Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
12-1	12	0	0	0	0	The writing is patchy in quality-much very good but some sections overlong and taxing to understand. There is much detail but not enough synthesis and assessment. Section 12.5 is particularly weak currently, while 12.4 is too long and detailed.(Qiyong Liu, China CDC) [Qiyong Liu, China]	We have endeavoured to provid much more synthesis and assessment in the SOD. Section 12.5 has been significantly revised. Section 12.4 is long because of the amount of material to assess and the different variables looked at.
12-2	12	0				This is probably the most important chapter of AR5 and it does, in general, a good job in giving caveats and pointing out limitations of climate projections. However, all this careful discussion is not reflected in the key statements that are derived. That's why I see multiple inconsistencies in this chapter, specifically pertaining to the temperature projections: The strong statements are inconsistent with the caveats Chapter 12 discusses itself, they are inconsistent with the careful statements in Chapter 9 on model evaluation, they contrast starkly with cautious ond more reflective wording in Chapters 11 and 13, and, finally, Chapter 12 violates the IPCC Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties. I'll explain this in the following, more specific comments. [Gregor Betz, Germany]	The executive summary has been substantially revised and we have endeavoured to coordinate, where possible, with the other chapters mentioned. We have also more carefully implemented the guidance on uncertainty language.
12-3	12	0				In general, my impressions is that the IPCC Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties is not very well implemented. AR5 chapters are, according to that Guidance Note, supposed to determine overall confidence in key statements by assessing, separately, agreement (within the scientific community, not amongst models!) and available evidence. Only if both are strong and confidence is, therefore, high, should likelihoods be assigned. However, Chapter 12 seems to assign likelihoods to all key statements whatsoever. In addition, agreement, evidence and confidence are hardly explicitly assessed. So, there are no "tracable accounts" that show why evidence and agreement are sufficiently high so as to assign probabilities/likelihoods. [Gregor Betz, Germany]	As stated above, we have endeavoured to implement the guidance on uncertainty language more carefully.
12-4	12	0				Several places in the chapter you use the term anomaly or anomalies. Please consider if a less technical language and also an indication of direction of change can increase the readability for layman. E.g. with words like "increase" , "decrease" or "changes". [Øyvind Christophersen, Norway]	The use of anomalies is widespread in climate science and we feel it is important to note when projections are expressed as anomalies w.r.t. to some baseline period (a standard period is adopted where appropriate). We have hopefully imprved readability in the SOD.
12-5	12	0				The framing could be improved. There seems to be some tendency to be overconfident in models and model results. [Sybren Drijfhout, Netherlands]	Review comment unspecific. Some gaps were due to the lateness in receiving much of the CMIP5 data in the production of the SOD and the lack of papers assessing the output. We have had more time now to provide synthesis and assessment of CMIP5 results and contrast those with AR4 statements and CMIP3 results.
12-6	12	0				For the many reasons outlined above, the CO2 forcings used in particular in RCP8.5 and also in other scenarios, seem to be grossly overestimated. RCP8.5 would assume a warming of ~ 0.4°C per decade which was not observed during the last decade. How can one believe in these projections in these conditions ? In accordance with observations, the suggestion in the comment about Chapter 9 Page 16, retaining a more realistic forcing of 0.0025 W/m2 per year for anthropogenic CO2, would give a contribution of ~ 0.7 W/m2 in 2300, much lower than 12 W/m2. 0.7 W/m2 is an insurance that anthropogenic warming will not exceed 2°C with an ample margin of safety, even in case of occurrence of positive feedbacks which are suspected but not proven. Lindzen, R.S. and Y.S. Choi, Geophys. Res. Lett. 36 (2009) L16705, and Spencer, R.W., W.D. Braswell, J. Geophys. Res. 115 (2010) D16109, rather suggest negative feedbacks. [François GERVAIS, France]	We do not ascribe likelihoods to the RCP scenarios. They are simply used as examples to facilitate the production of future projections under different levels of radiative forcing. This comment seems to confuse past forcing and response with future projections. For the purposeses of assesing projections (which are dependent on assumptions regarding emissions or concentations) there is no reason to expect that past or current trends will continue.
12-7	12	0				the phrase “ESM” meaning an earth-system GCM is becoming widespread and is used throughout the FOD. But I think it is misleading, and misses the key point that these models are still GCMs at their core – a better phrase is needed which emphasises the important difference between Earth System GCMs and, say EMICs (which are also “earth system models”). If AR5 uses the phrase ESM then this will become set in stone. Now is an opportunity to come up with a better one. One suggestion would be “ES-GCM” (analogous to A-GCM, O-GCM, AO-GCM etc... for Atmos-, ocean-, or coupled atmos-ocean-GCMs) [CHRIS JONES, United Kingdom of	At present we adopt the terminology of the FOD Glossary.

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						Great Britain & Northern Ireland]	
12-8	12	0				there is nothing on Geoengineering in this chapter – ch6/7 cover processes – should Ch12 cover projections? No GeoMIP proejctions shown in Ch6/7 yet [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland]	Noted. Geoengineering is not covered in this chapter.
12-9	12	0				This chapter is generally balanced, reads well and gives a good account of the physical mechanisms leading to the simulated changes, were known. It is still lengthy, but I don't have any inspiration on how to shorten it, except perhaps to omit some of the more specualtive areas of analysis [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Noted
12-10	12	0				My commendations to the authors on the enormous amount of work done to compile this chapter. As overall comments, it appears to me that uncertainties regarding hydrological cycle changes should be more clearly stated, and the breadth of work on the hydrological cycle changes should be more carefully represented. No doubt this will be addressed in the revisions as more results are available from other analyses of CMIP5 models. It is natural for the authors to write and reference what they know best (which often includes their own work). I have tried to supply some additional references and no doubt other reviewers will do the same. [J. David Neelin, United States]	We are thankful to this reviewer for his detailed comments on the hydrolgical cycle section which have helped us improve the section considerably.
12-11	12	0				This chapter is lacking explicit references to relevant material from the IPCC SREX (2012, chapter 3). [Sonia Seneviratne, Switzerland]	Accepted. References to SREX were added and consistency is now discussed.
12-12	12	0				None (could not figure out how to delete rows in this spreadsheet, it seems to be impossible) [Steven Sherwood, Australia]	Noted. Bug reports on Excel should be sent to Microsoft…
12-13	12	0				In general I found that non-CO2 GHG were given short shrift in the chapter. Although there were good discussions of the issues, mechanisms and uncertainties surrounding them, I often expected to see CO2-e in graphs where simply CO2 was shown. You may want to consider showing both CO2 and CO2e in some of these graphs, to show more clearly the contribution from the other gases. [Steven Sherwood, Australia]	The chapter shows only radiative forcing. RCPs are introduced in chapter 1.
12-14	12	0				This chapter admirably discusses the sources of uncertainty, but then says little in many places about how to interpret the model projections in light of known discrepancies between model and observed past behaviour. Actually the ocean and ice parts of the chapter did a very good job of integrating issues regarding past performance of models with the future projections; my main complaint is with the atmosphere-related phenomenon: rainfall, circulation, etc. For example, you show that models have dramatically undersestimated tropical expansion since 1980 but this passes almost without comment. Does this mean that actual regional climate changes could be far greater than predicted in the models? There are similar understimates of changes in precipitation extremes. I realise it is early days and you probably need the updated Chapter 9 to help with this. [Steven Sherwood, Australia]	Noted. Agree with the review that the integration of model evaluation, observation, and attribution into projections is an important aspect. We have attempted to improve this where possible, but the literature is often sparse, and CMIP5 model evaluation is only slowly becoming available.
12-15	12	0				Content of the present chapter is sufficiently descriptive. Readily available bibliography has been sufficiently taken into account. No significant modifications are suggested to text or figures at this stage. [Dirk Thielen, Venezuela]	Noted
12-16	12	0				As there are different estimates of radiative forcing for a given scenario, it is important to provide a complete view on how scenarios compares between each other. It would therefore be very useful to 1) include A1FI in comparisons with newer scenarios, such as in Fig 12.2; 2) assess the actual coverage of radiative forcing in the current scenario literature by the RCPs : given the ESM/AOGCM results, do the RCPs achieve their objective of covering all the published "emission space" ? [Jean-Pascal van Ypersele, Belgium]	SRES A2, A1B and B1which have been used for CMIP3 are shown in the figure. Assessment of the scenario range is outside the mandate of WG1.
12-17	12	0				The Executive Summary needs to be restricted to key findings from the underlying assessment. We therefore suggest to substantially shorten, e.g., the scenario introduction. The statement concerning the influence of volcanic eruptions (page 4, line 37-38) needs further quantification and has to be based on the assessment provided in the Chapter. [Thomas Stocker/ WGI TSU, Switzerland]	The executive summary has been sunstantially revised in the SOD. There is no statement about volcanic eruptions in this paragraph.
12-18	12	0				Section 12.4.4: We note you include a very comprehensive section on Extra-tropical cyclones (12.4.4.3). We therefore propose to include a note regarding why tropical cyclones are not covered in Chapter 12 (including a link to Chapter 11 and 14 where this coverage can be found) [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Good point, and the chapter now makes a distinction between extratropical and tropical storms. A note on this has been added to the preamble of the

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							section, and the reader is also referred to relevant information in other chapters, especially Box 14.3.
12-19	12	0				Using the term "most likely" will be confusing as it's not consistent with the terminology introduced in the uncertainty guidance for the AR5. We therefore suggest to find an alternative phrasing (e.g., in AR4 "best estimate" was used). [Thomas Stocker/ WGI TSU, Switzerland]	Rejected. Most likely is simply the value that is most likely, i.e. the mode of the distribution. It is not IPCC calibrated language, but IPCC has no corresponding language to express that. Most likely should not be in italics.
12-20	12	0				Temperature extremes (Section 12.4.3.3): When speaking generally of 'warm extremes' or 'hot extremes' it is not always clear whether you are referring to daily extremes of max and min, or including also heat waves/warm spells. For example, the executive summary statement, page 4, lines 31-35. We suggest to avoid such general terminology and be specific in these instances. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Rewritten to be more specific.
12-21	12	0				Section 12.5.4: Some of the phrasing concerning the 2degree target (and other policy goals) seems outside the WGI mandate, eg, line 18 "The most prominent target currently supported is the 2degree target....". Please avoid commenting on the prominence and/or the usefulness for the policymakers of any results/scenarios. [Thomas Stocker/ WGI TSU, Switzerland]	Rejected. It is a fact that the 2°C target is adopted by the governments, so it is clearly the most prominent target. The chapter does not comment on the usefulness, but simply provides context why this target is discussed.
12-22	12	0				The discussion on the historical evolution of climate change scenarios, including the link to UNFCCC and to the target of stabilizing atmospheric GHG concentrations, seems to be be information that is relevant for the context setting Chapter, i.e., Chapter 1. We suggest coordination with Ch1 in order to consider moving this section to Chapter 1. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Chapter 11 introduces the scenarios. A discussion of stabilization is still needed in the corresponding section.
12-23	12	0				The assessment of climate extreme events should be building on the SREX Chapter 3 assessment, We therefore suggest to carefully consider the SREX assessment when assessing extreme events. Potential differences in the assessment and revised likelihood statements will need to be carefully discussed as part of the Chapter 12 assessment. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. The SREX statements are reviewed for consistency with Chapter 12 statements, with differences also explained.
12-24	12	0				Please describe how multimodel results are combined, put on a common grid, and presented in, e.g., maps (incl. grid information etc). Please check and ensure consistency of approach across chapters, especially for Chapters 9,11, 12, 14 and, of course, Annex I: Atlas [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Processing is as consistent as possible and will be improved further, even though the sheer volume of data and incosistencies in some data files makes this very challenging.
12-25	12	0				We suggest that the RCP projections should be compared to SRES-based projections in the first figures showing key parameters (e.g. modify Fig 12.4, include SRES CMIP3 boxplots...). [Thomas Stocker/ WGI TSU, Switzerland]	There is a whole section which compares SRES and RCPs later in the chapter, with various methods. Comparison is not as simple as plotting them on the same figure, because both the scenarios and the models have changed.
12-26	12	0				Consider adding a table on key parameter changes for different RCPs (2100; abs/rel changes, e.g. T, Precip, Sea Ice etc.) [Thomas Stocker/ WGI TSU, Switzerland]	Rejected. Temperature is given in a table. Global precipitation is meaningless. Sea ice changes are given in the sea ice section.
12-27	12	0				I think it is very important that Chapter 12 makes an explicit statement about how it is using the various sources of information to make inferences about future climate. E.g. what is the status of the CMIP5 models vs CMIP3? PPEs vs MMEs? If each conclusion is based on an ad hoc expert judgement melding the various sources of information, that needs to be stated. Some conclusions at present seem to be simply arrived at by reading off numbers from the CMIP ensemble, whereas some bring in a wider range of information. There may be good reason for this, but in any case it would help the reader if the basis for inference could somehow be made clear in each section - for example through some kind of shorthand or footnotes. A common approach with Ch 9 would really strengthen the report. [Richard Wood, UK]	Noted. This is indeed an important point. Model validation in Ch. 9 does not necessariliy imply better projection of changes. There needs to be a detailed look at the physics behind each change, which may vary regionally. The new draft tries to make the link to model evaluation more explicit. Note also that Ch. 9 has extended their discussion on how model evaluation links to projections.
12-28	12	0				I very much welcome the presence of CMIP3 data in many of the figures and discussion. I realise this may partly be just a consequence of limited availability of CMIP5 data, but I would urge the authors to make full use of CMIP3 information in later drafts. This is a big opportunity for AR5 as it's the first time we have the chance	Accepted. We agree, so long as we recognize that they may rest on similar modeling assumptions and thus may all suffer from the same errors. Scenarios

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						for quite thorough comparison with earlier generation (but nevertheless quite mature) models. Consistency across model generations (or not) seems to me to be a useful tool in understanding the robustness of results. [Richard Wood, UK]	differences pose a problem when comparing CMIP3 and CMIP5
12-29	12	1	1	1		Long-term Climate Change: Projections, Commitments and Irreversibility [Medani Bhandari, Nepal]	Noted. No change requested.
12-30	12	1	1	8	20	The summary is comprehensive, but should be more focused. We propose to highlight the most important key findings from the chapter and decribe those in an easily manner in the executive summary. Furthermore, the key findings related to tipping points should be included in the executive summary. [Øyvind Christophersen, Norway]	The executive summary has been substantially revised to be more focused and comprehensive.
12-31	12	1				Note that the EC-Earth model results for CMIP5 have become available and published in peer-review literature. If not on the ESG server the data can be found on climexp.knmi.nl, together with the other CMIP5 runs. The references are 14. Hazeleger, W. et al, 2010. "EC-Earth: seamless earth system prediction in action." Bull. American Met. Soc., 91, 1351-1356. and 1. Hazeleger W., et al., 2011: "EC-Earth V2.2: description and validation of a new seamless Earth system prediction model." Clim Dyn. in press [Sybren Drijfhout, Netherlands]	EC-EARTH model results are now included in figures when it has been possible to obtain data.
12-32	12	1				This is a comprehensive chapter which covers a lot of ground and is well-written. Gibven how late many of the model results came in, the chapter is quite comprehensive. The authors may be planning to address many of the issues I raise in my comments in subsequent drafts, but I raise them now just in case. [Nathan Gillett, Canada]	Noted. The whole chapter has been reivsed with much more CMIP5 data available now.
12-33	12	1				Confidence ranges on projections are given throughout the chapter in different ways. In the ES confidence statements on future climate change in the real world are given e.g. 'For RCP4.5, 6.0 and 8.5, global temperatures likely exceed 2C warming with respect to present day by 2100.'. Some figures also give confidence estimates on future climate change in the real world e..g. 12.6, 12.39. Most other timeseries figures simply give a plus or minus one standard deviation range across the ensemble of models. No attempt is made in the text to relate this ensemble spread range to an uncertainty range on projected warming in the real world. The first recommendation of the IPCC Good Practice Guidance Paper states 'Forming and interpreting ensembles for a particular purpose requires.... clarity about the assumptions e.g., about model independence, exchangeability and the statistical model that is being used or assumed.'. This is lacking at present and is key to the interpretation of the projected changes which are shown throughout the chapter. Where uncertainty ranges on projected changes for the real world are given, the assumptions are not always clearly explained in the chapter (even if they are in references). On a closely related point, when maps or zonal means are shown, stipling shows model agreeement, with apparently arbitrary criteria (more than half models with a significant change, >80% agreeing on sign). It would be more helpful if the hatching told us something about actual expected future climate chagne, rather than model agreement, which is really telling us something about the models. For example the hatching might tell us where the future climate change is expected to be of the sign shown at the 5% confidence level. Of course, some assumptions woudl have to be made to do this, but it is better that these are clearly stated and justified in the report, rather than just showing model agreement and leaving the reader to guess what this means for the real world. [Nathan Gillett, Canada]	Stippling and hatching is revised and described in box 12.1. The chapter separates uncertainty from model range, and only uses the former when model quality, observations, process understanding has been consired. While we fully agree that this woudld be desirable for all variables, the is not much literature to assess except for temperature. Doing this in a formal way for CMIP5 would go beyond the literature and require a large number of assumptions (e.g. model independence, CMIP5 sampling the uncertainty) that are known to be problematic.
12-34	12	1				More emphasis should be given to the uncertainties in projected global mean warming, which are likely to be a key component of the summary for policymakers, and which currently are only discussed in a short paragraph 12.4.1.2. The sources of uncertainty considered, and the statistical model and assumptions underlying the ranges shown are not discussed - only a reference to Rogelj et al. is given. Given the importance of these ranges, I think these details should be discussed in the chapter. As noted in my previous comment, the IPCC Good Practice Guidance Paper recommends that such assumptions are clearly stated. [Nathan Gillett, Canada]	Section and figure revised, number mentioned in the summary. Uncertainty is also consistent with chapter 13.
12-35	12	1				This chapter would benefit with more references to chapter 9 and 10. The purpose of chapter 9 is primarily to inform the use of models in the projections chapter, but it is only cited a handful of times, and then usually a general reference to the whole chapter not to specific sections. Also in several instances, simulated changes over the historical period are compared to observed changes over the historical period, and this information is used to inform predictions. While I think this is a reasonable thing to do, it would be better if such comparisons	Accepted. References were added where possible, but coordination remains challenging when all chapters are written at the same time.Suggest that chapter 9/10 authors explcitly point to places where references should be placed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						were carried out in chapters 9 and 10, and chapter 12 referred to the asessment made in those chapters. [Nathan Gillett, Canada]	
12-36	12	1				Several places in the chapter 'annual' is used when 'annual mean' is meant. For example the captions to figures 12.11, 12.13, 12.15, 12.26, 12.27, 12.28 use 'annual' in place of 'annual mean'. 'Annual' by itself implies 'per year'. For example, annual runoff in Fig 12.27 would be the runoff in one year, but the units are mm per day, so the meaning must be 'annual mean'. [Nathan Gillett, Canada]	Accepted.
12-37	12	1				Unforced variability is called 'internal variability' not 'natural variability'. Natural variability includes the response to natural forcings, but chapter 12 deals mostly with internal variability, since natural forcings are consistent across models. Examples: pg 3, ln 10, pg 3, ln 14 - 'natural variability' should be 'internal variability'. See glossary definition of 'climate variability'. [Nathan Gillett, Canada]	Accepted. Changed throughout the text.
12-38	12	1				The explanation given for the regional pattern of precip changes several times in the chapter is too simplistic. Several times, the authors say that increases in high latitude precip are projected because of the 'additional water carrying capacity of the warmer troposphere' (e.g. pg 5, ln 42-43, pg 35 ln 10-11, pg 36, ln 35-37, pg 64 ln 57). But since the whole troposphere is projected to warm, this argument implies that precipitation should increase everywhere, whereas it does not. Held and Soden (2006) provide an explanation for the simulated pattern of precip changes: if you assume that RH remains constant, and atmospheric circulation stays the same, then moisture fluxes will increase but keep the same pattern, and the climatolgical pattern of P-E will intensify. This is why dry regions get drier and wet regions get wetter. This should be cited an briefly explained in the chapter. [Nathan Gillett, Canada]	The text was written specifically focusing on precipitation changes in high latitudes, based on previous studies. The implication that warming everywhere would give precipitation increases everywhere is not implied by a statement focused specifically on high latitudes. Also, there is additional discussion talking about factors controlling changes elsewhere.
12-39	12	1				Treatment of uncertainties should be revised to follow the IPCC Uncertainty Guidance Note. No calibrated confidence language is used at in the chapter, except for one instance of 'low confidence'. Calibrated likelihood language is used, including 'unlikely' and 'very unlikely'. According to the guidance note, a likelihood assessment (likely, unlikely etc) should only be given when a range can be given based on quantitative analysis or expert judgement. If only information about the sign of a change is given, for example, this should be accompanied by a confidence assessment. Most statements where low likelihood is expressed should be replaced by confidence assessments. [Nathan Gillett, Canada]	Accepted. Uncertainty language implemented more consistently throughout the chapter.
12-40	12	1				The CMIP3 models are sometimes referred to as AR4 models. CMIP3 was the intercomparison project and AR4 assessed the results, so the models should be called the CMIP3 models. E.g. pg 36, ln 48 'Annual surface evaporation in the AR4 increased' - presumably this means the CMIP3 models. [Nathan Gillett, Canada]	Accepted. Changed in hopefully all instances.
12-41	12	1				Different measures of model spread are used in different parts of the chapter. In some places one standard deviation, in other places two standard deviations. I would advocate showing 5-95% ranges of intraensemble variability. This was the consensus reached in AR4. [Nathan Gillett, Canada]	Partly accepted. Attempts are made to display maps with unified stippling and hatching. Uncertainies are not always derived in a straightfoward way as 5-95%. AR4 was not uniform in that respect. For example, Fig. SPM5 showed 1stddev and likely ranges, deviating already twice from the consensus that the reviewer claims.
12-42	12	1				An excellent first draft in all. In the areas where I am qualified to comment, I find few or no major omissions or inaccuracies in the discussion of long term climate changes. Any minor comments I have are shown below. [Benjamin Sanderson, United States of America]	Noted
12-43	12	1				hyphenate pre-industrial throughout. [Benjamin Sanderson, United States of America]	Editorial
12-44	12	2	1	8	20	ES too long and detailed. Suggest cut by factor of 2-3. Discussion on RCP's should be rationalised with discussion in Chap 11 ES. [Robert Colman, Australia]	Accpepted. Summary shortened.
12-45	12	3	1	8	20	Thanks to the authors for the clarity in language. Although this is probably the longest Executive Summary of all chapters it should not be shortened due to its policy relevance. It is recommended that other chapters align their language to the language of this chapter in order to avoid any inconsistences (e.g. using the term equilibrium climate sensitivity (ECS). [Klaus Radunsky, Austria]	Coordination of terminology such as the ECS is achieved via the Glossary.

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12-46	12	3	1			The subject is long term climate change. However the chapter seems to focus entirely on projections with large scale climate models. Qualify the subject of the chapter in the title "Long term climate change as studied by GCMs" or broaden the coverage. [Stephen E Schwartz, USA]	Regional models are covered mostly in chapter 14. Chapter title and scope can no longer be changed.
12-47	12	3	1			I am surprised that the exec summary starts with uncertainties. I would have thought that the summary would start with findings. Then qualify with uncertainties. But first some bold generalities that can be supported by material presented in the chapter. Example: All models agree that increasing CO2 concentrations will lead to a warmer world with more precip, more extreme weather events, higher sea level... To my thinking this is what the reader is looking for. Something that says that this community agrees on some things instead of hiding behind uncertainties due to natural variability. Another important set of findings that belong right at the top are those dealing with Equilibrium Climate Sensitivity, Transient Climate Response and Transient Response to Cumulative Carbon Emission, page 12-7. [Stephen E Schwartz, USA]	Accepted. Section rewriten and repositioned.
12-48	12	3	1			and throughout. Pay attn to use of first person plural; suggest restrict to we the authors of the chapter. "Our understanding". Whose? The authors? the scientific community. Here could simply strike "Our". Let it read "Understanding ... has not changed". [Stephen E Schwartz, USA]	Accepted. Clarified in most instances. We refers to the authors.
12-49	12	3	1			It is essential throughout this chapter to show projected aerosol forcings in addition to GHG forcings for every model run for which temperature results are presented. The community now recognizes the importance of aerosol forcing and how it is treated. One can imagine that for the severe reduction in emission scenario 2.6, the aerosol forcing is greatly reduced from that at present so that the total forcing will increase. One imagines as well that the reduction in total forcing depends on the present assumed aerosol forcing, which is highly uncertain, so that there must be a fairly broad envelope of total forcings associated with each of the ghg forcings. It would seem essential to show this envelope, and as well to indicate whether the modeled temperature changes that contributed to Fig 12.4, for example, propagated the uncertainty in forcing resulting from aerosols into each of the models that contributed to that figure and thus that the envelope reflects that uncertainty, as opposed to each model picking its own aerosol forcing that in some way best matches twentieth century observations, which would of course greatly narrow the uncertainty range. As many of the scenarios are used repeatedly, it would seem that the aerosol forcings and total forcings could be shown once and for all, for each of the scenarios and models. My speculation is that even for a given GHG scenario, the aerosol forcing and total forcing will differ substantially from model to model. Page 12-91 line 9-10 states "Aerosols and other forcing factors are implemented in subtly different ways in each ESM." This underscores the need to present the total forcings and to reveal whether and how the uncertainty in aerosol forcing is accounted for. [Stephen E Schwartz, USA]	Taken into account - text and figure(s) revised with cross-references to assessments in Chapters 8 and 9. Section 12.3.3 changed to include separate discussion of projected aerosol forcings based on CMIP5/ACCMIP results, the future total forcing envelope being summarised in the revised Figure 12.3. Models exhibit distinct aerosol forcing characteristics and simulations from any given model don't therefore explore the full uncertainty range of current/future aerosol forcing.
12-50	12	3	1			Executive summary: you probably know this already, but this section seems much too long (six pages??) and poorly written. No non-expert would be able to make head or tail out of this or find the key points. Also, I don't understand why the summary begins with uncertainty, rather than what is known and of more interest to readers. It is fair enough to explicitly discuss progress in how we understand uncertainty, and to elevate this to the exec summ level, but it is strange (and one could say evasive) to start off with this esoteric subject before even summarising the findings or predictions that are the point of the chapter. If the scoping team had decided to have an "uncertainty" chapter than this might have made sense. [Steven Sherwood, Australia]	Accepted. Uncertainty section is significantly revised and repositioned.
12-51	12	3	3	3	17	Strange to begin with uncertainties. Move this section to end of Exec Summ. [Scott Power, Australia]	Accepted. Uncertainty section is significantly revised and repositioned.
12-52	12	3	4	3	17	You cannot get any idea of the uncertainty of "projections" until they have been tested aginst future reality. Since this has noit ben done this work is futile [VINCENT GRAY, NEW ZEALAND]	IPCC projections conditional on scenarios. Preditions and uncertainty estimates can in principal be made based on physcial understanding without verification. Physcial theories (e.g. relativity) predicted a number of effects that were only later observed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
12-53	12	3	5			and elsewhere in this chapter "experiments." These are not experiments as the term is usually used in science. They are model calculations. Suggest replace "experiment" throughout by "model study" or the like. [Stephen E Schwartz, USA]	Rejected. Model experiment is a commonly used expression in the literature
12-54	12	3	5			More deeply and intrinsically, it appears that the community and individuals who are conducting the model "experiments" are the same community as the authors of the chapter. It would seem that a fair assessment of the state of the ability of models to project future climate can be carried out only by someone external to the community doing the modeling. One resolution of this issue is to have the comparisons of the models and the assessment that resides in this chapter be carried out by that set of authors in the peer reviewed literature, and then have that work assessed in the present document. [Stephen E Schwartz, USA]	Rejected. Procedural comment that does not address the content of the chapter.
12-55	12	3	6	3	8	One source of uncertainty that is not mentioned here is the lack of relevant observations for some variables (in particular extremes, see IPCC SREX, section 3.2.1). This uncertainty cannot be properly assessed. Indeed, structural or parametric uncertainty can only be partly estimated from multi-model range if observational data are not available to identify possible systematic biases shared by all models. [Sonia Seneviratne, Switzerland]	Rejected. Model evaluation belongs to Chapter 9.
12-56	12	3	6	3	9	Need to make terminology consistent with Chapter 11 (11.2.1). Aslo, the meaning of "prevalent" is unclear. [Rowan Sutton, UK]	Done.
12-57	12	3	7	3	7	replace 'future forcing scenarios' with 'future climate forcing' [Benjamin Sanderson, United States of America]	Accepted. Changed to "external" forcings and rephrased the entire sentence.
12-58	12	3	8	3	12	As mankind searches for the missing puzzle pieces to predictions of long term climatic cycles, polar shift, extinction of the species, cyclical population explosions, answers to alternatives to the theory of plate tectonics, reversals of geographic cli [Helen LookYat Taylor, United States]	Rejected. Extinction of species, plate tectonics etc. are outside the remit of the chapter.
12-59	12	3	8	3	12	Observation of the moving Sun is the key to true planetary motions and long-term climatic forecasting. True planetary motions and rhythmic climatic changes have not been accurately charted as this key factor has eluded all previous astronomical thought. N [Helen LookYat Taylor, United States]	Rejected. Variations in the orbit of the sun are not relevant on the time scales of the projections considered.
12-60	12	3	13	3	14	This sounds like a comment on a particular study. Surely any assessment of model differences should take account of internal variability when assessing whether differences between models are significant? If one study did not, I don't think this needs to be highlighted in the ES, unless this is a comment on the conclusions of the AR4. [Nathan Gillett, Canada]	We have explicitly stated that this changes our assessment of areas of model disagreent from past assessments.
12-61	12	3	16	3	16	What temperature-related quantities are meant here? I can think of some temperature-related quantities for which agreement is low (e.g. sea ice area, ice sheet mass balance, sea level). I would say just that agreement is higher for temperature itself and leave it at that. [Nathan Gillett, Canada]	Changed to simply "temperature"
12-62	12	3	20	3	52	Where are these elusive new scenarios?. I have so far found no description of what they assume [VINCENT GRAY, NEW ZEALAND]	Various references are included, in particular the overview by van Vurren et al., 2011. Scenarios are introduced in chapter 1.
12-63	12	3	22			need to stress that “ESM”s or whatever phrase we use, are still GCMs with additional capability. GCM is a well trusted phrase and don't want to leave it open to the reader's interpretation whether or not “ESM”s are still GCMs or not. They are [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland]	We currently use the terminology of the Glossary.
12-64	12	3	23	3	24	Chapter 10 has a proposal that the glossary definition of 'climate model' is revised such that the definition of 'AOGCMs' is broadened to include ESMs. Since ESMs include coupled oceans and atmospheres it seems strange to me that they are not included as AOGCMs. [Nathan Gillett, Canada]	The chapter is adopting the defintions in the glossary.
12-65	12	3	29			Were any of the CMIP3 models ESMs? [Nathan Gillett, Canada]	Not in the CMIP3 setup. No change requested.
12-66	12	3	31	3	32	It would help to know roughly what magnitude of forcing nitrates will produce (is it a major omission, or is small compared to other factors?) [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Radiative forcing is addressed in the forcing chapter and in Annex II.
12-67	12	3	33			As the 'concentrations-driven' and 'emissions-driven' projections being discussed here really refers only to CO2, I think that should be made clear at the outset. Perhaps start the line with "Considering CO2,…". Virtually all the models are 'emissions-driven' for aerosols, for example, even what's being called here the	Accepted.

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						'concentration-driven' projections. [Drew Shindell, USA]	
12-68	12	3	35			I suggest replacing 'more equal footing' with 'equal footing'. The concentrations of greenhouse gases in the emissions driven simulations are identical in the ESMs and other models. The only difference might be small changes in the land surface properties with climate change in the ESMs, but I should think that this difference is small compared to other differences between model forcings. [Nathan Gillett, Canada]	Accepted.
12-69	12	3	43	3	43	The 40% difference is presumably in 2100? [Timothy Carter, Finland]	Yes. Changed.
12-70	12	3	44	3	48	The difference between model-diagnosed concentration forcing for 2091-2100 and that idealised out of the IAMs is disturbingly large! How could this underestimation have happened, especially for the RCP 6.0 forcing out of the AIM model, for which even the idealised model forcing (at 5.5) was well below the 6.5 value it was supposed to emulate? Does this potentially undermine the "representativeness" of the Representative Concentration Pathways? Is the top end of the projections (7.5 W/m2) actually capturing the high end forcing in the literature? [Timothy Carter, Finland]	Noted. The analysis of this is still underway and literatur is sparse. Part of it may be the way the forcing is diangosed. The revised chapter includes whatever is available on this topic in section 12.3.
12-71	12	3	44	3	48	can we quantify why GCMs and IAMs get quite different RF? Given that GHG concentrations are prescribed it seems unlikely this is the cause, so is it because IAMs don't treat radiative forcing from aerosols well or from land-use at all? [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland]	Noted. The analysis of this is still underway and literatur is sparse. Part of it may be the way the forcing is diangosed. The revised chapter includes whatever is available on this topic in section 12.3.
12-72	12	3	44	3	48	This summary of the radiative forcing is confusing. Need to be clear - what type of models used, all forcings used? Including aerosols? The forcings are relative to what time period? Error bars? What is the assessed ranges? Etc. [Ronald Stouffer, USA]	Accepted. Now explicitly refers to adjusted forcing of CMIP5 and includes uncertainties and time periods.
12-73	12	3	49	3	52	Man is still attempting to gauge and predict ice- ages by examining ice- samples and tree barks for evidence of cyclical climate change when the accurate forecasting will now be available through this documented discovery of my concept of solar orbital pa [Helen LookYat Taylor, United States]	Rejected. Palaeoclimates are outside the remit of this chapter.
12-74	12	3	54	8	20	Here we go again, just personal opinions of the experts who were paid tor the jobs and have, therefore, a conflict of interest [VINCENT GRAY, NEW ZEALAND]	The chapter contains an assessment of the current state of knoweledge on future projections, assessed by the chapter lead author team, with input from contributing authors and based on the peer-reviewed literature. None of the authors were paid by IPCC for their input. Issues of conflict of interest are dealt with by the TSU.
12-75	12	3	55	3	55	Comments on next few decades should be restricted to Chapter 11 [Rowan Sutton, UK]	The general comment is noted but sometimes it makes sense, when describing long-term climate change to discuss the pathway towards those changes.
12-76	12	3	55	3	56	I suggest replacing 'irrespective of the GHG concentration pathways as represented by the RCPs' with 'under any of the GHG concentration pathways represented by the RCPs'. The point is that warming is virtually certain for any of the RCPs, not for any conceivable GHG concentration pathway. I think this is what the current wording is saying, but I think this could be spelt out more clearly. Zero emissions of all greenhouse gases or a geoengineering scenario could give cooling. [Nathan Gillett, Canada]	Accepted - text revised
12-77	12	3	55	3	56	Refer to chapter 11 [Michel Petit, France]	Taken into account - the general comment is noted but sometimes it makes sense, when describing long-term climate change to discuss the pathway towards those changes.
12-78	12	3	55	3	56	I disagree with the assessment of virtually certain. Caveats are needed. Absence of any large volcanoes is one caveat. There is a similar statement in the decadal prediction chapter ocean change section which assesses the likelihood as extremely likely in the absence of major volcanoes. The two statements need reconciled. I agree with the one in the decadal prediction chapter. [Ronald Stouffer, USA]	Taken into account - the 'virtually certain' is now separated from the 'continue to rise' sentence. It is now associated with global temperature change at the end of the 21st century and revised to 'very likely'

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12-79	12	3	55	4	2	This is just 1 example of the aforementioned comment. The last decade we experienced serious slow down, or even halt of global warming. Moreover, the geographical pattern associated with the trend-difference between 2000-2010 and 1990-2000 is, apart from the well-known La Nina pattern, quite anomalous. In all other aspects, it doesn't look like the geographical standard deviation-patterns of the observations, nor of that of the models. So, we have no real clue what is going on. More aerosol emissions than anticipated? A stronger indirect aerosol effect than modeled? Changes in differentiation between thermocline and deep ocean heat uptake that our models do not capture? Changes in water vapour between troposphere and stratosphere that our models do no resolve? Also, all RCP-scenario's are quite optimistic in predicting quick and sharp decreases in aerosol emissions, which may not occur in reality. They do not reflect the full range of uncertainty. I would be more prudent in this "virtually certain" statement, moreover, doesn't this belong to Chapter 11? I would suggest to more openly discuss possible biases in scenarios and model projections. Maybe a separate section for this issue? [Sybren Drijfhout, Netherlands]	Taken into account - this section has been revised so that the likelihood statement now refers to the end of the 21st century changes under the RCPs
12-80	12	3	55			"It is virtually certain that global-mean surface temperature will continue to rise over the next few decades irrespective of the GHG concentration pathways as represented by the RCPs. " Let it read: Model studies show that It is virtually certain that global-mean surface temperature will continue to rise over the next few decades irrespective of the GHG concentration pathways as represented by the RCPs. This is true for the RCP's examined, all of which have increasing forcing over the next few decades, Figure 12.3. So the statement really needs to be qualified. It may be virtually certain that the forcings will continue to increase, but that is dependent on emissions, which needs to be stated. So let it read: Model studies show that It is virtually certain that global-mean surface temperature will continue to rise over the next few decades irrespective of the GHG concentration pathways as represented by the RCPs, all of which show increasing forcing over the next several decades. Then the statement would be accurate. However it would need to be backed up by figures showing total forcing (and aerosol forcing, because the calculated temp will depend on the forcing as well as the model). It would seem that virtually all of the "will" type conclusions here and in the next several pages need to be qualified by inclusion of he assumption in the sentence. It seems essential to distinguish the results of model calculations that are subject to assumptions from predictions of what _will_ happen in the future. The language at page 4, line 52 is pretty good: " Models simulate a decrease in cloud amount in the future" [Stephen E Schwartz, USA]	Taken into account - this section has been substantially revised.
12-81	12	3	57			Replace 'radiative forcing' with 'scenario'. Temperature change is always dependent on the radiative forcing over the previous few decades, but the point here is that the scenarios have similar radiative forcing for the first few decades of the century. [Nathan Gillett, Canada]	Accepted - text revised
12-82	12	3				Exec summary – seems strange to open the whole chapter with the uncertainty paragraph – stressing what we don't know before we discuss what we do. The content is fine, but suggest you discuss results first and then their uncertainty. [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland]	Accepted.
12-83	12	3				Exec summary – seems strange to open the whole chapter with the uncertainty paragraph – stressing what we don't know before we discuss what we do. The content is fine, but suggest you discuss results first and then their uncertainty. [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland]	Accepted.
12-84	12	4	1	4	2	Is this warming relative to present day or preindustrial? [Nathan Gillett, Canada]	Taken into account - present day should have read preindustrial. This section has been revised.
12-85	12	4	1	4	41	.So now we are witness to the fact that continents do not drift, the Sun cycles around the galaxy affecting climatic manifestations on every planet in the solar system is the key to climatic changes on a long and short term basis throughout the galaxy. [Helen LookYat Taylor, United States]	Rejected. Variations in the orbit of the sun are not relevant on the time scales of the projections considered.

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12-86	12	4	2	4	2	Why not giving an absolute value helpful for decision makers ? [Michel Petit, France]	Taken into account - temperature ranges from the CMIP5 models are now given
12-87	12	4	3	4	9	seems confusing to mix up the reference period against which to measure climate change – even in the same paragraph! Would be nice to choose either present day or pre-industrial and stick to it. The oft quoted 2 degree target is relative to pre-industrial [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland]	Taken into account - present day should have read preindustrial. This section has been revised.
12-88	12	4	3	4	9	inconsistent use of pre-industrial and present day baseline [Benjamin Sanderson, United States of America]	Taken into account - present day should have read preindustrial. This section has been revised.
12-89	12	4	3		9	Repeat of comment on Figure 12.4 (page 12-94 of Hires figs). [Stephen E Schwartz, USA]	Comment addressed where figure is positioned.
12-90	12	4	8	4	8	make clear that emissions are net negative [Benjamin Sanderson, United States of America]	Reworded to clarify.
12-91	12	4	8			"negative emissions". This is clearly some sort of jargon. Emissions are inherently positive. One might imagine some means of removal of CO2 from the atmosphere by scrubbing or the like, and it seems that that is what is meant here, but surely that sort of activity needs some introduction beyond the term "negative emissions." [Stephen E Schwartz, USA]	Reworded.to clarify.
12-92	12	4	10	4	14	I'm not sure if this result is encouraging (being qualitatively similar to and hence confirming the AR4 uncertainty range) or disappointing (given the massive investment in the CMIP-5 exercise, which doesn't seem to have yielded too much more insight.) In any case, is it possible for AR5 to provide uncertainty ranges for intermediate periods (that were lacking in AR4 despite all of WG II's best efforts!) that are of more relevance for impacts and adaptation (e.g. for the time slices used in the atlas). [Timothy Carter, Finland]	Accepted. The CMIP5 5-95% range for global temperature can be interpreted as likely. Short term uncertainties are provided in chapter 11.
12-93	12	4	10	4	14	This may be true for literature published when the FOD was written. But surely even the additional observations will have helped constrain TCR and carbon cycle properties? Chapter 10 concludes that TCR can be better constrained from observations than at the time of the AR4 (although admittedly the asesssed range is still 1-3 C). [Nathan Gillett, Canada]	TCR assessment has been revised
12-94	12	4	11	4	12	The statement that the range for TCR has not changed since AR4 is not consistent with statements in Chapters 10&11. [Rowan Sutton, UK]	TCR assessment has been revised
12-95	12	4	11			Equilibrium climate sensitivity is not a good predictor of end of 21st century warming - TCR is much better. [Nathan Gillett, Canada]	Noted. Text does not claim that. This point is discussed in various places in the chapter.
12-96	12	4	12	4	14	As I explain below, I don't think this is well justified. Rather, I suggest to say that the overall confidence that temperature anomalies remain in this moderate range has decreased. In other words: We are less confident that climate change won't be extreme. And a simple statement of confidence, without assigning likelihoods, would be more appropriate here. See also comments below. [Gregor Betz, Germany]	Rejected. The assessment of ECS and TCR is supported with dozens of studies and different lines of evidence. AR4 assigned a likelihood to ECS and TCR and this is well accepted in the community.
12-97	12	4	13	4	14	Uncertainty is a percent of what (-40 to +60%)? Temperature change? Reword. [Ronald Stouffer, USA]	Summary statement changed
12-98	12	4	19	4	19	.. Arctic region warms more than others under all... [Benjamin Sanderson, United States of America]	Taken into account - text revised to clarify this statement
12-99	12	4	19	4	22	I also believed feedback mechanisms from melting sea ice contributing to the stronger T rise in the Arctic compared to the Antarctic [Matthias Zahn, United Kingdom]	Taken into account - text revised to indicate the persistence of the Antarctic ice sheet
12-100	12	4	21	4	21	plus lack of local albedo feedbacks? [Benjamin Sanderson, United States of America]	Taken into account - text revised to indicate the persistence of the Antarctic ice sheet
12-101	12	4	28	4	30	Confidence in this pattern is extremely high, even if there are doubts as to the magnitude of global warming. I suggest confidence for NH high lat max is virtually certain. [Robert Colman, Australia]	Taken into account - text revised to include the very high confidence in this pattern
12-102	12	4	31	4	35	OK, this says that the trend in extremes will be mixed with variability on all timescales. That's pretty obvious but the question of real interest is on what timescale (if ever) a change will become detectably outside the normal variability. On its own this bullet doesn't really say much. The next paragraph tries to quantify this in terms of return periods. Maybe better to combine this bullet with the second sentence of the next one?	Certain temperature extremes have already become detectible (See SREX section 3.3.1) However, this is the domain of Chapter 10.

