	61:48	Please follow the good practice of the other chapter and compare your results to the TAR. Please provide a detailed reasoning, why the AR-4 result of 290+-150mm (or 320+-100mm) by 2100 differ markedly from the TAR upper bound of 88cm? Is the uncertainty	Accepted. We will produce SL ranges for the marker scenarios. We will compare with the TAR for individual

No.	Batch	Page:line From	Page:line To	Comment	Notes
				substantially reduced? Aren't current observations of ice-stream acceleration rather point to higher uncertainty, and therefore possibly to a higher upper bound? Again, please provide results separately for the 6 SRES illustrative emission scenarios and in tabular form. [Govt. of Germany (Reviewer's comment ID #: 2011-206)]	scenarios.
10-1136	A	61:48	61:56	This section of the paragraph should be amplified and broken up into a discussion of each term and how the model estimate relate to observations both over the 1961-2003 and the 1993-2003 period. The argument about natural variability is weak and needs further justification as there are sound physical reasons for an acceleration in SLR, as observed. Qualitative scenarios for SLR for the ice sheets may need to be used because of the obvious lack of closure between models and ice sheet observations. [William Hare (Reviewer's comment ID #: 99-91)]	Taken into account. We will clarify how the 1993-2003 rate of SL rise is caused. We will include an allowance for ice-sheet dynamical acceleration in the projections. Components are dealt with in previous subsections.
10-1137	A	61:49	:56	If not otherwise included in a table, please either (a) report the projected sea-level rise as an acceleration compared with current or historic trends, or (b) report the historic contributions to sea-level rise estimated by the models used to project future sea-level rise. Justification: Coastal planners and engineers, and other users of IPCC projections, have to make local sea-level rise projections. Although there are several methods for doing so, many people follow the approach recommended by USEPA, which is to add the current local trends (based on tide gages) to a model-derived estimate of the acceleration. That method can only use IPCC projections if IPCC reports enough information for one to determine their estimated acceleration. In that regard, it may be worth noting that some models project a rise in sea level of less than 15 cm—less than the rise over the last century. But those models do not project a deceleration; they simply have a baseline lower than historic trends. Nevertheless, if acceleration and/or the baseline for the model are not reported, some readers might be left with the impression that the low scenario is a deceleration of sea-level rise when in fact it is a small acceleration projected by a model that just happens to have a very low baseline. By analogy, if a GCM had a baseline that was 2°C colder than today and projected a 1°C warming, you would not leave readers with the impression that such a model implies a 1°C cooling. Leaving readers with the impression that the model implies a deceleration of sea-level rise provides a similarly incomplete picture. [Govt. of United States of America (Reviewer's comment ID #: 2023-671)]	Taken into account by modifying the projections to match the 1993-2003 observations.
10-1138	A	61:50	61:52	Where does the 0.7mm/yr come from? [European Commission (Reviewer's comment ID #: 2008-72)]	Rejected. As stated, it is described from 5.5.6 and 9.5.2. However, we will clarify how the 1993-2003 rate of SL rise is caused.
10-1139	A	61:50	61:52	Please discuss the mismatch to observations in more detail, e.g. whether this mismatch	Accepted. We will clarify how the

No.	Batch	Page:line From	Page:line To	Comment	Notes
				could be due to G&IC contributions around the Antarctic Peninsula, WAIS etc… Most importantly, justify your choice of the worst case being a constant budget term of 0.7mm/yr. Why can you rule out a non-accounted process that might scale with temperature? For example, why can the current mismatch not be explained by, e.g., a process that contributes with a rate of 1 mm/yr/K, or 1mm/yr/K^2? If such process dynamics cannot be ruled out, please provide sensitivity results for such worst-cases, too. Furthermore, please provide all contributions in a tabular and graphic form, as the current text with lots of different numbers for different emission scenarios and different contributions is difficult to digest. [Govt. of Germany (Reviewer's comment ID #: 2011-207)]	1993-2003 rate of SL rise is caused. We note that natural and internal variability may have contributed to its high rate. We will include an allowance for ice-sheet dynamical acceleration in the projections.
10-1140	A	61:54	61:56	What type of decadal variability could cause such large differences? The most pronounced type of decadal variability involves changes in circulation, which may affect thermal expansion to some extent - but entirely? One could look at the data presented in Chapter 5 or the model results mentioned in Gleckler et al. [Nature 2006)675] for an estimate. Decadal variability as an explanation for the increase in contribution of G&IC is even harder to imagine: probably rising termperatures are the most significant factor, and on timescales of 10 years and longer variability is less important than the trend when averaged over appropriate areas. Or am I wrong and is variability dominated by precipitation effects? [Gerrit Burgers (Reviewer's comment ID #: 34-31)]	Taken into account by extending the relevant discussions in chapters 5 and 9.
10-1141	A	61:54		What is the point of this if-statement? As global warming is proceeding and the world is substantially warmer in 1993-2003 compared to the period 1961-2003, we clearly expect that sea level rise should be accelerating in this situation, based on the observed global warming and simple physics. So the "if" assumption that this is entirely caused by internal variability is highly unrealistic and very likely wrong - why try and derive a conclusion from such an unrealistic assumption? [Stefan Rahmstorf (Reviewer's comment ID #: 206-24)]	Taken into account by clarify how the 1993-2003 rate of SL rise is caused, here and in chapters 5 and 9.
10-1142	A	62:1	62:3	The results here relate to only one scenario (A1B) and although the scenario spread is said to be small for 2020, it is quite important at 2100. How is the scenario spread distributed relative to the A1B range and are the uncertainties under other scenarios of the same magnitude as given here for A1B? [Donald L. Forbes (Reviewer's comment ID #: 72-23)]	Accepted; we will produce SL ranges for the marker scenarios.
10-1143	A	62:5	62:5	Change to "Model based estimates indicate that thermal expansion is likely to be the largest component, however significant uncertainty surrounds ice sheet projections". [William Hare (Reviewer's comment ID #: 99-93)]	Taken into account.
10-1144	A	62:16	62:16	Replace "climate change" with 'changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1693)]	Rejected—no reason given for suggested change

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-1145	A	62:16		Section 10.7 Long Term Climate Change and Commitment: In addition to the literature cited see also Hare and Meinshausen (2005) for a discussion of the commitment concept. In this paper we developed an additional idea around plausible mitigation scenarios which are policy relevant (Hare, W. L., and M. Meinshausen (2005). "How much warming are we committed to and how much can be avoided?" Climatic Change, accepted) [William Hare (Reviewer's comment ID #: 99-92)]	Rejected—this paper is not available to the authors
10-1146	A	62:18	62:18	Replace "climate change" with 'changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1694)]	Rejected—no reason given for suggested change
10-1147	A	62:20	62:20	Replace "climate change" with 'changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1695)]	Rejected—no reason given for suggested change
10-1148	A	62:23	62:23	Delete "system" [VINCENT GRAY (Reviewer's comment ID #: 88-1696)]	Rejected—no reason given for suggested change
10-1149	A	62:24	62:26	The term "constant emission commitment" defined here is not used anywhere in the chapter – so why not drop it entirely to avoid confusion. [Martin Manning (Reviewer's comment ID #: 155-101)]	As noted in the paragraph, these are concepts related to commitment drawn from Wigley (2005), and are consistent with various interpretations of commitment in that paper. This information is provided as background—all of the terms are necessarily not used in the rest of this section, but all are provided at the outset to set context
10-1150	A	62:30	62:30	Delete "system" [VINCENT GRAY (Reviewer's comment ID #: 88-1697)]	Rejected—no reason given for suggested change
10-1151	A	62:38	62:38	Delete "system" [VINCENT GRAY (Reviewer's comment ID #: 88-1698)]	Rejected—no reason given for suggested change
10-1152	A	62:41	62:45	This is an important definitional information for the presented "commitment" results that have already been presented at various places before this section. Thus, please provide a definition of commitment along these lines as well at the beginning of the chapter and in the Executive Summary to avoid misinterpretations. [Govt. of Germany (Reviewer's comment ID #: 2011-208)]	Accepted—definition now accompanies this material in the executive summary
10-1153	A	62:53	62:53	10.3.2 should be 10.3.1.	Accepted—change made

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Govt. of Finland (Reviewer's comment ID #: 2009-141)]	
10-1154	A	63:6	63:6	For clarity repeat time point of emissions intervention. "For the B1 commitment run where emissions are stabilised at the 2100 level,…." [Govt. of Australia (Reviewer's comment ID #: 2001-435)]	Not quite right—this is for constant composition commitment, not constant emission; in any case, we now specify approximate CO2 concentrations for the two experiments above
10-1155	A	63:6	63:6	In referring to the 0.5 degree C rise (2100-2200), also indicate aggregate temperature rise since 1990. [Govt. of Australia (Reviewer's comment ID #: 2001-436)]	Rejected—we already note 6 lines previous that the warming of about 0.5C (in is what has been observed in the 20th century
10-1156	A	63:23	63:23	Replace "climate change" with 'changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1699)]	Rejected.
10-1157	A	63:25	63:30	Need to refer to Fig. 10.7.2 here. [Anthony Hirst (Reviewer's comment ID #: 107-5)]	Accepted.
10-1158	A	63:25	63:30	The 3000-year runs are complete absurdities and I will (again) argue that this should simply be removed from all of AR4. The assumption that CO2 goes to 4 X background and stays there for three millennia is a joke. No one knows what will power our society even a hundred years from now. Remove this section. [Patrick Michaels (Reviewer's comment ID #: 176-42)]	Rejected. WG1 does not assess scenarios.Commitment scenarios are academic scenarios and only used to illustrate timescales of atmospheric and oceanic response.
10-1159	A	63:37	63:38	In Fig. 10.7.2 why do several models feature jumps in surface warming? Are these realistic? [Govt. of Australia (Reviewer's comment ID #: 2001-437)]	Noted. Probably those are caused by switches in ocean convection which can be visible on global scale because of the coarse grid. But the authors are not in the position to discuss these points since the necessary type of data is not available.
10-1160	A	63:50	64:2	The sets of dots in panels c, f, and i of Figure 10.7.3 appear to show total C emmissions of about 1400 GtC for the 550 ppmv case, about 1700 GtC for the 750 ppmv case and only about 1780 GtC for the 1000 ppmv case (given the peak atmospheric concentrations as cited in the text on line 51). Need a sentence in this section to explain why there is such a small increase in total C emissions going from the 750 ppmv case to the 1000 ppmv case (roughly a 5% increase in total C emission for a 33% increase in peak atmospheric CO2 concentration). [Anthony Hirst (Reviewer's comment ID #: 107-6)]	Text clearly says that scenarios follow a pathway TOWARDS stable 550, 750ppm etc., but never reach those values because emissions are set to zero. The difference between SP750 and SP1000 in atm. CO2 is only about 10ppm, not 33%. Clarified 'targeting (but not actually reaching)' in the text.
10-1161	A	64:11	64:11	The time scale for establishment of a new equilibrium is likely to be somewhat model dependent, for example determined by the degree of meridional overturning decline and prescribed deep ocean vertical diffusivity. More accurate to write "(order several	Accepted. Changed 'four' to 'several'. Refs. given to Bi et al. (2001) and Stouffer (2004)