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						[Richard Wood, UK]	
12-103	12	4	33	4	38	Isn't the real issue that the aerosol forcing is not well known? That seems to be the single largest issue in estimating the climate sensitivity from observations. If so, this summary needs to make that much clearer…. [Ronald Stouffer, USA]	Rejected - the comment does not refer to the line numbers given
12-104	12	4	36	4	36	It would be good to define more clearly what is meant by “rare”. [David Sexton, UK]	This is a tricky point as the definition of extreme is so imprecise in the literature varying from the 5% event to the long period return value. In this statement, which refers to 20 year return values in the second sentence, rare refers to such long return period events. The statement is true for events with somewhat shorter return periods as well as for events with much longer return periods. We feel that the statement is general and elect to retain the FOD wording.
12-105	12	4	36	4	38	Confusingly written. Do you mean the warmth of low temperature extremes - i.e. they are getting warmer faster? [Robert Colman, Australia]	Taken into account - text clarified
12-106	12	4	36	4	38	I struggled to understand what this was saying about the cold extremes. After digging into the main text and Fig. 12.13 I think the text is wrong. The magnitude of the low temperature extremes decreases. Needs rephrasing. [Richard Wood, UK]	Taken into account - text revised to indicate the increases in the return values
12-107	12	4	36	4	41	to experience greater increases => to rise faster [Matthias Zahn, United Kingdom]	Taken into account - text clarified
12-108	12	4	43	4	49	This section on pattern scaling is very useful to have in the executive summary. It would benefit from a little more detail on how such stability of patterns might be applied in scenario studies (e.g. for impacts and adaptation). Thus, which variables might be scaled and under what circumstances? Is it valid to scale between different RCPs or between different time periods, or both? Obvioulsy the detail should be in the chapter proper, but the conclusions could be given here a little more explicitly, with confidence statements based on the authors' expert judgement. [Timothy Carter, Finland]	We have added more discussion/references/results in the section about limitations and the bullet in the ES better reflects those
12-109	12	4	44	4	49	I think this bullet (and the underlying text) paints slightly too rosy a picture of the applicability of pattern scaling. A number of recent studies show that because of a range of hyseteresis effects the pattern scaling approach breaks down under highly aggressive mitigation scenarios (stabilistaion/negative emissions), e.g for global and regional precipitation. While that might be primarily only applicable to RCP2.6, I think there is clear evidence since AR4 of limitations to the approach and they need to be stated more explicitly. (I have commented also on this in the main text section). [Richard Wood, UK]	We have added more discussion/references/results in the section about limitations and the bullet in the ES better reflects those. But the section already makes it clear that there are limitations for miitigation scenarios -- equilibrium warming.
12-110	12	4	45	4	45	the statement that pattern scaling "remains valid" should be more nuanced. [Rowan Sutton, UK]	We have added more discussion/references/results in the section about limitations and the bullet in the ES better reflects those. But the section already makes it clear that there are limitations for miitigation scenarios -- equilibrium warming.
12-111	12	4	52	4	55	Is this this true ? I would expect warming to cause more evaporation, and thus a higher amount of atmospheric water and more cloud [Matthias Zahn, United Kingdom]	Changes in clouds depend on many things, not only on water vapor. See corresponding section and figures.
12-112	12	4	56	4	57	I think it would be good to say first that TOA is positive over all scenarios throughout the 21st century. Although the current text is correct, I read 'even decreases' as implying negative TOA, though I realise it is just referring to the time evolution of positive TOA. [Nathan Gillett, Canada]	Accepted.
12-113	12	4		8		There is insufficient focus on what's new since AR4, and too much just describing model projection results. It would be [Rowan Sutton, UK]	Accepted. We have tried to provide this where possible. However, space is limited and the number of variables is very large. In most cases the assessment is not much different from CMIP3.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
12-114	12	4		8		most helpful if, for each set of variables there was a brief summary of the key AR4 results, and then specific comments [Rowan Sutton, UK]	Accepted. We have tried to provide this where possible. However, space is limited and the number of variables is very large. In most cases the assessment is not much different from CMIP3.
12-115	12	4		8		on the extent to which AR5 findings are consistent or not. There is large inconsistency between subsections in terms of whether raw model results are described uncritically, or whether they are "assessed", as they should be. [Rowan Sutton, UK]	Noted. The SOD provides more detail, but literature on CMIP5 only starting to become availabe. In many cases there are no methods/paper that allow us to go beyond simply showing the model range and mean.
12-116	12	5	2			I think saying that the flux imbalance at the TOA is smaller than the radiative forcing up to 2100 is not necessary. Since the surface temperature responds to the radiative forcing this will always be true, except after an instantaneous change in forcing, when the two would be the same, or if the forcing decreases strongly, in which case the TOA will be of the opposite sign, and may be larger. [Nathan Gillett, Canada]	Accepted.
12-117	12	5	5	5	30	The WG I SPM will clearly need to make statements on how findings in this chapter match ongoing trends in observed climate. For instance, there are some statements on drought frequency in Chapter 2 that may or may not be in accordance with the projected trends in atmospheric circulation in certain regions. However, it may be that the observed trends are not being analysed in the context of regions identified by models as projecting significant change, though I expect that the detection and attribution chapter would analyse such changes for certain variables. Some liaison is required between this chapter and the observation/attribution chapters to see if there is correspondence or not, and then to think about how this information should be reflected in summary statements. [Timothy Carter, Finland]	Discussion of consistency or lack thereof between observed changes in Chapter 2 and projected changes in Chapter 12 has been added (e.g., 12.4.4.3).
12-118	12	5	7	5	8	what does 'imbalance between LT water vapour and precip' mean?. What imbalance? [Robert Colman, Australia]	Statement dropped from the revised executive summary
12-119	12	5	11	5	17	The poleward shift of storm tracks is much less visible in the North Atlantic sector. My understanding of Tim Woolings work is that it is counteracted by an AMOC decline. This is consistent with the equatorward shift of storm tracks which occurs after an AMOC collapse. See also page 32, lines 4-5. [Sybren Drijfhout, Netherlands]	The statement about Northern Hemisphere storm-track changes was removed, because the revised section 12.4.4.3 indicates lowered confidence in proejcted changes on the basis of several analyses.
12-120	12	5	11	5	17	I think there needs to be a discussion here and in the main text of the recent results of Scaife et al. "Climate change projections and stratosphere–troposphere interaction", Climate Dynamics 2011 (published online). This raises the question of how robust the jet/storm track and regional precipitation changes predicted by standard climate models are, when a resolved stratosphere is added to the models. I think this study needs to be assessed, and its implications for regional climate projections commented on. [Richard Wood, UK]	The Scaife et al. paper has been discussed in 12.4.4, along with other relevant papers that do not always support its findings.
12-121	12	5	18	5	18	Ice free seasnally or continuously? Clarify. [Benjamin Sanderson, United States of America]	This comment appears to have been given incorrect page identifiers.
12-122	12	5	18	5	24	This is a good exmple of where the projections are consistent with an already observed trend, see work by Timball for example. As I said in a General Comment there needs to be a comparison between observed and projected trends somewhere in the report. [David Griggs, Australia]	Comparisons between observed trends and projected changes have been added, guided by results presented in Chapters 2 and 10.
12-123	12	5	23	5	24	This is too weak as it stands. What does a 'poleward shift' mean? Strictly this means even the slightest displacement and so says nothing quantifiable, and 'by the end of the century' is very cautious. I suggest a range be specified, or otherwise be reworded, indicating earlier dates, and that there will be a trend over the century. [Robert Colman, Australia]	This statement has been revised, though we note that the uncetainty guidance for WG1 authors includes making statements about robustness of simply the sign of a change.
12-124	12	5	25	5	25	"less indication". Clarify with respect line 12.. Is the meaning less indication of a poleward shift in the tracks in winter than in summer ? Based om models results ? [Michel Petit, France]	These statements have been revised to be consistent. In addition, the statement about Northern Hemisphere storm tracks was dropped due to lowered confidence in proejcted changes on the basis of several analyses.
12-125	12	5	28	5	28	"consistency with previous projections" is a very poor basis for assessing reliability. [Rowan Sutton, UK]	The statement about Northern Hemisphere storm-

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							track changes was removed, and the text on these tracks has been revised.
12-126	12	5	29	5	29	What exactly does "thermal energy" refer to here? Usually thermal energy refers to total potential energy, most of which is irrelevant for atmospheric dynamics (only the much smaller available potential energy associated with horizontal temperature differences matters). [Jouni Räisänen, Finland]	The statement about Northern Hemisphere storm tracks was dropped due to lowered confidence in proejcted changes on the basis of several analyses. The corresponding statement in 12.4.4.3 has also been modified to remove reference to "thermal energy".
12-127	12	5	32	6	8	No scenario dependency discussed for water cycle section. [Benjamin Sanderson, United States of America]	Noted. Pattern scaling implies that the water cycle changes also scale approximately with temperature. Space constraints in the summary do not allow us to provide more details.
12-128	12	5	34	5	36	No circulation changes are required for the decrease in relative humidity over land. Because of the land-sea contrast in warming, the increase in moisture transport from ocean will be unable to keep up with the increase in saturation humidity over land even for unchanged circulation. [Jouni Räisänen, Finland]	Rejected. Text does not imply that circulation changes are required, but they do happen in reality.
12-129	12	5	37	5	39	I don't think it is virtually certain that precip will increase at apporoximately 2%/K. This may be what the models show, but a numer of observational studies find a higher rate of precip increase with temperature (e.g. Zhang et al., 2007; Wentz et al., 2007; Allan and Soden, 2007), closer to the C-C rate. [Nathan Gillett, Canada]	Accepted. Statement revised for SOD.
12-130	12	5	37	5	39	"It is virtually certain, that precipitation increase will be much smaller, approximately 2% K–1, than the rate of lower tropospheric water vapour increase (~7% K–1), due to global energetic constraints." The degree of certainty, combined with the degree of precission noted, in this statement needs very careful consideration. It appears not consistent with the more conservative statement in the main text (p 12-34, line 5-8): "Overall, the global-mean precipitation change ... its rate of increase per oC global warming is very likely to be less than that of atmospheric water vapor." This latter statement appears much more supportable from the evidence given. The muted precip response is very robust across models so far, but all models have their limitations in terms of processes, resolution and/or domain. Global scale observations are largely too short at this stage to confirm this behaviour (though noting the Arkin et al. (2010, ERL, 5, doi:10.1088/1748-9326/5/3/035201) study of 20th century rainfall trends is constent with the muted precip increase). But, can we really say "virtually certain", rather than just "likely" or "very likely"? [Anthony Hirst, Australia]	Agreed. Statement in Executive Summary is changed to very likely.
12-131	12	5	40	5	40	Presumably this should read "average precipitation change in a much ...." [Timothy Carter, Finland]	Accepted. Changed to "changes in average precipitation "
12-132	12	5	40	5	40	Sentence doesn't make sense: needs 'changes' in there. In any case this should be reworded, possibly without the use of calibrated language in the first part. It is certain that some regional variation would take place. Also it is not clear what is virtually certain, that there is regional variation, or that some areas would dry? [Robert Colman, Australia]	Accepted. Changed to "changes in average precipitation " Virtually certain refers to the projection of a combination of increases and decreases
12-133	12	5	40	5	40	Should be: "precipitation change". [Jouni Räisänen, Finland]	Accepted. Changed to "changes in average precipitation "
12-134	12	5	40	5	40	that average precipitation in a much warmer => that average precipitation changes in a warmer [Matthias Zahn, United Kingdom]	Accepted. Changed to "changes in average precipitation "
12-135	12	5	40			Precipitation' should be replaced with 'precipitation change'. [Nathan Gillett, Canada]	Accepted. Changed to "changes in average precipitation "
12-136	12	5	46	5	49	This constitutes a modification of the IPCC SREX (2012, chapter 3) assessment. This should be stated in the main text of the chapter and differences in the underlying data basis or in the way the assessment was derived should be clarified. [Sonia Seneviratne, Switzerland]	Our assessment is based simply on patterns of reduced precipitation, runoff and soil moisture and not consideration of the varying types of drought some have analyzed. Also, our assessment includes the

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							CMIP5 ensemble, which was not available for the SREX.
12-137	12	5	50	5	52	(Repeat of earlier comment) I think there needs to be a discussion here and in the main text of the recent results of Scaife et al. "Climate change projections and stratosphere–troposphere interaction", Climate Dynamics 2011 (published online). This raises the question of how robust the jet/storm track and regional precipitation changes predicted by standard climate models are, when a resolved stratosphere is added to the models. I think this study needs to be assessed, and its implications for regional climate projections commented on. [Richard Wood, UK]	Scaife et al. (2012) is discussed in 12.4.4.1, along with other relevant papers that do not always corroborate their results. There is also evidence that the drying is strongly associated with changes in the downwelling branch of the Hadley Circulation.
12-138	12	5	54			Replace 'either increase or decrease' with 'change'. [Nathan Gillett, Canada]	Accepted
12-139	12	6	5	6	6	Can a corresponding statement be made on changes in the characteristics, i.e., frequency and intensity, of long-lasting precipiaition events, i.e., wet spells? [Wilhelm May, Denmark]	Noted. We feel this is an area that needs attention. The literature appears sparse in this regard.
12-140	12	6	6	6	8	Here the discussion is about regional changes in evapotranspiration, presumably actual evapotranspiration. In the previous bullet point, changes in potential evaporation are described for various regions. Are readers supposed to equate the regions of increased AET mentioned here, but not specified, with regions projected as experiencing increased PE in the previous bullet? These measures are not the same, but it would be very useful for policy makers to know which regions are implied as vulnerable to increased risk of agricultural drought. [Timothy Carter, Finland]	We have modified this part of the bullet statement as follows "Over land areas where increased evapotranspiration is projected, the evidence indicates medium confidence that soil moisture will decrease over many land areas over the 21st century particularly in dry regions despite an increase in the likelihood of more intense individual storms."
12-141	12	6	18	6	18	Should be: "nearly ice-free Arctic Ocean in summer". [Jouni Räisänen, Finland]	Accepted - Text revised.
12-142	12	6	26	6	26	In addition to the changes in total precipitation and ablation, the change in the phase of precipitation (more rain at the expense of snowfall) also matters. [Jouni Räisänen, Finland]	Noted. Space constraints do not allow to provide all details in the summary.
12-143	12	6	31	6	31	Should this read: "....by between 31% (RCP2.6) and 73% (RCP8.5)." ? [Timothy Carter, Finland]	Accepted.
12-144	12	6	34	6	39	I don't see that evidence and agreement are sufficiently strong so as to justify the assignment of likelihoods to AMOC projections. See also below. [Gregor Betz, Germany]	Taken into account - The AMOC threshold discussion has been moved entirely to 12.5.5.2 where a new discussion of model sensivity exists. Note that an assessment that it is "likely" that the AMOC would not undergo an abrupt transition direcly implies that there would be a 1 in 3 chance that it would collapse this century. There is no evidence to support a 1 in 3 chance of the AMOC collapsing in the 21st century. The additional evidence regarding model sensitivity does not change the assessments of the AR4 or SAP 3.4 (Abrupt Climate Change) of the US National Assessment. In addition,a new analysis of CMIP5 models and EMICs further underscores teh MOC's stability
12-145	12	6	34	6	39	Also the statement that it is very unlikely that the AMOC undergoes an abrupt transition in the 21st century, seems to me overconfident. It is also not entirely consistent with the section on page 57; lines 41-53, where it is stated that models may overestimate the stability of the AMOC. Also, 3 CMIP3 models show an AMOC evolution over the historical period plus A1B scenario that comes closes to a collapse, namely FGOALS, IPSL and CGCM_Mk0. [Sybren Drijfhout, Netherlands]	Taken into account - see response to 12-144
12-146	12	6	34	6	39	And section 12.4.7.2. Need to note some evidence that most GCMs may overestimate the stability of the Atlantic Meridional Circulation as noted in 12.5.5.2, [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Taken into account - see response to 12-144
12-147	12	6	34	6	39	The discussion of AMOC thresholds, here and in the main text, seems too simplistic, given a number of lines of recent evidence. See more detailed comments on main AMOC text. Since the later ES text on thresholds is	Taken into account - see response to 12-144

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						very vague, it's important to get the message right in this bullet. [Richard Wood, UK]	
12-148	12	6	34	6	41	While 20 lines was used to depict changes in the cryosphere, less than 10 lines are used for the ocean. Since the ocean is covering 70% of the earth and the cryosphere a few percents, I find this quite unfair. At least the authors should consider a few words on the response of ENSO (first mode of variability of the climate system, with important teleconnections) and the tropical Indian and Atlantic Oceans [Didier Swingedouw, France]	Modes of variability are discussed in chapter 14.
12-149	12	6	38	6	38	An assessment that AMOC shutdown is "very unlikely" is not justified in view of the known limitations of GCMs and recent evidence that that models might be too stable (e.g. Hawkins et al, GRL, 2011) [Rowan Sutton, UK]	Taken into account - see response to 12-144
12-150	12	6	38	6	39	There is possible codflict between this line and page 8, lines 17-18 [Laura Jackson, United Kingdom of Great Britain & Northern Ireland]	Rejected. The latter is a general statement about such events. Potentially does not imply any likelihood.
12-151	12	6	44	6	51	It might be worthwhile mentioning that in AR5 more climate models including a module for the carbon cycle are considered than in AR4. This may have effects on some of the climatic changes projected by these models, such as the timing of the stabilization of climate with constant anthropogenic forcing. [Wilhelm May, Denmark]	The section tries to provide differences in results, not in the structure of the ensemble, and since we cannot at this point in time attribute differences to the presence of an active carbon cycle module we have not dwelled on that aspect.
12-152	12	6	52	6	56	Some of this information could be used to back up the earlier statements about pattern scaling (P4, L43). In fact, the information in trhis bullet point could be moved to the earlier discussion, as it doesn't really provide a comparison of CMIP3 and CMIP5 [Timothy Carter, Finland]	Rejected. The discussion here focuses on the similarity between the patterns derived from the two sets of model experiments, so it belongs here.
12-153	12	7	1	7	1	“Long-term” needs defining somewhere, especially here as the first bullet talks about “beyond 2100” yet I think "long-term" in the chapter title really means “longer than near-term”. This then implies that the whole executive summary must be about "longer than near term" climate consistent with the chapter title, whilst in this subsection it must mean “Climate change beyond 2100” so that this subsection is distinct from the rest of the summary. Is that correct? [David Sexton, UK]	Accepted. Subsections of summary as well as chapter sections relabelled to clarify.
12-154	12	7	8	7	9	This fraction of warming realised at stabilization may be constant across the RCPs considered, but it can't be constant across all possible scenarios. For example an instantaneous doubling of CO_2 scenario would have zero warming at stabilisation. A scenario is which CO_2 increased linearly to doubling at 10000 years would have almost 100% realised warming at stabilisation. Replace 'and is almost independent of the forcing scenario' with 'for the RCP scenarios considered'. [Nathan Gillett, Canada]	Accepted. The numbers were derived for the RCP at year 2300 where the temperature is close to equilibrium when the forcing is kept constant. For more abrupt forcings the fraction is much smaller. Paragraph in summary as well as main text extended to clarify.
12-155	12	7	8			The finding "If radiative forcing were stabilized, the fraction of realized warming at that point is around 85 ± 10% of the total, and is almost independent of the forcing scenario. Equilibrium is reached only after centuries to millennia." is enormously important. The implication is that the great majority, 75 - 95%, of warming that is committed to by the forcing at any given time is realized at that time, provided that forcing would be maintained indefinitely. This "finding" in model calculations reflects the fact that the models are representing a rather small energy imbalance. Consider the energy balance eqn: dH/dt = N = F - lambda * DeltaT whence DeltaT = lambda^-1 * (F-N) So the implication is that N is 15 ± 10% of F; note lambda^-1 is equilibrium sensitivity. Is this finding correct? Only if N is such a small fraction of F. N is about 0.6 ± 0.25 W m-2; if F is 1.95 ± 0.9 (Chapter 8, p 3 line 9) then for central values N/F = 31% and at extremes 12 to 81%, the latter if aerosol forcing is large (negative) so that forcing is small. So the fact that the models are finding this result says something about the values of F and N that characterize	The numbers were derived for the RCP at year 2300 but forcing stabilizes at different at different times in different scenarios. For more abrupt forcings the fraction is much smaller. Paragraph in summary as well as main text extended to clarify. The assumption is indeed that forcing is kept constant. This is not an economic scenario but a way to estimate the response timescales of the model.

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						the models. This must be discussed. A second important implication is based on the requirement that the forcing be sustained indefinitely. This would require cessation of almost all emissions of GHGs, but maintenance of aerosol forcing (by geoengineering?). This also needs to be discussed. [Stephen E Schwartz, USA]	
12-156	12	7	8			The finding stated here also supports the utility of the transient climate sensitivity concept and quantity, that the increase in temperature is proportional to the forcing by the transient climate sensitivity. This is discussed notably in Held IM, Winton M, Takahashi K, Delworth T, Zeng F, Vallis GK (2010) Probing the Fast and Slow Components of Global Warming by Returning Abruptly to Preindustrial Forcing. J Climate 23:2418-2427. doi:10.1175/2009JCLI3466.1 Padilla, LE, Vallis GK, Rowley CW, (2011) Probabilistic Estimates of Transient Climate Sensitivity Subject to Uncertainty in Forcing and Natural Variability. J. Climate, 24: 5521–5537. doi: http://dx.doi.org/10.1175/2011JCLI3989.1 Schwartz S. E. (2012) Determination of Earth's transient and equilibrium climate sensitivities from observations over the twentieth century: Strong dependence on assumed forcing. Surveys Geophys. In press. http://www.ecd.bnl.gov/steve/pubs/ObsDetClimSensy.pdf Other terminology, e.g., "transient climate response," has been used Dufresne J-L, Bony S. (2008) An assessment of the primary sources of spread of global warming estimates from coupled atmosphere-ocean models. J. Climate 21: 5135-5144. doi: 10.1175/2008JCLI2239.1 [Stephen E Schwartz, USA]	This concept is now introduced in the summary box on TCR. More details about observational constraints on TCR/TCS from the observed warming are discussed in chapter 10.
12-157	12	7	11	7	12	This seems like a very important sentence, and potentially highly policy-relevant (SPM material?). To bring out the policy relevance better, can this statement be unpicked to consider changes over land and ocean separately? [Richard Wood, UK]	Noted. Land ocean contrasts are not much different from the transient.
12-158	12	7	14	7	15	maintained… to allow'. Too anthropomorphic: reword. [Robert Colman, Australia]	Accepted, changed.
12-159	12	7	14	7	15	a positive temkperature anomaly is maintained for decades to allow the ocean to lose its excess heat'. This seems to be imply an intelligent actor controlling the climate. In an experiment in which radiative forcing is increased then set to zero, the near surfcace air temperature after the forcing is set to zero is warmer than in a preindustrial control simulation because of a heat flux from the ocean to the atmosphere. [Nathan Gillett, Canada]	Accepted, reworded to clarify.
12-160	12	7	16	7	18	This bullet addresses a positive zero emissions commitment in a scenario including all major forcings. But I think some information should be given first about the zero emissions commitment to carbon dioxide alone to help set the context. 'For scenarios driven by carbon dioxide alone, global average temperature is projected to remain appxoximately constant for many centuries following a complete cessation of emissions, due to the competing effects of a slow reduction in atmospheric CO2 and a delayed warming response to the peak in radiative forcing. [Nathan Gillett, Canada]	Accepted. Clarified the difference between CO2 response and forcing which respond more quickly. See also FAQ 12.3
12-161	12	7	16	7	18	This reads strangely to me. The commitment is not due to reductions, but to what has been emitted. Perhaps end the sentence at "strongly positive", then have a second sentence starting with something like "Were all emissions to cease immediately, there would be continued warming due to a superpostiion...." or something like that. [Drew Shindell, USA]	Sentence reworded.
12-162	12	7	16		18	Finally there is language that indicates a recognition of the consequence of reduction of emissions of aerosol precursors. This needs to be presented earlier and reflected in the figures. [Stephen E Schwartz, USA]	Accepted. Clarified the difference between CO2 response and forcing which respond more quickly. FAQ 12.3 shows different cases, including one with aerosols set to zero

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12-163	12	7	18			a slow response to reduced CO2'. This bullett appears to deal with the high sensitivity case for which the ZEC for CO2 may be positive. But the explanation sugggests a cooling in response to CO2. This is confusing. I suggest separating the aerosol and CO2 effects in this bullett. 'For high climate sensitivities, ongoing warming for several centuries is simulated after a cessation of CO2 emissions. This positive commitment may be enhanced by the effect of an abrupt cessation of aerosol emissions, which will cause warming. By contrast cessation of emission of short-lived greenhouse gases will contribute a cooling influence.' [Nathan Gillett, Canada]	Accepted. Changed as suggested.
12-164	12	7	19	7	24	I think line 19-20 implies that temperature, although a widely assessed variable for various reasons, is the primary link to other physical/biogeochemical processes. Perhaps considering starting the paragraph with something like, "Global temperature is not directly linked to all aspects for the climate system, and hence its stabilization does not imply a similar response in the climate system as a whole". [Stephanie Downes, Australia]	Correct but too long for the summary. The authors believe that this is implicit in the current wording.
12-165	12	7	25	7	29	inconsistent units of carbon – either use GtC or GtCO2. Given this is a science report I'd favour GtC (not CO2 or CO2e), although a conversion might be appropriate in the glossary [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland]	Rejected. These results are from actual economic multi gas scenarios, so all forcings are considered (see UNEP GAP report).
12-166	12	7	25	7	29	I think you should also give the corresponding numbers for "very likely limit warming …" [Henning Rodhe, Sweden]	This will be discussed among the authors and added if possible, but may not be because the underlying literature does not provide it. The main difficulty is that a very likely statement requires a a very likely estimate of projections, TCR etc. which at least in AR4 was not feasible.
12-167	12	7	25		29	What assumptions here about aerosols? Specify. [Stephen E Schwartz, USA]	These results are based on various integrated asessment models, so aerosol forcings are consistent with GHG forcings in each scenario. Details are given in the section and the underlying literature.
12-168	12	7	28			This number for cumulative emissions for 2C seems rather exact. I think published confidence ranges on this number are broader. This should be an assessed 5-95% range. [Nathan Gillett, Canada]	Accepted. Clarified that these are ranges from a set of scenarios that can't be interpreted in terms of likelihoods or confidence ranges.
12-169	12	7	36	7	37	The statement that the range for TCR has not changed since AR4 is not consistent with statements in Chapters 10&11. [Rowan Sutton, UK]	Accepted. Statement revised for SOD.
12-170	12	7	36		37	"The range of equilibrium climate sensitivities (ECS) and transient responses (TCR) covered by CMIP3 and CMIP5 cannot be narrowed significantly by constraining the models with observations. " This raises the question why not? My judgment is that it because of the uncertainty in total forcing that is due to uancertainty in aerosol forcing (and very little due to uncertainty in planetary heating rate). This point was examined in detail in Schwartz S. E., Charlson R. J., Kahn R. A., Ogren, J. A., and Rodhe H. Why Hasn't Earth Warmed as Much as Expected? J. Climate 23, 2453-2464 (2010); doi: 10.1175/2009JCLI3461.1. also Schwartz S. E. (2012) Determination of Earth's transient and equilibrium climate sensitivities from observations over the twentieth century: Strong dependence on assumed forcing. Surveys Geophys. In press. http://www.ecd.bnl.gov/steve/pubs/ObsDetClimSensy.pdf and would seem to call for discussion here. [Stephen E Schwartz, USA]	This statement refers to constraints from present day climatology and has been clarified. The fact that climate sensitivity and TCR can't be constrained strongly from the observed warming has been the subject of dozens of papers and is assessed in detail in chapter 10.
12-171	12	7	40	7	40	I don't think that the uncertainty language is being used correctly here. "Most likely" is not permissible terminology - check the IPCC uncertainty guidance! [Timothy Carter, Finland]	Rejected. Most likely is simply the value that is most likely, i.e. the mode of the distribution. It is not IPCC

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							calibrated language, but IPCC has no corresponding language to express that. Most likely should not be in italics.
12-172	12	7	40	7	42	Again, I suggest that the traditional IPCC key statement be revised by admitting that we are now less confident that ECS takes no extreme value. I don't think agreement and evidence are sufficiently strong so as to justify a likelihood statement. See also below. [Gregor Betz, Germany]	Rejected. There is consensus among the authors that very high values of ECS are in fact less likely than judged in AR4. For example some of the results by Stainforth et al. in CPDN were shown to be inconsistent with observations (Rodwell and Palmer 2007)
12-173	12	7	42	7	47	The statement that the range for TCR has not changed since AR4 is not consistent with statements in Chapters 10&11. [Rowan Sutton, UK]	Accepted. Statement revised for SOD.
12-174	12	7	43	7	43	Again, the uncertainty language is not being used correctly here [Timothy Carter, Finland]	Rejected. Most likely is simply the value that is most likely, i.e. the mode of the distribution. It is not IPCC calibrated language, but IPCC has no corresponding language to express that. Most likely should not be in italics.
12-175	12	8	1	8	5	I am not sure whether the concept of TRCE and PRCE is clear to everybody. Maybe some explanation is need. [Irina Mahlstein, Switzerland]	TCRE is defined in the summary statement and the glossary. The concept is discussed in the section. PRCE is removed.
12-176	12	8	2	8	2	"Best estimates" are also not consistent with the uncertainty guidance. [Timothy Carter, Finland]	Rejected. Similar to "Most likely", the best estimate is simply the value that is judged to be the most likely outcome, i.e. the mode of the distribution. It is not IPCC calibrated language, but IPCC has no corresponding language to express that.
12-177	12	8	3	8	12	This chapter starts off way too negative in my view. I think the opening paragraph should say what projections are capable of, rather than a string of statements on what they cannot do, the uncertainties involved, etc. Who says they are like weather forecasts anyway? And who expects predictions of the frequency occurrence of "all possible outcomes"? Simply calling models 'inadequate' is vague and misleading, as well as being inconsistent with chapter 9. Yes there is 'incomplete' understanding -- by definition -- but nevertheless our understanding of the essential features of and underlying processes behind climate change is high (high enough for meaningful projections), as evidenced by this and previous reports. [Robert Colman, Australia]	We agree and it was not our intention to give such a pessimistic introduction to the content of outr chapter. We have reworded the introduction
12-178	12	8	3			Why is a lower bound given on the range of cumulative emissions for which the quoted values of TRCE is valid? I would expect less nonlinearity for small emissions. There is no evidence of any nonlinearity for low emissions in the C4MIP simulations (Fig 12.46a). I would suggest replacing this with 'for cumulative emissions less than 2 TtC'. [Nathan Gillett, Canada]	Accepted.
12-179	12	8	5	8	5	Refer to a definition of "peak response to cumulated carbon emissions" ? [Michel Petit, France]	Acronym no longer used.
12-180	12	8	6			Is it really true that the temperature response is more delayed for larger cumulative emissions? What is the evidence for this? Second, as written the text implies that PRCE will be larger than TRCE for larger cumulative emissions. What is the evidence for this? The temperature response per unit carbon emissions tends to decrease for high cumulative emissions (see figs 12.46e and 12.46f, in which the curves tend to curve down at higher emissions). This doesn't come across in this bullet, which seems to suggest the opposite. [Nathan Gillett, Canada]	Statement removed. Same comment adressed in the section.
12-181	12	8	10	8	15	This paragraph ties precipitation change and temperature change too tightly together. We know that the delta_P vs delta_T relationship isn't single valued, especially under stabilisation/negative emissions scenarios. I think this needs to be captured in this paragraph. [Richard Wood, UK]	Statement removed.
12-182	12	8	10			The statement	Accepted. CO2 and aerosol case clearly separated in the revised version.

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						If anthropogenic carbon emissions were set to zero at some point in the future, slow surface to deep ocean export of CO2 and heat would lead to a near constant global temperature for several centuries, is erroneous. It assumes continued offset of GHG forcing by aerosol forcing. This statement needs to be qualified and the implications of terminating aerosol forcing or maintaining it by some sort of geoengineering need to be addressed. [Stephen E Schwartz, USA]	
12-183	12	8	16	8	20	While I wouldn't disagree with this statement, I think there is more that should be said and will be of interest to users of the AR5. For example there has been quite considerable progress since AR4 in understanding AMOC thresholds. Something is also needed on the possibility of long term irreversibility in these systems. See also comments on scope of Section 12.5.5. [Richard Wood, UK]	The summary statements have been revised and the text in 12.4 and 12.5 has been consolidated. Long term irreversibility remains difficult to assess due to lack of simulations.
12-184	12	8	17	8	18	There is possible codflict between this line and page 6, lines 38-39 [Laura Jackson, United Kingdom of Great Britain & Northern Ireland]	Statements revised.
12-185	12	9	3	9	12	Charlesworth and Okereke (2010) adds significantly greater difficulties in prediction for policy purposes. Indeed much of the rest of Chapter 12 does not make as much use of this analysis as it might. [Mark Charlesworth, United Kingdom of Great Britain & Northern Ireland]	Rejected. We do not comment here on policy difficulties or implications as this is the realm of WGIII.
12-186	12	9	3	9	12	Projections are not like weather forecasts because they are never tested against real future climate and nobody knows whether they can be relied upon/ [VINCENT GRAY, NEW ZEALAND]	Accepted. We hope that the introduction coveys this message.
12-187	12	9	3			This is an important paragraph that may be quoted often in the future. I think each assertion should be briefly explained. [Ramon de Elia, Canada]	Rejected. It is not clear which assertions are being referred to here. However, these brief introductory comments are expanded on greatly in the following sections of the chapter.
12-188	12	9	7	9	7	Predictions - Historically we called this projections. I think it is a mistake to mix the terms prediction and projection. Initial value forecasts made from observations are predictions. DecCen climate forecasts are projections when the initial state is obtained from a control run. [Ronald Stouffer, USA]	Accepted. There was some sloppy use of language here that has now been rectified.
12-189	12	9	14	9	20	The assessments are purely the personal opinions of experts with a conflict of interest [VINCENT GRAY, NEW ZEALAND]	Rejected. We accept comments from a number of reviewers. All authors have signed up for the IPCC conflict of interest framework.
12-190	12	9	15	9	18	Simple energy balance models (in particular MAGICC) used in this chapter are not discussed/evaluated in Chapter 9, but should be. I make the corresponding comment to Chapter 9 as well. [Sarah Raper, United Kingdom of Great Britain & Northern Ireland]	Noted. No change requested.
12-191	12	9	22	9	51	This strong argument about why AR4 is an upgrade is very important. I would box it, and perhaps have it elsewhere in the AR5 earlier on. I stress this as the those analyzing the CMIP5 models, for example, may not necessarily see differences in large-scale trends between CMIP3 and CMIP5 models (as has been nicely pointed out in this chapter)...but there's clearly differences in the types of models (e.g. ESMs) as well as the experiemnts and stress on identifying physical processes. [Stephanie Downes, Australia]	Rejected. We agree it is an important point but we hope it is given enough prominence here by mentioning it in the introduction. The point is also brought out in Chapter 1.
12-192	12	9	22		47	The advances mentioned here are all model advances, not understanding advances. The understanding advances are those dealing with Equilibrium Climate Sensitivity, Transient Climate Response and Transient Response to Cumulative Carbon Emission, page 12-7. It would seem that these should be noted here. [Stephen E Schwartz, USA]	Accepted. Now mentioned in the first bullet point.
12-193	12	9	37			Reference Taylor et al. BAMS paper. [Ronald Stouffer, USA]	Accepted. Paper now cited.
12-194	12	9	41	9	47	There are studies that quantify the uncertainty through PDFs and it is important to report these studies. However, you should, already at this point, refer to the fact that such estimates are controversial and face several limitations and shortcomings, as discussed in, e.g.: Section 12.2.2; Chapter 12, page 51, lines 6-8; Section 9.2.3; Section 11.4.7 [Gregor Betz, Germany]	Rejected. We think that it is implied in the fact that we assess them that we will bring out any controversies.
12-195	12	9	42			PDF usually stands for 'probability density function' not 'probability distribution function'. [Nathan Gillett,	Accepted

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						Canada]	
12-196	12	9	49		51	Strike from here; trivial. [Stephen E Schwartz, USA]	Rejected. This has been a topic of much debate and is raised by other reviewers at other points. Hence we keep it in for clarity.
12-197	12	9	53			The statement "The focus of this chapter is on global and continental/ocean basin-scale climate projections" would seem to undermine the title of the chapter " Long-term Climate Change: Projections, Commitments and Irreversibility" that on its face goes well beyond model projections. And well beyond the important conclusions reached on page 12-7, lines 31- 12-8, line 7. [Stephen E Schwartz, USA]	Accepted. Now re-worded.
12-198	12	9	57	9	57	liked should probalby be linked [Irina Mahlstein, Switzerland]	Accepted.
12-199	12	9	57	9	57	replace "liked" by "linked" [Didier Swingedouw, France]	Accepted.
12-200	12	10	13	10	17	In this paragraph, you could include the basic and important fact that the models only provide a lower bound of the uncertainties we face. See also: Chapter 11, page 21, line 13; Chapter 11, page 44, line 4/5; and Chapter 9, page 20, line 1 [Gregor Betz, Germany]	Accepted. Added a sentence that elaborates on the difference between ranges and uncertainty quantification.
12-201	12	10	13	12	48	The main source of uncertainty is ignorance of how successful they might be in prediction. [VINCENT GRAY, NEW ZEALAND]	Noted. No change requested.
12-202	12	10	23	10	34	figure 12.1 may be improved in order to indicate how the RCPs were developed (by choosing RF levels) and then, as indicated, calculating concentrations and emissions. [Jan Fuglestvedt, NORWAY]	Accepted. We have modified the figure.
12-203	12	10	28		34	All the more reason that the forcings need to be explicitly presented. [Stephen E Schwartz, USA]	Noted. No change requested. Comment similar to many ohters by the same reviewer, see other responses.
12-204	12	10	36	11	43	I agree that emissions are a source of uncertainty and with RCP concentration runs this is linked to forcing. But there is also forcing uncertainty in its own right i.e. given that a lot of models now predict concentrations of CO2 and aerosols given emissions of fossil fuels and sulphur dioxide, and this affects the forcing, where do you include forcing uncertainty. Indeed Fig. 12.1 is talking about Earth system models which at start of chapter are defined as including interactive carbon cycle. The way I have described it, forcing uncertainty really should go in with modelling uncertainty but I have often seen modelling uncertainty in the literature described as the “uncertainy in response to a given forcing”. I think it would help to treat forcing uncertainty as an extra source of uncertainty (or explicitly mention it in modelling uncertainty though I think this will cause confusion). [David Sexton, UK]	Accepted. Added a sentence under the third bullet in the list of issues related to scenario uncertainty making the point that the conversion of emissions into concentrations could be considered a source of model uncertainty as well as forcing uncertainty.
12-205	12	10	54	10	54	"possible" seems odd here perhaps use "intended" [Benjamin Sanderson, United States of America]	Rephrased as "No probabilities or likelihoods hae been attached…".
12-206	12	10	54	10	55	It appears necessary to justify the statement that « Each of the[RCP] should be considered plausible [Pierre BRENDER, FRANCE]	This statement is just a short introduction for context, but details belong to WG3 assessment reports.
12-207	12	10	54	10	55	, more thorough considering the number of articles critisizing the IPCC projections on this point. [Pierre BRENDER, FRANCE]	Rejected. Assessment of scenarios is outside the mandate of WG1.
12-208	12	10	54	10	55	For exemple : [Pierre BRENDER, FRANCE]	Rejected. Assessment of scenarios is outside the mandate of WG1.
12-209	12	10	54	10	55	1. Höök M, Sivertsson A, Aleklett K. Validity of the Fossil Fuel Production Outlooks in the IPCC Emission	Rejected. Assessment of scenarios is outside the

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Scenarios. Natural Resources Research. 2010 Feb 18;19(2):63–81. [Pierre BRENDER, FRANCE]	mandate of WG1.
12-210	12	10	54	10	55	2. Patzek TW, Croft GD. A global coal production forecast with multi-Hubbert cycle analysis. Energy. 2010;35(8):3109–22. [Pierre BRENDER, FRANCE]	Rejected. Assessment of scenarios is outside the mandate of WG1.
12-211	12	10	54	10	55	3. Rutledge D. Estimating long-term world coal production with logit and probit transforms. International Journal of Coal Geology. 2011;85(1):23–33. [Pierre BRENDER, FRANCE]	Rejected. Assessment of scenarios is outside the mandate of WG1.
12-212	12	10	54	10	55	4. Mohr S, Evans G. Forecasting coal production until 2100. Fuel. 2009;88(11):2059–67. [Pierre BRENDER, FRANCE]	Rejected. Assessment of scenarios is outside the mandate of WG1.
12-213	12	10	54	10	55	If the RCP 8.5 appears as necessitating more recoverable reserves than what is possible from a full exhaustion of some estimates [Pierre BRENDER, FRANCE]	Rejected. Assessment of scenarios is outside the mandate of WG1.
12-214	12	10	54	10	55	of the fossil fuel (and in particular coal reserve), a short explanation must be at leat direct the reader toward estimates that are regarded as more reliable than [Pierre BRENDER, FRANCE]	Rejected. Assessment of scenarios is outside the mandate of WG1.
12-215	12	10	54	10	55	It should not be too hard to reject Rutledge claims considering that the « Hubbert curve approach » is by nature likely to lead to understimates when applied a the country/large regions sca(e and ignoring some of the hardly explored deposites. [Pierre BRENDER, FRANCE]	Rejected. Assessment of scenarios is outside the mandate of WG1.
12-216	12	10	54	10	55	Moreover the all he missquotes to some extent the World Energy Council Surveys is to some extent missquoted in his figures for additionnal recoverable reserves (table 4 of Rutledge 2011) as the values for that category are not provided for every countries in their report of 2007. [Pierre BRENDER, FRANCE]	Rejected. Assessment of scenarios is outside the mandate of WG1.
12-217	12	10	54	10	55	Indeed, the values are not filled for Russia, Australia, the US and China which should gather a large fraction of the additional recoverable reserves. [Pierre BRENDER, FRANCE]	Rejected. Assessment of scenarios is outside the mandate of WG1.
12-218	12	10	54	10	55	See p24 and further here : http://www.worldenergy.org/documents/ser2007_final_online_version_1.pdf [Pierre BRENDER, FRANCE]	Rejected. Assessment of scenarios is outside the mandate of WG1.
12-219	12	10	55	10	55	though not necessarily equally likely => skip that, if no likelyhood can be attached to the scenarios, how should scenarios be equally likely (or not) ? [Matthias Zahn, United Kingdom]	Eliminated as suggested.
12-220	12	11	9			you could add mention that neither land-use nor aerosol loading are related monotonically with RCP (nor are they intended to) - e.g. both 8.5 and 2.6 have a global increase in land-use while 4.5 and 6.0 have a decrease. This is a feature of the IAM that created the scenario and not a fundamental feature of achieving that RF. [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland]	Accepted. Added a sentence about land-use and aerosol where other aspects of models' uncertainties related to representation of forcings are discussed (third bullet of the list)
12-221	12	11	10	11	26	This section deals with unforced variability. The correct term for this is 'internal variability', not 'natural variability', which includes variability driven by variations in natural forcings. See the glossary definition of 'climate variability'. [Nathan Gillett, Canada]	Replaced throughout the chapter.
12-222	12	11	10	11	26	Please differentiate natural variability in the climate system from deterministic chaotic variation in a climate model due to the introduction of a random perturbation that propagates (e.g., Lorenz). Please clarifiy whether the models experience deterministic chaotic variation rather than natural variability. [Mark Z. Jacobson, U.S.A.]	Internal variability originates from the chaotic behavior of the system. Inserted clarification in the text.
12-223	12	11	13	11	13	Contaminated? Reword [Robert Colman, Australia]	Changed to "be affected by"
12-224	12	11	21	11	22	Suggest refer to Chapter 11, where this is discussed (including a figure). [Robert Colman, Australia]	Done
12-225	12	11	21			Replace 'more regional' with 'smaller'. [Nathan Gillett, Canada]	Done
12-226	12	11	22	11	24	Natural variability can also be estimated from long runs with constant external forcing. [David Rowell, United Kingdom of Great Britain & Northern Ireland]	Added
12-227	12	11	28	11	28	Point made is fine, but making the McWilliams reference the subject is at odds with the style of the rest of the chapter. [Richard G Williams, UK]	Eliminated since the point made is general enough not to require a reference

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12-228	12	11	34	11	34	Say what 'functional form' means. [Robert Colman, Australia]	Replaced by "analytic"
12-229	12	11	36	11	43	As explicited in Chap 9, the model "structural uncertainty exploration" is mostly ad-hoc and certainly not complete wrt our understanding of the physics and numerics. Indeed many models share common components or parameterisations.This point should be made more prominent here. [Eric Guilyardi, France]	Added "at least in part", the point is then made just below.
12-230	12	11	41	11	43	Or all models could have a common error in attempting to represent the a particular processs [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Point added
12-231	12	11	42	11	42	replace “Also, models” with “Also, all current models”? [David Sexton, UK]	Done except we dropped "all"
12-232	12	11	45	12	3	This is discussed in detail in Chapter 11, including Fig 11.4, so I suggest simply refer to that. [Robert Colman, Australia]	Done
12-233	12	11	56	12	3	Rowell (2012) shows clearly that natural variability is the dominant uncertainty source over large regions for precipitation changes at the end of the century. This become appraent by using a finer analysis scale than Hawkins and Sutton, and is also aided by use of a much larger ensemble (demonstrating the limitations of CMIP-sized ensembles for this type of analysis). Rowell, D.P., 2012: Sources of Uncertainty in Future Changes in Local Precipitation. Clim. Dyn., in press, DOI: 10.1007/s00382-011-1210-2 [David Rowell, United Kingdom of Great Britain & Northern Ireland]	Added reference and some more discussion of the role of internal variability.
12-234	12	11	57	12	3	difficult to understand, please rephrase [Irina Mahlstein, Switzerland]	Elaborated upon/rephrased.
12-235	12	12	5			SECTION 12.2.2. This section is welcome. However it discusses a number of approaches that have been taken to generating information on uncertainty, without making any explicit statement about the approach taken in this chapter. I think it is very important that Chapter 12 makes an explicit statement about how it is using the various sources of information to make inferences about future climate. E.g. what is the status of the CMIP5 models vs CMIP3? PPEs vs MMEs? If each conclusion is based on an ad hoc expert judgement melding the various sources of information, that needs to be stated. [Richard Wood, UK]	Accepted. We have added a description of how uncertainty language is arrived at and used in our chapter at the end of section 12.2.2.
12-236	12	12	5			SECTION 12.2.2. I also think it's very important that an explicit statement is made here about how the model evaluation information in Chapter 9 influences the projections in Chapter 12 [Richard Wood, UK]	Noted. When discussing how uncertainty language is arrived at we mention the role of model evaluation.
12-237	12	12	7		17	This para omits another major reason, self selection of forcings to get the right 20th century response for a given model's sensitivity; as shown by Kiehl 07 and others. Overcoming this requires each model to employ a range of forcings consistent with current understanding, as pointed out by Schwartz et al 07 Kiehl, J. T. (2007), Twentieth century climate model response and climate sensitivity, Geophys. Res. Lett., 34, L22710, doi:10.1029/2007GL031383. Schwartz, S. E., Charlson R. J. and Rodhe H. Quantifying climate change — Too rosy a picture? Nature Reports – Climate Change 1, 23-24 (2007). doi:10.1038/climate.2007.22 [Stephen E Schwartz, USA]	The effect of historical forcing included/excluded on the simulation of historical climate are in other chapters. The selection of forcings to match the 20th century appears to be less strong in CMIP5.
12-238	12	12	7		18	Alongside the rather vague (and perhaps even misleading: see Annan and Hargreaves, GRL 2010) discussion cited, it might be appropriate to mention that despite these worries about the possible inadequacy of the ensemble, Annan and Hargreaves GRL 2010, J. Clim 2011, Yokohata et al Climate Dynamics 2011 and Hargreaves et al Climate of the Past 2011 all contain quantitative evidence that the CMIP3 ensemble actually performs rather well (at least at global scale) across a wide range of measures. [James Annan, Japan]	Point inserted and references noted.
12-239	12	12	14	12	14	What is being referred to here as spurious and why? [Robert Colman, Australia]	clarified with an i.e., clause.
12-240	12	12	20	12	20	Could also mention prescribed feedbacks when discussing PPE architecture, Sokolov (2006) sampled a range of sensitivity through prescribed cloud feedback strengths. [Benjamin Sanderson, United States of America]	Rejected. Too technical, space is limited.
12-241	12	12	26	12	26	but to date, the atmospheric perturbations have been the dominant source of uncertainty in large scale response [Benjamin Sanderson, United States of America]	Accepted.
12-242	12	12	27	12	27	“statistical emulators” needs some definition here. How about “a statistical model which relates the model	Added as proposed

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						output to the model parameter values, trained on the ensemble, and used to predict the output for un-sampled combinations of parameter values". [David Sexton, UK]	
12-243	12	12	30	12	30	projection'? Should read 'scenario' I think, as the simulation itself is a projection. [Robert Colman, Australia]	Accepted.
12-244	12	12	30	12	33	I suggest toning this sentence down a little: "…. that are often different from one another …." [David Rowell, United Kingdom of Great Britain & Northern Ireland]	Accepted. Reworded as 'can be'
12-245	12	12	30		33	This final sentence (with which I broadly agree) appears to be contradicted in chapter 9 (p63). Yokohata et al Climate Dynamics 2011 demonstrates a clear separation in behaviour between the two types of ensemble. [James Annan, Japan]	Noted. Reviewer agrees with statement, comment to chapter cannot be addressed here.
12-246	12	12	35	12	38	The assessment that the uncertainty range on temperature projections has not changed at all since the AR4 seems a bit pessimistic to me. A lot of new simulations and studies have been carried out since the AR4 publication deadline. New observationally-constrained estimates of TCR have been published. We have a decade more observations to constrain properties of the physical climate system and carbon cycle (most previous studies used simulations and data to 2000 only). [Nathan Gillett, Canada]	Accepted. Projection uncertainties reassessed for the SOD based on new constraints on TCR and the fact that carbon cycle uncertainties are not included in the majority of RCP simulations.
12-247	12	12	40	12	40	add Sexton et al (2011a) after Piani et al. REFERENCE D. M. H. Sexton and James M. Murphy and M. Collins and Mark J. Webb Multivariate probabilistic projections using imperfect climate models Part I: outline of methodology Clim. Dyn. 2011 10.1007/s00382-011-1208-9 [David Sexton, UK]	Done
12-248	12	12	41	12	46	It is not clear what the two criteria are, I think because the sentence is too long. It would help to write "...treatment, (i) according to the choice..., and (ii) according to the fundamental..." [David Sexton, UK]	Done
12-249	12	12	44	12	44	put a space between "error" and "(Annan…" [Didier Swingedouw, France]	Done
12-250	12	12	44			Please add the following reference "truth to which each model adds an error(Annan and Hargreaves, 2010, WEIGEL ET AL, 2010) Weigel, A. P.; Knutti, R.; Liniger, M. A. & Appenzeller, C. (2010), 'Risks of Model Weighting in Multimodel Climate Projections', Journal of Climate 23(15), 4175-4191.. [Christof Appenzeller, Switzerland]	Accepted.
12-251	12	12	46	12	48	This is an important admission which is not fully reflected in the assessment of uncertainty quantifications. The preponderance of a-priori assumptions weakens the evidence for probabilistic climate forecasts. This fact should be stated more prominently so as to allow readers to rightly interprete the results reported in this chapter. It would also be helpful to add that these a-priori (i.e. arbitrary) assumptions (e.g. Bayesian priors) are not varied systematically and fully, which affects the robustness of the reported findings. [Gregor Betz, Germany]	We have noted the lack of robustness from formal (Bayesian or not) statistical approaches at present, and we have discussed how uncertainty quantification/confidence statements in the chapter are the result of a more comprehensive set of evaluations than just statistical estimation.
12-252	12	12	47	12	48	This bracketed material is heavy going, and strangely structured. If it is important, take it out of brackets and re-express. [Robert Colman, Australia]	Brackets eliminated
12-253	12	12	50	13	30	I didn't get a clear sense of what exactly a 'joint projection of multiple variables' is in this context, or why it is difficult to make such projections. If you can independently redict the changes in the distribution of two variables accurately, then a prediciton of at least the mean change in their sum or product is likely to be reasonably good even without information on their covariability. From the examples given, I got the impression this might relate largely to extermes. If this is the case, then I think it would be good to first discuss uncertainties in the prediction of extremes in individual variables. Then make the case that predicting changes in distribution of joint variables requires a good estimate of their covariability, which might not be available. [Nathan Gillett, Canada]	We have elaborated a little this part, making it clearer that it has to do with the relatively more limited understanding and skill in modeling complex interactions, esp. those that produce extreme behavior.
12-254	12	12	52	12	52	What are 'the key processes' being referred to here, model physics? Clarify and add reference to another part of this report. Why is this relevant to joint variable projection? [Robert Colman, Australia]	Rephrased, and made it clearer that the problem resides mainly in the modeling of complex interactions, and in the statistical modeling of joint variables.

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12-255	12	12	55	12	55	add Sexton et al (2011) to list of references as this did joint projections. [David Sexton, UK]	Reference added.
12-256	12	12		12		Sanderson & Knutti (in prep) is a document on model weighting which could be relevent to this section, the main thesis being that models could be weighted by uniqueness as well as observation bias. I could make a draft available in the near future if it would be of interest. [Benjamin Sanderson, United States of America]	There are many papers on this topic and the current draft does not review this in much detail. If this discussion is extended then the paper (if accepted) can be cited.
12-257	12	12				12.2.2 This section discusses, but does not assess in a useful way. It downplays the intermodel ensemble for what I would consider technical reasons, noting that the perturbed-physics spread is a statistically better-behaved alternative (usefully noting the importance of weighting) - but never says whether either should actually be accepted as a true measure of uncertainty. The third/final paragraph is very hard to follow. There is a literature on this topic outside the climate literature, and there was an IPCC report on uncertainty; there needs to be come connection to both. Perturbed-physics ensembles do not fully sample possibilities because they do not consider alternative equations, only alternative parameters, a crucial problem not mentioned. For example, the resulting pdf is different when one perturbs a different model. Some useful assessment statement seems necessary. [Steven Sherwood, Australia]	Accepted. The revised has matured towards a better assessment and better linkages with the treatment of uncertainty in the chapter.
12-258	12	13	1	13	17	This paragraph also needs to mention the concept of discrepancy (Rougier 2007) i.e. the importance of accounting for structural error not only in the projection variables (as discussed here already) but also the historical variables used to constrain the projections. This is because model imperfections make it harder to discern a good model from a relatively poor one, and so discrepancy protects against over-confident constraints that arise from assuming no structural uncertainty. It would be good to say the challenge set by Rougier 2007 has only started to be addressed e.g. Sexton et al 2011a. Actually the methods that don't assume “constant bias” are also meeting this challenge in a different respect, and Sanderson (in review) also tries to use multimodel to account for systematic errors. Related to all this is the need for multivariate metrics and it would be good to mention those in section 12.2.2 or 12.2.3. [David Sexton, UK]	Done for the most part but we would rather let Ch.9 talk about multivariate metrics.
12-259	12	13	1			At the end of sentence add ", linking summertime temperature and soil moisture to prior winter snowpack (Hall et al. 2008) or linking precipitation change to circulation, moisture and moist static energy budget changes (Neelin et al. 2003, Chou and Neelin 2004, Chou et al. 2006, Chou et al. 2009)." [J. David Neelin, United States]	Done
12-260	12	13	3	13	4	Add a reference to Boe and Terray (2008, GRL) on this point. [Sonia Seneviratne, Switzerland]	Done
12-261	12	13	24			Unclear sentence: "IPCC assessments often show model averages as best estimates, but such averages can underestimate variability, are not plausible model states (Knutti et al., 2010a) and do not necessarily represent the joint best estimate in a multivariate sense" [Ramon de Elia, Canada]	Accpted. Rephrased.
12-262	12	13	32			Section 12.3: It seems to me that there is little information on the non-CO2 forcings in the RCPs : is the role of individual GHGs and the role of aerosols discribed elsewhere ? I think that the time-evolution of the forcing due to aerosols and relatively short-lived gases such as CH4 may be important for the interpretation of the climate change results from the RCPs, and the associated discussion on possible mitigation targets (especially because some aerosols emissions are likely to follow CO2 emissions, not concentrations, potentially creating differences between high and low emission scenarios) [Philippe Marbaix, Belgium]	Further details on the RCPs is given in chapter 1 and the radiative forcing chapter.
12-263	12	13	37			Replace 'long lived greenhouse gas trajectories' with 'trajectories of long lived greenhouse gases and other forcings'. [Nathan Gillett, Canada]	Done.
12-264	12	13	41	13	41	Chapter 8 should be also mentioned for the historical radiative forcings. [Toshihiko Takemura, Japan]	Done
12-265	12	13	46	13	56	What a disappointment! I thought you were going to describe the scenaios but you let me down. [VINCENT GRAY, NEW ZEALAND]	Scenarios are briefly oulined in chapter 1, but a full assessment of the scenarios belong to WG3.
12-266	12	14	3	14	12	1% a year inrease of emissions is a very useful basis as it starts it off with an exaggeration of only 2½ times reality, so you can easily add even more without anybody noticing [VINCENT GRAY, NEW ZEALAND]	Rejected. 1%/yr is a standard simulation to compare model responses, not an economic scnenario.
12-267	12	14	10			Replace 'stylized' with 'idealized'. [Nathan Gillett, Canada]	Accepted. Stylized removed.

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12-268	12	14	28	18	7	At last some information on the new scenarios which is very confusing and lacking in detail [VINCENT GRAY, NEW ZEALAND]	Scenarios are briefly oulined in chapter 1, but a full assessment of the scenarios belong to WG3.
12-269	12	15	2	15	3	If I understand this correctly, this implies that the temperature projections for a given RCP scenario (e.g. 12.4.1.2) don't consider carbon cycle feedback uncertainties, because GHG concentration is fixed by the final RCP dataset. But that is somewhat unfortunate and should at least be stated clearly: a major uncertainty is disregarded in the climate projections based on RCP scenarios. [Gregor Betz, Germany]	The revised text now discusses both emission and concentration driven projections. But emission driven simulations are only availabe for RCP8.5 from CMIP5.
12-270	12	15	28	15	37	The discussion of other scenarios could be slightly expanded to include other and more recent examples of studies of various sectors; e.g. the transport sectors (Skeie et al. Atmos Environ, 43 (39): pp. 6260-6270; Olivie et al.: Modeling the climate impact of road transport, maritime shipping and aviation over the period 1860–2100 with an AOGCM; Atmos. Chem. Phys., 12, 1449–1480, 2012. [Jan Fuglestvedt, NORWAY]	Rejected. This appears to be beyond the scope of WG1.
12-271	12	15	28			Section 12.3.1.5: this chapter very usefully shows that estimates of radiative forcing for the same emissions depend on the model and method, especially when comparing simple models to AOGCMs. Would it be possible to give "advice" on how these different scenarios could be compared, given this difficulty ? [Philippe Marbaix, Belgium]	Accepted. There is a whole section comparing SRES and RCP which is based on simple models and on pulse response emulation.
12-272	12	15	43	15	43	"ECPs" should be explained. [Jan Fuglestvedt, NORWAY]	Accepted - text revised.
12-273	12	15	48	16	3	Rather than saying that the assumed lack of future natural forcings is "very unlikely" to be realistic, a statement that is very easy to misinterpret, it would be more constructive to estimate the additional uncertainty in future climate projections owing to the range of possible future changes in natural forcing. In the absence of other information it seems reasonable to use the spread of natural forcing over the last millenium as a pdf for this. In fact, it would be very useful to elevate to the SPM a statement on the likely range of natural forcings by, say, 2100 against which the antipicated anthropogenic forcing can be directly compared. [Steven Sherwood, Australia]	Taken into account - text revised with cross-reference to Chapter 8's assessment of future solar/volcanic forcing uncertainty in relation to anthropogenic forcing at 2100. We are not in the position to say what will be be in the SPM. In any case the natural forcing over the past millennium is not well known, and 1000yrs are likely to be too short to characterize the variability.
12-274	12	16	10	16	21	I am confused by this. How can a model with interactive carbon cycle obtain "identical" LLGHG concentrations with a model where these are specified as a boundary condition? If this is actually the case then what is the point of running the model with a carbon cycle? I assume what was meant is that an approximately equivalent simulation can be done with both kinds of model where it is likely that they will evolve similar LLGHG trajectories. [Steven Sherwood, Australia]	Taken into account - text revised. [See also response to comment 12-269]
12-275	12	16	23	16	23	not clear, please rephrase [Irina Mahlstein, Switzerland]	Taken into account - text revised.
12-276	12	16	23	16	24	define LLGHG the first time used. Make usage uniform throughout chapter and report. [Robert Colman, Australia]	Accepted - text revised.
12-277	12	16	24	16	24	LLGHGs should be introduced earlier (maybe line 11 same page?) [Irina Mahlstein, Switzerland]	Taken into account - combined with comment 12-276.
12-278	12	17	24		41	The different approaches to aerosols outlined in this para make it esential that the aerosol or SW or total forcing be determined for each model, if need be by Forster Taylor approach, and presented as function of time. Otherwise it is impossible to distinguish the reasons for differences among models: spread in forcing vs spread in response. [Stephen E Schwartz, USA]	Taken into account - combined with comment 12-276.
12-279	12	17	31	17	31	I think it will be more useful to keep the same nomenclature for "MPI-ESM-LR" and MPI-ESM-HR (which is "MPI_ESM_HR" for the moment). [Didier Swingedouw, France]	Accepted - text revised.
12-280	12	17	33	17	33	replace "much large" by "larger". [Didier Swingedouw, France]	Accepted - text revised.
12-281	12	17	34	17	34	Similar or larger? Sentence not clear. [Olivier Boucher, France]	Accepted - text revised.
12-282	12	17	34	17	34	"A similar, larger, fraction" is confusing. A kind of oxymoron. Is it similar or larger? [Didier Swingedouw, France]	Taken into account - combined with comment 12-281.
12-283	12	17	34	17	37	Terminology for the aerosol indirect effects should be aligned to chapter 7 and the glossary. It would be nice to mention if some models if any include the effects of aerosols on ice clouds. [Olivier Boucher, France]	Accepted - text revised. Table footnote added to Table 12.1 documenting the aerosol-ice cloud effect.

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12-284	12	18	9	19	13	In this section or somewhere else it might be helpful to show the time evolution of sulphate aerosol forcing in each of the scenarios in the CMIP5 simulations. This would help readers understand variations in temperature and radiative fluxes. [Nathan Gillett, Canada]	Taken into account - The time evolution of the aerosol forcing is not available for the CMIP5 models. Estimates of the aerosols radiative forcings are only available around year 2000 for the CMIP5 models and in 2100, for RCP8.5 scenarios, for the modles that participate to ACCMIP (see chapter 8). The time evolution of the aerosols forcings computed by IAMs is the only information available, and is now shown in figure 12.2.
12-285	12	18	16	18	16	replace "global warming" with "climate change" [Benjamin Sanderson, United States of America]	Accepted - text revised.
12-286	12	18	18	18	33	I don't understand this explanation. If aerosols decrease in the future, clearly this will augment the radiative forcing, but this doesn't explain the discrepancy between the RCP database and results from the CMIP5 simulations. There must either have been a greater drop in aerosols in the IAMs than in the GCMs, or else the crude treatment of clouds in the IAMs must have caused a systematic overestimate of the total radiative forcing by 2100. The text does not explain which of these happened (or if it was something else). [Steven Sherwood, Australia]	Taken into account - text revised. This paragraphe contained two discussions that were independent. First a comparison between CMIP5 and IAMs extimate of the aerosol radiative forcings. Second how the aerosol radiative forcings impact the all sky and clear sky radiative flux in the CMIP5 models. There is no direct link between the two and the text has been modified to avoid this confusion.
12-287	12	18	30	18	31	This sentence needs clarification: which RCPs have net forcing closer to LW clear-sky ? is it actually "closer", as the LW and 'net' lines do not converge in the "higher" RCPs, unlike for the "lower" RCPs ? In addition, the RCP 4 and 6 exhibit a substantially different evolution of the LW/net difference, can the average cloud fraction effect explain this alone ? (are there e.g. substantial differences regarding aerosols between these RCPs?) [Philippe Marbaix, Belgium]	Taken into account - combined with comment 12-287.
12-288	12	18	56			SECTION 12.4 and 12.5.5: It's important to be clear what this section is about. The title of the section is clear enough: abrupt change and irreversibility. However what it contains is a discussion of a number of vulnerable elements of the climate system. In some cases (e.g. AMOC, ice sheets, Arctic sea ice) there is a potential vulnerability to abrupt or irreversible change, but in others (particularly megadroughts and monsoons) it's more a case of an important system that might respond to climate change but not in a particularly abrupt or irreversibe way. So e.g. for AMOC the discussion is now split between two parts of the chapter (12.4.7 and 12.5.5), and currently inconsistent between the two parts. The sea ice text makes it clear what is discussed where, and there is no duplication, while monsoons are discussed in 12.5.5 but not in 12.4.4. It's not easy to find a tidy solution for this, but I think it would be useful and possible to get a bit more consistency about what aspects of the different climate elements are discussed in 12.4 and what in 12.5.5. [Richard Wood, UK]	Accepted. AMOC moved to 12.5.5. Table added to summarize the results. The section discusses also elements that are often referred to as abrupt but in fact they are not. We believe that will help to clarify what is indeed abrupt and what is not.
12-289	12	18				12.3.3 This section should specify that the radiative forcing being used is the unadjusted forcing. This is distinguished in chapters 7 and 8 from the forcing including rapid adjustments. [Steven Sherwood, Australia]	The revised text uses adjusted forcing consistent with the ohter chapters.
12-290	12	19	11	19	11	Also reference Power et al, 2011, Consensus on 21st century rainfall projections in climate models more widespread than previously thought. Journal of Climate, [Robert Colman, Australia]	Stippling and hatching is revised and described in box 12.1.
12-291	12	19	24	19	25	It is obvious that global temperature rise depends on GHG forcing - I don't think this is needed. Alternatively, perhaps the meaning is more subtle, in which case this should be clarifiied. [Nathan Gillett, Canada]	Taken into account. However, a mention of differences in the magnitudes of the projected global warming as a function of radiative forcing is still of interest to the reader.
12-292	12	19	35	19	37	It might be worthwhile mentioning the inconsistancy at the year 2100, with less simulations considered in the MME. In particular, the projected warming is somewhat reduced, when less simulations are considered. This is particularly the case for RCP8.5. [Wilhelm May, Denmark]	Taken into account. More model simulations are considered for the SOD and we have added a sentence to the caption noting the different numbers of models.
12-293	12	19	47	19	47	ice sheet also contributes': add reference and specify physical process. [Robert Colman, Australia]	Accepted. Reference added and the processes mentioned.

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12-294	12	19	47	19	47	Consider local snow/ice albedo feedbacks in arctic/antarctic asymettry [Benjamin Sanderson, United States of America]	Accepted. Processes responsible for the Arctic/Antarctic asymmetry mentioned
12-295	12	19	54	20	1	Table 12.2.: It is somewhat counterintuitive to see RCP2.6 results in larger warming than RCP4.5 and RCP6 in the 2016-2035 period, but I do not have the excact concentration pathways. [Matthias Zahn, United Kingdom]	Noted. This is real. RCP2.6 is actually warmer than RCP4.5 and RCP6 in the near term and has larger radiative forcing (Annex II Table A.II.6.12). We have added a sentence to the text.
12-296	12	19		20		Table 12.2: In the light of the fact that the selection of simulations with different climate models might differ for different scenarios and / or periods, I find it problematic to give the minimum and maximum range of the projected warming in the table. This may make the interpretation of the respective results difficult. As for the global mean change for the period 2016-2035, for instance, the ranges are 0.5-1.2 K for RCP2.6 and RCP8.5 but 0.4-1.0 K for RCP4.5 and RCP6.0. [Wilhelm May, Denmark]	Noted. This is meant to show the entire range of the projected temperature change as an indication of 'uncertainty', in addition to the standard deviation, which is also shown
12-297	12	20	5	20	6	As the precipitation changes in Fig. 12.5 are given in mm/day, the statement that the precipitation sensitivity is less that 3%/K for most models is not supported by a figure. Even less so, as the reader doesn't know anything about the magnitude of global mean precipitation for the control period. It might be better to present the relative changes in the figure and actually draw the slopes. [Wilhelm May, Denmark]	Noted. The relative precipitation changes are shown in a figure.
12-298	12	20	9	20	9	Allen and Ingram citation should relate to the radiative argument. [Benjamin Sanderson, United States of America]	Accepted.
12-299	12	20	16	20	16	Mention observational constraints on precipitation sensitivity? Alder et al (2008), Trentberth&Shea(2005) [Benjamin Sanderson, United States of America]	Taken into account.
12-300	12	20	24			Section 12.4.1.2: The authors are overconfident in assigning the likelihoods and don't take the limitations of methods for uncertainty quantification into account. I suggest that the authors simply describe the level of confidence without assigning likelihoods to temperature projections. I detail below. [Gregor Betz, Germany]	Rejected. Uncertainties in RCPs are determined by uncertainties in TCR, which is very well understood and constrained by a variety of observations.
12-301	12	20	24			Section 12.4.1.2: The assignment of probabilities to temperature projections rests heavily on PDFs of climate sensitivity. I think these PDFs are unjustified in the first place (see below), so probabilities should not be assigned to temperature projections. [Gregor Betz, Germany]	Rejected. No PDF of climate sensitivity and TCR is specified but likely ranges. Those translate directly into RCP uncertainties. Likely ranges for ECS/TCR are well accepted in the community.
12-302	12	20	24			Section 12.4.1.2: But even if we could assign probabilities to ECS, there is an additional problem: ECS does not capture all feedbacks in the Earth system that are relevant for temperature change. This is stressed in Chapter 12, e.g. p. 52, l. 41/42 or p. 55, l. 42/43! [Gregor Betz, Germany]	The dominant source of uncertainty for RCP projections to 2100 is TCR, and that is even better constrained that ECS. Long term feedback are unlikely to be important up to 2100. Note that a likely range is rather conservative and still allows up to 33% outside the range.
12-303	12	20	24			Section 12.4.1.2: The assignment of probabilities rests heavily on a single (meta-)study by Rogelj et al. (2011a). That study discusses an ensemble of PDFs. But figure 12.6 merely reports the average (or "representative") PDF of the ensemble. By doing so, the uncertainty is inappropriately reduced. [Gregor Betz, Germany]	Rejected. The assessment rests mainly on the assessment of TCR and of projections for SRES, which were supported by many studies. See AR4 section 10.5.4, Fig. 10.29
12-304	12	20	24			Section 12.4.1.2: It seems to be somewhat incorrect to say that ECS estimates have not changed significantly since AR4. As figure 12.45 shows, we have more extreme ECS values now. That's probably because more feedbacks are included in the simulations that determine ECS (specifically: no slab ocean, see ch. 12, p. 50, l. 34-36). Given more elevated values for ECS and even higher sensitivities of the Earth System as a whole (see above), it seems to follow that we are now less confident that temperature change will not be extreme. It has become more difficult to dismiss extreme projections. The IPCC should say so and modify one of its key statements accordingly. [Gregor Betz, Germany]	Rejected. The upper bound of ECS is in fact better constrained. See Box 10.2 for a summary. Earth System feedbacks are unlikely to be a dominant factor before 2100.
12-305	12	20	24			Section 12.4.1.2: For reasons of report-wide consistency, the authors should refrain from assigning likelihoods and simply report their confidence, as the authors of Chapter 13 do in an analogous situation. In Chapter 13, page 4, lines 47-56, the authors have only medium confidence in obtained GMSL ranges because a) there are relevant processes which are not fully represented in the models that are used to derive the ranges, and b)	Rejected. Temperature is much better understood than sea level. Confidence for the latter is lower because dynamic contributions from ice sheets are difficult to model.

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						some models give significantly higher values outside this range. Now, the situation vis-à-vis temperature projections seems to be analogous: a') some models yield way more extreme temperature change (think of climateprediction.net) and b') there are a couple of processes which are not taken into account when deriving the range (e.g. carbon cycle feedbacks, further feedbacks that affect earth sensitivity, etc.). Hence, medium confidence should be assigned to the narrow range of temperature projections. [Gregor Betz, Germany]	
12-306	12	20	26	20	40	I found this discussion, and fig 12.6 a bit confusing. Can the figure get more explanation? What are the red crosses? CMIP5 models or Gregory or Good? Why is the bottom panel so similar – wouldn't we expect a wider spread with carbon cycle? Can you clarify what is a placeholder using older results and what might be replaced when more CMIP5 results are available? [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland]	Section rewritten completely. Figure now shows raw CMIP5 results. Unfortunately only few methods have produced uncertainties for RCPs, so the assessment is difficult. The figure will include more results if they become available.
12-307	12	20	26		28	The uncertainties determined this way will certainly underestimate true uncertainty because of selectivity of forcings by modeling groups. It is essential to systematically explore uncertainty range in forcing with each of the models. [Stephen E Schwartz, USA]	Rejected. Ranges of CMIP5 are not directly interpreted as uncertainties. The revised makes that more explicit.
12-308	12	20	26			Replace 'variations in natural internal variability' with 'internal variability'. 'Natural variability' includes the response to natural external forcings. [Nathan Gillett, Canada]	Accepted.
12-309	12	20	27	20	29	How is the standard deviation interpretable in terms of the confidence in future projections? For example if we assume that the models are interchangeable with the obs, then the 5-95% range across the ensemble would be the same as the 5-95% coinfidence range on projections. This needs to be discussed, and the Good Practice Guidance Paper on the subject should be consulted. By itself the one standard deviation range on the models tells us only about agreement between the models, but nothing about actual expected future climate change. It is much better to discuss the assumptions involved in relating the model projections to the real world here, rather than just leaving the readers to do this for themselves. Moreover, statements on the warming in the SPM will likely relate to the real world, so somewhere the chapter needs to make this link. [Nathan Gillett, Canada]	The standard deviation is just a standard deviation but not interpretable in any way because of the opportunistic nature of the ensemble. The revised text makes that explict. Indeed that section is exactly trying to make that uncertainty assessment.
12-310	12	20	31	20	31	Plot suggests confusion between Good et al and Gregory et al. [Benjamin Sanderson, United States of America]	Accepted. Figure now shows raw CMIP5 results.
12-311	12	20	31	20	31	Points on plot should make clear which are original results and which are estimated. [Benjamin Sanderson, United States of America]	Accepted. Figure now shows raw CMIP5 results.
12-312	12	20	32			Seems to be first mention of MAGIC; seems to require an introduction for readers not familiar with that model; and a reference. [Stephen E Schwartz, USA]	Accepted.
12-313	12	21	11	21	11	Suggest add reference to Rotstayn LD, Cai W, Dix MR, Farquhar GD, Feng Y, Ginoux P, Herzog M, Ito A, Penner JE, Roderick ML, Wang M. 2007. Have Australian rainfall and cloudiness increased due to the remote effects of Asian anthropogenic aerosols? Journal of Geophysical Research. 112: D09202. doi:10.1029/2006JD007712. [Robert Colman, Australia]	Rejected. This is a nice paper about observed patterns of precipitation trends over Australia and the role of Asian aerosol emissions. It is not a paper about pattern scaling and its weaknesses in the presence of aerosols forcings, which is what we need to cite here. The paper may be relevant for attribution results in Chapter 10.
12-314	12	21	11			It is important to acknowledge that pattern scaling is by no means perfect, it carries an uncertainty with it, which is often neglected but in some cases has also been estimated from the coupled model runs and additionally included in the projections (e.g. Harris et al 2006, 2010). [David Sexton, UK]	Accepted. We added a sentence to highlight this further in the paragraph that starts by "There are basic limitations…", and reiterated the reference to Harris et al. 2006.
12-315	12	21	18			Replace end of sentence "and – to a lesser degree especially when aerosols are involved (Shiogama et al., 2010) – precipitation change." By new sentence "The precipitation pattern was shown to scale linearly with global average temperature to a sufficient accuracy in CMIP3 models (Neelin et al. 2006) for this to be useful for the hydrological cycle (the term per-T climate sensitivity was used in this context). Shiogama et al. (2010) find similar results with the caution that in the early stages of warming aerosols modify the pattern." [There is also a reference by Yi Ming of GFDL regarding aerosol modification of the pattern] [J. David Neelin, United	Accepted. Text modified.