No.	Batch	Page:line From	Page:line To	Comment	Notes
				thousand year as estimated by AOGCM experiments)". Note that there has been more than one such AOGCM experiment, such as the GFDL expermient and the Bi et al. (2001) experiment, both mentioned later. [Anthony Hirst (Reviewer's comment ID #: 107-7)]	
10-1162	A	64:14		The formula at the end of this sentence requires estimates of radiative forcing due to CO2 increases as well as the global energy balance – so that should be mentioned. [Martin Manning (Reviewer's comment ID #: 155-102)]	Accepted.Added a sentence for clarification.
10-1163	A	64:23	64:30	The table caption is very repetitive of the surrounding text. It should not be necessary to re-state the formula here or to re-state the full provenance for the different climate sensitivity values chosen which is given in the chapter already. Just cite the text and keep the table caption short and sweet. [Martin Manning (Reviewer's comment ID #: 155-103)]	Accepted.
10-1164	A	64:27	64:30	This caption text seems redundant with the text on page 64, starting at line 34. Thus please consolidate. [Govt. of Germany (Reviewer's comment ID #: 2011-209)]	Accepted.
10-1165	A	64:31		Table 10.7.1. The table would be easier to follow if the "best guess" column was placed between the "likely between" column and the one to its right. [Martin Manning (Reviewer's comment ID #: 155-104)]	Rejected. The upper and lower bound of the likely range need to be together. However, to improve clarity, merged 'likely between' and 'and' columns into one.
10-1166	A	64:34	64:36	Repeats these points for the third time! [Martin Manning (Reviewer's comment ID #: 155-105)]	Accepted. Equation deleted and previous paragraph shortened.
10-1167	A	65:1	65:31	The numeric results presented in these two paragraphs have to be double checked. Several numbers appear to be wrong, e.g. a suggested non-CO2 contribution to B1 forcing of 11ppm CO2 equivalent is only possible when assuming unusually high aerosol (indirect) cooling towards the end of 2100. Following from this, an equilibrium warming due to B1 of 3.0°C above pre-industrial seems unusually low. Note that the referred table 6.14 in the TAR only lists radiative forcing changes between 2000 and 2100, and that net radiative forcing in 2100 is based on changes from pre-industrial (thus, include results of table 6.1 in the TAR). Furthermore, please check that all temperature increase numbers refer to the same reference period, i.e. pre-industrial. Currently, some numbers do, others refer to above 1980-1999. [Govt. of Germany (Reviewer's comment ID #: 2011-210)]	Accepted. Numbers corrected, conclusions are the same.
10-1168	A	65:1	65:31	Many of the quantitative values in these paragraphs, particularly the first one appear to wrong, or at least vary substantially from what I have calculated. These all need to checked. [William Hare (Reviewer's comment ID #: 99-94)]	Accepted. Numbers corrected, conclusions are the same.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-1169	A	65:1	65:31	Stouffer (2004) should be referenced in this discussion. In line 29? [Ronald J Stouffer (Reviewer's comment ID #: 258-151)]	Accepted.
10-1170	A	65:33		Section 10.7.3: There appears to be two distinct meeings of the word "overshoot" used in this section. Firstly "overshoot" as in "overshoot scenario" is taken mean a CO2 concentration scenario featuring a peaking and then decline back to a lower level. This is the meaning intended by the section title. Secondly, "overshoot" is used to refer to the situation where the warming for a given stabilisation scenario exceeds ("overshoots") a particular predefined magnitude (e.g., 2 C). Casual duel usage of the word could cause confusion. [Anthony Hirst (Reviewer's comment ID #: 107-8)]	Accepted
10-1171	A	65:36	65:40	Is this limitation of scope justifiable? Not assessing mitigation overshoot scenarios seems to be a too limited scope of WG I mandate and should be reconsidered.It is the task of WG1 to assess the physical implications of scenarios that consider climate policy. [European Commission (Reviewer's comment ID #: 2008-74)]	Rejected—WG1 does not have the expertise to assess mitigation scenarios, and these scenarios were not completed in time for inclusion in WG1 models
10-1172	A	65:36	65:40	IMPORTANT: The current text surprisingly states "A new suite of mitigation scenarios is currently being assessed for the AR4. WG1 does not have the expertise to assess such scenarios, …". Please provide a detailed clarification on this and work undertaken by the Joint WG3-WG1 Task Force on Mitigation scenarios. It would clearly be unjustified, if the assessment of the phsyical implications of this new set of mitigation scenarios would not be included in the final report. WG1 does not have to assess the economic and technological foundation of any new mitigation scenarios, just as WG1 was never asked to assess the economic and technological foundation for the SRES scenarios. This is outside of WG1's expertise. However, who, if not WG1, would have the expertise to assess the PHYSICAL CLIMATE SYSTEM implications of new emission scenarios? Thus, it seems warranted that Chapter 10 provides the reader throughout the chapter with results for mitigation scenarios, whether they are run with EMICs or extrapolated from AOGCMs with the help of tuned simple climate models. This does especially affect section 10.5.3. and the figures therein. [Govt. of Germany (Reviewer's comment ID #: 2011-211)]	Rejected—these new mitigation scenarios were not completed in time for inclusion in WG1 models
10-1173	A	65:45		I do not think the authors have the expertise to use the language "huge and likely implausible" and to do so seems argumentative rather than useful. To make a statement like this is completely at odds with the important statements earlier on that WG1 authors do not make judgements about emission scenarios. You should be consistent. [Martin Manning (Reviewer's comment ID #: 155-106)]	Accepted
10-1174	A	66:34	66:34	Grammar: replace with "… stabilization that must be chosen. [Anthony Hirst (Reviewer's comment ID #: 107-9)]	Accepted
10-1175	A	66:36	66:36	The labeling within Figure 10.7.5 as placed will likely be confusing to the lay reader and	This figure has been moved to

No.	Batch	Page:line From	Page:line To	Comment	Notes
				misrepresent the statistical concept presented. The labels should be linked with the LINES between the colored areas and no the areas themselves. For example, the label "likely (>66%)" is meant, I believe, to imply that there is a greater than 2/3 probability that for an atmospheric CO2 concentration of, say, 600 ppm, the surface warming threshold would be equal to or greater than 4 degrees C. For each atmospheric CO2 concentration on the vertical axis, the corresponding threshold temperature appears on the LINE separating the white area of the graph from the light-green area of the graph. However, as presented in the figure, the lay reader would probably conclude that the AREA of the light-green region would contain greater than 66% probability of future CO2 concentration/threshold temperature occurances. Repeating my point, the labels should correctly refer to the LINES, and not the AREAS. [Chuck Hakkarinen (Reviewer's comment ID #: 96-1)]	supplementary material
10-1176	A	66:38	66:37	Suggest the title is changed to "10.7.4 Commitment to Sea-Level Rise" as this describes the monotonic directional character of the change. [William Hare (Reviewer's comment ID #: 99-95)]	Accepted.
10-1177	A	66:38		Section 10.7.4: For added clarity, it seems warranted to shift this whole section with the previous sea level rise section 10.6. The section 10.7.4 does not primarily focus on the "constant composition" commitment run scenarios undertaken by the GCMs. However this section does discuss fundamental processes related to sea level rise and has therefore to be merged with section 10.6. [Govt. of Germany (Reviewer's comment ID #: 2011-212)]	Rejected. No organization of the material is ideal. We chose this one because we want to put the long-term projections for sea level in 10.7 with the other long-term aspects of climate change, although we recognize that separates it from related material in 10.6. However, we will insert cross-refs.
10-1178	A	66:38		Section 10.7.4.: The content of this section has to be made more accessible to the readers by providing the individual contributions, descriptions of processes, uncertainty ranges for long-term sea level rise in tabular or graphic form, separated for different emission scenarios or long-term temperature thresholds. [Govt. of Germany (Reviewer's comment ID #: 2011-213)]	Taken into account.
10-1179	A	66:44	66:45	A diluted version of the last argument applies here. Imagine it is 1700, and some learned folks are talking about what the world will look like in 2000, assuming that the number of horses is stabilized (nice pun) in 1800. No one would take this seriously, nor should they. Nor should this be in AR4. [Patrick Michaels (Reviewer's comment ID #: 176-43)]	Rejected. Even though idealized, such scenarios illustrate the magnitude of long-term commitment.
10-1180	A	67:9	67:10	This statement does not seem to take into account the new satellite studies, which show the mass of Greenland decreasing. Somehow these results need to be incorporated into the analysis.	Rejected. We are talking about *surface* mass balance here.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Michael MacCracken (Reviewer's comment ID #: 152-292)]	
10-1181	A	67:19		I can not follow where the threshold of 2.7 C applies from. Is it from today's temperatures? [Martin Manning (Reviewer's comment ID #: 155-107)]	Accepted and clarified.
10-1182	A	67:25	67:49	I recommend that reference to this discussion is made on p61, line 20-38, and that the results here are incorporated in the line of reasoning at page 61. [Gerrit Burgers (Reviewer's comment ID #: 34-32)]	Rejected. The discussion of the threshold is not necessary for deriving 21st century projections. However, we will insert cross-refs.
10-1183	A	67:25	67:28	One set of results is described here. Other work by Kiilsholm et al indicates a much higher sensitivity to local warming than the work cited (the Wild et al paper with high resolution modelling of the GIS shows lower) and the point is that the uncertainty in this is larger than implied. In a context where ablation and dynamic responses appear to be faster than models indicate it may be prudent to discuss this issue further here A further issue is the timescale of ice loss: the Eemian may also carry a message as high resolution SLR records (Lambeck etc see Chapter 6) seem to imply a faster rate of loss than models would indicate (see Kiilsholm, S., J. H. Christensen, K. Dethloff, and A. Rinke (2003). "Net accumulation of the Greenland ice sheet: High resolution modeling of climate changes." Geophysical Research Letters 30(9): art. no.-1485.) [William Hare (Reviewer's comment ID #: 99-96)]	Taken into account by pointing out that although this is only one paper, it uses a large number of models. Dynamic response is the subject of the next paragraph.
10-1184	A	67:25	67:49	There is much too much focus here on temperature change--that is not what really melts the ice. It is the change in radiation fluxes, and these can be quite large without a temperature change (we could have more clouds, just for example). It would be more useful, perhaps, to talk in terms of a longer melt season, but talking in terms of the change in temperature seems to me missing the main mechanism for melting--it is not conduction driven, it is radiation driven. As a result of this, I think the time scales here are likely much too long. [Michael MacCracken (Reviewer's comment ID #: 152-293)]	Rejected. See 10-1096.
10-1185	A	67:42	67:43	Same argument applies. The vision of the future 300 years from now is simply blank. [Patrick Michaels (Reviewer's comment ID #: 176-44)]	Rejected. These scenarios are idealizations which give indicative results.
10-1186	A	67:53	67:56	See comment #140 regarding irreversibility/regeneration of the Greenland ice sheet and orbital conditions for the next ice age. [Adrian Simmons (Reviewer's comment ID #: 242-159)]	See 10-301.
10-1187	A	68:5	68:10	Only one model is cited here. There is also the work of Warner and Budd (1998) which investigates amongst other things the effects of increased basal melting on ice sheet response to a 3oC global warming with the opposite results to that the work cited. (See	Rejected. Only one *paper* is cited here, but is uses several models (now stated). At this point we are talking about