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						States]	
12-316	12	21	37	21	38	I would add "calibrated against the detailed climate models" to make it abundantly clear [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Added (changing "detailed" into "fully coupled")
12-317	12	21	52	21	52	Replace "Ruosteenoja and Ruokkoilanen (2007)" with "Ruosteenoja et al. (2007)" and "Raisanen et al. (2006)" with "Räisänen and Ruokolainen (2006)" [Jouni Räisänen, Finland]	Corrected
12-318	12	21	52			"Ruosteenoja and Ruokkoilanen (2007), Raisanen et al. (2006)" should probably be "Ruosteenoja et al. (2007), Raisanen and Ruokolainen (2006)," [Kirsti Jylhä, Finland]	Corrected
12-319	12	21	53	21	55	The study by May (2008a) looks also at the patterns linked to a warming of 2 K with respect to pre-industrial times and, hence, might be a suitable reference here. [Wilhelm May, Denmark]	Accepted. Cited now.
12-320	12	22	5	22	9	Shiogama et al. (2010) cited here is the multi-model analysis paper. The following paper is the MIROC3 analysis paper about precipitation pattern scaling. Shiogama, H., S. Emori, K. Takahashi, T. Nagashima, T. Ogura, T. Nozawa, and T. Takemura (2010), Emission Scenario Dependency of Precipitation on Global Warming in the MIROC3.2 Model, Journal of Climate, 23(9), 2404-2417. [Hideo Shiogama, Japan]	Corrected
12-321	12	22	9	22	12	The study by May (2008a) actually considers a.o. the "non-linear component" or error, respectively, when patterns with very different suplhate aerosol loads are scaled with the chnages in the global mean temperature, giving marked deviations between the actual and the scaled change patterns. [Wilhelm May, Denmark]	Reference added.
12-322	12	22	22	22	22	The recent study by May (2012) actually illustrates the limitation of obtaining regional changes in near-surface climate associated with a particular scenario by means of scaling the regional changes obtained from a widely used standard scenario with the ratio of the changes in the global mean temperature projected by these two scenarios. (May, W., 2012: Assessing the strength of regional changes in near-surface climate associated with a global warming of 2◦C. Climatic Change, 110, 619-644.) [Wilhelm May, Denmark]	Added citation and mention of the relevant results
12-323	12	22	27			Gillett et al. (2011) could also be cited here: They showed that in a simulation in which emissions cease, regional temperatures and precipitation patterns exhibit ongoing changes, even though global mean temperature remains almost constant. This is a case where pattern scaling would work less well, since it would project no regional changes in climate while global mean temperature is constant. [Nathan Gillett, Canada]	Added citation and mention of the relevant points.
12-324	12	22	29	22	33	Ishizaki et al. examiened the validity of temperature patten scaling on RCPs. Ishizaki Y, Shiogama H, and coauthors (2011) Temperature scaling pattern dependence on representative concentration pathway emission scenarios. Climatic Change, revised. [Hideo Shiogama, Japan]	Reference added.
12-325	12	22	37		38	The language: "we show geographical patterns (Figure 12.8) of warming and precipitation change and indicate measures of their variability across models and across RCPs" seems inappropriate for an assessment. This entire set of findings seems more like primary literature material that would then be assessed here. Nonetheless these seem important findings. [Stephen E Schwartz, USA]	Rejected. We consider this safe because of the straightforward nature of the analysis backed up by ample peer-reviewed literature material.
12-326	12	23	15	23	28	I selfishly suggest citing Allen and Sherwood (2010) showing that the land-ocean contrast is also sensitive to forcing by aborbing aerosol. [Steven Sherwood, Australia]	Accepted - text revised
12-327	12	23	18	23	28	Boer, G. J., The ratio of land to ocean temperature change under global warming, Clim. Dyn., DOI 10.1007/s00382-011-1112-3, 2011 should be cited here too. He showed that enhanced land warming results partly from an anomalous flux of heat from the ocean areas to the land. [Nathan Gillett, Canada]	Accepted - text revised
12-328	12	23	21	23	24	"may seem intuitively relevant / but is due to…" : this may be pushing the argument a bit too far - unless you have a proof that transient heat absorption by oceans is completely irrelevant. I think that the reference Lambert and Chiang 2007 is not sufficiently reflected in the current text - the purpose of their paper is not to simply say that the ratio is remarkably constant, but to discuss its origin - and they conclude that the ocean heat uptake is playing a role (based on observations). More may probably also be	Taken into account - Joshi, Lambert and Webb, 2012, submitted to Climate Dynamics addressed the relevance of ocean heat uptake to land-sea warming contrast and find that the surface heat flux anomalies associated with ocean heat uptake are well mixed

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						obtained from the ref. Lambert et al. 2011 - please check that the balance of views on this evolving topic is fully reflected. Detail: some readers will not know the meaning of "Bowen ratio", it would be easier for them to refer to latent and sensible heat fluxes. [Philippe Marbaix, Belgium]	spatially and therefore impact both land and ocean
12-329	12	23	24			Another reference which quantifies the role of soil moisture on future temperature change is Clark et al (2010) (Clark, R.T, Murphy, J. M and Brown, S. J. 2010 Do global warming targets limit heatwave risk? Geophys. Res. Lett. 37 L17703 DOI: 10.1029/2010GL043898 [Robin Clark, UK]	Accepted - text revised
12-330	12	23	40	23	41	Was stated on p19, line 47 that Antarctic ice sheet also contributes to lack of Antarctic amplification. Make consistent, specify principal process and add ref. [Robert Colman, Australia]	Accepted - text revised
12-331	12	23	40	23	41	In this phrase you seem to imply that it is clear why there is no southern polar amplification. Yet it seems that in the literature there is still a debate why this part of the world is not warming. Please add that the processes are not understood yet. [Irina Mahlstein, Switzerland]	Accepted - text revised to change word from "attribute" to "associate" which implies less certain understanding.
12-332	12	23	47	23	47	May add "Hu et al. 2004" between "Holland and Bitz, 2003; Winton, 2006b" [Zeng-Zhen HU, USA]	Rejected - suggested article makes no reference to the statement being made
12-333	12	23	47	23	47	Hu, Z.-Z., S. I. Kuzmina, L. Bengtsson, and D. M. Holland, 2004: Arctic sea-ice change and its connection with Arctic climate change in CMIP2 simulations. J. Geophys. Res., 109 (D10), D10106, doi: 10.1029/2003JD004454. [Zeng-Zhen HU, USA]	Rejected - suggested article makes no reference to the statement being made
12-334	12	23	56	23	56	it is not clear what this 8K warming refers to. What is the reference time period, and what RCP? [Irina Mahlstein, Switzerland]	Accepted - text revised
12-335	12	23				Subsection 12.4.3.1 : the title gives title information on the scope of the section - we suggest making it more precise, such as "key features of the surface warming pattern", or "global/general patterns of…" - as this section is not all about these patterns but merely the most important general aspects. There are several important topics in this subsection, maybe these should be highlighted by reaching the level of sub-sub-titles or other changes in the organisation of the section/chapter. [Philippe Marbaix, Belgium]	Taken into account - title changed to Patterns of surface warming: land-sea contrast, polar amplification and SSTs
12-336	12	24	1	24	5	This comparison of historical simulated trends with observations is more an issue for chapters 9 and 10. [Nathan Gillett, Canada]	Taken into account - text has been revised to refer back to the assessments of the evaluation and detection/attribution chapters
12-337	12	24	8	24	13	Those 2 sentences are very difficult to read - please rewrite (link with previous sentence, avoid using a so long subject) [Philippe Marbaix, Belgium]	Accepted - text revised
12-338	12	24	11	23	13	Simplify sentence. [Benjamin Sanderson, United States of America]	Accepted - text revised
12-339	12	24	25	24	25	on the magnitude => in magnitude [Matthias Zahn, United Kingdom]	Accepted - text revised
12-340	12	24	33	24	33	or that seasonal sea-ice variations are subject to the same feedbacks as those which drive long-term change [Benjamin Sanderson, United States of America]	Rejected - this comment appears to be missing some words at the beginning. It also does not correspond to the text on page 24, so the change requested cannot be made.
12-341	12	24	39	24	39	minimums should be minima [Irina Mahlstein, Switzerland]	Accepted - text revised.
12-342	12	24	39	24	40	I'm not sure how a shift in the ACC, if it were to occur, would cause cooling in the S. Ocean. Or is this referring to wind-driven changes in the strength of the Ferrell Cell? [Nathan Gillett, Canada]	Accepted - text revised to remove this statement
12-343	12	25	1			Replace 'zonal temperature' with 'Temperature of the free atmosphere'. 'Zonal temperature' presumabley means 'Zonal mean temperature' but this could refer to zonal mean temperature at the surface, as in Fig 12.8, or even zonal mean ocean temperature. [Nathan Gillett, Canada]	Accepted - title changed to Zonal Average Atmospheric Temperature
12-344	12	25	7	25	7	this explanation is over-simplistic, perhaps just refer beck to the previous seciton. [Benjamin Sanderson, United States of America]	Accepted - revised text refers to the previous section

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12-345	12	25	8	25	8	should this be RCP6? [Benjamin Sanderson, United States of America]	Taken into account - this statement was re-checked using the more complete CMIP5 ensemble now available. The statement has been corrected to state that the pattern resembles the A1B pattern (shown in the AR4), but with somewhat larger temperature changes.
12-346	12	25	13	25	21	Is the difference in pattern attributabl to RCP2.6 being closer to equilibrium? [Benjamin Sanderson, United States of America]	Taken into account - text revised using the now more extrensive CMIP5 ensemble. Similarities across all 3 RCPs appear in the troposphere, with differences in the polar stratosphere. Closeness to equlibrium thus does not appear to be a factor
12-347	12	25	13	25	28	If this refers to Fig. 12.11: I do not see a warming maximum in the stratosphere in RCP2.6 and I think, RCP2.6 and RCP4.5 resemble each other, and RCP8.5 deviates. [Matthias Zahn, United Kingdom]	Taken into account - text revised using the now more extrensive CMIP5 ensemble. Similarities across all 3 RCPs appear in the troposphere, with differences in the polar stratosphere, which are discussed in the text.
12-348	12	25	15	25	17	As I read Fig. 12.11, the spatial structure of the changes for RCP2.6 is not very different from the changes for RCP4.5. Therefore, I would say there is a kind of transition from RCP2.6 over RCP4.5 to RCP8.5 rather than a distinct structure for RCP2.6. Moreover, the corresponding changes in the horizontal wind component (Fig. 12.18) are not suport such a distinct structure of the change for RCP2.6. [Wilhelm May, Denmark]	Taken into account - text revised using the now more extrensive CMIP5 ensemble. Similarities across all 3 RCPs appear in the troposphere, with similarities and differences in the polar stratosphere, which are discussed in the text.
12-349	12	25	26	25	28	The wording here seems awkward and misleading. I don't think it's any harder to assess agreement between models and obs in the tropical upper troposphere than elsewhere. The issue is that the simulated trends tend to be larger than the observed trends in this region, and there is debate about whether or not these differences are significant or not after accounting for observational uncertainty. The text should say this. [Nathan Gillett, Canada]	Text revised - the statement was rewritten to make it more consistent with wording in Chapters 9 and 10.
12-350	12	25	37			Section 12.4.33 Temperature extremes. I'm very surprised nothing is written here about the relationship between global mean changes and changes in the regional temperature extremes. Clark et al (2010) found GCM simulations giving moderate globally averaged increases of 2degC to be accompanied by regional increases in heatwaves far in excess of 2 degrees. Clark et al also explicitly quantified the uncertainty (in regional tempearture extremes) for global responses of 2,3,4 degrees and found the uncertainty was of several degrees. Furthermore, the range (in the changes of the extremes) was found to overlap signficantly for the differing global responses, especially over Europe, East Asia and parts of North America. [Robin Clark, UK]	Taken into account - the aspect is highlighted in the section together with the comment 12-357 below.
12-351	12	25	39	25	40	what does "several types of exremes in temperature" specifically refer to?(Qiyong Liu, China CDC) [Qiyong Liu, China]	Accepted - text has been revised by removing the likelihood statement on an ambigious description
12-352	12	25	50	25	50	Sillmann and Roeckner 2008 have also done a model evaluation of the ECHAM5/MPI-OM model with the HadEX data set (Alexander et al. 2006) and could be cited as another example of the application of HadEX [Jana Sillmann, Canada]	Accepted - reference added
12-353	12	25	54	26	3	Further supporting evidence for changes in temperature extremes by the end of the 21st Century has been found for HadGEM2-AO under the emissions scenarios described by Johns et al. (2011). This is by Caesar and Lowe, to be submitted. [John Caesar, United Kingdom of Great Britain & Northern Ireland]	Taken into account - reference added
12-354	12	25	54			This conclusion (and following ones) are based entirely on model calculations. The conclusions seem pretty obvious and would seem to follow from a shifting of whatever pdf governs various events to warmer temperature central value. Still I question whether such a conclusion that is based entirely on model evidence reaches the level of confidence of "It is virtually certain". [Stephen E Schwartz, USA]	Taken into account - this uncertainty assessment is based on multiple-lines of evidence and is consistent with previous assessments (the original text mistakenly said it was an increase in confidence).
12-355	12	25	54			Replace "less" by "fewer" (gram). [Stephen E Schwartz, USA]	Accepted - text revised

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12-356	12	25	55			"our"; I would suggest remove if the authors are really confident in the conclusion; otherwise be specific and replace "our" by "the authors'" if that is what they mean. [Stephen E Schwartz, USA]	Accepted - this statement has been rewritten
12-357	12	26	1	26	2	Clark et al 2010 indicate that when extremes within a region are considered, constraining the level of average global warming (and thus to first order anthropogenic forcing) has little or no impact on reducing the uncertianty in the changes in extreme temperatures. Specifically constraining the global response to +-0.5 degC still results in local uncertainty of +-3 DegC. So the point is that extreme temperature uncertainty arising from forcing uncertainty is small compared to modelling uncertainty when considering local changes in extremes. Do global warming targets limit heatwave risk? , Clark, R. T., J. M. Murphy, and S. J. Brown (2010), Geophys. Res. Lett. , 37 , L17703, doi:10.1029/2010GL043898 [Simon Brown, UK]	Taken into account - this comment is valid particularly for the changes in magnitude extremes, whereas the forcing uncertainty is more important for counts of warm and cold days. This is now highlighted in the text.
12-358	12	26	3	26	3	I would add to that sentence the following "… the greatest increase in warm night/days is projected to occur in summer, whereas a similar decrease in winter and summer is projected for cold nights/days." [Jana Sillmann, Canada]	Taken into account - this figure has been replaced with other indices and their changes are now more fully described.
12-359	12	26	5	26	5	on average => on global average [Matthias Zahn, United Kingdom]	Rejected - statement was not referring to global average.
12-360	12	26	9	26	9	"will persist in a warmer climate", this will lead to mis-intepretation. Would something occur only once in 100 years still be counted as persist? [Xuebin Zhang, Canada]	Taken into account - text revised to 'cold extremes will continue to occur in a warming climate although their frequency declines'.
12-361	12	26	9			The phrase "cold extremes will persist in a warming climate" is not very informative --all distributions have extremes. Perhaps these extremes should be qualified (e.g. "extremes as we know today will persist"). [Ramon de Elia, Canada]	Taken into account - text revised to 'cold extremes will continue to occur in a warming climate although their frequency declines'.
12-362	12	26	14	26	14	"magnitude of temeprature extremes", this is confusing. In particular, there could be different different ways to define temperature extremes. [Xuebin Zhang, Canada]	Taken into account - text has been changed to 'absolute value'
12-363	12	26	20	26	20	Fischer et al. (2007) is not about projected changes. [Sonia Seneviratne, Switzerland]	Accepted - reference removed
12-364	12	26	21	26	22	Suggested reformulation: "probability of occurrence of a Russian heatwave at least as severe as the one in 2010" [Jouni Räisänen, Finland]	Accepted - text revised
12-365	12	26	21	26	23	I am confused here. Do you mean the probability for the occurrence of 2010 Russian heatwave has increased by a factor of 5 to 10 due to (past) increase in mean tempertaure? Or do you mean the probability for the type of Russian heatwave WILL increase by a fcator of 5 to 10 in the future? In the first instance, that should be discussed in Chapter 10. But if you meant for the future, then there is a problem: if the Russian heatwave was NOT due to anthropogenic forcing as claimed by some studies, can you expect the risk of that kind of heatwave to increase in the future? [Xuebin Zhang, Canada]	Taken into account - text revised in conjunction with previous comment (12-365)
12-366	12	26	34	26	35	Refer here to IPCC SREX (2012, Chapter 3) which addresses this aspect in detail (Section 3.1.4). [Sonia Seneviratne, Switzerland]	Accepted - text revised
12-367	12	26	40	26	40	Clarke et al 2010 demonstrate that the greatest contribution to modelling uncertainty in future temperature extremes arises through evaporative cooling mechanisms. The drying out of the soil allows an enhanced temperature response, however, this enhancement will apply only to a restricted range of temperature percentiles depending on the soil moisture climatology of the control simulation. The question is whether these mid-latitude continental areas with the enhanced warming due to drying are arising because we have the soil moisture climatology wrong or because the models are getting the soil drying right. I know which one I'd put money on. Do global warming targets limit heatwave risk? , Clark, R. T., J. M. Murphy, and S. J. Brown (2010), Geophys. Res. Lett. , 37 , L17703, doi:10.1029/2010GL043898 [Simon Brown, UK]	Taken into account - text revised to reflect this uncertainty and references made to the evaluation chapter
12-368	12	26	40			An addition mechanism for coastal regions, considered by Watterson et al. (2008), is a relative change in the temperature of air advected from a continent and from the ocean. They show that for southern Australia, summer heat waves driven by the 'hot northerlies' from the warmer interior can be more intense, in	Accepted - additional mechanism and reference added

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						comparison to the mean, which is moderated by the milder change of the Southern Ocean. Ref: Watterson I. G, J. L. McGregor, and K. C. Nguyen (2008) Changes in extreme temperatures of Australasian summer simulated by CCAM, and the roles of winds and land-sea contrasts, Aust. Meteorol.. Mag., 57, 195-212. [Ian Watterson, Australia]	
12-369	12	26	45	26	45	"cold winter extremes over Europe are driven by atmospheric blocking" sould be changed to "are driven in part by…" because atmospheric blocking is not the only driving factor to cause extreme cold temperatures [Jana Sillmann, Canada]	Accepted - text revised
12-370	12	26	46	26	46	Although this cannot be easily validated with models…. [Benjamin Sanderson, United States of America]	Taken into account - we have revised the statement to be less definitive
12-371	12	26	46	26	47	sentence should be rephrased to "changes in atmospheric blocking patterns in the future can lead to changes in the occurrence of cold temperature extremes regarding their frequency and spatial distribution, but cold extremes can still be expected even as global temperature increases. [Jana Sillmann, Canada]	Taken into account - text revised in conjunction with previous comments on cold extremes
12-372	12	26	49	26	51	Suggested reformulation: "Heat stress, defined by the combined effect of temperature and humidity, is expected to increase along with warming temperatures, which strongly dominate over local decreases in relative humidity due to soil drying". [Jouni Räisänen, Finland]	Accepted - text revised
12-373	12	26	49	26	56	This paragraph is highly related to WGII (chapter 11, Health). A link with this chapter must appear here [Eric Martin, France]	Rejected - cannot refer to WGII prior to its publication
12-374	12	26	49			"Enhanced morbidity and mortality during heat waves relates not only to temperature but also humidity." inappropriate for WG1 [Stephen E Schwartz, USA]	Accepted - text revised to remove quantification
12-375	12	26	54	26	54	replace "and humidity" with "which thus" [Benjamin Sanderson, United States of America]	Accepted - text revised
12-376	12	27	1	27	1	"rare temeprature extremes". What is the definition here? There is a need to have a cross chapter agreement on the terminology and definition of extremes. [Xuebin Zhang, Canada]	Taken into account - rare events in this context are long period return values. Chapter 10 assessed 20 year return values from Kharin et al 2012 and Chapter 12 uses the projections from this paper.
12-377	12	27	6	27	9	Please consider modifying the sentence to: Comparison to the changes in mean temperature shown in figure 12.15 reveals that both rare high and low temperatures are projected to experience greater increases than the mean with the largest changes in the rare low temperatures at high latitudes. [Tsz-cheung Lee, Hong Kong]	Accepted - this sentence has been rewritten
12-378	12	27	8	27	8	Too much generalization. Should be: "greater increases than the mean in most regions" or "greater average increases than the mean" [Jouni Räisänen, Finland]	Accepted - text revised to include 'in many regions'
12-379	12	27	12	27	12	Please add a reference of the SREX. [Tsz-cheung Lee, Hong Kong]	Accepted - reference added
12-380	12	27	15	27	16	I find the statement on the changes in extremes from CMIP5 too general and think it should be more specific. Either referring to the kind of extremes presented in Fig. 12.12 or to the kind of extremes covered in the preceding text. [Wilhelm May, Denmark]	Taken into account - text has been revised to refer specifically to the return values figure
12-381	12	27	24	27	29	One last plug for Clark et al 2010, It would seem appropriate to me to mention here that constraining global response does not constrain local response for extreme hot temperatures. Do global warming targets limit heatwave risk? Clark, R. T., J. M. Murphy, and S. J. Brown (2010), Geophys. Res. Lett. , 37 , L17703, doi:10.1029/2010GL043898 [Simon Brown, UK]	Taken into account - together with comment 12-357. Reference has been added to the discussion of uncertainties.
12-382	12	27	29	27	29	See also Figs. 3.5 and 3.7 of the IPCC SREX (2012). [Sonia Seneviratne, Switzerland]	Taken into account - reference to SREX Figure 3-5 added to the return values paragraph (3-7 is for precipitation)
12-383	12	27	34			Table 12.3 notes that there are likely to be 'numerous studies on this topic in coming years' in the cell on 'cold spells'. This may be the case, but they are only citable here if they are accepted for publication by July this year. [Nathan Gillett, Canada]	Accepted - table has now been removed

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12-384	12	27	44			Key addl refs: Levitus GRL 05; Hansen ACP, 11. [Stephen E Schwartz, USA]	Noted. The two suggested references are more focused on the 20th century period. They are more relevant for chapt. 9, 10 and 13 and are already cited there.
12-385	12	28	6			Better: "The top of atmosphere (TOA) energy _imbalance_ " [Stephen E Schwartz, USA]	Noted. Both expressions may be used, but budget is preferred here as it is more general (the budget may be balanced)
12-386	12	28	14			"regularly"? better "systematically"? [Stephen E Schwartz, USA]	Accepted, text revised
12-387	12	28	18	28	19	As noted in another comment, I think it would be a good idea to support this statement on the evolution of aerosols in the CMIP5 simulations with a plot of aerosol loading in the CMIP5 models for each scenario. [Nathan Gillett, Canada]	Taken into account. More information on the evolution of aerosol has been added in this chapter and are referred here.
12-388	12	28	23	28	23	Add 'and water vapour' after greenhouse gases. The LW change reflects interplay between temperatures responses, water vapour amounts and GHGs. Since common usage in the report does not include water vapour in the term GHG, need to ensure it is included here. [Robert Colman, Australia]	Taken into account, text revised. It has been specified that GHGs includes water vapor.
12-389	12	28	27			"increases the net LW flux TOA". Puzzling. net LW flux _at_ TOA? But why net? increases LW flux at TOA? Yes, it is an increase (panel b), which corresponds to a decrease in emitted flux according to the sign convention. So once the text is changed to "increases the LW flux at the TOA" it is correct, though still requiring some mental gymnastics. The volcanic peaks in the LW are broader than in the SW as expected (thermal lags) but surprising that the net in panel a doesnt seem to reflect this. [Stephen E Schwartz, USA]	Noted. Both conventions are possible. Net flux (i.e. downward-upward) is preferred in order to treat all the flux in the same way.
12-390	12	28	29	28	29	add 'although partially offset by increasing water vapour ' at end of sentence. Indeed it is this offsetting that is responsible for most of the warming (the 'water vapour feedback'), and so this should be clarified in the discussion. The LW decline would be roughly twice as steep without it. [Robert Colman, Australia]	Taken into account, text revised
12-391	12	28	29	28	29	recplace "driving increases" with "drive increases in" [Benjamin Sanderson, United States of America]	Accepted, text revised
12-392	12	29	4	29	29	This is a reasonable synopsis of the longer discussion in Chapter 7. It would be useful to include a reference to that chapter (7.2 in particular) for more details and discussion. [Steven Sherwood, Australia]	Taken into account, text revised, reference to chapter 7 has been added
12-393	12	29	5	29	7	First noted by Wetherald, R. T., S. Manabe, 1988: Cloud Feedback Processes in a General Circulation Model. J. Atmos. Sci., 45, 1397–1416. [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted, text revised
12-394	12	29	14			It would be helpful to comment on the sign of the SW cloud feedback when it is introduced. [Nathan Gillett, Canada]	Accepted, text revised
12-395	12	29	16	29	17	This sentence is not informative without any specification of the physical mechanisms. Also, the reference to different parametrizations gives the (probably unwanted) impression that the decrease in cloudiness may be a model artefact. [Jouni Räisänen, Finland]	Accepted, text revised. More informations are given as well as reference to chapter 7
12-396	12	29	18	29	19	I found this confusing . I would (1) put in a separate paragraph. (2) Turn the second sentence round - this leads to a decreased absorption around Antarctica where the ocean is open in summer (but presumably not in the Arctic where low cloud can lead to increased solar absorption through multiple reflection between sea-ice and the bottom of the cloud layer?) [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted, text revised. Discussion for both SH and NH has been inclued
12-397	12	29	19	29	21	The radiative response to the change in clouds in the high lats is described for the SH but not for the NH. [Nathan Gillett, Canada]	Accepted, text revised. Discussion for both SH and NH has been inclued
12-398	12	29	21			Replace 'are the dominant effect' with 'exert the dominant effect' [Nathan Gillett, Canada]	Accepted, text revised
12-399	12	29	22	29	22	robust results => robust result [Matthias Zahn, United Kingdom]	Accepted, text revised
12-400	12	29	49			The increases occur in the subtropics as well as the mid-latitudes, particularly in the SH (Fig 12.17). [Nathan Gillett, Canada]	Accepted - text revised

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12-401	12	29	54	29	55	This sentence tries to explain tropical increases in SLP in simulations of the response to future anthropogenic forcing by citing Gillett and Stott (2009). But Gillett and Stott (2009) attributed observed changes in SLP using model simulations of the response to anthropogenic forcing. So the reasoning is circular. [Nathan Gillett, Canada]	Accepted - text revised
12-402	12	29	55	29	55	Other important robust MSLP projections exist. Recommend inserting: "MSLP is projected to increase over the maritime continent and decrease in the eastern equatorial Pacific, in association with a weakening of the Walker circulation (Vecchi et al.2007; Power and Kociuba 2011ab)". [Scott Power, Australia]	Taken into account - text revised in combination with description of CMIP5 projections
12-403	12	29				Figures 12.17 and 12.18 show easterly shifts in high latitude northern hemisphere winds extending from the stratosphere to the surface. The Arctic pressure decrease and corresponding mid latitude high pressure is also considerably weaker than the previous IPCC AR4 multimodel mean. Both of these features agree with Scaife et al 2011 (Clim. Dyn., DOI 10.1007/s00382-011-1080-7), where vertically extended models show these features much more strongly than the IPCC set of models used in CMIP3. It therefore seems more reasonable to note this shift rather than to simply state that the response looks similar to the last IPCC report. - especially as CMIP5 contains several high vertical resolution models. [Adam Scaife, United Kingdom of Great Britain & Northern Ireland]	Taken into account - text revised and a number of studies highlighting CMIP3 and CMIP5 differences and high and low-top models cited.
12-404	12	30	6	30	10	How does figure 12.18 compares with CMIP3? What is the role of the fact that most of the CMIP5 set of models include the stratosphere, in the NH polar response? For possible stratospheric roles see: Karpechko, A. Y. and E. Manzini, 2011: Stratospheric influence on tropospheric climate change in the Northern Hemisphere. J. Geophys. Res (in press) and Scaife, A., T. Spangehl, D. Fereday, U. Cubasch, U. Langematz, H. Akiyoshi, S. Bekki, P. Braesicke, N. Butchart and M. Chipperfield, et al. (2011), Climate change projections and stratosphere–troposphere interaction . Clim. Dyn., DOI: 10.1007/s00382-011-1080-7 [Elisa Manzini, Germany]	Taken into account - combined with previous comment
12-405	12	30	12	30	13	Is the dependence of the poleward shift of the jet-stream on GHG forcing consistent with SLP changes? Is there a dependence of the Southern Annular Mode on GHG forcing? It is impossible to assess SLP changes from Figure 12.17 because it only shows (erroneously) the results for RCP 8.5. Earlier studies based on CMIP 3 models found a dependence of SAM response on GHG forcing (Simpkins and Karpechko 2012; Paeth and Pollinger, 2010), which seems to be consistent with the reported dependence of the jet shift. I suggest referring to the results by Simpkins and Karpechko (2012) and Paeth and Pollinger (2010). Missing references: (1) Simpkins, G. R. and A. Yu. Karpechko, Sensitivity of the Southern Annular Mode to Greenhouse Gas Emission Scenarios, Climate Dynamics, v.38, N. 3-4, 563-572, doi: 10.1007/s00382-011-1121-2, 2012; (2) Paeth, H. and Pollinger, F., Enhanced evidence in climate models for changes in extratropical atmospheric circulation. Tellus A, 62:647–660. doi:10.1111/j.1600-0870.2010.00455.x, 2010. [Alexey Karpechko, Finland]	Taken into account -however the primary discussion of modal behavior occurs in Chapter 14.3.
12-406	12	30	15	30	16	The text says that the mechanisms have been explored in simple and complex models and cites several studies, but it doesn't say what those studies find. [Nathan Gillett, Canada]	Taken into account - text revised
12-407	12	30	18	30	31	In this paragraph devoted to the influence of ozone recovery it should be mentioned that, according to chemistry model simulation, ozone will likely return to 1980 level around the midcentury (WMO, 2011) and therefore the influence of ozone recovery will mainly affect the atmospheric circulation during the first half of the 21 century. After that the GHG influence will likely dominate (see e.g. Simpkins and Karpechko 2012). References: (1) Simpkins, G. R. and A. Yu. Karpechko, Sensitivity of the Southern Annular Mode to Greenhouse Gas Emission Scenarios, Climate Dynamics, v.38, N. 3-4, 563-572, doi: 10.1007/s00382-011-1121-2, 2012; (2) World Meteorological Organization (WMO): Scientific assessment of ozone depletion: 2010, Global Ozone Research and Monitoring Project, Rep. No. 52, 516 pp., Geneva, Switzerland, 2011. [Alexey Karpechko, Finland]	Taken into account - more detailed discussion of ozone on SH circulation moved to Chapter 11 and modal behaviour is in Chapter 14
12-408	12	30	18	30	31	In my view, this rather detailed discussion of the effect of the ozone changes included in some climate models on the stratospheric winds in the SH fills quite a bit compared to the other text. Maybe just focus on the most important aspect, giving less references as well. [Wilhelm May, Denmark]	Accepted - more detailed discussion of the role of ozone on SH circulation moved to Chapter 11
12-409	12	30	25	30	26	Karpechko et al. (2010) studied the dependence of future atmospheric circulation changes on different plausible ozone recovery scenarios. They found that, in their model, the future changes in the upper	Taken into account - more detailed discussion of ozone on SH circulation moved to Chapter 11.

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						tropospheric winds differed significantly between the two ozone recovery scenarios, while the differences in the sea level pressure responses were small. This result is relevant to this discussion and can be mentioned. Missing reference: Karpechko, A. Y., N. P. Gillett, L. J. Gray, and M. Dall'Amico (2010), Influence of ozone recovery and greenhouse gas increases on Southern Hemisphere circulation, J. Geophys. Res., 115, D22117, doi:10.1029/2010JD014423. [Alexey Karpechko, Finland]	
12-410	12	30	26	30	31	No conclusions are drawn for the jet shift in the CMIP5 simulations. Fig 12.18 shows a poleward shift in DJF by the end of this century on average in the CMIP5 simulations examined so far. [Nathan Gillett, Canada]	Accepted - text revised and summary statements added.
12-411	12	30	42			Section 12.4.4.2: One fairly robust feature that appears in future climate model simulations is a strengthening of the Brewer-Dobson (BD) circulation, see for example Butchart et al. (2010) and references therein. Since changes in the BD circulation may have implications for tropospheric climate change, I think it is important to discuss the BD circulation changes in this section. Reference: Butchart et al., 2010: Chemistry–Climate Model Simulations of Twenty-First Century Stratospheric Climate and Circulation Changes. J. Climate, 23, 5349–5374. doi: 10.1175/2010JCLI3404.1 [Alexey Karpechko, Finland]	Taken into account. A discussion of the projected changes in Brewer-Dobson circulation is included in the SOD.
12-412	12	31	17	31	17	These findings are supported by more recent research that has explicitly examined projections in the Walker circulation in more detail than previous studies. Suggest rewording to: "… under global warming (Power and Kociuba 2011ab), more than …" References: Power, S.B., and G. Kociuba, 2011a: What caused the observed twentieth century weakening of the Walker circulation? J. Climate, 24, 6501–6514, doi: http://dx.doi.org/10.1175/2011JCLI. Power, S.B., and G. Kociuba, 2011b: Impact of global warming on the Southern Oscillation Index. Climate Dynamics, 37, 1745-1754, DOI: 10.1007/s00382-010-0951-7. [Scott Power, Australia]	Accepted. More recent literature has been cited in the SOD.
12-413	12	31	24	31	24	Note that while some of the responses in the tropical Pacific have some commonality with El Nino, many do not (Collins et al. 2010; Power and Kociuba 2011b). For example, during El Nino the Southern Oscillation Index (SOI) tends to decline, whereas 21st century projections exhibit a very robust increase (Power and Kociuba, Climate Dynamics 2011b). Rainfall teleconnection patterns associated with El Nino also exhibit many differences to projected changes and so the analogy can cause confusion (Collins et al., Nature Geoscience, 2010). Recommend avoiding use pof term or add sentence or two making point made in this review comment. [Scott Power, Australia]	Accepted. Text has been edited and suggested references considered.
12-414	12	31	26	31	38	I would suggest to move this paragraph further up, placing it before the discussion of the projected changes in the Walker Circulation. [Wilhelm May, Denmark]	Accepted. The paragraphs have been interchanged.
12-415	12	31	33	31	38	This section seems to focus on explaining observed changes in the Hadley Cell. The text should instead refer to Section 10.3.3.1 which examines attribution of changes in tropical circulation. [Nathan Gillett, Canada]	Taken into account. Reference has been made to Section 10.3.3
12-416	12	31	36	31	38	This really glosses over what looks on the face of it like a major problem. The fact that the change Is in the same direction in the models and obs is not a validation (one has a 50/50 chance with a random model)--the amplitude is way off, yet this is hardly mentioned. [Steven Sherwood, Australia]	Noted. That models underestimate the observed widening of the Hadley cell is mentioned in the text. The Figure 12.20 has been removed in the SOD.
12-417	12	31	47	32	40	I wonder why it is not referred to the reduced frequency of sub-synoptic scale storms (polar lows) and increased vertical stability over the ocean in this section (Decreased frequency of North Atlantic polar lows associated with future climate warming Zahn, M. and H. von Storch (2010) Nature, Volume: 467, Pages: 309-312, DOI:10.1038/nature09388). [Matthias Zahn, United Kingdom]	The focus in Ch 12 is not on regional changes (see 14.4.7 and Box 14.4), but on broader scale behavior, especially as resolved by CMIP5 models. Polar lows are poorly resolved, if at all, in the majority of these models. The referenced paper is discussed in Box 14.4, where there is much greater focus on regional behavior.
12-418	12	31	47			Sect.12.4.4: Why is there a sub-section on subtropical storms but not one on tropical storms. I think it's essential to add this (or add a pointer if it appears elsewhere, though it would seem odd to put them together) given the great significance of these events. [David Rowell, United Kingdom of Great Britain & Northern Ireland]	The focus in this section is on behaviour that CMIP5 GCMs resolve well. Thus, the section includes discussion of extratropical cyclones but not tropical cyclones: extratropical cyclones are fairly well resolved by CMIP5 GCMs, whereas tropical cyclones are not, except at resolutions finer than used by the

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							large majority of CMIP5 GCMs (see Chapter 9, Section 9.5.4.3). Detailed discussion of tropical cyclones appears in Box 14.3, and the section directs the reader to this Box.
12-419	12	31	55	31	55	Has this shift "several degrees in latitude" been quantified by directly comparing the latitude distributions of storm track activity in the present-day and future simulations? One cannot derive the shift by just comparing the present-day distribution with the distribution of the change. [Jouni Räisänen, Finland]	We arrived at the shift by comparing distributions of storm tracks in present-day and future simulations, not by examining the change in distribution.
12-420	12	31	57			Figure 12.11 referred to here shows changes in zonal mean temperature. It does not show baroclinicity. Knowledge of the mean state would be needed to infer this. At least some discussion of how the zonal mean temperature changes relate to changes in baroclinicity is needed, if not a plot of changes in baroclinicity. [Nathan Gillett, Canada]	Accepted. Good point. The baroclinicity changes are really suggested by the changes in jet structure and location. Reference to Fig. 12.11 removed.
12-421	12	32	4	32	5	Not sure about how well established the link between storm tracks and the ocean meridional overturning circulation is. I think this comment is rather speculative. I thought the the path of the storm tracks is really linked to the incidence of atmospheric blocking, which is also connected to changes in the stratosphere. Certainly plausible that the ocean heat transport plays a role, but not really proven yet. A better case is provided in Chapter 14-31 L28 to L52 discussing different processes affecting the NAO and the associated storm tracks. [Richard G Williams, UK]	There is a published literature on changes in storm tracks linked to oceanic changes, such as the MOC. We list this link as one of multiple factors that may influence simulation of storm tracks and their changes. We have added to this list a reference to Chapter 14 for discussion of modal processes that might affect storm tracks.
12-422	12	32	6	32	8	Consistency with previous projections seems a weak basis for a likelihood assessment. Many of the models used in CMIP5 are closely related to those used in CMIP3. Would the CMIP5 ensemble alone not support such an assessment? [Nathan Gillett, Canada]	This sentence has been removed from the revised text, on the basis of additional, new literature that highlights several sources of uncertainty in related processes.
12-423	12	32	23	32	24	What exactly does "thermal energy" refer to here? Usually thermal energy refers to total potential energy, most of which is irrelevant for atmospheric dynamics (only the much smaller available potential energy associated with horizontal temperature differences matters). [Jouni Räisänen, Finland]	This sentence has been removed as too vague, consistent with the reviewer's remark.
12-424	12	32	34	32	37	This section describes links between a poleward shifting NH stromtrack and the AO and Arctic climate. But on line 3 of this page, there is an assessment that there is 'less indication of a poleward shift in the stromtracks' in the NH. These discussions should be consistent. [Nathan Gillett, Canada]	The statement was revised to note that it is a result that might occur, and is based on one set of simulations, not a CMIP ensemble.
12-425	12	32	35			So the poleward shift of the SH stormtrack enhances the (weak) warming at the South Pole? Or does this refer to the NH only? [Nathan Gillett, Canada]	Analysis in Kug et al. focuses on the Northern Hemisphere, so the results are now restricted to the Arctic.
12-426	12	32	54			What does 'hydrologic activity' mean in this context? If this just means precipitation, then replace with 'precipitation'. If this mean evaporation, replace with 'evaporation'. If this means runoff, replace with 'runoff' etc. Or is this some sort of combined variable? If so then it should be clearly defined. If this does not have a definition, then it isn't possible to say which regions will experience a decrease and which an increase, and the statement is empty. [Nathan Gillett, Canada]	Accepted. Changed language to be more precise
12-427	12	32	58	33	1	I doin't think that this situation is particularly complex. A forced change is superposed on internal variability. This is the same for all climate variables. The signal to noise ratio might be lower for precip than for say temperature, but I don't think there is any fundamental difference. Averaging over ensemble members will reduce the effects of internal variability. [Nathan Gillett, Canada]	Issue is not simply internal variability, but substantial modal behavior that is partly noise and possibly partly signal.
12-428	12	32				12.4.4.3 This section confuses extratropical cyclones and other types of storms. Most of the conclusions, including any that employ GCMs, relate to extratropical cyclones, not other entities such as mesoscale convective systems. The statement that more "thermal energy" gives rise to stronger storms (l25) is probably incorrect, especially for the cyclones discussed e.g. by the cited reference. It is not clear what "storm strength" means in this context, since for example it is likely that on average extratropical cyclones will be less energetic but rain or snow harder due to greater moisture content. There is a discussion of this in Chapter 7 currently. The Donat et al study is based on reanalysis of surface pressure anomalies, not a very strong indicator of	The preamble to section 12.4.4 now notes that the focus is on behavior resolved by the CMIP5 GCMs. The statement about thermal energy is indeed misleading and has been removed. Donat et al. study uses SRES A1B projections of changes. However, most of the material in that paragraph has been removed, as it is discussed in Box. 14.4, where the

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						future trends in "damaging winds" even if we believed the reanalysis trends, and the others probably deserve scrutiny especially if they are regional and may reflect shifts of the storm track into the study area. I personally do not agree with the conclusion in lines 30-32, especially since it doesn't define what "strong" means. [Steven Sherwood, Australia]	regional focus is more appropriate. Reference is made to that Box in the revised text.
12-429	12	33	5			The variable discussed here is near-surface humidity, but the title suggests that free atmospheric humidity is coinsidered. [Nathan Gillett, Canada]	Section discusses both atmospheric and near surface humidity.
12-430	12	33	9	33	9	finding' not 'experience'. Also change 'common' to 'universal'. On global scales no models show large RH changes. [Robert Colman, Australia]	Rejcted. "finding" is appropriuate. "Universal" implies that the RH changes have been examined in every model. That may not be true.
12-431	12	33	12	33	12	add 'regional' before changes so as to avoid contradiction with line 10. [Robert Colman, Australia]	Rejected. It might be contradictory, as regional may imply smaller scales than "planetary".
12-432	12	33	22	33	23	An explanation is given for why RH decreases over land. But Fig 12.22 also shows that RH increases over ocean, in many regions significantly. What is the explanation for this? [Nathan Gillett, Canada]	They are statistically significant, but the largest changes in magnitude are the decreases over land. There is no apparent understanding apparent for why RH might increase a small amount over the ocean.
12-433	12	33	26	33	26	very likely? The mechanism is simple and both theory and models point the same way. Should also probably add 'modest' before reductions. [Robert Colman, Australia]	"modest" is not clearly defined. Given uncertainties in modeling precipitaiton, landuse changes, etc., "likely" seems more appropriate.
12-434	12	33	37	33	37	find another word rather than 'steadily'. Precip changes are anything but that! [Robert Colman, Australia]	Accepted - text revised by deleting steadily
12-435	12	33	41	35	21	In this section, changes in annual range of prcipitation, i.e., the difference between wet and dry seasons, such as in Chou and Lan (2012, J. Climate, 25, 222-235) should be discussed here. The changes are very robust not only among the CMIP3 climate models, but should also be the same in the CMIP5 models. [Chia Chou, Taiwan, ROC]	This aspect of precipitation change is discussed at length in following paragraphs.
12-436	12	33	44	33	45	Please explicitly state here that "future *global mean* precipitation increases are primarily the result of changes in the energy balance of the atmosphere". [Elizabeth Kendon, United Kingdom of Great Britain & Northern Ireland]	Adding global mean to the statement is too restrictive. For instance, changes in the energy balance alter the circulation which alters precipitation locally. Similar statements could be made for other local sources of available water.
12-437	12	33	45			After Boer 1993" insert "Chou and Neelin 2004", and at end of sentence add "and the way that these changes in energy balance interact with circulation, moisture and temperature." [J. David Neelin, United States]	Accepted - reference added and text revised.
12-438	12	33	46	33	46	This would be clearer if you said "radiative cooling" rather then "radiative budget" [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
12-439	12	33	47	34	9	This section reflects rather particular perspective in attempting to interpret hydrological cycle changes. While elements of this perspective may be useful, it would be hard to say that it reflects the consensus of the community. I'm suggesting some wording below that will help to broaden this a bit, although it's not comprehensive. [J. David Neelin, United States]	We have shortened this section, highligting the key result that one can identify a planteary precipitation responses that are roughly similar across models and RCPs
12-440	12	33	50	33	51	Rather than decomposing into a fast and slow response, I think it is more physically meaningful to decompose into a response to GHG changes and a response to surface temperature changes, as Allen and Ingram (2002) propose. [Nathan Gillett, Canada]	Taken into account - The text has been clarified. Instead of "slow" and "fast", which is confusing, we write "direct effect of CO2 increase" and "effect of temperature increase". The direct effect of CO2 is due to the direct warming of the troposphere when CO2 increases, even the surface temperature does not change. Studies show that even if the CO2 increase is slow, the direct effect of CO2 is not negligible compared to that of temperature increase. These points are now

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							also addressed in chapter 7, section 7.6
12-441	12	33	50	33	55	The decomposition into a "fast" and "slow" response is one particular way of partitioning things but in practice this division tends to be artificial since the radiative change occurs slowly. I suggest reducing this discussion because only the slow part is relevant applications. The reference to figure 12.23 is also confusing, and the global average precipitation shown in figure 12.23 is not really a key factor in applications compared to the amount of space being afford it. I suggest cutting this figure and simply describe it in words. [J. David Neelin, United States]	Taken into account - The text has been clarified. Instead of "slow" and "fast", which is confusing, we write "direct effect of CO2 increase" and "effect of temperature increase". The direct effect of CO2 is due to the direct warming of the troposphere when CO2 increases, even the surface temperature does not change. Studies show that even if the CO2 increase is slow, the direct effect of CO2 is not negligible compared to that of temperature increase. These points are now also addressed in chapter 7, section 7.6
12-442	12	33	52			Replace 'for positive forcing' with 'to an increase in GHGs'. It is the increase in GHGs which drives the precip reduction, not the positive radiative forcing. [Nathan Gillett, Canada]	Accepted - text revised
12-443	12	33	55			Allen and Ingram (2002) argue that precip is controlled by the energy budget of the free troposphere, not by the availability of atmospheric moisture, and I thin Held and Soden (2006) agree with this. So I think 'and atmospheric water vapour content' should be deleted here. [Nathan Gillett, Canada]	Accepted - text revised.
12-444	12	33	58			This rate of increase of P with T is a model result, but several observational studies suggest that the true value may be higher (e.g. Zhang et al., 2007; Went et al., 2007; Allan and Soden, 2007), although this remains controversial. See also the discussion in 10.3.2.2. The possibility that the true value of dP/dT lies above the model value should be discussed. [Nathan Gillett, Canada]	Accepted - text revised to the following "For CO2 forcing, the modeled ratio between the relative change of precipitation (dP/P) and the temperature change (dT) is in the range dP/P/dT = 2–3% K-1 (Allen and Ingram, 2002; Held and Soden, 2006). Several observational studies suggest that this ratio may be higher (e.g. Zhang et al., 2007; Wentz et al., 2007; Allan and Soden, 2007)."
12-445	12	34	1	34	8	Perhaps worth mentioning that, in particular, the large uncertainty due to model differences in shortwave cloud feedbacks contributes little to uncertainty in precipitation change per degree warming because shortwave cloud feedbacks have little effect atmospheric radiative absorption? Lambert, F. H. and M. J. Webb, Dependency of global mean precipitation on surface temperature, Geophys. Res. Lett., Vol. 35, L16706, 2008. [Francis Hugo Lambert, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised to "The inter-model spread may be due to differences in modelled shortwave absorption by water vapor (Takahashi, 2009b) but not the large uncertainty in shortwave cloud feedbacks (Lambert 2008)"
12-446	12	34	2	34	8	I found that this description of the response to absorbing aerosols was not clear without reading the cited references. This should be clarified and the 'slow response' and 'fast response' more clearly defined. Also absorbing aerosols are discussed, but the role of sulphate aerosol is not discussed - this must also be important for precipitation. [Nathan Gillett, Canada]	We have shortened this section and moved the discussion to earlier in the section, removed the figure and stated the key result, that multiple models tend to have global precipitation increasing with global temperature.
12-447	12	34	8	34	8	This likelihood is specified as virtually certain in the ES. [Robert Colman, Australia]	Accepted. Text and summary are now consistent.
12-448	12	34	17	34	28	Shiogama et al. (2010a,b) found the robust emission scenario dependency of precipitation sensitivity in the CMIP3 ensemble due to different aerosols emissions. It seems that this dependency holds in the CMIP5 ensemble (Fig. 12.24). Shiogama, H., et al., 2010: Emission scenario dependencies in climate change assessments of the hydrological cycle. Climatic Change, 99, 321-329. Shiogama, H., S. Emori, K. Takahashi, T. Nagashima, T. Ogura, T. Nozawa, and T. Takemura 2010: Emission Scenario Dependency of Precipitation on Global Warming in the MIROC3.2 Model, Journal of Climate, 23(9), 2404-2417. [Hideo Shiogama, Japan]	Accepted - text revised. Added "The lower values in the high GHG scenarios are consistent with an enhanced damping effect on future global mean precipitation from larger aerosol concentrations (Shiogama et al. 2010a, 2010b).
12-449	12	34	17	34	28	The results in Figure 12.24 seem to show a strong decrease in delta P with temperature for higher RCPs for the 'global' case, an even stronger decrease for the sea-only case and no apparent difference (certainly not a statistically significant one looking at the data) for the land-only case. This should be mentioned in the text,	Taken into account - text revised

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						and if possible an explanation given. [Seth Westra, Australia]	
12-450	12	34	25	34	26	"As temperature approaches stabilisation in RCP2.6 projections the gradient of precipitation versus global temperature change steepens (Figure 12.24),..." It is not clear how this shown in Figure 12.24. [Seth Westra, Australia]	Taken into account - text revised [the reference to Figure 12.24 was incorrect and is replaced with a reference to Figure 12.5 which does show the effect described]
12-451	12	34	26	34	28	I did not find this explanation very clear. The point is that since GHG concentrations are declining in RCP 2.6 it has a higher ratio of warming to CO2 concentration than the other scenarios. [Nathan Gillett, Canada]	Taken into account - text revised.
12-452	12	34	38			Change to "…. likely that many (but certainly not all) arid …." to avoid over-stating this important point in a way that would mislead some readers. [David Rowell, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
12-453	12	34	39			After "moist regions will experience more", insert "simply because the increase of water vapor leads to additional moisture convergence within tropical convergence zones in additional moisture divergence in the descent zones (Chou and Neelin 2004, Held and Soden 2006). However, the reaction of the tropical circulation to this basic effect includes strong local convergence feedback that can yield much stronger precipitation changes at the regional scale (Chou et al. 2006), especially in the seasonal response. These regional changes can differ considerably from model to model, especially along the margins of the convection zones (Neelin et al. 2006) where spatial inhomogeneities, including the rate at which air masses tend to flow into the convection zone from dry regions, can yield considerable sensitivity in precipitation response.. [J. David Neelin, United States]	Accepted - text revised
12-454	12	34	41	34	42	I suggest cite the recent study of Li et al. (2011, ERL, 6, doi:10.1088/1748-9326/6/3/034018) along side Wentz et al (2007), to better indicate the mixed results noted here in the text. Unlike Wentz et al (2007), the Li et al study supports precipitation changes over the past two decades broadly consistent with the modelling results. [Anthony Hirst, Australia]	Accepted - text revised
12-455	12	34	45			Change to "…. decreases or in some regions not much change at all". The current text "even" implies some surprise at this result, whereas it is of course inevitable that such regions will exist between areas of increase and decrease. [David Rowell, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
12-456	12	34	47	34	48	I agree that multi-model projections are not probabilistic statements about the likelihood of changes. But the role of this chapter is in my view to generate such statements. So the authors need to discuss what assumptions need to be made and/or other evidence accounted for, in order to generate probabilisitic projections of precip change from the multi-model ensemble. For example, if the authors assume exchangeabilitiy of the obs and models, then say the 5-95% range across the model projections can be interpreted as a 5-95% confidence range on projected changes. But is this justifiable? See Knutti et al. (2010). [Nathan Gillett, Canada]	Uncertainty estimates for future global mean temperature provided in AR5 and other literature have accounted for the undersampling of the CMIP-type ensembles. This has not been done for precipitation, either globally or regionally. The Atlas shows percentiles of the CMIP ensemble, but there is no basis for a probabilistic projection in the literature.
12-457	12	34	47	35	2	As for precipitation, the wide range of deficiencies of the different climate models in CMIP5 might also be a major contributor to the uncertainty of the projected changes in precipitation at a regional scale. Hence, I think this kind of uncertainty should also be mentioned here, possibly referring to the part of the report, where this issue is discussed in further detail. [Wilhelm May, Denmark]	Accepted - text revised on line 20. Changed to "Part of this variance is due to genuine differences between the models including their ability to replicate observed precipitation patterns (See Chapter 9)."
12-458	12	34	51	34	52	Rowell (2012) separate these 2 sources of variance at fine scales (see citation in #12). [David Rowell, United Kingdom of Great Britain & Northern Ireland]	Accepted - reference added
12-459	12	34	56	34	56	This claim could be validated using the Deser et al ensemble [Benjamin Sanderson, United States of America]	Reference added.
12-460	12	34	57	35	2	This statement is dependent on how "confidence" is defined. By a variance measure? Also, the point could be broadened to saying that uncertainty is larger where changes have large spatial gradients. [David Rowell, United Kingdom of Great Britain & Northern Ireland]	Confidence is an expert judgement that takes into account several factors, including the relative magnitudes of the projected change and its variance. We have added the sentence Regarding large spatial gradients, these regions between positive and negative changes are among the highest spatial gradients, so we feel the point somewhat redundant.

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12-461	12	35	1	35	2	I don't agree with this. For any location, there will be a mean projected precip change, and some spread about this. Are the authors saying that the model spread is larger for regions with small precip changes than it is for regions with large precip changes? If so, they could test this. In the absence of such results, I don't think there is evidence to support this statement. [Nathan Gillett, Canada]	For the regions in between positive and negative mulitmodel projected changes, the ratio of the intermodel variance to the mean change is indeed very large. I.e. the location of the zero line varies a great deal. This is reflected in all the various hatching/stippling schemes under consideration for AR5.
12-462	12	35	4	35	4	The first sentence of the paragraph seems to be out of place here, actually breaking the connection with the preceeding paragraph. [Wilhelm May, Denmark]	The sentence is included to introduce the importance of seasonality in precipitation projections.
12-463	12	35	10	35	11	This argument implies that precip should increase everywhere. See my general comment on the chapter and Held and Soden (2006). [Nathan Gillett, Canada]	We argue that the sentence does not imply this as the qualifier "high latitudes" is included. Circulation changes are secondary in this region.
12-464	12	35	11	35	13	Although this statement is most likely true in the coldest regions, increases in snowfall are unlikely to extend as far south as increases in total winter precipitation (Räisänen 2008, Climate Dynamics, 30, 307-319). [Jouni Räisänen, Finland]	Accepted - reference added and text revised to "...as increases in snowfall at the highest latitudes and in rainfall in the southern extents of these regions (Räisänen 2008)
12-465	12	35	17	35	19	This is somewhat simplistic. It is the pattern of P-E which is intensified. Held and Soden (2006) should be cited here. [Nathan Gillett, Canada]	Accepted - reference added and text revised on line 19 to "...dry become dryer (Held and Soden 2009)
12-466	12	35	17			In addition to Chou et al. (2009), Allan (2012) shows precipitation changes (in CMIP3 models) as a function of dynamic regime (drying of warm descending regimes and more precipitation in wet tropics (despite reduced Walker circulation) and extra tropics. [Allan, R.P., (2012) Regime dependent changes in global precipitation, Climate Dynamics in press, 10.1007/s00382-011-1134-x] [Richard Allan, UK]	Accepted - reference added
12-467	12	35	19			Replace "areas that are currently wet become wetter, areas that are currently dry become dryer" by "areas that are currently wet tends to become wetter, while areas that are currently dry tend to become dryer. This holds well in the annual average for a multi-model ensemble mean (such as in figure 12.24), but it is important to note that significant exceptions can occur in particular regions, especially on a seasonal basis and especially on the margins of convective zones. These exceptions are less well agreed upon by the models, which tend to put them in slightly different locations so that they are washed out in the multi-model ensemble average. The amplitude of the multi-model ensemble mean precipitation response thus significantly under predicts the median amplitude calculated from each individual model (Neelin et al. 2006, Knutti et al 2010b). [J. David Neelin, United States]	Accepted - text revised
12-468	12	35	30			sec 12.4.5.3: it would be an interesting result if soil moisture shows more model consensus than precip. This seems possible given it depends on T as well as P – can you comment if this is the case? [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland]	Consensus in soil moisture is rather small, as is for precipitation over land. Note that internal variability is probably as important as model differences.
12-469	12	35	34	35	36	This topic is addressed in detail in the IPCC SREX (2012, chapter 3, Section 3.5.1). [Sonia Seneviratne, Switzerland]	Accepted - reference added
12-470	12	35	39			temporal bvariability of soil moisture predictions' sounds strange. Unless the authors mean predictions of soil moisture variability, I woiuld drop 'temporal variability of'. [Nathan Gillett, Canada]	Accepted - text revised
12-471	12	35	48	35	51	Some recent simulations with models with high vertical resolution show a southward movement of the European storm track towards the Mediterranean in winter which may weakne the drying signal [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Scaife maintains it is not higher vertical resolution per se (in their first study) that is crucial but rather the higher top and the consequences of that. It's also possible to take issue with the word "Mediterranean" in terms of the drying signal, as Scaife et al. Fig. 6 illustrates the Med Sea gets even drier with the extended top in model 1, and a mixture of wetter/drier (relative to low top) in model 2. There is an equatorward shift of the storm track, but the regional

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							precipitation response in the Med region (land and sea) is more subtle. Nonetheless the first study considers 2 models and the second considers a different one (though perhaps with a family resemblance to one of the two models in the first study), and I suppose there's some robustness about the storm track movement between low-top and high-top. The following sentence has been added "An additional shift of the storm track has been shown in models with a better representation of the stratosphere, and this is found to lead to an enhanced increase in extreme rainfall over Europe in winter (Scaife et al., 2011). "
12-472	12	35	48	35	51	Modification compared to assessment of IPCC SREX (2012; "medium confidence"). Please provide more detailed argumentation. [Sonia Seneviratne, Switzerland]	Accepted - text revised
12-473	12	36	24	37	25	I would have liked more discussion on changes to evapotranspiration, and particular projected changes to potential evapotranspiration which is an important input to many hydrological models. PET changes as a function of net radiation, moisture deficit and wind, each of which might change in a future climate. Some discussion on GCM projections related to PET would be useful. See also my review comment in relation to Chapter 2 line 34. [Seth Westra, Australia]	PET is not available directly from the CMIP5 archive. The Thornthwaite approximation implies that PET would vary at temperature changes varied, which is implicit in our discussion where relevant.
12-474	12	36	33	36	34	"The large decreases in runoff in southern Europe and the southwestern U.S. are consistent with increases in the intensity of the Hadley circulation..." In other parts of the report it is suggested that the Hadley circulation is expected to slow down, and become wider and deeper. Need to be clearer about what is meant by the 'intensity' of the Hadley circulation. [Seth Westra, Australia]	Accepted - text revised on line 34 from "increases in the intensity" to "changes"
12-475	12	36	34			But models project a weakening of the Hadley Circulation as discussed two page previously in 12.4.4.2. This is confusing. [Nathan Gillett, Canada]	Accepted - text revised on line 34 from "increases in the intensity" to "changes"
12-476	12	36	35	36	37	This argument implies that precip should increase everywhere. See my general comment on the chapter and Held and Soden (2006). [Nathan Gillett, Canada]	Accepted - text revised. Changed “with the greater precipitation possible in a warmer climate with more atmospheric moisture” to “with the projected precipitation increases”
12-477	12	36	52	36	53	Again, some recent simulations with models with high vertical resolution show a southward movement of the European storm track towards the Mediterranean in winter which may weakne the drying signal [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	There is some disagreement about how much storm tracks will extend further. Box 14.4 discusses this. Text modified to refer to Box 14.4 on this region.
12-478	12	36	55	36	57	Warming should increase potential evaporation everywhere, but some areas exhibit an increase in evaporation and others a decrease, so this does not provide an explation. [Nathan Gillett, Canada]	This sentence refers to the northern high latitudes. In figure 12.28, evaporation increases nearly everywhere over land.
12-479	12	37	2	37	12	I would suggest to move these two paragraph further up, placing it before the discussion of the projected changes in evaporation. [Wilhelm May, Denmark]	Accepted - text revised by rewriting the two paragraphs and inserting at line 37, page 36
12-480	12	37	24	37	24	could not find any Figure 12.23d. [Matthias Zahn, United Kingdom]	Comment refers to page 38, line 24. Change to figure 12.25
12-481	12	37	27	38	37	I am wondering, whether there isn't any assessment of projected changes in the frequency and intensity of long-lasting precipitation events (i.e., wet spells) based on global climate model simulations in the scientific literature. [Wilhelm May, Denmark]	The literature is sparse in this area, especially in a global context.
12-482	12	37	31	37	31	"On short time scales,..." Be more precise - define 'short' timescales. [Seth Westra, Australia]	Accepted - text revised. Changed "On short time scales" to "At daily to weekly scales" and changed "On longer time scales" to "At seasonal or longer time scales"

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12-483	12	37	32	37	33	But in case of intense drying, evapotranspiration is limited which provides a negative feedback. [Sonia Seneviratne, Switzerland]	More of a limitation on the severity of prolonged drought rather than a negative feedback, which would imply lessening of the drought. We elect to leave the sentence as is.
12-484	12	37	35	37	56	A new recent publication, Chou et al. (2012, in press (Chou, Chia, Chao-An Chen, Pei-Hua Tan and Kwan-Ting Chen, 2012: Mechanisms for global warming impacts on precipitation frequency and intensity. J. Climate , doi:10.1175/JCLI-D-11-00239.1)), can be added here. This study discusses changes in precipitation frequency and intensity in all strenths of precipitation. [Chia Chou, Taiwan, ROC]	Accepted - reference added and text revised by adding at line 46 "...mid latitudes consistent with Chou at al. 2012)
12-485	12	37	35	37	56	This paragraph largely duplicates a similar discussion in Chapter 7 but misses some important additional studies noted there. Since it is likely that Chapters 11 and perhaps 14 are also going to talk about this, it would make sense to leave detailed discussion to Chapter 7 and shorten here in order to leave more room to discuss the projections shown in Fig. 12.29? [Steven Sherwood, Australia]	At the moment, we feel that this discussion is necessary in Chapter 12 for context.
12-486	12	37	37	37	37	The first consider => The first considers [Matthias Zahn, United Kingdom]	Accepted - text revised
12-487	12	37	41	37	41	Suggest adding to end of discussion of first mechanism: "Increases in atmospheric water vapor are expected to increase the intensity of individual precipitation events, but have less impact on their occurrence. As a result increases in extreme precipitation may be more reliable than increases in mean precipitation in some regions (Kendon et al., 2010)." [Elizabeth Kendon, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
12-488	12	37	44	37	44	Emori & Brown 2005 showed (and I think still stands) that the dominant process, at least spatially, is the thermodynamic. They found the tropical convection/moisture flux convergence is confined to the tropical warm pool which the Li sonal mean diagnostics do not show. Emori, S; Brown, SJ. “Dynamic and thermodynamic changes in mean and extreme precipitation under changed climate.” GEOPHYSICAL RESEARCH LETTERS 32 (17): 2005. [Simon Brown, UK]	Accepted - reference added and text revised to add at line 44 "Emori & Brown 2005 showed that the thermodynamic mechanism dominated nearly everywhere outside tropical warm pool. However, Li et al…"
12-489	12	37	51	37	52	It would be worth to add references in order to support this statement. [Igor Shkolnik, Russian Federation]	Accepted - reference added and text revised.
12-490	12	37	51	37	53	I am not sure it is true that projections of extreme precipitation "often tend" to be more robust that for mean precipitation. I would suggest the following alternative wording: "Mechanisms of natural variability are a large factor in assessing the robustness of these projections (Kendon et al., 2008). Projections of future extreme precipitation may be more robust at the regional scales than for future mean precipitation, although there is a tendency for signal-to-noise ratios to decrease on considering increasingly extreme metrics." This is because although the signal generally increases for more extreme (rarer) events, there is a greater increase in noise due to internal variability (Kendon et al., 2008). [Elizabeth Kendon, United Kingdom of Great Britain & Northern Ireland]	Accepted - reference added and text revised.
12-491	12	37	51			Can this assertion be explained and/or given a reference? [Ramon de Elia, Canada]	Accepted - reference added and text revised.
12-492	12	37	53	37	55	I disagree with the statement "the mechanisms implicitly assume that circulation characteristics, such as storm tracks, will not change substantially in a future climate". In Kendon et al. (2010) large-scale circulation change is found to have a secondary role in driving future changes in extreme precipitation across Europe. I would therefore suggest the following alternative wording: "In addition, large-scale circulation changes, which are uncertain, could dominate over the above mechanisms. However, analysis of CMIP3 models suggests circulation changes are unlikely to be sufficient to offset the influence of increasing atmospheric water vapour on extreme precipitation change over Europe at least on large spatial scales (Kendon et al., 2010). An additional shift of the storm track has been shown in models with a better representation of the stratosphere, and this is found to lead to an enhanced increase in extreme rainfall over Europe in winter (Scaife et al., 2011)." [Reference: Scaife A. A. et al (2011) Climate change projections and stratosphere-troposphere interaction. Clim. Dyn. DOI 10.1007/s00382-011-1080-7] [Elizabeth Kendon, United Kingdom of Great Britain & Northern Ireland]	Accepted - reference added and text revised.
12-493	12	37	53	37	56	Circulation changes are discussed elsewhere in the chapter, including a poleward shift in the stromtracks. Does this mean the influence of these circulation changes on extremes is limited? [Nathan Gillett, Canada]	Discussion of the role of circulation changes on precipitation extremes has been expanded. The

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							following statements excerpted from the new text addresses the understanding of this role: "In addition, large-scale circulation changes, which are uncertain, could dominate over the above mechanisms depending on the rarity and type of events considered. However, analysis of CMIP3 models suggests circulation changes are unlikely to be sufficient to offset the influence of increasing atmospheric water vapour on extreme precipitation change over Europe at least on large spatial scales (Kendon et al., 2010). An additional shift of the storm track has been shown in models with a better representation of the stratosphere, and this is found to lead to an enhanced increase in extreme rainfall over Europe in winter (Scaife et al., 2012)."
12-494	12	37				Section 12.4.5.5: There is some overlap with Chapter 2 regarding discussion of changes in precipitation extremes. Some space could be saved by referring back to this chapter. [Richard Allan, UK]	We feel that this discussion is necessary in Chapter 12 for context.
12-495	12	38	1	38	24	Further supporting evidence for changes in precipitation extremes by the end of the 21st Century has been found for HadGEM2-AO under the emissions scenarios described by Johns et al. (2011). This is by Caesar and Lowe, to be submitted. [John Caesar, United Kingdom of Great Britain & Northern Ireland]	Rejected. Material that is not submitted cannot be assessed.
12-496	12	38	2	38	3	Check also Section 3.1 and Box 3.1 of IPCC SREX (2012, chapter 3), which address these aspects in detail. [Sonia Seneviratne, Switzerland]	Accepted. Section now refers to SREX
12-497	12	38	10	38	37	Please provide more detailed and quantitative assessment (with diagrams) on the projected changes in the probability of occurrence of extreme rainfall events in different regions by CMIP5 model simulations, in particular for those densely populated sub-regions. [Tsz-cheung Lee, Hong Kong]	This is a topic for Chapter 14 and/or WG2.
12-498	12	38	22	38	22	"on days with large storms" is an assumption that is not supported by the definition of the index R95p, which refers to precipitation on wet days (>1mm precipitation/day). [Jana Sillmann, Canada]	Accepted - text revised to "wet days"
12-499	12	38	32	38	34	Seems to be a discussion of model validation. Cite chapter 9 if this is covered there. [Nathan Gillett, Canada]	Accepted - text revised
12-500	12	38	48	38	49	Fig 12.30(b) shows a mean September decrease of about 5.5 x 10^6 km^2 by 2100 in RCP 8.5, and the mean observed present day ice extent is 6.8 x 10^6 km^2 according to the title of the figure. Thus the mean model has 1.3 x 10^6 km^2 of ice left in 2100 assuming its climatology is correct, which seems to disagree with the statement that 90% of models have less than 10^6 km^2 by 2100. Is there a low bias in the climatology of the models? If not, how else is this explained? [Nathan Gillett, Canada]	Taken into account - The apparent inconsistency mentioned by the reviewer results from the fact that the model distribution around the mean is not Gaussian. Figure 12.30 and the associated text have been modified to clarify this point. In particular, we now show the 5-95% range of intra-ensemble variability (which is a a more judicious choice than the standard deviation to quantify the model spread when the mean approaches zero) and give in each panel the value of the multi-model mean sea ice extent averaged over 1986-2005.
12-501	12	38	54	38	55	It seems more related to the locations of the deep water formation. Most of the Arctic is not close to the sinking regions in the GIN and Lab Seas. The SH ice edge is located very near the sinking regions for the intermediate waters and some bottom waters locations in the models (but not obs). [Ronald Stouffer, USA]	Rejected - To our knowledge, there is no evidence in the peer-reviewed literature to support the reviewer's claim.
12-502	12	38	55			Why do the models show more variability of their projections in Antarctic September ice extent that for the other months/locations? [Nathan Gillett, Canada]	Rejected - The answer is unknown.
12-503	12	39	14			But according to Fig 12.30 the Antarctic trend in SH in both February and september is overstimated. [Ramon de Elia, Canada]	Rejected - This para is about Arctic sea ice. We stress in the last para of this section that the majority of CMIP5 models simulate a decreasing trend in

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							Antarctic sea ice extent for all seasons over the recent decades, in contrast to the small observed increase.
12-504	12	39	16	39	20	This comparison would be more informative if extended up to 2011. [Jouni Räisänen, Finland]	Taken into account - In the SOD of Chapter 12, we do not make a detailed comparison between the CMIP5 multi-model mean trend in September Arctic sea ice extent and observations but instead make reference to Chapter 9, in which the analysis is extended up to 2011.
12-505	12	39	16	39	37	This is a model validation/attribution discussion. Cite chapters 9 and 10, and shorten the discussion here. [Nathan Gillett, Canada]	Taken into account - Text revised to avoid redundancy with Chapters 9 and 10. However, sea ice is one of the cases where model evaluation can help to constrain projections. So, we think it is important to connect the pieces.
12-506	12	39	18			check this (unreferenced) trend against the values given in Chapter 4. [David Vaughan, UK]	Accepted - All observational sea ice data discussed/shown in Chapter 12 are now consistent with those analysed in Chapter 4.
12-507	12	39	31	39	37	This section is a little confusing, make clear which numbers refer to which bounds [Benjamin Sanderson, United States of America]	Accepted - Text revised.
12-508	12	39	45	40	6	most of this text seems to be about 20th and 21st century change, not really about the longterm changes. [David Vaughan, UK]	Rejected - Section 12.4 is restricted to the projected climate changes over the 21st century. Furthermore, only a limited number of CMIP5 models have provided sea ice data beyond the 21st century, which prevents a sound assessment of sea ice changes at that time scale.
12-509	12	39	45	40	6	This discussion, and Fig. 12.33, is interesting but seems a bit speculative, more the sort of idea that needs to be written about in more depth in a research paper. Maybe it will be, but at present this seems to me to be going a bit beyond the IPCC remit of assessing existing research. Unless there is a paper in the works on Fig. 12.33, I suggest simply discussing the existing literature and its implications (which is done here). [Richard Wood, UK]	Accepted - Figure 12.33 and most of the associated text have been replaced by a new figure and a new text that are solely based on published or submitted papers.
12-510	12	39	48	39	50	I didn't find the relationship between the area of thin ice and the rate of extent change shown in Fig 12.33b convincing. Is the correlation coefficient statistically significant? Has this been shown in the literature? Cite the relevant studies. [Nathan Gillett, Canada]	Taken into account - The correlation was modest but statistically significant, and references discussing this relationship for CMIP3 models were given at page 39, lines 47-48 of the FOD. Nonetheless, to answer comment 12-509, Figure 12.33 and most of the associated text have been replaced by a new figure and a new text that are solely based on published or submitted papers. Both correlations and one-tailed p-values are now given in the figure.
12-511	12	39	48	39	50	Considering the low correlation (-0.4) this statement seems too optimistic. [Jouni Räisänen, Finland]	Taken into account - We agree with the reviewer that the statement was too optimistic. To answer comment 12-509, Figure 12.33 and most of the associated text (incl. this sentence) have been replaced by a new figure and a new text that are solely based on published or submitted papers.
12-512	12	39	54	39	56	This paragraph and particularly this sentence I had to read several times. One solution might be to move this sentence to the end of the first sentence on page 40, and put the next sentenceon page 40 in a new paragraph beginning " Overall , conditions in the the .." [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Taken into account - To answer comment 12-509, Figure 12.33 and most of the associated text (incl. these sentences) have been replaced by a new figure and a new text that are solely based on published or

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							submitted papers.
12-513	12	39				Figure 12.20- would be useful in some way to say allow the reader to assess what the fractional decrease is- perhaps just by noting what the climatological ice extents are , especially for the Arctic in summer. [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted - We guess that this comment is about Figure 12.30. The value of the multi-model mean sea ice extent averaged over 1986-2005 is now given in each panel.
12-514	12	40	1			What does this mean? This reads strangely. [Nathan Gillett, Canada]	Accepted - Text revised.
12-515	12	40	4	40	6	In fact, the separation between CMIP3 and CMIP5 seems very vague in Fig. 12.33a. [Jouni Räisänen, Finland]	Taken into account - To answer comment 12-509, Figure 12.33 and most of the associated text (incl. these sentences) have been replaced by a new figure and a new text that are solely based on published or submitted papers.
12-516	12	40	43	40	53	This is an attribution issue - cite section 10.5.1.1 and shorten the discussion here. [Nathan Gillett, Canada]	Taken into account - We now make reference to Chapters 9 and 10. However, we feel it is important to recall here the major model deficiencies in the Southern Ocean to put into perspective the Antarctic sea ice projections.
12-517	12	40	47	40	52	Resolving eddies would give an effect of the wrong sign. The wind driven changes in the Southern Ocean overturning should lead to an increase in poleward heat transport by eddies. This would warm the Southern Ocean more strongly, if anything. Cecilia Bitz has also carried out eddy resolving simulations to examine the sea ice response to changing winds in the Southern Ocean. These simulations do not show a more positive trend in sea ice than non-eddy resolving simulations. So far these results are unpublished. [Nathan Gillett, Canada]	Rejected - The eddy heat transport response to an increase in surface westerlies has been shown to depend on latitude in eddy resolving models. The behaviour of low-resolution models is similar and also model dependent. We await publications about the sea ice response.
12-518	12	41	1	41	5	A percentage change in snow cover days (SCD) of -40...-30 % was projected by Jylhä et al. (2007, Table 2) to occur in northern Europe from the 1970s to 2080s. The projected absolute decreases of SCD (in days) were largest around the northern Baltic Sea, on the western slope of the Scandinavian mountains and in the Alps. Conversely, the simulated percentage decrease in SCD was most pronounced in the western and southern regions of Europe. In northern Europe, the largest percentage reductions in SCD and also in SWE were found in autumn. Percentage decreases were smaller for SCD than that for SWE. See Jylhä K., Fronzek, S., Tuomenvirta, H., Carter, T.R. and Ruosteenoja, K. 2008: Changes in frost, snow and Baltic Sea ice by the end of the twenty-first century based on climate model projections for Europe. Clim. Change, 86, 441-462. http://www.springerlink.com/content/b74186u33916vw82/ [Kirsti Jylhä, Finland]	Taken into account State more generally that snow cover duration and snow cover area changes have the same sign but not necessarily the same amount, while SWE changes are more complicated
12-519	12	41	1	41	49	The figures and discussion focus exclusively on snow cover and permafrost in the Northern Hemisphere, with no mention of the Southern Hemisphere. The SH should at least be discussed, even if only to say why it is not covered in more detail (lack of data, lack of sensitivity to climate change, small snow-covered/permafrost area?). [Nathan Gillett, Canada]	Taken into account. Nowsays why the Southern Hemisphere is not discussed (small snow-overed/permafrost area that is subject to change under RCP-style climate change). State more clearly that we talk about seasonal snow cover, not ice sheets.
12-520	12	41	2			Is this annual mean snow covered area? Or snow covered area in the middle of the winter? [Nathan Gillett, Canada]	Taken into account. Now states this more clearly. In fact, the statement is sufficiently general to apply both to annual, winter, or spring snow cover.
12-521	12	41	3	41	5	What period is referred to hear? The end of the 21st century? Is this Northern Hemisphere only? What happens to snow-covered area in mid-winter? [Nathan Gillett, Canada]	Taken into account. This sentence will be completely withdrawn and replaced by a more general statement. In the FOD version, it lacked both a statement on the period and on the scenario. Thank you for pointing out this imprecision.
12-522	12	41	14	41	16	What are the uncertainties here? I would advocate using 5-95% ranges throughout the chapter. [Nathan Gillett, Canada]	Taken into account - It's inter-model spread (1 sigma), as will be done in the rest of the chapter. Calculations

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							redone with more models and ensemble means.
12-523	12	41	14			The text doesn't say what sign the changes in SCA are, and the change is given as positive implying an increase in SCA. Presumably it's a decrease, in which case use a minus sign or say 'decrease'. [Nathan Gillett, Canada]	Taken into account. Now stated.
12-524	12	41	35	41	36	The assertion is trivial. One might not expect any other responses of temperature except warming or cooling. In addition, it would be relevant to note that snow metamorphizm representation (e.g., snow density) and forest vegetation treatment in climate models can have pronounced effect on the projected seasonal thawing/freezing depths in the permafrost areas (Shkolnik et al., 2010; Shkolnik et al., 2012)". References: (1) Shkolnik I.M., E.D.Nadyozhina, T.V.Pavlova, E.K.Molkentin and A.A.Semioshina, 2010: Snow cover and permafrost evolution in Siberia as simulated by the MGO regional climate model in the 20th and 21st centuries. Environ. Res. Lett. 5 015005 doi:10.1088/1748-9326/5/1/015005, (2) Shkolnik I.M., E.D. Nadyozhina, T.V. Pavlova, E.I. Khlebnikova, A.A. Semioshina, E.K. Molkentin, E.N. Stafeeva, 2012: Simulation of the regional features of the seasonal thawing layer in the Siberian permafrost zone. Earth Cryo., 2 (in press). [Igor Shkolnik, Russian Federation]	Taken into account. Meant to say that one might naIvely expect that the snow changes only have warming effects, but this is not true. Now stated more clearly. More references on snow conductivity changes, as suggested.
12-525	12	41	53	41	53	This subsection does not discuss heat TRANSPORT, but rather heat CONTENT. The two are not the same. In adddition, only SURFACE temperature and salinity are mentioned. Hence I recommend renaming the subsection as "12.4.7.1 Surface temperature and salinity and ocean heat content" [Stephanie Downes, Australia]	Accepted - title changed as suggested
12-526	12	41	57	42	4	Ocean heat content: Recent work shows increasing dOHC/dtime; this is quite important in terms of interpreting planetary energy imbalance and amount of disequilibrium and committed future warming. One would not discern the importance of this from this para. It would be of interest to know how models might inform that discussion in the future. [Stephen E Schwartz, USA]	Accepted - Additional text added to emphasize importance
12-527	12	41	57	42	12	This section deals largely with comparison of simulated changes with observed changes. This is a detection and attribution issue, so the discussion in chapter 10 should be cited. [Nathan Gillett, Canada]	Accepted - Cross references to 10.4.1 and 10.4.2 added
12-528	12	42	6	42	12	A link/consistency check with corresponding section in Chap 3 is needed here [Eric Guilyardi, France]	Accepted - Cross references to 3.2.3 and 3.3.2.1 added
12-529	12	42	6	42	12	These results are also supported by study of Terray et al. (2011) (Terray L., Corre L., Cravatte S., Delcroix T., Reverdin G., Ribes A., 2011: Near-surface salinity as Nature's rain gauge to detect human influence on the tropical water cycle. Journal of Climate, Vol. 25, n°3, 958-977.) [Didier Swingedouw, France]	Accepted - Reference to Terray et al. (2011) added.
12-530	12	42	7	42	10	A link/consistency check with corresponding section in Chap 9 is needed here [Eric Guilyardi, France]	Response will be provided when CMIP5 data is fully processed
12-531	12	42	20			sec 12.4.7.2: is it now the case that models are thought biased stable for AMOC? And that freshwater transport across 30S into the Atlantic can be used as a real-world observable of possible stability/instability of AMOC? This seems like a real advance but not mentioned here. [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland]	Taken into Account - The issues of model stability and freshwater transportinto the North Atlantic are addessed in the revised Abrupt Climate Change section 12.5.5.2
12-532	12	42	21	42	41	This section does not acknowledge the recent body of work that suggests that the AMOC in the current generation of coupled climate models may be overly stable as a result of deficiencies in freshwater transports. For example, see chapter 9, p30, lines 42-44: "there is evidence that a bias in ocean fresh water transport seen in various climate models may make the Atlantic Meridional Overturning Circulation (AMOC) overly stable in current models (Weber et al., 2007)" and section 12.5.5.2 (this chapter): "Moreover there is some indication that most climate models may overestimate the stability of the Atlantic ocean circulation (Drijfhout et al., 2010; Hofmann and Rahmstorf, 2009)". For this reason, the conclusions that "it is unlikely that the AMOC will collapse beyond the end of the 21st century" and that it "remains very unlikely that the AMOC will undergo an abrupt transition or collapse" are stated with too much confidence. It is also unclear to what extent these conclusions are reliant on the CMIP3 and the few available models plotted in figure 12.37. Is it possible that some of the CMIP5 models do show a rapid collapse, but have not yet been analysed? [Chris Roberts, Uk]	Taken into account - The AMOC threshold discussion has been moved entirely to 12.5.5.2 where the discussion of model sensivity exists. Note that an assessment that it is "likely" that the AMOC would not undergo an abrupt transition direcly implies that there would be a 1 in 3 chance that it would collapse this century. There is no evidence to support a 1 in 3 chance of the AMOC collapsing in the 21st century. The additional evidence regarding model sensitivity does not change the assessments of the AR4 or SAP 3.4 (Abrupt Climate Change) of the US National Assessment.

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12-533	12	42	24	42	25	RCP2.6 does NOT clearly imply a WEKENING of the AMOC as the other RCPs do. Perhaps note this. [Stephanie Downes, Australia]	Rejected. There is some weakening in most models.
12-534	12	42	24	42	27	This sentence about the two coordinate systems does not appear policy-relevant. [Jouni Räisänen, Finland]	Noted - but it is an important scientific point.
12-535	12	42	26	42	27	Although the results from Zhang et al. (2010a) are really convincing, I believe they are only based on results of one particular model. I think this should be indicated in the sentence, adding a "in one AOGCM" for instance. [Didier Swingedouw, France]	Accepted - they used GFDL CM2.1
12-536	12	42	27	42	28	The statement that once radiative forcing stabilizes, the AMOC returns to its preindustrial strength doesn't appear to be generally true. For example 12.37d shows no return to preindustrial strength in RCP 8.5 after stabilization of radiative forcing. RCP 2.6 has decreasing radiative forcing for part of the experiment, and the return to preindustrial AMOC is not clear in the figure to me. [Nathan Gillett, Canada]	Accepted. Reworded.
12-537	12	42	27	42	28	The AMOC only recovers back to preindustrial levels once radiative forcing is stabilised in the weakest scenario, and this is only based on two models. [Laura Jackson, United Kingdom of Great Britain & Northern Ireland]	Accepted. Reworded.
12-538	12	42	27	42	28	This statement seems to be based on results from two models that show no significant change in response to the weakest RCP forcing. Why have the changes in response to the other models/RCP forcings not been described? Figure 12.37 should be updated when more CMIP5 model results are available (ocean meridional velocity data should be available for more models, even if overturning streamfunctions are not). [Chris Roberts, Uk]	Accepted. All available models are shown.
12-539	12	42	27	42	28	When looking at Fig. 12.37 I do not find a clear recovery of the AMOC to its preindustrial level except for rcp26. This shoud be specified at least that it is only for this scenario (and not true for the others). In fact, I am not sure this sentence is useful and could be deleted. [Didier Swingedouw, France]	Accepted. Reworded.
12-540	12	42	27	42	28	It will be important here to check for control run drift when interpreting these figures. These can vary a lot between models and may be significant over 450 years in some cases. I'd also urge precision in describing the forcing. Is the radiative forcing really stabilised in the RCP extensiopn runs? I realise this figure and discussion are preliminary due to limited CMIP5 data availability, so apologies if this is stating the obvious! [Richard Wood, UK]	Full response will be provided when CMIP5 data is processed. Control drift is assessed in the model evaluation chapter. So far, there doesn't appear to be strong drifts in the MOC or suface temperature. There is drift in the ocean heat content but that is accounted for when calculating thermal expansion.
12-541	12	42	28	42	28	The Gregory et al. (2005) refernce uses models pred-dating the IPCC AR4. I think it should be noted what CMIP the models from each refernce in this paragraphy are from. Otherwise you're implying that the "models" (a very loosely used term in this section, by the way) are all of CMIP3/CMIP5 standard. [Stephanie Downes, Australia]	Accepted - there were six models from CMIP2/3 and five EMICs
12-542	12	42	28	42	29	Please specify that Gregory et al. (2005) was analysing results from the CMIP3 ensemble, and that these results were included in AR4. [Chris Roberts, Uk]	Accepted - there were six models from CMIP2/3 and five EMICs
12-543	12	42	28	42	29	It should be specified that the Gregory et al. Study was done with a few CMIP3 model. [Didier Swingedouw, France]	Accepted - there were six models from CMIP2/3 and five EMICs
12-544	12	42	31	42	41	I don't see that these likelihoods are derived explicitly. Maybe these are rather statements concerning the available evidence? They should be rephrased accordingly in line with the IPCC Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties. E.g.: Given the CMIP5 models, there is "robust evidence" that ... Taking into account important shortcomings of these models and persistent disagreement between AOGCMs, there is only "limited evidence" that ... [Gregor Betz, Germany]	Taken into account - the paragaph has been deleted here and combined with 12.5.5.2. A Table has been included in that section which provides further confidence statements.
12-545	12	42	31	42	41	More bullish than section 12.5.5.2 which notes that most models may be more stable than "reality" [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Noted - the paragraph has been deleted here and combined with 12.5.5.2
12-546	12	42	31	42	41	This paragraph is a model of clarity in placing what is predicted by the models in the larger context of what could happen vs. what will probably happen. [Steven Sherwood, Australia]	Noted

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12-547	12	42	32	42	32	…behaviour have not changed …. [Benjamin Sanderson, United States of America]	Accepted
12-548	12	42	32	42	34	I anticipate a rather wider range of responses once you have more CMIP5 models. For example HadGEM2 shows quite a big AMOC weakening (around 70%) by 2200 under RCP8.5 continuation. SO this conclusion may need to be modified. [Richard Wood, UK]	Accepted. Revised section and figure are based on all available models.
12-549	12	42	33	42	35	It should be mentionned here that the AOGCMs used for the estimations of AMOC weakening still do not include melting of Greenland ice sheet in their projections and that the effect of such a melting remains matters of debate but should further decrease the AMOC, with different rate depending on the models, and their proximity to thresholds. [Didier Swingedouw, France]	Taken into Account - The assessment has been moved into the revised Abrupt Climate Change section 12.5.5.2. A note is added there concerning the lack of an interactive ice sheet in coupled model. Studies which add additional representative freshwater from ice sheets are cited.
12-550	12	42	34	42	41	Based on the results from current GCMs and the definition of 'very likely' used in the report, I can see why this was included, but it implies a high degree of confidence that the AMOC will not collapse which many people would disagree with. There has been a body of work which has suggested that most GCMs are biased in a way which makes the AMOC less sensitive than that in the real world (Drijfhout et al., 2010; de Vries and Weber, 2005; Hawkins et al, 2011). This should be into discussed and taken into account when expressing likelihood. In particular section 12.5.5.2 seems to contradict section 12.4.7.2 which should be resolved within the sections and in the executive summary. Refs: -Drijfhout, S. S., S. L. Weber, and E. van der Swalow, 2010: The stability of the MOC as diagnosed from model projections for pre-industrial, present and future climates. Published online by Clim. Dyn., DOI:10.1007/s00382-010-0930-z -Hawkins, E., R. S. Smith, L. C. Allison, J. M. Gregory, T. J. Woollings, H. Pohlmann, and B. de Cuevas, 2011: Bistability of the Atlantic overturning circulation in a global climate model and links to ocean freshwater transport, Geophys. Res. Lett., 38, L10605, doi:10.1029/2011GL047208. -de Vries, P., and S. L. Weber, 2005: The Atlantic freshwater budget as a diagnostic for the existence of a stable shut down of the meridional overturning circulation. Geophys. Res. Lett. 32, L09606, doi:10.1029/2004GL021450 [Laura Jackson, United Kingdom of Great Britain & Northern Ireland]	Taken into account - The AMOC threshold discussion has been moved entirely to 12.5.5.2 where the discussion of model sensivity exists. The Drijfhout and Hawkins papers are cited there.
12-551	12	42	34	42	41	The discussion of AMOC thresholds here duplicates (and is inconsistent with) the discussion in section 12.5.5.2. It's far too simplistic here, as it ignores the increasing evidence that models may be biased towards being too stable (see refs in 12.5.5.2), and increasing evidence of the existence of thresholds in GCMs (Hawkins et al. GRL 2011) [Richard Wood, UK]	Taken into account - The AMOC threshold discussion has been moved entirely to 12.5.5.2. Hawkins citation has been included.
12-552	12	42	36	42	40	"classical El Niño response" is a very confusing term to describe a mean state change. I suggest blending the two phrases: The weakening of the Pacific Walker circualtion (section …) leads to reduced equatorial upwelling... [Eric Guilyardi, France]	Accepted
12-553	12	42	39	42	39	I think citation to Hu et al. (2009) and Swingedouw et al. (2007) after "be required" will be useful to correctly illustrate the assertion.(Hu et al. (2009) Transient response of the MOC and climate to potential melting of the Greenland Ice Sheet in the 21st century. Geophys. Res. Lett., 36, L10707, doi:10.1029/2009GL037998. Swingedouw D., Braconnot P., Delecluse P., Guilyardi E. and Marti O.,Quantifying the AMOC feedbacks during a 2xCO2 stabilization experiment with land-ice melting.Climate Dynamics 29: 521-534, 2007.) [Didier Swingedouw, France]	Accepted
12-554	12	42	55	42	56	What is the reference for the statement that the southward displacement of the ACC induces warming at 35-40S in the CMIP3 models? I don't think this is true. The SST response to the SAM is driven mainly by a combination of latent, sensible and Ekman heat fluxes in the obs and CMIP3 models (Screen et al., 2010). While the response mechanism may differ somewhat on longer timescales, when eddy heat fluxes might become important, I think these contributions are likely to remain important. I think it is wrong to characterise the surface warming at these latitudes as a response to a shift in the ACC. Ref: Screen et al., Mixed Layer Temperature Response to the Southern Annular Mode: Mechanisms and Model Representation, J. Clim., 23,	Taken into account - Text revised to have a more balanced discussion of the various mechanisms responsible for the warming. We now discuss more precisely the response in various latitude bands and at different depths.The warming induced by the southward wind shift is clear at subsurface around 40°S but also at the surface (see Fig 4 of Fyfe et al.