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Warner, R. C. and W. F. Budd (1998). "Modelling the long-term response of the Antarctic ice sheet to global warming." Annals of Glaciology 27: 161-168. The primary effects of global warming on the Antarctic ice sheet can involve increases in surface melt for limited areas at lower elevations, increases in net accumulation, and increased basal melting under floating ice. For moderate global warming, resulting in ocean temperature increases of a few OC, the barge increase in basal melting can become the dominant factor in the long-term response of the ice sheet. The results from ice-sheet modelling show that the increased basal melt rates lead to a reduction of the ice shelves, increased strain rates and flow at the grounding lines, then thinning and floating of the marine ice sheets, with consequential further basal melting. The mass loss from basal melting is counteracted to some extent by the increased accumulation, but in the long term the area of ice cover decreases, particularly in West Antarctica, and the mass loss can dominate. The ice-sheet-ice- shelf model of Budd and others (1994) with 20 km resolution has been modified and used to carry out a number of sensitivity studies of the long-term response of the ice sheet to prescribed amounts of global warming The changes in the ice sheet are computed out to near-equilibrium, but most of the changes take place within the first few thousand years. For a global mean temperature increase of 3 degrees C with an ice- shelf basal melt rate of 5 m a(-1) the ice shelfs disappear within the first few hundred years, and the marine-based parts of the ice sheet thin and retreat. By 2000 years the West Antarctic region is reduced to a number of small, isolated ice caps based on the bedrock regions which are near or above sea level. This allows the warmer surface ocean water to circulate through the archipelago in summer, causing a large change to the local climate of the region. [William Hare (Reviewer's comment ID #: 99-97)]	*surface* mass balance. Basal melting could promote increased ice flow, and this is discussed in the next two paragraphs.
10-1188	A	68:5	68:10	Is it not the case that Antarctica was substantially smaller during the warm conditions of the Pliocene. This notion of Antarctica growing bigger and bigger as warming goes on seems in conflict with what has happened during the Holocene (the background is assumed to be a contribution to sea level rise, not the reverse) and during the Pliocene. It would seem to me that we should not be at all confident that the models are including the right processes for this evaluation, especially as the amount of downward IR increases. [Michael MacCracken (Reviewer's comment ID #: 152-294)]	Rejected. No specific action is suggested and indeed, we are not at all confident! As stated, there is little agreement about what dynamical changes could occur.
10-1189	A	68:19	68:23	The discussion of ice shelf issues need not be limited to the shelves surrounding the WAIS. The Amery Ice Shelf is also vulnerable (See Williams, M. J. M., R. C. Warner, and W. F. Budd (2002). "Sensitivity of the Amery Ice Shelf, Antarctica, to changes in the climate of the Southern Ocean." Journal of Climate 15(19): 2740-2757.) to basal melting. The focus in the paragraph is on surface warming but simple scaling calculations show that where ice shelves are exposed to CDW the time frame for both basal melting and	Noted. This does not seem inconsistent with the text, since we say basal melting could eliminate the ice shelves within centuries. Accepted that it is not relevant only to WAIS.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				surface warming to be come significant issues is about the same. Warming trends have been observed (eg Weddell DW from the 1970s to 1990s of 0.012°C/yr (Robertson et al. 2002)) and projections estimated a CDW warming of around 2-2.4°C by the 2080s for a CO2 tripling experiment, similar forcing to the IS92a scenario (Hirst(1999). Deep ocean warming is expected to continue long after greenhouse gas concentrations stabilize hence for this thought experiment the trends implied in this scenario are continued beyond 2100. Grosfeld and Gerde (1998) have estimated the sensitivity of basal melting beneath the FRIS to warming and here we use the estimate without consideration of the HSSW shutdown, of 0.76 m/a/°C. With the temperature scenario above and assuming that; 1) the basal melting rate beneath the FRIS is of the order of 0.19m/a at present (Joughin and Padman 2003), 2) the ice shelf is in balance, and 3) there is no change in accumulation over the FRIS then 170-240 years are needed to reduce the average ice depth from 475 metres to 200 metres. [William Hare (Reviewer's comment ID #: 99-98)]	
10-1190	A	68:27	68:27	Grammar: replace with "We were not able …" [Anthony Hirst (Reviewer's comment ID #: 107-10)]	Accepted with thanks.
10-1191	A	68:34	68:34	"… about what dynamical changes could occur." Changes in what? [Anthony Hirst (Reviewer's comment ID #: 107-11)]	Accepted and clarified.
10-1192	A	68:48	68:49	See also work of Warner, R. C. and W. F. Budd (1998). "Modelling the long-term response of the Antarctic ice sheet to global warming." Annals of Glaciology 27: 161-168. which appears to have higher loss rates. [William Hare (Reviewer's comment ID #: 99-99)]	Rejected. This is an old reference.
10-1193	A	68:49	68:49	I am a bit baffled here. If the sea level was depressed by 120 m at LGM and it took, say 12000 years to melt back, then the average rate of rise is 10 mm per year, well above what is being said to be a maximum rate of one quarter this amount. If this maximum rate of rise is the contribution due to the melting of Antarctica, then this point needs to be made more clear. [Michael MacCracken (Reviewer's comment ID #: 152-295)]	Accepted and clarified; it was the future of the Antarctic ice sheet.
10-1194	A	69:0		BOX 10.1: Future Abrupt Climate Change, "Climate Surprises", and Irreversible Changes: COMMENT. The material in this box needs to be made consisent with any changes in the underlying chapter sections [William Hare (Reviewer's comment ID #: 99-100)]	ACCEPT: check for consistency
10-1195	A	69:5	72:8	Box 10-1 does a good job of describing "imaginable surprises" (sensu Schneider), but it never addresses the the central problem of ignorance and true surprise - the fact that a system as complex as the "earth system" (atmosphere/ocean/biosphere) may behave in ways that simply were not imagined prior to their occurrence.	Thanks

No.	Batch	Page:line From	To	Comment	Notes
				[Paul Baer (Reviewer's comment ID #: 10-6)]	
10-1196	A	69:5	72:8	This box is far too long and repeats far too much of what is already elsewhere in the chapter – some of it almost verbatim. [Martin Manning (Reviewer's comment ID #: 155-108)]	PARTIALLY ACCEPT: have shortened somewhat. However, Quantitative information across many climate system components require space
10-1197	A	69:5		Box 10.1. This Box provides a very useful compilation. Congratulations. [Govt. of Germany (Reviewer's comment ID #: 2011-214)]	Thanks
10-1198	A	69:8	69:8	Delete "the fact" [VINCENT GRAY (Reviewer's comment ID #: 88-1700)]	REJECT: no reason given for suggested change
10-1199	A	69:9	69:9	Replace "climate change" with 'changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1701)]	REJECT: no reason given for suggested change
10-1200	A	69:9	69:12	While very nice this definition doesn't include the 8.2kyr event which is (I think correctly) defined as an abrupt climate change in Chap. 6. It doesn't mean that ACC in the defined sense doesn't happen, but it should be clear that this is not an exclusive definition. [Gavin Schmidt (Reviewer's comment ID #: 227-17)]	ACCEPT: 8.2 kyr event now mentioned and ref. included
10-1201	A	69:10	69:10	Replace "climate change" with 'changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1702)]	REJECT: no reason given for suggested change
10-1202	A	69:12	69:12	Delete "system" [VINCENT GRAY (Reviewer's comment ID #: 88-1703)]	REJECT: no reason given for suggested change
10-1203	A	69:21	69:22	Add "for physical systems, not for biological systems" to the sentence so that the new text reads "Irreversibility (for physical systems, not for biological systems,) is only a theoretical concept and assumes …." [Govt. of Germany (Reviewer's comment ID #: 2011-215)]	
10-1204	A	69:29	69:29	Replace "climate change" with 'changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-1704)]	REJECT: no reason given for suggested change
10-1205	A	69:30	69:30	Delete "system" [VINCENT GRAY (Reviewer's comment ID #: 88-1705)]	REJECT: no reason given for suggested change
10-1206	A	69:36		In order to be honest, one should add the important caveat: "None of the AOGCM simulations shows an abrupt change when forced with the SRES emission scenarios and assuming there is no additional freshwater runoff from the Greenland ice sheet…" [Stefan Rahmstorf (Reviewer's comment ID #: 206-25)]	REJCET: estimated extra freshwater flux would be insignificant (Stouffer et al 2006)
10-1207	A	69:54		This statement is inconsistent with our chapter 6, which concludes that DO events are "associated with latitudinal shifts in oceanic convection between the Nordic Seas and the open mid-latitude Atlantic". Thus, change "rapid switch-ons of the MOC" to "rapid latitude shifts of the MOC", or something similar.	ACCEPT: reworded