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						664-678, 2010, [Nathan Gillett, Canada]	(2007) and the discussion of Fig. 14 in Sen Gupta et al. (2009)).
12-555	12	43	3	43	7	Changes in the Ferrell Cell aren't discussed, but are potentially important for climate and the carbon cycle. [Nathan Gillett, Canada]	Taken into account - We are not aware of any peer-reviewed paper addressing this issue. Nevertheless,we now more deeply discuss the changes in Southern Ocean upwelling.
12-556	12	43	6	43	7	The reference to projections of Subantarctic Mode Water and Antarctic Intermediate Water subduction is Downes et al. (2010)- not 2011. However, the Downes et al. (2011) refrence used later in the paragraph IS correct. So the correct full reference for lines 6-7 is: Downes, S. M., N. L. Bindoff and S. R. Rintoul (2010). Changes in the subduction of Southern Ocean water masses at the end of the 21st century in eight IPCC models. J. Climate, 23(24), 6526-6541. [Stephanie Downes, Australia]	Accepted - Reference corrected.
12-557	12	43	31			Replace 'IPCC AR4' with 'CMIP3'. [Nathan Gillett, Canada]	Accepted
12-558	12	43	51	43	51	Should the work "between" be inserted after "Differences"? [Stephanie Downes, Australia]	Yes. Changed.
12-559	12	43	54	43	55	The comment implies that the AR5 should focus exclusively on CMIP5 and not consider CMIP3. Studies on CMIP3 published since the AR4 deserve to be assessed here, and should contribute to the conclusions of the AR5. [Nathan Gillett, Canada]	Rejected. The sentence makes that explicit albeit in the parenthetical, stating that some of the chapter results are still based on CMIP3 results. But this section addresses specifically the differences and similarities between the two set of modeling results.
12-560	12	43		45		It should be mentioned why CMIP5/CMIP3 are different (significant levels) and which we should be trusted. [Zong-Ci Zhao, China]	We have added an analysis of the significance of some of the differences (geographic patterns) described in the SOD. The general message is that the two families of models produce very similar results, so the issue of which of the two should be trusted or not is muted.
12-561	12	44	4		4	Awkward language: Rerunning has not been done. [Stephen E Schwartz, USA]	Rephrased.
12-562	12	44	8	44	15	This text describes the method, but it does not assess the results. [Nathan Gillett, Canada]	Accepted. We have added a better assessment in the SOD. These results were obtained close to the submission deadline of the FOD leaving little time to assess.
12-563	12	44	11	44	11	radiative spelling [Benjamin Sanderson, United States of America]	Corrected
12-564	12	44	13	44	13	Can uncertainties for the emulation be quantified? [Benjamin Sanderson, United States of America]	We cannot strictly quantify, but we have added a discussion of the reliability of the emulation results.
12-565	12	44	26	44	42	Using more complex model (so-called EMIC) constrained by comtemporary observation data, we obtained similar result for RCP 2.6 and 4.5 (Tachiiri, K., Hargreaves, J. C., Annan, J. D., Huntingford, C. and Kawamiya, M (submitted to Nature Climate Change, and now in revision): Allowable carbon emissions for a medium mitigation scenario.) [Kaoru Tachiiri, Japan]	Reference added.
12-566	12	44	35	44	38	Delete sentence starting, "Observational or other historical constraints…", as this does not affect the message of the already lengthy paragraph. [Stephanie Downes, Australia]	Rejected. There is no mention of the use of observational constraints prior to this, and it is an important aspect of the uncertainty characterization.
12-567	12	45	5			Carbon cycle differences are very unlikely to explain differences between model simulations shown in Figures 12.38 and 12.39. These simulations all had prescribed concentrations of CO2, so carbon cycle differences woiuld only effect the land surface, which would only have a small effect on the climate. [Nathan Gillett, Canada]	We have added this consideration to the discussion.
12-568	12	45	6	45	8	Are the differences between the patterns statistically significant? Secondly, quote the exact correlation	We have added an assessment of the significance (by

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						coefficients. The correlation for temperature looks higher than 0.9. [Nathan Gillett, Canada]	bootstrap)
12-569	12	45	6	45	8	The differences in patterns will deserve more discussion when more CMIP5 models are available. [Jouni Räisänen, Finland]	The patterns computed using more models show a high level of geographic agreement. We discuss the significance in the intensity (and the difficulty, in general, of attributing the sources of differences between CMIP5 and CMIP3 at this stage) in the updated section.
12-570	12	45	10			Figure 12.40: There is no mention in the caption (or associated text) of how many models are used in the averaging for the CMIP3 and CMIP5 models. Are there the same amount of models, or perhaps significantly more for CMIP3? [Stephanie Downes, Australia]	We have now specified the number of models/scenarios included, after adding more models to the analysis of the patterns from CMIP5.
12-571	12	45	25		51	purpose not clear; evidently placeholder [Stephen E Schwartz, USA]	Editorial
12-572	12	45	43			Replace 'the values used by ESM forced in CO2 concentrations' with 'the values prescribed in the RCPs'. [Nathan Gillett, Canada]	Done.
12-573	12	45	53			sec 12.4.9.2: Ch6 also has a paragraph on projections of methane so we should cross-check for consistency and maybe remove overlap. This section needs mention of permafrost and hydrates as well as wetlands [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland]	Done, paragraph moved to chapter 6
12-574	12	46	17	46	19	In polluted tropospheric air, higher water vapor increases ozone (as do higher temperatures, independently. This is shown in Figure 1 of Jacobson, M.Z, On the causal link between carbon dioxide and air pollution mortality, Geophysical Research Letters, 35, L03809, doi:10.1029/2007GL031101, 2008. [Mark Z. Jacobson, U.S.A.]	Section moved to chapter 11
12-575	12	46	17	46	22	This paragraph doesn't mention changes in ozone precursors - these are only mentioned in the second paragraph. These are likely to be an important driver of future change in tropospheric ozone, so should be at least mentioned at the start of this section, even if a detailed discussion of this topic is not within the scope of the chapter. [Nathan Gillett, Canada]	Section moved to chapter 11
12-576	12	46	17			sec 12.4.9.3 – tropospheric ozone can also damage plants and reduce carbon uptake – this could have a radiative forcing effect similar in size to its direct greenhouse forcing. Cross reference with Ch6 on this. [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland]	Section moved to chapter 11
12-577	12	46	38	47	12	This section on stratospheric ozone projections doesn't cite the 2010 WMO Scientific Assessment of Ozone Depletion. This is by far the most authoritative source on stratospheric ozone projections, and should be cited here, and used as a basis for this section. I think a focus here on scenario-dependence and interactions with climate change makes sense, as well as any new insights gained from stratospheric changes in the CMIP5 simulations. But I woiuld definitely base the section on the WMO (2010) conclusions. [Nathan Gillett, Canada]	Section moved to chapter 11
12-578	12	47	14	47	30	Good to see this discussion here, although land use change (from direct human intervention, as described here), isn't a feedback, it's a forcing. This section also needs discussion on the biophysical effects of vegetation change on climate as a feedback (again through processes such as surface albedo change and evapotranspiration, but with the vegetation responding to climate change itself) [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Correct, but hard to asess as no literature on the specific impact of change in natural vegetation dynamics on the climate. Discussion is limited on this specific point due to space constraints.
12-579	12	47	14			sec 12.4.9.5 – I found the section on land-use much too short as this is important. Biogeophysical effects are potentially strong and the LUCID project has investigated this. Studies by Pitman or de Noblet would be beneficial to report on here. The biogeochemical effects are also important with a sizeable fraction of CO2 emissions coming from land-use historically. Brovkin, Matthews, Pongratz have all published on this effect. Physical effects seem to be more important on seasonal timescales and in certain regions. Thomson et al (2008) showed that land-use scenarios are very uncertain even within a given IAM, so future forcing by land-use, especially on regional/decadal scales is potentially important but under-represented in typical studies. Interactions between human land-use and climate change could decrease ecosystem resilience [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland]	LUCID data and papers mentioned here added in the SOD.

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12-580	12	47	32			Section 12.5: The titles and organisation of content in the subsections may benefit from further improvement : the title of 12.5 itself is nearly the same as the title of the chapter, so that the meaning of "Long term" may be context-dependent and unclear for the reader. I would suggest to take the following into account : 1) clearly show where post-2100 results are discussed ("RCP extension" is very technical for a title and the results are not in that sectin anyway); 2) clearly separate the discussion on climate stabilisation from the discussion on post-2100, because some of the issues regarding stabilisation relate to the 21st century -- "stabilisation" in the broad sense is not necessarily equilibrium, and the RCP 2.6 peaks in the 21st century (see specific comment on P53 L43) [Philippe Marbaix, Belgium]	Accepted. Title changed to "Climate Change Beyond 2100, Commitment, Stabilization and Irreversibility". The reviewer is correct that stabilization can occur before 2100 and some scenarios actually peak. It is difficult to put that all in the titles but the text makes it clear. The way the data is provided it it most logical and tracable to discuss CMIP5/RCP to 2100 in 12.4, and the rest in 12.5.
12-581	12	47	52	50	7	A general comment to section 12.5.2: It may be an idea to make a table giving an overview of the various types of "climate change commitment" concepts that are used in the literature; how they are calculated and their applications/what they tell us. "Climate change committment" is a concept that is useful and popular, and by giving such an overview and explanantion, it may function as a guide for the various applications. [Jan Fuglestvedt, NORWAY]	An overview of the various commitments (zero emission, constant emission, constant forcing) is provided in FAQ 12.3 and should serve exactly that purpose.
12-582	12	47	54	50	7	It's a bit curious that no reference at all is given to Sea Level Rise. A few lines from Ch. 13 would be in place, possibly in connection with ocean heat uptake. Also, the fact that much commitment and (quasi-)irreversibility is associated with ocean heat uptake and primarily occurs for processes tied to the ocean's surface temperature could be more explicitly stated here. [Sybren Drijfhout, Netherlands]	Sea Level rise is mentioned and shown in Figure 12.44. The role of the ocean heat uptake in driving the commitment is also discussed in several places. Sea level projections are in the corresponding chapter and cross references will be given where appropriate.
12-583	12	48	7	48	16	It may be useful to illustrate the multiple timescales of the reponse with an Impulse Response Function for temperature. (One example is given in the appendix (fig A1) of the paper by Boucher & Reddy Energy Policy 36 (2008) 193–200.) And IRF_dT would correspond to the figures in Chapter 6 showing a similar thing for the CO2 response (figures Box 6.2, Figure 1 and FAQ 6.1, Figure 2. (Alternatively, since a step change in forcing is discussed in the text, it may be easier to add a figure for a step case rather than a pulse case.) [Jan Fuglestvedt, NORWAY]	The examples given in the FAQ 12.3 show the timescales relevant for projections are in our view are easier to understand than a pulse response function.
12-584	12	48	18	48	20	The transition from the previous paragraph on temperature response to this paragraph about the CO2 is a little abrupt since this is about a concentration response to emissions. It would be good to make this transition smoother. It could also be an idea to combine the IRFs for dT and CO2 in one single figure (with the necessary explanation) in order to illustrate two important mechanisms for slow responses and long memory. A reference to the dicussions of this issue in chapter 6 should be given. [Jan Fuglestvedt, NORWAY]	Sentence removed
12-585	12	48	18			I wouldn't say that the carbon cycle is 'another component that can delay a response to a change in CO2 emissions'. It is fundamental in controlling the respnse to CO2 emissions, since it determines what fraction of CO2 is removed from the atmosphere. With no carbon cycle, CO2 woudl not be removed from the atmosphere at all, so the response to CO2 emissions would be even more delayed. [Nathan Gillett, Canada]	Agreed. Sentence rmoved
12-586	12	48	31		36	constant composition commitment. Given short lifetimes of aerosols, seems unlikely absent some sort of geoengineering; need to explain why examined. The 85± 10% of final value would seem dependent on the model and the aerosol forcing. Need to discuss. But an important finding. [Stephen E Schwartz, USA]	Taken into account, these experiments were extensions beyond 2300 of the RCP scenarios only. There isn't any geoenginerring in these stabilisation, and the aerosol forcing is close to zero by 2300 in the RCP extensions. The 85% depends on the forcing history. This section has been extended to clarify. The range of realized warming is larger.
12-587	12	48	35	48	36	This fraction of warming realised at stabilization may be approximately constant across the RCPs considered, but it can't be constant across all possible scenarios. For example an instantaneous doubling of CO2 scenario would have zero warming at stabilisation. A scenario is which CO2 increased linearly to doubling at 10000 years would have almost 100% realised warming at stabilisation. Replace 'and is almost independent of the forcing scenario' with 'for the RCP scenarios considered'. Even for the scenarios shown, I'm not sure this is completely true, since radiative forcing for RCP4.5 appears to stabilise around 2100 when the fraction of realised warming is around 0.7. [Nathan Gillett, Canada]	Taken into account. Discussion extended.
12-588	12	48	56	49	4	Can solve for how much carbon remains in the atmosphere and the associated radiative heating. (1) The	Noted. No changes proposed, and material does not

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						amount of carbon remaining in the atmosphere after 1000 years can be solved for based on this cumulative emissions: CO(t)=CO(t_preindustrial)exp(Iemission/IB) where Iemission is the cumulative carbon emissions and IB is the buffered carbon inventory; reference is Goodwin, P., R.G. Williams, M.J. Follows and S. Dutkiewicz, 2007: Ocean-atmosphere partitioning of anthropogenic carbon dioxide on centennial timescales. Global Biogeochemical Cycles, 21, GB1014, doi:10.1029/GB002810. (2) The link between longterm radiative heating and cumulative carbon emissions is set out in Goodwin, P., R.G. Williams, A. Ridgewell and M.J. Follows, 2009. Climate sensitivity to the carbon cycle modulated by past and future changes in ocean chemistry. Nature Geosciences, doi:10.1038/ngeo416. [Richard G Williams, UK]	appear to add substantially to the discussion.
12-589	12	48	57	49	1	The statement "20-30% of the anthropogenic carbon emissions still will remain in the atmosphere…." may cause confusion. I think one should make it clear that it is the perturbation, or the change in concenetration that remains. (Alternativeley, one could write "emission signal" instead of "emissions".) [Jan Fuglestvedt, NORWAY]	The authors don't think this can cause confusion, but added 'cumulative' to be entirely clear.
12-590	12	49	1	49	2	A reference to chapter 8 (which also discusses an shows this) could also be given here. [Jan Fuglestvedt, NORWAY]	Taken into account.
12-591	12	49	6	49	7	The deep ocean is not the only sink of carbon which is important following a cessation of carbon dioxide emissions. In the decades immediately following a cessation of emissions the land takes up more carbon than the ocean. Also the mixed layer is probably takes up more carbon than the deep ocean immediately following a cessation of emissions. See Gillett et al (2011), Fig 1. [Nathan Gillett, Canada]	Taken into account, text clarified.
12-592	12	49	14	49	15	A recent paper of Tanaka and Raddatz (2011, Climatic Change Letters, http://www.springerlink.com/content/k160023102g83v4v/) is also along this line and can be discussed here. The paper shows that the warming due to aerosol removal would be large in case the climate sensitivity is high. The paper also points out a risk of ignoring the relationship between climate sensitivity and aerosol forcing estimates as it leads to an underestimation of the warming due to an aerosol removal. [Katsumasa Tanaka, Switzerland]	Taken into account.
12-593	12	49	17	49	18	This type of warming due to the elimination of aerosols is termed "hidden commitment" in Tanaka and Raddatz (2011, Climatic Change Letters, http://www.springerlink.com/content/k160023102g83v4v/), which could be indiated here. [Katsumasa Tanaka, Switzerland]	Not taken into account. The reference given is the only paper that uses that terminology.
12-594	12	49	35		46	Similar in Held et al J Clim 2010. [Stephen E Schwartz, USA]	Held et al., paper is referred to in the following paragraph.
12-595	12	49	51	49	52	a positive temperature anomaly is maintained for decades to allow the ocean to lose its excess heat'. This seems to be imply an intelligent actor controlling the climate. In an experiment in which radiative forcing is increased then set to zero, the near surcface air temperature after the forcing is set to zero is warmer than in a preindustrial control simulation because of a heat flux from the ocean to the atmosphere. [Nathan Gillett, Canada]	Accepted, text changed.
12-596	12	49	54	49	54	Should 'Beside the commitments described above, due inertia' be 'due to inertia'? [Mark Charlesworth, United Kingdom of Great Britain & Northern Ireland]	Taken into account.
12-597	12	50	4	50	7	This is important and it is good that this is mentioned here. I also think that links to the relevant chapters in WGIII could be established so that they can build on WG1 chapter 12. [Jan Fuglestvedt, NORWAY]	Noted.
12-598	12	50	9			Section 12.5.3 and 12.5.4 : it does not appear easy to identify where estimates of climate sensitivity, their uncertainty, and relations with model performance should be found, it is devided in at least 2 subsections (+ ch 9) - especially because the box on climate sensitivity is in section 12.5.4. Suggestion: move the box, and possibly other information on climate sensitivity, to section 12.5.3 (or possibly another place earlier in the chapter, as this is a very general topic). [Philippe Marbaix, Belgium]	Noted. Position of box will be decided at the final stage. Cross references to the different sections where climate sensitivity is discussed will be added.
12-599	12	50	19	50	20	As written this might imply that there is a unique nonlinear relationship between TCR and ECS, but of course there is not. TCR also depends on the rate of ocean heat uptake. [Nathan Gillett, Canada]	Taken into account, added "through ocean heat uptake".
12-600	12	50	21	50	22	I'm not sure that TCR and ECS are so important for policy. I imagine that policymakers are more interested in	As stated in the following sentences, the warming for

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						projected warming for specific scenarios and time horizons. [Nathan Gillett, Canada]	specific scenarios and time horizons is determined by the radiative forcing, the TCR and ECS. So they are very important, and more universal than the warming for a particular scenario, because with TCR/ECS the response to any forcing can be quantified.
12-601	12	50	25			"the ratio of temperature to forcing is nearly constant and invariant across scenarios" Surely the statement is meant to refer to temperature _change_, not temperature. This ratio is expected to be model dependent. Numbers should be given in text (or table), and, as well, provide equilib sensitivities for the models so that the numbers can be compared. The proportionality of temp increase and forcing is seen also when observed DeltaT is plotted against forcings, leading to transient sensitivity as slope. Gregory JM, Forster PM (2008) Transient climate response estimated from radiative forcing and observed temperature change. J Geophys Res 113:D23105. doi:10.1029/2008JD010405 Padilla, LE, Vallis GK, Rowley CW, (2011) Probabilistic Estimates of Transient Climate Sensitivity Subject to Uncertainty in Forcing and Natural Variability. J. Climate, 24: 5521–5537. doi: http://dx.doi.org/10.1175/2011JCLI3989.1 Schwartz S. E. (2012) Determination of Earth's transient and equilibrium climate sensitivities from observations over the twentieth century: Strong dependence on assumed forcing. Surveys Geophys. In press. http://www.ecd.bnl.gov/steve/pubs/ObsDetClimSensy.pdf [Stephen E Schwartz, USA]	Taken into account. Added "in any given model". TCR numbers are provided in the model evaluation chapter.
12-602	12	50	29	50	30	Climate sensitivity is also discussed in Chapter 10 (Section 10.9) of FOD, which can be linked here. [Katsumasa Tanaka, Switzerland]	Noted. Cross references will be improved in later versions of the chapters.
12-603	12	50	33			"From the models available so far, the range of climate sensitivities in CMIP5 is 2.1–4.6°C". Should be provided in a table, with models identified. Also TCR. [Stephen E Schwartz, USA]	Numbers are provided in model evaluation chapter.
12-604	12	50	38	50	38	A histogram for the sensititviities in the two ensembles would help visualize this. [Benjamin Sanderson, United States of America]	Figure 12.45 shows CMIP3 and CMIP5 ECS and TCR. Because the distribution in CMIP is arbitrary and should not be interpreted as PDF, and we prefer not to show a histogram.
12-605	12	50	55	50	55	Although these ranges are contingent upon mostly arbitrary decisions for the parameter perturbations. [Benjamin Sanderson, United States of America]	Noted. No changes made.
12-606	12	50	55	51	1	Perhaps this is clear already, but it might be worth emphasizing that this tells us that surface parameters are not that important for controlling climate sensitivity (at least the ones which were perturbed), rather than that climate sensitivity may be closely constrained based on such experiments. [Nathan Gillett, Canada]	Taken into account. Added "indicating that those parameters do not strongly control climate sensitivity"
12-607	12	51	4	51	28	Many sentences in this paragraph are difficult to read and should be reworked so that their message appears clearly. For example, in the last sentence of the paragraph, "results are less clear" is not a clear wording ! if what is meant is that the above hypothesis is not supported by the new results, explain this clearly. [Philippe Marbaix, Belgium]	Last sentence clarified, rest of comment not specific.
12-608	12	51	4	51	28	Since the spread in models is dominated by cloud feedback, and constraints on this are discussed in 7.2, it would make sense to refer readers to this section for more details on that (we currently have an analogous pointer to Chapter 12). In particular, section 7.2.4.3 echoes the conclusions of 12.5.3.1 that observations of the present-day climate system as yet have not provided any useful constraint on the cloud feedback (ergo climate sensitivity). [Steven Sherwood, Australia]	Added cross reference.
12-609	12	51	18	51	18	Need to be careful about stating this interpretation without a citation. In any case, it should be moved to the next but one paragraph beginning "The main difficulty" [Benjamin Sanderson, United States of America]	Taken into account. Reworded.

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12-610	12	51	19	51	19	Suggest "calibrate" or "tune" instead of "evaluate". [Jouni Räisänen, Finland]	Deleted, no longer applies.
12-611	12	51	19	51	20	If this information had only been used to evaluate GCMs, this would have no effect on the spread of their climate sensitivities. Presumably what the authors mean to suggest is that the available information may have been used to tune GCMs. [Nathan Gillett, Canada]	Deleted, no longer applies.
12-612	12	51	35	51	37	This sentence does not properly reflect the findings of the paper, which show no geographically uniform impact of the magnitude of temperature change. Suggestion: "Räisänen et al. (2010) report only small (10-20%) reductions in cross-validation error of simulated 21st century temperature changes when weighting the CMIP3 models based on their simulation of the present-day climatology, and note that the effects of the weighting on real-world temperature projections are sensitive to the predictor variable and to some extent the observational dataset used." [Jouni Räisänen, Finland]	Reworded as suggested.
12-613	12	51	39	51	45	Break up and reword this very long complex sentence. [Robert Colman, Australia]	Taken into account.
12-614	12	51	39		52	This is a powerful and troubling paragraph, if true. I examine the para; text from para in quotes; my comments not in quotes. "The main difficulty in constraining AOGCMs with climatological data is measurement uncertainties, sparse coverage in many observed variables, short time series for observed trends," The necessary measurements need to be specified. Arguably the key uncertainty is forcing data. Temperature change data are availabe from observations as are ocean heat content data. That is all that is needed to infer equilibrium sensitivity from observations. "lack of correlation between observed quantities and projected past or future trends (Jun et al., 2008b; Knutti, 2010; Knutti et al., 2010a; Tebaldi and Knutti, 2007), " arguably this speaks as much if not more to deficiencies in the models, not in observations. "the ambiguity of possible metrics and the difficulty of associating them with predictive skill (Eyring et al., 2005; Gleckler et al., 2008; Knutti et al., 2010b; Parker, 2006; Pierce et al., 2009; Pincus et al., 2008; Reichler and Kim, 2008) again a model issue, not an observational issue. "and computational cost of running large samples of coupled state of the art models at high resolution. " certainly a modeling issue. So it would seem that the first sentence of the para is not supported. "In addition the sample of structurally different models is small and many models share biases. The effective number of independent models is therefore likely to be smaller than the actual number of models (Annan and Hargreaves, 2011b; Jun et al., 2008a; Knutti et al., 2010b; Masson and Knutti, 2011; Tebaldi and Knutti, 2007). Another issue is selection bias, i.e., the fact that statistical methods that test for correlations based on a large number of metrics, patterns and variables are bound to find cases with significant correlations that appear by chance and are not robust when tested in a different ensemble. This is particularly important for small ensembles like CMIP3 and if many predictors are screened (DelSole and Shukla, 2009; Huber et al., 2011; Raisanen et al., 2010)." This part of the para should be very troubling to the modeling community and to the broader community that relies on the models. [Stephen E Schwartz, USA]	Taken into account. Climate models are strongly constrained by observations, but the paragraph was meant to say why it is difficult to constrain them much further. Reworded to: "The main difficulty in constraining AOGCMs with climatological data to a range much narrower than that covered by the CMIP ensemble..." Uncertainties in observations are just one of many points listed, there is no implication that everything is due to observational uncertainty. We do not interpret that paragraph as troubling, but as an honest assessment of why it is difficult to narrow the range of uncertainty as covered by models.
12-615	12	51	46		48	Annan and Hargreaves 2011 did not discuss the "effective number of independent models", a concept which seems to be rather vague, incoherent and inconsistent across the rest of the literature you cite here. I suggest	Taken into account. "Effective number of independent models" removed.

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						it would be better to omit this paragraph altogether at this time, since this concept does not appear to be usefully quantified anywhere in the literature. Note that Masson and Knutti provided no quantitative analysis of the number of independent models. At a bare minimum, surely you must mention that this concept is not clearly defined or understood. [James Annan, Japan]	
12-616	12	51	54	51	56	First, there is some evidence that the range of TCR can be more closely constrained using observations than it could at the time of the AR4 (10.9.1). Second, even if the ranges derived using observational constraints are no narrower than the spread of the model ensemble, the uncertainties are objectively derived based on agreement with observations, rather than based on the spread across an ad-hoc ensemble of models. [Nathan Gillett, Canada]	Noted. Results by Gillett et al. GRL 2012 appear to be rather specific to the model used and do not hold as clearly for other models.
12-617	12	51	54	52	7	The estimate of the likely range of climate sensitivity seems to be based essentially on models. On lines 54-55 you state the the model based range cannot be significantly narrowed by constraints from observations. But the equally important issue is to what degree the observations disagree with sensitivites outside the model based range. Or, in other words, can the same range of sensitivity be defended based on observations alone? There is at least a theoretical possibility that all models are biased. [Henning Rodhe, Sweden]	Incorrect statement. Assessment is based on models, observed temperature trends, Pinatubo, paleoclimate, climate feedbacks and process understanding. See Box 12.1.
12-618	12	52	3	52	3	As a recent study claiming that high climate sensitivity cannot be ruled out (due to the forcing uncertainty), Tanaka et al. (2009, GRL, http://www.agu.org/pubs/crossref/2009/2009GL039642.shtml) could be cited here. [Katsumasa Tanaka, Switzerland]	Not considered. This section is about climatological mean state constraints. Constraints based on observed warming are in the attribution chapter.
12-619	12	52	12	52	12	Section 12.5.3.2: Could the following be introduced in this subsection where appropriate? When one considers the forcing uncertainty more fully by allowing it to change over time, high climate sensitivities cannot be ruled out. However, climate sensitivity lower than 2 deg is still unlikely (Tanaka et al., 2009, GRL, http://www.agu.org/pubs/crossref/2009/2009GL039642.shtml). [Katsumasa Tanaka, Switzerland]	Not considered. Constraints based on observed warming and forcing are in the attribution chapter.
12-620	12	52	17			"fast feedbacks"; no; should read "fast responses" but not feedbacks as the term is conventionally used in climate research. [Stephen E Schwartz, USA]	Taken into account.
12-621	12	52	17			"fast feedbacks" is inaccurate terminology, and Chapters 7+8 are using "rapid responses" for this. [Steven Sherwood, Australia]	Taken into account.
12-622	12	52	21	52	21	Suggest add reference to Colman, R.A. and S.B. Power, 2010: Atmospheric feedbacks under unperturbed variability and transient climate change. Climate Dynamics, 34, 919-934, doi: 10.1007/s00382-009-0541-8, which showed structural evolution of feedbacks with warming in an AOGCM. [Robert Colman, Australia]	Taken into account.
12-623	12	52	23	52	23	Definition of "effective climate sensitivity". Not sure you can just leave this important concept to the glossary. [Gareth S Jones, UK]	Not considered due to space limiations.
12-624	12	52	25		33	"full equilibrium". I would argue that if the committed warming is 85 ± 10% of the committed warming, it is not necessary to run to full equilibrium (strictly speaking, steady state); Get the 85% and multiply by 1.15 and you are within 12%. Good enough for all practical purposes. The only problem is need to know the forcing. That is much more uncertain than the 12%. So that needs to be the focus of future research. [Stephen E Schwartz, USA]	Noted. This section is simply assessing what has been done, and is not recommending future research. The 85+-10% is from the RCP scenarios, but this range is much larger when including other scnearios. It depends on the forcing history. Discussion extended.
12-625	12	52	31	52	32	There is a lot more litereature on the linear additivity of the temperature responses to different forcings than that cited here. E.g. Gillett et al., 2004; Sexton et al. 2003; Boer and Yu, 2003. These studies generally find a linear temperature response to most forcings, but some evidence of departures from additivity of the indirect aerosol effect. The precip response may also exhibit departures from linearity - see discussion in section 10.2.1. [Nathan Gillett, Canada]	References to papers and chapter 10 added.
12-626	12	52	35			The state dependent climate sensitivity is discussed in three different chapters, 5, 10, 12, worth consolidating a bit [Gabi Hegerl, UK]	Taken into account.
12-627	12	52	46			Some of these studies (at least, Pagani et al) were indeed from warmer climates. [Steven Sherwood, Australia]	Reworded to clarify.

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12-628	12	52	47	52	48	Why would we want to estimate climate sensitivity for a warmer world than current? Climate sensitivity in the context of future climate change is almost always expressed relative to preindustrial climate. [Nathan Gillett, Canada]	Reworded to clarify.
12-629	12	52	57	52	57	Suggest add reference to Colman, R.A., and B.J. McAvaney, 2009: Climate feedbacks under a very broad range of forcing, Geophysical Research Letters, 36, L01702, doi: 10.1029/2008GL036268., which evaluates feedback and sensitivity under much warmer conditions. [Robert Colman, Australia]	Added reference further up in the text where state dependence is discussed first.
12-630	12	53	2			Section 12.5.4 (especially 12.5.4.2) : I think that the discussion on stabilisation is very important, and that care should be taken to avoid giving the impression that indices such as TRCE are most of what is needed to provide policy-relevant information on how to achieve possible targets. The TRCE seems very useful to have a rough idea of the problem, its magnitude and dependence on cumulative emissions, but the discussion on its limitation would likely benefit from being more integrated with the general presentation and numbers. Indeed, I think that the problem with TRCE is not only a classical problem of uncertainty in models, it is also one of 'definition' : the large range of values that can be obtained from fig 12.46 is probably explained by differences in non-CO2 forcings, in particular aerosols. I suspect that the lower TRCEs are biased because they likely comes from cases (such as all cases in the past) in which emissions are still going on, together with short lived forcers including aerosols. In a future stabilisation context, these aerosols should logically be much reduced, together with emissions, so that the TRCE would be larger. This is illustrated in figure 12.3, where the "zero emissions" scenario shows a jump in TRCE mostly due to aerosols. Whenever aerosols are strongly decreased, this would logically lead to an higher TRCE as compared to a constant aerosols case. While cumulative emissions are an useful concept, the TRCE cannot provide the necessary information to evaluate the risks of failing to stay below a given temperature target in a given policy scenario. I would stress this in order to avoid inappropriate uses or interpretations. Evaluating scenarios others than the RCPs will require some accounting of all the forcing changes, presumably using a method that will be calibrated to the CMIP5 runs in all their key aspects - not just carbon emissions, but also aerosols, CH4, etc. (as currently explained at the end of section 12.5.4, regarding Rogelj at al 2011 etc., - I would expect this to be broadened later and based on CMIP5) [Philippe Marbaix, Belgium]	Accepted. Paragraph added stating that the uncertainty in TRCE is due to uncertainties in feedbacks and ocean heat uptake (i.e. TCR) and carbon cycle climate feedbacks. Non CO2 forcings need to be considered separately. Emission reductions for multi gas scenarios are shown in Fig. 12.47.
12-631	12	53	4	53	5	The second half of this sentence needs improvement: uncertainties between emissions and climate target do not only relate to climate aspects - what does "equilibrium climate response to emissions" mean ? (is there such thing as an equilibrium response to emissions ? I would expect that we need constant concentrations to define an equilibrium). You may perhaps just write "equilibrium climate response" and mention that there are other factors taken into account when linking with emissions, such as C-cycle) [Philippe Marbaix, Belgium]	Accepted, rewritten to clarify.
12-632	12	53	7	53	7	Suggestion: "than temperature OR atmospheric CO2" (only in the very long term could these two targets become identical) [Philippe Marbaix, Belgium]	Accepted, text changed accordingly
12-633	12	53	14	53	15	"returning to a level before impacts become too large" is a relatively strange concept - is it needed to introduce such suggestion here ? By contrast, limiting the rate of climate change may possibly be relevant. The sentence would benefit from being more general - eg "Avoiding impacts from climate change beyond a certain level requires Limiting climate change impacts is a key requirement to curb its impacts" (= introduction without mentioning magnitude, rate, duration, "appropriate level", etc.) [Philippe Marbaix, Belgium]	Accepted. Reworded to clarify.
12-634	12	53	21	53	21	Perhaps cite the original source, if possible. [Benjamin Sanderson, United States of America]	References are provided at the end of the sentence.
12-635	12	53	34	53	36	The link between cumulative carbon emission and peak warming has been estabilished from climate models. In addition, analytical theory has been used to connect cumulative carbon emissions to the longterm equilibrium radiative heating from CO2: radiative heat flux from CO2, F=5.3Wm-2*Iemission/IB where Iemission is the cumulative carbon emissions and IB is the buffered carbon inventory. Reference is Goodwin, P., R.G. Williams, A. Ridgewell and M.J. Follows, 2009. Climate sensitivity to the carbon cycle modulated by past and future changes in ocean chemistry. Nature Geosciences, doi:10.1038/ngeo416. [Richard G Williams, UK]	Accepted. Reference added and summarized in the revised chapter.

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12-636	12	53	34	53	55	Cumulative emissions are proportional to global mean warming, but not to peak atmospheric CO2. For example an instantaneous emissions of 1 TgC would give a much higher peak CO2 than 1Tg C emitted over 100 years, but the resulting warming would be about the same after 20 years or so. If there is a study showing proportionality of peak CO2 to cumulative emissions, then cite it, but I don't think there is, in which case delete this. [Nathan Gillett, Canada]	Accepted, text changed accordingly
12-637	12	53	36	53	36	Should probably be: "ratio of global temperature change" [Jouni Räisänen, Finland]	Accepted, text changed accordingly
12-638	12	53	40	53	41	The last sentence in this paragraph comes a bit abruptly and this point could be explained better; with reference to treatment of this issue in chapter 8. Reisinger et al. (2011) also studied this issue; which could also be mentioned here. (Reisinger, A., M. Meinshausen, M. Manning, and G. Bodeker, 2010: Uncertainties of global warming metrics: CO2 and CH4. Geophysical Research Letters, 37) [Jan Fuglestvedt, NORWAY]	Rejected. This reference to Caldeira is given because it mentions that the GWP is independent of the scenario. This discussion is not about the use of the GWP to compare gases.
12-639	12	53	40	53	41	What is the meaning of "global warming potential" here? absolute GWP ? As the term GWP is widely used, any related term should be used in a clear and careful way. [Philippe Marbaix, Belgium]	Yes, the text refers to GWP, which is defined in the IPCC glossary.
12-640	12	53	43	53	44	Stabilising climate requires near zero emissions, not constant atmospheric concentrations of GHGs. Constant concentrations will give rise to ongoing warming - see commitment simulations in AR4. Zero emissions and constant concentrations of CO2 are not the same thing. Due to the thermal inertia of the oceans, stabilising climate in the near term requires declining atmospheric CO2. [Nathan Gillett, Canada]	Taken into account. The statement was unclear.
12-641	12	53	43	53	45	I think that a clear distinction between stabilisation of temperature and equilibrium needs to be done, and is lacking in this sentence : a rough stabilisation of global average temperature does not require stabilisation of concentrations, mainly due to ocean thermal inertia. The sentence is therefore only true in the very long term (which is valuable only to give some general idea on the potential commitment, as in practice there is no reason to beleive that the forcing will remain fully constant over millenia, so equilibrium might never be acheived). I think that it is very important to consider the scale of the century (human life) in the discussion about "stabilisation", and to account for the possibility of "overshooting" concentrations, which corresponds to an equilibrium T that is never reached due to subsequent decrease of the concentration. This was already noted in AR4, and is the basis of scenarios like RCP2.6. [Philippe Marbaix, Belgium]	Noted,clarified.
12-642	12	53	43		48	Stabilization. Once again the draft fails to recognize that going to zero emissions of CO2 will result in decrease of aerosol forcing and resultant rapid warming. [Stephen E Schwartz, USA]	Rejected, this section is about long-term stabilisation, not about the short term effect of suppressing aerosols. This effect is discussed in section 12.5.2
12-643	12	53	44	53	44	requires the stabilization of atmospheric concentrations. [Benjamin Sanderson, United States of America]	Accepted.
12-644	12	53	46	53	48	Matthews et al. (2009), and Gillett et al. (2011) are relevant here too. N. P. Gillett, V. K. Arora, K. Zickfeld, S. J. Marshall and W. J. Merryfield, Ongoing climate change following a complete cessation of carbon dioxide emissions, Nature Geosci., 4, 83-87, 2011. H. D. Matthews, N. P. Gillett, P. A. Stott, K. Zickfeld, The proportionality of global warming to cumulative carbon emissions, Nature, 459, 829-832, 2009. [Nathan Gillett, Canada]	References added.
12-645	12	53	50	54	42	How can degC/TtC be proper units? It seems to assume CO2 forcing is proportional to linear concentration, not the well known ln(CO2). [Stephen Gaalema, USA]	Rejected. degC/tgC is simply the warming simulated for a cumulated CO2 emission of 1TgC. This has nothig to do with the forcing being a log function of CO2 concentration.
12-646	12	53	55	53	55	Figures shoud be the same order as the text, or perhaps combined onto a single plot. [Benjamin Sanderson, United States of America]	Accepted. Text order changed.
12-647	12	53	56	53	56	Is the EMIC range for TCRE or for the peak warming? [Jouni Räisänen, Finland]	Text clarified.
12-648	12	54	6	54	9	This approach to estimating TCRE is not valid. TRCE is only constant because cumulative airborne fraction changes with time and with amount of CO2 emitted. This calculation uses a temperature change evaluated at CO2 doubling and an AF at 1 TtC emissions. If the authors are to use the current value of airborne fraction of	Taken into account. Sentence removed.