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Stefan Rahmstorf (Reviewer's comment ID #: 206-26)]	
10-1208	A	70:37	70:38	Is the 60% volume loss by 2050 consistent with the more modest estimates of glacier melting in Section 10.6.3? [Govt. of Finland (Reviewer's comment ID #: 2009-142)]	quoted from Schneeberger. Number will be added in 10.6.3
10-1209	A	70:46	70:46	Insert after"years" "but growth in the centre" [VINCENT GRAY (Reviewer's comment ID #: 88-1706)]	REJECT: no reason given for suggested change
10-1210	A	70:48		change "reduce" to "decrease" [Danny Harvey (Reviewer's comment ID #: 101-78)]	ACCEPT!
10-1211	A	70:51	70:51	What does "abrupt" mean in this context: "The melting would not proceed abruptly but take many centuries to complete". The melting would occur slower than the forcing which caused it even if the forcing remains constant? It may be more accurate to describe this in this way "The melting could proceed rapidly with SLR contributions of around 0.4/century". [William Hare (Reviewer's comment ID #: 99-101)]	REJECT: correct as is
10-1212	A	71:7	71:7	It seems not just "possible" but "likely". [William Hare (Reviewer's comment ID #: 99-102)]	REJECT: assessment concludes possible
10-1213	A	71:7	71:7	Add after "concerns", "and are localized examples of abrupt changes". [William Hare (Reviewer's comment ID #: 99-103)]	REJECT: no need to be longer
10-1214	A	71:21	71:24	Absurd. The tropical rainforest survived the ice age because of microclimatic refugia. Why wouldn't the same process occur for an equivalent warming? [Patrick Michaels (Reviewer's comment ID #: 176-45)]	REJECT: assessment of current literature
10-1215	A	71:26	71:39	refer to chapter 7, (section 7.3.3.1.1) for more details on soil respiration sensitivity to temperature [Chris Jones (Reviewer's comment ID #: 120-35)]	ACCEPT: ref added
10-1216	A	71:28	71:30	Mention that this is also the case when the experiments are run with a more complex soil carbon model (RothC - Jones et al., 2005, GBC), so this results is largely due to modelled climate and vegetation response rather than soil itself [Chris Jones (Reviewer's comment ID #: 120-34)]	REJECT: could not locate reference
10-1217	A	71:35	71:37	Add, "But, observations over the boreal forest indicate that (Chen et al., 2006), in warm years, photosynthesis increases more than respiration". [Patrick Michaels (Reviewer's comment ID #: 176-46)]	REJECT: here we discuss projections not present
10-1218	A	72:6	72:8	Interdecadal variations in NAO and other circulation modes occur simply as a result of internal variability. It is not correct to say that they are caused by global warming. [Govt. of Finland (Reviewer's comment ID #: 2009-143)]	PARTLY ACCEPT: clearer wording
10-1219	A	72:10	73:53	Delete this box. It is redundant with, and depends heavily on, material presented in Chapter 9.	Rejected. The box gives gives an overall assessment from material on

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Lenny Bernstein (Reviewer's comment ID #: 20-71)]	climate sensitivity discussed in chapters 8, 9 and 10.
10-1220	A	72:10	73:53	This material has already been presented in Chapter 9. It is redundant. [Jeff Kueter (Reviewer's comment ID #: 137-65)]	Rejected. The box gives gives an overall assessment from material on climate sensitivity discussed in chapters 8, 9 and 10.
10-1221	A	72:10	73:57	This box is too long. [Martin Manning (Reviewer's comment ID #: 155-109)]	Rejected. The summary statement on climate sensitivity will draw attention and needs careful explanation of the underlying arguments.
10-1222	A	72:10		Box 10.2. Unfortunately, there are still many problems with your discussion of climate sensitivity. Of course, this is substantially due to problems in the underlying literature, but nevertheless it will be a shame if you cannot provide a better assessment of the state of play. There are some serious problems in the way that the so-called "observationally constrained estimates" of climate sensitivity have been presented and interpreted in the literature. It seems likely that those on the outside of this field will have been misled into assigning more credibility to this work than it deserves. It is clear that the standard ansatz for generating the so-called "observationally constrained estimates of climate sensitivity" is fundamentally flawed in a way as to automatically assign unrealistically high probability to extremely high sensitivities. There really is nol support for such a belief to be rationally held. The use of a highly restricted subset of available data must result in exaggerated uncertainties, which in the current context implies an unrealistic probability assigned to high sensitivities. Obviously our recent GRL paper (Annan and Hargreaves, GRL 2006) addresses this point in a rather simplistic manner - the manuscript was written in little more than a weekend after our shock at reading the first draft of the AR4 - but the point still appears incontestable. The vague waffle about how the assumptions of different studies may not be compatible hardly justifies completely discarding huge reams of data. Justifying this would require the argument (which is nowhere made in the literature, let alone credibly so) that the evidence which was not considered in the particular study is quite literally worthless. The Forster and Gregory paper (which we were unaware of at the time of writing Annan and Hargreaves 2006) is in our view particularly valuable as it does not rely on any numerical modelling at all, and so strongly enhances the growing mountain of evidence in favour of a mid-range climate sensitivity. [James Annan (Reviewer's comment ID #: 6-22)]	Noted. The reviewer criticizes the underlying literature rather than the chapter text. IPCC is supposed to report the consensus of the literature, not to do new research or correct flaws in the literature. The assessment given is the result of long discussions and careful evaluation of all lines of evidence, and represents the opinion of many authors from chapters 6,8,9 and 10. The papers using multiple constraints are discussed, but there was agreement that the argument is rather conceptual at this stage (a weekend of work, as the reviewer points out), and needs further investigation, e.g. evaluation of a model against multiple constraints rather than trivially multiplying published PDFs. The authors do not feel comfortable with giving a 95% upper bound or a master PDF for sensitivity. As the reviewer has show (Annan and Hargreaves, 2006) the numbers depend too much on which studies are considered. Combining PDFs also assumes that each of the underlying

No.	Batch	Page:line From	To	Comment	Notes
					studies is unbiased and independent.
10-1223	A	72:10		To recap, the standard approach has been to take some data set, and a so-called "ignorant" uniform prior in sensitivity S, and combine them using Bayes' Theorem to generate a supposed pdf. However, it is hard to see how such a result could credibly represent the beliefs of an informed scientist. The two basic problems are in the choice of prior, and the limited use of data. On the first point, it is not at all reasonable to claim that a uniform prior actually represents "ignorance". It is incontestable that a uniform prior for S of the form U[0C,20C] as recommended by Frame et al (GRL 2005) actually represents a prior assertion that S>6C at the 70% ("likely") level. Even the [0C,10C] of some other authors puts P(S>6C) as four times as likely, a priori, as P(2.5C<S<3.5C). The limited justification of various authors on this subject seems to be little more than wordplay that cannot deny this trivial mathematical point. Choosing the prior is not straightforward, but this does not excuse completely unreasonable ones! We note that Chris Forest has already presented results using a more credible "expert prior", in 2002 and 2006. Unfortunately they were not strongly highlighted, in favour of the more exciting results when uniform prior was used. We recommend that they are included in some manner in Fig 1 of Box 10.2, perhaps with our results(Annan and Hargreaves 2006 GRL) and/or those of Hegerl et al (Nature 2006). Even though none of these are beyond criticism, they do appear to be the only results that approach a credibly defensible pdf for climate sensitivity, in our opinion. Actually, the results of Forster and Gregory - even though they used a rather unconventional prior - also seem quite reasonable, although perhaps this is more by luck than design. It must be noted that a prior that is non-uniform in S (such as one which is uniform in L=1/S) in no way "rules out high sensitivity a priori", to use the popular phrasing. If there was actually any evidence of high sensitivity, then the posterior would show it even with a prior that is uniform in L! In respect of the use of data, in addition to our recent GRL paper (Annan and Hargreaves, GRL 2006) it might be worth pointing out some clear misunderstandings that have commonly occurred in the literature. It is absolutely not necessary for any specific data set to itself provide any sort of strong "constraint" (by which people seem to mean a firm limit when combined with a uniform prior) in order for it to strengthen and enhance an existing estimate when combined via Bayes' Theorem. For instance, the discussion of whether any one of the different "constraints" considered in Chapter 29, "Avoiding Dangerous Climate Change" can be used to "rule out" high sensitivity completely misses the point that they all point fairly clearly towards a mid-range value, and thus jointly they "rule out" high sensitivity with much higher probability than any one could do in isolation. You also clearly repeat this error in this box when you talk about LGM data "providing support for" the range from other sources bu not constraiing it further. You	Noted. See response to comment 10-1222. It is somewhat meaningless to argue about priors in Bayesian studies. As the name says, it's a prior assumption, and anyone may agree or disagree with it. The expert prior used in Forest is questionable, since the experts were probably aware of the sensitvitiy range approximately consistent with the observed warming, thus the same data is used twice. Again IPCC is not supposed to 'point out misunderstandings in the literature'. The Wigley and Forster/Gregory work ist assessed in chapter 9. The final assessment indicates that there are many lines of evidence pointing to a mid-range sensitivity. For the first time, a best guess value of about 3°C is given.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				could usefully discuss how the workof Wigley et al (volcanic eruptions) and Forster and Gregory (ERBE data) clearly strengthen the evidence for a mid-range climat sensitivity. [James Annan (Reviewer's comment ID #: 6-23)]	
10-1224	A	72:10		It should be recognised somewhere in the document that the observationally-based analyses actually significantly strengthen our justified belief in a mid-range sensitivity in the region of 3C. The primary explanation for the "alarm" over the probability of high climate sensitivity is actually generated by a change in the prior belief. Where scientists used to be comfortable in expressing a subjective belief that S was close to 3C, now they seem to have decided that the "objective" choice is to assert a strong prior belief in very high sensitivity, and then a limited use of data may fail to overturn this prior assumption. [James Annan (Reviewer's comment ID #: 6-24)]	See replies to comments 10-1222 and 10-1223. The text does mention that 'several independent lines of evidence indiceate similar most likely values and ranges'.
10-1225	A	72:10		Box 10.2. This Box provides a very useful synthesis on the climate sensitivity studies. Congratulations. [Govt. of Germany (Reviewer's comment ID #: 2011-216)]	Noted. Thank you. No changes requested.
10-1226	A	72:10		This text box is very important and will stand out. It needs to be very carefully worded and consolidated with Chapter 9. Fro one, the term "climate sensitivity" has at least two meanings (equilibrium or transient 1%/yr) and is confused with confused with the climate sensitivity parameter. So please expand the title to be precise and unambiguous, perhaps add 'uncertainty' or 'prob distrib' to the box title. To justify its placement here, it needs to have a focus on providing a context for uncertainty estimates in the projections. [Govt. of United States of America (Reviewer's comment ID #: 2023-672)]	Statements are consistent with chapter 9. Equilibrium climate sensitivity is clearly defined in the glossary and refers to the equilibrium response, not to a transient response. Added 'equilibrium' to the title for clarification.
10-1227	A	72:12	72:13	This "1 in 3" is clearly not correct. According to the precise wording, the probability could be anywhere between 1 in 10 and 1 in 3, and Wigley and Raper (who contributed the relevant chapter in the TAR, one as a lead author!) used the 1 in 10 figure in their own work. I understand the SAR also gave a "most likely" value of 2.5C, so reverting to this format (albeit with an increased value) in the AR4 is hardly new. It is not made clear on what evidence you have decided to increase the "most likely" value, compared to the SAR - it would be appropriate to explain in more detail. [James Annan (Reviewer's comment ID #: 6-25)]	Reworded to 'likely range 1.5-4.5' to be consistent with TAR Tech. Summary F.3
10-1228	A	72:12	72:13	In the various references to the uncertainty in the climate sensitivity in the TAR, only in the WGI Technical summary is the term "likely" used to describe the probability that the true CS is between 1.5 and 4.5°. Furthermore, based on the history of the usage of the range, it is unlikely that the authors of the TAR deliberately used the term "likely" to imply the calibrated quantified range used througout WGI. Finally, even if they did mean to use "likely" in that fashion, it is supposed represent probabilities between 66% and 90%, thus at best the phrase as written can only be interpreted to mean "between a 1 in 10	Reworded to 'likely range 1.5-4.5' to be consistent with TAR Tech. Summary F.3

No.	Batch	Page:line From	Page:line To	Comment	Notes
				and 1 in 3 probability of values outside that range." I would suggest dropping the quantified likelihood. [Paul Baer (Reviewer's comment ID #: 10-1)]	
10-1229	A	72:13	72:15	Why is the range "nearly the same"? As far as I can tell its exactly the same. [Paul Baer (Reviewer's comment ID #: 10-2)]	Accepted.
10-1230	A	72:13		There is no statement in Chapter 9 of the TAR matching your "1 in 3 probability of values outside that range". There are statements about the standard deviations of sets of model results, but as you will realize that does not provide a probability for the full assessed range of climate sensitivity values. Many people who were associated with the TAR have said that the IPCC has not given a probabilistic range for climate sensitivity in the past. That statement is even given again in this report – see Chapter 1, page 24, line 56. So please remove this reference to a probabilistic assessment for the TAR range. [Martin Manning (Reviewer's comment ID #: 155-110)]	TAR Tech. Summary F.3: 'Climate sensitivity is likely in the range of 1.5 – 4.5°C'. Text reworded to be consistent with that. If not a probabilistic estimate, this is certainly a confidence level. Chapter 1 is wrong in that case (e.g. SOD Chapter 1, page 26, line 1)
10-1231	A	72:16	72:18	It should be stated whether the mean differences in a climate sensitivity between the TAR under present results (a drop from 3.5 to 3.2C) are statistically significant. This point should be addressed explicitly by the authors as should the large range that is provided in this assessment. [Govt. of Australia (Reviewer's comment ID #: 2001-438)]	Rejected. The AOGCMs are an ensemble of opportunity and are not sampled in any random or systematic way. A significance test is impossible. The range given is discussed in detail in chapter 9 and 10.
10-1232	A	72:16	72:18	It should be stated whether the mean differences in climate sensitivity between the TAR and the present results (a drop from 3.5 to 3.2C) are statistically significant. This point should be addressed explicitly by the authors as should the large range that is provided in this assessment. [Govt. of Australia (Reviewer's comment ID #: 2001-441)]	Rejected. The AOGCMs are an ensemble of opportunity and are not sampled in any random or systematic way. A significance test is impossible. The range given is discussed in detail in chapter 9 and 10.
10-1233	A	72:34		add Harvey and Kaufmann (2002), as we provided one of the most physically thorough analyses of this question ever published. The reference is: Harvey, L.D.D. and R. Kaufmann. "Simultaneously constraining climate sensitivity and aerosol radiative forcing", Journal of Climate 15, 2837-2861, 2002 [Danny Harvey (Reviewer's comment ID #: 101-79)]	Reference added. Study provides no PDF to include in Figure.
10-1234	A	72:37	72:38	Please rephrase this sentence to avoid using the language "very unlikely" because that carries a defined probabilistic interpretation – viz that there was a 90% probability of climate sensitivity being > 1.5C – and as we know there has been no such probabilistic statement in the past, so you can not say that new results confirm that. [Martin Manning (Reviewer's comment ID #: 155-111)]	Accepted. Reworded.
10-1235	A	72:49	72:55	The disagreement between Annan et al. and Schneider von Deimling et al. is not only due use of different models but also due to more realistic boundary conditions in the study by	Partly accepted. Revised text mentions differences in forcings. The authors are

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Schneider von Deimling et al.: in their study, LGM-specific dust & vegetation forcing is accounted for what is not the case in Annan et al. (as it is not in the PMIP-2 protocol). Schneider von Deimling et al. also estimated the magnitude of this effect in their publication: proper accounting for both boundary conditions shifts inferred climate sensitivity down by a value up to 1.4°C. Hence, the values derived by Annan et al. would have to be lowered by up to 1.4°C. Keeping this in mind, the discrepancy between the two studies does not look that large any more (compare also Fig 9.6.1, p.9-119). Hence I strongly disagree that their method would not constrain CS further: Quite the contrary, they show that 3 independent sources (Annan et al., Schneider von Deimling et al.-tropics, Schneider von Deimling et al., Antarctica) are compatible with CS=1.5...4.5°C and strongly disagree with values above 5°C. [Hermann Held (Reviewer's comment ID #: 104-15)]	not in the position to 'correct' problems in published studies. The Schneider v.D. study likely underestimates the uncertainties due to overly tight relationships between observables and sensitivity, and a single paper with a single model provides insufficient evidence for a stronger statment in IPCC.
10-1236	A	72:53		Here the impression is given that the results of Annan et al. and Schneider von Deimling et al. contradict each other, which is the reason for the rather cautious phrasing. However, it should be noted that (1) Annan et al. used an atmosphere-only model, which thus ignores changes in ocean heat transport which were shown to be important for glacial climate by Ganopolski et al. 1998; (2) none of the models in the Annan ensemble had a climate sensitivity below 4 degrees, so by definition this study cannot possibly find a sensitivity below 4, and (3) only a subset of glacial forcings was used, ignoring dust and vegetation. Both the neglected processes (ocean heat transport change, missing forcings) are first-order processes which would have lead to a much lower climate sensitivity estimate for the same data constraints. The role of IPCC is to make an assessment, not a comprehensive review of every single publication, so I would not cite the Annan et al paper in the report, as no robust conclusions can be drawn from it. At the very least the above caveats should be stated clearly. [Stefan Rahmstorf (Reviewer's comment ID #: 206-27)]	Partly accepted. Revised text mentions differences in forcings. The authors are not in the position to 'correct' problems in published studies. The text says that the results differ substantially, which is correct. It does not say that they contradict each other.Figure captions states clearly that the Annan paper provides only an upper but no lower bound.
10-1237	A	72:54	72:54	this concept of "providing support" to a range is essentially alien to the Bayesian estimation procedure as I understand it. Can you explain what this means? If a value of around 3C is found tohave a higher likelihood than 6C or 10C, then this can (and must) be accounted for in the overall estimate, eg by using a non-uniform prior (as Forest has done a couple of times), or simply by combining the likelihoods as we did. [James Annan (Reviewer's comment ID #: 6-26)]	Noted. The idea of multiple constraints is discussed in the box. But the publised work is rather conceptual and assumes independence. There is no consensus in the community on how to combine different lines of evidence, and the authors cannot go beyond what is published and widely accepted in the community.
10-1238	A	72:54	72:55	I have to disagree strongly with this line. In Chap 6, there are estimates for the forcings at the LGM, and for best estimates of the global temperature changes. These can be clearly	Partly accepted. What the statement was supposed to say is that the LGM