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						cumulative emissions, then they should use the present day value of CO2-attributable warming. This approach was taken by Matthews et al. (2009), and Allen et al. (2009). Alternatively the authors need to know the airborne fraction of cumulative emissions in a 1% CO2 simulations, and multiply this by TCR. [Nathan Gillett, Canada]	
12-649	12	54	8	54	9	NO!! You can't assume airborne fraction will remain roughly constant. Results in Ch6 show it can change massively (25%-85% perhaps) by 2100. And this is driven largely by the emissions scenario, not necessarily climate feedbacks or saturating sinks. The further the emissions profile is from an exponential increase, the more AF will change. There are many studies (Knorr, Raupach etc) that show a historically constant AF is mainly a result of approximately-exponentially increasing emissions. Anything like a stabilisation scenario will see reducing emissions and a big drop in AF. Also, the long-term cumulative AF is very different from the short term year-to-year AF [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Sentence removed.
12-650	12	54	14	54	18	These statements require citations. [Benjamin Sanderson, United States of America]	Rejected. This is the assessment of the evidence discussed in the previous paragraphs
12-651	12	54	15	54	18	This implies that PRCE is higher for larger cumulative emissions. But this is not the case. PRCE is lower for larger cumulative emissions. See figure 12.46e and 12.46f which show temperature vs emissions curves curving down for high cumulative emissions. [Nathan Gillett, Canada]	Taken account. We focus on the transient response in the revised text, which is covered wll in the literature. The peak warming and long term warming is not briefly mentioned. PCRE/ECRE are no longer used as acronyms.
12-652	12	54	20		22	"The results by Schwartz et al. (2011) are inconsistent with the above evidence and are questioned in the literature (Knutti and Plattner, 2011). They are not based on a climate model and neglect the relevant response timescales." This statement is presented out of context and without exposition of the argument presented by Schwartz et al. It would seem that more appropriate for the present document would be a brief synopsis of the argument presented by Schwartz et al. followed by an assessment of that argument: Schwartz et al (2010) argued that for forcing by the long lived greenhouse gases of 2.6 W m-2 together with an equilibrium climate sensitivity of 3 K, then, assuming no other forcings, attainment of equilibrium response to the forcing would result in an increase in global mean surface temperature (GMST) of 2.1 K, well greater than the observed 0.7-0.8 K increase, a difference that they denoted a warming discrepancy. Schwartz et al argued that the planetary energy imbalance, which they took as 0.4 K, is subtractive from the forcing and cannot account for the discrepancy. They concluded that the discrepancy must be due to a combination of offsetting aerosol forcing and/or lower equilibrium sensitivity, although they noted that the large uncertainty range in the AR4 estimate of total forcing, due mainly to the uncertainty in aerosol forcing, did not preclude an even greater climate sensitivity. Schwartz et al also argued that an abrupt cessation of emissions of CO2 and of associated aerosol precursors would result in a rapid net increase in forcing and, if the equilibrium sensitivity were 3K, a resultant rapid increase in GMST. An assessement of that argument might then be presented: " The results by Schwartz et al. (2010) are inconsistent with the above evidence" with some statement of particulars. Only then would it seem appropriate to refer to Knutti and Plattner (2011). The arguments of Schwartz et al were questioned by Knutti and Plattner (2011) as not based on a climate model and neglecting the relevant response timescales. Knutti and Plattner (2011) presented calculations for their model (sensitivity near 3 K) showing that immediate cessation of both CO2 emissions and aerosol forcing results in a committed warming above preindustrial of about 1.6 K. However it would seem that the calculation reported by Knutti and Plattner supports the original assertion of Schwartz et al (2010), rather than refuting it, as noted by Schwartz et al. (2011). [Stephen E Schwartz, USA]	Once emissions are stopped then atmospheric CO2 decreases, so it is inappropriate to assume constant GHG forcing to estimate the equilibrium warming. We do not feel that Knutti and Plattner support the original assertion by Schwartz, quite the opposite. Knutti and Plattner argue that the conclusions by Schwartz are incorrect. Here we simply present the original result by Schwartz, the criticism by Knutti and Plattner, and the response by Schwartz, without implying who is correct. While we appreciate the detailed comments by the reviewer, space does not allow us to discuss a single paper such great detail.

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12-653	12	54	20			References:. Schwartz S. E., Charlson R. J., Kahn R. A., Ogren, J. A., and Rodhe H. Why Hasn't Earth Warmed as Much as Expected? J. Climate 23, 2453-2464 (2010); doi: 10.1175/2009JCLI3461.1. Reply To Comment on "Why Hasn't Earth Warmed as Much as Expected?" by R. Knutti and G.-K. Plattner. Schwartz S. E., Charlson R. J., Kahn R. A., Ogren, J. A., and Rodhe H. J. Climate. Accepted, October, 2011. Journal of Climate 2011 ; doi: http://dx.doi.org/10.1175/2011JCLI4161.1 [Stephen E Schwartz, USA]	Part of earlier comment.
12-654	12	54	21	54	22	The reference "Schwartz 2011" is missing and most probably wrong - the paper is from 2010. In addition, thee was a reply to the comment by Knutti and Plattner - it could perhaps (?) be mentionned to be comprehensive. Regarding the discussion itself, I would rather tend to agree that there are problems with the Schwartz et al. paper. However, while I have no definitive personal conclusion, I doubt that writing that their paper is "inconsistent with the above evidence" is sufficient to inform the reader : if a minority of authors (only those that signed the paper ?) thinks that there is more uncertainty, especially related to aerosols, than stated here, the ideal situation would be to capture in a few words why they are wrong (just due to neglecting timescales?). [Philippe Marbaix, Belgium]	Reference corrected. The discussion was extended to give more detail. Both positions are discussed without saysing who is wrong. The reference to the reply is added. Space constraints prevent us from discussing this in greater detail.
12-655	12	54	24	54	33	The concept of an equilibrium response to cumulative emissions seems flawed to me. After a cessation of carbon dioxide emissions, the system is never in equilibrium until the CO2 has declined to its preindustrial concentrations after tens of thousands of years. CO2 is removed from the atmosphere at a progressively decreasing rate, and is progressively fluxed into the ocean. The atmosphere either warms then cools or cools progressively after a cessation of emissions, but on a very long timescale. Warming might not peak for several thousand years after a cessation of emissions. There is initially a flux of heat into the ocean, and after several centuries or millenia this will reverse sign and there will be a heat flux out of the ocean. It is possible to pick a time horizon of say 1000 years, and evaluate the ratio of warming to cumulative emissions at that time. But I don't think this should be called an equilibrium response, because the system is not in equilibrium. Has ECRE or something like it been defined in the literature, or is this definition new? Either way, I would argue that it should be called something else if it is retained. [Nathan Gillett, Canada]	Taken into account. Indeed there is no true equlibrium, but the warming after 1000yrs (i.e. long after emissions have stopped) is informative. The discussion is kept as a comparison of the transient with the long term (1000yr) warming, but the term equilibrium is removed.
12-656	12	54	29	54	29	Tanaka and Raddatz (2011, Climatic Change Letters, http://www.springerlink.com/content/k160023102g83v4v/) can be cited here as they investigate the uncertainty in the magnitude of an abrupt warming caused by an elimination of aerosols. [Katsumasa Tanaka, Switzerland]	Rejected, this section is about long-term stabilisation, not about the short term effect of suppressing aerosols. This effect is discussed in section 12.5.2 .
12-657	12	54	31	54	33	I think it is too simplistic to say that climate and carbon feedbacks increase for high cumulative emissions. The radiative forcing due to CO2 is dependent on the logarithm of the CO2 concentration, so a unit change in atmospheric CO2 will have a larger effect for low pCO2 than for high pCO2. This tends to reduce the temperature response per unit cumulative emissions for high cumulative emissions, and is the reason why the temperature curves curve downward in Figs 12.46e and 12.46f. [Nathan Gillett, Canada]	Accepted, text changed accordingly.
12-658	12	54	45	56	15	I would suggest expand this box to include transient climate sensitivity, evaluated as proportionality coefficient of temp increase and forcing. This is an important new development that lends great insight to interpretation of climate change over industrial period. I would argue that transient climate sensitivity is more important than transient climate response, which is limited in applicability to a very idealized forcing scenario that is unrealistic in the real world (1% per year increase in CO2, compounded). Transient climate sensitivity is pertinent to any forcing profile. References: Gregory JM, Forster PM (2008) Transient climate response estimated from radiative forcing and observed temperature change. J Geophys Res 113:D23105. doi:10.1029/2008JD010405 Held IM, Winton M, Takahashi K, Delworth T, Zeng F, Vallis GK (2010) Probing the Fast and Slow	We include the concept of transient climate sensitivity (or climate resistance as termed by Gregory) in the revised chapter, but note that the transient climate sensitivity (K/Wm-2) is not constant for any forcing profile. It depends on how close the system is to equilibrium. Once the forcing is kept constant, the temperature continues to rise, so the scope of this quantity is limited. It further assumes that all forcings have equal efficacies. We agree that the 1%/yr CO2 is not realistic, but it is not intended to be a scenario. Rather, it is a benchmark number by which models can be compared, and which relates strongly to the transient warming. In contrast to the proposed K/Wm-2, the transient climate response TCR is well defined.

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						Components of Global Warming by Returning Abruptly to Preindustrial Forcing. J Climate 23:2418-2427. doi:10.1175/2009JCLI3466.1 Padilla, LE, Vallis GK, Rowley CW, (2011) Probabilistic Estimates of Transient Climate Sensitivity Subject to Uncertainty in Forcing and Natural Variability. J. Climate, 24: 5521–5537. doi: http://dx.doi.org/10.1175/2011JCLI3989.1 Schwartz S. E. (2012) Determination of Earth's transient and equilibrium climate sensitivities from observations over the twentieth century: Strong dependence on assumed forcing. Surveys Geophys. In press. http://www.ecd.bnl.gov/steve/pubs/ObsDetClimSensy.pdf Other terminology, e.g., "transient climate response," has been used Dufresne J-L, Bony S. (2008) An assessment of the primary sources of spread of global warming estimates from coupled atmosphere-ocean models. J. Climate 21: 5135-5144. doi: 10.1175/2008JCLI2239.1 [Stephen E Schwartz, USA]	The results by Schwartz 2012 are included in the TCR summary figure assuming a radiative forcing of 3.7Wm-2 for 2xCO2. Note that Padilla et al. in fact estimate the temperature change itself, not the ratio of temperature to radiative forcing. Details to the different studies are given in Chapter 10.
12-659	12	54	47			Box 12.1: The assignment of probabilities and likelihods to ECS seems to be unjustified. The IPCC Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties stresses that both agreement and evidence (in sum: confidence) must be high before likelihoods can be assigned and calibrated language (likelihood scale) be used. I take it that neither agreement nor evidence are sufficiently high to assign likelihoods. The following comments explain why. [Gregor Betz, Germany]	Rejected. Evidence is based on many different studies and independent lines of evidence. Disagreement between different estimates can be explained by different assumptions in different studies. Assessment of ECS and TCR in AR4 is widely accepted as a consensus.
12-660	12	54	47			Box 12.1: The authors overestimate the evidence because they disregard the major limitations and unwarrented (a-priori) assumptions of PDF estimates discussed elsewhere in this and other chapters. See, e.g.: 9.2.3; 11.4.7; 12.2.2. [Gregor Betz, Germany]	Not considered. Limitations and unwarranted assumptions claimed without being specific.
12-661	12	54	47			Box 12.1: The authors overestimate the evidence because they disregard the methodological limitations that are stressed in the IPCC Good Practice Guidance Paper on Assessing and Combining Multi Model Climate Projections. ("It is problematic to regard the behavior of a weighted model ensemble as a probability density function (PDF).") [Gregor Betz, Germany]	Not considered. Limitations and unwarranted assumptions claimed without being specific. Most studies that provide constraints on climate sensitivity do so without weighting AOGCMs.
12-662	12	54	47			Cumulative emissions are proportional to global mean warming, but not to peak atmospheric CO2. For example an instantaneous emissions of 1 TgC would give a much higher peak CO2 than 1Tg C emitted over 100 years, but the resulting warming would be about th	Correct. But the text does not refer to peak CO2. No changes needed.
12-663	12	54	47			Box 12.1: The authors ignore the lack of agreement concerning the assignment of probabilities to ECS in general. Several studies have voiced doubts that this really makes sense, e.g.: 1. Stainforth, D. A., M. R. Allen, et al. (2007). "Confidence, uncertainty and decision-support relevance in climate predictions." Philosophical Transactions of the Royal Society a-Mathematical Physical and Engineering Sciences 365(1857): 2145-2161; 2. Gregor Betz, "Probabilities in climate policy advice: A critical comment", Climatic Change, 85(1-2), November 2007, pp. 1-9; 3. Wendy S. Parker, Predicting weather and climate: Uncertainty, ensembles and probability, Studies In History and Philosophy of Science Part B: Studies In History and Philosophy of Modern Physics, Volume 41, Issue 3, September 2010, Pages 263-272, ISSN 1355-2198, 10.1016/j.shpsb.2010.07.006. [Gregor Betz, Germany]	Not considered. Opinion of the author that is not supported by the vast amount of literature. The AR4 statments on climate sensitivity are generally agreed as being resonable, and if anything are criticised as being overly pessimistic (Annan et al.)
12-664	12	54	47			Box 12.1: In sum, I suggest to rephrase this box in terms of confidence rather than likelihood. The authors might consider whether we have very high confidence that ECS is greater than 1.5, high confidence that ECS is greater than 2 but only medium confidence that it is smaller than 4.5. [Gregor Betz, Germany]	Not considered. Summary statements on climate sensitivity in AR4 are generally accepted and further supported by a wide range of literature since 2007.
12-665	12	54	49	54	53	Before discussing the estimation techniques, the box should define what Equilibrium Climate Sensitivity and Transient Climate Response are. [Jouni Räisänen, Finland]	Taken into account.
12-666	12	54	55	55	36	There is little (no) substance in these paragraphs. One would like to know the basis of the conclusions. What was the evidence, the procedure used in the cited studies. [Stephen E Schwartz, USA]	Synthesis of the various assessments discussed in great detail in different chapters. Cross referencing clarified.

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12-667	12	54	58	55	2	What does this mean? This seems to say that the most direct estimates of warming on centennial timescales are derived using estimates of TCR and ECS. I don't think this is the case. The most direct estimates of future warming come directly form model simulations. Clarify. [Nathan Gillett, Canada]	Statement removed.
12-668	12	55	9	55	9	The PDFs need explanation, perhaps with reference to the original literature [Benjamin Sanderson, United States of America]	Figure or caption will include references. Part of the figure appears in the attribution chapter and is discussed there.
12-669	12	55	25			It seems very odd to portray our work as an outlier here. Sokolov et al 2009, Urban and Keller 2010, Olson et al (in press JGR) have also recently presented similar results (and there may be more as yet unpublished, eg Aldrin at the INI meeting back in 2010). Such "observationally constrained pdfs" were all the rage a few years ago and featured heavily in the last IPCC report, there is no clear explanation for your sudden dismissal of them in favour of what seems to be a small private opinion poll. A more balanced presentation could be: "Annan and Hargreaves (2011a) criticize the use of uniform priors and argue that sensitivities above 4.5°C are extremely unlikely (<5%). Similar results have been obtained by a number of other researchers [add citations from the above]." [James Annan, Japan]	Partly considered. References to Sokolov et al. and Olson et al. added. Urban and Keller only find a strong constraint on the upper bound when ignoring the forcing uncertainty, which is the actual cause for the fat tail. The reference to Zickfeld is retained, not to imply that this is better than other lines of evidence but to support the statement that there is not really a consensus. The sentence makes that very clear.
12-670	12	55	41			Would make sense to use the term Charney sensitivity where these concepts are first introduced earlier in your chapter, then refer back to it here rather than re-explaining a different way. [Steven Sherwood, Australia]	Not considered. Decision was made not to use that term in the report in this context.
12-671	12	55	46			Inserting cumulative emissions in the middle of a para whose lead sentence is paleo seems odd. [Stephen E Schwartz, USA]	Not considered. The paragraph discusses implications of paleoclimate evidence on cumulative carbon emissions.
12-672	12	55	51		53	Estimating TCR (or Eq sensitivity) from obs over industrial period requires forcing. Just limiting to GHGs guarantees the wrong answer. Need aerosol forcing, which, of course, is uncertain. If forcing is 1.95 ± 0.9 two sigma,(range a factor of 2.7) pretty much says you can't do better than that in estimate of TCR etc from observations over industrial period. [Stephen E Schwartz, USA]	Comment unclear. Added "forcing" as a required quantity. Paragraph is only the overall assessment of a much more detailled discussion in the attribution chapter.
12-673	12	55				Box 12.1. Suggest briefly defining the TCR at the beginning of the box. Also, where noting the existence of outlier studies suggesting low sensitivity, aren't there outlier studies suggesting high sensitivity? If so then they should equally be mentioned. [Steven Sherwood, Australia]	Taken into account. TCR now defined. Outliers discussed in detail in the underlying sections
12-674	12	56	1			"several different lines of evidence": specify. [Stephen E Schwartz, USA]	These are given in the above discussion (observed change, Pinatubo, climatology, paleoclimate, model based estimates).
12-675	12	56	29	56	30	The application of cumulative carbon budget in a policy context could be mentioned earlier in section 12.5.4. in order to strengthen and clarify the motivation for discussing this concept. So the points made at line 29-30 could be made earlier. [Jan Fuglestvedt, NORWAY]	Short motivation is given at the start of 12.5.4, but mentioning cumulative carbon without explaining what it is appears unhelpful. Therefore the sections first have to introduce it and explain why the cumulative number is important.
12-676	12	56	45	56	45	Tanaka and Raddatz (2011, Climatic Change Letters, http://www.springerlink.com/content/k160023102g83v4v/) explores the magnitude of warming due to an elimination of aerosols under different climate sensitivities and can be thus cited here. [Katsumasa Tanaka, Switzerland]	Taken into account.
12-677	12	56	51		52	mitigation, economics perhaps inappropriate for WGI [Stephen E Schwartz, USA]	The sentence merely states the obvious fact that other constraints are important, but makes no assessment.
12-678	12	56	54	56	54	imply a temporary overshoot [Benjamin Sanderson, United States of America]	Corrected.
12-679	12	56	57	56	57	The units should be GtCO2eq / year. [Jouni Räisänen, Finland]	Corrected.
12-680	12	57	1	57	4	What about the role of forcings other than CO2? The numbers quoted assume zero net forcing from these other factors, but aerosols are likely to decrease in the future, whereas concentrations of non-CO2 GHGs will increase, giving an overall warming from non-CO2 forcings. This needs to at least be mentioned, even if the	Taken into account. Sentence added as a caveat.

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						effect can't be quantified. [Nathan Gillett, Canada]	
12-681	12	57	2	57	4	Are these numbers for CO2 alone or for all greenhouse gases in CO2 equivalents? [Jouni Räisänen, Finland]	Taken into account. Clarified that the cumuative carbon constraint considers only CO2.
12-682	12	57	19	62	19	The selection criteria for the inclusion of variables in this section are unclear. On the selection issue: Section 12.5.5.5.1 cites Solomon et al. (2009) in support of the idea that precipitation changes are irreversible on human timescales. But, due to the long lifetime of CO2-induced climate chagne, this argument applies equally well to all aspects of CO2-induced climate change, so why is it only applied to megadroughts? I would suggest dividing this section into two - irreversibility, and abrupt changes. I would start the section on irreversibility with a discussion of how CO2-induced climate change is irreversible on centennial timescales, and then discuss any climate elements which would not return to their unperturbed states even after CO2 returned to its preindustrial conditions after ~30000 years, such as ice sheets (forests, permafrost and clathrates might also be relevant here if the timescales are long enough). Given this long-lifetime of CO2-induced climate change irreversibility on shorter timescales is mainly of academic interest, and deserves less attention. Abrupt changes could then be treated separately. AMOC and sea ice have been suggested as exhiiting abrupt changes, but the consensus is that they will not. Ice sheets could exhibit abrupt changes. I am not convinced that megadroughts or monsoon circulation are any more irreversible or abrupt than other climate elements not considered here. [Nathan Gillett, Canada]	Taken into account - In this section we use a definition of abrupt climate change as follows: "as a large-scale change in the climate system that takes place over a few decades or less, persists (or is anticipated to persist) for at least a few decades, and causes substantial disruptions in human and natural systems." In the revised text we recognize that other definitions exist. We have renamed the section as "Potentially Abrupt or Irreversible Changes". We believe it is important to assess those elements within the Earth system that have been proposed in the literature as potentially being abrupt or irreversible (see the new Table). Rather than breaking the section into two, we have reoganized it slightly.
12-683	12	57	19	62	19	There seems to be a tendency for this section to seek out evidence for the existence of abrupt changes, rather than focusing on a balanced assessment of available evidence. [Nathan Gillett, Canada]	Rejected - We examine the literature that argues a particular element is abrupt or not. Our summary table now provides our overall assessment.
12-684	12	57	19			SECTION 12.4 and 12.5.5: It's important to be clear what this section is about. The title of the section is clear enough: abrupt change and irreversibility. However what it contains is a discussion of a number of vulnerable elements of the climate system. In some cases (e.g. AMOC, ice sheets, Arctic sea ice) there is a potential vulnerability to abrupt or irreversible change, but in others (particularly megadroughts and monsoons) it's more a case of an important system that might respond to climate change but not in a particularly abrupt or irreversibe way. So e.g. for AMOC the discussion is now split between two parts of the chapter (12.4.7 and 12.5.5), and currently inconsistent between the two parts. The sea ice text makes it clear what is discussed where, and there is no duplication, while monsoons are discussed in 12.5.5 but not in 12.4.4. It's not easy to find a tidy solution for this, but I think it would be useful and possible to get a bit more consistency about what aspects of the different climate elements are discussed in 12.4 and what in 12.5.5. [Richard Wood, UK]	Taken into account - We believe it is important to assess those elements within the Earth system that have been proposed in the literature as potentially being abrupt or irreversible (see the new Table). We have removed the discussion of abrupt MOC changes in 12.4.7 and exclusively deal with that here.
12-685	12	57	23	57	37	In line with the former statement: I suggest to include a few lines at the beginning of 12.5.5 in which limitations are mentioned. Abrupt changes may results from feedbacks and nonlinearities that are resolved in current models (ice sheet dynamics), or may occur in different areas of phase space than is attained by the model. In the latter case models can not be used to infer the likelihood of abrupt changes. This may apply to the AMOC, Monsoons, vegetation dynamics. [Sybren Drijfhout, Netherlands]	Accepted
12-686	12	57	29	57	32	This paragraph is based on a single study, and the discussion which follows argues against the likely existence of tipping points in most of the systems listed. [Nathan Gillett, Canada]	Taken Note - this was not meant to be the case. The paragraph has been modified to point out that this section examines those elements within the Earth system that have been proposed in the literature as potentially being abrupt or irreversible
12-687	12	57	29	57	32	This is better expressed in the summary on page 8 "Several components or phenomena in the climate system could potentially exhibit abrupt or non-linear behaviour" . There is reasonable body of evidence that the Atlantic overturning circulation and the Greenland Ice sheet might have so -called "tipping points" , albeit on very different timescales, but as later text shows, very little evidence that Arctic sea-ice does. [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted. This paragraph has been modified.
12-688	12	57	39	57	53	This section comes to rather different conclusions about the probability of an abrupt slowdown in the THC than 12.4.7.2 and the ES, which concludes than an abrupt transition in the 21st century is very unlikely. This assessment seems one-sided compared to 12.4.7.2. [Nathan Gillett, Canada]	Taken Note - The relevant paragraph of section 12.4.7.2 has been brought into this section. Our overall assessment remains unchanged (from 12.4.7.2) but the inconsistencies have now been

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							addressed.
12-689	12	57	41	7	53	There are inconsistencies between this section and lines 21-41, p42 (same chapter) regarding the stability of the AMOC, which should be resolved. [Chris Roberts, Uk]	Taken Note - The relevant paragraph of section 12.4.7.2 has been brought into this section. Our overall assessment remains unchanged (from 12.4.7.2) but the inconsistencies have now been addressed.
12-690	12	57	41	57	41	I think it should be mentionned when the results are from EMIC and when they are from AOGCMs. Study by Hawkins et al. (2011) can also be noticed concerning the hysteresis diagram and our position under present-day conditions using an AOGCM. Moreover, while the beginning of the paragraph indicates that the position on the hysteresis diagram is model dependent, at the end , two studies using state-of-the art AOGCMs showing a large effect of Greenland ice sheet melting (models closer to the bifurcation point) are ignored (Fichefet et al. 2003, Swingedouw et al. 2007). I believe the conclusion shoud be consistently weakened concerning the effect of this melting. (Fichefet, T., C. Poncin, H. Goosse, P. Huybrechts, I. Jansses, and H. Le Treut (2003), Implication of changes in freshwater flux from the Greenland ice sheet for the climate of the 21st century, Geophys. Res. Lett., 30(17), 1911, doi:10.1029/2003GL017826.) [Didier Swingedouw, France]	Taken Note - The Rahmstorf reference is with respect to EMICs. Hawkins et al and Swingedouw et al. 2007 are now cited. There are some concerns as to the drift in the Fichefet et al model. It has not been cited. There are also some concerns with Swingedouw et al 2007 as it has no Labrador Sea Water formation in the control which means its overturning is already unrealistically weak.
12-691	12	57	41	57	42	While it is true that those models which have performed hysteresis experiments have found hysteresis, it should be pointed out that (for logistical reasons) they have mostly been EMICS which may not have as good representation of physical processes as more complex GCMs. The exception to this is the low resolution GCM FAMOUS (Hawkins et al, 2011). Refs: Hawkins, E., R. S. Smith, L. C. Allison, J. M. Gregory, T. J. Woollings, H. Pohlmann, and B. de Cuevas, 2011: Bistability of the Atlantic overturning circulation in a global climate model and links to ocean freshwater transport, Geophys. Res. Lett., 38, L10605, doi:10.1029/2011GL047208. [Laura Jackson, United Kingdom of Great Britain & Northern Ireland]	Accepted - Hawkins et al has been cited
12-692	12	57	41	57	43	The threshold behaviour has hitherto been largely seen in simpler models (up to EMICs, as discussed in the Rahmstorf 2005 reference). There is an 'urban myth' that AOGCMs don't show the same behaviour due to some feedbacks missing from the EMICs, but actually it's just that nobody has been able to afford to do this type of experiment with an AOGCM before. The recent paper by Hawkins et al. GRL 38, L10605 (2011) shows that the FAMOUS GCM (a low resolution version of HadCM3) has the same behaviour as the EMICs. To my mind this increases confidence in the phenomenon and so it's worth mentioning. [Richard Wood, UK]	Accepted - Hawkins et al has been cited
12-693	12	57	41	57	53	Study by Hawkins et al. (2011, GRL) found bistability of the AMOC to freshwater hosing in a complex (low resolution) GCM. [ED HAWKINS, United Kingdom of Great Britain & Northern Ireland]	Accepted - Hawkins et al has been cited
12-694	12	57	41	57	53	It should be noted that the majority of predictions of the MOC by GCMs have not shown a shutdown (or rapid change). There is some contradiction between this section and section 12.4.7.2 which should be resolved within the sections and in the executive summary. [Laura Jackson, United Kingdom of Great Britain & Northern Ireland]	Taken Note - The relevant paragraph of section 12.4.7.2 has been brought into this section. Our overall assessment remains unchanged (from 12.4.7.2) but the inconsistencies have now been addressed.
12-695	12	57	41	57	53	This paragraph about ocean overturning changes is rather inconclusive. Unclear how close the models are to the threshold that the thermohaline circulation then collapses. What skill do these climate models have which are being used to make this deduction? Are they process models or realistic general circulation models? In practice, as cited on L51, several model studies show a very limited effect of meltwater. [Richard G Williams, UK]	Taken Note - The relevant paragraph of section 12.4.7.2 has been brought into this section. Our overall assessment remains unchanged (from 12.4.7.2) but the inconsistencies have now been addressed.
12-696	12	57	43	57	44	"…global warming will move the climate system towards this threshold." No evidence is cited for this, and I'm not sure there currently is any (indeed I suspect the conclusion may be wrong. Aplologies for self-promotion but we have a paper in the works on this which I'll send to the authors for consideration when it's ready). If there is no evidence I suggest just deleting this sentence. [Richard Wood, UK]	Accepted - statement removed
12-697	12	57	45	57	47	The fresh water transport diagnoostic discussed by Drijfhout and others is potentially a very important climate	Accepted - Drijfhout's and other studies now cited.

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						model metric. It would be very helpful if this assessment could be extended to the CMIP5 models (either here or with a link to Chapter 9). [Richard Wood, UK]	
12-698	12	57	53			A somewhat different kind of commitment/reversibility behaviour has recently been noted by Wu et al. 2011:Extended warming of the northern high latitudes due to an overshoot of the Atlantic meridional overturning circulation Peili Wu, Laura Jackson, Anne Pardaens, and Nathalie Schaller Geophys. Res. Lett., 38, 24, doi:10.1029/2011GL049998, 2011. Here accumulated salinity anomalies mean that if aggressive mitigation/negative emissions follows a period of global warming, the AMOC not only recovers but overshoots its unpertubed value, with implications for the recovery of European climate. This is a new type of behaviour and extends the range of possible AMOC responses, so I think it's worth noting here. [Richard Wood, UK]	Accepted - Wu et al now cited (this result was also found earlier with EMICs).
12-699	12	57	57	57	57	I do not think that the "popular media" should be cited here. Please delete reference to them. [Didier Swingedouw, France]	Accepted - Text revised.
12-700	12	58	7		8	The correct qualifiers need to be used, there is no clear indisputable example in the geological record of the grounding line instability (without climate forcing) leading to an abrupt change in ice-sheet mass. The instability is hypothesised in models but not demonstrated. There is certainly no evidence of the timescale on which it might act. [David Vaughan, UK]	(Refers to page 59, lines 7-8). Taken into account. Will state that theoretical work suggest groundling-line instability to exist in certain conditions.
12-701	12	58	20	58	21	Is the Ridley et al 2007 reference the right one? Or is Ridley et al 2008 "The demise of Arctic sea ice during stabilisation at high greenhouse gas concentrations", Clim. Dyn. 30, 333-341 better? [Richard Wood, UK]	Accepted - The reviewer is correct. The reference is Ridley et al. (2008). Text revised.
12-702	12	58	27			The same result has been seen in another of the CMIP3 models (Ridley JK et al. How reversible is sea ice loss? The Cryosphere 2012 (accepted)), and in one of the CMIP5 models (Ridley JK et al. Understanding the climate response of sea ice in an earth System Model J. Clim. submitted 2012). [Richard Wood, UK]	Accepted - Reference added.
12-703	12	58	35			In contrast to the Arctic, there is evidence from at least two models that loss of Southern hemisphere sea ice may have an irreversible (or long recovery timescale) component. (Ridley JK et al. How reversible is sea ice loss? The Cryosphere 2012 (accepted)), (Ridley JK et al. Understanding the climate response of sea ice in an earth System Model J. Clim. submitted 2012). This should probably get a mention here. [Richard Wood, UK]	Accepted - Mention added.
12-704	12	58	40			The period for which the warming needs to be maintained to lose the ice sheet should be mentioned here. It's quite long and I think that's a very policy-relevant question. It might be most helpful to the reader if this could be linked with the discussion at [p59, l2] on the timescles over which the first irreversibility threshold might be passed. [Richard Wood, UK]	Accepted - Mention added.
12-705	12	58	42	58	42	Define GIS [Benjamin Sanderson, United States of America]	Accepted - Greenland Ice Sheet
12-706	12	58	44	58	44	3.1 +/- 0.8C from which baseline period? [Jouni Räisänen, Finland]	Taken into account - Reference is preindustrial. But this section will be essentially reduced to becoming not much more that a pointer to chapter 13.
12-707	12	58	46			Replace 'sufficient but not necessary' with 'necessary but not sufficient'. [Nathan Gillett, Canada]	Not taken into account - original text is correct. A negative surface mass balance is a sufficient condition for ice sheet decay: If SMB is negative, the ice sheet will disappear (no other positive mass balance component possible). But it is not a necessary condition: An ice sheet can disappear even if the surface mass balance is positive, for example if there is a strong acceleration of ice sheet drainage or basal melt.
12-708	12	58	54		57	The paper by Ridley was extremely informative, but its conclusions (that were clearly stated) have (in my opinion) have always been exaggerated by those wanting to identify tipping points (with the implication that we cross those tipping points at our peril). Actually, the conditions that might push Greenland into a protracted retreat may be more complex. It certainly, is unwise to characterise the “tipping point” in terms of a single	Taken into account - As this section will be strongly reduced, the reference to temperature threspholds will not give any numbers (will refer to chapter 13 for details). Will state however that the possible snowfall

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						temperature increase. It is, for example, possible that temperature increase may promote higher snowfall rates, allowing the ice sheet to equilibrate in a variety of possible equilibria. [David Vaughan, UK]	increase is usually taken into account in these studies aiming at identifiying temperature thresholds (Ridley paper and new Nat Geosc paper by Robinson et al)
12-709	12	59	8	59	8	Weertmann should be Weertman [Philippe Huybrechts, Belgium]	Taken into account.
12-710	12	59	11			Although there is marine ice in East Antarctica, it is much less likely to be vulnerable since the thickness of ice above the floatation is some much greater that destabilisation is less likely. The Le Brocq et al., 2010, did not really add to this debate, but the later papers with new data you cite, certainly have. [David Vaughan, UK]	Taken into account. No longer cited.
12-711	12	59	18			The use of the term "Megadroughts" especially as a section head and as cavalierly as it is used here appears unwise. What is really meant is "very long-term drought", but the amplitude is not necessarily as large as is suggested to a general reader by using "megadrought", and for much of the regions under discussion the certainty of this response is low. I suggest revising to use long-term drought almost everywhere as befits the IPCC's scholarly and balanced approach. [J. David Neelin, United States]	Accepted - We now use the term long term droughts generally, but note that in the literature these have sometimes been referred to as megadroughts.
12-712	12	59	32	59	39	Solomon et al. (2009) calculated precipitation change patterns by scaling 21st century precipitation changes in the CMIP3 ensemble by global mean temperature changes simulated following a cessation of emissions in an EMIC. This approach assumes that precip scales with global mean temperature, which is not completely valid, especially for conditions with declining atmospheric CO2, and also it exceludes any abrupt transitions which are the subject of this section. Gillett et al. (2011) and Frohlicher and Joos (2010) examined precipitation vartions following a cessation of emissions in a coupled model, and reached broadly consistent conclusions. Note that precipitation is no more irreversible based on these experiments than other aspects of CO2-induced climate change. [Nathan Gillett, Canada]	Taken in Account - Gillett et al (2011) and Frohlicher and Joos (2010) have now been cited. See also responses to 12-682, 12-683 and 12-686.
12-713	12	59	50	59	50	Mitchell, J. F. B., 1990. Is the Holocene a good analogue for greenhouse warming? J. Climate, 3, 1177-1192. [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted - Mitchell et al cited
12-714	12	59	50	59	50	Also shown in Mitchell, J. F. B., 1990. Is the Holocene a good analogue for greenhouse warming? J. Climate, 3, 1177-1192. (duplicate of comment 62 which I could not delete) [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted - Mitchell et al cited
12-715	12	60	3	60	3	The statment "rapid AMOC weakening which is considered very unlikely during the 21st century" is stated with too much confidence. It is inconsistent with chapter 9, p30, lines 42-44: "there is evidence that a bias in ocean fresh water transport seen in various climate models may make the Atlantic Meridional Overturning Circulation (AMOC) overly stable in current models (Weber et al., 2007)" and section 12.5.5.2 (this chapter): "Moreover there is some indication that most climate models may overestimate the stability of the Atlantic ocean circulation (Drijfhout et al., 2010; Hofmann and Rahmstorf, 2009)". [Chris Roberts, Uk]	Taken into account - This same comment was made by the same reviewer earlier (12-532). The AMOC threshold discussion has been moved entirely to 12.5.5.2 where the discussion of model sensivity exists. Note that an assessment that it is "likely" that the AMOC would not undergo an abrupt transition direcly implies that there would be a 1 in 3 chance that it would collapse this century. There is no evidence to support a 1 in 3 chance of the AMOC collapsing in the 21st century. The additional evidence regarding model sensitivity does not change the assessments of the AR4 or SAP 3.4 (Abrupt Climate Change) of the US National Assessment.
12-716	12	60	34	60	35	the effect of increased atmospheric loading of aerosols'. But global aerosol loading is projected to decrease for all the RCPs. Or is this a regionally-specific effect? [Nathan Gillett, Canada]	Noted - This is regional
12-717	12	60	44			sec 12.5.5.6.1 on Amazon forest. Although Amazon has received all the attention, no reason why it should be treated differently from tropical forests in general. Recent papers by Good et al show models can predict climatic thresholds of where tropical forests exist, and how these might change with increasing CO2 [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland]	Accepted - This section has been renamed to be Tropical and Boreal Forests. Also, Good et al is cited in a newly titled (Tripical forests) 12.5.5.6.1
12-718	12	60	44			Section 12.5.5.6.1: See also IPCC SREX (2012, Table 3.3) for assessments of projected changes in drought occurrence in the Amazon region (assessed to be of low confidence). [Sonia Seneviratne, Switzerland]	Accepted - and referenced

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12-719	12	60	48	60	50	Over what period and under what scenario? [Nathan Gillett, Canada]	Accepted - Text revised to clarify
12-720	12	60	49	60	49	The precise number (70%) should be supported by the scenario and the time period. [Jouni Räisänen, Finland]	Accepted - Text revised accordingly
12-721	12	61	9			why do you judge the probability as "low"? Is this based on evidence? It is still very rare for coupled GCMs to have dynamic vegetation models, and no papers are yet written on CMIP5 results beyond the Hadley model as far as I know. Good et al (J. Clim, accpeted) show that HadGEM2-ES doesn't get a dieback, and this is due to not getting a large drying as seen in HadCM3. But it seems based on earlier precip plots in this chapter that drying over the Amazon is not unlikely? On what grounds do you quantify a threshold and make statements on likelihood of crossing it? [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland]	Accepted - We no longer make a likelihood statement. This has been rewritten as a confidence statement (see also new Table).
12-722	12	61	12			also need to discuss that boreal forest might expand at the northern edge. There is already evidence of treeline shifting north, and evidence this happened in previous warmer periods. The chapter should not leave itself open to criticism of only presenting "negative" or "bad" climate effects. [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland]	Accepted
12-723	12	61	55	61	56	I don't think steric sea level rise due to ocean warming would affect clathrate stability. My understanding is that bottom temperature and pressure are the important quantities. Bottom temperature warms of course but the steric sea level rise shouldn't on its own make any difference to bottom pressure. I think this sentence needs revising to make it clear that it's only the compoinent of sea level due to ocean mass changes that's important. [Richard Wood, UK]	Accepted - Revision made to clarify
12-724	12	61				12.5.5.7 Given the potential importance of this reservoir it seems that even though current studies don't yield a coherent picture, some assessment should be made of the likely range of possible carbon outputs. Is there a reasonable upper bound? Do we at least know that some nonzero carbon emission will occur? [Steven Sherwood, Australia]	Taken into account - We can state that all available studies suggest a positive feedback to anthropogenic climate change. But the strength of this feedback is very uncertain at this stage.
12-725	12	62	6	62	6	"Will provide" seems too deterministic for the current level of understanding. [Jouni Räisänen, Finland]	Taken into account -The words 'will' and 'significant' have been removed
12-726	12	62	13	62	13	Capitalize Boreal and Arctic [Benjamin Sanderson, United States of America]	Taken in account - paragraph deleted and relevant material is in Chapter 6.
12-727	12	62	22	64	8	FAQ12.1: In line with the standard WG1 FAQ style, can an italicised "overview answer" paragraph be produced please, and inserted at the beginning of this FAQ. [David Wratt, New Zealand]	We have extensively rewritten this FAQ and we have now the italicized "short answer" as the first paragraph.
12-728	12	62	26	62	28	Future climate is deternined by what actually happens not by any assumptions. Although you might be lucky if any of them work [VINCENT GRAY, NEW ZEALAND]	Accepted. That was an awkward statement. This part has been rewritten, and "assumptions" has been dropped.
12-729	12	62	32	62	37	Prediction is even more difficult if you never compare what you think will happen with what actually happens,. [VINCENT GRAY, NEW ZEALAND]	Noted. It is impossible to do this for future projections. That is why we use projections rather than predictions.
12-730	12	62	32			History has shown that predictions of human behaviour are extremely unsuccesful, even for averaged properties; so this line is a bit of an understatement (see Global catastrophes and trends, V Smil 2008, MIT press). [Ramon de Elia, Canada]	Noted. Scenarios do not pretend to be predictions. Rather, they are representation of plausible future development paths. WG1 does not attempt to predict human behaviour.
12-731	12	62	48	62	52	The underlying reason is that socio economic scenarios are cheaper to produce and analyse than climate simulations. [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Agreed, but we feel like the discussion does not need this justification/point to be made.
12-732	12	62				FAQ12.1 It is stated that socio-economic development is hard to predict, but I think it is well worth emphasising somewhere in this chapter how predictably human carbon emissions overall have grown since the beginning of the industrial revolution. The data offer more support for predictability of this than for predictability of climate. I agree nobody in 1960 would have predicted the iPhone, but they could and did	Noted. While this may be true for the past, there is no a priori reason it will hold for the future. For example, population growth shows similar behaviour until a country is fully industrialized, then the population

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						predict accurately how much fossil fuel we would be using. [Steven Sherwood, Australia]	starts decreasing. The fact about historical carbon emissions is interesting but belongs to the carbon cycle chapter.
12-733	12	62				FAQ 12.1: Current text does not answer the question. This is highlighted by the lack of any concluding statements, that would also serve the basis of a brief answer to be provided in an opening chapeau. [Thomas Stocker/ WGI TSU, Switzerland]	We have extensively rewritten this FAQ and we have now the italicized "short answer" as the first paragraph.It has undergone the evaluation and approval of the FAQ writer, Dave Hansford.
12-734	12	62				FAQ 12.1: We would suggest to turn the structure around so that it begins first with the limitations/uncertainties of the models, before moving on to the scenarios. [Thomas Stocker/ WGI TSU, Switzerland]	We feel like the FAQ deserve a better chance now that it has been extensively changed. The order as it is now, however, reflects the cascade of information in a climate simulation, and the figure.
12-735	12	63	4			I like this FAQ on models. It would be good to stress here that model properties such as climate sensitivity or carbon cycle feedback are emergent from the underlying equations and understanding. It is still perceived by some that these feedbacks are somehow “put into” the models and hence we simply get out the answers we expected. Here is a good place to really press home that the models are based on fundamental understanding and these properties emerge – and sometimes surprise us. [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland]	Rejected. We feel that introducing these high level concepts in the FAQ would complicate it unnecessarily.
12-736	12	63	14	63	15	I would cut this sentence about there not being a big enough computer to solve the equations without approximations. Whatever resolution was used, there would still be a need for approximations. [Nathan Gillett, Canada]	Accepted. The sentence has been eliminated in the new version.
12-737	12	63	14	63	15	I propose to delete or rephrase the sentence "there simply is not a computer big enough to solve the equation on a fine enough grid". I find it a bit too friendly and I am not sure it is really clear what a fine enough grid is. Can we quantify it? As mentionned just after, the main question is what kind of processes we want to model (because they play a role). [Didier Swingedouw, France]	Accepted. The sentence has been eliminated in the new version.
12-738	12	63	16	63	18	Reformulate as something like "many models now include mathematical description of some biological and chemical processes rooted, for example, in conservation laws, but formulations mainly based on empirical understanding are also common" [Jouni Räisänen, Finland]	We have reformulated the sentence as "Also, many small-scale physical, biological and chemical processes, such as cloud processes, cannot be described by those equations, and need to be approximated instead by so-called parametrisations within those climate models".
12-739	12	63	32	63	39	I find this paragraph too ready to praise the multimodel ensemble with no example of a weakness, whereas the perturbed physics is praised for being able to produce large number of runs!!! (which is not much praise) and damned for not being able to sample all possible choices of model formulation. The multimodel ensemble cannot do this either! I think this paragraph needs to be better balanced. So the multimodel is the ensemble used to sample uncertainty from different ways to build climate models but it is sampled in an ad hoc manner and so it is not easy understand the sources of uncertainty in this ensemble. In contrast, the perturbed physics ensemble is designed to sample variants of one particular climate model which allows us to understand how model parameters affect a particular climate response and thereby identify the key processes e.g. Joshi et al 200?). But as you say, it does not sample structural uncertainty. [David Sexton, UK]	Rejected. A decision was made to limit the discussion to muti-model ensembles, consistently ith the thrust of the question, and not introduce the concept of PPE that was felt would be in this limited context not familiar enough,or easy enough to understand, by readers of the FAQs.
12-740	12	63	33	63	33	replace "to evaluated" by "to evaluate" [Didier Swingedouw, France]	Corrected in the new version
12-741	12	63	35	63	35	I think "industry-standard" is a but too friendly. Please try to be more specific on what is meant here. [Didier Swingedouw, France]	Changed to a "standard choice"
12-742	12	63	50	63	56	As written this paragraph implies that statistical techniques are an optional extra for formulating projections from model output. As noted in the guidance paper on combining and assessing multi model projections (Knutti et al., 2010), 'Forming and interpreting ensembles for a particular purpose requires an understanding of the variations between model simulations and model set-up (e.g., internal variability, parameter perturbations, structural differences, see Section 2), and clarity about the	It was felt that the mention of statistical techniques in this context was introducing unnecessary complication to the discussion so the new version actually does not mention them anymore.