No.	Batch	Page:line From	Page:line To	Comment	Notes
				combined to demonstrate that no amount of reasonable uncertainty allows the high end sensitivities to be consistent (you'd need to increase the global T change estimates by a factor of 2 or 3 or the forcings need to be reduced by similar factors – just not possible!). This is an old argument (Lorius et al, 1990 for instance) and does provide a constraint that is better than the 20th Century data. Annan and Hargreaves (2006) use this in a Bayesian analysis to show that it all but eliminates the possibility of sensitivty > 5 deg C (in combination with other constraints). [Gavin Schmidt (Reviewer's comment ID #: 227-18)]	data does not constrain sensitivity better than the 'likely 2-4.5'. Reworded and ref. added to chapter 9 where this is discussed in detail. Note that the argument based on global forcing and global cooling oversimplifies the problem, and simple models show too tight relationships between e.g. LGM tropical SST and sensitivity.
10-1239	A	73:11	73:11	More explanation is needed on the point that climate sensitivity is "very unlikely below 2C". This is apparently a primary basis for a lead conclusion of the AR4, the revision of climate sensitivity estimate upward from that in the TAR and previous assessments. Furthermore, 2 of the 7 model results that were the basis for the TAR estimates of future warming had a climate sensitivity below 2 (see TAR table 9.A1). Suggest explanation and revision of conclusion as appropriate. [Haroon Kheshgi (Reviewer's comment ID #: 125-44)]	Rejected. The final statement is 'very likely above 1.5°C', the 2°C only quotes the papers using climatology. The box only summarizes the material. Support for the numbers is provided in 10.5.4., 9.6 and 8.6. The latter discusses progress in GCM feedbacks since the TAR.
10-1240	A	73:15	73:16	Why show the unweighted Murphy results? The "weighted" results would seem more appropriate since they take account of errors in the control climate in weighting sensitivity. If the authors have a legitimate reason for including the unweighted Murphy results and taking this approach, this should be explained in the text. [Govt. of Australia (Reviewer's comment ID #: 2001-439)]	Rejected. Weighted Murphy results are given in panel c/d. Unweighted distributions are for comparison only and not used in the overall assessment.
10-1241	A	73:15	73:16	Why show the unweighted Murphy results? The "weighted" results would seem more appropriate since they take account of errors in the control climate in weighting sensitivity. If the authors have a legitimate reason for including the unweighted Murphy results and taking this approach, this should be explained in the text. [Govt. of Australia (Reviewer's comment ID #: 2001-442)]	Rejected. Weighted Murphy results are given in panel c/d. Unweighted distributions are for comparison only and not used in the overall assessment.
10-1242	A	73:16	73:20	In the Stainforth study, the distribution of a perturbation study depends on the unperturbed case as stated. And it depends on any idiosyncrasies of the model used. What were the physics that allowed the model to give unstable climates, but not climate sensitivity below 2C and was this tied to the lack of ocean circulation change in the model? Why is the distribution for the Stainforth study referred to when that paper itself said that this was not appropriate? Suggest stating the robust finding of this study, and not those that are not even claimed by the authors. For example, that paper showed that a very wide range of climate sensitivity could be generated with a plausible range of parameters. Suggest either deleting this section or revising. [Haroon Kheshgi (Reviewer's comment ID #: 125-45)]	Partly accepted. Figure caption revised to state that panels e/f should not be interpreted as distributions. Apart from that, the paragraph states the findings of that paper, and several coauthors of that paper helped writing the paragraph.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-1243	A	73:19	73:23	This passage is important but is hard to follow. For example, what are the "well known physical reasons". [Anthony Hirst (Reviewer's comment ID #: 107-12)]	Noted, no change.Explanation is provided a few paragraphs earlier ('This inability to rule out very high values is common to many methods, since for well understood physical reasons, the rate of change (against sensitivity) of most quantities that we can observe tends to zero as the sensitivity increases (Hansen et al., 1985; Knutti et al., 2005; Allen et al., 2006).')
10-1244	A	73:25	73:25	This invites the facetious response that Bayes Theorem does indeed provide such a well-established formal method. Applying it in detail, to a standard that will satisfy all, may require some work, but to pretend that the bulk of the existing literature (which simply ducks the question, in some cases apparently through completely misunderstanding the issues) is credible as an alternative is nonsense. [James Annan (Reviewer's comment ID #: 6-27)]	Noted. The reviewer criticizes the literature rather than the chapter text, and again provides no useful suggestions for improvement.
10-1245	A	73:35		Page 43, line 38 gives the upper bound of this range as 4.4 C not 4.5 C. You need to be consistent. [Martin Manning (Reviewer's comment ID #: 155-112)]	Fixed to be consistent.
10-1246	A	73:36	73:40	Repeats what is already in the chapter text. [Martin Manning (Reviewer's comment ID #: 155-113)]	Noted. Everything in the box is somewhere in a chapter already. The idea of the box is to summarize the evidence to justify the overall assessment. No change on text.
10-1247	A	73:44	73:44	The comment should be made that although the uncertainty due to clouds is the largest factor, that the spread in other feedbacks also make contributions to the spread in climate sensitivity. [Govt. of Australia (Reviewer's comment ID #: 2001-440)]	Accepted.
10-1248	A	73:44	73:44	The comment should be made that although the uncertainty due to clouds is the largest factor, that the spread in other feedbacks also make contributions to the spread in climate sensitivity. [Govt. of Australia (Reviewer's comment ID #: 2001-443)]	Accepted.
10-1249	A	73:44	73:44	"Section 8.6)" should be "Section 8.6).". [Chiu-Ying LAM (Reviewer's comment ID #: 139-19)]	Accepted.
10-1250	A	73:53	73:55	Please clarify "substantially higher than 4.5C" and "cannot be excluded", the latter with	Explanation is provided a few

No.	Batch	Page:line From	Page:line To	Comment	Notes
				reference to the standard terms for the probability of assessed outcomes defined for this purpose. Eg, do you judge it to be exceptionally unlikely that S>6C, or very unlikely, or merely unlkely? I don't think you can reasonably duck this decision. "For fundamental physical reasons, as well as data limitations". I think I know what you are trying to say, but this camel of a statement is horribly misleading. In fact it could only be true if S really was substantially higher than 4.5C! If S really is close to 3, then of course more (paleo reanalyses, as well as future) data is bound to improve our confidence in this value, and "fundamental physical reasons" can hardly prevent this. Furthermore, your (correct) comment that the observational agreement is generally worse for high values is a severe threat to your conclusion that such values cannot be reasonably be ruled out. [James Annan (Reviewer's comment ID #: 6-28)]	paragraphs earlier ('This inability to rule out very high values is common to many methods, since for well understood physical reasons, the rate of change (against sensitivity) of most quantities that we can observe tends to zero as the sensitivity increases (Hansen et al., 1985; Knutti et al., 2005; Allen et al., 2006).'). There was agreement among a large group of authors from chapters 6,8,9 and 10 that there is no consensus on a 'very likely' upper bound on sensitivity from the literature, or on the likelihood for e.g. S>6°C. IPCC cannot provide statements where there is no consensus in the published literature.
10-1251	A	75:47		add ref: Bretherton, C. S., M. E. Peters, and L. Back, 2004: Relationships between water vapor path and precipitation over the tropical oceans. J. Climate, 17, 1517-1528. [J. David Neelin (Reviewer's comment ID #: 187-32)]	Rejected—this paper has nothing to do with climate change projections, the subject of Ch. 10
10-1252	A	76:13		add refs: " Chou, C. and J. D. Neelin, 2004: Mechanisms of global warming impacts on regional tropical precipitation. J. Climate, 17, 2688-2701." , and "Chou, C., J. D. Neelin, J.-Y. Tu, and C.-T. Chen, 2006: Regional tropical precipitation change mechanisms in ECHAM4/OPYC3 under global warming. J. Climate, in press." (accepted 12/05) (already in the IPCC archive under chpt 11) [J. David Neelin (Reviewer's comment ID #: 187-31)]	Rejected--Chou and Neelin (2004) and Chou et al. (2006) describe a mechanism to account for possible regional and sub-regional details of precipitation change, and thus pertains to Ch. 11.
10-1253	A	86:3		add ref: J. D. Neelin, M. Munnich, H. Su, J. E. Meyerson and C. Holloway, 2006: Tropical drying trends in global warming models and observations. Proc. Nat. Acad. Sci., 103, 6110--6115. [J. David Neelin (Reviewer's comment ID #: 187-26)]	Accepted—though this paper is more appropriate for Ch. 11, we now refer to it in our discussion of general precipitation trends
10-1254	A	86:3		add ref: Neelin, J. D., C. Chou, and H. Su, 2003: Tropical drought regions in global warming and El Ni\~no teleconnections. Geophys. Res. Lett., 30(24), 2275, doi:10.1029/2003GL018625. [J. David Neelin (Reviewer's comment ID #: 187-33)]	Rejected—this paper describes a mechanism to account for possible regional and sub-regional details of precipitation change, and thus pertains

No.	Batch	Page:line From	Page:line To	Comment	Notes
					to Ch. 11.
10-1255	A	86:26	86:26	This paper has been published in "J. Met. Soc. Japan, 84, 259-276". [Masato Sugi (Reviewer's comment ID #: 259-17)]	Accepted.
10-1256	A	88:32	88:32	The final title of Rougier (2006) is "Probabilistic inference for future climate using an ensemble of climate model evaluations". [Jonathan Rougier (Reviewer's comment ID #: 221-5)]	Accepted.
10-1257	A	89:7	89:7	Write Hock (not Hoeck) [Wilfried Haeberli (Reviewer's comment ID #: 94-17)]	Accepted.
10-1258	A	91:17	91:18	Tsushima, Y., et al., 2006: Importance of the mixed-phase cloud distribution in the control climate for assessing the response of clouds to carbon dioxide increase - a multi-model study. Clim. Dyn., in press. (The title has been correct here) [Seita Emori (Reviewer's comment ID #: 62-31)]	Accepted. Suggested title is the same as in the SOD draft. See comment 1259
10-1259	A	91:17	91:18	An update on the reference: Tsushima, Y., S. Emori, T. Ogura, M. Kimoto, M. J. Webb, K. D. Williams, M. A. Ringer, B. J. Soden, B. Li, and N. Andronova , 2006: A multi-model analysis of the distribution of mixed-phase clouds in AGCMs, for control and doubled carbon dioxide simulations. Clim. Dyn., doi:10.1007/s00382-006-0127-7. [Masahide Kimoto (Reviewer's comment ID #: 127-6)]	Accepted.
10-1260	A	92:3	92:3	Replace "pp. 300" with "pp. 240". [Xiaolan L. WANG (Reviewer's comment ID #: 282-23)]	Accepted.
10-1261	A	93:37	93:37	Publication year "2006" should be corrected as "2005". [Masato Sugi (Reviewer's comment ID #: 259-18)]	Accepted.
10-1262	A	93:40	93:40	This paper has been published in "J. Met. Soc. Japan, 84, 405-428". [Masato Sugi (Reviewer's comment ID #: 259-19)]	Accepted.
10-1263	A	95:1	95:47	FAQ 10.1: It would be helpful to include a figure for FAQ 10.1. Two suggestions for such are simplified versions of Figures 10.3.16 or 10.3.17. [Melinda Marquis (Reviewer's comment ID #: 162-82)]	We appreciate this comment, but it is unclear whether a figure would clarify anything in this answer given the very short nature of the text and length limitations
10-1264	A	95:4		A longer short answer would be more appropriate here as in other questions. Also the notion of 'change' in this question is not clear. Suggest clarifying whether changes in type, frequency, or intensity are being considered in short and long answer sections. Also, the order of heat waves, droughts, or floods is best kept in the text of the answer. Otherwise, change the order in the question. [David Wratt & David Fahey (Reviewer's comment ID #: 67-71)]	It is unusual for a reviewer to suggest lengthening the text (!), and we prefer a shorter to a longer answer. We have made suggested clarification, and now we have changed the order of the discussion so that the order is

No.	Batch	Page:line From	Page:line To	Comment	Notes
					comparable to that in the question.
10-1265	A	95:7	95:7	Insert after "futere" "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-1707)]	Rejected—reviewer gives no reason for suggested change
10-1266	A	95:7		Suggest for clarity changing to '..simulate increased summer dryness..' [David Wratt & David Fahey (Reviewer's comment ID #: 67-72)]	Accepted
10-1267	A	95:9	95:12	The underlying physical explanation (more water vapour in a warmer atmosphere) should be mentioned. [Govt. of Finland (Reviewer's comment ID #: 2009-144)]	Accepted
10-1268	A	95:10		Suggests for clarity changing to '…this might occur because precipitation will be concentrated…' [David Wratt & David Fahey (Reviewer's comment ID #: 67-73)]	Accepted
10-1269	A	95:12		Suggest improving structure as "Another aspect ….. Is related to the mean changes of precipitation, namely, wet extremes are projected to become…..increase, and dry extremes…..' [David Wratt & David Fahey (Reviewer's comment ID #: 67-74)]	Accepted
10-1270	A	95:16	95:19	Suggest dividing sentence for ease of reading. '…extra-tropical storms. Specifically over northern hemisphere land, an increase is expected in the likelihood…' [David Wratt & David Fahey (Reviewer's comment ID #: 67-75)]	Accepted
10-1271	A	95:16	95:20	In addition to our suggestion of breaking this long sentence up into two for ease of reading, we have some comments about terms. The sentence says that even if the storms in a future climate do not change much in intensity, there would be an increase in extreme rainfall intensity within extra-tropical storms ... But to the non-specialist, a storm which has heavier rainfall in it is by definition a more intense storm. We assume that when the question authors talk about storm intensity they are referring to the pressure gradient across the storm. We suggest the wording be changed to refer specifically to rainfall intensity, wind strength and pressure , rather than using the term "intensity" to refer to some but not all of these features. Also the term "extra-tropical storm" may be confusing to general readers. We suggest replacing it by something like "storms outside of tropical latitudes". [David Wratt & David Fahey (Reviewer's comment ID #: 67-118)]	Accepted
10-1272	A	95:30	95:33	Diurnal temperature range, number of frost days and growing season length fit poorly under the title "extreme events". Consider deleting. [Govt. of Finland (Reviewer's comment ID #: 2009-145)]	Rejected. Though it has been pointed out before (and here again) that such events are not "extreme events", the literature treats them as such and we are assessing that literature.
10-1273	A	95:33	95:37	Suggest dividing sentence for ease of reading. '…winter in most area. Exceptions will	Accepted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				occur in areas…' [David Wratt & David Fahey (Reviewer's comment ID #: 67-76)]	
10-1274	A	95:34	95:37	I think there is too much geographical detail in this sentence. Consider replacing by something like "Additionally, there is likely to be a large decline in frequency of cold air outbreaks ( ) in most of the Northern Hemisphere, although changes in atmospheric circulation may reduce this change in some areas." [Govt. of Finland (Reviewer's comment ID #: 2009-146)]	Rejected. The literature specifically mentions these areas and that should be reflected in the text.
10-1275	A	95:37		Probably should use LLGHGs rather than GHGs since this was the notation used earlier for long lived greenhouse gases [Wilmer Anderson (Reviewer's comment ID #: 5-52)]	Sentence has been re-worded.
10-1276	A	95:45	95:46	Suggest replace "extratropical storms" by "storms outside tropical latitudes". Suggest replace "higher waves" with "higher ocean waves". [David Wratt & David Fahey (Reviewer's comment ID #: 67-119)]	Accepted
10-1277	A	96:1	97:36	FAQ 10.2: It would be helpful to include a figure for FAQ 10.2. One suggestion is to show a graphic of the MOC and the likely effects of a reduced N. Atlantic MOC. [Melinda Marquis (Reviewer's comment ID #: 162-83)]	NOTED: this possibility will be checked. But FAQ becomes longer.
10-1278	A	96:4		Suggest answering the question more directly as 'Abrupt climate changes, such as the loss of the Greenland…are not likely in the 21st century, based on currently available results from a hierarchy of models. However, the occurrence of abrupt changes becomes more likely as the perturbation of the climate system progresses.' [David Wratt & David Fahey (Reviewer's comment ID #: 67-77)]	ACCEPT: reworded
10-1279	A	96:14		Suggest more common usage as 'phases of abrupt changes' [David Wratt & David Fahey (Reviewer's comment ID #: 67-78)]	REJECT: too technical
10-1280	A	96:24		It would help if a short explaination of what the North Atlantic deep water formation is since the questions will be published separately for non-experts. [Wilmer Anderson (Reviewer's comment ID #: 5-53)]	PARTLY ACCEPT: wording expanded
10-1281	A	96:36		"In the public, the most widely discussed abrupt change is the collapse or shut-down of the Gulf Stream". This statement may be true for the European public, but is it true of the Chinese, Indian or Australian public? [Adrian Simmons (Reviewer's comment ID #: 242-160)]	ACCEPT: wording changed
10-1282	A	96:41	96:41	Sentence should read: Nevertheless, its northern … The style manual by Strunk and White says you should not start a sentence with the word however, but you can use nevertheless to start a sentence. [Wilmer Anderson (Reviewer's comment ID #: 5-54)]	REJECT: there is a subtle difference between "nevertheless" and "however". Prefer "however" here
10-1283	A	97:8	97:8	Sentence should read: Nevertheless, even if such a … The style manual by Strunk and White says you should not start a sentence with the word however, but you can use	ACCEPT: changed

No.	Batch	Page:line From	Page:line To	Comment	Notes
				nevertheless to start a sentence. [Wilmer Anderson (Reviewer's comment ID #: 5-55)]	
10-1284	A	98:4	98:6	FAQ 10.3: The italicized summary is a bit confusing. Perhaps beginning the statement with, "Concentrations depend …" instead of "This depends …" would help. The second sentence says that concentrations could *change* almost immediately … or they could continue to *increase* for centuries. Does the word "change" encompass both a possible increase and a possible decrease in response to a GHG reduction? Shouldn't the word "change" be replaced with the word "decrease"? (The question asks how quickly concentrations will *decrease* if GHG emissions are reduced.) Perhaps the sentence should read something like: "Concentrations could decrease immediately for a short time after a reduction in emissions, but then concentrations could begin to increase for centuries." [Melinda Marquis (Reviewer's comment ID #: 162-84)]	ACCEPT: sentence changed
10-1285	A	98:4		Suggest a more direct and complete short answer here: 'When emissions of a specific greenhouse gas are reduced, its atmospheric abundance will increase more slowly or may begin to decrease. The response in each case depends crucially on the balance of emission rates and atmospheric removal rates, which in turn depend on a wide variety of chemical and physical processes in the atmosphere. [David Wratt & David Fahey (Reviewer's comment ID #: 67-79)]	ACCEPT: sentence changed
10-1286	A	98:5	98:6	If emissions of GHG are reduced . . . Concentrations … could actually continue to increase for centuries? Somehow, this sentence just doesn't seem to be logical. [Andrew Lacis (Reviewer's comment ID #: 138-17)]	REJECT: emissions vs concentration!
10-1287	A	98:8		Saying that the concentration of a gas depends 'primarily' depend on emissions is not correct. Suggest changing to 'The concentration of a greenhouse gas in the atmosphere depends on the balance between the rate of emissions of the gas and the physical, chemical, and biogeochemical processes that remove it from the atmosphere. [David Wratt & David Fahey (Reviewer's comment ID #: 67-80)]	ACCEPT: wording changed
10-1288	A	98:13		Suggest clarifying by changing to: '…within well less than a century..' [David Wratt & David Fahey (Reviewer's comment ID #: 67-88)]	REJECT: CFCs and other GHGs have a wide range of lifetimes
10-1289	A	98:14		Suggest for clarity changing to: 'A measure of the timescale is called the lifetime of a gas….' [David Wratt & David Fahey (Reviewer's comment ID #: 67-81)]	REJECT: ok as is
10-1290	A	98:15		Because 'perturbation' is not defined or motivated here, suggest changing to '…time it takes for a gas concentration to decrease to 37% of its current value after its emissions are stopped.' [David Wratt & David Fahey (Reviewer's comment ID #: 67-82)]	REJECT: present def is correct, suggested change is not.
10-1291	A	98:18		Suggest for clarity changing to '…defined uniquely.'	REJECT: There is only one lifetime

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[David Wratt & David Fahey (Reviewer's comment ID #: 67-83)]	and that cannot be defined.
10-1292	A	98:18		Noting the long-lived fraction of CO2 is only half the story. It is important to juxtapose the fact that there is a rapid removal fraction. Suggest changing to 'While more than half of emitted CO2 is currently removed from the atmosphere after a century, some fraction (about 20%) of emitted CO2 will remain in the atmosphere....' [David Wratt & David Fahey (Reviewer's comment ID #: 67-84)]	ACCEPT: wording changed
10-1293	A	98:29	98:57	Need to mention which model was used to obtain the CO2 concentration curves illustrated in Question 10.3, Figure 1. Question 10.3 is important, and better referencing overall would help make the discussion more convincing. [Anthony Hirst (Reviewer's comment ID #: 107-13)]	ACCEPT: puls response added
10-1294	A	98:29		The illustration of these test case is problematic for the unnamed gases. Figure (Fig. 1) states the assumption of 'emissions at current level' but no values are given. If the gas is generic then this value must be cited. If they are N2O and CH4, then the values can be assumed to be known. There does not seem to be any good reason not to identify the unnamed gases as N2O and CH4. [David Wratt & David Fahey (Reviewer's comment ID #: 67-85)]	REJECT: long discussion in the writing team.Here, these are illustrations of concepts and not projections
10-1295	A	98:38	98:46	"behaviour of CO2 is completely different" - how was this simulated? Which model? [Chris Jones (Reviewer's comment ID #: 120-36)]	ACCEPT: clarified
10-1296	A	98:43	98:43	Replace "reduction" by "elimination". [Govt. of Finland (Reviewer's comment ID #: 2009-147)]	REJECT: reduction better
10-1297	A	98:43		Suggest clarifying by changing to: 'In fact, only in the case of essentially complete emissions reduction can the atmospheric concentration….' Truly complete emissions reduction is only required for an infinite lifetime. [David Wratt & David Fahey (Reviewer's comment ID #: 67-86)]	ACCEPT: wording changed
10-1298	A	98:49		This sentence is problematic if taken out of context because it states that 'reductions in emissions would not result in stabilization of CO2.' This is strictly not true because complete emissions reductions would stabilize CO2. So suggest restructuring thought or adding qualifier as: '...less than nearly complete reductions in emissions would not result in stabilization of CO2 concentrations, but rather [David Wratt & David Fahey (Reviewer's comment ID #: 67-87)]	ACCEPT: reworded
10-1299	A	98:54	98:54	Consider explaining "… well-known chemical and biological adjustments" by adding something like, "As reduced emissions lead to decreased concentration of carbon dioxide in the atmosphere, the oceans and land will release to the atmosphere some of the carbon dioxide they've been storing." [Melinda Marquis (Reviewer's comment ID #: 162-93)]	REJECT:dont go into more detail
10-1300	A	105:0		It should be stated in the figure caption what the zero line is. Presumably temperatures are	Accepted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				plotted relative to the 1980-1999 average, as in subsequent figures. [Adrian Simmons (Reviewer's comment ID #: 242-144)]	
10-1301	A	110:0	110:0	The nearly complete lack of stippling in areas of decrease (for both precipitation and sea level pressure) suggests an error in the way these (and possibly some other) maps were generated. [Govt. of Finland (Reviewer's comment ID #: 2009-148)]	Rejected. Stippling is correct.
10-1302	A	110:0	113:	Figs. 10.3.6, 10.3.7, 10.3.9 are the maps for which it seems most important to mask out regions that do not pass statistical significance (see overall chapter comment). [J. David Neelin (Reviewer's comment ID #: 187-24)]	Rejected. The stippling is not a statistical significance test. The ensemble of opportunity does not allow for a significance test. Further, significance depends on the spatial scale considered.
10-1303	A	110:0		Fig. 10.3.6: Wet=red, dry=blue?? [Stefan Rahmstorf (Reviewer's comment ID #: 206-28)]	Accepted, colors changed.
10-1304	A	110:0		The colour convention for the precipitation plots could be changed to the more usual convention of blue for wetter, red for drier. This is what is used in the corresponding precipitation plot in Figure 10.3.9. [Adrian Simmons (Reviewer's comment ID #: 242-146)]	Accepted, colors changed.
10-1305	A	111:6	119:14	"Multi model" should be "Multi-model" in the figure captions of Figure 10.3.7, Figure 10.3.9, Figure 10.3.10, Figure 10.3.12 and Figure 10.3.15. [Chiu-Ying LAM (Reviewer's comment ID #: 139-20)]	Accepted.
10-1306	A	117:1	117:17	This figure is unclear and confusing. I would strongly suggest truncating all timeseries at 2100, and removing the lines corresponding to the models that clearly do not match 20th C conditions. The description should be left as is. [Gavin Schmidt (Reviewer's comment ID #: 227-16)]	Rejected. Showing all models is important to communicate the spread and difficulty in modelling overturning. Selecting only models that fit nicely and removing others is misleading and implies more skill than there is.
10-1307	A	120:0	120:	Changes are given in units of standard deviations; such units are not easily understood by policy makers, at least without an adequate explanation of the relevance of such a normalization [Govt. of France (Reviewer's comment ID #: 2010-82)]	Rejected. Model aggreation is not possbible without normalization. Explanation is clear. More details can be found in the Tebaldi et al. Paper.
10-1308	A	120:0		Fig 10.3.16: The units of these figures are unclear: is this what the final sentence of the caption is trying to explain ? If so it needs to be made clearer, and moved nearer to the beginning of the caption. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-48)]	Rejected. Explanation is clear. More details can be found in the Tebaldi et al. Paper.
10-1309	A	120:5	120:5	Replace "extremes" by "precipitation intensity and number of dry days".	Rejected. Details given in the following