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						assumptions, e.g., about model independence, exchangeability, and the statistical model that is being used or assumed'. A statistical model, implicit or explicit, is vital for the interpretation of output from the model ensemble. [Nathan Gillett, Canada]	
12-743	12	63	50	63	56	Nice paragraph – Sexton et al 2011b would be a good reference here as it tests the sensitivity of pdfs to different expert choices. REFERENCE D. M. H. Sexton and James M. Murphy Multivariate prediction using imperfect climate models part II: robustness of methodological choices and consequences for climate sensitivity Clim. Dyn. 2011 10.1007/s00382-011-1209-8 [David Sexton, UK]	We cannot have references in an FAQ but the paper is referenced in the uncertainty section now.
12-744	12	64	11	65	43	FAQ 12.2: The language and explanations of this FAQ are well tailored for a non-specialist audience. [David Wratt, New Zealand]	Thanks
12-745	12	64	15	64	19	Please italicise this first paragraph "overall summary" , in line with the standard WG1 FAQ style. [David Wratt, New Zealand]	Done
12-746	12	64	15	65	41	What is 'expected" is purely the subjective opiniion of biased modellers [VINCENT GRAY, NEW ZEALAND]	We are summarizing results of physically based models that have been evaluated and verified from numerious perspectives. This is not opinion, but scientific assessment.
12-747	12	64	19			I'd suggest adding "In other areas the seasonal cycle of water availability will alter." [David Rowell, United Kingdom of Great Britain & Northern Ireland]	Accepted. Sentence added in the next paragraph, where discussion talks about different changes in different locations, noting also that changes may vary throughout the year.
12-748	12	64	35	64	44	You might mention that the hydrological cycle is also constrained by energy balance (as noted in the body of the chapter) and to a (much?) smaller extent by carbon dioxide (through links with evapotranspiration) [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Done. Energy mentioned in this paragraph. CO2 effects mentioned in the paragraph discussing evapotranspiration.
12-749	12	64	55	64	55	The increase in the water holding capacity of the atmosphere (i.e., increased ability to transport water vapour away from source areas) also affects the subtropical drying, particularly over the oceans. [Jouni Räisänen, Finland]	On p 64, lines 49-50, we note that we are focussing on changes over land, for specific reasons. We have added after p. 65, line 1, that the warmer temperatures allow more water transport into high latitudes, which should be a more important factor than possible changes in transport from already dry areas over land.
12-750	12	64	57	65	1	This mechanism would imply an increase in precip everywhere. See Held and Soden (2006) for a better explanation. [Nathan Gillett, Canada]	We are only talking about high latitudes in this sentence. Discussion just before this has noted that circulation changes lead to a decrease in precipitation in the subtropics.
12-751	12	64				FAQ 12.2 It might be helpful to note theat the signal to nosie ratio for precipitation etc is much lower than for temperature [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Done. This point is mentioned where we talk about precipitation variability.
12-752	12	64				FAQ12.2 This is a nice FAQ, but there is one question I know is bugging a lot of relatively knowledgeable people who already know most of what is in here: is the hydrological cycle intensifying or slowing down? The confusion comes because terms have not been defined clearly; the throughput of water increases in a warmer world, but the wind speeds generally are likely to decrease (still carrying more water due to Clausius-Clapeyron) and the atmospheric residence time of a water vapour molecule will likely increase. This needs to be clearly layed out. The same ambiguity affects discussion of "storm strength", where it is likely that instantaneous rain rates will increase but not necessarily winds. [Steven Sherwood, Australia]	We address this confusion on p. 64, lines 28-33, where we have pointed out that it is the wrong question to address. We note the importance of changes in humidity vs. changes in wind speed in a new sentence added after p. 65, line 1.
12-753	12	64				FAQ 12.2: Page 65, lines 15 - 17: Please avoid simplified and unquantified links to impacts (floods/droughts). [Thomas Stocker/ WGI TSU, Switzerland]	We have softened the links, simply noting that the changes in precipitation intensity and frequency might yield these hydrologic responses.

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12-754	12	64				FAQ 12.2, Fig 1: The current figure is very nice, but does not yet provide a comprehensive representation of the water cycle - land storage/cryosphere etc are missing. [Thomas Stocker/ WGI TSU, Switzerland]	The intent of the figure is to show where major changes are occurring, not to depict the entirety of the water cycle, which would require a very complex figure. Focussing on changes allows simplification that is in accord with the text's discussion.
12-755	12	65	4	65	11	It should be noted here that the climatic regions are not static with climate change, and that e.g. some regions with humid climate could shift to transitional / semi-arid conditions (see for instance Seneviratne et al. 2006, Nature; Seneviratne et al. 2010, Earth-Science Reviews). [Sonia Seneviratne, Switzerland]	A sentence noting this and its consistency with Figure 1 was added at the end of the paragraph.
12-756	12	65	4	65	11	Some of these statements seem questionable to me, although I am not a land-surface expert (e.g., I though rainfall shifts really were the 1st-order driver of soil moisture change, with veg playing a at best a feedback role). The first point made could be stated more simply as "Precipitation increases on average in a warmer world, but so must evaporation. Each will change with a different geographic pattern, ..." The claim that a warmer atmosphere "holds more water vapour" is poorly worded and carries the huge unstated caveat that relative humidity near the surface remain constant; in an FAQ it is probably best to sidestep this, but somewhere in the report it should be explained in a bit more detail. [Steven Sherwood, Australia]	We acknowledge on p 65, line 4, that precipitation as well as evapotranspiration control soil moisture. Because we discuss precipitation changes in the previous paragraph, we focus in this paragraph on evapotranspiration, to complete discussion of the (generally) two largest water fluxes at the surface. Further, we state that a warmer atmosphere "can contain" more moisture withough stating that it always does, thereby avoiding the issue of whether or not relative humidity changes. The potential for a larger amount of water vapor to be present in a warmer atmosphere means that evapotranspiration could occur, if there is sufficient terrestrial water. We do not state that greater evapotranspiration always must occur. We also purposely avoided the physically erroneous statement that the atmosphere "holds" water, using instead the verb contain, which by definitioni means that the atmosphere can have water present. However, some dictionaries also define "contain" to mean "hold", so to avoid any ambiguity, we changed "contain" to "have". Changes in relative humidity, and possible causes for the changes, are discussed in section 12.4.5.1
12-757	12	65	10	65	11	The standard WG1 FAQ style does not include references to chapter material, since FAQs are designed to be read "stand-alone". Can the reference in this line to Figure 12.27 and Figure 12.26 be dropped ? [David Wratt, New Zealand]	Yes - figure references dropped. The statements do not need figures to support them.
12-758	12	65	13	65	15	It seems counter-intuitive to draw a cause-effect relationship from the decrease in precipitation frequency to the increase in intensity. For a given increase in moisture content, the increase in precipitation intensity should not depend on whether this increase in moisture resulted from a longer time of accumulation between precipitation events or from some other reason. [Jouni Räisänen, Finland]	We agree that increased precipitation intensity does not necessarily mean less frequent precipitation events. However, as stated on p. 65, line 13, we are simply citing the behavior seen in model projections.
12-759	12	65	28	65	29	Does the runoff not first increase before the glaciers disappear? [Jouni Räisänen, Finland]	We do not state that runoff will disappear in all cases; rather, we state that it may disappear. How runoff changes in the transient situatiot depends on how rapidly the glaciers melt, and with respect to a chosen point of reference in time. The glaciers could lose mass by sublimation, too.
12-760	12	65	30	55	31	Would be clearer if first and second phrases were reversed - "If overall annual precipitation decreases, then these results do not …." [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Agree - change made.
12-761	12	65	34	65	41	FAQ 12.2, Fig. 1: Would it be possible to add a symbol, illustrating the main mechanism leading to the projected increase of precipitation in the NH extratropics? [Wilhelm May, Denmark]	There does not seem to be a clear, simple way to indicate on this diagram that atmospheric humidity will increase. We have attempted to show that moisture

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							transport into high latitudes will increase.
12-762	12	65	46	66	53	FAQ 12.3: The language and explanations of this FAQ are well tailored to a non-specialist audience. However, can the authors please provide a high-level one paragraph summary answer to the question, to be placed (in italics) ahead of the present first parapgraph - in line with the standard WG1 FAQ style. [David Wratt, New Zealand]	Taken into account
12-763	12	65	48	65	48	Prediction is difficult, particularly about the future, but prediction of impossible futures is even more difficult [VINCENT GRAY, NEW ZEALAND]	Noted. No changes requested.
12-764	12	65	50			Text notes that zero emissions case is "not plausible"; but does not address plausibility of constant forcing case, which requires near zero emissions of CO2 but maintaining aerosol negative forcing. This would seem just as implausible absent deliberate geoengineering. [Stephen E Schwartz, USA]	Noted. Comment is correct, but the text is correct as it stands.
12-765	12	65	57	65	58	The word "lifetime" (and decrease by a factor of e (2.71)) is a bit problematic here since this is not a good concept for CO2. I think a clear distcinction and a different wording should be used for CO2 (which is already partly done on line 2-3 on page 66). So some rewording would help. [Jan Fuglestvedt, NORWAY]	Accepted. Text now states states that CO2 is removed on mulitple timescales.
12-766	12	65	58	65	58	if you write "e ~2.71" it doesn't look like a reference to another chapter [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Rejected. Context should be obvious.
12-767	12	65	58	65	58	Please replace "e (2.71)" by "Euler's number e (equals to around 2.71)" [Didier Swingedouw, France]	Rejected. Too specific for a FAQ.
12-768	12	65				FAQ 12.3: Opening line - we would suggest that you avoid an opening that makes the question seem to be of academic significance only (....is not plausible..). We suggest an opening passage that engages the reader, by laying out why this particular benchmark (0 emissions from this point forward) is of interest. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. FAQ revised completely with input from science writer.
12-769	12	65				FAQ 12.3, Fig 1: Suggest raising the 'blue' zero emissions ensemble range up front, given this is the focus of the FAQ. Consider removing the 'constant forcing' case, which is not central to the FAQ response. [Thomas Stocker/ WGI TSU, Switzerland]	Rejected. Zero forcing line is clerly visible in front. FAQ was created with the idea not to discuss zero emissions only. Constant forcing (commitment) was an important concept introduced in AR4 and should be kept to illustrate the difference.
12-770	12	66	1	66	2	Regarding lifetimes of the various (non-CO2) gases: A reference could be given to chapter 8. [Jan Fuglestvedt, NORWAY]	Rejected. FAQs are standalone and have no references.
12-771	12	66	4	66	6	The sentences "About half of the anthropogenic CO2 is removed within a few decades but the remaining fraction stays in the atmosphere for much longer. About 20% of emitted CO2 is still in the atmosphere after 1000 years." should be re-written to avoid giving the impression that it is the same molecules that remain in the atmosphere. It should be explained that it is the change in concentration (or the perturbation) that remains over longer timescales. Response time or adjustment time are words that may be used. I had similar comments to chapter 6 and other parts of chapter 12. The explanation of the response to CO2 emissions could be better coordinated within and accrooss the chapters. [Jan Fuglestvedt, NORWAY]	Rejected. While true, this is an detail that is not of relevance for the lay reader. For the climate it is unimportant which molecules are where, only the concentration counts.
12-772	12	66	4		6	CO2 removal fraction: qualify by "according to most current models" One exception:Jacobson, M. Z. (2005), Correction to ‘‘Control of fossil-fuel particulate black carbon and organic matter, possibly the most effective method of slowing global warming,’’ J. Geophys. Res., 110, D14105, doi:10.1029/2005JD005888. [Stephen E Schwartz, USA]	Rejected. CO2 lifetime appears to be prescribed in that model (Fig. 1) rather than simulated.
12-773	12	66	17	66	22	The effect of CO2 and aerosols is discussed, but not the effects of short-lived GHGs. These are comparably important to aerosols. [Nathan Gillett, Canada]	Short lived GHG are mentioned in the text, but the figure would get too complex with more cases.
12-774	12	66	34	66	45	The effects of CO2 and non-CO2 GHGs are quite different and should be differentiated here. [Nathan Gillett, Canada]	Short lived GHG are mentioned in the text, but the figure would get too complex with more cases.
12-775	12	66	35		36	"the inertia of the climate system would delay the temperature response." It is clear from blue curve in Figure that much of the response to zeroing emission is rapid, not slowed by inertia of climate system. Need to qualify discussion. Not at all clear how that response curve implies that long-term global temp is controlled largely by	Rejected. FAQ is supposed to explain the main idea to the lay person. Response timescales are discussed at length in section 12.5

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						total CO_2 emissions. [Stephen E Schwartz, USA]	
12-776	12	66	40			"delays the necessary emission reduction." No. Simply limits allowable integrated emissions. Doesnt speak to timing at all. [Stephen E Schwartz, USA]	Accepted. Reworded.
12-777	12	66	47	66	51	I think this FAQ is very useful. While I really think it is important to show uncertainties related to calculations of future global temperature, I'm not sure if this is really needed in this figure. As I see it, the main function of FAQ 12.3, figure 1 is to show the main differences in result of these fundamentally different situations; i.e. zero emissions, constant emission, constant forcing. Removing the uncertainty would make it much easier to see the differences in development; and I believe that is the main point of this FAQ. [Jan Fuglestvedt, NORWAY]	Rejected. Because of the large uncertainties in the transient response and equilibrium response, it is imporant to show that a range of responses is possible for the same scenarios. We belive a single line woudl be misleading and imply a certainty that isn't there.
12-778	12	67	1			Reference to add: Neelin, J. D., M. Munnich, H. Su, J. E. Meyerson, and C. E. Holloway, 2006: Tropical drying trends in global warming models and observations. Proc. Nat. Acd. Sci., 103, 6110-6115. [J. David Neelin, United States]	Rejected. References must be justified in the text
12-779	12	67	1			Reference to add: Hall, A., X. Qu, and J. D. Neelin, 2008: Improving predictions of summer climate change in the United States Geophys. Res. Lett., 35, L01702, doi:10.1029/2007GL032012. [J. David Neelin, United States]	Rejected. References must be justified in the text
12-780	12	67	1			Neelin, J. D., C. Chou, and H. Su, 2003: Tropical drought regions in global warming and El Nino teleconnections. Geophys. Res. Lett.,, 30(24) 2275, doi:10.1029/2003GLO018625. [J. David Neelin, United States]	Rejected. References must be justified in the text
12-781	12	67	1			Chou, C. and J. D. Neelin, 2004: Mechanisms of global warming impacts on regional tropical precipitation. J. Climate, 17, 2688-2701. [J. David Neelin, United States]	Rejected. References must be justified in the text
12-782	12	67	1			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, 19 (17), 4207-4223 [J. David Neelin, United States]	Rejected. References must be justified in the text
12-783	12	67	30		31	Should be: Annan, J.D. and J.C. Hargreaves [Julia Hargreaves, Japan]	Fixed.
12-784	12	81	62			Santer Reference is incomplete. This may be a reference to an MPI report; if so consider upgrading to a reference from the refereed literature. [J. David Neelin, United States]	Fixed. This is the first publication to explicitly use pattern scaling, so the reference is retained.
12-785	12	85	16			The doi given by the journal for Watterson (2008) is 10.1029/2007JD009254 [Ian Watterson, Australia]	Editorial
12-786	12	85	17	85	18	The paper is now accepted, with the modified title 'Joint PDFs for Australian climate in future decades and an idealized application to wheat crop yield'. [Ian Watterson, Australia]	Fixed
12-787	12	85	19	85	20	The paper is now accepted, with the modified title 'Calculation of joint PDFs for climate change with properties matching recent Australian projections'. Note that the author initials are 'I. G.' (not I. M.) [Ian Watterson, Australia]	Fixed
12-788	12	85	26	85	26	Weertmann should be Weertman [Philippe Huybrechts, Belgium]	Fixed
12-789	12	86	9		10	M.J. Webb, K.D. Williams, J.C. Hargreaves, J.D. Annan [Julia Hargreaves, Japan]	Reference correct in the database. Endnote style problem.
12-790	12	87				It would be useful to know which models include the CO_2 physiological effect and which do not. [Olivier Boucher, France]	Taken into account. Information included.
12-791	12	87				Comment on entry for CSIRO-Mk3.6 in Table 12.1: We now have a core reference for CSIRO-Mk3.6 and its use in CMIP5: Rotstayn, L. D., S. J. Jeffrey, M. A. Collier, S. M. Dravitzki, A. C. Hirst, J. I. Syktus, and K. K. Wong (2012). Aerosol-induced changes in summer rainfall and circulation in the Australasian region: a study using single-forcing climate simulations. Atmos. Chem. Phys. Discuss. (in press). Hopefully, we will be able to report that it is published in ACP by the time of the Second Order Draft. [Leon Rotstayn, Australia]	Taken into account - reference cited.

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12-792	12	90				Figures general: In some figures, there seems to be extensive stippling, up to a level at which the figure becomes not very userfriendly. This issue of overstippling in figures has therefore to be resolved, and coordination with all relevant chapters needed ensuring a consistent approach. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.Stippling and hatching has been changed.
12-793	12	91				Fig. 12.1: Neither the figure caption nor FAQ text ever clearly tells us what an RCP is. According to the diagram, the RCP determines (via an IAM) both the emissions and the concentrations, which sounds exactly like SRES. I was quite confused by this diagram, its caption and the text (12.3.1). If I understand correctly, an ensemble of IAM simulations were considered, and a few representative cases selected that span the range of forcings at 2100; the emissions or concentration data from these are then used to drive carbon-equipped or traditional GCMs, respectively. The text never actually explains this, at least not in a way that was comprehensible to me. Once the above is understood, it is then clear why (as repeatedly stated) the forcing in the GCM won't exactly equal that associated with that scenario in the model that generated it. [Steven Sherwood, Australia]	Accepted. RCP scenarios are now described in the chapter. Description and figure changed in this chapter.
12-794	12	93	1	93	13	Does the term "climate forcing" have a consensus throughout the IPCC report? [Toshihiko Takemura, Japan]	The quantity is different from radiative forcing and therefore needs a separate name. It is explicitly explained in the caption. To our knowledge it is not defined in the glossary.
12-795	12	93	1			Figure 12.3 Forcings. And throughout. For all line graphs, identify the individual models; present the data online to permit analysis. [Stephen E Schwartz, USA]	Rejected. With >30 models individual lines are unreadable. All data is available from PCMDI
12-796	12	93				I would have said that I don't see any reason to be giving the clear sky longwave forcing, but in fact it is instructive that these are shown because the spread is much greater than I would have expected based on Collins JGR 06 RTMIP study. So the reasons for the spread need to be discussed. Is it because the concentrations are different or because of differences in treatment of radiation transfer. Certainly in a given year the spread of forcings is much greater than the canonical 10% 2 sigma uncertainty associated with LW GHG forcing. In this figure and throughout. For all line graphs, identify the individual models; present the data in online tables to permit analysis. The gray lines in panel b are hard to discern. These also should be identified by model and the data presented numerically in online tables. [Stephen E Schwartz, USA]	Noted. Reasons can only be dicussed if there is literature to support it. With >30 models individual lines are unreadable. All data is available from PCMDI
12-797	12	94				Fig 12. 4. A strong argument can be made that if the emissions of aerosol precursors is rolled back in proportion to ghg emissions in scenario 2.6 the temperature on that scenario will rise rapidly as the emissions are rolled back. See for example Brasseur GP, Roeckner E (2005) Impact of improved air quality on the future evolution of climate. Geophys Res Lett 32:L23704. doi:10.1029/2005GL023902 Knutti R, Krähenmann S, Frame DJ, Allen MR (2008) Comment on ‘‘Heat capacity, time constant, and sensitivity of Earth’s climate system’’ by S. E. Schwartz. J Geophys Res 113:D15103. doi:10.1029/2007JD009473 Knutti R., and G.-K. Plattner, 2012: Comment on “Why Hasn't Earth Warmed as Much as Expected?” by Schwartz et al. 2010. J. Climate. In press, http://dx.doi.org/10.1175/2011JCLI4038.1 Matthews HD, Caldeira K (2007) Transient climate-carbon simulations of planetary geoengineering. Proc Natl Acad Sci USA 104:9949-9954 The only way that temperature cannot increase is if the aerosol forcing is somehow maintained even as GHG emissions are decreased. Hence, again, the need to show total forcing, and uncertainty envelope that was	Noted. Unclear what exactly should be changed. This chapter does not assess scenarios. Temperature uncertainty is indicated by shading in the figure.

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						employed in the calculations ot temperature change. As well, any motivation for maintaining aerosol forcing while reducing GHG emissions would need to be presented. [Stephen E Schwartz, USA]	
12-798	12	94				It is essential that the results for individual models and ensemble members be shown and made available, digitally. It is essential that the forcings employed in each model be shown and made available, digitally. It is essential that absolute temperatures (not just anomalies) be shown and made available, digitally. It seems likely that the spread in model results is artifically low because modeling groups with high sensitivity employ low forcing and vice versa. But this can be determined only if the information is available to test this. [Stephen E Schwartz, USA]	Rejected. All data from CMIP5 is already available from PCMDI for those who want to do analysis.
12-799	12	94				Fig 12.4: Please explain in the caption the displayed bump of graph… [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Stated in the caption.
12-800	12	96	4	96	6	Say in the caption that the warming is for 2090-2099. Also say what sources of uncertainty are accounted for in the uncertainty estimates. [Nathan Gillett, Canada]	Figure and caption revised.
12-801	12	96				Fig 12.6--Caption should note that this is for late 21st-century. [Steven Sherwood, Australia]	Figure and caption revised.
12-802	12	97	4	97	5	Fig. 12.7: I think it would be interesting to know, on how many ensemble members the maps presented for the individual models are based, in particular if this number differs between models. [Wilhelm May, Denmark]	Accepted. Caption states that only one ensemble is shown.
12-803	12	97		97		Perhaps accompany this plot by a single zonal mean plot with all models SAT shown? [Benjamin Sanderson, United States of America]	Information is shown in the pattern scaling figure.
12-804	12	98	11			The 95th percentile of the distribution of what? Control variability? Interannual variability or something else? [Nathan Gillett, Canada]	Accepted. Distribution of model, now specified.
12-805	12	99				Figs such as this are useful, but impt also to show differences; this can be accomplished by evaluating the mean across the model set and showing a similar set of figs as difference from mean. This will bring out differences in pattern from model to model, as well as amplitude. There were similar figs in suppl to Ch 8 in AR4. Supplement might be useful vehicle here. [Stephen E Schwartz, USA]	The Atlas shows 25%, 50% and 75% of the distribution.
12-806	12	100				Figure 12.10: please explain the right diagram (supplement the caption): what is the "change" (ice loss compared to ?).+ couldn't the axis be displayed in the positive other direction, to make the correlation more visually evident ? [Philippe Marbaix, Belgium]	Figure deleted from the SOD
12-807	12	100				Fig 12.10: The message resulting from this figure is not very clear to us. Consider improving the information content of this figure and providing more detail in the caption. [Thomas Stocker/ WGI TSU, Switzerland]	Figure deleted from the SOD
12-808	12	102				Figure 12.12 : Please indicate whether the maps for the CMIP5 multimodel mean geographical changes in warm nights refer to JJA, DJF or annual. [Tsz-cheung Lee, Hong Kong]	Fixed.
12-809	12	102				Fig 12.12: the fonts are too small and the y-label cannot be read [Irina Mahlstein, Switzerland]	Fixed
12-810	12	103	1	103	1	The color scale should extend to higher values to avoid the high-latitude burn-off for the change in the 20-year minima in the last panel of Fig. 12.13. [Jouni Räisänen, Finland]	Accepted. Colors changed.
12-811	12	103				Fig 12.13: Does the color bar cover the whole range (>7.5°C)? In any case, the maximum level should be included in the range. With regard to the spatial resolution, it appears to us that it is rather very high. Which grid basis is used? Include significance shading if possible. [Thomas Stocker/ WGI TSU, Switzerland]	Colormaps changed. Data processing by other groups, therefore limited control. Significance shading not straightforward.
12-812	12	104				The figure is marked placeholder. Next draft should show these plots for the each of the models, together with data provided. This is a very valuable diagnostic.One really needs to see the forcings on the same chart to compare the forcings and the SW and LW anomalies. Net should be absolute, not anomaly. It should be possible to superimpose measmts from satellite onto the figure. [Stephen E Schwartz, USA]	Noted. Individual model results can not be shown due to lack of space. Anomalies instead of absolute values are shown for a better clarity of the figure. Link between model and observations are addressed in chapt. 9 and 13, and reference to these chapters has been included in the text.
12-813	12	105		107		Suggest show for each model; maybe in supplement. [Stephen E Schwartz, USA]	Rejected due to space limitations. All data is available

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							from PCMDI.
12-814	12	107	4	107	4	Mismatch labels and caption [Elisa Manzini, Germany]	Corrected.
12-815	12	107				The seasons in the map titles (JJA and ANN) don't agree with those in the caption (DJF and JJA). [Nathan Gillett, Canada]	Corrected.
12-816	12	108	5			Please include a sentence in the caption that explains the interval between the black contours. [Øyvind Christophersen, Norway]	Accepted - text revised.
12-817	12	109	6	109	7	Changes over what period and under what scenario? [Nathan Gillett, Canada]	Figure deleted from the SOD
12-818	12	110				What periods are the trends calculated over? You could use a longer period to get a high signal-to-noise for the projections? Why are the uncertainties for the obs smaller than the uncertainties for the model, particularly in panel (a). Ensemble averaging, complete spatial coverage, and a lack of observational errors should make the uncertainties smaller for the model. Are confidence intervals uncertainties in the mean, or do they show ensemble spread? [Nathan Gillett, Canada]	Figure deleted from the SOD
12-819	12	112				Suggest show for each model; maybe in supplement. [Stephen E Schwartz, USA]	Rejected due to space limitations. All data is available from PCMDI.
12-820	12	113				It would be easier to relate these results to others in the literature if precip changes were expressed as percentage changes of the climatology, and then F and Y woiuld be in % and %/K respectively. [Nathan Gillett, Canada]	Figure deleted from the SOD
12-821	12	113				Very powerful figure. Rather astonishing result. [Stephen E Schwartz, USA]	Figure deleted from the SOD
12-822	12	113				Fig. 12.23. Not sure this figure is essential. There are a lot of figures in this chapter. [Steven Sherwood, Australia]	Figure deleted from the SOD
12-823	12	113				Fig 12.23: Isn't the information provided in this figure, using the 4xCO2 runs, redundant with the information gained from Fig 12.5. If so, is this figure actually needed? [Thomas Stocker/ WGI TSU, Switzerland]	Figure deleted from the SOD
12-824	12	115				Given the approximately linear scaling of precipitation with forcing, I wonder if it is worth showing precip for three separate periods. [Nathan Gillett, Canada]	There are differences in the areas of significant changes, which is relevant when considering signal/noise and when changes will rise above internal variability.
12-825	12	115				Suggest show for each model; maybe in supplement. Suggest also show as absolute change (cm yr-1). Fractional change is not all that informative. Same for other similar figures in this series. [Stephen E Schwartz, USA]	Rejected due to space limitations. All data is available from PCMDI.
12-826	12	116				fig 12.26. can you clarify here if you plot TOTAL soil moisture, or just liquid (i.e. unfrozen?). [CHRIS JONES, United Kingdom of Great Britain & Northern Ireland]	Variable mrsos, i.e. all phases. Caption clarified.
12-827	12	117				Fig 12.27/12.28: Make figures and captions consistent. [Thomas Stocker/ WGI TSU, Switzerland]	Fixed.
12-828	12	120				The chapter text refers to the number of models which have an ice-free Arctic by the end of the century, but this is impossible to judge from this figure because the models may have different biases in their climatologies and only the anomalies are plotted here. It might be better to plot absolute values of sea ice extent. [Nathan Gillett, Canada]	Taken into account - We prefer to show anomalies instead of absolute values for coherency with Figure 10.13 of AR4 and for readability reasons. However, to meet the reviewer's comment, we now give in each panel the value of the multi-model mean sea ice extent averaged over 1986-2005. Furthermore, we now depict the 5-95% range of intra-ensemble variability, which is a more judicious choice to quantify the model spread when the mean approaches zero.
12-829	12	123				Fig 12.33: Caption needs to be expanded. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - Figure 12.33 has been replaced by a new one that is based on results from a

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							submitted paper and the caption has been expanded.
12-830	12	125				Why do the different scenarios exhibit different means in the historical period? Was this because different models were used for each scenario? Better to use a consistent set of models for all, even if this means throwing away some data. [Nathan Gillett, Canada]	Rejected. Standard in IPCC chapter is to use all models that are available for each scenario, otherwise some figures would be nearly empty.
12-831	12	126				Model agreement is apparently much higher for salinity changes (12.36) than for precip (12.25). Given that P-E changes are the major driver of surface salinity changes, why is this? Is this a mistake, or is this due to a different set of models being used to prepare each figure? [Nathan Gillett, Canada]	Noted. Presumably because the ocean integrates the signal and salinity has a much smaller temporal variability.
12-832	12	127				Figure 12.37: Estimates of variability (and uncertainty in the mean state) from continuous obersvational estimates of the AMOC have improved significantly since AR4 and should be plotted on these figures to allow the reader distinguish between 'good' and 'bad' models. See Chapter 3, figure 3.12 and the associated text. [Chris Roberts, Uk]	Not considered for the SOD, but might be an option to add later.
12-833	12	128				Show CMIP3 and CMIP5 models on the same plot, otherwise it is hard to compare. [Nathan Gillett, Canada]	Accepted. Figure redone.
12-834	12	128				Forcing needs to be shown; models identified; time series available in digital form in archive. I think the caption is meant to refer to AR4. All that said, it is not clear why this is presented. What is the point? [Stephen E Schwartz, USA]	Rejected. Forcings for SRES and RCP are shown in section 12.2. Data is available from PCMDI. This section discusses the differences between CMIP3 and CMIP5. Because the scenarios are different we have to use emulation methods to predict what CMIP3 would have given for the RCPs.
12-835	12	128				Fig. 12.38 might be expendable. [Steven Sherwood, Australia]	Figure redone to better make the point.
12-836	12	129				This figure showing projected 21st century warming with uncertainties is based on a single study. Can the results of other similar studies be included? [Nathan Gillett, Canada]	Noted. To our knowledge no other model has produces uncertainty estimates for RCP and SRES. If there are other estimats they could be consdered.
12-837	12	130				It would aid the reader if the areas of significant difference between the CMIP3 pattern and the CMIP5 pattern were hatched. If there are no significant differences based on a field significance test, then this could be stated in the caption, and no hatching is needed. [Nathan Gillett, Canada]	Accepted. Stippling included in revised figure.
12-838	12	131				Fig. 12.41 might be expendable, can just say in text that there is an x% bias by year Y. [Steven Sherwood, Australia]	Rejected. Figure is essential to discuss carbon cycle feedbacks and to support projections from emission driven scenarios.
12-839	12	131				Fig 12.41: Which emission scenario forms the basis for this figure? What is the source of the original CO2 concentration used to prescribe CO2 in the models come from? Please expand caption. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. RCP8.5 specified in the caption. Details on CO2 concentration is given in the text.
12-840	12	132		133		Need to show forcings. All depends on aerosol forcing after CO2 emissions cease. [Stephen E Schwartz, USA]	Rejected. Forcings for SRES and RCP are shown in section 12.2.
12-841	12	132				This figure could be merged with Fig 12.43. Also - panel (a) shows CO2 concentration not forcing. How were the uncertainty ranges calcualted? You could CMIP5 simulated temperatures to the graph to allow comparisoin of the EMICs with AOGCMs. [Nathan Gillett, Canada]	Rejected. Figures have different purposes and different timescales.
12-842	12	132				I find this figure 12. 42 a bit redondant with 12.43. They can be combined into one. Also, it will be interesting to look at the AMOC behavior up to year 3000 (collapse, revovery?). In my view, this is the main interest of using such simple model (EMIC): looking at long timescale that AOGCMs can hardly reach. [Didier Swingedouw, France]	Rejected. Figures have different purposes and different timescales. An AMOC assessment would be problematic because the scenarios are idealized and those models are limited in their ability to simulate the proceses relevant for the AMOC.
12-843	12	133				Figure 12.43 c) This way of expressing the "realized warming" seems to mis a key aspect of what "realized" means - could it be possible to show how much warming is realized as compared to the equibrium temperature corresponding to the maximum forcing of each scenario ? Or better, ideally, to show the realized	Noted. Realized warming is with respect to the end, now specified in the caption.

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						warming as compared to the equilibrium temperature associated with the forcing at the given point in time ? This would possibly clarify the meaning of the "150%" (1.5) peak for RCP2.6, which currently is a combination of true "realized" warming changes and concentration changes. [Philippe Marbaix, Belgium]	
12-844	12	134	4	134	7	Only CO2 emissions are shown, but presumably other forcings were included. This needs to be clarified in the caption. [Nathan Gillett, Canada]	Done
12-845	12	134				Show forcings; surprised at rapid drop of temp in panel C at year 2300 given more or less constant CO2. Needs explanation [Stephen E Schwartz, USA]	Clariified that shortlived forcings are eliminated in 2300 as well.
12-846	12	136				Figure 12.46: please use uniform units, and avoid PgC as the text is using tons (not grams). [Philippe Marbaix, Belgium]	PgC is the default in AR5.
12-847	12	140				FAQ12.1 Figure. "Responses" is a strange word to use, it implies human responses. Maybe "outcomes"? Anyway I like very much the idea of showing three model predictions as examples of what could happen. Titles on right-hand side should note the forecast time (end of century) but dont' need to repeat "high/low emission scenario" which it already says in the left panel. [Steven Sherwood, Australia]	Accepted. Changed to "temperature responses in 2081-2099".
12-848	12	140				It is strange to only show such a small region in the right panels of FAQ12.1 Fig. 1. Quality of these panels is also not very good. I suggest to show larger region, because here, people from America or China will feel a bit sad not to see their region. I understand that this is just a pedagogical example. Nevertheless, I think it should be better to have the whole globe. [Didier Swingedouw, France]	Rejected. This is purely for illustration that the temperature response differs between models, not to show any details. Those are given in the Atlas and the chapter.
12-849	12	141	4			FAQ 12.2, Figure 1: This figure is a drastic oversimplification of the likely changes in the hydrological cycle. It washes out into the zonal average features that are actually quite dependent on local dynamics such as the eastern basin descent zones or the equatorial Pacific, and gives an impression of much greater simplicity and certainty that is actually warranted based on the set of models and current understanding. The text of the FAQ is also simplified, but significantly better than the figure. I suggest flagging this as something to revise after the July 12 deadline for the current round of analysis papers being undertaken by the community on the CMIP5 archive. [J. David Neelin, United States]	We have not found compelling evidence in the CMIP5 results, to date, of important change in the Walker Circulation or other east-west circulations that would require reporting them here. The figure does show greater drying in the Mediterranean Basin and northwest Mexico/southwest U.S., which is a robust feature in the model output. The point of the FAQ is to present the broad picture in terms that the non-scientist can understand, so the simplicity is intentional.
12-850	12	141				I suggest to move the arrow "evaporation" from FAQ 12.2 Fig. 1 towards the tropics, where most of evaopration over the Earth indeed occurs. I find it a bit misleading to put the arrow on a region ice covered during a large part of the year… [Didier Swingedouw, France]	Agreed - done.
12-851	12	142				Fig FAQ 12.3 Fig 1, Text and caption should call attn to bump in zero emissions case due to turning off aerosol forcing; Point out that the magnitude of the bump is uncertain because of uncertainty in aerosol offset of total forcing. [Stephen E Schwartz, USA]	Accpted.The aerosol case is now explcitly referred to in the text. Note that the FAQs are targeting a non-expert audience.
12-852	12	142				FAQ12.3 Figure. Caption should say a few words about why T lurches upward after 2010 in the zero-emission scenario (e.g. "the sudden warming after 2010 arises because air pollution, which cools the planet (see FAQ 7.2), also disappears in this scenario." Anyone looking at the figure will immediately be confronted by this. [Steven Sherwood, Australia]	Accepted. The aerosol case is now explcitly referred to in the text.
12-853	12	142				FAQ 12.3 Fig. 1: It is strange to see that with zero emissions we observe a very rapid warming. I understand that this is due to aerosols effect, but this figure aims at being pedagogical. I dread it will not be well understood by a large-audience but I this is just my opinion. I let the authors decide what to do with it. [Didier Swingedouw, France]	The aerosol case is now explcitly referred to in the text.