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Govt. of Finland (Reviewer's comment ID #: 2009-149)]	sentence.
10-1310	A	121:0	121:	Changes are given in units of standard deviations; such units are not easily understood by policy makers, at least without an adequate explanation of the relevance of such a normalization [Govt. of France (Reviewer's comment ID #: 2010-83)]	Rejected. Model aggreation is not possbible without normalization. Explanation is clear. More details can be found in the Tebaldi et al. Paper.
10-1311	A	121:5	121:5	Replace "extremes" by "growing season length and number of frost days". [Govt. of Finland (Reviewer's comment ID #: 2009-150)]	Rejected. Details given in the following sentence.
10-1312	A	122:0		Fig 10.4.1: What are the blue (?) lines in panel (b) ? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-49)]	ACCEPTED: new caption
10-1313	A	124:0		Figure 10.4.3: Please add results for SP350, a peaking and subsequent stabilization at 350ppm CO2 levels. Given that various current policy goals aim for long-term stabilization below 2°C temperature rise above pre-industrial levels, it seems policy-relevant (not policy-prescriptive) to inform decision-makers about CO2 emission implications of stabilization levels at 350ppm CO2 and its uncertainties. Leaving these low stabilization levels out, could be interpreted as policy-prescriptive. [Govt. of Germany (Reviewer's comment ID #: 2011-217)]	NOTED : Scenario SP450 has been added in figure 10.4.2. For figure 10.4.3 we use the same scenarios as the ones used in the TAR (450, 550, 650, 750, 1000).
10-1314	A	128:0		Figure # 10.5.1 y-axis titles on panel b and c could read "equilibrium precipitation change" and "transient precipitation change" so clarify that they are different. [David Sexton (Reviewer's comment ID #: 233-12)]	Accepted.
10-1315	A	129:0	129:	In common with your usual unscientiufic practice you have chosen once agin to present graphs with uncertainties which are only ± 1 standard deviation. This is quite unacceptable. The whole lot should be redrawn with 95% confidence limts. Doubling the uncertainties given.The minimum temperature rise for 2100 is now 1.5°C for scenario B1, somethiong which we can live with, after we have made allowance for all the upward biases of the other scenarios and models [VINCENT GRAY (Reviewer's comment ID #: 88-1708)]	Rejected. The uncertainty doesn't get any smaller or bigger whether 1SD or 95% are drawn, as long at it is clearly stated what the uncertainty is. The probability for e.g. 1.5°C warming is independent of what uncertainty is plotted.
10-1316	A	129:16	129:16	"Moberg, 2003)" should be "Moberg, 2003).". [Chiu-Ying LAM (Reviewer's comment ID #: 139-21)]	Accepted.
10-1317	A	130:5		Figure #10.5.3 : Please include mitigation scenarios in this overview figure for cumulative emissions below 5 TtCO2eq. [Govt. of Germany (Reviewer's comment ID #: 2011-218)]	Rejected. WGI cannot assess mitigation scenarios at this stage due to the lack of a consensus and publications on mitigation scenarios.
10-1318	A	133:0	133:	I am surprised that you have the arrogance to present the results of a 1% increase per annum of carbon dioxide when you know that the current rate is 0.4% a year which has been stable for 30 years. This graph is nonsense [VINCENT GRAY (Reviewer's comment ID #: 88-1709)]	Figure deleted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
10-1319	A	133:0		Figure 10.5.6: On checking through the AR4 models listed here, one finds that results from several models listed on the PCMDI web site as having contributed 1%to2x simulations are not shown here. These include BCC-CM1; CGCM3.1 (T63); CSIRO-MK3.0; and GISS-EH. Some of these models have had the relevant fields up on the PCMDI site for a long time now. Suggest update the Figure to include the more complete set of models. [Anthony Hirst (Reviewer's comment ID #: 107-14)]	Figure deleted.
10-1320	A	134:0	134:0	The directly model-based distributions (AR4 AOGCMs) should be made thicker for easier identification from the other curves. [Govt. of Finland (Reviewer's comment ID #: 2009-151)]	Rejected.
10-1321	A	134:0	134:	Here we have another outrageous use of the one standard deviation method for descrtibinmg uncertainty, unique to the IPCC. If you were honest you would present the normal method of usiong two standard deviations and 95% confidence limits. This graph is deliberately misleading [VINCENT GRAY (Reviewer's comment ID #: 88-1710)]	Accepted. Uncertainties will be 5-95% where possible.
10-1322	A	134:0		Figure # 10.5.7 I think it would be better to have a different legend for each panel as this clarifies that different papers contribute to different panels. [David Sexton (Reviewer's comment ID #: 233-13)]	Rejected, figure becomes overloaded.
10-1323	A	136:2	136:6	Figure 10.6.1. How is the zero level chosen and what is its meaning? Several models don't cross zero until well into the first one or two decades of the century? [Donald L. Forbes (Reviewer's comment ID #: 72-24)]	Figure has been re-done
10-1324	A	137:0	137:0	Instead for showing the contours for standard deviation, this figure could also use stippling to indicate the areas where the mean change exceeds the standard deviation. [Govt. of Finland (Reviewer's comment ID #: 2009-152)]	This figure has been re-done.
10-1325	A	140:8		For clarity perhaps panel (a) should be described as the "total carbon added to the atmosphere, ocean and biosphere". (You do not mean total carbon on the planet.) [Martin Manning (Reviewer's comment ID #: 155-114)]	Rejected. Label in a) says 'CHANGE in total carbon' already, as does the caption.
10-1326	A	141:0	141:	The CO2 projections on the top graph are obviously wrong. There is no sign thatn the current rate is deviating from 0.4% a year [VINCENT GRAY (Reviewer's comment ID #: 88-1711)]	Rejected. CO2 follows SRES scenario as in most other projections shown.
10-1327	A	146:0		Fig 1 An extra panel with the results of say Forest et al 2002,2006 (with expert prior), Annan and HargreavesGRL 2006, Hegerl et al 2006 (entitled "Multiple constraints" perhaps) would help to give a more credible impression. [James Annan (Reviewer's comment ID #: 6-29)]	Rejected. Expert priors are questionable. Multiple constraints are poorly understood, not well established and depend on an arbitrary choice of individual constraints to be combined.
10-1328	A	146:0		Fig. 1 of Box 10.2: I think one cannot show Annan et al 2005 as a serious estimate here,	Rejected. Box 10.2. only sumarizes

No.	Batch	Page:line From	Page:line To	Comment	Notes
				as neither all glacial forcings nor any changes in ocean heat transport were allowed for. The caption says "only provide an upper bound", but the upper bound is strongly affected by these omissions of first-order effects and thus is not robust on the basis of current scientific knowledge. The role of IPCC is to make an assessment, not a comprehensive review of every single publication. This involves the need to select the references with state-of-the-art methodology and robust and relevant conclusions. [Stefan Rahmstorf (Reviewer's comment ID #: 206-29)]	LGM constraints from chapter 9, whereach study is discussed.
10-1329	A	146:9		As Annan et al. omitted changed forcings (dust & vegetation) for LGM conditions, their results (on CS) might be too large by up to 1.4°C (derived from results on the effect of those forcings in Schneider von Deimling et al., 2006). The figure should be corrected for accordingly, or at least this key effect should be mentioned H19in the caption. [Hermann Held (Reviewer's comment ID #: 104-16)]	Rejected. Box 10.2. only sumarizes LGM constraints from chapter 9, whereach study is discussed.
10-1330	A	147:4	147:8	This figure caption is not understandable. I do not see the blue curves? For example what is meant by individual (rred curves) as opposed to the green curves? Suggest that the basis for the calculations, be stated in caption. For example, one color could be used for each of the sets of data from the panels in Figure 10.2.1. [Haroon Kheshgi (Reviewer's comment ID #: 125-46)]	Fixed. No blue curve is shown, legend reworded.
10-1331	A	147:4	147:8	I question the assignment of likelihood to the calculations since this implies the assessment that each estimate is a complete estimate of uncertainty, and this is stated on page 48 (chapter 10) not proven. Specifically I refer to the caveat on lines 36-38 of that page. Suggest removing the likelihood indicators, and including a discussion of the assumptions needed to take this step. [Haroon Kheshgi (Reviewer's comment ID #: 125-47)]	Rejected. Figure simpley indicates quantiles for each PDF and how they translate into likelihood statements.